Fathers' Education and Children's Human Capital: Evidence from the World War II G.I. Bill

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Abstract

Children who grow up in more highly educated families have better labor market outcomes as adults than those who grow up in less educated families, but we do not know whether this is because education bestows parents with skills that make them better parents or because unobservable endowments that contribute to the parents' educational levels are shared by their children. This paper attempts to improve our understanding of the causal processes that contribute to intergenerational immobility by exploiting variation in fathers' education induced by the World War II G.I. Bill. I use two different identification strategies, both of which rest on the timing of the war: the G.I. bill had different effects on different cohorts depending on their likelihood of military service and the probability that their schooling had been completed before the war began. The two different strategies establish upper and lower bounds on the causal impact of father's schooling: I find that a one year increase in a father's education reduces the probability that his child is retained in school by between two to four percentage points. This implies that parental schooling levels have an affect on children's outcomes that is independent of their innate ability and suggests that public policies aimed at increasing educational attainment may have important intergenerational effects.

Numerous social scientists have documented a positive relationship between the education levels of parents and their children, but the processes that drive this phenomenon are not well understood. While estimated intergenerational correlations appear to be robust to the inclusion of a wide variety of family background variables this is not sufficient to conclude that increasing a parent's level of education will have a causal effect on the next generation's human capital. In fact, these correlations may reflect unobservable parental endowments such as "ability" that are passed on to children.

The purpose of this paper is to improve our understanding of the causal processes that contribute to intergenerational immobility by exploiting variation in fathers' education induced by the World War II G.I. Bill. Quantifying the extent to which a child's human capital can be improved by increasing his parent's education may shed light on the larger debate over whether similarities across generations result from "nature vs. nurture." It also has important implications for public policy: most discussions about the government's role in providing educational aid, for example, focus on the individual's return to education and ignore the possibility of social benefits. Knowing that there are intergenerational returns to increased schooling would provide a further rationale for such programs. On a more basic level, understanding how changes in the distribution of parents' socioeconomic characteristics contribute to changes in the corresponding distributions for their children is a fundamental part of understanding inequality in American society.

While there already exists a large literature that attempts to identify the causal effect of an individual's education on her own earnings (see Card (2001) for a survey), studies that evaluate other possible returns to education are more limited in number.

Among these, only a handful attempt to isolate the causal impact of education on the next generation's well-being (Behrman and Rosenzweig, 2002; Behrman, Rosenzweig and Taubman, 1994; Black, Devereux and Salvanes, 2005; Chevalier, 2003; Currie and Moretti, 2002; McCrary and Royer, 2006; Oreopoulos, Page and Stevens, forthcoming; Rosenzweig and Wolpin, 1994). This is probably due, at least in part, to the fact that it is difficult to find plausible sources of identifying variation: a few studies have used fixed effects models to control for parents' innate characteristics but a drawback of this approach is that it is unclear why the parent's education varies between siblings. Factors that contribute to a parent's changing level of education may also affect his children's outcomes.[1] Other studies have identified the effects of parental education using variation in educational opportunities or requirements across geographic areas and over time, but these changes may in part reflect changes in other location-specific factors that affect children's outcomes. As in family fixed effects models, the source of the variation in geographic-specific educational opportunities or restrictions is usually unknown. Across these studies (which focus on a variety of child outcomes in addition to using different estimation strategies) the estimated causal effects of parental education range from large and statistically significant to indistinguishable from zero.

This study introduces two novel sources of identifying variation based on cohort level differences in schooling levels induced by the G.I. Bill—variation which I will argue is driven simply by the timing of the draft and not by innate individual level characteristics or underlying trends. The dramatic impact of the G.I. Bill on the level of

[1] It is also well known that relative to OLS, such estimates are more prone to errors-in-variables bias (Griliches, 1979). A less widely appreciated problem is that if the "within" variation in unobserved characteristics gives rise to "within" differences in education levels then fixed effects models may actually exacerbate omitted variables problems (Bound and Solon, 1999; Griliches, 1979).

educational attainment among returning veterans has been well documented (see Bound and Turner, 2002; and Stanley, 2003). Furthermore, the bill had different effects on different cohorts depending on the likelihood of military service and the probability that an individual had completed his education before the war began. I use between-cohort variation in the probability of military service and G.I. take-up rates to motivate instruments that are used to identify the effect of fathers' educational attainment on children's progress in school. An advantage of this approach is that the genesis of the identifying variation is transparent.

To be sure, cross-cohort variation in G.I. benefit eligibility will be commensurate with both variation in education levels and any residual effects of military service. A particularly nice feature of the two research designs used in this paper is that if such effects exist, they will lead to upward biases in one case and downward biases in the other. Thus, the estimates produced by the two estimation strategies can be thought of as upper and lower bounds on the true effect of father's education.

The two estimation strategies produce strikingly similar estimates. I find that a one year increase in a father's schooling will reduce the probability that his child repeats a grade by between two to four percentage points. While these estimates are a little larger than the estimate I obtain using OLS, the IV and OLS estimates are not statistically different from each other. My results suggest that the observed correlation between fathers' education and children's grade retention is not driven soley by fathers' innate ability. This suggests that policies that increase educational attainment may be a successful means of reducing inequality among future generations.

The remainder of the paper is organized as follows: Section I describes the G.I. Bill. Sections II and III outline my estimation strategy and data, respectively, and Sections IV and V present the results based on the two identification strategies. Section VI concludes.

I. World War II and the G.I. Bill

The G.I. Bill is widely regarded as one of the most significant education policies to have taken place in modern America. Signed into law on June 22, 1944, it provided unprecedented educational aid to returning veterans who had served for at least 90 days or had been discharged early because of disabilities acquired during service. Anyone who had served on active duty between September 1940 and July 1947 was eligible for support, provided that he began his schooling before July 1951. The number of years of benefits for which a veteran qualified was determined according to the individual's age and length of service, and ranged from one to four years. Most veterans were eligible for all four years of benefits.

The G.I. Bill made very generous financial provisions. It provided full tuition, books and supplies towards virtually any institution of higher education in the country, as well as a monthly stipend that varied by family size. Previous studies have estimated that for a single veteran this cash allowance was equal to about half the opportunity cost of not working, and for a married veteran it was equal to about 70% of the opportunity cost.[2]

[2] Bound and Turner (2002)

The causal effects of this legislation have been thoroughly investigated by Bound and Turner (2002) and by Stanley (2003).[3] Bound and Turner estimate that G.I. benefits increased collegiate attainment by about 40%, using between cohort differences in military service generated by wartime changes in manpower requirements to identify the likelihood that an individual was G.I. eligible. Stanley's estimates are based on comparisons of postsecondary education levels among cohorts of veterans who were less likely to avail themselves of the G.I. Bill because they had already completed their education to those who likely entered the military straight out of high school. This estimation strategy suggests that among veterans born between 1923 and 1926 the G.I. Bill increased postsecondary education levels by about 20%.

