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# The Long-Term Effects of College Education on Morbidities: New Evidence From the Pre-Lottery Vietnam Draft [1]

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*Abstract*

The easy availability of college deferments in the pre-lottery Vietnam draft, followed by their abrupt elimination by the draft lottery, creates a discontinuity in educational attainment between males of cohorts 1946–1950 and their immediately adjacent cohorts. Exploiting this discontinuity as a means of identifying quasi-random variation in schooling, we find the return to college education includes a reduction in obesity and its co-morbidities such as hypertension and adult-onset diabetes.

*JEL codes:* I12, I18, I21, I28.

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## 1. Introduction

Education and health are the two most basic forms of human capital stock. Better education has always been associated with better health. Of great interest to health policy is whether better education causes better health, and specifically, whether education reduces major health risks such as obesity, hypertension and diabetes, which have become serious public health issues with enormous economic costs. Nearly one-third of American adults (over 60 million) and one-sixth of American children (over 9 million) are obese. Obesity-related medical expenditures were $78.5 billion in 1998, accounting for 9.1 percent of total annual U.S. medical expenditures (Finkelstein, Fiebelkorn, and Wang 2003). Diabetes is the 6th leading cause of death in the United States, and its age-adjusted death rate grew from 17.6 in 1981 to 25.3 in 2001, per 100,000 populations[2].

There is a need for public policy to reduce health care expenditures and improve American health, making it economically important to identify factors that have causal impact on major health risks. While the largest gradient in health used to occur between high school graduates and high school dropouts, we present evidence of a potentially larger gradient that occurs between high school graduates and college graduates. Another troubling yet unexplained fact is the steady decline in males' college enrollment rate since the 1970s, whose consequence on the rate of growth of human capital accumulation and on the United State economy is predicted to be permissive (Card 2000).

Because of the correlation between what drives one to pursue postsecondary schooling and what makes him/her healthy it is difficult to obtain a consistent estimate of the effect of education on health, however a consistent estimate may be obtained if a quasi-natural experiment generates instrument variables that can be used to reduce this correlation[3]. In this paper, we exploit an exogenous source of variation in educational attainment that is induced by the rules in the pre-lottery period combined with the drastic change in the rules brought about by the lottery during the Vietnam era, and obtain new evidence of a causal effect of college-level schooling on health. Our findings shed light on potential policy-making to reduce obesity, hypertension and adult-onset diabetes.

During the Vietnam era, men became eligible for the draft upon reaching the age of 19. During the pre-lottery peak draft years 1965–1969, draft-eligible men who wanted to avoid the draft could easily obtain college deferments by enrolling in college, and continue to obtain college deferments until they completed college or reached the age of 24. Because the age susceptible to being drafted correlated closely to a natural college age, some males, particularly of cohorts 1946–1950, chose to enroll in and/or remain in college specifically to obtain college deferments. Since Card and Lemieux (2001) first established that the Vietnam draft serves as a quasi-natural experiment of college education, the Vietnam draft has been exploited by several studies (Grimard and Parent 2005 and de Walque 2004) to examine the effect of education on smoking.

Our contribution is to exploit the institutional details of drafting rules and changes in drafting rules brought about by the draft lottery system to identify that cohorts 1942–1944 and 1951–1953 constitute valid counterfactuals to these most affected males—males of cohorts 1946–1950. During the pre-lottery Vietnam draft years, males of cohorts of 1942–1944 (hereafter, older control cohorts) faced a low risk of induction during the years they were college

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[2] Data source is Death by Major Causes, 1960-2002 (age-adjusted death rates per 100,000 population), the CDC, 2005. The first five leading causes of deaths are heart diseases, cancers, cerebro-vascular diseases, chronic lower respiratory diseases, and accidents.

aged, and had a low incentive to use college deferments to avoid the draft. With the high risk of induction and easily obtainable college deferments, males of cohorts 1946–1950 (hereafter, treatment cohorts), who were aged suitably for both the draft and college, had a high incentive to enroll in college to avoid the draft.

Males of cohorts 1951–1953 (hereafter, younger control cohorts) faced entirely different drafting rules and risks than males of the treatment cohorts: they had greatly reduced exposure to a greatly reduced number of inductions and college deferments were eliminated entirely. They had no incentive to enroll in college as a means of avoiding the draft[4]. Consequently, there is an exogenous discontinuity gap in college education: the "rise" in the treatment cohorts in contrast to older control cohorts and subsequent "fall" in younger control cohorts. If we assume the unobserved variables change continuously between males of adjacent cohorts, males of the older and younger control cohorts constitute valid counterfactuals to males of the treatment cohorts.

Using the National Health Interview Survey 1998–2003, we visually demonstrate the close association between induction risk, which could be eliminated by college deferments, and the "rise and fall" in college education. The visual illustration of the close association between the "rise and fall" in college education and the "fall and rise" in health outcomes such as smoking and obesity provides a transparent identification of the impact of education on health. Our identification condition relies on the assumption that, in the absence of the pre-lottery draft, the male-female difference in college education rate would follow a smooth trend, and the deviation from the trend in the treatment cohorts is attributed to the pre-lottery draft. Although the treatment cohort males also used means other than college deferments to avoid the pre-lottery

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[3] For example, Arkes 2001, Adams 2002, Currie and Moretti 2003, de Walque 2004, Grimard and Parent 2005, and Lleras-Muney 2002 employ the instrument variable approach.

[4] Similar results have also been reported in Card and Lemieux (2001) and Angrist and Kruger (1992), i.e. draft-avoidance behavior had little or no effect on the average schooling outcomes for men born after 1950.

draft, historical evidence that is presented in the robustness checks section suggests that such other reasons do not explain the discontinuity in educational outcomes and that our identification assumption is reasonable.

We obtain our main results by implementing a difference-in-difference estimator, where we use an indicator for males of the treatment cohorts as the instrument variable for education, and age-based draft eligibility as the instrument variable for veteran status. Our preferred specification uses females as controls controlling for no trend in gender difference in education and health outcomes using females as controls. Using the National Health Interview Survey 1998–2003, we find the pre-lottery Vietnam draft caused approximately a 3.1% increase in college enrollment and 3.7% increase in college completion. We find that college completion reduces obesity, and may also reduce hypertension and adult-onset diabetes.

In the remainder of the paper, Section 2 reviews the literature. Section 3 describes the quasi-natural experiment of college education. Section 4 presents visual identification and the specification, with results in Sections 5 and robustness checks in Section 6. Section 7 discusses the health benefits of college education and Section 8 concludes.

## 2. Education-Health Literature

The strong and positive relationship between education and health can be interpreted in several ways: an increase in education improves health, better health increases educational attainment, or an unknown "third variable" affects both education and health in the same direction, such as genetic characteristics and/or rate of time preference (Fuchs 1982). An ideal but implausible experiment to measure the effect of education on health would be to randomly assign educational attainment levels while keeping other factors constant; the corresponding improvement in health, if any, would represent the overall positive effect of education on health. Random assignment would eliminate the correlation between education and unobserved health-related factors. In the absence of a true experiment (due in part to the ethical unacceptability of creating one), a natural or quasi-natural experiment may generate instrument variables that can be used to reduce this correlation (Arkes 2001, Adams 2002, Currie and Moretti 2003, De Walque 2004, Grimard and Parent 2005, and Lleras-Muney 2002).

Lleras-Muney (2002) and Adams (2002) both use compulsory school law and find that education reduces mortality (Lleras-Muney) and increases the probability of good health (Adams). Arkes (2001) uses the state unemployment rate during a person’s teen years as the instrument for schooling and argues that a higher unemployment rate increases incentives to attain more education because it reduces the opportunity cost of attending school. He finds that more years of completed schooling reduce the probability of having work-limiting health conditions. Currie and Moretti (2003) construct an education availability measure based on the information of new college openings and use it as an instrument for education; they find that as a woman’s education level increases, so does her child’s birth weight.

Most relevant to this study is De Walque (2004) and Grimard and Parent (2005); both studies use Vietnam draft avoidance behavior to instrument education andfind education reduces smoking initiation, but find little evidence that education has any impact on smoking cessation. We have made two major advancements to De Walque and Grimard and Parent. First, we recognize and empirically verify the discontinuity in college education rates created by the drafting rule changes, above and beyond what is documented in Card and Lemieux (2004). We exploit this discontinuity in the research design to identify the effect of college education on health, while the other two studies do not. Consequently, we implement our empirical analysis by comparing cohorts immediately before and after the point of discontinuity, and carefully identify the valid counterfactuals to the treatment cohorts. The long span of cohorts used in other two studies—birth cohorts of 1937-1956 in De Walque and birth cohorts of 1935-1974 in Grimard and Parent (2005)—is likely to introduce cohort bias[5].

Second, we formally take into account of the effect of veteran status on educational attainment and health outcomes, and model veteran status as endogenous. Grimard and Parent obtain their main results focusing on non-veterans, and it is difficult to interpret their results because the selection into veteran status varies by cohort. De Walque recognizes the effect of veteran status on smoking, but fails to account for the endogeneity of veteran status; this limitation is recognized in the paper.

