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Schooling and the AFQT:
Evidence from School Entry Laws†

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Schooling and the AFQT: Evidence from School Entry Laws

Abstract

Is the AFQT a measure of achievement or innate ability? In this paper, we test for a relationship between schooling and AFQT performance in the NLSY 79 by comparing test-takers with birthdays near state cutoff dates for school entry. We instrument for schooling at the test date with "academic cohort"—the year in which an individual should have entered first grade given her birthday and the state in which she was likely to have entered school—in a model which allows age at the test date to have a direct effect on AFQT performance. This identification strategy reveals large impacts of schooling on the AFQT performance of blacks and Hispanics, but no schooling effect for whites. Within race, schooling is generally found to have larger impacts on the more academic components of the underlying test battery. On balance, these findings provide support for the hypothesis that the AFQT measures school achievement (JEL I2, J2)

I. Introduction

The relative importance of "nature" and "nurture" to performance on standardized tests has long been a matter of academic debate. This debate was given new life in the mid-1990s with publication of *The Bell Curve*, where Herrnstein and Murray (1994) argued that a variety of social and economic outcomes could be predicted with performance on a single cognitive test—the Armed Forces Qualifying Test, or AFQT. While this finding alone was surprising, more controversial was the authors' interpretation of the AFQT as a measure of innate ability, rather than as a measure of achievement. Given this interpretation, their analysis implied that investments in human capital by individual choice or government intervention could do little to break the intergenerational transmission of socioeconomic status.

Understanding whether the AFQT is a measure of ability or achievement is thus critical to drawing inferences from studies where it is employed. Nonetheless, there appears to be little consensus in interpretation in the myriad of other studies in which the AFQT—available for most respondents in the National Longitudinal Survey of Youth 1979 (NLSY 79)—has been used as a proxy for unobservable skill.[1] In large part, this lack of consensus can be attributed to an identification problem. Testing the ability interpretation of the AFQT requires examining the relationship between test performance and measurable forms of human capital, such as completed schooling. However, schooling could itself be related to innate ability, so that higher ability individuals will have both scored better on the test and accumulated more years of schooling by the time it was administered. Simple correlations,

[1] Many of these studies (though certainly not all) have been catalogued at http://www.chrr.ohio-state.edu/nls-bib/.

such as that presented in Fischer, *et al.* (1996), may therefore overstate the effect of schooling on the AFQT, biasing researchers toward rejecting the ability interpretation of the test.

In this paper, we present new estimates of the effect of schooling on AFQT performance for the youngest cohorts in the NLSY, who were between the ages of 15 and 19 when administered the test. To address the endogeneity of schooling, we base our analysis on comparisons of test-takers with birthdays near state-mandated cutoff dates for school entry. Because school entry laws specify an exact date by which entering first graders must reach age six (e.g., September 1), they generate sharp differences in average age at school entry—and therefore in average completed schooling during the period of enrollment—among students of nearly the same age and arguable the same level of innate ability, on average.[2] We construct an instrument for schooling on the basis of this discontinuity. In practice, date of birth need not be randomly assigned or its effects on the AFQT continuous for the model to be identified: given variation across states in school entry cutoff dates for the cohorts of interest, we are able allow for a fairly unrestricted direct effect of birth date—and implicitly age at the time of test administration—on test performance.

This study is not the first to address the endogeneity problem that arises when estimating the relationship between schooling and the AFQT, though previous identification strategies have relied on stronger assumptions. Following Herrnstein and Murray (1994), Winship and Korenman (1997) assume that schooling at the test date is randomly assigned

[2] Angrist and Krueger (1991) were the first to use school entry legislation to motivate an instrument for schooling.

conditional on family background and a "pretest"—performance on an IQ test at a younger age. To identify a structural model, Hansen, Heckman, and Mullen (2003) make somewhat restrictive assumptions on unobservable characteristics, such as age at school entry, grade progression, and the effect of age on AFQT performance. Like this paper, Neal and Johnson (1996) motivate their analysis with school entry policies. However, they use quarter of birth as an instrument for schooling, and must consequently assume that season of birth—as well as small changes in age—have no direct effect on test performance.

Our approach is less restrictive along several dimensions. We instrument for schooling at the test date with "academic cohort"—the year in which an individual should have entered first grade given her exact birth date and the state in which she was likely to have entered school—in a model which allows date of birth to have a direct effect on AFQT performance. We therefore use only that variation of schooling in season of birth that can be directly tied to school entry policies, minimizing the extent to which identification derives from seasonal differences in average ability or in other unobservable correlates of test scores.[3] At the same time, given that age at the test date and date of birth are highly correlated in the NLSY, our model implicitly allows for an effect of age on AFQT performance. This is useful, as small increases in age may in fact affect test scores within a relatively young subpopulation.

On balance, our findings support the achievement interpretation of the AFQT. We find relatively large impacts of schooling on the test performance of minorities: for blacks and Hispanics, two-stage least squares estimates imply that an additional year of completed

[3] The quarter of birth instrument for schooling has been criticized on this account (Bound, Jaeger, and Baker, 1995).

schooling by the test date raises performance by 0.35 standard deviations. By contrast, our identification strategy reveals no evidence of an effect of schooling on the test performance of white NLSY respondents, who were arguably more likely to have learned the material tested on the AFQT at younger ages. Consistent with our causal interpretation of the results, we also find larger effects of schooling on components of the underlying test battery that should be more sensitive. These results are robust to controls for family background and do not appear to be biased by censoring at the top of the test score distribution.

II. Background

A. NLSY Data on the AFQT

The NLSY 79 is an ongoing panel survey of a nationally representative sample of individuals born between 1957 and 1964. When first initiated in 1979, the NLSY had 12,686 participants between the ages of 14 and 22, more than 5000 of which constituted a supplemental sample of Hispanics, blacks, and economically disadvantaged whites.[4] In 1980, the U.S. military's Armed Services Vocational Aptitude Battery (ASVAB) – a ten section written exam administered to all entering military recruits – was given to 11,914 of the NLSY respondents, around 94 percent of the original sample. NLSY participants were recruited to take the AFQT (and the larger ASVAB) through a Department of Defense project that sought a nationally representative sample of youths from which they could update the norms of the test from the previous World War II reference population.

[4] Appropriate use of the sampling weights generates a nationally representative sample.

Each section of the ASVAB attempts to measure skills in a separate subject area. Some subject areas are academic, and some are vocational.[5] Four of the ten sections comprise the Armed Forces Qualifying Test (AFQT), used by the military as the primary criterion to determine eligibility for enlistment and "trainability" (Center for Human Resource Research, 2001; p. 94). Originally, AFQT percentile scores were derived from a weighted sum of raw scores from two math and two verbal sections of the test. Beginning in 1989, the Department of Defense changed which math sections were in the AFQT, as well as the procedures for calculating the AFQT scores from the raw section scores.[6] In this paper, we use these "revised" AFQT scores, transformed into standard deviation units so that our results can be more readily compared to previous studies.

There are several additional aspects of the AFQT and its administration in the NLSY that are relevant for this analysis. First, the AFQT was given during the summer and fall of 1980, though the NLSY does not record the exact date that each respondent took the exam. While this time frame is narrow enough to precisely measure years of completed schooling at the test date, it is too wide to measure age at the test date with the same degree of precision. In our analysis, we therefore control for date of birth, which can be precisely measured given the available data and should be highly correlated with age at the test date. In specifications where we include smooth functions in age, we are recoding date of birth as age as of July 1,

[5] The subjects are (1) general science; (2) arithmetic reasoning; (3) word knowledge; (4) paragraph comprehension; (5) numerical operations; (6) coding speed; (7) auto and shop information; (8) math knowledge; (9) mechanical comprehension; and (10) electronics information.

[6] Originally, AFQT percentile scores were based on the sum of the word knowledge, paragraph comprehension, and arithmetic reasoning, plus one-half of the numerical operations raw scores. The post-1989 version of the AFQT first replaced numerical operations with math knowledge. In addition, the revised AFQT score is based on a weighted sum of "standard," rather than raw, composite verbal and math scores, where the composite verbal raw score is the sum of scores in word knowledge and paragraph comprehension, and the verbal composite gets twice the weight of the math scores (NLS User Services, 1992, Tables C and D).

1980—the approximate date when the test was first given—for the purposes of interpretation.[7]

Second, the same test was administered to all NLSY respondents regardless of age or schooling attainment, though the test is designed to be appropriate only for individuals aged 17 and over (Center for Human Resource Research, 2001).[8] The AFQT is therefore not a school achievement test in the standard sense, as it is not intended to the skills acquired from any given year of school. Thus, to the extent that the AFQT measures skills imparted through "nurture," it should be thought of a *cumulative* measure of skill: any individual's score may be affected by human capital investments made from birth through the time of test administration.

B. Schooling and the AFQT: The Existing Evidence

In this paper, we focus on the role of *later* investments in raising AFQT performance. In particular, we examine whether an additional year of high school impacts skill as measured through the AFQT. We are interested in a model of the form

$$(1) \qquad T_i = \theta S_i + f(V_i) + v_i$$

where T_i is the AFQT score of individual *i*; S_i is completed schooling by the time of the test; $f(V_i)$ is some function of a vector of control variables, which might include measures of

7 Since we do not know the exact date when the test was administered for each respondent, we also unfortunately cannot address the problem of "summer setback"—the loss in skill that occurs when school is not in session. However, to the extent that summer setback affects children of all races similarly, and that the time of test administration is not related to race, comparisons of our results across race should remain meaningful. Fryer and Levitt (2002) provide evidence that summer setback is equally prevalent across race using a sample of young students.

8 Though the youngest respondents in the NLSY took the AFQT, their scores were not used to create new norms for the test. The NLSY codebook suggests that considerable care be used in interpreting and using test scores for these respondents (Center for Human Resource Research, 2001).

family background, and v_i is a mean independent error term. Since the AFQT was administered in the NLSY to individuals of different ages and levels of schooling, most researchers have also included age in the vector of controls, V_i. The parameter θ gives the marginal effect of a year of schooling on AFQT performance.[9]

In practice, it is impossible to estimate model (1): $f(\cdot)$ is an unknown function, and the entire vector of appropriate control variables may be unobservable to the researcher. Instead, the existing literature has focused on a restricted version of the model:

(2) $$T_i = \theta S_i + \beta' X_i + \varepsilon_i ,$$

where $X_i \subset V_i$ is some subset of controls. A new error term, ε_i, now includes unobservable determinants of test performance (such as fixed, innate ability) that are potentially correlated with S_i. Thus, the parameter of interest, θ, will not necessarily be identified through simple least squares regression.

Researchers have addressed this endogeneity problem in several ways. One approach, employed by Winship and Korenman (1997), assumes that schooling is randomly assigned conditional on X_i, provided that it includes a "pretest," or a measure of IQ at a younger age. In this framework, estimates of θ identify the effect of having an unexpectedly high level of schooling (given early IQ, age at the test date, etc.) on the "gain" in skill from early childhood to high school.

