

The Impact of College on Migration: Evidence from the Vietnam Generation

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This version: December 2011

ABSTRACT

We examine the causal effect of education on migration using variation in college attainment due to draft-avoidance behavior during the Vietnam War. We use national and state-level induction risk to identify both college attainment and veteran status for men observed in the 1980 Census. 2SLS estimates imply that additional years of college significantly increased the likelihood that affected men resided outside their birth states later in life. Most of our estimates suggest a causal impact of higher education on migration that is larger in magnitude but not significantly different from the OLS estimates.

(JEL: J61, J24, I23)

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Acknowledgments: The authors would like to thank David Figlio and two anonymous referees as well as Dan Black, Kerwin Charles, Bill Evans, Lawrence Katz, Caroline Hoxby, Thomas Lemieux, Laddie Sula and Lars Lefgren for helpful conversations and comments. Comments from seminar participants at the NBER Education Program Meeting, the Midwest Economics Association Meetings, Society of Labor Economists Meetings, University of Notre Dame, University of Illinois-Urbana Champaign, Dartmouth College, University of Illinois-Chicago, Indiana University-Purdue University Indianapolis, Rutgers University, Princeton University, and the University of Maryland improved the paper. All errors are our own.

I. Introduction

Data from the 1940-2000 U.S. Censuses show that the long-distance migration rates of college graduates are roughly twice those of high school graduates.¹ Strikingly, these migration rates by education group have been largely stable over these six decades, despite large increases in postsecondary educational attainment. While this suggests that higher education may have a causal impact on migration, we cannot rule out other explanations for this pattern, such as falling transportation and communication costs, without a credibly identified empirical analysis.² However, in contrast to the extensive evidence on the causal impact of education on wages (Card 1999), little is known about the role of education as a causal determinant of migration. Geographic mobility may afford an individual a greater range of choices over jobs and locations and may also reflect an ability to respond appropriately to spatial disequilibria across labor markets. As such, migration constitutes an important return to schooling that is distinct from increased wages. In this paper, we use exogenous variation in college going to provide the first credible analysis of the causal relationship between education and migration.

We exploit variation in college attainment induced by draft-avoidance behavior among men affected by the Vietnam conflict to isolate the causal effect of education on migration. This approach is inspired by Card and Lemieux (2000, 2001a), who document excess educational attainment among cohorts induced to enter college as a means of deferring conscription into the Armed Services during the Vietnam conflict. While Card and Lemieux focus on differences in the risk of induction into the military across birth cohorts, we further exploit state level variation in induction risk *within* cohorts. The existence of state level variation allows us to decompose national induction risk into its constituent parts: induction risk faced by a young man's own state cohort and

¹ This is true for both out-of-birth state residence and migration across state/county lines in the past five years in the Census. For early evidence on educational migration differentials, see Ladinsky (1967) and Greenwood (1975). Wozniak (2010) and Greenwood (1997) provide more recent evidence. Ferrie (2005) provides evidence on migration rates over the past 150 years, but education is only recorded in the Census beginning in 1940.

² Hoxby (2009) describes how falling transportation and communication costs have affected the migration patterns of college students.

risk faced by young men of that cohort in the rest of the country. We use this new source of variation to consistently estimate our parameter of interest in the presence of two endogenous variables—education and veteran status. This approach is an advance over studies that use the Card and Lemieux measures to identify college going using only year-to-year variation in induction risk (De Walque 2007; Grimard and Parent 2007; MacInnis 2006). We also draw on six-month rather than yearly records of induction rates, which provides for greater temporal variation in induction risk. Finally, the decomposition of induction risk into its national and state components is a novel use of two instruments to address the endogeneity of both education and military status that is common in studies using changes in military policies as instrumental variables (Angrist and Krueger 1992; Bound and Turner 2002; Stanley 2003).³

Using micro-data from the 5% sample of the 1980 U.S. Census of Population, we provide OLS and 2SLS estimates of the effect of college education on migration. In cross-sectional OLS regressions, we observe that an additional year of postsecondary schooling is associated with an increase in the probability of residing outside one's birth state of 1.8 to 3.4 percentage points, or approximately five to ten percent of the mean of this variable. When we instrument for college education with cohort-level induction risk, the causal effect of an additional year of postsecondary schooling on migration is generally larger, but estimates range from 2.7 percentage points to 9.0 percentage points. These results imply that the entire disparity in migration rates between those with and those without a college degree—a gap of roughly 20 percentage points—is due to the causal impact of obtaining a college education. Moreover, the large reduced-form estimates for the effect of induction risk on out-of-state migration suggest that the Vietnam War led to substantial migration across the national labor market.

We conclude with an examination of the mechanisms that might explain our findings. We are able to rule out two potential mechanisms behind the effects we document. First, the college

³ Angrist and Krueger (1992) faced a similar problem in their study of the effects of educational attainment on earnings using draft lottery numbers to identify educational attainment.

migration premium is not driven by moving away to college. We show instead that the propensity to migrate is similar across individuals with different ex ante propensities to attend college out of state, based on historical patterns in college going. Using a similar strategy, we also show that the college migration premium is not due to exposure to a geographically diverse set of peers in college. These results suggest that a useful avenue for future research is to explore the ways in which other mechanisms might contribute to the migration impact of a college education.

Our paper contributes to a long-standing literature on the determinants of migration and to the more recent literature examining the causal impact of education on non-pecuniary outcomes. By testing for a causal relationship between a college education and migration, we document the interaction between these two key dimensions of human capital.⁴ This has important implications for individual welfare. Geographic mobility, like health, expands the set of choices available to individuals and therefore provides individuals with options that can increase utility. For example, by allowing individuals to move to avoid negative economic shocks, migration can have a direct impact on individual earnings and potentially mitigate these shocks on those individuals who remain (Wozniak 2010; Bound and Holzer 2000).

In a related fashion, the focus on migration also serves to expand the notion of human capital acquired through schooling to include a broader set of skills and attributes. From an economic perspective, migration can be viewed as an individual response to labor market disequilibria (Bowles 1970). Documenting a causal link between education and migration would therefore suggest that education confers—in addition to other skills—the ability to deal with disequilibria. Indeed, there was a period in which leading labor economists considered the question of whether education confers the ability to respond to economic disequilibria, but this line of

⁴ See Becker (1962) and Schultz (1961) for a discussion of migration and education as key aspects of human capital. Schwartz (1976) provides an analysis of the interplay between migration and education.

analysis withered without resolution.⁵ Examining the nature of the relationship between education and migration may provide new insight into this important question.

II. The Vietnam Draft

Between 1964 and 1973, almost 2 million men were drafted through the Selective Service System to serve in the Vietnam conflict. The Selective Service System, which comprised over 4,000 local draft boards across the nation at that time, was responsible for registering recruits and classifying them for either deferment or selection. All men over the age of 18 were required to report to their local draft board for classification. Those who were classified as ‘available for service’ and passed the pre-induction examinations were liable for induction.⁶ Local draft boards were also able to issue deferments for various reasons, such as school attendance or the presence of dependent children. Prior to the introduction of the draft lottery in 1969, the process of determining which men were drafted was set according to the following priorities: highest priority for “delinquents”—those who failed to register or failed to report for the pre-induction physical; second priority for volunteers; and third priority for non-volunteers aged between 19 and 25. It is worth noting that not everyone who was called before a draft board was inducted into the military, and not everyone who was inducted actually served in Vietnam.

College enrollment was a well known and virtually foolproof way to defer and avoid conscription. Before 1967, deferment for the purposes of four-year college enrollment was almost guaranteed in practice through the individual decisions of local draft boards.⁷ The Military Service Act of 1967 made this official by stating that college students in good standing could defer induction until receipt of an undergraduate degree or age 24, whichever occurred first. Indeed, over 1.7 million

⁵ Nelson and Phelps (1966) postulate a higher return to education in the presence of technological change and Welch (1970) provides related evidence. Schultz (1975) offers a survey of studies examining the ability to deal with disequilibria.

⁶ Men who were drafted were generally assigned to serve in the Army for up to 3 years, although they could potentially choose their branch of service by volunteering and thereby also qualify for a shorter period service.

⁷ Tatum and Tuchinsky, *Guide to the Draft*, Ch. 3. By contrast, enrollment in a two year college was not considered grounds for automatic deferment. Students at two-year programs were only eligible for occupational deferments under the same rules as those already employed. See Rothenberg (1968).

college deferments were granted in 1967 alone.⁸ Although men who received college deferments were technically eligible for induction until age 35, very few men between the ages of 26 and 35 were ever drafted. Card and Lemieux (2000) estimate that, among men born between 1945 and 1947, those with a college degree were only one-third as likely to serve in Vietnam as compared to those without a college degree. Thus, there was a very large incentive for men to enroll in college to avoid the draft during these years.

The introduction of the draft lottery in 1969 led to a substantial change in the induction process. The first lottery, held on December 1, 1969, determined priority for induction in 1970 according to random sequence numbers (RSNs) assigned to the day and month of men born between 1944 and 1950. Additional draft lotteries took place in 1970, 1971 and 1972 which were applicable to men born in 1951, 1952, and 1953, respectively. College deferments continued to be issued until September 1971, and men who were already enrolled were allowed to retain their deferment until the end of the school year. For our purposes, the relevant fact is that real and perceived risk of induction during this period was much lower than in the draft board era. Men were at risk of induction under the lottery for only a single year, and the overall rate of inductions was substantially lower (as documented below).⁹ Importantly, in contrast to the draft board era, most men could accurately gauge their own risk of induction once their lottery number (RSN) was assigned.

A. Variation in Inductions over Time

The number of inductions varied considerably over the course of the Vietnam War. From 1960 to 1963, inductions were fairly low at approximately 8,000 men per month. However, following the Gulf of Tonkin incident on August 2, 1964, Congress authorized an expanded role for the U.S. military in Vietnam. Inductions more than doubled from 1964 to 1965 and again from 1965 to 1966.

⁸ The number of college deferments remained above 1.7 million in 1968 and 1969, and then fell to 1.5 million and 1.3 million in 1970 and 1971 respectively (Semiannual Reports of the Director of the Selective Service System, 1967-1973).

⁹ Note that the lottery also altered the risk of induction within a cohort: men who received high (low) RSNs faced relatively low (high) risk of induction. The net effect of these changes is theoretically ambiguous but likely to be small.

By the spring of 1968, in the midst of raging student protests, the rate of inductions reached a peak of almost 42,000 a month. During the period that the draft lottery was in place, the rate of inductions fell rapidly from about 20,000 per month in late 1969 to 2,000 per month in late 1971. In February of 1973, the draft was suspended and no more inductions took place.