Third, besides smoking initiation and cessation, we examine the effect of college education on obesity, hypertension and adult-onset diabetes. These major health risks and diseases have become increasingly prevalent and even reached epidemic proportions in the recent decades, and medical treatment for adult-onset diabetes, for example, is extremely costly. Existing studies on obesity have not examined the causal effect of education on obesity or obesity-related co-morbidities such as hypertension or diabetes (Lakdawalla and Philipson 2002; Cutler, Glaeser, and Shapiro 2003; Chou, Grossman, and Saffer 2004).

We focus on college education, rather than high school graduation, because new evidence shows that the largest gradient in many health outcomes occurs between high school graduates and college graduates, rather than between high school dropouts and high school graduates. Data from the 2003 Medical Expenditure Panel Survey (MEPS) shows that among non-institutionalized adults, 29.9% of high school dropouts and 27.0% of high school graduates were

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[5] Only U.S. born citizens were subject to draft, however, De Walque apparently did not take this essential information into account in determining draft eligibility.

current smokers, while 14.6% of adults with some college or higher education were current smokers. The changing gradient is probably most apparent in the case of obese adults. Data from the 1987 MEPS reports that 18.9% of high school dropouts, 13.9% of high school graduates, and 10.9% of adults with some college or higher education were obese. Data from the 2001 MEPS presents a disturbingly different picture: while adults became more likely to be obese across all education levels, the gap between high school dropouts and high school graduates virtually disappeared, and the gap between adults with college education and adults without enlarged. In the 2001 MEPS, 27.1% of high school dropouts and 27.2% of high school graduates were obese adults, while 20.8% of adults with college education were obese[6]. Data from the National Health Interview Surveys 1998-2003, shown in Table 1, provides additional evidence of the upward shift of the education-health gradient, most apparently in the cases of smoking and obesity.

There may be three potential mechanisms through which the impact of education on health operates: productivity, income and substitution effects[7]. Productivity effect refers to the increased productivity of health inputs: education may increase the allocative efficiency in health inputs (Grossman 1972, Grossman and Kaestner 1998) and/or input-neutral productive efficiency in health production (Grossman 1972). Income effect refers to the protection effect of higher income resulting from additional years of schooling. Substitution effect captures the fact that schooling may raise the value of health and longevity, and lowers the time preference rate, even when income and productivity are held constant.

The impact of college education on smoking, obesity and other health risks may operate through productivity, income and substitution effects. While income and substitution effects are apparent, productivity effect may provide an additional mechanism. College education, and

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[6] These evidences are obtained from the statistical reports by the Agency for Health Care and Quality, accessed in January 2006. http://www.meps.ahrq.gov/PrintProducts/PrintProdLookup.asp?ProductType=StatisticalBrief.

college completion particularly, teaches and trains people to develop the kind of determination required in smoking cessation because of the addictive nature of tobacco. It may be the general principle and habit of learning to learn that comes from college education, and college completion particularly, that makes the more educated more efficient producers of their own good health than the less educated. Learning to learn enables an individual to think critically and process information more discriminately, better understand how research is done, and even conduct his or her own research.

As technology becomes more advanced, and information becomes more abundant, diverse, and confusing, just knowing how to read may not be sufficient. Using nutritional labeling as an example, it is commonly understood that reading labels on nutritional packages is important, and this would explain health differences between high school graduates and high school dropouts. We argue that just reading nutritional labels on food packages may no longer be sufficient; for example, it may take more than a high school education to discern the relevance between the different forms of fats and oils. Understanding these differences may require additional resources to do research, and/or the ability to know how to reconcile or distinguish between information and disinformation. Vegetables are commonly thought to be a good choice, but there are different kinds of vegetables; for example, the more educated consume vegetables such as broccoli and the less educated may consider potato chips and French fries to equally healthy vegetables.

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[7] The author thanks an anonymous referee for suggestions these potential mechanisms.

## 3. A Quasi-Natural Experiment of College Education

### *3.1. The Pre-Lottery Vietnam Draft 1965–1969 and College Deferments*

During the Vietnam era, between 1965 and 1973, the Selective Service System comprised a nationwide network of 4,085 local draft boards, which were responsible for registering recruits, classifying them for deferment or selection, and forwarding those selected for pre-induction examination and induction[8]. The classification of recruits, operated under the Military Service Act of 1967, was the basic process of military manpower procurement through the Selective Service System. Deferment was an important type of classification, as it was used to protect some registrants from military service in the national interest[9]. The Military Service Act of 1967 states that college students in good standing may be deferred until receipt of undergraduate degree or age 24, whichever occurs first.

During 1965 through 1969, the local draft board could issue deferments for a variety of reasons including school attendance and the presence of spouse and children, or classify the registrant as "available for service" and require that he undergo a pre-induction physical. Men who passed the physical were liable to induction and could be ordered to report for duty at any time. The drafted men were mostly assigned to the Army and served typically three years.

The process of deciding which men were drafted was set by order of the President, with highest priority for "delinquents"—defined as those who failed to register or failed to report for the pre-induction physical; second priority went to volunteers; and third priority was for non-volunteers aged between 19 and 25. Local draft boards were able to fill their quotas from these

[8] This section draws largely from the various reports of the Selective Service Agency, which provided semi-annual reports to the Congress of the United States since the enactment of the Military Selective Service Act of 1967.
[9] National interests are such as holding the agricultural, industrial, and social structure of the nation in balance during peace or war. There were five classifications of deferments: fatherhood and dependency hardship, not qualified for military services due to physical and mental limitations, qualified only in time of war, National Guard and Reserves, and educational deferments for college and high school students.

three categories throughout the entire Vietnam era. Consequently, men who were able to maintain a deferment until their $26^{th}$ birthday could effectively avoid the draft, even though they were technically eligible for induction until age 35. Until 1968 men could apply for a graduate deferment and occupational or dependent deferments.

Table A1 in the Appendix shows that the peak years of induction were 1965–1969, and the greatest number of college deferments were during the same period. There were 1.731 million college deferments and 228,263 inductees in 1967. While induction numbers remained high in 1968–1969, college deferments increased to 1.772 million in 1968 and reached a record high of 1.789 million in 1969.

### *3.2. The Draft Lottery 1970–1973 and the Elimination of College Deferments*

The nation was concerned with the fairness of draft. In November 1969, the President authorized a draft lottery to establish the priority for induction using a random sequence number (RSN) that was randomly drawn based on birthday and independent of age. The first lottery, held on December 1, 1969, assigned RSNs to males of cohorts 1944–1950, which determined the draft order for 1970. Men were called into military service in order, starting with the lowest RSNs. In 1970, college deferments were retained but family and occupational deferments were eliminated.

The second lottery, held in July 1970, was applicable to males of cohort 1951, who were due to be inducted in 1971. In September 1971, all new college deferments were eliminated, though those currently enrolled could retain their deferments. The third lottery, held in August 1971, was applicable for males of cohort 1952 to be inducted in 1972. The fourth lottery was applied to males of cohort 1953 to be inducted in 1973. The draft ended in February 1973.

The introduction of the draft lottery system substantially changed the deferment and selection rules. With the draft lottery system, males of the younger control cohorts had an entirely different exposure to induction risk than males of earlier cohorts. They were at risk of induction for only a single year rather than the entire period between the ages of 19 and 25; in addition, the annual induction numbers declined drastically in 1971–1973 (Table A1). For these three reasons (elimination of college deferment, reduced induction numbers, and reduced duration of draft exposure), younger control cohorts had substantially reduced incentives to enroll in college to avoid the draft.

### *3.3. Educational Outcomes of the Treatment and Control Cohorts*

We use the National Health Interview Survey (NHIS) 1998–2003 to explore the difference in educational outcomes between the treatment and control cohorts[10]. Figure 1 displays the unadjusted college enrollment and completion rates of males and females along with the induction risk that could be eliminated by college deferments during the Vietnam War era[11]. The induction risk shows a large increase starting with cohort 1945, continuing until cohort 1950, and then sharply declining to nearly zero for the younger control cohorts. Parallel to the "rise and fall" in induction risk, males' college enrollment rate shows a sharp rise in the treatment cohorts, followed by a sharp fall in younger control cohorts, but this "rise and fall" is clearly absent in females of the same cohorts. Consequently, as shown in Figure 2, the

[10] NHIS is an annual nationwide probabilistic household survey of households conducted since 1957 that focuses on the civilian, non-institutionalized population in the United States. See http://www.cdc.gov/nchs/nhis.htm. Our base sample is restricted to U.S. born individuals because only male U.S. citizens were subject to be drafted during the Vietnam War, and therefore only male U.S. citizens were assigned to the treatment of using college enrollment as an effective method of draft avoidance. The choice of survey years is due to the data limitations in the NHIS 1969–1997, which do not contain birthplace information. We use the base sample throughout the analysis, unless specified otherwise.

unadjusted gender differences in college enrollment and completion rates are widened in the treatment cohorts compared to the older control cohorts, and followed by a sharp decline in the younger control cohorts. This visual evidence lends support to our identification assumption: in the absence of the pre-lottery Vietnam draft, the male-female differences in college enrollment and completion follow smooth trends, and we attribute the deviations from the smooth trends in the treatment cohorts to the effect of the pre-lottery Vietnam draft.