An alternative approach is to find an instrument for schooling at the test date, then estimate model (2) using two-stage least squares. For example, Neal and Johnson (1996)

[9] This is a restricted version of a more general model discussed in Hansen, Heckman and Mullen (2003). We restrict ourselves to this model for several reasons. First, this specification has formed the basis of most other studies of the relationship between AFQT performance and schooling, as discussed below. Second, our estimation strategy is sufficient to identify one parameter (θ); more general models would not be identified.

use quarter of birth as an instrument to estimate the effect of schooling on the AFQT. Like Angrist and Krueger (1991), they argue that quarter of birth is related to schooling through school entry laws. Specifically, if a child's birthday is such that he would turn age six after the mandated cutoff date for school entry (e.g., September 1), he must wait an additional academic year before starting first grade, and thereby fall a full grade behind slightly older peers born during the same calendar year. Children born later in the calendar year (i.e., in the fourth quarter) will then tend to have less schooling than children born earlier in the year. This within-cohort grade difference will be most apparent when children are still enrolled in school, and will persist provided that high school dropout decisions are responsive to compulsory schooling regulations.[10]

Both of these reduced-form approaches have generated similar estimates of effect of schooling on AFQT performance. Using the sub-sample of NLSY respondents for whom early IQ is recorded, Winship and Korenman (1997) estimate that an additional year of schooling raises AFQT performance by 4 percentile points, or approximately 0.1 standard deviations.[11] When they take into account measurement error in early IQ and education, they estimate that the effect could be larger—on the order of 5 to 6 percentile points, or 0.15 standard deviations. Applying the instrumental variables strategy discussed above to NLSY cohorts born between 1962 and 1964, who were largely still enrolled when the AFQT was

[10] The use of quarter of birth as an instrument for schooling in Angrist and Krueger (1991) depends upon the constraint on high school dropout behavior imposed by school entry legislation. Individuals with birthdays immediately after the school entry cutoff date will reach the minimum age of school exit having completed less schooling than individuals born right before.

[11] Their approach is in fact similar to that undertaken by Herrnstein and Murray (1994). However, in correcting several errors made by Herrnstein and Murray (1994) in their analysis (e.g., failing to control appropriately for age and problems with coding of missing data), Winship and Korenman (1997) find considerably larger effects of schooling on the AFQT.

administered, Neal and Johnson (1996) estimate that a year of schooling is associated with a 0.22 to 0.25 standard deviation gain in AFQT scores.

III. Identification Strategy

A. Motivations

The empirical strategy undertaken in this paper has similar motivations to Neal and Johnson (1996). However, we attempt to identify the effect of schooling on the AFQT using only that component of season-of-birth variation in schooling that is in fact driven by school entry legislation. As shown in Table 1, the school entry cutoff dates faced by the NL.SY cohorts were concentrated in the third and fourth quarters of the calendar year.[12] It therefore seems inappropriate to base comparisons on children born earlier in the year, particularly given that season of birth may not be randomly assigned (Bound, Jaeger, and Baker, 1995).[13]

To demonstrate more plainly the effects of school entry legislation, Figures 1 through 4 plot average completed schooling at the test date by date of birth for NLSY respondents born between 1961 and 1964 and in states with different cutoff dates (indicated with vertical lines). Figure 1 uses data for native-born respondents with non-missing information on date of birth, while the remaining figures limit the samples to non-Hispanic

[12] These cutoff dates were acquired by the authors from legal archives. See Appendix A. Dates are adjusted for consistent interpretation: a child residing in a given state is able to begin first grade if she has reached the required age *no later than* the date given. Cutoff dates will later be matched to respondents on the basis of expected year of entry into first grade and their state of birth. Cutoff dates relevant for first grade rather than kindergarten entry are perhaps a better gauge of institutional constraints on school commencement during the period of interest, since public kindergarten were not yet available in all parts of the country when the NLSY cohorts entered school (Cascio, 2004).

[13] Historically, fertility of lower SES groups has been more sensitive to season, resulting in a relatively high probability of giving birth at certain times during the year (Warren and Tyler, 1979; Seiver, 1985; Kestenbaum, 1987; Lam and Miron, 1991). This suggests that seasonal fluctuations in schooling *per se* are not a compelling source of variation with which to identify the effect of schooling on test scores.

whites (Figure 2), blacks (Figure 3), and Hispanics (Figure 4). In each figure, the panels correspond to the modal cutoff dates for school entry, shown in Table 1: September 1 (Panel A), October 1 (Panel B), December 1 (Panel C), and December 31/January 1 (Panel D). Respondents have been assigned school entry dates on the basis of their state of birth, since state of school entry is not recorded in the NLSY.[14]

The figures suggest that the effects of school entry legislation are in fact highly "local" to the school entry cutoff date. Though compliance appears far from perfect, the largest and most abrupt changes in completed schooling occur among individuals with birthdays near the cutoff dates in their states of birth. Further, there is no apparent discontinuity of completed schooling around December 1 or January 1 for the sample of individuals born in states with September 1 cutoff dates for school entry, and *vice versa*. This is what we would expect: if subject to a September 1 regulation, for example, a child would be able to begin school if born on August 31 or September 1, but would not be able to begin school if born on September 2 or later during the same calendar year. In a state with a December 1 cutoff, these abrupt changes should occur among individuals with birthdays at the beginning of December, not those born earlier in the academic year.

In principle, we should be able to exploit these abrupt changes in schooling by birthday to estimate the relationship between schooling and the AFQT.[15] Such a regression discontinuity approach would be particularly compelling in this application (Cook and Campbell, 1979). For individuals with birthdays near school entry cutoff dates, differences

[14] In the few instances where state of birth is missing, we use state of residence to assign cutoff dates to respondents, provided that the respondent reports having never moved from his place of residence.
[15] This approach is employed by Cahan and Cohon (1989) to separately identify the effects of schooling and age on achievement test scores in Israel.

in schooling will be relatively large, though differences in age at the test date—which might also matter for performance—relatively small. Non-random sorting around the cutoff date (the selection threshold) is also highly unlikely. For example, respondents born on September 1 are arguably of equal ability, on average, to those born on September 2, even if born or residing in states with September 1 cutoffs.[16] Thus, the presence of discontinuities in average test performance among individuals with birthdays around school entry cutoff dates should be revealing of any relationship between schooling and the AFQT.

In practice, small sample sizes mean that there is limited scope to make such local comparisons, however compelling.[17] The remainder of this section describes our parametric approach to exploiting this policy-induced discontinuity of schooling in birth date to identify the relationship between schooling and the AFQT. For the most part, our model will retain the desirable properties of the comparisons described above. Given differences across states in school entry cutoff dates, our approach has the additional benefit of allowing us to control for relatively unrestricted functions of birth date, our proxy for age when the test was administered.

B. Empirical Framework

The model of interest is a simple extension of (2):

(3) $$T_{is} = \theta\, S_{is} + \psi(A_{is}) + \beta' X_{is} + \alpha_s + \varepsilon_{is}$$

[16] Fertility and birth timing decisions—particularly the "choice" to give birth one day as opposed to the next—are likely to be made with regard to the immediate health of the mother or child, not the prospective age at which a child will begin first grade. Though women today might exert more power over the exact day in which they give birth (i.e., through scheduling of cesarean sections), this was much less likely to be the case for mothers of the NLSY participants in our sample, who were born in the early 1960s.

[17] In their study of maternal education and birth outcomes, McCrary and Royer (2003) have the sample sizes needed to apply a non-parametric estimation strategy.

where $\psi(A_{is})$ is a function in the approximate age of individual *i* from state *s* at the time that the AFQT was administered (measured in days); α_s is a state fixed effect; and all other variables are as previously defined. In our estimates, we allow for $\psi(A_{is})$ to be either a smooth function (e.g., a linear function or higher-order polynomial) or an unrestricted function (e.g., a linear function of dummies for age in months). While the former is more intuitive—age or maturation itself will not have abrupt or discontinuous impacts on test performance—the latter may be preferable for reasons discussed below. Regardless of the exact form taken by $\psi(A_{is})$, the close attention paid to age effects makes our approach distinct from the literature on the AFQT and schooling, which has generally controlled for age only crudely, if at all.

As described above, schooling at the test date might still be related to unobservable determinants of AFQT performance, even conditional on age and family background. We therefore propose the following instrument for S_{is}— a series of indicators for academic cohort. More formally:

(4) $$C_{is}^{k} = \begin{cases} 1 & if\ \ y_{is} = k-6\ amd\ d_{is} \le d_{is}^{*}\ or\ y_{is} = k-7\ and\ d_{is} > d_{is}^{*} \\ 0 & otherwise \end{cases}$$

where y_{is} represents year of birth and $d_{is} = 1,\ldots,365$ represents day of birth within the calendar year (which together define an individual's date of birth), and d_{is}^{*} stands for the day of the year on which the school entry cutoff date fell in state *s* during the calendar year when individual *i* turned age six. Thus, an individual born in 1962 in a state with a September 1 cutoff date should have started first grade in the fall of 1968 if born before or on September 1, in the fall of 1969 otherwise. With a few exceptions (e.g., North Carolina and Delaware), the school entry dates faced by all respondents born in the same state were identical, so $d_{is}^* = d_s^*$ for all individuals *i* in state *s*.

Given these instruments, the relationship between schooling, academic cohort, and age is captured by the model

$$(5) \qquad S_{is} = \sum_{k=m}^{M} \mu_k' C_{is}^k + \vartheta(A_{is}) + \gamma' X_{is} + \mu_s + \omega_{is},$$

where μ_s is a state fixed effect, and all other variables are as previously defined. Coefficients on the academic cohort indicators, μ_k; $k = m, \ldots, M$, give the average impact on schooling of being assigned to enter first grade in year *k*, holding constant family background, average educational attainment in the state, and what would have otherwise been expected for individuals of the same age. Assuming that the dummy for the youngest academic cohort in the sample is omitted for the purposes of identification, perfect compliance with the law predicts that $\mu_{M-j} = j + 1$, or that every one-year reduction in academic cohort raises completed schooling at the test date by on average one year.

More generally, if the marginal effect of academic cohort is constant across all cohorts under consideration, it will be the case that $\mu_{M-j} = \mu(j+1)$, where $\mu \equiv \mu_M$. Imposing this restriction on equation (5), we get an alternative model for schooling:

$$(5') \qquad S_{is} = \mu C_{is} + \vartheta(A_{is}) + \gamma' X_{is} + \mu_s + \omega_{is},$$

where $C_{is} = \sum (j+1) C_{is}^{M-j}$. We estimate this alternative model to improve precision, and the restrictions are in practice only rarely rejected.

For both models of schooling, it is important to note that our proxy for age is birth date: if there were no variation in cutoff dates across states, our models would only be identified if $\vartheta(\cdot)$ were specified as a smooth function. In this case, equations (5) and (5') should be thought of in terms of a parametric regression-discontinuity (RD) framework.[18] Given that there is variation in these dates, we can allow the function $\vartheta(\cdot)$ to be more unrestricted, such as a series of date of birth dummy variables (e.g., year of birth dummies interacted with month of birth dummies). In this case, equations (5) and (5') should be interpreted as differences-in-differences (DD) models. The DD approach may be preferable: while the effects of age on the AFQT might be smooth, the effects of birth season may not be.

We use versions of equations (5) and (5') as first-stage models in estimating equation (3) with two-stage least squares (2SLS). Academic cohort will be a valid instrument if it exhibits no relationship with ε_{is}, conditional on age and other observable characteristics. While this assumption cannot be directly tested, we document below that there is little if any relationship between academic cohort and observed correlates of test scores.