These empirical strategies are motivated by concerns about selection into military service. Comparisons of educational attainment between veterans and non-veterans are likely to lead to overestimates of the legislation's effect because one of the primary reasons for deferment from WWII service was physical or mental disability.[4] Since individuals with low mental capacity probably had lower levels of education than average, veteran status alone is unlikely to make a plausible instrument for father's education.

Bound and Turner's identification strategy gets around this problem by comparing outcomes for birth cohorts whose eligibility fell on either side of the sharp decline in manpower needs after 1945. Figure 1 documents the dramatic variation in WWII participation across cohorts. About 30% of men born in 1910 were enlisted and

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[3] In a related study, Lemieux and Card (2001) estimate the effect of the Canadian G.I. Bill on education and earnings.
[4] Among 19-25 year old men deferred in 1945, for example, 56% were deemed physically or mentally unfit (Bound and Turner, 2002).

enlistment rates show a rapid increase among those born between 1914 and 1919. Military service was voluntary until 1940, when Congress passed the Selective Service Act, which mandated registration of young men and required enlistment among those who were deemed eligible. Thus, for cohorts born between 1920 and 1926, who would have been subject to the draft, the participation rate was nearly constant at a little over 80%. Among those who turned 18 after V-J day (cohorts born after the third quarter of 1927) service plummeted. Since the draft produces a sharp correlation between benefit eligibility and an individual's birth date, but birth cohort is unlikely to be correlated with other innate characteristics, a comparison of education levels between pre-1927 and post-1927 cohorts provides clean estimates of the effect of military service and the G.I. Bill.

This study draws on the same identification strategy. Effectively, I compare outcomes for children whose fathers were born between 1923 and 1927 to outcomes among children whose fathers were born after 1928. Since the estimates are identified purely off of variation induced by the timing of the draft, they will not reflect innate parental characteristics such as ability or motivation. One way of assessing the potentially confounding effect of military service is to conduct a companion analysis which focuses on cross-cohort variation among *veterans*. Even among cohorts of veterans who were close in age and faced similar conscription rates, take-up of the G.I. Bill appears to have varied a great deal. Table I shows that while educational attainment at the end of service varied very little across cohorts, benefit take-up rates increased substantially from about 36% of veterans born in 1919, to 62% of veterans born in 1927. In other words, veterans who reached the age of 18 before the U.S. entered the war were less likely to return to school than their younger counterparts. A number of stories could explain this pattern. For example, older veterans were more likely than younger veterans to have completed their education before being drafted, and would therefore have been less likely to obtain additional years of education when they returned. Because older veterans were more likely to have held jobs before entering the military, they were also more likely to have had jobs that were waiting for them when they came back. Finally, since older veterans would have had a shorter time horizon over which to recoup their educational investments, they would have had less incentive to take-up G.I. benefits (although this effect is likely to be small).

I further exploit this variation to identify the effect of father's education on children's outcomes.[5] Specifically, using a sample of cohorts with roughly equal probabilities of being drafted, and conditioning on veteran status, I estimate predicted levels of education for each cohort/veteran cell and use these to create instruments for father's education. This strategy will allow me to simultaneously control for the direct effects of father's age (cohort) and veteran status on a child's human capital, and resulting estimates will only reflect the residual effects of military service if the effects of being a veteran vary across cohorts. *Importantly, such effects would bias the estimates in the opposite direction from the estimates generated by variation in conscription rates before and after VJ day.*

II. Estimation Strategy

In order to set ideas, consider a simple model of the relationship between father's education and children's human capital

[5] Stanley uses this variation in take-up rates to estimate the effect of the G.I. bill on men's education.

$$R_{ic} = \alpha + \beta_1 FathEd_{ic} + \beta_2 FCohort_{ic} + \beta_3 X_{ic} + \varepsilon_{ic} \qquad (1)$$

where R is a relevant outcome for child *i* whose father belongs to cohort *c*, *FathEd* measures the educational attainment of the child's father, *FCohort* is a linear variable measuring the cohort (by birth year and birth quarter) to which the father belongs, and X is a vector of other family background characteristics including the child's sex, age, quarter-of-birth and family size. I do not include measures of family income, father's work experience, or mother's education since these may be affected by educational attainment. The coefficient β_1 should, therefore, be thought of as capturing the overall relationship between father's education and R. The analysis will focus only on white children since previous studies have shown that the effects of the G.I. Bill were quite different across racial groups.[6]

Since it is impossible to fully observe all family background characteristics that belong in *X*, OLS estimates of β_1 are likely to be biased. I attempt to circumvent this omitted variables problem by employing an instrumental variables (IV) strategy that relies on cross-cohort variation in G.I. benefit eligibility. To begin with, consider the first-stage equation

$$FathEd_{ic} = \phi_1 FCohort_{ic} + \phi_2 (Post1927)_{ic} + \phi_3 X_{ic} + \mu_{ic} \qquad (2)$$

Where *Post1927* is a dummy variable that is equal to 0 for cohorts born before 1928 and 1 for cohorts born in 1928, or after. If equations (1) and (2) were estimated using 2SLS

[6] Turner and Bound (2002) show that it had little effect on the collegiate outcomes of black veterans living in Southern states, probably because their educational choices were already so limited. As a result, the G.I. Bill may have exacerbated the education gap between Southern blacks and whites.

then identification would be based on the abrupt decline in conscription among cohorts turning 18 after VJ-Day. As Figure 1 and Table 1 make clear, the vast majority of men born after 1927 would not have been eligible for G.I. benefits provided to WWII veterans. By including a linear trend in both the first and second stage, and focusing on cohorts born within narrow windows, it is reasonable to assume that the *Post1927* dummy would do a good job of capturing the part of educational attainment that is unrelated to innate parental characteristics, or changes in these characteristics over time.