One concern about using females as controls is the potential crowding-out effect of the rise in males' college education. Assuming that the college education rate of females follows a smooth trend in the absence of a crowding-out effect, if the crowding-out effect were an important feature of the data, we would expect to see the college education rate of treatment cohort females to lie below the trend. Figures 1 show no evidence of below the trend deviation for the treatment cohorts' females, suggesting the absence of the crowding-out effect and supporting the use of females as controls.

We next take a closer look at how the pre-lottery draft shifts the distribution of educational attainment levels. Because the treatment cohort males had varying lengths of exposure to the draft, there should be heterogeneity in the increase in educational attainment levels. Males of birth cohort 1946, for example, would need to stay in college longer than males of cohort 1948 if they wanted to avoid the draft altogether. Consequently, we would expect that males of cohort 1946 were more likely to complete college than males of cohort 1948, compared to females in the same cohorts; and Figure 3 confirms that. The rise in college education rate—whether college enrollment, attainment of associate degree, or college graduation—is the greatest in cohort 1946, followed by cohort 1948, and the lowest for cohort 1949. As a validity

[11] We construct a measure of induction risk by first using the average number of inductions during the years when a cohort was naturally college-aged divided by cohort size and then adjusting it by the changes in drafting rules,

check, there is no such shift when comparing cohort 1951 to cohorts 1952–1953. The evidence in Figure 3 also confirms that our instrument variable is a valid instrument for the educational attainment levels that meet the monotonicity condition (Angrist and Imbens 1995).

Table 1 presents the sample means of selected health outcomes by educational attainment categories, confirming that better education is uniformly associated with decreased probability of hypertension, obesity, diabetes, and serious psychological distress

## 4. The Specifications

We consider the following relationships to identify and obtain accurate estimates of the impact of education on health:

$$y_i = \beta d_i + \beta_v v_i + X_i \beta_X + T \beta_T + \varepsilon_{iy} \qquad (1)$$

where *i* is the individual script; *y* denotes the health outcome dummy; *d* is a college education dummy; *v* is a veteran status dummy; *X* are observed variables; *z* and *w* are instrument variables for education and veteran status; *T* represent no trend, linear trend, or quadratic trend, in gender difference; and *ε* are error terms. We estimate (1) using two-stage least square and treating college education and veteran status as endogenous with their instruments discussed below.

We employ a linear probability model for college education, veteran status, and health outcome. College education is measured as college enrollment, attainment of associate degree, and college completion. Because both education and health outcome are binary variables, we implement a difference-in-difference (DID) estimator and an instrument variable (IV) estimator, and instruments *z* and *w* are defined as follows. In the DID estimator, we define z as a single indicator for the treatment cohort males as the instrument variable for education, and we control

brought about by the draft lottery, to affected cohorts.

for the mean shifts in females between the treatment and control cohorts, i.e., dummies for older and younger control cohorts. In the IV estimator, we define z as a measure of induction risk, and we also control for unrestricted dummies for each cohort because the induction risk is defined to be the same for all males of specific cohorts.

In both DID and IV estimators, we define $w$ as a polynomial approximation of draft eligibility represented by quartic interaction terms of male dummy and the difference between 19 and the subject's age in 1967. Because the ages for attending college and being eligible for the draft overlap, the instrument variables for education and veteran status in the DID estimator are based on the same concept, and the identification relies on the choice of two particular functional forms that capture the same concept. In addition, we control for: male dummy; white dummy; quartic terms of age; quartic terms of age and white; survey year dummies; interaction of age and survey year; and region dummies. The robustness standard errors are obtained using clustering by birth cohort and gender (Moulton 1990; Lee and Card 2004).

Based on the observations in Figures 1-4, we consider three specifications of the trend in gender difference $T$ in (1): no trend, linear trend, and quadratic trend, which are referred to as specifications (A), (B), and (C), respectively. In specification (B), we also control for linear trend interacted with male dummy; interaction of age and male; interaction of age, male, and survey year dummies; triple interaction of age, male, and white; and interaction terms of age, male, white, and survey year. Specification (C) additionally controls for quadratic trend interacted with male; quadratic term of age and male; and quadratic term of age, white and male.

It is important to note that our estimates of the impact of education on health are obtained using the Vietnam era pre-lottery draft as the instrument variable for education, whether using the DID or IV estimator. These instrument variable estimates should be interpreted as the effect of education on the subgroup of males that chose to enroll in and/or stay in college to avoid the draft. Imbens and Angrist (1995) defines this as the local average treatment effect (LATE), and the LATE is not a consistent estimate of the average effect of education on health in the overall population in the presence of heterogeneity of the effect of education on health (Card 2001).

## 5. The Results

Table 2 presents the first stage DID estimation of (1), specifically, the coefficients estimates of the instrument variable for college education—indicator of males of cohorts 1946-1950—using females of cohorts 1942–1953 and males of cohorts 1942–1944 and 1951–1953 as controls. Panels A and B show the unadjusted DID estimates, and the DID estimates that adjust for veteran status only, respectively, indicating that the pre-lottery draft caused approximately a 3.4% increase in college enrollment rate, and 2.0% increase in college completion rate. Panels C-E show the regression-adjusted DID estimates under specifications (A)-(C), respectively. We find that the pre-lottery draft is a strong predictor for college enrollment and completion, and yields virtually identical estimates of its impact on college enrollment and completion under specifications (A)-(C). Compared to the controls, treatment cohort males are approximately 3.1% more likely to have attended college and 3.7% more likely to complete college. The estimates of the impact of pre-lottery draft on attainment of associate degree or higher are positive but imprecise.

We choose specification (A) using females as controls—the regression-adjusted estimates controlling for no trend in gender difference—as our preferred specification because the graphic analysis in Figure 1-4 provides no strong evidence of the presence of a linear or quadratic trend in postsecondary educational and health outcomes. We present the estimates from our preferred specification of the impact of education on health as our basic results, along with the unadjusted DID estimates. We will later perform specification checks on our estimates of the impact of education on health using specifications (B)-(C) to control for linear or quadratic trend in gender difference.

Panel A of Table 3 presents the DID estimates of the impact of education on obesity, hypertension, psychological distress and diabetes. We find that college completion has a strong negative effect on obesity: it reduces the chance of obesity by nearly 70 percentage points, while the estimates of the effects of college enrollment and attaining an associate degree on obesity are negative but imprecise. We find the estimates of the effect of college education on hypertension are negative but borderline significant or imprecise. We find that college education has a strong negative impact on mental distress: college enrollment reduces the chance of serious psychological distress by approximately 14 percentage points, associate degree by approximately 16 percentage points, and college completion by approximately 12 percentage points. We find that college completion reduces the chance of Type 2 diabetes by about 33 percentage points, and we find limited evidence of the effect of college enrollment and associate degree on Type 2 diabetes.

## 6. Robustness Checks

We check the robustness of our results to (i) specifications (B)-(C) that control for linear and quadratic trend in gender difference, and (ii) the use of induction risk as the instrument variable to education. We then discuss the following three issues and calculate the bounds of their potential biases to our estimates: (iii) sample selection that might result from emigrants to

Canada during the Vietnam era, and (iv) the deferments available to draft-eligible males other than college deferment during the pre-lottery draft period.

First, we check the sensitivity of our results of the impact of education on health to specifications (B)-(C) when we control for linear and quadratic trend in gender difference, in Panels B and C in Table 3. Precision of the estimate of the impact of college completion on hypertension improves when we control for linear trend in gender difference, however, the estimates of the impact of college education on serious psychological distress become small and insignificant. The estimates of the impact of college completion on obesity and Type 2 diabetes are insensitive to the inclusion of linear trend in gender difference.

Second, we construct three measures of induction risk that could be eliminated by college deferments, faced by males of each cohort. The first is calculated as the average annual number of inductions during the years a cohort was aged 19–22 divided by the cohort size, with females' induction risk being zero, and hereafter referred to as unadjusted induction 19-22[12]. With the merit of simplicity, this measure does not take into account institutional details of the drafting rules. For example, each cohort of 1951–1953 was exposed to draft risk for a single year at age 20, rather than four years, while cohort 1949 faced induction risk under the pre-lottery rules when they were aged 19–20, under the draft lottery when they were aged 21, and no risk at age 22. Our second measure adjusts for the details of the drafting rules, and is hereafter referred to as rule-based induction 19-22. Given that the natural ages for college attendance were 18-21 and males were subject to draft at age 19 and older, the induction risk when a cohort was aged 22 may not be relevant to college attendance. The induction risk when a cohort was aged 21-22

[12] This is identical to that in Card and Lemieux (2001) and in de Walque (2004). The annual number of inductions is shown in Table A1, and also available at http://www.sss.gov/induct.htm. The sizes of birth cohorts 1942–1953 are taken from Table 101: High school graduates compared with population 17 years of age, by sex and control for

years may not be relevant to attaining an associate degree. Our third measure focuses on the inductions during the years a cohort was aged 19-20, which are the most relevant years in college education, and is hereafter referred to as rule-based induction 19-20.