IV. Data

A. Sample

Because it is the subpopulation for which the relationship between school entry laws and schooling is both theoretically and empirically the strongest, we restrict attention to the youngest NLSY respondents in the analysis that follows. Specifically, we focus on individuals who, given their birth date and state of birth, should have entered first grade

[18] As in any RD framework, it is useful to control for a smooth function in the index determining selection into the treatment (i.e., birthday); failure to do so may yield spurious discontinuities in schooling and AFQT performance (Cook and Campbell, 1979).

between 1968 and 1970.[19] These respondents would have been between the ages of 15 and 19 during the summer of 1980, and therefore would have been relatively more likely to have been enrolled in school during the prior academic year. Otherwise, we limit the sample to respondents with non-missing information on day of birth, and born in states where school entry cutoff dates are set by the state, not local discretion (see Table 1). So that we have sample sizes sufficient to conduct the analysis separately by race, we use data from the supplemental NLSY sample. The resulting sample consists of 3691 observations.[20]

B. *Summary Statistics*

Table 2 presents the summary statistics of key variables for our sample, both overall and stratified by race. The table first presents means of schooling at the test date and several measures of AFQT performance (Panel A). These measures include the revised (1989) AFQT percentile score, the standardized version of this score (used in the regressions below), and the standardized scores on the four ASVAB components that comprise the AFQT. Overall, the sample was on average slightly over age 17 on July 1, 1980 and had on average completed 10.6 years of schooling when the test was administered. The average sample respondent also scored in the 44th percentile on the test, or about a one-quarter of a standard deviation below average for the population as a whole. This below average performance is due to the fact that test norms are based on the 1957 through 1962 birth

[19] Because the full 1971 academic cohort cannot be observed, as suggested by Figure 1, we omit individuals in this cohort from our analysis. In practice, we only drop individuals born at the end of 1964 as a result of this selection rule. It might in fact be appropriate to omit these individuals from the analysis, given evidence of "non-random self-selection at the edge of the age range" for the survey (Center for Human Resources Research, 2001; p. 17).

[20] Further details on sample selection and key variables are provided in Appendix B.

cohorts (Center for Human Resource Research, 2001), but our sample contains the youngest respondents in the NLSY.

The remaining columns of Panel A demonstrate the well-known differences in average AFQT performance across race. White respondents score on average in the 49th percentile, while black and Hispanic respondents have AFQT scores in the 20th and 30th percentiles, respectively. Interestingly, although the same age as white respondents on average, blacks and Hispanics have also completed less schooling at the time of the test. This difference might arise from differences across race in the incidence of grade retention or delayed school entry, or in dropout behavior.[21]

Regardless of race, academic cohort appears to have significant predictive power with respect to both completed schooling and test performance. Alongside the sample means for each variable are F-statistics testing for significant differences in means across academic cohorts.[22] For the sample overall, across-cohort differences are significant at the one percent level for schooling attainment and for all test score measures except the ASVAB component on math knowledge. Academic cohort appears to bear a strong relationship with other ASVAB components listed. We revisit these observations later in discussing the internal validity of our estimates.

The F-statistics also reveal some interesting differences by race. While academic cohort is a significant predictor of schooling attainment regardless of race, it appears to have a particularly strong relationship to test performance for blacks and a relatively weak

[21] Grade retention is likely to play the most important role here, though this cannot be confirmed with the available NLSY data. However, in other data sets, including several years of the October Current Population Survey School Enrollment, minorities tend to have higher rates of retention (Cascio, 2003).
[22] These means are not regression-adjusted for age, as will be the case in the analysis that follows.

relationship to AFQT performance for Hispanics. While this general pattern of results does not hold up when we adjust for age effects, it points to the potential for racial heterogeneity in results. Given this and the history of using the AFQT to understand race-specific differences in outcomes, we conduct the entire analysis separately by race. This stratification ends up being both interesting and important to interpretation, providing for an additional check of the internal validity of the research design.

Before moving on to this analysis, it is worth making several other observations from Table 2. First, even though it appears to be highly correlated with schooling and AFQT scores, academic cohort is for the most part uncorrelated with measures of family background.[23] For example, only in one case (maternal education for black respondents) do we find that the academic cohort indicators are statistically significant (Panel B). This suggests—though by no means confirms—that academic cohort might be a valid instrument for schooling at the test date.

Second, as anticipated, academic cohort is highly correlated with age at the time of the test, approximated as the respondent's age on July 1, 1980 (Panel B). This suggests that it will be very challenging to separate out the effects of age and schooling on test performance, though smooth functions in age are identifiable even in the presence of only one cutoff date. As discussed above, more unrestricted functions in age can be identified provided that there is variation in the cutoff dates to which the NLSY cohorts were exposed. The remaining panel (Panel C) summarizes this variation in cutoff dates. While 4th quarter cutoff dates were the most common for these cohorts, more than one-third of the sample

[23] Family background is measured as of age 14. Where missing, these family background characteristics have been imputed with race-specific means.

overall was subject to a cutoff date in the 1st or 3rd quarter of the year. Within each quarter, there is of course further variation in the cutoff dates, as shown in Table 1.

Finally, while we might expect the regression-adjusted first stage relationship to be fairly strong—91 percent of sample respondents were enrolled in school the year prior to the test—it might be weakened by misclassification of cutoff dates to respondents. Such misclassification would arise primarily from inter-state migration: as shown in Panel C, about 22 percent of respondents in the sample overall moved from their state of birth by age 14. To the extent that these moves occurred between states with different cutoff dates and before respondents entered school, some respondents may be assigned an incorrect academic cohort. We address this misclassification problem below by re-estimating the models for non-migrants and by re-assigning cutoff dates to individuals on the basis of state of residence at age 14. The latter change results in reclassification of less than three percent of respondents, though it does have some impact on the estimates.

V. The First-Stage: Academic Cohort and Schooling

A. Unrestricted Estimates

Before turning to our 2SLS estimates of the effect of schooling on the AFQT, it is useful to demonstrate that the relationship between school entry legislation and schooling remains strong in our specification and sample. To this end, Table 3 gives estimates of equation (5), the unrestricted first stage relationship between academic cohort indicators and schooling at the test date. The dummy for the youngest academic cohort (1970) is omitted for the purposes of identification. In all first-stage and subsequent regressions, we present standard errors that are consistent for pairwise or spatial correlation of errors across individuals from the same state.

The baseline specifications with no controls for age (columns (1) and (2)) align with expectations. While we can soundly reject perfect compliance in all specifications, the marginal effect of academic cohort appears to be roughly constant across cohorts, ranging in value between 0.8 and 0.95 years depending on race.[24] For example, in the specification with no controls (column (1)), white respondents who should have entered first grade in 1969 have on average 0.948 more years of schooling at the test date than their counterparts who should have entered first grade one year later. White respondents who should have entered first grade in 1968 have on average 1.81 one more years of schooling than those who should have entered in 1970. Coefficients for blacks and Hispanics (Panels B and C) are smaller in magnitude, perhaps due to higher rates of grade retention among these groups (or a higher dropout rate for Hispanics). As expected, these estimates are little affected by the inclusion of controls for family background and state of birth (column (2)).

The remaining specifications in Table 3 add various controls for approximate age at the test date (date of birth) to the model. There are several reasons to believe that age might have a direct effect on schooling for individuals in the same academic cohort. In particular, individuals who would be relatively young in their class (i.e., those born in the second half of the year) might be more likely to experience delayed school entry or grade retention (National Center for Education Statistics, 2000).[25] Relative to their older peers in the same

[24] As alluded above, perfect compliance would imply that $\mu_{1969} = 1$ and $\mu_{1968} = 2$.

[25] In the 1993 and 1995 National Household Education Surveys, between 11 and 13 percent of first and second graders born in the third and fourth quarters of the year experienced delayed entry into kindergarten, compared

cohort, these younger students might therefore tend to have lower levels of schooling on average when observed later in the school career.[26]

This prediction is borne out in the data, though the strength of the age-schooling relationship differs dramatically by race. Columns (3) and (4) present estimates of RD specifications for the first stage, with linear and quartic controls for age at the test date, respectively. For white respondents, including these age terms reduces coefficients on the academic cohort indicators by more than 50 percent, though the instruments remain strong predictors of schooling. For minorities, within-cohort age effects are not as strong and consequently, the impacts of including age in the model not as dramatic. For blacks and Hispanics, academic cohort continues to have a large and highly significant impact on schooling at the test date.

The next column (5) presents estimates of the DD specification for the first stage model, where age is controlled for with an interaction of dummy variables for month of birth and year of birth. These specifications therefore allow the standard instrument for schooling—season of birth—to have a direct effect on schooling attainment. Season of birth might have a direct effect on schooling through the grade retention and delayed entry avenues described above, as well as through its correlation with other unobservable characteristics (e.g., related to family background or socioeconomic status). When these less restrictive functions in age are included in the model, the academic cohort indicators give the

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to only 5 to 6 percent of children born earlier in the year. Children born in the second half of the year were also more likely to have been retained in kindergarten (National Center for Education Statistics, 2000).

[26] We are not controlling for "relative age" in this analysis. Given concentration of cutoff dates in the last four months of the calendar year, however, our controls for date of birth may be picking up some of these relative age effects.

average effect of school entry legislation on schooling attainment for individuals born in the same month and year.

As above, estimates from this specification differ considerably by race. The effects of academic cohort on schooling are not much changed for blacks and Hispanics by in the DD specification, though estimates become less precise. For blacks, the first-stage remains quite strong (F-stat=15.4), but the loss in precision has a greater impact on Hispanics (F-stat=3.1). For whites, the point estimates are further diminished in magnitude and the predictive power of academic cohort considerably weakened: the F-stat on joint significance of the instruments falls to 1.65.

This weakening of the first-stage relationship can be attributed in part to misclassification, as demonstrated in the remaining columns of the table. Column (6) re-estimates the DD model for the subsample of respondents that have not moved from their birth state by age 14. This sample therefore discards the respondents for whom school entry cutoff dates are likely to be incorrectly assigned. The first-stage relationship for whites regains some strength as a result of this restriction: the coefficients rise in magnitude and become more precisely estimated, and the F-stat on the instruments rises to 5. A similar result occurs for Hispanics, who also have a relatively high migration rate. Column (7) then reassigns re-assigns cutoff dates to individuals on the basis of state of residence at age 14. These changes result in marginal improvements in the strength of the first-stage for whites and Hispanics, though a slight weakening of the relationship for blacks.

While misclassification might still be playing a role in these estimates, there are several alternative explanations for the observed differences across race in the effects of school entry legislation. First, there may be differences across race in the extent of noncompliance with school entry legislation. While we cannot directly observe age at school entry in the NLSY, data for more recent cohorts has revealed relatively high rates of delayed kindergarten entry among white and Hispanic children (National Center for Education Statistics, 2000). Given that the same data also shows a strong age gradient in delay, we might therefore expect to uncover stronger age effects for schooling among these groups. Similarly, although grade retention rates of blacks and Hispanics are higher than those of whites, age-based grade retention decisions might be more common in the schools that white children attend. Unfortunately, the NLSY also does not record any retrospective information on grade retention, so we cannot easily investigate this explanation.

B. Restricted Estimates

To make the most of the existing entry legislation-based variation in schooling, we now turn to the more parsimonious specification for the first stage. Table 4 presents estimates by race of equation (5'), along with its reduced-form counterpart—the relationship between academic cohort and AFQT performance (discussed in the next section). So that the instrument can be more readily interpreted, we replace C_{is} with a linear term in academic cohort. Because these terms are perfectly collinear ($\tilde{C}_{is} = 1971 - C_{is}$), this otherwise has no impact on the model.