Responsibility for devising and meeting the national target number of conscriptions rested with the federal Department of Defense (DoD). To achieve this target, the DoD issued monthly “draft calls” that divided the national number into quotas assigned to state draft boards, which did the active work of ordering men to be inducted. The monthly state quotas sum to the national draft call each month.¹⁰ The sum of monthly state and national draft quotas over consecutive 6 month periods were reported in the Semiannual Reports of the Director of the Selective Service System to Congress. We use this information to construct a measure of induction risk based on semi-annual induction rates in our empirical analysis.

B. Variation in Risk of Induction across States

Institutional factors also caused induction risk to vary across states within a cohort. State level variation in induction risk arose for several reasons. First, there was a great deal of variation in operating procedures across local draft boards that, in turn, led to local variation in induction risk. Davis and Dolbear (1968) identify three sources of variation in local board risk, “...[1] variation based on differences in socioeconomic characteristics of jurisdictions; [2] variation among states based on differences in policy interpretations provided to local boards and exaggerated by success in achieving standardization around such particular practices; and [3] variation produced by idiosyncratic discretionary decision-making by local boards” (Page 18). They later write, “The conclusion seems inescapable: local board autonomy implies both within state and between state variability, even among socioeconomically similar board jurisdictions,” (Davis and Dolbear 1968, p. 84). Similar idiosyncrasies were enumerated in the 1967 report of the National Advisory

¹⁰ Information in this paragraph is based on Shapiro and Striker, *Mastering the Draft*, Chapter 20.

Commission on Selective Service.¹¹ Finally, Shapiro and Striker (1970, Ch. 20) write that “ a registrant’s chances of being inducted vary with his board’s deferment policy.”

A second source of variation in induction risk across states and over time was the severe communication delays between the federal, state, and local officials in charge of the draft. These delays meant that local draft boards knew the current number of registrants available in their jurisdiction while the DoD assigned quotas using registrant numbers that were several months old. Thus, induction risk for an eligible man at a point in time was not only a function of the number of men in his state currently eligible for the draft but also of the number available several months ago. The current pool could be much larger than the past pool if, for example, a large number of local men graduated high school thus becoming draft eligible or much smaller if a large number married or aged out of the draft pool in the intervening months. In practice, boards were encouraged to have just enough eligible registrants to match the number of inductees they would be asked to produce in response to the next call. Under this pressure, communication lags led to a cobweb-type feedback loop as boards struggled to achieve the successful “standardization” identified in the second item of the Davis and Dolbeare list (See Shapiro and Striker 1970, Ch. 20).

III. Identification Strategy

We identify the causal effect of education on migration using variation in college going caused by draft-avoidance behavior in response to the risk of being drafted during the Vietnam conflict. An artifact of our identification strategy is that the likelihood an individual is a veteran also varies across cohorts in our sample. Insofar as veteran status is a plausible determinant of migration (Pingle 2007), it is important to control for this variable in our empirical estimation. To complicate

¹¹ Echoing Davis and Dolbeare’s item [2], the commission noted, “Much of the System’s uneven performance relates to the lack of standardization in the guidance the local boards receive.” The commission also noted a high level of inconsistencies in performance within and across boards. For example, they found that among the individual records they reviewed, 27% of those reclassified to I-A (eligible for service) were so classified prior to the conclusion of their existing deferment (National Advisory Commission on the Selective Service 1967, Ch. 3).

matters, veteran status is also plausibly endogenous, since unobserved individual characteristics might drive both the decision to become a veteran and the decision to migrate.¹² Our equation of interest therefore includes both college attainment and veteran status as endogenous variables.

Accordingly, our identification strategy uses two instruments to isolate exogenous variation in both endogenous variables.

Despite the complication of an additional endogenous variable, we prefer our approach to other instrumental variables for postsecondary education that have been used in the recent literature. Two prominent examples include new college openings (Currie and Moretti 2003) and the introduction of state-level merit aid programs such as the Georgia Hope Scholarship (Dynarski 2008). However, these alternative instruments increase access to college by lowering the relative price of nearby or in-state colleges, and may therefore also affect the likelihood of migrating out-of-state through the decision to attend a local or in-state college. An important feature of our instrumental variable strategy is that it identifies the effect of higher education on migration in the absence of such relative price changes, and is therefore more appropriate for our question.

A. The National and State-Level Cohort Risk Instruments

We extend an instrumental variables strategy inspired by Card and Lemieux (2000, 2001), henceforth CL. They show that Vietnam era induction risk varied significantly across birth year cohorts due to differences in military manpower requirements and cohort size, and find that young men responded to an increased risk of induction by attending and completing college at higher rates than pre-draft cohorts.¹³ In particular, CL use total national inductions as the numerator in the following annual measure of induction risk for a birth cohort:

¹² A number of related papers face the same problem. De Walque (2007) includes veteran status but treats it as an exogenous variable. MacInnis (2006) instruments for veteran status using a quartic polynomial in age while Grimard and Parent (2007) consider a specification that instruments for veteran status using information about early health problems.

¹³ Specifically, increased induction risk led to significant deviations in male college attainment gains from the inter-cohort path traced out by females, whose attainment gains followed a similar trajectory to that of pre-draft males.

$$(i) \quad CL \text{ induction risk}_c = \frac{\sum_{a=19}^{22} I_{ca}}{N_c}$$

where I is the number of inductees from a birth year cohort, N is the number of men in that cohort, c indexes birth cohorts, and a indexes age since a cohort was most at risk to be drafted between the ages of 19 and 22. We have experimented with alternatives to the CL induction risk measure, such as using induction risk at age 19 only or incorporating population size of other cohorts. We prefer the CL measure, however, since it both incorporates additional risk faced beyond age 19 and normalizes that risk for an individual's own cohort size.

Like CL, we assume that draft avoidance was proportional to the risk of induction. However, in order to account for the mechanical relationship between inductions and veteran status, we exploit state-level variation within the cohort-level variation identified by CL. The existence of state- and cohort-level variation allows us to break national induction risk into its constituent parts. We thereby obtain two separate instruments that can be used to identify both college attainment and veteran status. Moreover, we use semiannual data on inductions by state to construct a measure of induction risk based on six month birth cohorts, which provides additional temporal variation (as in Siminski and Ville 2011). Thus, young men faced state cohort risk that is analogous to the CL measure, where s indexes state of residence:

$$(ii) \quad staterisk_{sc} = \frac{\sum_{a=19}^{22} I_{sca}}{N_{sc}}$$

This measure of induction risk is our first instrument. We then use our state-level data on I and N to construct a second instrument in the following manner:

$$(iii) \quad nationalrisk_{sc} = \frac{\sum_{a=19}^{22} I_{-sca}}{N_{sc}}$$

This measure defines national cohort risk for a man living in state s and born in the six month birth cohort c as the number of inductees from all other states, $-s$, and six month birth cohort c , divided

by the total number of such men. This is highly correlated with CL's measure but varies across birth state and birth cohort. Note the inclusion of the national and state cohort risk measures is similar to the inclusion of state and national unemployment rates as explanatory variables (as in Ziliak, Wilson, and Stone 1999).

To construct the measures in equations (ii) and (iii), we obtained data on the number of inductees from each state for each six-month period spanning 1961 to 1972 from reports of the Selective Service.¹⁴ We estimate state cohort size using enrollment numbers spanning 1959 to 1970, the academic years in which our cohorts of interest were in 11th grade.¹⁵ Thus state- and cohort-level induction risk for a young man born in Alabama in the first half of 1950 equals the number of inductees from Alabama in the first half of 1969 (the year he turned 19) divided by half the number of students enrolled in 11th grade in Alabama in 1967.¹⁶ National level risk for the same young man equals the number of men inducted nationally in the first half of 1969 divided by the size of his six month birth cohort, after subtracting own state inductions from the numerator and own state cohort size from the denominator. Like CL, we construct an average induction risk for the years a man was 19 to 22, since induction risk was non-trivial for men ages 20 to 22.

Figure 1 graphs the variation in state-level induction risk for a selection of states over our period of interest. Panel A shows raw induction risk as defined in (ii) while Panel B shows residual induction risk after controlling for a cohort trend, state-of-birth fixed effects, and national risk. Given that we include these controls in our main empirical specifications, the patterns in Panel B approximate our identifying variation. Note that there is indeed variation across states in the induction risk that is not explained by fixed state characteristics alone. Moreover, residual induction

¹⁴ Semiannual induction data are not available from 1961 to 1966 so we divide yearly inductions by 2 for the six month intervals in this period.

¹⁵For 1959 and 1960, we only have information on total high school enrollment so we divide this figure by 4 to approximate 11th grade enrollment. We divide yearly enrollments by 2 for all six month intervals. We also check our estimates using enrollment in 10th grade.

¹⁶ We use birth state to proxy for residence state at the time of the draft. Evidence in Wozniak (2010) shows that although this assumption generates measurement error by misclassifying individuals into incorrect states of residence at draft age, the misclassification error is not substantially different across education groups.

risk in a state appears to fluctuate smoothly over time. This is not surprising given that our measure of induction risk is based on the average risk faced by men between the ages of 19 and 22. By averaging the risk of induction over several years, we mechanically introduce some degree of serial correlation and use lower frequency variation to identify the impacts of draft avoidance behavior.¹⁷

The observed variation in induction risk is consistent with the known sources of cross-state variation in induction risk. While Davis and Dolbeare identify idiosyncratic variation in draft board decision-making (the third item in their list), the other sources of state and local variation in risk would tend to generate smooth fluctuations in relative state risk. For example, if the demographics of a local board jurisdiction are changing smoothly such that more men are aging into the draft pool, but the local board is slow to adjust the number of deferments it granted, risk will slowly decline in this jurisdiction over time. In any case, these smooth fluctuations do not pose a threat to our identification strategy as long as our instruments meet the IV assumptions. Furthermore, any serial correlation in our instrumental variables can be handled through appropriate standard error adjustments along the lines discussed in Bertrand, Duflo, and Mullainathan (2004) and Angrist and Pischke (2009, Ch. 8).

