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2007/42/ST

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by

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June 30, 2007

This is a revised version of Chapter 4 of my Northwestern University Ph.D. dissertation. I would like to thank Stefan Bender, Douglas Frank, Sr., Shane Greenstein, Paul Oyer, Scott Schaefer, Scott Stern and Dennis Yao for helpful comments and suggestions. All errors are my own.

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Abstract

Using an original data set matching individual birthdays to Vietnam War draft lottery numbers, I study how wartime draft risk affects representation in a sample of top corporate executives decades after the war's end. I find that men with lottery numbers placing them at risk of induction are underrepresented among top U.S. executives in the 1990s. Furthermore, I find that differences in lottery numbers explain differences in executives' compensation and age of entry into the top executive ranks in a way that is consistent with draft-induced differences in incentives to acquire human capital early in their careers.

JEL Codes: J24, J62

1 Introduction

It is hard to find an aspect of American society that was untouched by the Vietnam war. Young, draft-eligible men were especially affected, facing important choices about how to respond to the risk of conscription, combat, and death. In this paper, I study how this risk affected long-term career outcomes for one segment of the Vietnam generation. I examine executives of publicly traded U.S. companies who were of draft age in the years 1970-72. In those years, qualified men were drafted according to a random lottery number assigned to their birthday. Lottery numbers were called for induction from lowest to highest. Thus, men drawing low numbers were at high risk of induction. Using an original data set matching individual birthdays to wartime lottery numbers, I study how this ex-ante draft risk affects the composition of the executive sample decades after the war's end.

Studying these effects is valuable for several reasons. First, this study builds on and complements the established literature documenting negative labor market outcomes for Vietnam veterans. The canonical reference, Angrist (1990), inspires my use of the draft lottery as a natural experiment. However, Angrist explores a fundamentally different question: What is the effect of military service on civilian earnings? Vietnam is the specific context, and the draft lottery plays a supporting role as an instrument for military service. In contrast, I am interested in the effect of the draft lottery *per se* on career outcomes for *all* draft-eligible men, whether or not they served in the military. This is motivated by the observation that young men did not respond passively to the draft; they took active steps to avoid it. It is natural to think that their decisions might have had lasting career impact, even if these men avoided service. However, these more general career effects of the draft

are not well understood.

Second, the draft represents a random shock that affected young men just at the age of labor market entry. My results therefore contribute to the large literature that seeks to understand – as yet without any clear consensus – whether early shocks to individuals' careers have persistent effects and, if so, just how persistent they are. I contribute to this literature in two main ways. First, I examine a supply-side shock to human capital formation, as distinct from a shock to labor demand, as is typical in the literature. Second, I study a clean natural experiment that suffers from none of the identification issues facing the existing literature. Because the lottery was random, any unobservable determinants of labor market outcomes are uncorrelated with the lottery number received. Cohorts are separated only by the accident of their birth dates, not by time, geography, skill, or any other factors that might affect their decisions or outcomes. Therefore, different cohorts respond to the shock under identical circumstances. In contrast, existing work must invariably confront the issue that correlation between the observed shock and unobservable differences across cohorts may be driving the results.

The paper proceeds as follows: In Section 2, I summarize the relevant details of the Vietnam draft lottery and discuss related literature. In Section 3, I develop testable hypotheses for the relationship between wartime lottery numbers and the composition of the population of corporate executives. Section 4 describes the data, Section 5 the estimation approaches I employ, and Section 6 the results. Section 7 concludes.

2 Background

2.1 The Vietnam Draft Lottery[1]

On November 26, 1969, President Nixon signed Public Law 91-124 and Executive Order 11497, drastically altering the mechanism for drafting men for military service in Vietnam. Under the previous system, men had registered at age 18 and remained exposed to the draft until age 26. Local draft boards were instructed by law to draft the oldest eligible man first. Under the new system, a registrant was to have a single year of exposure to the draft – the calendar year in which he turned 20 – and the order of induction was to be determined by an annual lottery, held in the year prior to the actual draft.[2]

In each lottery, a Random Sequence Number (RSN) was assigned to a birthday.[3] Men were called for induction by their birthdays in order of increasing RSN. Each year, there was a maximum RSN called, but this ceiling was not determined until well into the draft year, once the military's manpower requirements for that year were finalized.[4] Therefore, an individual had no way of knowing at the time of the lottery whether he would ultimately be called. Lottery drawings were conducted through 1975, but only the first three lotteries resulted in inductions; no one was drafted after 1972.