In this alternative specification, first-stage coefficients are indeed more precisely estimated, and the average effects of school entry legislation more readily seen. In the fully-saturated DD specifications for the first stage (columns (5) and (7)), a one-year increase in academic cohort is estimated to reduce completed schooling at the test date for whites by on average 0.22 years. The effects are stronger for minorities: with every one-year increase in academic cohort, blacks experience on average a 0.6 to 0.75 year reduction in completed schooling, while Hispanics experience an average 0.4 to 0.45 year reduction. As discussed above, it is through these otherwise unanticipated "breaks" in schooling attainment that we hope to uncover an effect of schooling on the AFQT, should one exist.

VI. The Effect of Schooling on the AFQT

A. OLS and 2SLS Estimates

Table 5 gives the 2SLS estimates of the effect of schooling on the AFQT for each of the samples and specifications given above. Since we reject the restrictions equation (5') places on equation (5) in only a few instances, we base our 2SLS estimates on the former, restricted version of the first-stage relationship.[27] Our model is therefore just identified. For the purposes of comparison, the table also presents ordinary least squares (OLS) estimates. There is a strong cross-sectional relationship between schooling and the AFQT: OLS estimates of the effect of schooling on the AFQT are quite stable, ranging between 0.3 (for blacks and Hispanics) to 0.4 standard deviations (for whites).

Regardless of race, the baseline 2SLS estimates (which exclude controls for age) are significantly lower than their OLS counterparts. For whites and Hispanics, estimated effects of schooling are only about one-third of their previous magnitudes (0.12 and 0.10, respectively), and for blacks, an additional year of schooling is associated with only a 0.15 standard deviation increase in AFQT performance. As expected, controls for family

[27] F-statistics on the model restriction are suppressed due to space constraints, but are available from the authors upon request. 2SLS estimates where academic cohort indicators are used as instruments are similar, and are also available from the authors upon request.

background and state of birth (column (2)) have little effect on these point estimates. This is in sharp contrast to the corresponding OLS estimates, where the addition of family background covariates reduces coefficients by between 10 and 20 percent. If anything, adding these controls in 2SLS serves only to absorb residual variation in the AFQT, improving the precision of the estimates.

While the choice of functional form for the age effects has little impact on OLS estimates, 2SLS estimates of the effect of schooling on the AFQT are highly sensitive to the way in which age enters the model. The first two specifications control for the smooth functions in age: a linear term (column (3)) and a quartic term (column (4)), corresponding to the RD specification of the first stage. For the white sample, these smooth age functions in age completely eliminate the schooling effect, with estimates becoming negative and very imprecisely estimated. For minorities, however, estimates of the effect of schooling remain stable (blacks) or increase in magnitude (Hispanics). Regardless of race, coefficients in these specifications are very imprecisely estimated.

The final specifications control for age non-parametrically, with a series of interactions between month of birth and year of birth dummy variables. As was the case with the first-stage, this specification yields considerably different findings by race. For whites, the effect of schooling becomes both more negative and even less precisely estimated (column (5)), even in the subsamples where the first-stage is stronger (columns (6) and (7)). The estimates are so imprecise that we cannot rule out large positive or even more negative effects of schooling on the AFQT. Returning to Table 4, we see the root of the problem: academic cohort has very little predictive power with regard to AFQT performance for whites, with F-statistics on instrument ranging between 0 and 1. As discussed further in the next section, these findings could result from the relatively small and highly non-random nature of the subpopulation for which effects for whites are being identified.

For blacks and Hispanics, by contrast, the DD specification for the first-stage yields effects of schooling on the AFQT that are both marginally significant and with the expected sign. When cutoff dates are assigned on the basis of birth state (column (5)), an additional year of schooling is estimated to raise the AFQT performance of blacks and Hispanics about 0.35 standard deviations. When migrants are excluded from the sample (column (6)) and cutoff dates assigned on the basis of state of residence (column (7)), the 2SLS estimate for blacks falls to about two-thirds of its previous value (to 0.23) and is no longer statistically distinguishable from zero, while the 2SLS estimate of the school effect for Hispanics nearly doubles (to about 0.58) and is highly significant. The differences in these estimates point to the importance of determining which cutoff assignment strategy is likely to result in the least misclassification, an exercise that we leave to future work.[28]

B. Comparison to Results Using the Quarter of Birth Instrument

While somewhat dependent on how we classify cutoff dates to respondents, the results presented so far suggest some effect of schooling on the AFQT, particularly for minorities. How do these results compare to those that come from the standard quarter-of-birth instrument for schooling, which is ostensibly based on the same source of policy variation? Before turning to a discussion of the internal validity of our research design, it is

[28] In a future draft of the paper, we will use the 1970 and 1980 Decennial Censuses to determine which cutoff assignment strategy is likely to have the lowest misclassification rate, given timing of inter-state migration for the cohorts under consideration.

useful to address this question, particularly since Neal and Johnson (1996) employ this alternative identification strategy.

Table 6 compares our 2SLS estimates of the effect of schooling on the AFQT to two alternative 2SLS estimates based on quarter of birth. The first two columns give our preferred 2SLS estimates, based on the DD specification for the first stage, with the full set of controls and linear academic cohort instrument.[29] The remaining columns present 2SLS estimates based on dummies for quarter of birth (columns (3) and (4)) and dummies for quarter of birth interacted with dummies for state of birth (columns (5) and (6)). Both models include year of birth dummies as a crude control for age and, where indicated, a series of state dummies.

These estimates are dramatically different from those presented thus far. Consider the 2SLS estimates based on quarter of birth dummies, the model considered by Neal and Johnson (1996) for a similar sample of NLSY respondents.[30] Though Neal and Johnson (1996) do not present separate results by race, their results appear to correspond most closely to the specification in column (3), which does not include controls for family background or state of birth.[31] In this specification, an additional year of schooling is estimated to increase AFQT performance by 0.2 standard deviations for whites, 0.38 standard deviations for blacks, and 0.58 standard deviations for Hispanics. When controls for family background and birth state are included in the model, these effects fall to 0.11

[29] These results are repeated from columns (5) and (7) of Table 5.
[30] Neal and Johnson (1996) use a sample of respondents from the 1962 to 1964 birth cohorts of the NLSY. Our sample drops some of the 1964 birth cohort, and uses some of the 1961 cohort.
[31] Neal and Johnson (1996) report an effect of schooling on the AFQT of 0.22 standard deviations for men and of 0.25 standard deviations for women. For the specification in column (3), we estimate an effect of 0.235 for the pooled sample (including race fixed effects), when limited to respondents born after 1961 (n=3501). When we use our entire sample, we estimate an effect of 0.219 for the pooled sample.

standard deviations for whites and to approximately 0.3 standard deviations for both blacks and Hispanics. Only for blacks can the effect of schooling be distinguished from zero.

The remaining columns employ an instrument closer to that used by Angrist and Krueger (1991). This specification allows the data to detect differences in quarter-of-birth patterns in completed schooling that might arise across states due to differences in school entry cutoff dates.[32] At the same time, however, this approach derives identification in part from further season-of-birth variation that may not be excludable from the model of test scores. This model generates estimates that are even more different from ours, particularly for whites and Hispanics: for whites, an additional year of schooling is estimated to raise AFQT scores by a significant 0.22 standard deviations, while for Hispanics, this effect is a marginally significant 0.19 standard deviations.

The differences in results across specification can be traced, at least in part, to a strong direct effect of "exact age" on test scores. The data strongly reject the exclusion restriction in the quarter-of-birth models, as evidenced by F-statistics on the year of birth/ month of birth interactions in the models presented in this paper (see columns (1) and (2)). All of F-statistics are extremely large in magnitude (in all cases over 100), suggesting that age or season-of-birth (or both) has an highly significant independent effect on test performance.

While we remain agnostic on the reasons for this effect, this finding suggests that it is indeed inappropriate to instrument for schooling using quarter of birth, at least in this application. Further, our approach is more successful in uncovering entry-legislation based

[32] Angrist and Krueger (1991) also use the interaction of quarter of birth with year of birth as an instrument. We do not include the interaction with year of birth since school entry laws for the cohorts in this study were constant over time.

variation in schooling in the groups for which it should theoretically be the strongest. For example, the quarter of birth instrument suggests a very weak relationship between school entry legislation completed schooling for Hispanics, with F-stats on the instruments in the first-stage regression (coefficients not reported) between 1 and 2. Given the relatively high dropout rate of this sample, we might expect a relatively strong first-stage. On the other hand, quarter of birth suggests a particularly strong relationship between entry legislation and schooling for whites, the sample for which selective delay and age-based retention could be the strongest. By contrast, with our identification strategy, the opposite is true: the first-stage is relatively strong (and large in magnitude) for Hispanics and relatively weak (and small in magnitude) for whites.

C. Internal Validity: Estimates by ASVAB Component

The above comparisons have lent some credibility to our identification strategy. However, because our estimates are not particularly precise and vary somewhat according to how cutoff dates are classified, we now consider a test of the internal validity. Specifically, if schooling has an impact on the AFQT, our instrument should uncover larger effects of schooling on ASVAB components that are more "academic," or which test subjects more likely to be taught in school. To bolster our argument, we consider all ten ASVAB components, not only those used to calculate the AFQT.

Before turning to these estimates, it is useful to classify these tests by the strength of their academic component. While neither the military nor the NLSY provide such a classification, we have gained some insight into these individual tests from test preparation guides prepared by Kaplan and Princeton Review.[33] From our cursory read of these exam preparation materials, including practice exams, we have classified the ASVAB tests into three categories: academic tests (word knowledge, paragraph comprehension, math knowledge, arithmetic reasoning, general science, and mechanical comprehension), vocational tests (auto and shop information and electronics information), and tests of speed (coding speed, numerical operations).

This classification is crude, so deserves more explanation. Among what we consider to be academic tests are those included in the AFQT. Two of these tests (paragraph comprehension and arithmetic reasoning) are essentially tests of basic literacy, and so might be less affected by an additional year of high school.[34] The other two tests on the AFQT on potentially more amenable to change from a high school education. For example, the test of word knowledge is a test of vocabulary, requiring test-takers to find synonyms for words used in short sentences or phrases. The math knowledge component tests knowledge of basic algebra and geometry—subjects for many not learned until high school. Two additional ASVAB components—general science (a test of elementary biology and chemistry) and mechanical comprehension (a test of basic physics)—we also classify as academic. Auto and shop information and electronics information are more vocational, perhaps more easily learned "hands-on" or on-the-job. The tests of speed involve matching numbers to words from a key (coding speed) and simple arithmetic problems (numerical operations), and so may or may not be affected by more schooling.[35]

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[33] We thank David Neumark for suggesting that we consult these test preparation materials.

[34] Paragraph comprehension tests understanding of short written passages, and arithmetic reasoning involves word problems that require using of addition, subtraction, multiplication, and division.

[35] Our classification lines up fairly well with previous classifications used in the literature. For example, Blackburn and Neumark (1993) and Blackburn and Neumark (1995) classify ASVAB tests into two categories:

This classification matches up somewhat well with 2SLS estimates of the effect of schooling, presented in Table 7. Estimates presented are those from the fully saturated DD model for the first-stage, where age is controlled for in the most unrestricted possible way. Regardless of race or whether individuals are classified using state of birth or state of residence, schooling appears to have the largest effects on tests of speed. While this is somewhat surprising, schooling also appears to have relatively large effects on academic tests. For example, as seen in column (6) (which corresponds to the estimates from column (7) of Panel C in Table 5), Hispanics experience relatively large gains in performance on academic tests (improvements on the order of 0.5 standard deviations for an additional year of schooling), with the largest effects of schooling for perhaps the most academic tests—math knowledge, word knowledge, general science, and mechanical comprehension. For blacks, effects of schooling on academic and vocational tests appear to be quite similar.