B. Validity of Our Instruments

We present our first-stage estimates in the results section, but given the novelty of our IV strategy, the exclusion restriction and the monotonicity condition bear some discussion. The exclusion restriction requires that the components of induction risk have no influence on migration choices except through the channels of college attainment and veteran status. Our instruments could fail to meet this restriction if young men attempted to exploit local variation in induction risk by moving between localities. In fact, draft board policies prohibited this type of “local board shopping.” Rothenberg (1968) notes that it is a myth that “you [can] change draft boards the way

¹⁷ Note that the rationale for this average measure is derived from the fact that men faced induction risk over several years (rather than the presence of adjustment lags as in Baker, Benjamin, and Stanger [1999]). Appendix Figure 1, which plots induction risk based on inductions at age 19 only, displays substantially less serial correlation.

you change your patronage of a supermarket or a bank.” A young man was required to register with his local board at age 18, and this remained his local board for his entire period of draft eligibility. In the event that he did move away, his original local board always maintained final decision making authority over his draft eligibility although some particulars of draft processing may have been handled by a board closer to his new residence.¹⁸ It was also unlikely that young men had accurate information about their particular board’s deferment policies. Indeed, a lack of transparency regarding board operating procedures was a main criticism of the Selective Service during this period (National Advisory Committee on the Selective Service 1967, Ch. 3).

Monotonicity requires that there be no “defiers” in the population—i.e., individuals who would have gone to college or become veterans in the absence of the draft but who did not because of the draft (Angrist and Imbens, 1994; Angrist et al. 1996). It is difficult to test for such a subpopulation directly, and most IV applications simply assume there are no defiers. We are, however, somewhat concerned that blacks may have been crowded out of college slots by large numbers of whites attempting to avoid conscription, a violation of monotonicity. In light of this, we verified that our results are not sensitive to this assumption by estimating our main models separately for blacks and whites (results available upon request).

C. Interpretation in the Presence of Indirect Effects of Veteran Status and Education

It is possible that our two endogenous variables influence one another, in addition to the outcome variable of interest, and this possibility complicates the interpretation of our results.¹⁹ This possibility is probably strongest for an indirect effect of veteran status on migration through education. Beyond a direct effect through the experience of training for combat in another state or actually going to war in Vietnam, there may be an indirect effect of veteran status on migration

¹⁸ We have also consulted several reference guides on the draft—books written for young men at the time to inform them about their draft duties and obligations—to determine whether or not the practice of board shopping may have been common even though it was prohibited by board policy. We find no evidence of this. See Evers (1961), Tatum and Tuchinsky (1969), and Sanders (1966). Only later did advocates of reforming the Selective Service System highlight this variation.

¹⁹ In the language of the literature on indirect effects, these are called mediating variables. For detailed exposition, see Robins and Greenland (1992), Pearl (2005), and Rosenbaum (1984).

through education because the GI Bill subsidized education for most veterans. Recent work by Angrist and Chen (2011) indicates that veterans were more likely to attend college than non-veterans, primarily as a result of the educational benefits conferred to them by the GI Bill. Given this additional pathway, we cannot separately identify the direct and indirect effects of veteran status on migration.

It might seem that there is also an indirect effect of education on mobility via veteran status. After all, the key to our identification strategy is that college students could delay and eventually avoid conscription by staying in school. However, induction rates at the *birth state and birth cohort level* were unlikely to be affected by draft-avoidance behavior. This is because local draft boards were required to fulfill specific absolute manpower requirements from the Department of Defense. If a certain individual avoided the draft, someone else from his state-cohort would need to be drafted instead. Indeed, Flynn (1993, Ch. 7) notes that even with deferments, the Johnson administration – which was by far the largest user of conscripted servicemen--never faced significant manpower constraints on its Vietnam plans.²⁰ Moreover, there is no evidence that supply constraints among the non-college enrolled population ever resulted in local boards overturning the draft exemption for this group. This historical evidence suggests that, to a first approximation, veteran status at the state-year cohort level was unaffected by college-going decisions. We therefore assume that education does not affect migration through veteran status *at the level of our identifying variation*. Since our empirical analysis employs specifications in which the data has been aggregated to the state-cohort, this allows us to interpret our coefficients on education in the second stage equations as the direct effect of education on migration.

A final consideration in interpreting our results is the possibility for heterogeneous treatment effects. Specifically, some marginal college students will have been “sent directly” to college because

²⁰ Also, Shapiro and Striker (1970, Ch. 20) explain that while there were six categories of potential inductees that boards could use to meet their mandatory draft quotas, those in categories four or higher were rarely called, suggesting that local draft boards had ample scope for meeting the calls for manpower from the Department of Defense.

of the desire to avoid induction risk, and others will have been “sent indirectly” to college because the instruments affected their veteran status which gave them access to the GI Bill and, in turn, college. The estimates in (1) are therefore free from omitted variables bias, but our coefficient on education will reflect the average treatment effect of college going for both the “draft dodging” and the GI Bill-funded college students. To the extent that the treatment effect varies across these different types of induced college going, our estimates reflect the average of two different local average treatment (LATE) effects.

IV. Data and Empirical Specifications

A. Data

Our data on the “Vietnam generation” come from the IPUMS microdata 5% sample of the 1980 Census (Ruggles et al. 2004). CL focused their analysis on men born between 1935 and 1955. Due to data limitations on state-level induction rates and enrollment, we restrict our sample to men born between 1942 and 1953. However, these cohorts experienced the largest rise and fall in induction risk, so we do not lose much time-series variation relative to CL by excluding the earlier and later cohorts. MacInnis (2006), who also focused on men born between 1942 and 1953, argues that a shorter span of cohorts is also less likely to introduce cohort bias.

We restrict our analysis to men. CL use women as a comparison group by assuming that female college attainment is an appropriate counterfactual for male college attainment in the absence of the Vietnam War. However, there are reasons to believe that female college attendance may have been affected by male college going during the Vietnam years, with large inflows of men into college either crowding out women who would otherwise have attended or encouraging more women to attend to take advantage of marriage market prospects.²¹ Regardless of whether or not women offer an appropriate counterfactual for men in the context of college going, the presence of spousal links

²¹ In a related point, Horowitz, et. al. (2009) argue that household-level borrowing constraints led to lower college attendance by the sisters of men who avoided the Vietnam draft.

between men and women in their geographic location decisions suggests that they may not be a useful counterfactual in the context of migration. For these reasons, we prefer to control directly for secular trends in male college going and use our birth state and birth cohort variation to identify changes in male college going driven by induction risk.

Our main measure of migration is a dummy variable indicating whether or not an individual resides outside his birth state when we observe him in the 1980 Census (during his late 20s or early 30s). This measure captures long-distance migration that generally involves a change in local labor markets.²² Hence, such moves are more likely to capture the relationship between education and the ability to make changes in the face of disequilibria. We can also show that educational differences in 1-year and 5-year migration rates are similar to those in the out-of-birth state measure.²³ We prefer the lifetime rates since they more closely approximate the stock of migration choices. Finally, although parental migration patterns contribute to the lifetime migration measure, the migration differential between children of more and less educated parents is small compared to differentials observed across education groups later in life (Wozniak 2010).

Our primary measure of education attainment is years of postsecondary schooling, which we term “years of college.”²⁴ We also present alternative specifications using discrete measures such as attending or completing college. Since these measures typically suffer from different types of potential bias, it is useful to examine robustness to these alternative measures of educational attainment.²⁵ Our measure of veteran status is based on the veteran indicator available in the 1980 Census.

²² See Molloy, Smith, and Wozniak (2011) for a discussion of the measurement error involved in assuming a move across state lines involves a move between local labor markets.

²³ We tabulated 1-year and 5-year migration rates for 20-to-45 year-olds in our four education groups from the 1981 Current Population Survey and the 1980 Census, respectively.

²⁴ Using years of schooling (at any level) as an outcome yields broadly similar results. But, as we demonstrate in a subsequent section, the impact of our instruments is focused on educational attainment at the postsecondary level.

²⁵ College completion has been shown to be the most accurately measured higher education outcome and to contain little measurement error overall (Black et al. 2003). On the other hand, years of postsecondary schooling is unlikely to be plagued by error that is negatively correlated with the recorded value, as is the case with the dummy variable schooling measures (Kane, Rouse, and Staiger 1999; Black et al., 2000).

We also consider two additional variables that are intended to capture labor market conditions facing a cohort at the time of the college enrollment decision: (a) the employment to population (epop) ratio in the individual’s state of birth in the year his cohort turned 19, and (b) the log of the number of respondents from a birth state and birth year cohort in the 1960 Census. Together, these approximate the changes in labor demand and labor supply which may have occurred alongside changes in state-level induction risk.²⁶ For example, Molloy and Wozniak (2011) show that internal migration rates, including inter-state migration rates, are procyclical. Descriptive statistics are shown in Table 1, which summarizes the variables used in our analysis for the sample of men born between 1942 and 1953.

B. Empirical Specification

We estimate all of our regressions at the birth state by birth cohort level because our source of exogenous variation occurs at this level. Consequently, our main econometric model is as follows:

$$(1) \quad mig_{sc} = \lambda' X_{sc} + \beta vet_{sc} + \gamma educ_{sc} + \tau trend_{sc} + \delta_s + \varepsilon_{sc}^m$$

where s indexes state of birth and c indexes six month birth cohorts, mig_{sc} is the fraction of men residing outside their state of birth, vet is the fraction of veterans, and $educ_{sc}$ is average years of postsecondary (college) schooling completed. In addition, $trend_{sc}$ is a linear trend in birth cohort, X_{sc} is our set of demographic controls (which includes fraction of blacks and Hispanics), and δ_s represents a full set of state-of-birth dummies. Allowing for state-of-birth fixed effects removes variation arising from states with persistently higher or lower than average induction rates, which may be associated with other state characteristics (e.g. industrial composition) that are correlated with migration rates.

In addition, we show results for two specifications that include an expanded set of controls. The first adds our proxies for labor market conditions facing a cohort at the time of the college enrollment decision, employment to population ratio and log cohort size. The second adds nine

²⁶ The literature tends to find no consistent, significant relationship between local labor market conditions and college attendance (Wozniak 2010; Card and Lemieux 2001b).

linear birth region trends to the estimating equations. These trends further account for changes in college-going or migration that may have been correlated with changes in state level induction risk. For specifications using only national induction risk as a single instrument, we use national aggregates for cohort size and the employment rate.

Our endogenous variables, $educ_{sc}$ and vet_{sc} , are predicted from first-stage equations that include the remaining right-hand side covariates in Equation (1) plus $staterisk_{sc}$ and $nationalrisk_{sc}$ as defined in (ii) and (iii), respectively. Estimation is done via standard linear 2SLS, weighted by the number of observations in each state-cohort cell.²⁷ Standard errors are clustered by birth state and by birth cohorts following the multilevel clustering procedure in Cameron, Gelbach, and Miller (2006). For the purposes of comparison with the previous literature, we also present results from specifications in which we predict for education using only $nationalrisk_{sc}$ (and where we cluster at the birth cohort level).