The first (1970 draft) lottery was anomalous in several key respects. First, to prevent men from escaping the draft due to the transition from an oldest-first to a

[1] This section is based on Tarr (1981) and Selective Service System (1984).

[2] To avoid confusion, I will identify lotteries and their corresponding drafts with the year in which the actual draft occurred. So, for example, the "1970 lottery" refers to the lottery conducted on December 1, 1969, that determined the order of induction for the 1970 draft.

[3] I will use the terms "RSN" and "lottery number" interchangeably throughout the text.

[4] The ceilings were 195 in 1970, 125 in 1971 and 95 in 1972.

youngest-first policy, this initial lottery applied to all men born in the years 1944-50. Subsequent lotteries applied to single birth years: the 1971 lottery covered men born in 1951, and the 1972 lottery men born in 1952. Second, inductions began just one month after the lottery, whereas the 1971 and 1972 lotteries were held six and five months, respectively, before inductions began. Third, because it was the inaugural draft conducted under the new system, there was a high degree of confusion among those affected by the draft – and those administering the draft – as to exactly how the new system worked. Finally, the 1970 lottery was not completely random: statisticians quickly established that the lowest numbers were assigned disproportionately to birth dates in the second half of the year, and the procedure was corrected for subsequent drawings.

Individuals could have many different statuses with respect to the draft. A man was *eligible* for a draft if he was of draftable age, regardless of his fitness for service. For example, a man born in any of the years 1944-50 was eligible for the 1970 draft. An eligible man could avoid the draft through a *deferment* or an *exemption*. A deferment was for a temporary circumstance, such as college study, and expired when that circumstance changed. An exemption was for a more permanent circumstance, such as poor eyesight. A draft-eligible man with no deferments or exemptions from military service was *exposed* to the draft. The draft *ceiling* was the highest lottery number called in a given year. A man holding an RSN below the ceiling for his year of eligibility was *called* for induction, whether or not he was exposed. Finally, a man was *drafted* if he was involuntarily inducted into the armed forces. Therefore, to be drafted, a man had to be eligible, exposed and called.[5] If any one of these conditions was not satisfied, he was not drafted.

[5] Technically, a man could avoid being drafted even at this point by voluntarily enlisting in

2.2 Effects of the Vietnam Draft

The draft had both direct and indirect effects on young men's lives. Being drafted was a direct effect. Numerous sources document the negative life outcomes experienced by Vietnam veterans [Frey-Wouters and Laufer (1986), Hendin and Haas (1984), Laufer (1985)]. These outcomes are generally associated with combat exposure. One might doubt that potential executives would have seen combat if drafted, perhaps because they qualified for non-combat duty by virtue of superior mental aptitude or privileged economic backgrounds. Gimbel and Booth (1996) find that in Vietnam the probability seeing combat decreased with mental aptitude as measured by the Armed Forces Qualifications Test (AFQT). On the other hand, they also find that draftees were more likely to see combat, all else equal. Also, Barnett et al. (1992), using data on the home addresses of war casualties, find no strong correlation between neighborhood incomes and per-capita death rates in Vietnam. In contrast to popular perceptions, war deaths were evenly distributed across all economic classes.

Even if one was not a combat veteran, however, conscription into the military meant two years of involuntary service rather than two years spent pursuing one's own life objectives. One might imagine that this put draftees at a serious disadvantage in the labor force. In fact, Angrist (1990) finds that in the early 1980s the earnings of white Vietnam veterans were approximately 15 percent less than those of comparable nonveterans. He also provides evidence that this effect is at least in part due to loss of experience: military service is a poor substitute for civilian job experience. His finding complements similar conclusions from Rosen

the armed forces, so these are necessary but not sufficient conditions.

and Taubman (1982) and Schwartz (1986).

Only Hirsch and Mehay (2003) find a positive earnings effect of active-duty military service, but it is confined to officers. Furthermore, they cannot rule out unobserved ability as an alternative explanation. Since draftees rarely became officers, there is no reason to believe that *involuntary* service in Vietnam was anything but bad from a career standpoint.