Such a research design would be easy to implement, but the Korean War draft, which affected many men born after 1927, makes it hard to interpret. More than a third of the 1928 cohort in my sample served in Korea, and the fraction increases among later cohorts. Like those who served during WWII, Korean War veterans were also eligible for educational benefits, but unlike the WWII draft, men who wanted to avoid serving in Korea could obtain educational deferments. This means that estimates based on simple comparisons between cohorts who turned 18 on either side of VJ day are likely to be compromised by the effects of the Korean War. Instead of estimating equations (1) and (2) I, therefore, estimate the following augmented equations

$$R_{ic} = \alpha + \beta_1 FathEd_{ic} + \beta_2 FCohort_{ic} + \beta_3 \%Korea_{ic} + \beta_4 \%Korea_{ic} * FCohort_{ic} + \beta_5 X_{ic} + \varepsilon_{ic} \quad \text{(1a)}$$

$$FathEd_{ic} = \phi_1 FCohort_{ic} + \phi_2 \%WWII_{ic} + \phi_3 \%Korea_{ic} + \phi_4 \%Korea_{ic} * FCohort_{ic} + \phi_5 X_{ic} + \mu_{ic} \quad \text{(2a)}$$

where *%Korea* is the fraction of men in the father's year and quarter-of-birth cell who identified themselves as Korean war veterans and the interaction term between *%Korea* and the linear trend allows for the possibility that the Korean conflict may have had a differential effect on later cohorts. This seems likely, as Korean war educational deferments were not introduced until 1951.

This specification also replaces the *Post27* dummy with *%WWII*—the fraction of men in the father's birth cohort who served during WWII and were thus eligible for G.I. benefits. This allows me to make use of the substantial variation in participation rates across quarter-of-birth cohorts who turned 18 right around VJ day. It is conceptually similar to using year and quarter-of-birth dummies as instruments for father's education, but produces a more easily interpretable first stage.

As Figure 1 shows, there is every reason to believe that this instrument will be strongly correlated with father's schooling. The success of the identification strategy hinges on two additional assumptions, however: 1) innate family background characteristics are the same across cohorts, and 2) intergenerational effects of military service beyond the effects on father's education, are small. With respect to 1) concerns about cross-cohort differences are minimized by including a linear trend and focusing on fathers born within a narrow time interval.[7] I have also used data from the 1973 Occupational Change in a Generation Survey (OCG) to look at the extent to which pre-service characteristics (an individual's family income and father's occupation at age 16, whether he lived with both parents at age 16, and his parents' educational attainment) varied across these cohorts. In no case could I reject the null hypothesis that these characteristics were the same across cohorts, although this is partly due to the fact that the OCG sample is small and yields large standard error estimates.[8]

The second issue is probably of more concern. While previous studies have concluded that—at least with respect to individual earnings—the effects of WWII service were small and negative (Angrist and Krueger, 1994; Lemieux and Card, 2001) in

[7] Replacing the linear trend with year-of-birth dummies and quarter-of-birth dummies yields very similar results.
[8] Results available upon request.

principal one can imagine the bias going in either direction. Military service may have provided some men with skills that improved their children's outcomes. In addition, the general public viewed returning veterans as heroes, which may have positively influenced their social interactions within their families as well as in society at large. At the same time, the stress resulting from combat may have left permanent scars on other fathers' abilities to interact with their children and provide for their families.

Since a father's experience in the service may have a direct influence on his child's human capital, I also employ an alternative specification that controls directly for father's veteran status. Specifically, let

$$R_{ic} = \alpha + \beta_1 FathEd_{ic} + \beta_2 FathVet_{ic} + \beta_3 C_{ic} + \beta_4 X_{ic} + \varepsilon_{ic} \qquad (3)$$

and let the first stage be given by

$$FathEd_{ic} = \delta + \phi_1 FathVet_{ic} + \phi_2 C_{ic} + \phi_3 FCohort_{ic} * FathVet_{ic} + \phi_4 X_{ic} + \mu_{ic} \qquad (4)$$

where *FathVet* is a dummy variable indicating whether the father was a WWII veteran, and C is a vector of dummy variables indicating the year and quarter during which the father was born. This specification controls directly for the possibility that military service has an effect on the next generation: the exclusion restriction is based on variation in the veteran effect across birth cohorts. Table 1 makes clear that although pre-service educational attainment varied little across the cohorts born between 1919 and 1927,[9] younger cohorts were much more likely to avail themselves of G.I. benefits. Table 2 shows that by the time of the 1970 Census, educational attainment varied dramatically

[9] Unlike the Survey of Veterans, the OCG suggests that pre-military education levels may have been slightly lower among veterans born in the late 1920's compared to those born in the early 1920's. The Census also shows higher dropout rates for cohorts born in the mid-late 1920's. One interpretation of this pattern is that towards the end of the war young men who were eager to do their part in the war dropped out of school in order to sign up. During the height of the war the military did not officially take volunteers, however, so instances of this behavior should have been limited. Regardless, both first and second stage estimates are robust to samples that exclude fathers with less than a high school diploma.

across cohorts. Veterans born between 1923 and 1927 had 17% more years of post-secondary education than veterans born between 1919 and 1922. Among non-veterans belonging to the same birth cohorts, post-secondary educational attainment actually declined by 6%. Thus, the average *difference* between veterans and non-veterans, which is what the interaction terms capture, grew by 46%. Together, the tables make a strong case that the instrument is strongly correlated with father's educational attainment.[10]

This approach will be successful if there is no cross-cohort variation in who becomes a veteran; a possibility that is minimized by focusing on cohorts with very similar conscription rates. Furthermore, using the OCG, I find no evidence that observable measures of family background prior to military service are correlated with *FCohort*FathVet*.[11] Since father's veteran status is included in both the first and second stage, the instrumental variables estimate will not reflect the average effect of military service.

If there are cross-cohort differences in the effect of WWII (and if they affect the next generation), however, then the estimates will reflect those differences. A reason to be concerned about such a possibility is that those born earlier would have been more likely to see combat and would have served longer.[12] While I cannot completely rule out the possibility that differential military service effects are contributing to the identification of the estimated coefficient on father's education, it is important to note

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[10] My preferred version of the model specifies this variation as a linear trend because cohort-specific deviations away from the trend may reflect variation in unobservable characteristics across cohorts that would also affect their children's outcomes. A specification that replaces the *FCohort*FathVet* interaction with a series of cohort dummies interacted with *FathVet* is also presented, however. The estimates produced by this specification are very similar to those produced in the main analysis.

[11] Results available upon request.

[12] Using data from the OCG, I estimate that among my cohorts average months of service decrease by 2.3 months for each year of birth.

that *biases resulting from the effects of military service using this specification will be in the* ***opposite*** *direction of the biases produced by the first identification strategy. Thus, the two specifications produce upper and lower bounds on the causal effects of education.*[13]

III. Data

III.A. Data on Children's Characteristics

The main analysis is based on a sample of children ages 7-15 taken from the 1960 Integrated Public Use Microdata Series (IPUMS) and 1970 IPUMS state files, both of which consist of individual and household level data from the decennial census.[14] Each of these files provides a 1/100 sample of children in the United States. Children younger than age 7 are excluded because they will not have had sufficient time to have been held back a grade. Children older than 15 are excluded in order to avoid under-representing those who left home before they completed high school.[15] I exclude any individuals for whom information on race, sex, age, or veteran status (fathers only) was allocated. I also exclude individuals who could not be matched to their father, who were not born in the United States or whose fathers were not born in the United States. The sample is limited to children whose fathers are white.