Figure 2 plots the means of selected variables separately for each birth cohort. The pattern of induction risk across cohorts is evident in Panel A, which also shows the high correlation between our measure of national risk in (iii) and CL's measure in (i). The increase in college going is also visible in Panel A, particularly for cohorts born between 1944 and 1950, during the main rise and fall in induction risk. Panel B plots our measures of education and migration (adjusted for a basic set of controls) alongside our national risk measure. This is essentially a graph of our reduced form results, and it indicates that migration and educational attainment increase along with national induction risk across cohorts.

The inclusion of controls in our 2SLS regressions removes a large amount of variation from our instruments. To assess the remaining variation, we regressed both risk measures, (ii) and (iii), separately on the sets of controls just described. The R-squared from all six versions of this

²⁷ See Wooldridge (2002) pp. 622-624 concerning 2SLS versus an approach with a probit first stage when the endogenous variable is a dummy variable. In some cases, the probit approach is more efficient but may tend to produce larger point estimates. Given our concerns about possible upward bias, we implement 2SLS estimation.

exercise—two outcome variables times three sets of controls, used in Tables 3 through 6 below—ranged between 0.25 and 0.42. This means that we are using approximately two thirds of the variation in the risk measures to identify our endogenous variables.²⁸

V. Results

A. First-stage and Reduced Form Results

Table 2 presents first stage estimates from Equation (1) as well as the two augmented specifications. In Panel A, national and state-level cohort risk are entered separately as the only variables identifying years of schooling. This is similar to the approach taken in previous studies in which education is assumed to be the only endogenous variable. Columns (1) through (3) of Panel A show that a 10 percentage point increase in national cohort risk (roughly the entire range of this variable) increased the average years of college in our sample by 0.32 to 0.37 years with the impact declining somewhat as we add additional controls to the basic specification in column (1). In other words, cohorts with higher national risk also attain more years of postsecondary schooling. This is consistent with Figure 1 and the results in CL (2000, 2001). Although our specification differs from that of CL, the magnitude of our coefficients is similar. The F-statistics suggest that this first stage has substantial power.²⁹ We repeat the first stage estimation using only state cohort risk as the identifying variable in columns (4) through (6) of Panel A. When national cohort risk is excluded, state cohort risk positively predicts schooling for men in our sample, although the point estimates are slightly smaller than those generated by national risk. Figure 3 plots the coefficients and standard errors from separate regressions of educational attainment at each grade level and higher (inclusive),

²⁸ In this exercise, most of the variation is explained by the inclusion of the linear cohort trend and *epop*. However, we did not perform an exhaustive set of regressions that examined all possible subsets of our control variables.

²⁹ Table 1 in Card and Lemieux (2001a) shows estimates corresponding to a 4.6 percentage point increase for college graduation. Using our specification with a dummy variable for college graduation suggests an increase of 3.5 to 4 percentage points in college completion for the same change in induction risk.

and confirms that higher national and state-level induction risk increased male educational attainment at all postsecondary levels.

Panel B shows first stage equations from our preferred approach of identifying both years of college and veteran status in Equation (1). For transparency, we estimate two separate first stage equations—predicting years of college and veteran status separately—although 2SLS estimates these equations jointly. Consistent with the manner in which 2SLS identifies endogenous variables, both equations include national *and* state cohort risk as identifying variables. Columns (1) through (3) of Panel B show that, as in Panel A, increased national induction risk is strongly associated with increases in years of college. The F-tests reported in the final row of the table are calculated according to the (corrected) Angrist and Pischke (2009) procedure and indicate that our instruments have sufficient power. The point estimates are larger than when using national risk alone, indicating increases of 0.45 to 0.52 years of college over the range of induction risk in our sample. On the other hand, years of college are negatively related to state cohort risk when national induction risk is included. This is in sharp contrast to the relationship between years of college and state risk in Panel A, which suggests that increases in schooling attainment are largely driven by variation in national risk.

It is not clear a priori what relationship we should expect between state risk and years of college in the Panel B specifications. If young men were aware of their state risk, we might expect a positive relationship between state risk and years of college as men “dodged in to college” more in riskier state-years. Our review of the historical literature suggests that this was not the case, and the results support this. On the other hand, if young men were unaware of their state level risk, we would expect no relationship. Indeed, while national cohort risk was relatively easy to judge as national draft calls were widely reported, deviations of state cohort risk from national risk were probably unknown *ex ante*. The presence of a negative relationship may be an artifact of the high correlation (collinearity) between national and state level risk. The coefficient pattern across Panels

A and B—in which state level risk positively predicts education when national risk is excluded and the point estimates on national risk increase when state risk is included—is consistent with collinearity (Verbeek 2000; Wooldridge 2002). While collinearity does not introduce systematic bias since the predicted values of the endogenous variables in the first stage are correct in expectation, it may contribute to imprecision in our second stage estimates.

Columns (4) through (6) address the second endogenous variable in Equation (1). These show that national and state cohort risk both covary positively with veteran status. This is reassuring since higher rates of induction risk at both the state and national level should push more young men into this group. We interpret the fact that the coefficient on national risk exceeds that of state risk in the veteran equation to mean that the time series variation in induction risk generated by the massive fluctuation in aggregate military manpower demands is responsible for more of the variation in veteran status than are the differences in induction risk across states.

Table 3 presents reduced form estimates of the impact of induction risk on our measure of migration.³⁰ The results are striking. The estimates show that increases in national cohort risk are associated with a large increase in the likelihood that an individual is residing outside his birth state. Men from the highest risk cohort are 8 to 13 percentage points more likely to reside outside their states of birth than men in the lowest risk cohort. This difference is the equivalent of roughly one-fourth to one-third of the mean of this variable in Table 1. Once state cohort risk is included, the impacts of national risk on migration are even larger and state cohort risk has generally weak negative impacts on migration.

B. OLS and 2SLS Effects of College Education on Migration

Table 4 presents our main results. It reports both OLS and 2SLS estimates of Equation (1). For the purposes of comparison, Panel A reports our results using only national risk as a single instrument for years of college (without controls for veteran status since it is likely to be

³⁰ Note that we drop specifications using only state cohort risk to identify education in our subsequent analysis. This was only presented as an alternative first stage in Table 2.

endogenous). Our preferred estimates are in Panel B, where we instrument for both years of college and veteran status using our two risk measures. We report our OLS estimates in columns (1) through (3) of both panels. An additional year of postsecondary schooling is associated with about a 2 percentage point increase in the likelihood that a man resides outside of his birth state in our most parsimonious specifications. Including additional controls yields a coefficient of approximately 3 percentage points, or roughly ten percent of the mean of our dependent variable. Interestingly, veteran status is not associated with increased migration in these OLS specifications.

Columns (4) through (6) of Panel A show results from 2SLS estimation using only national risk to instrument for years of college and omitting veteran status from the equation. The impact of schooling on migration is both economically and statistically significant, ranging from 2.7 percentage points in our most parsimonious specification to over 4 percentage points in our augmented specifications.

Columns (4) through (6) of Panel B show our preferred estimates from 2SLS estimation of Equation (1) using both national and state-level induction risk to instrument for years of college and veteran status. These causal estimates of years of college on migration are larger than the single IV estimates of Panel A, although they also cover a fairly large range. The smallest estimate implies that an additional year of schooling raises the probability of an out-of-state residence by 6.6 percentage points while the largest implies an increase of 9.0 percentage points. The 2SLS estimates in Panel B have large standard errors, yet all are significant at conventional levels. The estimates across all three 2SLS specifications in Panel B consistently imply a large causal role for years of college in determining migration. Instrumenting for veteran status indicates no significant impact on subsequent migration, which matches the OLS estimates on this variable.

The OLS coefficients on years of college typically lie within the 95% confidence interval around our 2SLS estimates because the latter are estimated with large standard errors. We compare our estimates more formally using a Durbin-Wu-Hausman test for endogeneity and find that we

cannot reject the null hypothesis that the OLS and IV estimates are the same for any of the specifications in Table 4.³¹ Nevertheless, the magnitudes of our IV estimates are consistently larger than those of OLS. Two standard explanations immediately come to mind. First, measurement error might bias OLS estimates downward so eliminating this bias can lead to larger IV estimates. This bias might be more likely in our sample if older respondents are more likely to misreport educational attainment in retrospective surveys. Second, heterogeneity in the response to treatment may lead to local average treatment effects on the marginal college graduate (identified in our 2SLS estimates) that differ from the average effect (identified in OLS). We find some evidence for the possibility of heterogeneous effects in the National Longitudinal Survey of Youth. In that data, we observe higher returns to college completion among groups who were more likely to be marginal college students during the Vietnam era.³²

It is also possible that the historical setting of our instrument leads to larger treatment effects than would be found in a different context. One might worry that induction risk increased the likelihood of attending an out-of-state college because marginal college students had to search more widely to find a college that would accept them. If so, and if moving away to college is an important mechanism through which college has a causal effect on migration, this may have led to larger estimated effects of college on migration in our setting. In fact, using data from the Department of Education's Residence and Migration Surveys of 1963, 1968, 1975, we do not find that students attended out-of-state colleges at higher rates during periods of higher induction risk.

C. Specification Checks

³¹ The F-statistic of this test for the 2SLS specifications in Panel A of Table 4 are 0.04, 0, and 0.09 respectively (p-values of 0.844, 0.952, and 0.768). The F-statistic of this test for the 2SLS specifications in Panel B of Table 4 are 0.10, 0.16, and 0.97 respectively (p-values 0.746, 0.686, 0.324).

³² Specifically, we estimate higher OLS returns for men in the upper quartile of the AFQT distribution and with mothers who only had some college education. Results are available from the authors upon request.

We conducted a number of tests to examine the robustness of the results in Table 4. First, we repeated the Table 4 analysis using alternative measures of educational attainment: dummy variables for college attendance (measured as 13 or more years of schooling) and for college completion (16 or more years of schooling). The results are reported in Appendix Table 1.³³ Both college attendance and college completion are associated with significantly higher probabilities of out-of-state residence among the men in our sample. Our IV estimates of the causal impact of college completion are statistically indistinguishable from the impact of college attendance, although the magnitudes are larger for college completion. Additionally, dropping national induction risk and replacing the trend with birth year dummies in the Table 4 specifications—which forces our identifying variation to come solely from cross-state differences in induction rates—does not qualitatively alter our conclusions, nor does using overall national cohort risk as defined in (i) instead of (iii) as one of our instruments.

Table 5 reports the results of two placebo-type analyses intended to verify that the causal relationship we identify is in fact driven by the response of young men in our affected cohorts to the induction risk they faced. First, we re-estimate our reduced form specifications for men in our sample of cohorts using data from the 1960 Census. At this point in their lives, these men had not yet been subject to the incentives of enrolling in college to avoid the Vietnam draft. If the cohorts who would eventually face the greatest induction risk were somehow also more likely to have migrated by our measure *before* the draft, this might indicate that our causal estimates in Table 4 are spurious. It would suggest that high-risk cohorts were somehow different from low-risk cohorts in a way that might lead them to have both higher educational attainment and migration rates.