These results are further bolstered with evidence on the types of programs or "tracks" that are common by race. For example, blacks in our same are more likely to be enrolled in "vocational" programs than Hispanics (10.65 percent of blacks versus 8.85 percent of Hispanics). Blacks are also slightly more likely than Hispanics to be enrolled in college preparatory courses (30.88 percent versus 29.94 percent) and less likely to be on a "general education" track (56.77 percent versus 58.9 percent). Together, these tabulations suggest that Hispanics might show relatively large effects of schooling on the academic tests—assuming that some blacks learn the same material at a slightly younger age—and that blacks might be expected to show relatively large effects of schooling on vocational tests.

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academic and non-academic. There are only two differences in our classification schemes: we classify mechanical comprehension as an academic test, and we have a separate classification for tests of speed.

VII. Discussion

On balance, our results suggest that there could be a relatively large impact of schooling on the AFQT for minorities, but no discernible effect of schooling for whites. Our test of internal validity has been fruitful, revealing larger impacts of schooling on ASVAB components that test academic subjects, such as algebra and vocabulary. Further, the differences that do exist seem consistent with differences across race in the types of programs in which individuals are enrolled. While our identification strategy is alone relatively unrestrictive, this test of internal validity has provided further support for the notion that we have uncovered a causal impact of schooling on the AFQT.

Thus, these results suggest that the AFQT is a measure of achievement, not of ability. If this is so, however, is it reasonable that we have found such different effects across race? In particular, our finding of no effect of schooling on the AFQT scores of whites might appear inconsistent with the achievement interpretation of the test: given that the average white student attends a school with more resources, should we not expect the marginal effect of schooling to be higher for these students? However compelling this argument might seem, it might in fact be reasonable to find no effect for whites. As mentioned earlier, the AFQT is a test of cumulative achievement, not a test of material learned from any given year of school. Given the above description of the ASVAB tests, it seems quite possible that the average white student may have been exposed at a younger age to the material tested on the AFQT.[36]

[36] This is supported by evidence on curriculum: whites in our sample are also more likely than minorities to be enrolled in college preparatory courses and considerably less likely to be on a "general education" track.

Given that our point estimates for whites are negative, however, it is useful to consider several other factors that could be biasing our estimates. Below, we briefly discuss two such factors: differences across race in the subpopulation for which schooling effects have been identified and censoring of test scores.

A. Who is the Marginal Student?

As demonstrated by Figures 1 to 4, there is a considerable and perhaps surprising amount of variation in completed schooling among young individuals of the same age. Some individuals entered first grade earlier or later than they should have given their birth date and the relevant cutoff. Others were been retained in grade, or permitted to skip ahead. Consequently, our 2SLS estimates will essentially be uncovering the effect of schooling on the AFQT for a school entry law complier that did not repeat or skip grades—a modal, though non-random, student in our sample (Imbens and Angrist, 1994; Angrist, Imbens, and Rubin, 1996).

In light of this, our first-stage results suggest that there could be large differences across race in the representativeness of these complying subpopulations. For whites, the estimated effect of academic cohort on schooling is small—on the order of 0.2 years—suggesting that a relatively small fraction of white respondents comply with the legislation. If this group is highly non-representative (or "relative age" effects strong) we are not necessarily identifying anything close to the average treatment effect for this population. For blacks and Hispanics, by contrast, the effect of academic cohort is relatively large independent of the effects of age, suggesting that the subpopulations for which effects are identified are somewhat more representative.

B. Are "Ceiling Effects" Affecting the Estimates?

In addition to the fact that we are not identifying average treatment effects for the overall population, our results may be biased by censoring on the dependent variable. In particular, although an additional year of schooling may raise the *latent* skill of all individuals sampled, this gain in skill will not be revealed to the extent that an individual would have otherwise scored close to (or at) the maximum. Given that whites have much higher average test scores than minorities (Table 2), we might expect this to be more of a problem for this group.

While whites are more likely to have maximum test scores, it is still not particularly common. Only 1.7 percent of the white sample, and none of the black or Hispanic samples, scored in the 99th percentile on the AFQT. Of course, maximum test scores are more common on the individual tests that comprise the AFQT. Among whites, approximately 13 percent score at the maximum on at least one of the four ASVAB tests. Corresponding figures for blacks and Hispanics are 2.3 and 3 percent, respectively. Among those respondents who score at the max on at least one test, it is, furthermore, most frequent to score at the max on *only* one test.[37]

To further investigate the censoring problem, we have re-estimated the reduced-form regressions using tobit. This exercise had very little impact on the effects of academic cohort on AFQT performance. If anything, implied indirect least squares estimates of the effect of schooling on test scores became slightly more negative for whites relative to 2SLS.

[37] Hansen, Heckman, and Mullen (2003) document higher fractions of respondents at the max, though their sample includes older respondents in the NLSY.

All in all, these results suggest that our estimates for whites are not biased downward due to censoring.

VIII. Conclusion

Given its availability for a nationally representative sample, the AFQT has been widely used by researchers as a measure of skills that would otherwise be unobservable. Whether these skilled are largely innate or learned—and therefore amenable to investments in human capital—is a question that should be critical to interpreting results in such studies, as well as to determining whether the AFQT is in fact appropriate for any given application.

In this paper, we have performed a relatively rigorous test of the ability interpretation of the AFQT. Building on the work of Neal and Johnson (1996), Winship and Korenman (1997), and Hansen, Heckman, and Mullen (2003), we have presented new evidence on the impact of schooling on the AFQT for the youngest respondents of in the NLSY 79. To identify the effect of schooling, we made use of a variation arising from school entry legislation. Using information on exact birth dates, and matching school entry to individuals on the basis of likely state of residence at age six, we identify the "academic cohorts" of each NLSY respondent. Exploiting variation in cutoff dates across states, we then use academic cohort as an instrument for schooling in a model that allows for fairly unrestricted direct effects of age on test performance.

Our results provide evidence in support of an achievement interpretation for the AFQT, adding to a broader literature on the effects of schooling on tests purported to measure IQ (Ceci, 1991). We find that an additional year of high school has large effects on the AFQT performance of blacks and Hispanics. Our 2SLS estimates for minorities—in the neighborhood of a 0.35 standard deviation increase in scores with every additional year of schooling—are in fact larger than previous estimates in the literature for pooled samples (Neal and Johnson, 1996; Winship and Korenman, 1997; Hansen, Heckman, and Mullen, 2003). However, we find no evidence of an effect for white students. Like our finding of a stronger schooling gradient in the more academic tests of the ASVAB, these race differences implicitly demonstrate the validity of our research design: on the margin, schooling should matter more for individuals being exposed to relevant material in the classroom.

It is important to note that these results should in no way be construed as precluding earlier human capital investments from having an impact on AFQT performance, or more broadly, on skills that are rewarded in the labor market. In fact, we believe that a crucial question for future research concerns these earlier investments, e.g., which years of schooling matter most for building skill?[38] Understanding the answer to this question is crucial for interpreting tests such as the AFQT, as well as to gaining insight into the optimal timing of investments in human capital.

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[38] In future work, we hope to apply a similar identification strategy to estimate the effects of schooling on the AFQT for the more recent cohorts in the NLSY 97, who were between the ages of 13 and 17 when tested.

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Appendix A. Coding of School Entry Ages

This paper required knowledge of exact school entry cutoff dates facing several cohorts in the NLSY. We derived dates listed in the Table 1 from archival work based on the histories of current age at school entry statutes and our knowledge of the first grade entry ages in 1955, given in Angrist and Krueger (1992). More specifically, if the current statute's history did not indicate any change in the school entry law over 1955 to 1970, the date recorded pertains to that effective in 1955. On the other hand, if the statute's history indicated a change in the statute over the period, we investigated relevant state session laws over this period. If the state changed its cutoff date, and this cutoff date affected NLSY cohorts, we record the dates in effect in 1968, 1969, and 1970. We later discovered cutoff dates for 1965 and 1972 for a subset of states in the *Digest of Education Statistics*; dates that we recorded are largely consistent with those recorded from this source.

Appendix B. The NLSY Sample

	Number of observations dropped	Total
Number of NLSY 79 respondents (Base year Survey)		12686
Drop if:		
Did not take AFQT/ASVAB	772	11914
Cannot impute schooling for summer 1980	24	11890
Not born in U.S.	778	11112
Born in U.S. or in Puerto Rico or outlying area	130	10982
State of birth missing, state of residence at age 14 missing	3	10979
State of birth missing, state of residence at age 14 is Puerto Rico or territories	0	10979
State of birth missing, have moved since birth	35	10944
AFQT/ASVAB completed under altered test conditions	21	10923
Missing day of birth	15	10908
States where cutoff is at discretion of LEA (GA, IN, MA, WA)	985	9923
1962 - 67 academic cohorts	6178	3745
1971 academic cohort	54	3691
Size of sample used in this study		3691