[13] A third identification strategy that has been suggested would rely on cross-sectional variation in conscription rates instead of variation across cohorts. Acemoglu, Autor and Lyle (2004) have shown that military mobilization rates varied substantively across states, with the fraction of eligible men who served ranging between 41 and 54 percent. As expected, these cross-state differences are strongly correlated with father's educational attainment. When I use the mobility rate as an instrument the estimated intergenerational effect is quite similar to the estimates based on cross-cohort variation. However, one needs to interpret this estimate very cautiously: Acemoglu et. al. document correlations between state mobilization rates and other state characteristics that may be correlated with children's outcomes, and when these characteristics are included as control variables the magnitude of the estimated coefficient on father's education increases considerably. This suggests that the instrument is correlated with the error term.

[14] I cannot use the 1% county group and neighborhood files because they do not provide information about state of residence for all survey respondents. State of residence is used to create an accurate measure of grade retention.

[15] Information on father's education is only available for individuals who are still living with them. Children older than 15 are more likely to have left home than younger children.

Estimation of equations (1a) and (2a), which use the abrupt change in conscription rates following VJ-Day to identify the effect of fathers' education (identification strategy 1), is based on a sample of children whose fathers were born between 1923 and 1929-32. These cohorts are chosen because they are close in age (and should thus have had similar life experiences prior to the war), and because the pre-1928 cohorts faced similar probabilities of being drafted. The sample includes between 126,000 and 169,000 child/father pairs, depending on the exact cohorts used.

Estimation of equations (3) and (4) (identification strategy 2) rests on variation in G.I. take-up across cohorts of veterans. Since I want to reduce the possibility that the coefficient on the interaction between *FCohort* and *FathVet* is picking up cross-cohort differences in selection into military service, I focus on a sample of children whose fathers had similar probabilities of being drafted. Figure 1 shows that military service was roughly constant among cohorts born between 1919 and 1927. Participation rates ranged from a high of 85% for the 1921 cohort, to a low of 81% for those born in 1927. I focus on a sample of children whose fathers were born between the third quarter of 1918 and the second quarter of 1927 because these cohorts all had participation rates of over 80%. The sample includes 160, 638 child/father pairs.

Table 3 shows descriptive statistics for these two samples, along with similar statistics for all 7-15 year olds in the 1960 and 1970 Censuses who could be matched to their fathers. As expected, the fathers of the children analyzed in this study are more likely to be WWII veterans. Also, since I focus on cohorts of fathers who would be most likely to be eligible for G.I. benefits, it is not surprising that the level of education among the fathers' in my sample is slightly higher than among all 7-15 year olds. Otherwise, the children in the three samples have very similar characteristics.[16]

The size of this dataset is an enormous advantage in terms of enabling me to obtain precise estimates. Many previous studies of intergenerational education effects have been hampered by researchers' reliance on datasets of fewer than 1,000 observations. The main disadvantage of the IPUMS, and the reason that it has not been used for intergenerational mobility studies, is that it is a cross-sectional dataset that contains little information on children's outcomes. Typical measures of children's success, such as test scores or behavioral problems are not collected by the Census,[17] but it does contain information on the level of educational attainment obtained by each member of the household. I use this information, together with information on the child's age, birthquarter and state of residence, to determine whether or not his schooling level is below the typical grade for his age. Grade repetition is both a widespread phenomenon in the United States and is correlated with many, more commonly used, measures of eventual educational achievement. A recent report from the National Longitudinal Study of Adolescent Health, for example, indicates that over 20% of American adolescents have repeated a grade (Resnick, et.al. 1997), and Feldman (1997) estimates that in many urban districts more than half of all students will be retained. The likelihood of being held back is also highly correlated with family background: in 1995, the probability of being

[16] About 10% of children in the 1960 and 1970 censuses could not be matched to their fathers. These children tend to live in families with lower incomes and lower maternal education levels. Unfortunately, my estimation strategy cannot provide any information about how children without co-resident fathers would have benefited from higher levels of paternal education.

[17] Measures of longer term outcomes such as wages, labor force participation or teenage childbearing are available in IPUMS, but since many of these attributes are relevant only for individuals who are in their late teens or older, analyses of these outcomes becomes complicated by the fact that older children are more likely to live apart from their family of origin. If an individual is living outside of his parents' household then there is no way to link him up with his relevant family background characteristics.

retained was about 40% higher among young adults from low income families relative to young adults from middle income families, and about 50% higher for blacks compared to whites (National Center for Education Statistics, 1997).

There is also evidence that grade retention is correlated with other measures of children's success. The National Center for Education Statistics (1997) for example, estimates that approximately one quarter of young adults who had repeated a grade had dropped out of school by 1995. Similarly, Smith and Shepard (1989) find that students who have previously repeated a grade tend to have worse academic outcomes than similar students who have not repeated a grade. Since educational attainment and test scores are well known to be predictors of adult earnings and health, these studies suggest that if we can obtain estimates of the causal effect of family income and parental education on the probability that a child repeats a grade, we will also gain insight into the causal relationship between these family background characteristics and children's long-run success.

Determining whether or not a child has repeated a grade is complicated by the fact that there is variation across states and over time in the minimum age at school entry, incomplete information on school entry cut-off dates across states and over time, and questions about the degree to which school districts comply with those dates. Since I cannot directly observe grade repetition, I instead follow Oreopoulos et. al. and classify children as repeaters if their educational attainment is below the median for their state, age, quarter of birth, and census year cell. More details about the construction of this variable are included in the Appendix. Individuals' completed education levels are heavily clustered around the median, so that about 18% of children in each cell are classified as below grade-for-age. This fraction compares favorably to other studies.[18]

Since the dependent variable is binary, all second stage regressions are specified as linear probability models. In order to adjust for the fact that older children have had more of an opportunity to repeat a grade, and to adjust for possible gender differences in the probability of being held back, the regressions include controls for age, quarter-of-birth and gender. I also control for family size, and include a dummy variable indicating that the observation is taken from the 1960 Census since the probability of repeating a grade may change over time.

III.B. Data on Father's Characteristics

The 1960 and 1970 Census report father's completed educational attainment in years. This is the measure of father's education that is used in the analysis. A World War II Veteran is defined as anyone who served in World War II, and a Korean War veteran is defined as anyone who served in the Korean war.

The sample is necessarily selected on men who became fathers. If military service altered the likelihood of marriage or the probability of having a child, then the children I observe will be somewhat different from a random sample of children who would have been born if the war had not taken place. Using all men in the 1960 and 1970 censuses, however, I find no evidence that these probabilities vary across the cohorts in my sample.[19]

[18] Oreopoulus et. al. use several measures of grade repetition, including the one used in this study, and find that between 10 and 15% of children are classified as repeaters.