The results from this exercise are in Panel A of Table 5. The specifications in columns (2) and (3) using only national induction risk to instrument for education indicate that men who face

³³ Since the inclusion of veteran status does not affect the OLS coefficient on education in these specifications either, we only report one set of OLS results. First stage estimates follow similar patterns to those in Table 2, with F-statistics indicating that we do not have any problems associated with weak instruments.

higher national risk in the future were also more likely to have moved out of their birth states prior to 1960. This suggests estimates using national risk alone to identify years of schooling in our augmented specifications should be interpreted with caution. However, we find no significant relationship between the induction risk a man will face in the future and his out-of-state residence in 1960 for specifications that use both national and state-level induction risk. The magnitudes of these reduced-form estimates are also substantially smaller than those from Table 3. Hence, we take these results as supporting our preferred identification strategy of instrumenting for both education and veteran status.

In Panel B, we estimate our 2SLS models on a set of placebo cohorts. Unfortunately, there is no ideal placebo group on which to run such a test. We use men born between 1930 and 1941—the twelve cohorts preceding those in our sample—who were too old to be affected by induction orders during the Vietnam conflict although by virtue of their age these men were not subject to the same non-draft related cohort factors experienced by the men in our main sample. The estimates in Panel B show no significant relationship between education and migration when we use our Vietnam induction risk measures to identify years of college. This is consistent with our claim that these instruments affect educational attainment only for our main sample of cohorts and that it is the education of these cohorts that leads to increased migration.

VI. Mechanisms behind the Causal Relationship

Our results suggest a large causal role for higher education in migration. It is then natural to ask, what is the mechanism through which college has these effects? We hypothesize that the causal effect may arise through either direct or indirect channels. On the one hand, higher education may *directly* confer skills or provide information that improves one's ability to make a long-distance move. For example, individuals may develop general cognitive skills that facilitate the accumulation of information about alternative employment possibilities in other places. Individuals may also gain

specific information about new opportunities for migration either through attending college away from home or by interacting with peers who come from other places. In addition, the process of moving to college may foster greater skills for migration, especially when associated with a move across state lines. A college education may also lower the psychic costs associated with migration by fostering openness to new experiences and increasing exposure to a diverse peers which may lessen the difficulties of adjusting to a new place.

On the other hand, higher education may *indirectly* affect the likelihood of undertaking a long-distance move. Research shows that increased schooling is causally related to higher wages (Card, 2001). If moving involves a fixed monetary cost, higher incomes may increase migration. Across the United States, the market for college graduates is often considered more geographically integrated than the market for lower-educated workers. If the skills associated with a college education are marketable in many regions, this may make it easier to consider moving out of state (Bound et al. 2004). Finally, education may alter other non-economic characteristics such as marriage which can affect the likelihood of migration (Lefgren and McIntyre 2006).

Using data on the population enrolled in college from the 1960 Census, we provide evidence on two of the direct mechanisms mentioned above: exposure to a geographically diverse set of peers and a move out of state to attend college. Specifically, we use data on individuals who report attending college in the 1960 Census to create two separate rankings of states: the “out of state” ranking and the “geographic diversity” ranking.³⁴ The first ranks states according to the shares of their college-attending natives getting their degrees out of state, with quartile 1 having the lowest shares of its natives attending college out of state and quartile 4 the highest. If the experience of moving to go to college is an important channel behind the causal impacts we estimate, we might expect the college migration premium to be larger for natives of the highest quartile states. The

³⁴ We have also used the Department of Education’s Residence and Migration Surveys of 1958, 1963, 1968, 1975 to construct comparable measures of out-of-state college going based on state of residence in high school. These rankings do not appear to change much over time, suggesting that they are a good proxy for the fixed differences across states.

“geographic diversity” ranking divide states into quartiles according to the geographic diversity of their college populations measured using a Herfindahl index.³⁵ If exposure to a geographically diverse set of fellow students increases awareness of other labor markets or improves one’s network in distant locations, we might expect the college migration premium to be larger for natives in states with more geographically diverse colleges. The states in each set of quartiles are listed in Table 6.

We use our sample of Vietnam era men from the 1980 Census to estimate versions of our main 2SLS specification in which years of postsecondary schooling is interacted with quartile indicators for each of the two state rankings. Since this specification contains five endogenous variables—the four interactions with schooling plus veteran status—we instrument using interactions of the four quartiles with national risk plus (non-interacted) state risk.³⁶ The results, including OLS estimates, are shown in Table 6.

We find that for natives of states in the bottom quartile of out-of-state college going, the OLS migration premium to an extra year of postsecondary schooling in our sample, of 0.028, is significantly lower than that for natives in the higher three quartiles, who get returns of 0.040 to 0.047. As was the case with the 2SLS estimates of the migration premium in earlier tables, the IV estimates in the second column of Table 6 have large standard errors. These prevent us from saying definitively whether or not the causal effect of higher education on migration is larger for natives of one quartile than another, although the point estimates in the IV column are similar in magnitude across quartiles, suggesting little difference in the causal impact of college attendance on migration across quartiles of the out-of-state ranking.³⁷ In the case of the geographic diversity of one’s college peers, the OLS estimates show no consistent difference in the schooling migration premium across natives from states in the four quartiles of this distribution. Again, the 2SLS estimates are noisy, but

³⁵ We also estimated linear specifications of the interaction of percent out-of-state and the state Herfindahl measures with qualitatively similar results.

³⁶ We find qualitatively similar results when we estimate these effects using interactions of the four quartiles with national risk as well as interactions of the four quartiles with state-level risk (an overidentified specification).

³⁷ This is largely consistent with what is known about the causal effect of attending college out of state. Groen (2004) finds a modest impact of state of college attendance on early career location. Parey and Waldinger (2011) find a somewhat larger effect of studying abroad on subsequent international labor mobility in Europe.

they do not suggest that the causal impact of higher education on subsequent migration is higher for natives of states in which the college system enrolls a more geographically diverse set of students.

VII. Conclusion

This paper examines the causal impact of a college education on the likelihood of making a lasting, long-distance move out of one's birth state by early- to mid-adulthood. We exploit changes in national and state-level risk of induction into the Armed Forces during the Vietnam conflict as a source of exogenous variation in the years of postsecondary schooling. Using a 5% sample from the 1980 Census, we show that this variation increased migration rates substantially among affected cohorts. It also significantly increased the probability that a young man completed some postsecondary education. When we use induction risk to identify the causal effect of higher education on subsequent migration, we find that a college education significantly increases the probability of a long-distance move. Specifically, our 2SLS estimates indicate that an additional year of college increases the probability of living outside one's birth state by 0.03 to 0.09 percentage points, which is a broad range representing 10 to 25 percent of the average probability of 0.36 in this variable for men in our sample. Though these estimates are not significantly different from our OLS estimates, they suggest that selection is unlikely to explain the large increases in migration rates associated with a college education.

What mechanisms might explain the relationship between college and migration? Our preliminary investigation of this question appears to rule out a large role for two major features of the college experience: the probability of attending college at a distance from home and the geographic diversity of one's peer students. Instead, other mechanisms may be more important in explaining why college education has the observed effect on mobility. These include either direct mechanisms, such as general cognitive skills acquired while in college, or indirect channels, such as entry into a more geographically integrated labor market for college-acquired skills and degrees.

While these conclusions are tentative, we nevertheless believe they are a helpful step in understanding the nature of the relationship between higher education and migration. Future research should investigate the contributions of various mechanisms to the greater mobility observed among the college educated.

These findings also have important implications for the broader literature on returns to education. Viewing migration as an individual response to labor market disequilibria, the causal link between education and migration suggests that education may confer, in addition to other skills, the ability to deal with disequilibrium. As shown by Wozniak (2010) and Bound and Holzer (2000), migration can have a direct impact on individual earnings by allowing individuals to migrate in response to negative economic shocks, and that highly educated workers are more likely to make such moves. Indeed, we also observe these patterns for our sample of men from the Vietnam generation.³⁸ Consequently, our evidence does suggest that migration is an important return to a college education.

³⁸ Unfortunately, we are not able to use our identification strategy to show the presence of such benefits due to data limitations. However, we can confirm that more educated men are more likely to move out of states with negative economic shocks between 1970 and 1980 in simple OLS regressions.