The indirect effects of the draft relate to eligible men who were not drafted. First, there is some evidence that men with low draft numbers might have faced discrimination in the labor market. Firms would naturally have been reluctant to hire and train new workers, only to lose them to the military. Oi (1967) cites a 1964 survey of college placement offices indicating that 48 percent of employers restricted hiring of "draft-liable" males ("exposed" in my terminology). Note, however, that this survey was taken under the pre-lottery regime, when a man's exposure to the draft could last for many years. Under the lottery regime, where exposure lasted only one year, such effects are likely to have been less pronounced, but they may still have been present.

A second indirect effect of the draft is its influence on the decisions made by eligible men. Significant numbers of men took active steps to avoid being drafted. According to a survey cited in Baskir and Strauss (1978, p. 7), "approximately 15 million (60 percent) of the draft-age men who did not see combat took positive steps to help fate along. More than half of all men who escaped the draft, and almost half of all servicemen who escaped combat, believe today that the actions they took were wholly or partly responsible for keeping them away from the fighting."

There were three main ways in which men could avoid being drafted: volun-

teer, obtain an exemption or deferment, or violate the draft laws by, for example, failing to register for the draft or to report when called for induction. Volunteers could enter as enlisted men or, if qualified, as officers. Either option gave the volunteer some negotiating ability over specific military assignments (Kendall and Ross 1970). Exemptions and deferments were of five main types: unqualified (mentally, physically or morally unfit for service), conscientious objectors, college and graduate students, special occupations, and personal status (mainly hardship, marriage or fatherhood).[6]

Table 1 presents a breakdown of draft outcomes for the Vietnam generation taken from Baskir and Strauss (1978).[7] During this time, 26,800,000 men were eligible for the draft. Of these, 32.5 percent enlisted voluntarily in the armed forces. Many of these enlistments were direct responses to the draft, because volunteers had more discretion over their branch and type of service. Angrist (1991) finds that men with low RSNs are overrepresented among voluntary enlistees, and Cook, Jr. (1972) finds a significant effect of the draft on Air Force enlistments. Altman and Barro (1971) show that Reserve Officer Training Corps enrollments increased as a result of the draft, and they assert that there is evidence that ROTC enrollments are a good proxy for the officer volunteer rate. The proportion of eligible men who avoided service through a deferment or exemption was 32.7 percent. This figure is broken down as follows: unqualified, 19.7 percent; conscientious objectors, 0.6

[6] In 1967 graduate student deferments were eliminated for all fields of study except medicine, dentistry, osteopathy, veterinary medicine and optometry. Men in other fields entering their second year of graduate study in the fall of 1967 were allowed to keep their deferments for up to five years. Since anyone born after 1944 is unlikely to have entered their second year of graduate school by 1967, graduate student deferments are essentially irrelevant for the age group under study (born 1944-50).

[7] Baskir and Strauss define the Vietnam generation as all Americans born between July 1, 1939, and June 30, 1954.

percent; student deferments, 1.4 percent; occupational deferments, 1.8 percent; personal status deferments and exemptions, 9.0 percent. Finally, 2.1 percent of eligible men took illegal actions to avoid the draft.

Student deferments are of special interest for this study, as one might think that potential executives would have attended college and thus avoided the draft altogether. It is true that many men used college as a haven from the draft. Card and Lemieux (2000) show that the draft induced an increase in male college attendance rates between 1965 and 1970 and raised the fraction of men born in the late 1940s with a college degree by up to two percentage points. Also, Angrist and Krueger (1992) find some evidence that men at high risk of being drafted were more likely to have attended college. However, it is a big step from here to the conclusion that "everyone" in the pool of potential executives avoided the draft by going to college. First, recall that per-capita death rates in Vietnam did not vary significantly by neighborhood income. To the extent that income predicts college attendance, this suggests that plenty of college-eligible males served in Vietnam. Second, Useem and Karabel (1986) find that 16.6 percent of top-level executives at large, public U.S. companies in 1977 had no college degree. Admittedly, their sample is nearly a generation ahead of mine. Because of trends in educational attainment, the corresponding figure for the population I study is probably lower. However, it seems a reasonable assumption that a non-trivial fraction of the pool of future executives was not enrolled in college during the Vietnam war.