[19] It is well known that WWII shifted the timing of births (i.e. the "Baby Boom"). Cross-cohort differences in the probability of having a child are not apparent within the narrow window of cohorts used in this study,

Another potential issue is that cross-cohort variation in the probability of experiencing combat and the risk of death may induce cross-cohort variation in unobserved characteristics of fathers in my sample. The sample includes only those men who survived the war. Suppose that more "able" veterans were less likely to be on the front-lines. Since later cohorts of veterans were also less likely to engage in combat, this would lead to positive selection among the oldest cohorts in my samples. I investigate this potential problem by estimating the (OLS) rate of return to education for each cohort. If older cohorts are more "able" than younger cohorts, one would expect their rate of return to be higher. I find no evidence of differential rates of return--the average rate of return among both the pre and post 1927 cohorts is 0.085. Furthermore, even if this type of selection exists, it will bias the estimates produced by the two identification strategies in opposite directions.

IV. Estimates Based on Cross-Cohort Variation in Military Service

IV.A. First-Stage and Reduced Form Results

Before turning to the 2SLS estimates of the effect of father's schooling on the next generation's human capital, it is useful to demonstrate that the first-stage effect of the %WWII variable is strong. The first column of Table 4 provides estimates of equation (2a), along with the F-statistic for the hypothesis that this variable is equal to 0. In these and all subsequent regressions, standard error estimates take into account the probable error correlation across children born to fathers in the same cohort. Importantly, given my identification strategy, the estimated coefficient on the *%WWII* variable is

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however. Furthermore, first and second stage regressions used in the main analysis include controls for father's age and family size.

positive and statistically significant, indicating that a ten percentage point increase in the probability of serving is associated with an increase in father's years of schooling of 1/10 of a year.[20] The standard deviation in father's education is approximately three years, so this represents a substantive difference in educational attainment. Given well-known concerns about the usefulness of instrumental variables estimators when first-stage results are weak (e.g. Bound, Jaeger and Baker, 1995; Staiger and Stock, 1997) I also present the corresponding F-statistic, which is 48. This is substantially above recommended cutoff values.

The next column of Table 4 provides the reduced form results. The post VJ-day effect is strong enough to observe a substantive reduced form effect on children's grade repetition: children whose fathers had high probabilities of serving are less likely to have repeated a grade than children whose fathers were born later. A ten percentage point increase in the probability of service is associated with a 0.5 percentage point decline in the probability of being retained in school. The sign of the estimate is exactly what one would expect if father's education has a positive effect on children's human capital.

IV.B. OLS and 2SLS Results

Table 5 gives the 2SLS estimates of the effect of father's education on children's probability of being retained. For comparison purposes, the OLS estimate is also presented in the first column of the table. Like other studies, this estimate suggests that there is a strong correlation between parental education and children's human capital: an

[20] This estimate is bigger than Bound and Turner's estimate. Two factors account for the majority of the difference: 1) Bound and Turner estimate the relationship between %WWII and years of college, whereas I measure father's education at all points on the distribution, and 2) my estimates are based on a sample of fathers whereas Bound and Turner's sample includes all men in the cohort.

additional year of parental education appears to reduce the probability that a child repeats a grade by about 2 percentage points. What is unclear is the extent to which this estimate reflects a causal relationship.

The 2SLS estimates presented in the remainder of the table provide some insight into this question. These estimates continue to imply that an increase in father's schooling reduces the probability that a child is held back in school. Specifically, they suggest that raising fathers' education by one year (about a third of a standard deviation) will reduce the probability that their children are retained by between 2 and 4 percentage points (a 10-20 % decline).

Estimates based on a sample that extends through the 1929 birth cohort are shown in column 2. Restricting the analysis in this way mitigates the possible effect of other cross-cohort differences, but it also eliminates cohorts with the least eligibility for G.I. benefits. Comparisons extending the period of analysis through 1930 and 1932 are shown in columns 3 and 4. Since later cohorts have virtually no eligibility for G.I. benefits, these comparisons more closely approximate a classic treatment/control analysis, but the longer window also increases the potentially contaminating influence of other changes taking place over time. The estimates across the 3 columns are all close to 0.04.

The next three columns of Table 5 show what happens to the estimates when somewhat more sophisticated regression models are employed. Estimates are based on cohorts born between 1923 and 1932, but are not sensitive to which cohorts are included. Column 5 adds a quadratic trend, and an interaction between the quadratic trend and the fraction of the cohort who fought in Korea. This specification is preferred by Bound and

Turner, because it allows the effects of service in Korea to vary across birth cohorts in a non-linear way which is possible given that Korean War educational deferments were not introduced until 1951. This specification decreases the magnitude of the point estimate to -0.03, but it remains statistically different from zero.

The estimate in the next column is based on a specification that replaces the *%WWII* variable with a set of cohort specific dummies. This loosens the linear restriction on the instrument, which leads to a smaller, more precisely estimated coefficient estimate. Column 7 shows what happens to the estimates when the specifications in columns 5 and 6 are combined. Specifically, this regression replaces the *%WWII* variable with cohort dummies and includes a quadratic trend and an interaction with the quadratic trend and *%Korea*. This results in a coefficient estimate that is closer to 0.02.[21] The 99% confidence intervals around most of these estimates include the OLS estimate, but allow one to reject the hypothesis that the effect of father's education is equal to zero.[22]

One concern with the identification strategy is that the experience of serving in WWII may have had a long-lasting effect on fathers over and above its effect on schooling. There are a number of ways in which military service might have affected children's outcomes beyond its affect on education: relative to later cohorts, cohorts

[21] Though not shown, I have also estimated versions of the model that include state fixed effects. The differences in the point estimates are negligible.
[22] I have also experimented with specifications that focus on different parts of the distribution of father's education. When the sample is restricted to children of fathers who have twelve or fewer years of education, the first-stage estimates are weak. When the sample is restricted to children whose fathers have twelve or more years of education the first-stage F-statistic is large compared to the cutoffs recommended by Bound, Jaeger and Baker (1995) and Staiger and Stock (1997), and the second stage estimates are similar to those presented in Table 5.

participating in WWII would have been more likely to be disabled,[23] and more likely to have experienced psychological trauma associated with combat. These effects would likely bias the estimates towards zero. On the other hand, military service might have provided some men with skills that improved their labor market opportunities, which would bias the estimates upward. While the last column of Table 5 shows that the estimates are robust to the inclusion of several observable measures of the father's socioeconomic success (whether the father is currently married, whether he is employed, and the log of family income) it is impossible to completely rule out the possibility that military service itself affected fathers in ways which subsequently influenced the next generation's outcomes. In order to address this concern, I turn to an alternative identification strategy.