The first stage results of the IV estimates of (1) in Table 4 indicate that all three measures of induction risk are strongly and positively associated with postsecondary education, and associate degree and college completion in particular, whether we control for no trend or quadratic trend in gender difference. Considering males only, we find that college enrollment and completion rates are strongly and positively associated with all three measures of induction risk, and that attainment of associate degree is weakly associated with all measures of induction risk. The IV estimates in Table 5 are mostly consistent with our main results in Tables 3-4.

Third, we discuss a possible sample selection issue because some males emigrated to Canada to avoid the draft. If the emigrants to Canada were more likely than those who remained in the U.S. to be smokers and obese, our estimates of the impact of postsecondary education on smoking and obesity would be biased upward. Shown in Table A1, there was a total of 38,525 males aged 19-25 who emigrated to Canada during the period of 1965 to 1974. A worst-case scenario would lead to no more than 1% upward bias in our estimates[13]. Alternatively men could disobey the law and becoming a delinquent, but delinquents represented no more than 2% of college deferments.

Lastly, we consider the issue of dependency deferments. Besides college deferments and emigration, the treatment cohorts could, and some of them did, obtain dependency and

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school: 1869–70 to 1999–2000, in Digest of Education Statistics, 2003, U.S. Department of Education. http://nces.ed.gov/programs/digest/d03.

[13] The worst-case scenario is characterized by: (1) these 38,525 males were from cohorts 1942-1953, (2) they never came back to the U.S., (3) they were all smokers or obese at the time of interview. With less than 1 in 1000 sampling rate, these males would represent less than 38 additional observations to our base sample, while there are 3,713 college graduates who ever smoked (Table 1).

fatherhood deferments by getting married or conceiving a first child. Figure A1 shows that total births and birth rate have a small increase in 1968 and large increases in 1969-1970. Years 1968 and 1969 are relevant to our analysis because fatherhood deferments were eliminated when the draft lottery was instituted. Figure A2 indicates that in 1969, there is an increase of no more than 20 per 1,000 in the birth rate among men aged 20-24—the primary age group for draftees—compared to draft-ineligible men. A worst case scenario—one male obtained the fatherhood deferment to avoid the draft for each of all these additional births—would cause no more than a 0.2% increase in fatherhood deferments, and would bias our estimates upward by approximately 5% given that the pre-lottery draft caused approximately a 3.7% increase in college completion. There is no noticeable change in the male-female difference in the median age of first marriage during the pre-lottery draft period (Figure A3), indicating that getting married is not as important a means as college attendance to avoid the draft.

## 7. Discussion

We quantify the benefit of college completion as reduced direct medical expenditures and indirect productivity losses resulting from reduced probability of unhealthy behaviors and illnesses. Table 6 presents the results on obesity and diabetes. We calculate the benefit of college completion on each health outcome as the net present value, at a 3% discount rate, of the per affected person cost of the health outcome for the entire adult population, multiplied by the reduced probability of the health outcome among college graduates. The benefit of college completion on reducing the medical expenditures of treating obesity-related illness amounts to more than $23,000, and more than $44,000 total including productivity losses per person over the life of that person. These savings are twice as large as to the per-student amount of public financing of college education in the United States i.e. $22,234 in 2001 (OECD 2005), and represent more than 8%, with a lower bound of more than 4%, of the return to college degree in terms of the increased per-person lifetime earnings of $500,000 compared to earnings of high school graduates[14].

**8. Conclusions**

The pre-lottery Vietnam era draft followed by the drastic change in drafting rules brought about by the draft lottery system creates a discontinuity gap in postsecondary educational attainment, which we exploit to infer a causal effect of education on health. We find that while college enrollment exerts an effect in reducing smoking initiation and increasing smoking cessation similar to that of college completion, the impact of college completion on obesity and adult-onset diabetes is considerably stronger than that of college enrollment. A focus of our future research is to empirically verify potential mechanisms by which college education improves health, as well as mechanisms that drive the differential impact between college enrollment and college completion that is documented in this study.

[14] We use 10% as return to schooling, which is based on recent twin studies and viewed as the "best available evidence" in Card (1999), and $32,672 as the average annual earnings of high school graduates in 2002 based on The Current Population Survey 2002, at 3% discounting rate, to calculate the lifetime increase in earnings of college degree of approximately $500,000.

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Table 1: Sample Means of Selected Health-Related Behaviors, Markers and Outcomes by Educational Attainment Categories

| | College degree and above | Some college | High school graduates | High school dropouts |
|---|---|---|---|---|
| Ever vs. Never Had Hypertension | 0.2200 | 0.2812 | 0.3004 | 0.4023 |
| | (0.0045) | (0.0047) | (0.0051) | (0.0072) |
| | [8,273] | [8,928] | [8,176] | [4,594] |
| Obesity | 0.3881 | 0.5162 | 0.5206 | 0.5877 |
| | (0.0067) | (0.0067) | (0.0069) | (0.0089) |
| | [5,212] | [5,630] | [5,240] | [3,049] |
| Serious Psychological Distress | 0.0170 | 0.0389 | 0.0401 | 0.1083 |
| | (0.0014) | (0.0020) | (0.0022) | (0.0046) |
| | [8,208] | [8,844] | [8,071] | [4,504] |
| Ever vs. Never Had Type 2 Diabetes | 0.0356 | 0.0540 | 0.0591 | 0.0951 |
| | (0.0020) | (0.0024) | (0.0027) | (0.0045) |
| | [8,098] | [8,573] | [7,820] | [4,216] |

*Notes*: Data source is the National Health Interview Surveys 1998–2003. The sample contains U.S. born birth cohorts 1942–1953. Standard errors are in parentheses and sample sizes are in square brackets. Obesity is set to 1 if one's body mass index (BMI, body weight in kilograms divided by square of height in meters) was 30 or more, and 0 if one's BMI was under 25. Serious psychological distress is having one of the following feelings all of the time during the 30 days prior to the time of interview: extremely sad, nervous, restless, hopeless, everything was an effort, or worthless.

Table 2: DID Estimates of the Impact of Pre-Lottery Vietnam Draft on Postsecondary Education

| | | | Adjusted and Controlling for | | |
|---|---|---|---|---|---|
| Specifications | Unadjusted | Adjusted for Veteran Status Only | No Trend in Gender Difference | Linear Trend in Gender Difference | Quadratic Trend in Gender Difference |
| | [1] | [2] | [3] | [4] | [5] |
| College enrollment | 0.0349 *** | 0.0345 *** | 0.0311 * | 0.0388 ** | 0.0335 ** |
| | (0.0103) | (0.0112) | (0.0172) | (0.0170) | (0.0173) |
| AA and above | 0.0331 *** | 0.0276 ** | 0.0254 | 0.0250 | 0.0282 |
| | (0.0100) | (0.0117) | (0.0180) | (0.0178) | (0.0175) |
| BA and above | 0.0264 *** | 0.0202 ** | 0.0366 ** | 0.0367 *** | 0.0363 *** |
| | (0.0089) | (0.0098) | (0.0148) | (0.0143) | (0.0141) |

*Notes*: Presented are coefficients of the instrument variable—an indicator of males born in between 1946 and 1950 in the first stage estimation of specification (1) applied to birth cohorts 1942–1953 from the National Health Interview Surveys 1998–2003. Sample size is 30,158. Robust standard errors in parentheses are obtained by clustering at age, survey year and gender level.

Columns [2]-[5] adjust for veteran status, and additional instrument variable is represented by quartic terms of interaction of male and age over 19 in year 1967. Columns [3]-[5] also control for mean shifts in females in control cohorts (while using females as controls); gender dummy; race dummy; quartic terms of age; quartic terms of interaction of age and race; unrestricted survey year dummies; interaction of age and survey year; and region dummies. Column [4] also controls for linear trend interacted with gender; interaction of age and gender; interaction of age, gender and race; interaction of age, male and survey year; and interaction of age, gender, race and survey year. Column [5] also controls for quadratic trend interacted with gender; quadratic term of interaction of age and gender; quadratic term of interaction of age, gender and race. ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level.