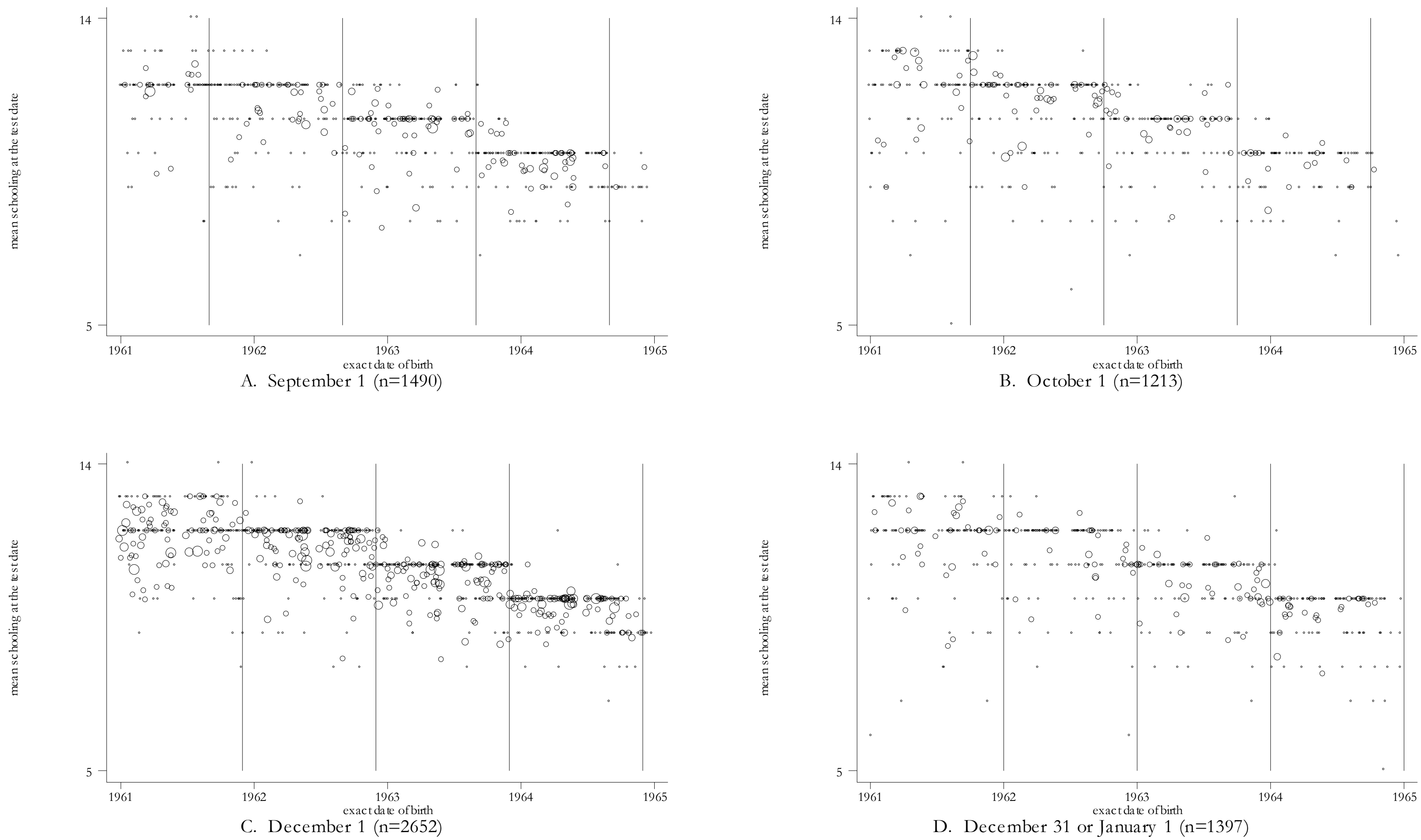

Figure 1: Mean Schooling at the Test Date, by Cutoff Date

Notes: Samples are from the NLSY 79 and consist of all individuals that took the AFQT, and for which schooling at the test date and exact day of birth are observed. Individuals are assigned cutoff dates on the basis of state of birth. Sample includes respondents that have migrated from their state of birth. Point sizes represent the number of observations used to calculate the average. States are classified by cutoff dates in Table 1.

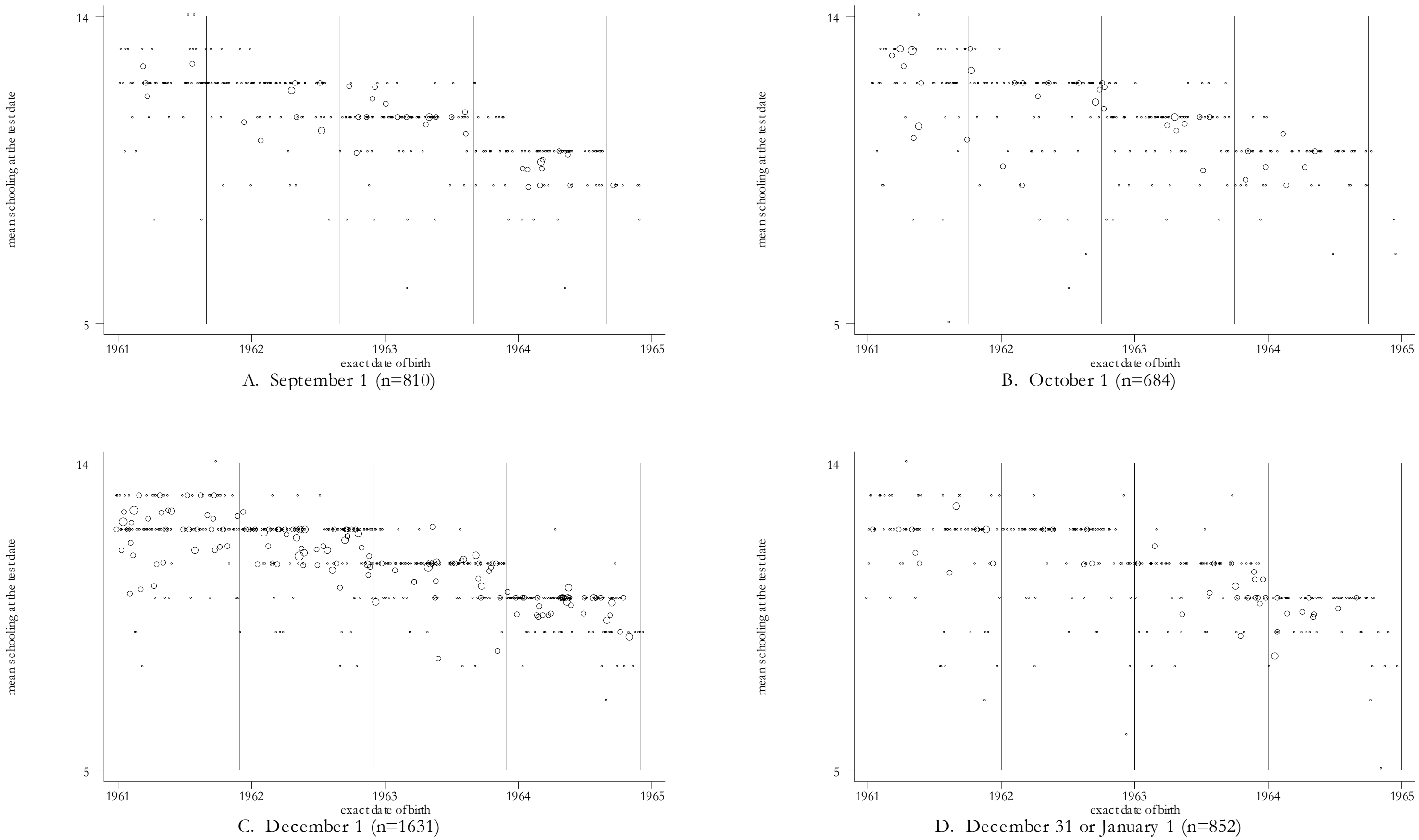

Figure 2: Mean Schooling at the Test Date, by Cutoff: White Sample

Notes: See Figure 1.

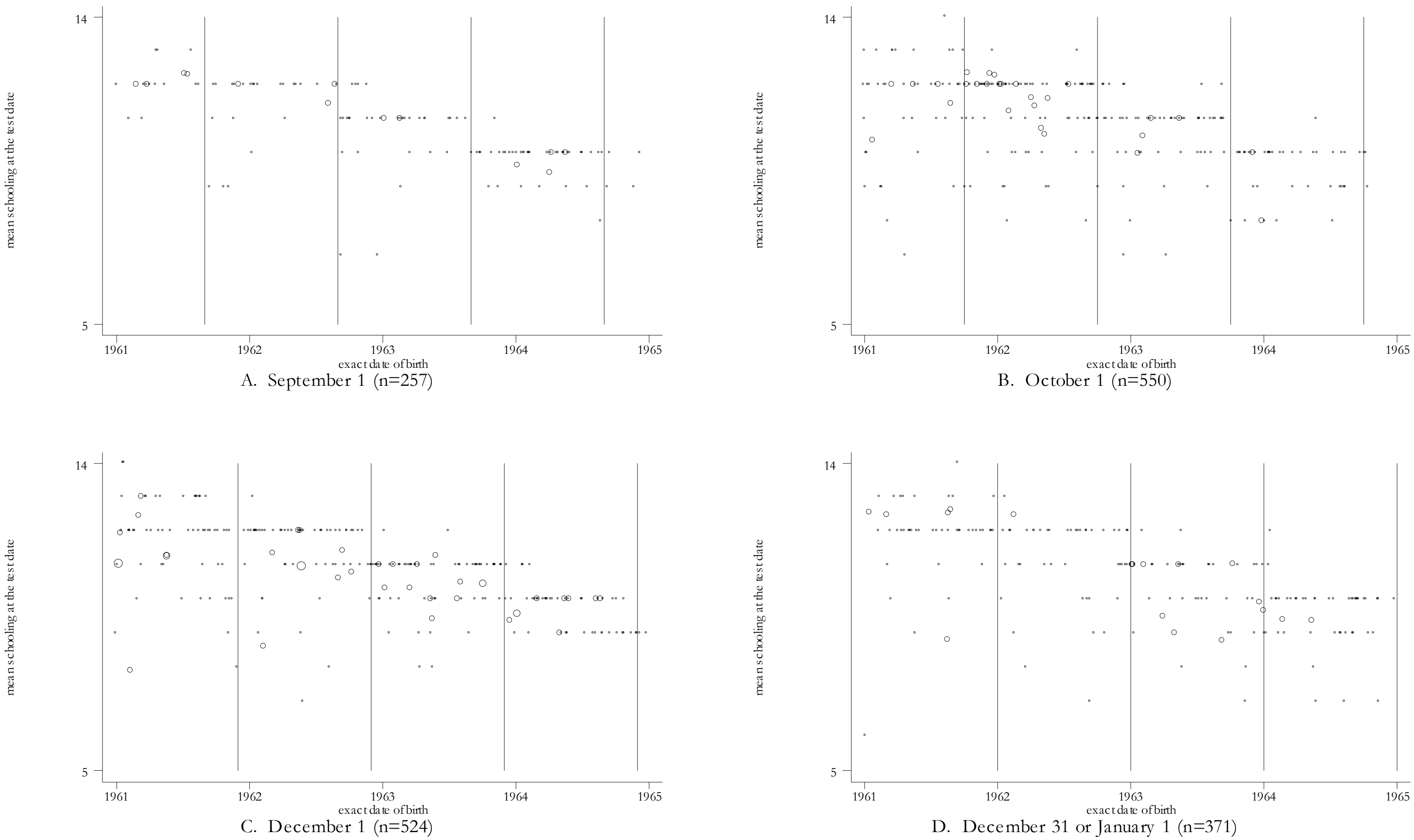

Figure 3: Mean Schooling at the Test Date, by Cutoff: Black Sample

Notes: See Figure 1.