V. Estimates Based on Cross-Cohort Variation among Veterans

In this section I employ an alternative estimation strategy, which allows me to directly control for the effect of military service on children's grade-for-age. I estimate equation (3), which directly controls for WWII veteran status and uses the interaction between father's cohort and veteran status to identify the causal effect of education. This interaction term is highly correlated with father's education because different cohorts of veterans exhibited different propensities to take-up G.I. benefits.

V.A. First-stage and Reduced Form Estimates

[23] Bedard and Deschenes (2003) show that WWII service is associated with early mortality and disability. Unfortunately, the 1960 Census did not include information about individuals' health. The 1970 Census does include information about whether the individual is disabled, but information about veteran status and disability are not on the same form, so it is impossible to include a measure of disability in my analysis.

First, I demonstrate that the proposed instrument is strongly correlated with father's education by showing the first-stage estimate of equation (4) in the first column of Table 6. I also show the partial F-test for the hypothesis that the *FCohort*WWII Veteran* interaction is equal to 0. Importantly, given my identification strategy, the estimated coefficient is positive and statistically significant, indicating that veterans born in any given year get approximately 8 % of a year (4*0.019) more schooling than veterans born one year earlier. The F-statistic is 23.

The next column of Table 6 provides the reduced form results from regressing children's educational progress on the interaction term. The first-stage effects are strong enough to observe a substantive reduced form effect on children's grade repetition: children whose veteran fathers were born in 1927 are on average 3 percentage points less likely to have repeated a grade than children whose fathers were born in 1919.[24] The negative estimate is exactly what one would expect if younger veterans are more likely to have taken advantage of the G.I. Bill. Furthermore, it is strikingly similar to the reduced form estimate in Table 4: in both cases the association between a one year increase in father's educational attainment and child's probability of being at grade-for-age is about 5 percentage points.

V.B. OLS and 2SLS Results

Table 7 gives the OLS and 2SLS estimates of the effect of father's education on children's probability of being at their grade-for-age. These estimates are remarkably similar to those presented in Table 5, and hold up across the different samples and

[24] This is calculated by multiplying the estimated coefficient -0.001, by the 32 birthquarters between the beginning of 1919 and the end of 1927.

specifications. Furthermore, the estimated effect of father's veteran status is very small (though very imprecisely estimated) suggesting that military service is likely to have only a small influence on the estimates presented in Table 5.

In column 2 we see the IV results that are produced by the full sample. The coefficient estimate is -0.036 and the standard error estimate 0.010. The third column excludes children of fathers who were born before 1920 and after 1926. These cohorts are dropped because their probability of being drafted was slightly lower than for the others and the characteristics of veterans born in 1919 and 1927 may, therefore, be somewhat different than for the rest of the sample. In fact, excluding these cohorts raises the estimated standard error but has no effect on the point estimate.

In the next column I present estimates based on a specification that relaxes the assumption that the take-up of the G.I. Bill decreased linearly with age. Here, I replace the linear *FCohort*WWII Veteran* interaction with cohort dummies interacted with veteran status. This is not my preferred specification because the source of the cohort specific deviations from trend is not known and could be related to cohort characteristics that are correlated with children's outcomes. The specification choice is unimportant, however, because the estimated coefficient on father's education is virtually the same.

The final column of Table 7 shows how the estimates are affected by adding controls for whether the father is married, whether he is working, and the log of family income. If the inclusion of these variables significantly changes the estimated effect of father's education then one might be concerned that the effects of WWII service are affecting children's outcomes through another avenue. The inclusion of these variables barely changes the magnitude of the estimate, however.

VI. Conclusion

Previous studies have shown that the WW II G.I. Bill had substantial effects on men's educational attainment. This paper exploits two different features of that relationship to estimate the effect of father's education on children's accumulation of human capital. I find that a one year increase in father's schooling reduces the probability that his child is behind the typical grade for his age by 2 to 4 percentage points. While the point estimates are not very precise, they are statistically different from zero, enabling one to rule out the possibility that innate parental characteristics are the only source of the intergenerational correlation.

The estimation strategies I employ necessarily identify a combined effect of military service and the G.I. Bill. Importantly, any biases that result from my inability to fully control for the separate effects of military service will be in opposite directions across the two identification strategies. The fact that the two approaches yield similar results therefore suggests that such biases are likely to be very small. Taken together, the estimates suggest that children's educational outcomes can be improved by increasing their parents' educational opportunities.

Since grade retention is negatively correlated with other academic outcomes, the positive effect of parental education on children's grade progression is likely to have long-term socio-economic benefits as well. The results suggest that there are substantive social benefits to policies that increase educational attainment beyond increases in an individual's own earnings. In particular, such policies may help reduce inequality among future generations.

My results imply that at least some of the intergenerational transmission of inequality can be attributed to environmental influences. There are many ways in which higher levels of parental education might affect a child's environment, including increases in family income, better parenting, and improved social networks. Understanding the mechanism by which parental education increases children's human capital is an important question that deserves further research.

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Figure 1
World War II Participation Rates