Table 3: DID Estimates of the Impact of Education on Health

| | Obesity [4] | Hypertension [5] | Mental Distress [6] | Type 2 Diabetes [7] |
|---|---|---|---|---|
| *Panel A: Control for No Trend in Gender Difference* | | | | |
| College enrollment | -0.4162 | -0.4246 | -0.1403 ** | 0.1013 |
| | (0.3819) | (0.3183) | (0.0647) | (0.1406) |
| AA and above | –0.9306 | -0.6418 * | -0.1614 * | -0.1054 |
| | (1.1003) | (0.3882) | (0.0895) | (0.1456) |
| BA and above | –0.7050 ** | -1.0113 | -0.1217 ** | –0.3309 * |
| | (0.3256) | (1.0229) | (0.0613) | (0.1851) |
| *Panel B: Control for Linear Trend in Gender Difference* | | | | |
| College enrollment | -0.3438 | -0.5272 * | -0.0928 | -0.0271 |
| | (0.4042) | (0.3084) | (0.0578) | (0.1576) |
| AA and above | –0.8769 * | -0.8542 | -0.0748 | -1.3145 |
| | (0.4827) | (0.6191) | (0.0763) | (1.6387) |
| BA and above | –0.8056 ** | -0.6778 * | -0.0718 | –0.3552 ** |
| | (0.3708) | (0.3736) | (0.0643) | (0.1763) |
| *Panel B: Control for Quadratic Trend in Gender Difference* | | | | |
| College enrollment | -0.1818 | -0.4085 | -0.0572 | 0.0362 |
| | (0.4055) | (0.2949) | (0.0531) | (0.1424) |
| AA and above | –0.6063 * | -0.5830 | -0.0294 | -0.1278 |
| | (0.3371) | (0.4313) | (0.0653) | (0.1849) |
| BA and above | –0.7013 ** | -0.4996 | -0.0491 | –0.1699 |
| | (0.3234) | (0.3729) | (0.0838) | (0.1955 ) |
| N | 19,143 | 29,985 | 29,631 | 28,715 |

*Notes*: Presented are coefficients of education using specification (1)-(3) applied to U.S. born birth cohorts 1942–1953 from NHIS 1998–2003. N is sample size. Dependent variables and specifications are defined in Table 3. Panel A-C controls for no/linear/quadratic trend interacted with gender, respectively; interaction of age and gender; interaction of age, gender and race; interaction of age, male and survey year; and interaction of age, gender, race and survey year. Panel B also controls for quadratic trend interacted with gender; quadratic term of interaction of age and gender; quadratic term of interaction of age, gender and race. ***: Significant at 1% level; **: significant at 5% level; *: significant at 10% level

Table 4: Specification Checks: IV Estimates of the Impact of Pre-Lottery Vietnam Draft on Postsecondary Education Using Induction Risk

| | Females as Controls | | |
|---|---|---|---|
| Induction Risk | Unadjusted 19-22 | Rule-based 19-22 | Rule-based 19-20 |
| *Panel A: Control for No Trend in Gender Difference* | | | |
| College enrollment | 0.0499 * | 0.0880 ** | 0.0871 ** |
| | (0.0262) | (0.0427) | (0.0428) |
| AA and above | 0.0925 *** | 0.1547 *** | 0.1498 *** |
| | (0.0276) | (0.0443) | (0.0444) |
| BA and above | 0.0982*** | 0.1816 *** | 0.1621 *** |
| | (0.0241) | (0.0421) | (0.0434) |
| *Panel B: Control for Linear Trend in Gender Difference* | | | |
| College enrollment | 0.0368 | 0.0616 | 0.0662 |
| | (0.0336) | (0.0484) | (0.0437) |
| AA and above | 0.0621 * | 0.0981 ** | 0.1072 ** |
| | (0.0329) | (0.0493) | (0.0443) |
| BA and above | 0.0598 ** | 0.1209 *** | 0.1140 *** |
| | (0.0300) | (0.0463) | (0.0439) |
| N | 30,158 | 30,158 | 30,158 |

*Notes*: Presented are coefficients of the instrument variable—a measure of induction risk—in the first stage estimation of specification (1) applied to U.S. born birth cohorts 1942–1953 from the National Health Interview Surveys 1998–2003. N is sample size. Induction risk measures “Unadjusted 19-22”and “Rule-based 19-22” are calculated as the average annual number of inductions when a cohort was aged 19-22, and the latter is adjusted for the drafting rules. “Rule-based 19-20” is calculated as the average annual number of inductions when a cohort was aged 19-20, and adjusted for the drafting rules. All measures of induction risk are adjusted for the cohort size.

All estimates control for unrestricted cohort dummies; male dummy; race dummy; quadratic terms of age; quadratic interaction terms of age and race; veteran status dummy; unrestricted survey year dummies; interaction of age and survey year; and region dummies. Panel B also controls for linear trend interacted with gender; interaction of age and gender; interaction of age, gender and race; interaction of age, gender and survey year; and interaction of age, gender, race and survey year. ***: significant at 1% level; **: significant at 5% level; *: significant at 10%.

Table 5: Specification Checks: IV Estimates of the Impact of Education on Smoking Initiation and Cessation, Obesity, and Diabetes

| | Obesity [4] | Hypertension [5] | Mental Distress [6] | Type 2 Diabetes [7] |
|---|---|---|---|---|
| *Panel A: Control for No Trend in Gender Difference* | | | | |
| College enrollment | -1.3901 | -0.2190 | -0.0850 | 0.0756 |
| | (0.9541) | (0.3438) | (0.0737) | (0.2092) |
| AA and above | -1.3049 * | -1.2203 | 0.2338 | -0.5984 |
| | (0.6842) | (1.0503) | (0.2006) | (0.4991) |
| BA and above | -1.0036 ** | -0.6829 | -0.1200 | -0.5243 * |
| | (0.4774) | (0.4966) | (0.0838) | (0.2830) |
| *Panel B: Control for Linear Trend in Gender Difference* | | | | |
| College enrollment | -0.6523 | -0.2857 | -0.1393 ** | 0.0302 |
| | (0.5244) | (0.2868) | (0.0645) | (0.1709) |
| AA and above | -1.4401 | -1.2173 | -0.2809 | -0.6545 |
| | (0.8896) | (0.8323) | (0.1875) | (0.4676) |
| BA and above | -0.9094 ** | -0.6402 | -0.1358 * | -0.5586 * |
| | (0.3795) | (0.4599) | (0.0826) | (0.3117) |
| N | 19,143 | 29,985 | 29,715 | 28,715 |

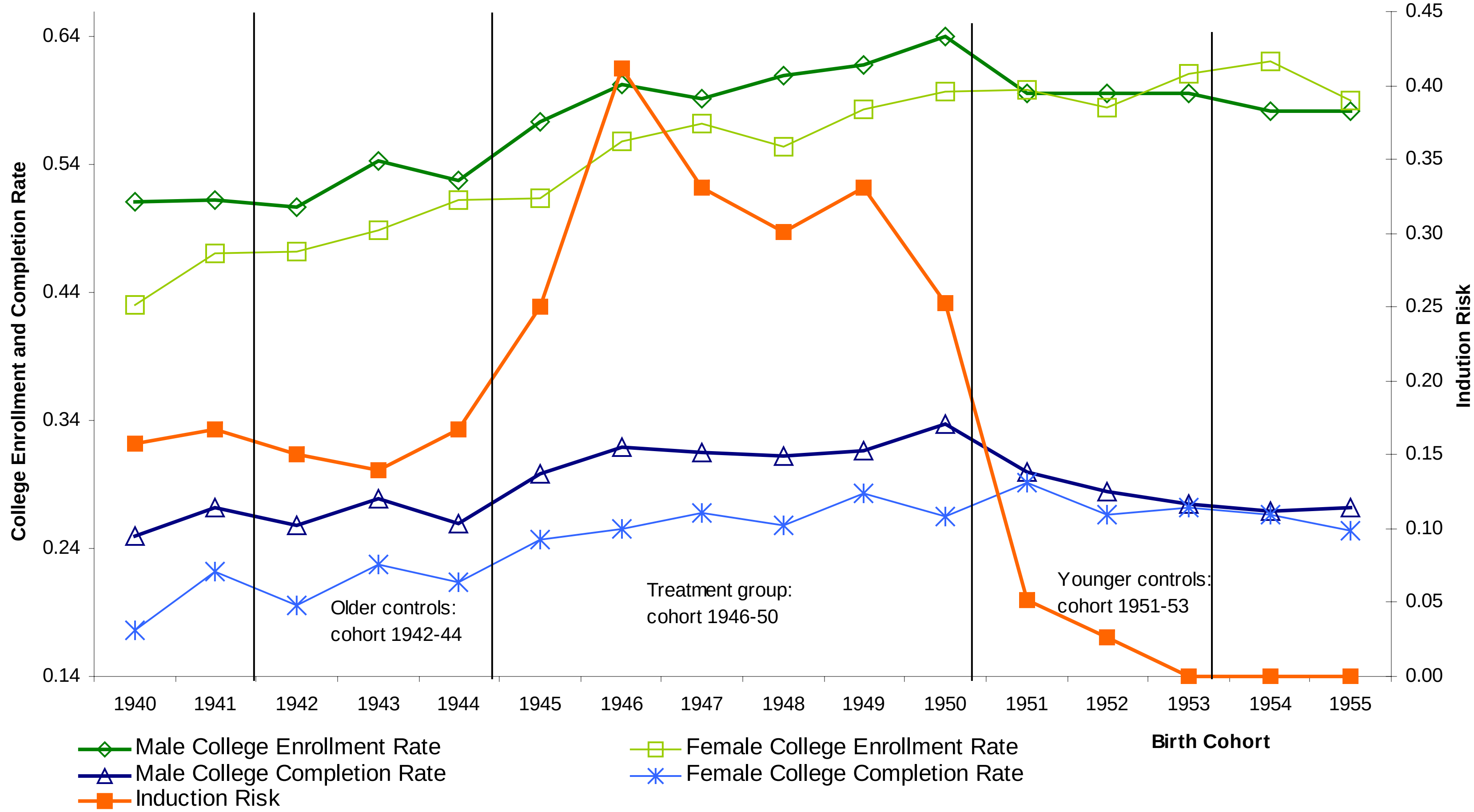
*Notes*: Presented are coefficients of education using specification (1)-(3) applied to U.S. born birth cohorts 1942–1953 from NHIS 1998–2003. Instrument variable includes the rule-adjusted induction risk in age 19-20. N is sample size. Panels A-B adjust for no and linear trend interacted with gender, respectively. Dependent variables are defined in Table 3. All estimates use the same specifications as Table 3, except that instead of mean shift in females in control cohorts, we control for unrestricted cohort dummies. ***: Significant at 1% level; **: significant at 5% level; *: significant at 10% level