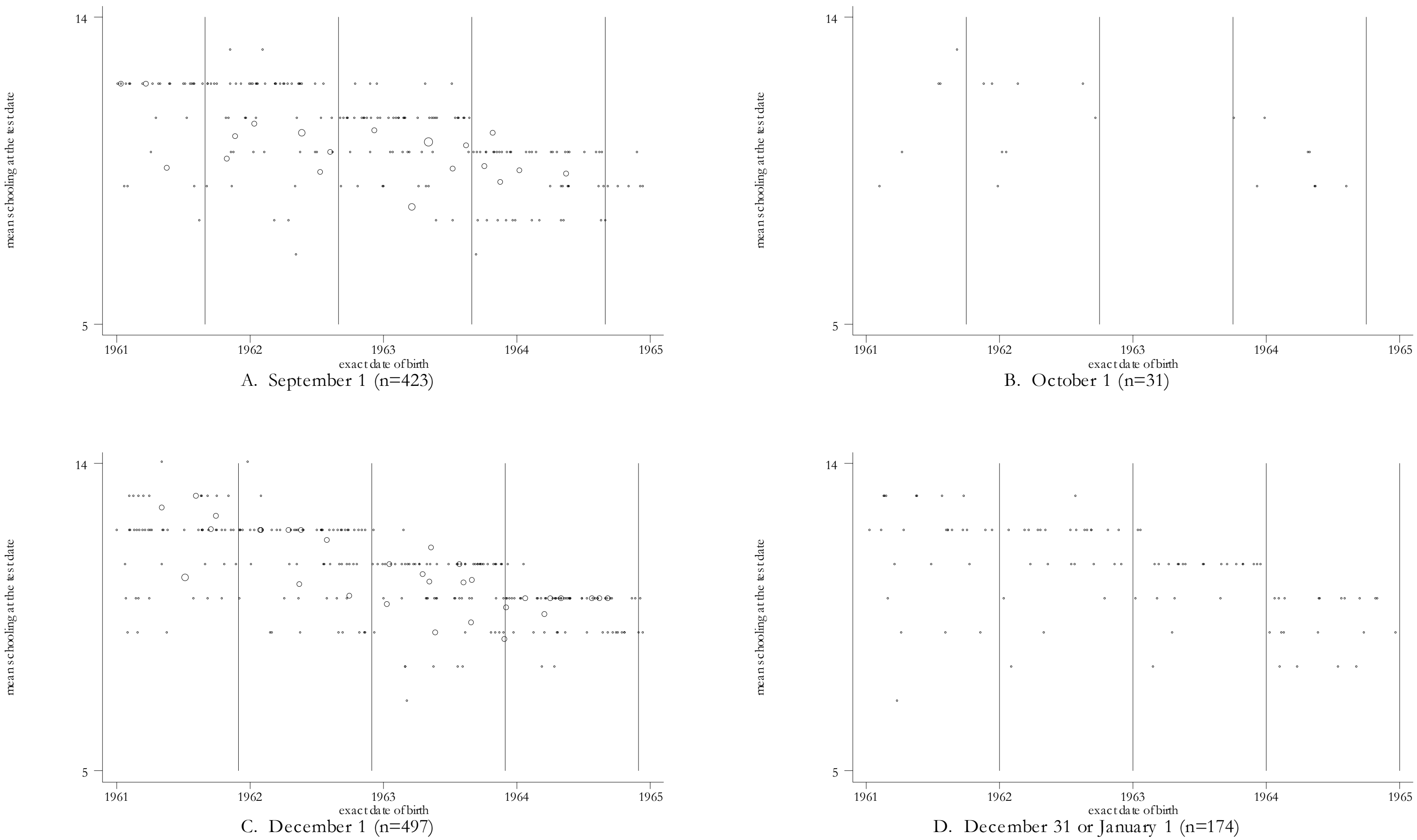

Figure 4: Mean Schooling at the Test Date, by Cutoff: Hisp. Sample

Notes: See Figure 1.

TABLE 1 - CLASSIFICATION OF STATES BY MINIMUM AGE AT SCHOOL ENTRY LAWS, 1968-1970

	States
Third Quarter Cutoffs	
September 1	CO, $DE_{(68)}$, KS, MD, MN, MT, TX
September 2	UT
September 13	NH
September 15	IA, WY
September 30	MO, TN, VA
Fourth Quarter Cutoffs	
October 1	AL, AR, NJ, $NC_{(68,69)}$
October 15	ME, NE, $NC_{(70)}$
October 16	ID
October 31	ND, OH, SD
November 1	OK, SC, WV
November 2	AK
November 15	OR
December 1	CA, IL, MI, NY
December 31	KY, LA, HI, NV, RI, WI
First Quarter Cutoffs	
January 1	AZ, CT, $DE_{(69,70)}$,FL, MS, NM, VT
February 1	PA
At Discretion of Local Education Agency	
	GA, IN, MA, WA

Notes: A child is permitted to begin first grade in a given academic year if that child reaches the age of six by the date specified. Dates are relevant for the youngest of the NLSY cohorts, eligible to enter first grade between the fall of 1968 and the fall of 1970. See Appendix A and text for more details.

TABLE 2 - DESCRIPTIVE STATISTICS FOR THE NLSY SAMPLE

	All		White		Black		Hispanic	
	Mean (S.D.)	F-Stat	Mean (S.D.)	F-Stat	Mean (S.D.)	F-Stat	Mean (S.D.)	F-Stat
A. Schooling and AFQT Measures:								
Schooling, summer of 1980	10.62	1305 **	10.67	833 **	10.47	296.2 *	10.32	91.5 *
	(1.14)		(1.12)		(1.17)		(1.26)	
AFQT Revised Score (Percentile)	43.67	14.8 **	48.69	8.1 **	20.50	11.7 **	30.19	3.4 *
	(27.81)		(26.99)		(18.81)		(23.96)	
AFQT Revised Score (Standardized)	-0.26	16.4 **	-0.06	9.4 **	-1.15	11.1 **	-0.75	3.0
	(1.00)		(0.94)		(0.82)		(0.93)	
ASVAB Arithmetic Reasoning (Standardized)	-0.21	10.3 **	-0.04	5.5 **	-0.99	6.9 **	-0.64	2.1
	(0.96)		(0.93)		(0.64)		(0.80)	
ASVAB Word Knowledge (Standardized)	-0.33	24.3 **	-0.14	16.9 **	-1.23	8.7 **	-0.79	2.5
	(1.00)		(0.90)		(0.98)		(1.02)	
ASVAB Paragraph Comprehension (Standardized)	-0.26	19.8 **	-0.08	11.3 **	-1.09	12.9 **	-0.71	3.4 *
	(1.08)		(1.00)		(1.04)		(1.06)	
ASVAB Math Knowledge (Standardized)	-0.08	2.7	0.06	0.8	-0.72	6.4 **	-0.49	1.9
	(0.99)		(0.99)		(0.71)		(0.87)	
B. Background Variables:								
Age at Time of the Test	17.14	12550 **	17.14	7161 **	17.14	3631 **	17.11	1810 **
	(0.87)		(0.86)		(0.88)		(0.87)	
Dad's HGC	11.86	0.6	12.24	0.3	10.57	2.6	9.60	0.4
	(3.24)		(3.12)		(2.91)		(3.91)	
Mom's HGC	11.677	0.2	11.975	0.0	11.037	4.0 *	8.938	0.3
	(2.51)		(2.28)		(2.44)		(3.76)	
Number of Children in Family	3.24	2.5	2.97	2.9	4.42	0.3	4.15	2.3
	(2.23)		(1.93)		(2.92)		(2.91)	
C. School Entry Variables:								
1st Quarter Cutoff	0.12	3.1	0.11	2.8	0.13	0.5	0.11	0.0
3rd Quarter Cutoff	0.25	1.7	0.25	1.0	0.21	1.5	0.37	0.3
4th Quarter Cutoff	0.63	0.9	0.64	0.5	0.66	2.0	0.52	0.4
Migrated from State of Birth	0.22	3.4 *	0.23	3.4 *	0.18	1.4	0.21	0.3
Enrolled in School, 1979-80	0.91	28.4	0.91	21.6	0.91	32.2	0.87	19.9
N	3691		2116		1028		547	

Notes: See text and Appendix B for description of sample selection. F-statistics test the joint significance of the academic cohort indicators in a regression where the dependent variable is the variable specified. * Significant at the 0.05 level. ** Significant at the 0.01 level.

TABLE 3 - UNRESTRICTED FIRST STAGE REGRESSIONS

	Dependent Variable: Schooling, 1980						
	Cutoff Assigned using State of Birth						Cutoff Assigned using State of Residence
	Full Sample					Non-movers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Non-Hispanic White Subsample							
Enter First Grade 1969	0.948	0.96	0.457	0.5	0.162	0.304	0.226
	(0.039)	(0.042)	(0.105)	(0.128)	(0.144)	(0.117)	(0.122)
Enter First Grade 1968	1.818	1.827	0.811	0.826	0.45	0.741	0.446
	(0.035)	(0.035)	(0.176)	(0.170)	(0.260)	(0.242)	(0.228)
R^2	0.44	0.51	0.53	0.53	0.55	0.56	0.55
F-Stat (instruments)	1334	1447	10.7	13.0	1.7	5.0	2.2
N	2116	2116	2116	2116	2116	1640	2110
B. Black Subsample							
Enter First Grade 1969	0.858	0.9	0.723	0.694	0.657	0.600	0.435
	(0.059)	(0.064)	(0.132)	(0.171)	(0.177)	(0.189)	(0.181)
Enter First Grade 1968	1.726	1.779	1.43	1.41	1.522	1.442	1.197
	(0.087)	(0.086)	(0.253)	(0.267)	(0.274)	(0.350)	(0.383)
R^2	0.37	0.44	0.44	0.44	0.46	0.44	0.43
F-Stat (instruments)	206	220	16.4	14.0	15.4	8.5	4.9
N	1028	1028	1028	1028	1028	843	1017
C. Hispanic Subsample							
Enter First Grade 1969	0.888	0.876	0.588	0.569	0.58	0.718	0.493
	(0.124)	(0.126)	(0.151)	(0.157)	(0.232)	(0.228)	(0.198)
Enter First Grade 1968	1.525	1.556	0.966	0.857	0.803	1.213	0.888
	(0.126)	(0.088)	(0.231)	(0.253)	(0.393)	(0.493)	(0.345)
R^2	0.25	0.41	0.41	0.41	0.46	0.46	0.45
F-Stat (instruments)	133	234	11.3	12.4	3.1	5.0	3.5
N	547	547	547	547	547	430	527
Type of Age Control†	N	N	L	Q	M x Yr	M x Yr	M x Yr
Family Background	N	Y	Y	Y	Y	Y	Y
State dummies	N	Y	Y	Y	Y	Y	Y

Notes: The dependent variable is schooling completed as of the summer of 1980. See text for more details. Family background controls include linear terms in mother's education, father's education, and number of siblings (all measured as of 1979). Indicators for imputed family background are also included in the regressions. Columns (2) through (6) include state of birth dummies, while column (7) includes dummy variables for state of residence at age 14. All regressions are weighted by AFQT sampling weights, and standard errors are robust to arbitrary residual correlation for individuals born in the same state. †Age controls: N=none, L=linear in exact age (as 1 July 1980), Q=quartic in exact age (as of 1 July 1980),M=month of birth dummies, Yr=year of birth dummies, M x Yr = interaction.