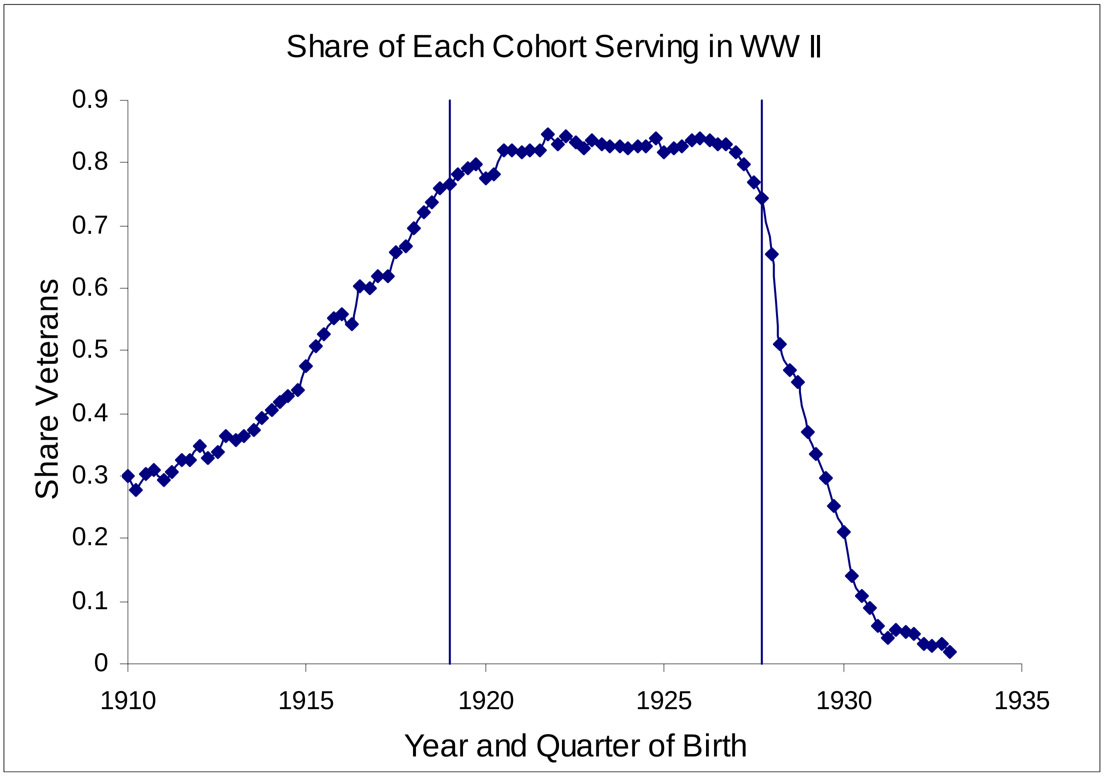

Table I
Educational Attainment and Use of GI Benefits among WWII Veterans
As Reported in Bound and Turner (2002)

Year of Birth	N	Age at Military Discharge	Educational Attainment at End of Service	Used GI Benefits	Months of GI Benefits	Received BA with GI Benefits	Years of College with GI Benefits
1915	143	31.2	11.6	0.27	2.8	0.01	0.01
1916	163	30.4	11.5	0.27	3.2	0.00	0.04
1917	192	29.4	11.1	0.33	4.7	0.03	0.15
1918	237	28.3	11.5	0.37	4.3	0.03	0.21
1919	**234**	**27.3**	**11.4**	**0.36**	**6.0**	**0.04**	**0.21**
1920	**268**	**26.8**	**11.4**	**0.40**	**6.0**	**0.06**	**0.32**
1921	**324**	**25.5**	**11.1**	**0.40**	**6.3**	**0.06**	**0.32**
1922	**315**	**24.6**	**11.4**	**0.49**	**7.6**	**0.10**	**0.55**
1923	**295**	**23.9**	**11.5**	**0.51**	**8.4**	**0.12**	**0.69**
1924	**275**	**23.8**	**11.4**	**0.48**	**8.4**	**0.14**	**0.73**
1925	**280**	**22.3**	**11.4**	**0.54**	**9.3**	**0.15**	**0.78**
1926	**261**	**21.7**	**11.2**	**0.55**	**11.1**	**0.12**	**0.86**
1927	**256**	**21.8**	**11.4**	**0.62**	**11.9**	**0.12**	**0.98**
1928	97	22.4	11.3	0.49	9.0	0.15	0.89
1929	31	24.8	11.1	0.35	3.9	0.03	0.29

Source: Data are from the 1979 Survey of Veterans.

Table 2
Educational Attainment Among Veterans and Non Veterans

	Veteran			Non Veteran			Veteran/Non Veteran Difference		
	%Hs	Yrs Coll	%Coll	%Hs	Yrs Coll	%Coll	%Hs	Yrs Coll	%Coll
1915	0.58	1.11	0.18	0.42	0.54	0.08	0.16	0.56	0.10
1916	0.60	1.04	0.17	0.44	0.59	0.92	0.16	0.45	-0.75
1979	0.61	1.01	0.16	0.43	0.65	0.11	0.18	0.36	0.05
1918	0.61	1.05	0.17	0.46	0.77	0.13	0.15	0.28	0.04
1919	**0.62**	**1.05**	**0.17**	**0.44**	**0.68**	**0.11**	**0.19**	**0.37**	**0.06**
1920	**0.65**	**1.20**	**0.20**	**0.45**	**0.66**	**0.11**	**0.20**	**0.53**	**0.09**
1921	**0.67**	**1.22**	**0.20**	**0.49**	**0.74**	**0.12**	**0.18**	**0.48**	**0.08**
1922	**0.67**	**1.30**	**0.22**	**0.43**	**0.57**	**0.10**	**0.24**	**0.73**	**0.12**
1923	**0.68**	**1.33**	**0.23**	**0.44**	**0.55**	**0.09**	**0.24**	**0.79**	**0.14**
1924	**0.68**	**1.42**	**0.25**	**0.43**	**0.54**	**0.09**	**0.25**	**0.88**	**0.16**
1925	**0.67**	**1.42**	**0.25**	**0.43**	**0.62**	**0.10**	**0.24**	**0.80**	**0.15**
1926	**0.66**	**1.40**	**0.24**	**0.44**	**0.68**	**0.12**	**0.22**	**0.72**	**0.12**
1927	**0.68**	**1.42**	**0.25**	**0.47**	**0.73**	**0.12**	**0.20**	**0.69**	**0.13**
1928	0.65	1.24	0.21	0.55	0.89	0.15	0.09	0.34	0.06
1929	0.57	0.78	0.12	0.56	0.94	0.16	0.01	-0.15	-0.04
1930	0.54	0.57	0.07	0.53	0.97	0.16	0.01	-0.40	-0.09
Average 1919-1922	0.65	1.19	0.20	0.45	0.66	0.11	0.20	0.53	0.09
Average 1923-1927	0.67	1.40	0.24	0.44	0.62	0.10	0.23	0.77	0.14
% Increase Post 1922-Pre 1922	**0.04**	**0.17**	**0.24**	**-0.01**	**-0.06**	**-0.04**	**0.15**	**0.46**	**0.58**

Source: 1970 Census

Table 3

Descriptive Statistics

	All 7-15 Year Olds with White Fathers	7-15 Year Olds Whose Fathers Were Born Between 1923 and 1930	7-15 Year Olds Whose Fathers Were Born Between 1918:3 and 1927:3
Father's Year of Birth	1923.4	1926.3	1922.6
	(9.07)	(2.26)	(2.57)
Father WWII Veteran	0.44	0.58	0.81
	(0.50)	(0.49)	(0.39)
Family Income	37,298	39,025	37,623
	(21,416)	(21,851)	(21,958)
Father's Highest Level of Education	11.2	11.5	11.4
	(3.40)	(3.28)	(3.32)
Child Female	0.5	0.49	0.50
	(0.50)	(0.50)	(0.50)
Child's Age	10.8	10.7	10.8
	(2.50)	(2.52)	(2.47)
Child has Repeated A Grade	0.18	0.18	0.17
	(0.39)	(0.38)	(0.38)
Number of Observations	446,226	141,405	160,638

Source: 1960 & 1970 Census.
Note: standard deviations in parentheses.