Table 6: Economic Costs and Benefit of College Degree

| | College Impact | Total Costs | Per Affected Person Annual Cost: Total | Medical Expenditures | Productivity Losses | NPV Benefit by Education: Total | Medical Expenditures | Productivity Losses |
|---|---|---|---|---|---|---|---|---|
| Direct and indirect costs of smoking, obesity, diabetes and mental disorder: | | | | | | | | |
| Obesity | -70% | 117 | 1,908 | 995 | 913 | 44,520 | 23,217 | 21,303 |
| Diabetes | -33% | 132 | 14,696 | 10,683 | 4,013 | 161,656 | 117,513 | 44,143 |
| Distress | -12% | 42 | 1,542 | 1,286 | 256 | 6,168 | 5,144 | 1,024 |
| Public finance of per-student college education: 22,234 | | | | | | | | |
| NPV of wage gains of college degree compared to high school graduation: 500,000 | | | | | | | | |

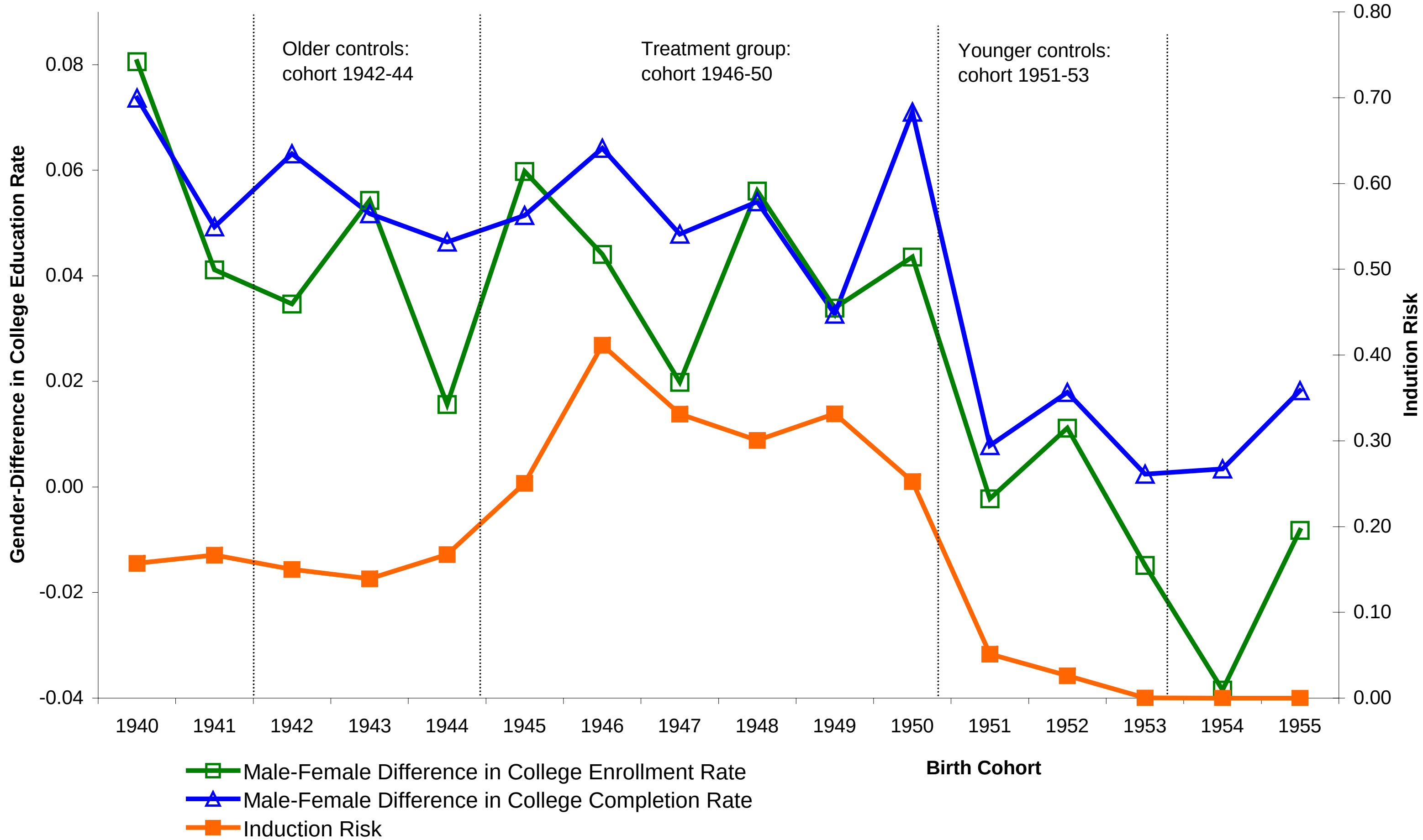
*Notes*: Presented in the "Impact of College" column are the DID point estimates of the impact of college completion. Values in the "Total Costs" column are in U.S. billion dollars, and in other columns in U.S. dollars. Net Present Value (NPV) is calculated using 3% discounting rate using the point estimates. The impact of college completion is used to calculated the benefit of education in reduced costs. Data source for smoking is Annual Smoking-Attributable Mortality, Years of Potential Life Lost, and Economic Costs, United States, 1995—1999, The Centers of Disease Control and Prevention (CDC), available at http://www.cdc.gov/mmwr/preview/mmwrhtml/mm5114a2.htm. Data source for obesity is Statistics Related to Overweight and Obesity, The National Institute of Health, available at http://win.niddk.nih.gov/statistics. Data source for diabetes is Data source of diabetes is Diabetes: Disabling, Deadly and On the Rise, At a Glance, 2005. CDC. Available at http://www.cdc.gov/nccdphp/aag/aag_ddt.htm. Data source for Mental Illness is Mental Health: A Report of the Surgeon General, 2005; http://www.surgeongeneral.gov/library/mentalhealth/home.html.

Figure 1: Vietnam War Draft and Unadjusted College Enrollment, Associate Degree Attainment, and College Completion Rate of Men and Women