2.3 Long-Term Effects of Career Shocks

The Vietnam draft lottery was a random shock that hit men just as they were entering adulthood and the workforce. There is a large body of empirical work examining the effects of exogenous shocks on individual employment outcomes, and it it is divided on the question of whether these shocks produce persistent effects. The vast majority of this work is concerned with macroeconomic shocks. Freeman (1979) and Welch (1979) show that the demographic shock of the baby boom generation's entry into the workforce caused relative wages for younger workers to fall in the 1970s. This makes the point that factors beyond workers' control – such as when they are born – affect economic outcomes, although it does not necessarily imply that the effects are long-lasting. Freeman (1981) finds that wages grew less rapidly for college graduates relative to high school graduates in the 1970s, when the market for college graduates was depressed. This result suggests that shocks to labor demand may not only affect the level of current wages, but also put workers on a lower wage trajectory. On the other hand, Beaudry and DiNardo (1991) find that current wages are negatively correlated with the lowest unemployment rate during the employee's tenure. In other words, employees can overcome early shocks when the labor market subsquently improves, and their wage is determined by the best labor market they experience. Devereux (2002) uses the Panel Study of Income Dynamics to study state-dependence in wages. He finds that 60 percent of the wage difference for otherwise comparable individuals is present four years later, and 50 percent for workers who have switched employers.

One problem in interpreting Devereux's results is that the decision to change jobs at midcareer is endogenous, and it is not immediately clear how the compo-

sition of workers in the labor market – and therefore wages – should be affected by demand shocks. On the one hand, one might expect firms to let their marginal workers go during economic downturns, which would lower the average quality of workers searching for jobs. On the other hand, marginal workers might be less likely to leave their firms voluntarily during downturns, so the average quality of those voluntarily switching jobs might be higher. More recent work avoids these problems by focusing on labor market shocks to new entrants. Kahn (2005) uses the National Longitudinal Survey of Youth to analyze the labor market performance of white male college graduates as a function of economic conditions when they graduate. She finds large, persistent wage effects that are not eliminated even 14-23 years out. Oreopoulos et al. (2006) analyze the long-term effects of graduating in a recession on earnings, job mobility and employer characteristics for a large sample of Canadian college graduates. They find that young graduates entering the labor market in a recession suffer significant initial earnings losses that eventually fade, but after 8 to 10 years. Raaum and Røed (2006) perform a similar study for Norway and come to a different conclusion. Individuals who graduate from secondary school in depressed local labor markets suffer relatively high unemployment over their entire prime-age working career. Oyer (2006a, 2006b) looks at labor market outcomes for specific occupations. Oyer (2006a) uses the state of the job market as an instrument for Ph.D. economists' initial job placement and finds that this placement is correlated with their long-term job outcomes (institution rank) and research productivity. Oyer (2006b) finds a causal effect of initial macroeconomic conditions (stock market returns) on the propensity of Stanford MBAs to choose Wall Street careers. This initial career choice is correlated with the probability of being on Wall Street later in the career, and it results in large

lifetime earnings differences that are attributable to macroeconomic conditions at entry into the labor market.

A small number of papers uses lower-level variation in economic conditions as the source of the labor market shock. Jacobson et al. (1993) use matched employee-employer data from Pennsylvania to study earnings losses of displaced workers. They find large and persistent wage effects of displacement. Baker et al. (1994) study a single firm and find wage differences among entry cohorts that persist over time. Von Wachter and Bender (2004) study long-term losses of German trainees who are separated from their training firm at the end of apprenticeship. They find wage losses of 15 percent that are eliminated within five years, but more persistent losses for workers leaving large firms.

Broadly, authors of these papers connect their results to one of two theoretical explanations. Authors finding no persistent effect appeal to search models of the labor market, in which labor market shocks temporarily worsen matches for job seekers, but in which workers can overcome this disadvantage by searching for an appropriate match. Authors finding persistent effects generally appeal to human capital models, in which early shocks to human capital acquisition make subsequent human capital acquisition more difficult and lead to persistent disadvantages in the labor market.[8] As suggested by the discussion in the preceding section, I interpret the Vietnam draft lottery as a shock to human capital formation.