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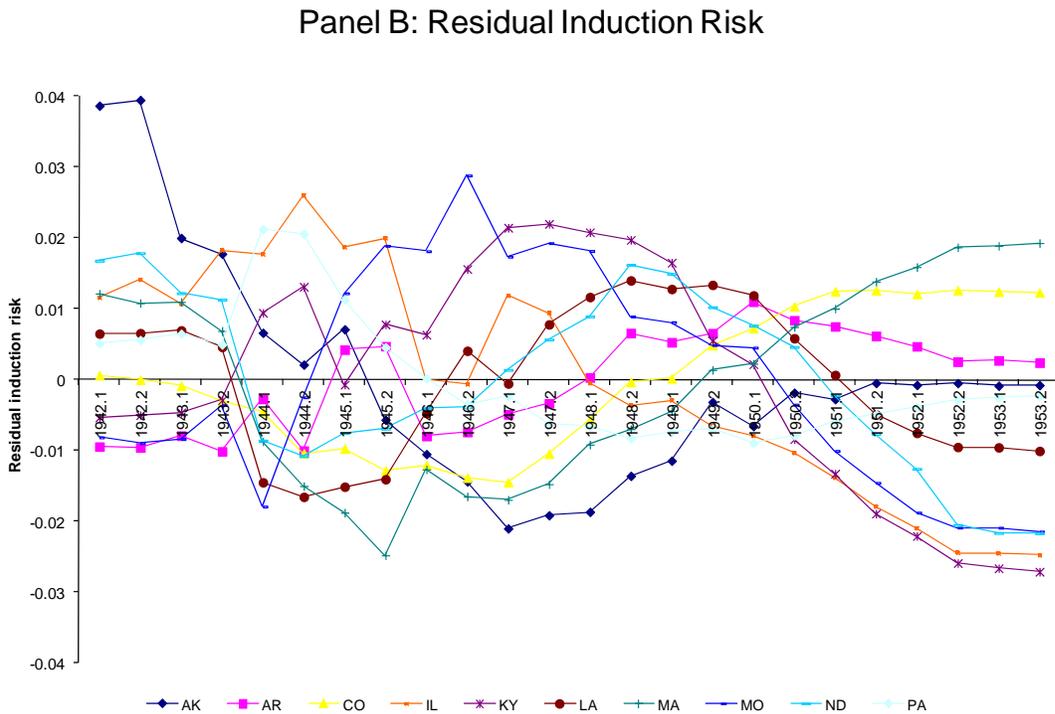
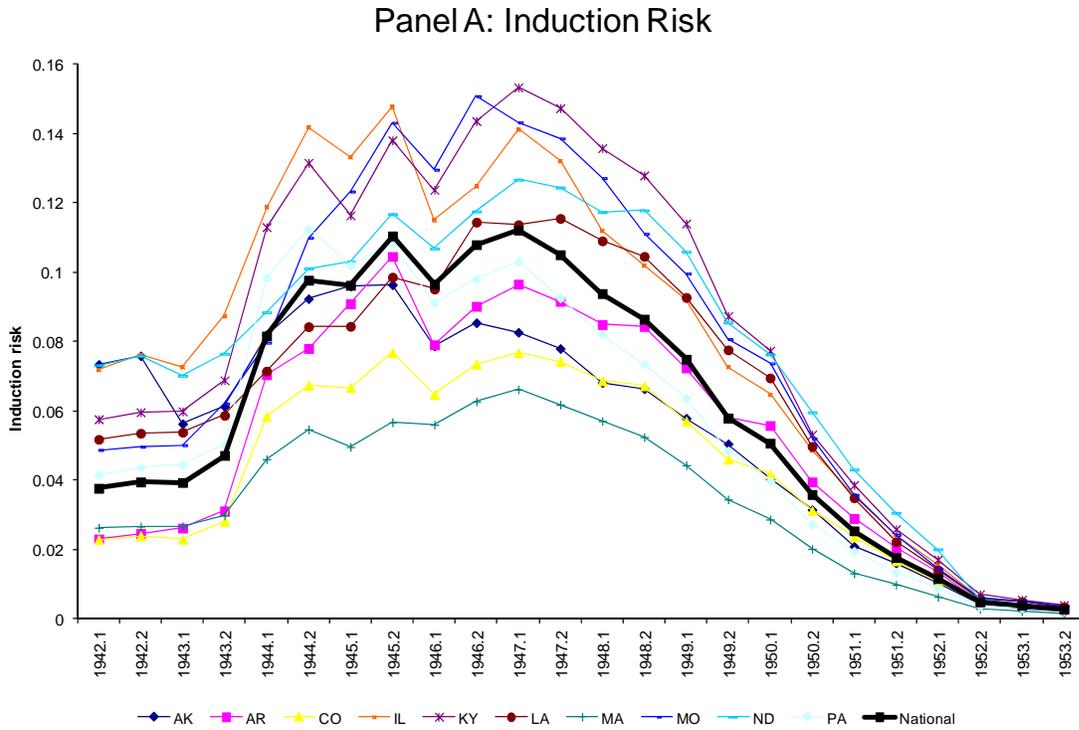
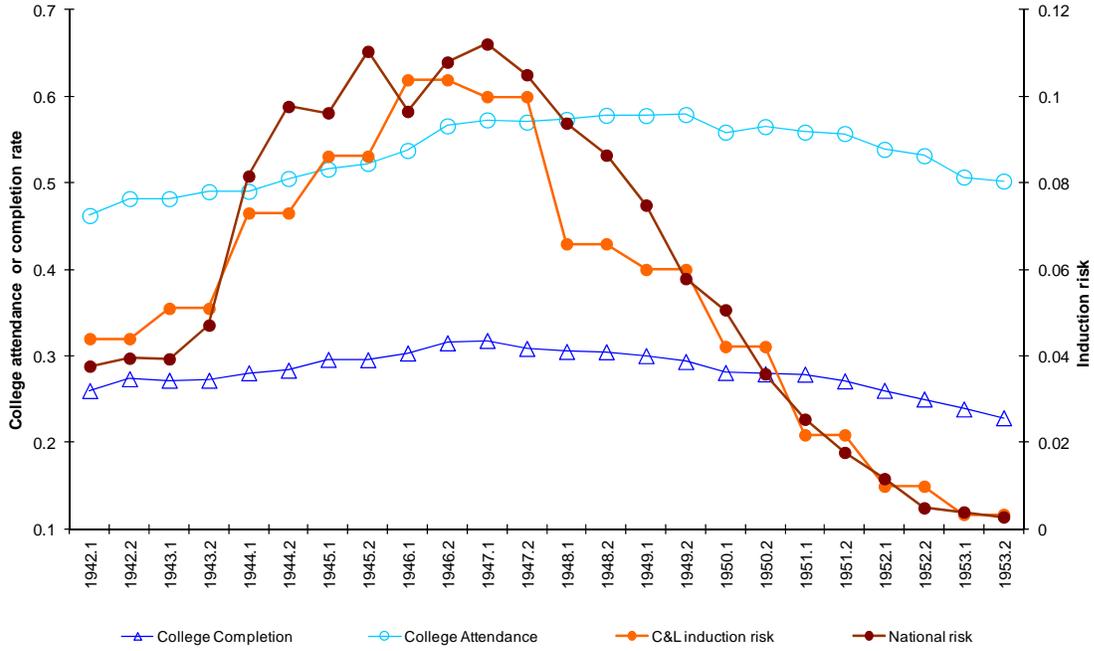


Figure 1. Birth state – birth cohort variation in induction risk. Panel A plots state risk as defined in (ii). Panel B plots (ii) adjusted for birth state fixed effects, birth year trend, and national risk, or as defined in (iii).

Panel A: Induction Risk and College Attendance/Completion



Panel B: Induction Risk, Residual Migration, and Education

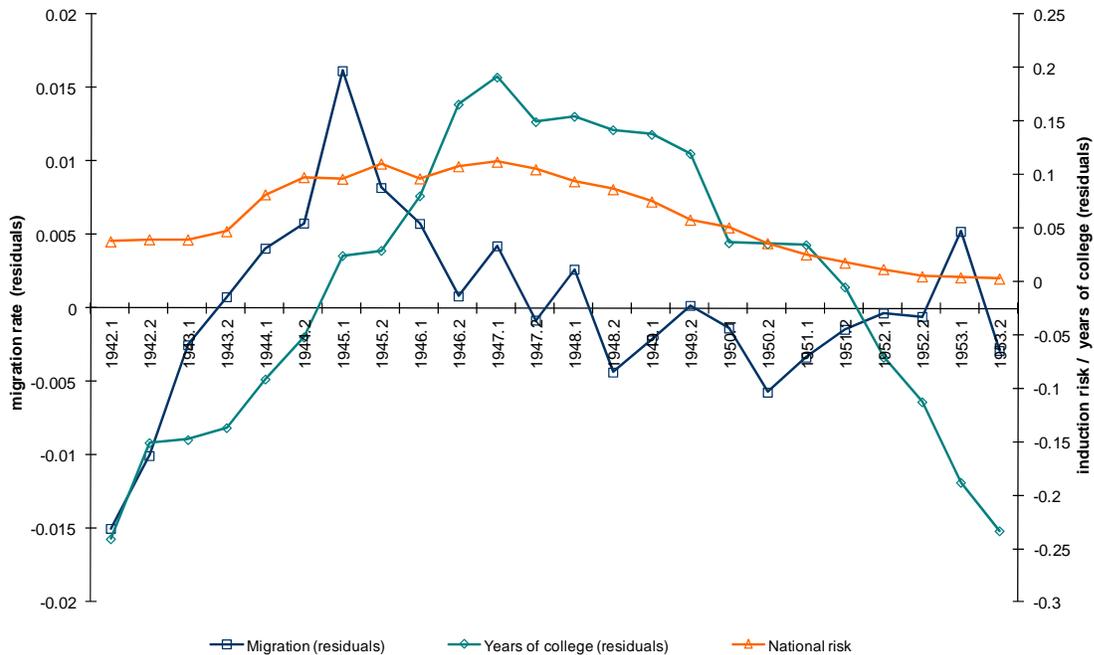
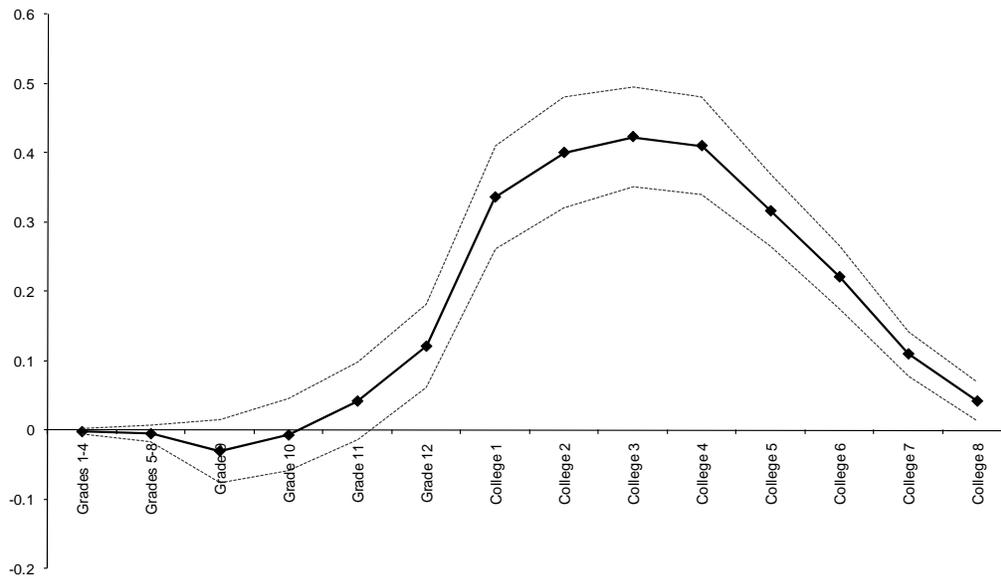


Figure 2. Panel A plots shares of each birth year cohort with 1+ and 4+ years of post-secondary schooling (left axis) and induction risk defined in Equations (i) and (ii) (right axis). Panel B plots birth cohort’s average years of schooling and migration rate adjusted for state of birth fixed effects and birth cohort trend as well as national risk as defined in (ii) (left axis) and national risk (right axis).