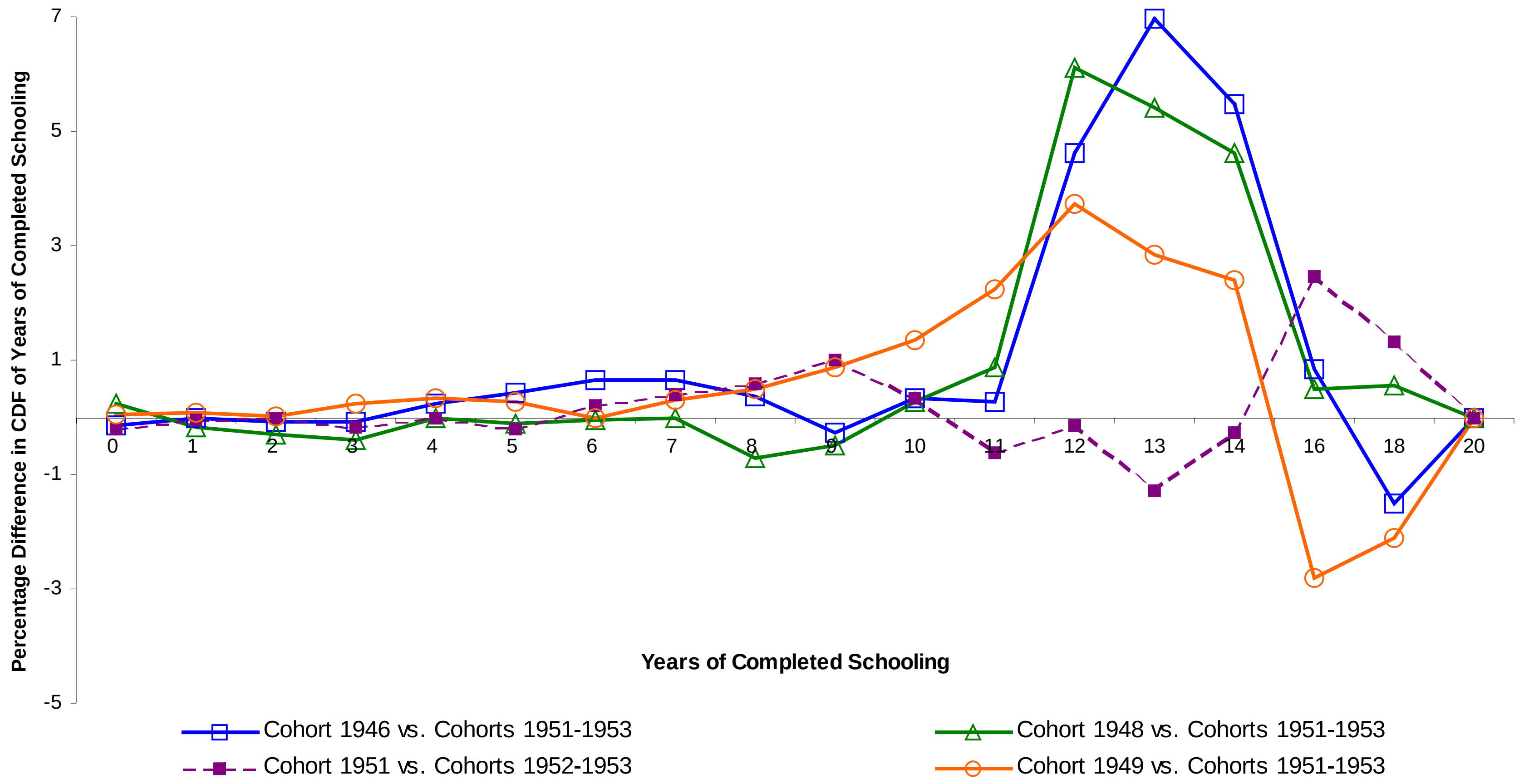

*Notes*: Presented are the unadjusted means of, among all U.S. born birth cohorts 1942–1953 from the National Health Interview Surveys 1998–2003, (1) the fraction with college enrollment, associate degree attainment, and college completion rate among males and females at the left y-axis; (2) a measure of induction risk adjusted for drafting rules (see main text for more details) at the right y-axis.

Figure 2: Vietnam War Draft and Unadjusted Male-Female Difference in College Enrollment, Associate Degree Attainment, and College Completion Rate




*Notes*: Presented are the unadjusted means of male-female difference in, among all U.S. born birth cohorts 1942–1953 from the National Health Interview Surveys 1998–2003, (1) the fraction with college enrollment and completion rate at the left y-axis; (2) a measure of induction risk adjusted for drafting rules (see main text for more details) at the right y-axis.

Figure 3: Gender-Difference and Control-Treatment Gender-Difference in Schooling CDF




*Notes*: Presented are the difference in the male-female difference in the cumulative distribution of years of completed schooling (1) between cohort 1946 and cohorts 1951-1953; (2) between cohort 1948 and cohorts 1951-1953; and (3) between cohort 1951 and cohorts 1952-1953 among all U.S. born individuals from the National Health Interview Surveys 1998–2003.

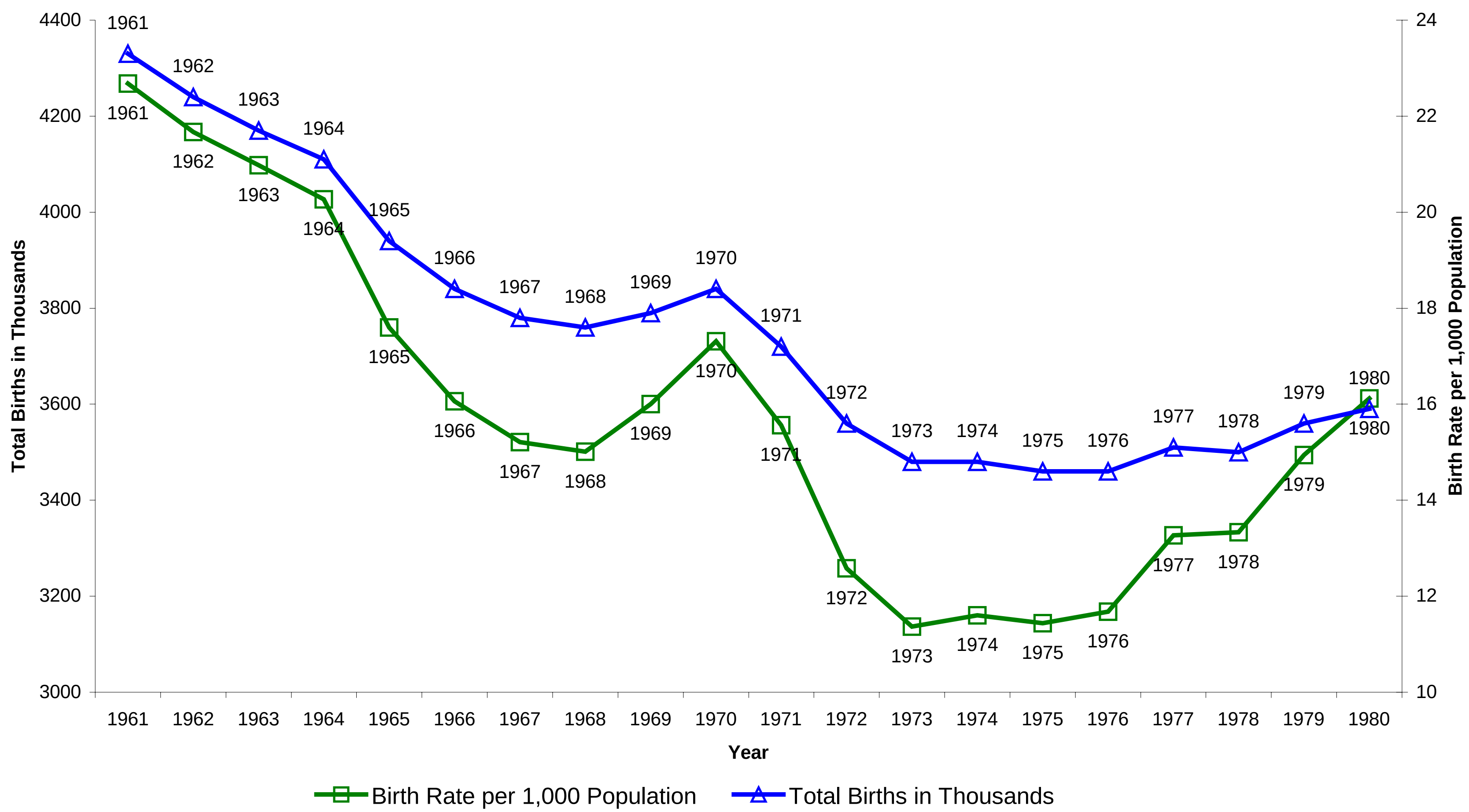
## Appendix

Table A1: Educational Deferments, Emigrants to Canada, Delinquents, Induction [those were deleted, right?]

| | Emigrants to Canada | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| Year | Male Emigrants | Male Resisters | Female Emigrants | Female Resisters | H.S. Deferments (in 1,000) | College Deferments (in 1,000) | Dependency Deferments (in 1,000) | Number of Delinquents | Number of Inductions |
| 1964 | — | — | — | — | — | — | — | — | 112,386 |
| 1965 | 1,922 | 656 | 2,592 | 625 | — | — | — | — | 230,991 |
| 1966 | 2,447 | 1,181 | 3,329 | 1,362 | — | — | — | — | 382,010 |
| 1967 | 3,032 | 1,766 | 3,750 | 1,783 | 461 | 1,731 | 7,813 | 13,500 | 228,263 |
| 1968 | 4,076 | 2,810 | 4,330 | 2,363 | 489 | 1,772 | 8,193 | 22,500 | 296,406 |
| 1969 | 4,405 | 3,139 | 5,112 | 3,145 | 499 | 1,789 | 8,365 | 30,000 | 283,586 |
| 1970 | 5,510 | 4,244 | 5,714 | 3,747 | 459 | 1,529 | 8,188 | 18,500 | 162,746 |
| 1971 | 4,778 | 3,512 | 5,477 | 3,510 | 450 | 1,308 | 3,961 | 22,000 | 94,092 |
| 1972 | 3,980 | 2,714 | 5,278 | 3,310 | 232 | 881 | — | — | 49,514 |
| 1973 | 4,120 | 2,854 | 5,356 | 3,389 | 30 | 454 | — | — | 646 |
| 1974 | 4,255 | 2,989 | 5,536 | 3,569 | — | — | — | — | 0 |