TABLE 4 - RESTRICTED FIRST-STAGE AND REDUCED FORM REGRESSIONS

	Cutoff Assigned using State of Birth						Cutoff Assigned using State of Residence
	Full Sample					Non-movers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Non-Hispanic White Subsample							
First Stage: Schooling, 1980							
Academic Cohort	-0.909	-0.914	-0.407	-0.414	-0.222	-0.37	-0.223
	(0.018)	(0.018)	(0.088)	(0.085)	(0.131)	(0.122)	(0.113)
F-Stat (instruments)	2550.3	2578.4	21.4	23.7	2.9	9.2	3.9
R^2	0.44	0.51	0.53	0.53	0.55	0.56	0.55
Reduced Form: AFQT (standardized)							
Academic Cohort	-0.107	-0.12	0.047	0.022	0.052	0.074	0.066
	(0.026)	(0.027)	(0.081)	(0.088)	(0.105)	(0.111)	(0.097)
F-Stat (instruments)	16.9	19.8	0.3	0.1	0.2	0.4	0.5
R^2	0.01	0.26	0.27	0.27	0.29	0.27	0.29
N	2116	2116	2116	2116	2116	1640	2110
B. Black Subsample							
First Stage: Schooling, 1980							
Academic Cohort	-0.863	-0.89	-0.715	-0.705	-0.758	-0.711	-0.596
	(0.043)	(0.043)	(0.126)	(0.134)	(0.136)	(0.175)	(0.193)
F-Stat (instruments)	403	428	32.2	27.7	31.1	16.5	9.5
R^2	0.37	0.44	0.44	0.44	0.46	0.44	0.43
Reduced Form: AFQT (standardized)							
Academic Cohort	-0.128	-0.142	-0.116	-0.103	-0.269	-0.164	-0.139
	(0.038)	(0.032)	(0.085)	(0.103)	(0.145)	(0.162)	(0.144)
F-Stat (instruments)	11.3	19.7	1.9	1.0	3.4	1.0	0.9
R^2	0.02	0.2	0.2	0.21	0.24	0.22	0.23
N	1028	1028	1028	1028	1028	843	1017

TABLE 4 - RESTRICTED FIRST-STAGE AND REDUCED FORM REGRESSIONS (Continued)

	Cutoff Assigned using State of Birth						Cutoff Assigned using State of Residence
	Full Sample					Non-movers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
C. Hispanic Subsample							
First Stage: Schooling, 1980							
Academic Cohort	-0.766	-0.781	-0.492	-0.44	-0.416	-0.614	-0.446
	(0.061)	(0.042)	(0.112)	(0.117)	(0.186)	(0.233)	(0.168)
F-Stat (instruments)	158	346	19.3	14.1	5.0	6.9	7.0
R^2	0.25	0.41	0.41	0.41	0.46	0.46	0.45
Reduced Form: AFQT (standardized)							
Academic Cohort	-0.078	-0.075	-0.095	-0.124	-0.145	-0.36	-0.256
	(0.072)	(0.069)	(0.172)	(0.183)	(0.095)	(0.144)	(0.134)
F-Stat (instruments)	1.2	1.2	0.3	0.5	2.3	6.3	3.6
R^2	0	0.24	0.24	0.25	0.31	0.36	0.32
N	547	547	547	547	547	430	527
Type of Age Control†	N	N	L	Q	M x Yr	M x Yr	M x Yr
Family Background	N	Y	Y	Y	Y	Y	Y
State dummies	N	Y	Y	Y	Y	Y	Y

Notes: The dependent variable is either schooling completed as of the summer of 1980 (first stage regressions) or the revised AFQT score, standardized to have a mean of zero and a standard deviation of one. (Standardization based on the mean and standard deviation of AFQT scores for the 1957-1962 birth cohorts. See text for more details.) Family background variables are as listed in Table 3. Columns (2) through (6) include state of birth dummies, while column (7) includes dummy variables for state of residence at age 14. All regressions are weighted by AFQT sampling weights, and standard errors are robust to arbitrary residual correlation for individuals born in the same state. †Age controls: N=none, L=linear in exact age (as 1 July 1980), Q=quartic in exact age (as of 1 July 1980),M=month of birth dummies, Yr=year of birth dummies, M x Yr = interaction.

TABLE 5 - OLS AND 2SLS ESTIMATES OF THE EFFECT OF SCHOOLING ON AFQT PERFORMANCE

	Dependent Variable: AFQT Score (standardized)						
	Cutoff Assigned using State of Birth						Cutoff Assigned Using State of Residence
	Full Sample					Non-movers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Non-Hispanic White Sample							
OLS: Schooling, 1980	0.351	0.280	0.406	0.408	0.414	0.434	0.409
	(0.017)	(0.017)	(0.024)	(0.025)	(0.028)	(0.030)	(0.025)
R^2	0.18	0.36	0.38	0.38	0.40	0.39	0.4
2SLS: Schooling, 1980	0.118	0.132	-0.117	-0.054	-0.234	-0.2	-0.297
	(0.028)	(0.029)	(0.206)	(0.215)	(0.552)	(0.326)	(0.485)
R^2	0.1	0.33	0.19	0.24	0.13	0.14	0.07
N	2116	2116	2116	2116	2116	1640	2110
B. Black Sample							
OLS: Schooling, 1980	0.289	0.249	0.302	0.303	0.306	0.311	0.309
	(0.022)	(0.024)	(0.026)	(0.027)	(0.025)	(0.029)	(0.023)
R^2	0.17	0.30	0.31	0.32	0.35	0.34	0.35
2SLS: Schooling, 1980	0.149	0.159	0.162	0.146	0.355	0.23	0.233
	(0.042)	(0.034)	(0.112)	(0.143)	(0.213)	(0.239)	(0.252)
R^2	0.13	0.29	0.29	0.29	0.34	0.33	0.34
N	1028	1028	1028	1028	1028	843	1017
C. Hispanic Sample							
OLS: Schooling, 1980	0.309	0.257	0.324	0.326	0.319	0.304	0.302
	(0.022)	(0.018)	(0.015)	(0.015)	(0.014)	(0.034)	(0.032)
R^2	0.18	0.34	0.36	0.37	0.42	0.45	0.42
2SLS: Schooling, 1980	0.102	0.096	0.194	0.281	0.349	0.585	0.573
	(0.087)	(0.085)	(0.339)	(0.413)	(0.232)	(0.203)	(0.180)
R^2	0.1	0.3	0.34	0.36	0.42	0.37	0.34
N	547	547	547	547	547	430	527
Type of Age Control†	N	N	L	Q	M x Yr	M x Yr	M x Yr
Family Background	N	Y	Y	Y	Y	Y	Y
State Dummies	N	Y	Y	Y	Y	Y	Y

Notes: The dependent variable in all models is the composite index used to create the 1989 AFQT percentile score, converted to standard deviation units. Academic cohort (linear) is used as an instrument in the 2SLS models. See text for more details. See Table 3 for a list of family background controls. Columns (2) to (6) include dummies for state of birth, while column (7) includes dummies for state of residence. All regressions are weighted by AFQT sampling weights, and standard errors are robust to arbitrary residual correlation within the same state. †Age controls: N=none, L=linear in exact age (as 1 July 1980), Q=quartic in exact age (as of 1 July 1980),M=month of birth dummies, Yr=year of birth dummies, M x Yr = interaction.

TABLE 6. THE EFFECT OF SCHOOLING ON THE AFQT: A COMPARISON OF 2SLS ESTIMATES WITH ALTERNATIVE INSTRUMENTS

	Dependent Variable: Revised AFQT Score (Standardized)					
	Instrument:					
	Academic Cohort (Linear)		QOB (dummies)		QOB x SOB (dummies)	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Non-Hispanic White Sample						
Schooling, 1980 (2SLS)	-0.234	-0.200	0.203	0.119	0.316	0.224
	(0.552)	(0.326)	(0.083)	(0.081)	(0.065)	(0.068)
R^2	0.13	0.14	0.15	0.33	0.25	0.36
F-stat (on age controls, 2SLS)	154	4450	3.3	5.1	5.5	6.0
F-stat (on instruments, first stage)	2.9	9.3	39.5	39.5	301.8	175.3
F-stat (on instruments, reduced form)	0.2	0.5	2.9	1.0	146.5	466.6
N	2116	2110	2116	2116	2116	2116
B. Black Sample						
Schooling, 1980 (2SLS)	0.355	0.230	0.378	0.326	0.341	0.322
	(0.213)	(0.239)	(0.159)	(0.129)	(0.075)	(0.071)
R^2	0.34	0.33	0.2	0.31	0.24	0.31
F-stat (on age controls, 2SLS)	116613	2853	1.7	1.4	4.4	3.8
F-stat (on instruments, first stage)	30.9	16.4	12.7	18.4	252.5	141.1
F-stat (on instruments, reduced form)	3.5	1.0	4.6	3.6	26.7	196.3
N	1028	1017	1028	1028	1028	1028
C. Hispanic Sample						
Schooling, 1980 (2SLS)	0.349	0.585	0.582	0.295	0.285	0.191
	(0.232)	(0.203)	(0.230)	(0.344)	(0.105)	(0.118)
R^2	0.42	0.37	0.16	0.37	0.29	0.35
F-stat (on age controls, 2SLS)	1540	817	7.9	7.7	4.6	5.5
F-stat (on instruments, first stage)	5.0	7.0	1.7	1.0	105.8	546.0
F-stat (on instruments, reduced form)	2.3	6.2	1.4	0.5	148.1	703.8
N	547	527	547	547	547	547
Type of Age Control†	M x Yr	M x Yr	Yr	Yr	Yr	Yr
Family Background	Y	Y	N	Y	N	Y
State of Birth Dummies	Y	Y	N	Y	Y	Y
Sample	1	2	1	1	1	1

Notes: The dependent variable in all models is the composite index used to create the 1989 AFQT percentile score, converted to standard deviation units. All regressions are weighted by AFQT sampling weights, and standard errors are robust to arbitrary residual correlation within the same state. † See Table 3 for description of age controls and other control variables. Sample 1 is the full sample (where cutoff dates are assigned using birth state) and Sample 2 is the sample resulting when cutoff dates are assigned using state of residence.

TABLE 7 - 2SLS ESTIMATES OF THE EFFECT OF SCHOOLING ON EACH ASVAB TEST

	White Sample		Black Sample		Hispanic Sample	
Cutoff Classification by State of:	Birth	Residence	Birth	Residence	Birth	Residence
	(1)	(2)	(3)	(4)	(5)	(6)
A. AFQT Components (Academic):						
Arithmetic Reasoning	-0.266	-0.157	0.268	0.200	0.352	0.389
	(0.668)	(0.455)	(0.164)	(0.195)	(0.233)	(0.241)
Math Knowledge	-0.693	-0.87	0.416	0.23	-0.013	0.533
	(0.799)	(0.625)	(0.169)	(0.188)	(0.381)	(0.351)
Word Knowledge	-0.012	-0.04	0.195	0.082	0.628	0.764
	(0.401)	(0.484)	(0.242)	(0.306)	(0.483)	(0.290)
Paragraph Comprehension	0.109	-0.013	0.504	0.445	0.032	0.068
	(0.521)	(0.455)	(0.276)	(0.305)	(0.244)	(0.205)
B. Other ASVAB Components:						
General Science (Academic)	-0.543	-0.249	0.134	0.129	0.653	0.583
	(0.540)	(0.355)	(0.236)	(0.281)	(0.486)	(0.250)
Mechanical Comprehension (Academic)	-0.218	-0.314	0.199	0.217	0.27	0.548
	(0.462)	(0.469)	(0.134)	(0.161)	(0.332)	(0.348)
Numerical Operations (Speed)	0.254	0.073	0.21	0.355	0.329	0.471
	(0.512)	(0.470)	(0.276)	(0.406)	(0.304)	(0.346)
Coding Speed (Speed)	0.974	0.408	0.532	0.504	0.878	0.759
	(0.591)	(0.441)	(0.266)	(0.356)	(0.357)	(0.271)
Auto & Shop Information (Vocational)	-0.324	-0.153	0.266	0.336	0.081	0.205
	(0.472)	(0.412)	(0.130)	(0.139)	(0.313)	(0.235)
Electronics Information (Vocational)	-0.228	-0.026	0.427	0.438	0.000	0.386
	(0.586)	(0.593)	(0.228)	(0.265)	(0.334)	(0.272)
N	2116	2110	1028	1017	547	527
Type of Age Control	M x Yr	M x Yr	M x Yr	M x Yr	M x Yr	M x Yr
Family Background Controls	Y	Y	Y	Y	Y	Y
State Dummies	Y	Y	Y	Y	Y	Y

Notes: Dependent variable is ASVAB test named in far left column of table. All tests have been convered to standard deviation units using the mean and standard deviation score for NLSY cohorts born between 1957 and 1962. See previous tables for description of controls.