Panel A: Estimated Effect of National Risk



Panel B: Estimated Effect of State-level Risk

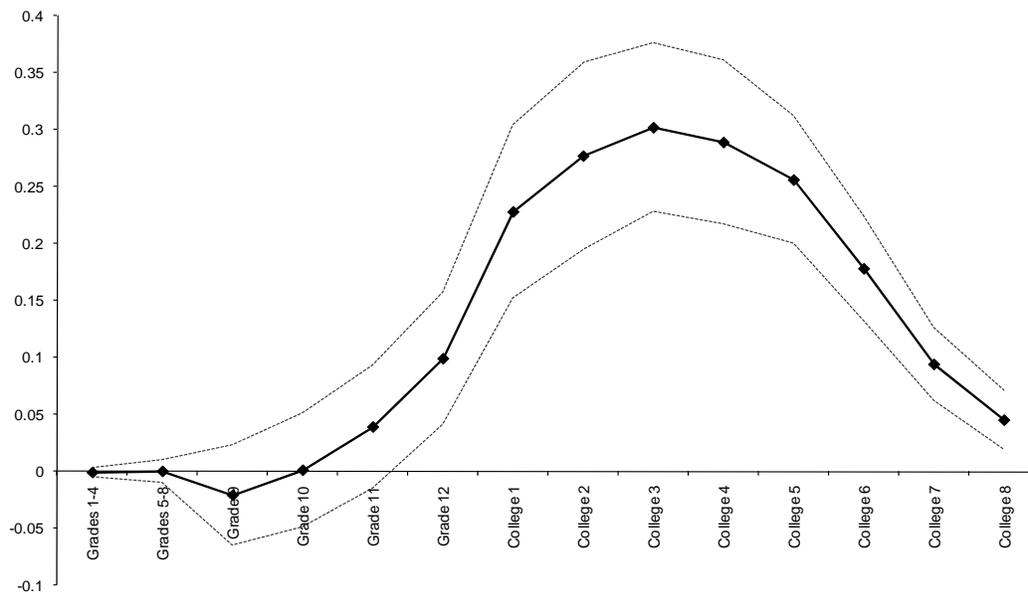
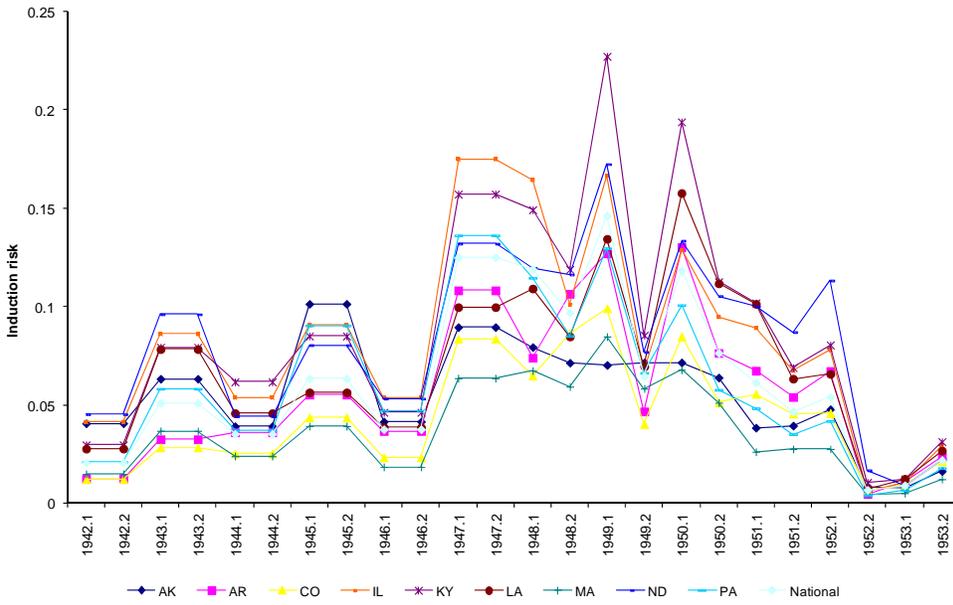
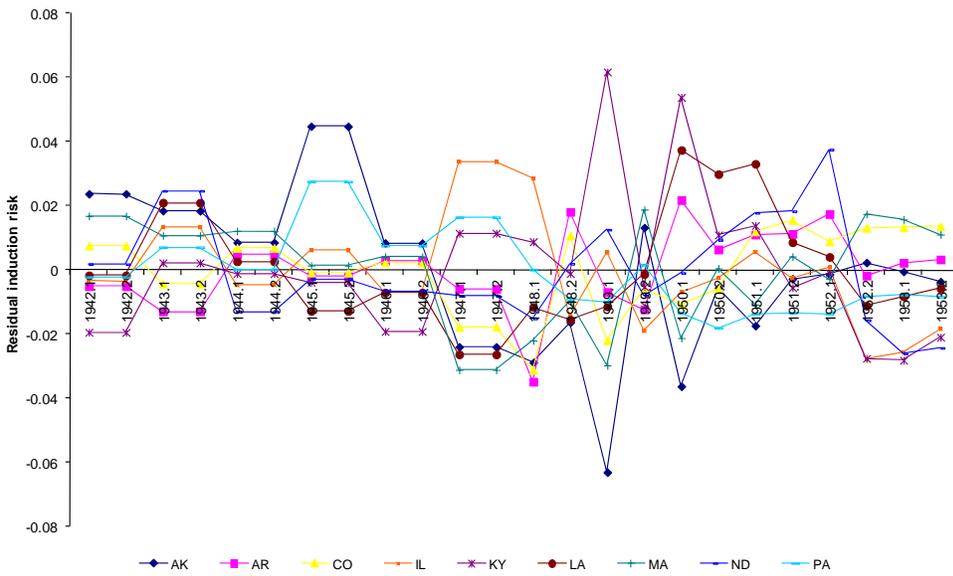


Figure 3. Coefficients and standard errors from OLS regressions of dummy variables for completed education of x-axis grade and higher, inclusive, on national risk and state risk as defined in Equations (iii) and (ii), respectively.

Panel A: Induction Risk (19-year olds only)



Panel B: Residual Induction Risk (19-year olds only)



Appendix Figure 1. Birth state – birth cohort variation in induction risk for 19 year-olds only.

Table 1: Summary Statistics for 1980 Census

	Mean	Standard deviation	Min	Max	Observations
Living outside of birth state	0.358	0.077	0.150	0.838	1200
Years of schooling	13.425	0.631	11.353	14.780	1200
Years of college	1.853	0.415	0.758	3.068	1200
College completion	0.281	0.068	0.050	0.448	1200
College attendance	0.537	0.097	0.210	0.804	1200
Veteran status	0.329	0.118	0.092	0.667	1200
National induction risk (6 month)	0.059	0.038	0.003	0.112	1200
State-level induction risk (6 month)	0.059	0.041	0.001	0.153	1200
Age	31.412	3.414	26.000	37.557	1200
Black	0.089	0.097	0.000	0.496	1200
Other nonwhite	0.045	0.088	0.000	0.853	1200
Log cohort size (birth year level)	10.298	0.237	9.835	10.518	1200
Log cohort size (birth-year-state level)	6.759	0.822	2.398	8.039	1200
Employment/population (birth year level)	375.57	18.65	346.26	397.26	1200
Employment/pop'n (birth year and birth state)	374.04	64.79	269.33	1018.51	1200

Notes: Data are from the 5% sample of the 1980 U.S. Census, available from IPUMS. All specifications are restricted to men born between 1942 and 1953. Uncollapsed sample size is 793,704. Cohort size is derived from the 1960 Census. Mobility is equal to 1 if respondent resides outside his state of birth at the time of the Census, and 0 otherwise. The employment-population ratio is defined for the year in which individuals are 19 years of age. Log cohort size is defined as the logarithm of the number of respondents to the 1960 Census in the corresponding year of birth.

Table 2: First-Stage Estimates of Induction Risk

Panel A: National and state-level induction risk, entered <i>separately</i>						
<i>dependent variable:</i>	<i>years of college</i>			<i>years of college</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
National induction risk	3.689*** [0.335]	3.269*** [0.257]	3.237*** [0.256]			
State-level induction risk				3.284*** [0.297]	2.946*** [0.273]	2.942*** [0.265]
Trend	X	X	X	X	X	X
Demographic controls	X	X	X	X	X	X
Cohort size		X	X		X	X
Employment rate		X	X		X	X
Birth region trends			X			X
F-test on IV	121.1	162.2	159.9	122.5	116.4	122.8
Panel B: National and state-level induction risk, entered <i>together</i>						
<i>dependent variable:</i>	<i>years of college</i>			<i>veteran status</i>		
	(1)	(2)	(3)	(4)	(5)	(6)
National induction risk	5.127*** [0.794]	5.164*** [0.498]	4.537*** [0.536]	1.327*** [0.207]	1.296*** [0.172]	1.357*** [0.140]
State-level induction risk	-1.440** [0.704]	-1.804*** [0.420]	-1.240*** [0.446]	0.743*** [0.156]	0.629*** [0.119]	0.581*** [0.082]
Trend	X	X	X	X	X	X
Demographic controls	X	X	X	X	X	X
Cohort size		X	X		X	X
Employment rate		X	X		X	X
Birth region trends			X			X
A-P F for multivariate endogenous variables	13.5	41.7	21.4	92.6	193.8	260.5

Notes: Robust standard errors in brackets, clustered by six month birth intervals in Panel A and by birth state and six month birth interval (two-way, multilevel) in Panel B. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent level, respectively. Data are from the 5% sample of the 1980 U.S. Census, available from IPUMS. All specifications are restricted to men born between 1942 and 1953. Number of observations is 1200 in each regression. Trend is a linear trend in age. Demographic controls include race (white, black, other) and birth state fixed effects. Cohort size is derived from the 1960 Census and defined at the birth year-level in columns 1, 2, and 3 and birth year and birth state in columns 4, 5, and 6. Employment rate is the employment to population ratio, also defined at the birth year-level in columns 1, 2, and 3 and birth year and birth state in columns 4, 5, and 6.

Table 3: Reduced-Form Estimates of Induction Risk on Migration

dependent variable: living outside state of birth (mean = 0.358)

	(1)	(2)	(3)	(4)	(5)	(6)
National induction risk	0.100*** [0.033]	0.132*** [0.027]	0.132*** [0.028]	0.272** [0.131]	0.270** [0.126]	0.281** [0.121]
State-level induction risk				-0.173 [0.116]	-0.153 [0.113]	-0.166 [0.110]
Trend	X	X	X	X	X	X
Demographic controls	X	X	X	X	X	X
Cohort size		X	X		X	X
Employment rate		X	X		X	X
Birth region trends			X			X
R ²	0.92	0.92	0.92	(a)	(a)	(a)

Notes: Robust standard errors in brackets, clustered by six month birth-year interval in (1)-(3) and by birth state and six month birth interval (two-way, multilevel) in (4)-(6). ***, **, and * indicate statistical significance at the 1, 5, and 10 percent level, respectively. Data are from the 5% sample of the 1980 U.S. Census, available from IPUMS. All specifications are restricted to men born between 1942 and 1953. Number of observations is 1200 in each regression. Trend is a linear trend in age. Demographic controls include race (white, black, other) and birth state fixed effects. Cohort size is derived from the 1960 Census and defined at the birth year-level in columns 1, 2, and 3 and birth year and birth state in columns 4, 5, and 6. Employment rate is the employment-to-population ratio, also defined at the birth-year level in columns 1, 2, and 3 and birth year-state in columns 4, 5, and 6. (a) To facilitate multilevel clustering, estimates in Panel B obtained from ivreg2 package, which does not report R-squared.