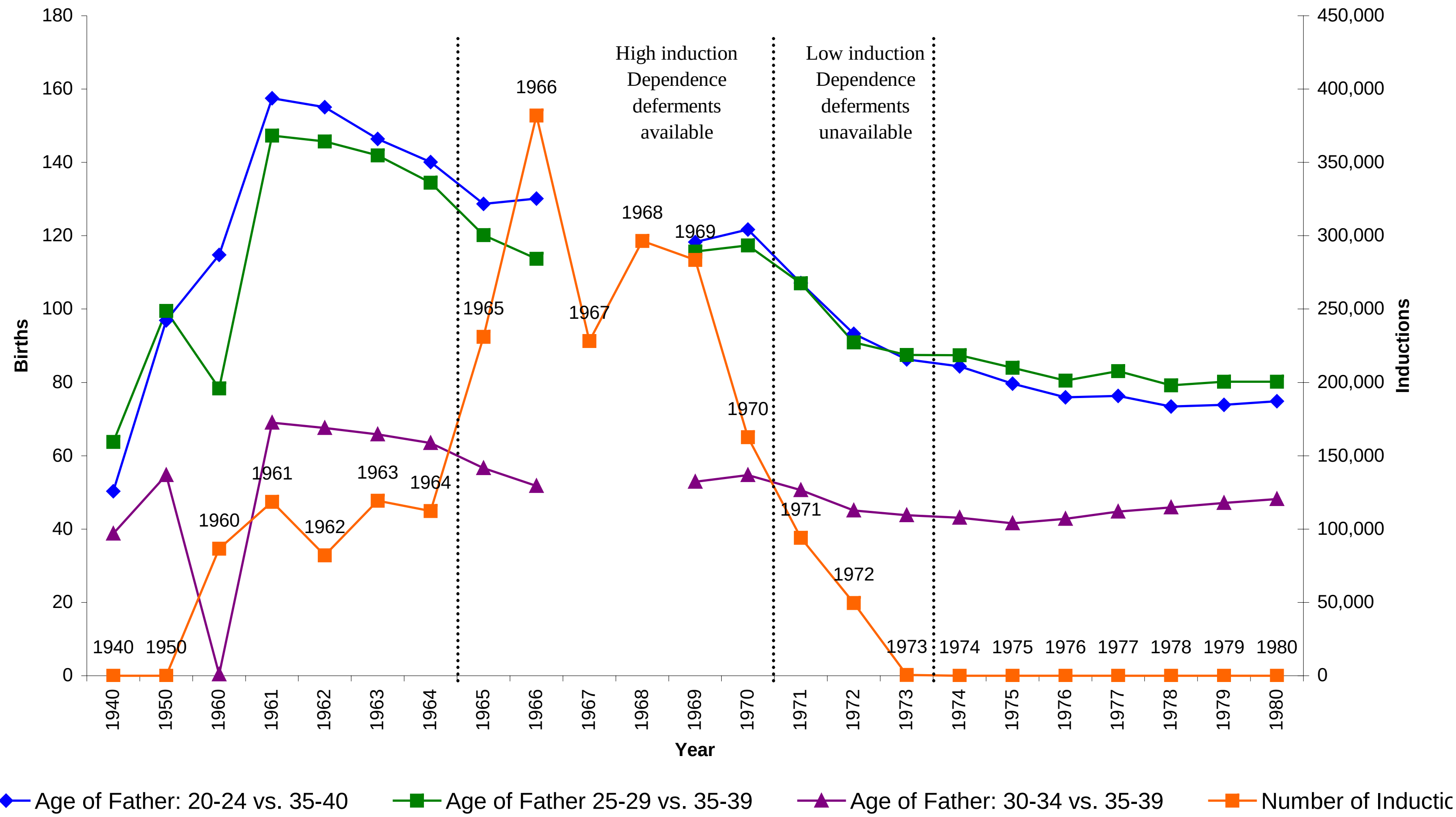
*Notes*: Cells with "—" indicate a lack of data. Data sources for high school, college, and dependency and fatherhood deferments, number of delinquents, and inductions are various issues of the Semi-annual report of the director of the Selective Service System to the Congress, 1967-1973. [deleted, right?] Data source for emigrants to Canada is Figure 2. Estimated numbers of men and women aged 15-29 who immigrated from the United States to Canada to resist the Vietnam War, 1965-1974, page 241 in *Northern Passage: American Vietnam War Resisters in Canada* by John Hagan. Male and female emigrants are total number of emigrants during each year, and male and female resisters are calculated by the arriving number minus average annual migration of American men, 15-29, for preceding five years.

Figure A1: Annual Number of Total Live Births and Live Birth Rate



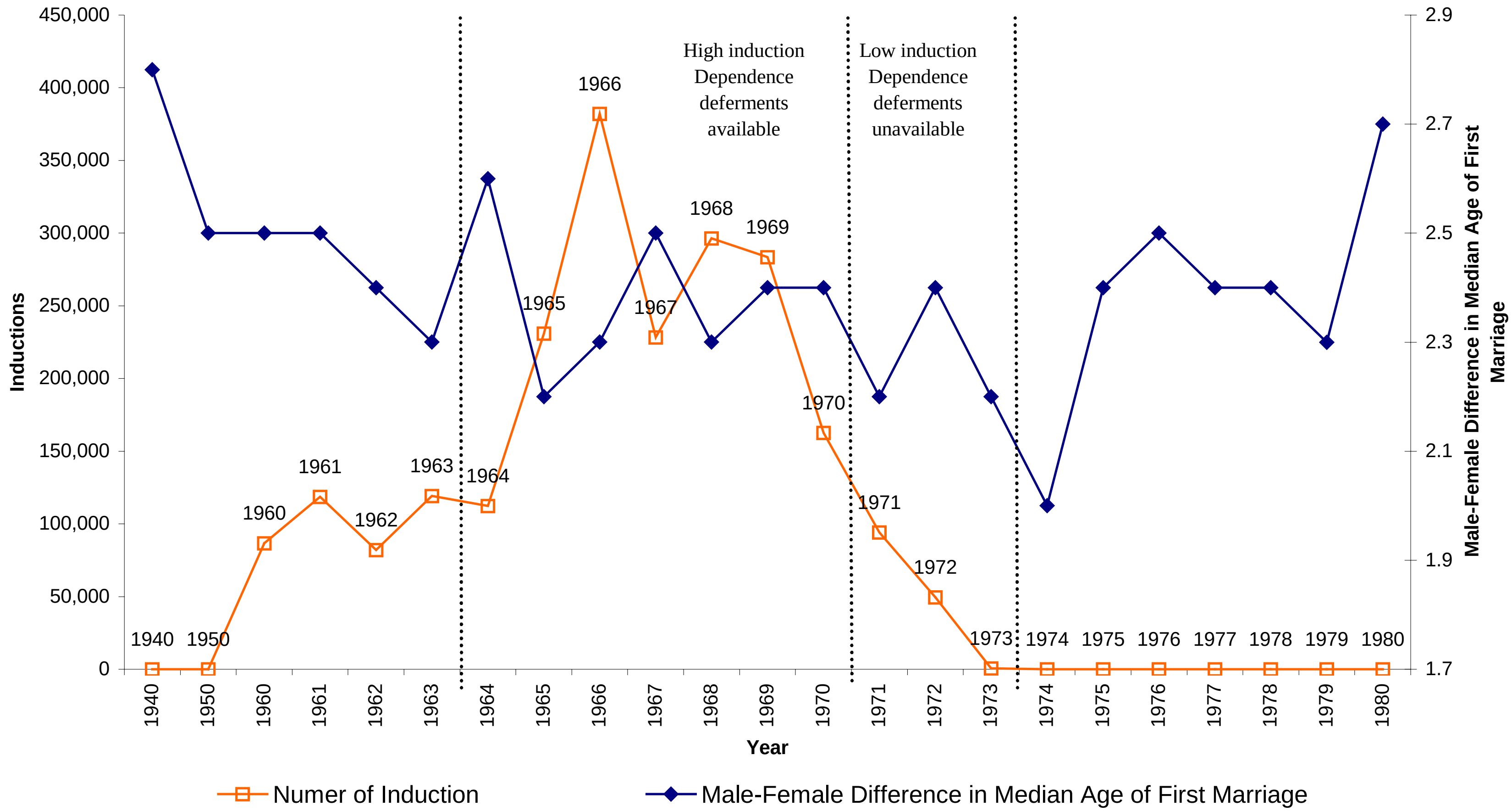

*Notes*: Presented at the left y-axis is the annual number of total live births in thousands, and at the right y-axis is the annual live birth rate per 1000 population, obtained from Table 1-1. Live Births, Birth Rates, and Fertility Rates, by Race: United States, 1909-94. The Centers for Disease Control and Prevention, 2005.

Figure A2: Number of Inductions and Difference in Live Births per 1000 Men Between Draft-Eligible versus Draft- Ineligible Men




*Notes*: Presented at the right y-axis is the number of inductions from Table A1, and at the left y-axis is the difference in number of live births per 1000 men between draft-eligible and draft-ineligible men by their ages, obtained from Table 1-23. Birth rates by age of father and race: United States, 1940, 1950, and 1960-94. The CDC. Accessed in October 2005. http://www.cdc.gov/nchs/datawh/statab/unpubd/natality/natab94.htm.

Figure A3: Number of Inductions and Male-Female Difference in Median Age of First Marriage




*Notes*: Presented at the left y-axis is the number of inductions obtained from the Table A1 at the left y-axis, and at the right y-axis is male-female difference in median age of first marriage, obtained from Table A2: Median Age at First Marriage, by Sex: 1890-1994, from Marital Status and Living Arrangements: March 1994, Population Characteristics P20-484, Current Population Reports, Bureau of the Census, 1994.