Table 4
First Stage and Reduced Form Regressions
Using Cross-Cohort Variation in Military Service
Cohorts Born 1923-1930
(Standard Errors in Parentheses)

	Dependent Variable	
	Father's Yrs of Ed	Below Grade-for-Age
Father's Cohort	-0.022	0.001
(measured in birthquarters)	(0.004)	(0.000)
% Korean War Veterans	3.580	-0.166
	(0.834)	(0.043)
Father's Cohort* % Korea	-0.031	0.002
	(0.023)	(0.001)
% WWII Veteran	1.231	-0.053
	(0.177)	(0.011)
F-Statistic	48	
Number of Observations	141,405	141,405

Source: 1960 & 1970 Census.

Sample includes all 7-15 year olds with matching fathers born in relevant years.

Other control variables include family size, child's gender, and an indicator for whether the observation comes from the 1960 Census.

Table 5
Estimated Effects of Parental Education on Probability of Being Below Grade-for-Age
Father's Cohort Born Between 1923-1932
(Standard Errors in Parentheses)

	OLS	IV						
					Including interaction between quadratic trend and % who fought in Korea	Replacing % who fought in WWII with cohort dummies	Replacing % who fought in WWII with cohort dummies and quadratic trend	Additional Control Variables + cohort dummies + quadratic trend
Father's Birth Cohort	1923-1929	1923-1929	1923-1930	1923-1932	1923-1932	1923-1932	1923-1932	1923-1932
Father's Education	-0.018	-0.037	-0.043	-0.045	-0.031	-0.032	-0.021	-0.030
	(0.000)	(0.010)	(0.010)	(0.009)	(0.012)	(0.006)	(0.008)	(0.017)
% Korean War Veterans	-0.065	0.022	-0.012	-0.046	-0.049	-0.049	-0.128	-0.045
	(0.060)	(0.010)	(0.032)	(0.015)	(0.126)	(0.013)	(0.109)	(0.127)
Married								-0.080
								(0.019)
Work								-0.033
								(0.014)
Log Income								-0.001
								(0.039)
Child age and bqrtr dummies	x	x	x	x	x	x	x	x
Linear trend	x	x	x	x	x	x	x	x
Linear trend * % Korea	x	x	x	x	x	x	x	x
Quadratic trend					x		x	x
Quadratic trend * % Korea					x		x	x
Number of Observations	126,361	126,361	141,405	168,730	168,730	168,730	168,730	168,098

Source: 1960 & 1970 Census.
Sample includes all 7-15 year olds with matching fathers born in relevant years.
Other control variables include family size, child's gender, and an indicator for whether the observation comes from the 1960 Census.

Table 6
First Stage and Reduced Form Regressions
Using Cross-Cohort Variation Among Veterans
Cohorts Born Between 1918:3 & 1927:2
(Standard Errors in Parentheses)

	Dependent Variable	
	Father's Yrs of Ed	Below Grade-for-Age
Father WWII Veteran	1.25	-0.031
	(0.064)	(0.005)
Father's Cohort*WWII Veteran	0.019	-0.001
	(0.004)	(0.0002)
F-Statistic	23	
Number of Observations	160,638	160,638

Source: 1960 & 1970 Census

Sample includes all 7-15 year olds with matching fathers born in relevant years.

Other control variables include family size, child's gender, child's age dummies, child's quarter-of-birth dummies, father's age and quarter-of-birth dummies and an indicator for whether the observation came from the 1960 Census.

Table 7

Estimated Effects of Parental Education on the Probability of being Below Grade-for-Age
Using Cross-Cohort Variation Among Veterans
(Standard Errors in Parentheses)

	OLS	IV		Full Set of Interactions	Additional Controls
Father's Birth Cohort	1919-1927	1918:3-1927:2	1920-1926	1918:3-1927:2	1918:3-1927:2
Father's Education	-0.019	-0.036	-0.036	-0.032	-0.039
	(0.0004)	(0.010)	(0.026)	(0.008)	(0.012)
Father WWII Veteran	-0.013	0.014	0.013	0.007	0.016
	(0.002)	(0.016)	(0.044)	(0.013)	(0.013)
Married					-0.089
					(0.016)
Work					-0.04
					(0.012)
Log (Family Income)					0.021
					(0.026)
Number of Observations	160,638	160,638	128,742	160,638	159,860

Source: 1960 & 1970 Census
Sample includes all 7-15 year olds with matching fathers born in relevant years.
Other control variables include family size, child's gender, child's age dummies, child's quarter-of-birth dummies, father's age and quarter-of-birth dummies and an indicator for whether the observation came from the 1960 Census.

Appendix

Following Oreopoulos *et. al*. I classify children as repeaters if their educational attainment is below the median for their state, age, quarter of birth and census year cell. Specifically, I estimate the number of years that a child "should" have completed based on the median grade reached among those who are the same age, and were born in the same birthquarter, live in the same state, and were observed in the same census year. The variable REPEAT is then set equal to 1 if a child's grade is below the median for other corresponding children and 0 otherwise. This measure thus takes into account year and state specific characteristics (such as the current cohort's likely enrollment cutoff date) and individual level characteristics (quarter of birth) that affect the age at which the child was likely to have enrolled in school.

This is not a perfect measure. Students who entered school late, for example, will be classified as having been held back, and delayed entry into kindergarten is a fairly common practice. Nine percent of first and second graders in the mid-1990's had entered kindergarten late, whereas only 5-6% had repeated kindergarten (National Center for Education Statistics, 2000). In a recent paper, Cascio (2003) compares directly reported measures of grade repetition in the 1992, 1995 and 1999 Current Population Survey School Enrollment Supplement to a below grade proxy using educational attainment data available in the Census. She finds that about 20% of all children are incorrectly classified by the below grade proxy, and that about 94% of such errors are comprised of children who have not repeated a grade but who are classified as such by the proxy.

Normally, researchers are not concerned that noisy dependent variables will generate biased estimates because measurement error in a normally distributed dependent variable merely generates inefficient standard error estimates. Cascio points out, however, that when the dependent variable is an indicator for whether or not the individual has repeated a grade, then consistency may be a problem. Measurement error in a binary dependent variable will produce attenuated parameter estimates (Aigner, 1973; Hausman, 2001). Cascio estimates that the attenuation factor on her measure of grade repetition *may* be as high as 0.35. Obviously, it would be preferable to generate unbiased estimates, but downward biased estimates will still be informative because the prior is that the intergenerational correlation is not wholly causal. Estimates that are statistically different from zero, therefore, still allow me to reject the hypothesis that there is not an exogenous effect of parental education on children's human capital. In addition, because the measure of grade repetition I use is less noisy than Cascio's, the bias should be smaller.