Table 4: OLS and IV Estimates for the Impact of Years of Schooling on Migration

dependent variable: living outside state of birth (mean = 0.358)

Panel A: IV with national-level induction risk

	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Years of college	0.018*** [0.005]	0.031*** [0.005]	0.034*** [0.005]	0.027*** [0.009]	0.040*** [0.008]	0.041*** [0.008]
Trend	X	X	X	X	X	X
Demographic controls	X	X	X	X	X	X
Cohort size		X	X		X	X
Employment rate		X	X		X	X
Birth region trends			X			X
R ²	0.92	0.92	0.92			

Panel B: IV with national and state-level induction risk

	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Years of college	0.021** [0.008]	0.029*** [0.009]	0.029*** [0.008]	0.076* [0.046]	0.066* [0.038]	0.090** [0.045]
Veteran Status	-0.01 [0.015]	0.00 [0.011]	-0.001 [0.012]	-0.087 [0.073]	-0.054 [0.059]	-0.094 [0.072]
Trend	X	X	X	X	X	X
Demographic controls	X	X	X	X	X	X
Cohort size		X	X		X	X
Employment rate		X	X		X	X
Birth region trends			X			X
R ²	(a)	(a)	(a)			

Notes: Robust standard errors in brackets, clustered by six month birth intervals in Panel A and by birth state and six month birth interval (two-way, multilevel) in Panel B. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent level, respectively. Data are from the 5% sample of the 1980 U.S. Census, available from IPUMS. All specifications are restricted to men born between 1942 and 1953. Number of observations is 1200 in each regression. Trend is a linear trend in age. Demographic controls include race (white, black, other) and birth state fixed effects. Cohort size is derived from the 1960 Census and defined at the birth year-level in columns 1, 2, and 3 and birth year and birth state in columns 4, 5, and 6. Employment rate is the employment-to-population ratio, also defined at the birth-year level in columns 1, 2, and 3 and birth year-state in columns 4, 5, and 6. Columns 4, 5, and 6 of Panel A use national-level induction risk to instrument for years of schooling. Columns 4, 5, and 6 of Panel B use national and state-level induction risk at six month intervals to instrument for schooling and veteran status. (a) To facilitate multilevel clustering, estimates in Panel B obtained from ivreg2 package, which does not report R-squared.

Table 5: Placebo Regressions

*dependent variable: living outside state of birth***Panel A: Reduced-Form Estimates of Induction Risk on Childhood Migration (1960 Census)**

	(1)	(2)	(3)	(4)	(5)	(6)
National induction risk	0.041 [0.032]	0.088*** [0.026]	0.086*** [0.026]	0.055 [0.109]	0.075 [0.106]	0.018 [0.102]
State-level induction risk				-0.014 [0.110]	-0.006 [0.121]	0.046 [0.113]
Trend	X	X	X	X	X	X
Demographic controls	X	X	X	X	X	X
Cohort size		X	X		X	X
Employment rate		X	X		X	X
Birth region trends			X			X
R ²	0.02	0.02	0.02	0.02	0.02	0.02
Mean of dep. variable	0.163	0.163	0.163	0.163	0.163	0.163

Panel B: IV Estimates for Schooling on Migration for older cohorts born 1930-1941(1980 Census)

	2SLS with national risk			2SLS with national & state-level risk		
	(1)	(2)	(3)	(4)	(5)	(6)
Years of schooling	0.047 [0.212]	-0.568 [5.556]	0.187*** [0.029]	0.823 [101.358]	-0.144 [0.236]	-0.15 [0.457]
Veteran Status				-8.441 [969.336]	-0.055 [0.236]	-0.086 [0.225]
Trend	X	X	X	X	X	X
Demographic controls	X	X	X	X	X	X
Cohort size		X	X		X	X
Employment rate		X	X		X	X
Birth region trends			X			X
Mean of dep. variable	0.396	0.396	0.396	0.396	0.396	0.396

Notes: Robust standard errors in brackets, clustered by birth-year in columns 1, 2, and 3 and birth year-state in columns 4, 5, and 6. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent level, respectively. Data in Panel A is from the 1% sample of the 1960 U.S. Census; data in Panel B is from the 5% sample of the 1980 U.S. Census. Specifications in Panel A are restricted to men born between 1942 and 1953; specifications in Panel B are restricted to men born between 1930 and 1941. Number of observations is 1200 in each regression. Trend is a linear trend in age. Demographic controls include race (white, black, other) and birth state fixed effects. Cohort size is derived from the 1960 Census for both panels and defined at the birth year-level in columns 1, 2, and 3 and birth year and birth state in columns 4, 5, and 6. Employment rate is the employment-to-population ratio, also defined at the birth-year level in columns 1, 2, and 3 and birth year-state in columns 4, 5, and 6. Columns 1, 2, and 3 of Panel B use national-level induction risk to instrument for years of schooling while columns 4, 5, and 6 use national and state-level induction risk to instrument for schooling and veteran status.

Table 6: Potential Mechanisms

dependent variable: living outside state of birth

	Out-of-State College Going (by quartile)		Geographic Diversity of State Colleges (by quartile of Herfindahl Index)	
	OLS (1)	2SLS (2)	OLS (3)	2SLS (4)
Years of schooling*Quartile 1	.028 (.005)***	.031 (.065)	.045 (.003)***	.107 (.059)*
Years of schooling*Quartile 2	.044 (.002)***	.073 (.054)	.040 (.003)***	.088 (.044)**
Years of schooling*Quartile 3	.044 (.002)***	.078 (.066)	.037 (.003)***	.145 (.060)**
Years of schooling*Quartile 4	.047 (.003)***	.061 (.060)	.040 (.005)***	.097 (.049)**
Veteran Status	.057 (.005)***	-.042 (.097)	.056 (.005)***	-.123 (.078)
Trend	X	X	X	X
Demographic controls	X	X	X	X
Cohort size	X	X	X	X
Employment rate	X	X	X	X
Birth region trends	X	X	X	X
Geographic diversity controls	X	X		
Out-of-state college controls			X	X
R ²	0.06		0.06	
Mean of dep. Variable				

Notes: Robust standard errors in brackets, clustered by birth year and birth state. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent level, respectively. Data are from the 5% sample of the 1980 U.S. Census, available from IPUMS. All specifications are restricted to men born between 1942 and 1953. Number of observations is 794,331 in each regression. Columns 2 and 4 use national-level induction risk interacted with quartile and state-level induction risk to instrument for years of schooling interacted with quartiles and for veteran status. Trend is a linear trend in age. Demographic controls include race (white, black, other) and birth state fixed effects. Cohort size is derived from the 1960 Census and defined at the birth year and birth state level. Employment rate is the employment-to-population ratio, also defined at the birth year-state. Geographic diversity and out-of-state college controls include linear and quadratic trends of these variables. Correlation between percent-out-of-state and state Herfindahl is 0.46; between quartiles of these two variables is 0.36

Out-of-state college going quartiles: Quartile 1 (AL, CA, IN, LA, MI, MN, MS, NC, SC, TN, TX, UT); Quartile 2 (CO, GA, KS, KY, MA, ME, NY, OH, OK, OR, PA, WA, WI); Quartile 3 (AR, AZ, FL, HI, IA, IL, MD, MO, MT, NE, SD, VA, WV); Quartile 4 (AK, CT, DC, DE, ID, ND, NH, NJ, NM, NV, RI, VT, WY). Herfindahl index quartiles: Quartile 1 (AK, AZ, DC, DE, FL, ME, NH, NM, NV, RI, VT, WY); Quartile 2 (CO, CT, HI, ID, MD, MT, ND, OR, SC, SD, UT, VA, WV); Quartile 3 (AL, AR, GA, IA, IN, KS, KY, LA, MO, MS, NE, TN, WA); Quartile 4 (CA, IL, MA, MI, MN, NC, NJ, NY, OH, OK, PA, TX, WI).

Appendix Table 1: OLS and 2SLS Estimates for the Impact of College Attendance and Completion on Migration

dependent variable: living outside state of birth (mean=0.358)

Panel A: College Attendance

	OLS			2SLS with national risk			2SLS with national and state-level risk		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Attendance	0.060**	0.140***	0.144***	0.125***	0.200***	0.201***	0.417	0.357*	0.544*
	[0.023]	[0.021]	[0.024]	[0.048]	[0.044]	[0.044]	[0.255]	[0.206]	[0.280]
Trend	X	X	X	X	X	X	X	X	X
Demographic controls	X	X	X	X	X	X	X	X	X
Cohort size		X	X		X	X		X	X
Employment rate		X	X		X	X		X	X
Birth region trends			X			X			X
R ²	0.91	0.92	0.92						

Panel B: College Completion

	OLS			2SLS with national risk			2SLS with national and state-level risk		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Completion	0.123***	0.187***	0.198***	0.163***	0.239***	0.241***	0.358*	0.321*	0.427**
	[0.029]	[0.025]	[0.029]	[0.056]	[0.046]	[0.047]	[0.206]	[0.181]	[0.208]
Trend	X	X	X	X	X	X	X	X	X
Race & Birth state	X	X	X	X	X	X	X	X	X
Cohort size		X	X		X	X		X	X
Employment rate		X	X		X	X		X	X
Birth region trends			X			X			X
R ²	0.92	0.92	0.92						

Notes: Robust standard errors in brackets, clustered by six month birth intervals in (1)-(6) and by birth state and six month birth interval (two-way, multilevel) in (7)-(9). ***, **, and * indicate statistical significance at the 1, 5, and 10 percent level, respectively. Data are from the 5% sample of the 1980 U.S. Census, available from IPUMS. All specifications are restricted to men born between 1942 and 1953. Number of observations is 1200 in each regression. Veteran omitted from specifications in (1) – (6); veteran included as an endogenous variable in (7)-(9). Trend is a linear trend in age. Demographic controls include race (white, black, other) and birth state fixed effects. Cohort size is derived from the 1960 Census and defined at the birth year-level in columns 1-6 and birth year and birth state in columns 7-9. Employment rate is the employment to population ratio, also defined at the birth year-level in columns 1-6 and birth year-state in columns 7-9.