In their estimations, authors of the more recent works surveyed here take great pains to deal with multiple endogeneity issues. The two main ones are (a) corre-

[8] These are the leading, but not the only, theoretical explanations for the persistence of initial labor market outcomes. Oyer (2006a) provides a good overview of relevant theory. Kahn (2005) contains a good discussion of alternative human capital explanations for the persistence of early career shocks.

lation of inital shocks with unobserved determinants of subsequent labor market outcomes and (b) endogenous timing of labor force entry (i.e., workers can "wait out" a bad labor market to enter under more favorable conditions). The first issue is particularly troublesome to the extent that empirical estimation relies on comparing different cohorts across time, since unobserved, time-varying differences between the groups may affect their labor market experience.[9] In contrast, I am dealing with a random lottery. The conditions affecting responses to the lottery are unrelated to the lottery number drawn. In other words, conditioning on the lottery number does not affect the distribution of unobservables that might affect short-term responses to the lottery or long-run labor market outcomes. The second limitation of the existing literature is overcome by the fact that the lottery was universal. If you were a 20-year-old male citizen, you had a lottery number, and you couldn't wait a year to enter a different lottery. It is true that men could defer the effects of their lottery number, but such behavioral responses are partly what I am interested in measuring. Men could not defer the assignment of their lottery number.

3 Hypotheses

As discussed above, other authors have documented both direct and indirect effects of the lottery on young men of the Vietnam era. Both of these might have affected career outcomes for this generation. The direct effect is actual conscription. The documented consequences of conscription leave little reason to believe that being drafted would have improved the draftee's long-term career prospects, and ample

[9] Even work that features contemporaneous sources of variation must contend with similar issues.

reason to think it would have harmed them.

The indirect effects relate to potential influences on the early labor market experience of men who ultimately were not drafted. One of these influences is discrimination by employers against men with low draft numbers. To the extent that initial job placements matter for future career success, these men would have been at a disadvantage in the long-run tournament for top executive positions.

The other indirect effect is individual responses to the draft. Assuming that men of this era viewed being drafted as a negative outcome, I will refer to these responses as "(draft) avoidance activities." The predicted effects of these responses are ambiguous. On the one hand, many responses would have reduced draft risk but also damaged long-term career prospects. Common methods of draft avoidance were fleeing to Canada, deliberately injuring oneself, intentionally fathering a child, developing a history of petty criminal behavior, feigning mental illness or homosexuality during the preinduction screening, and claiming membership in a subversive organization (Baskir and Strauss 1978). Since the draft boards who made the deferment decisions consisted of local community leaders, even the most innocuous of these methods were potentially stigmatizing and damaging to one's long-term career prospects.

On the other hand, some avoidance activities would have reduced draft risk and simultaneously improved long-term career prospects by leading men to acquire more human capital before entering the workforce. The classic example of such a response is working hard to maintain a college deferment. Another example is enrolling in Officer Candidate School (OCS) or the Reserve Officer Training Corps, which might have given men leadership skills that accelerated their civilian careers.

Even though some responses to the draft might have had positive side effects,

a simple optimization argument leads to the prediction that the net effect of the draft on men's careers should have been negative. Consider two groups of men, one at risk for being drafted and the other not at risk. In attempting to maximize their long-term career outcomes, men in the former group are constrained by the positive probability of being drafted and possibly also by discrimination from employers. Under the assumption that conscription reduces utility, these men will make choices that are suboptimal with respect to the unconstrained maximization of expected career outcome. Therefore, they should be underrepresented afterwards among men who meet some criterion for career success, such as attainment of a top executive position.

The optimization argument has two important implications for empirical analysis. First, since it is an argument about responses to the *possibility* of being drafted, information about actual draft outcomes is not required to test its main predictions. Second, because men had to make their choices before uncertainty about the draft ceiling was resolved, tests should be based on *ex-ante* assessments of draft risk – that is, beliefs formed after the lottery but before conscription began. Immediately after the first lottery in late 1969, there was widespread speculation about which lucky numbers would avoid the draft. Ex-ante projections for the 1970 RSN ceiling ranged from 150-300 (Rosenbaum 1970a). However, the most consistently reported projection was the Department of Defense's estimate that the bottom third of the lottery was certain to be called, the top third certain not to be called and the middle third uncertain (Rosenbaum 1969, 1970a, Kendall and Ross 1970). Therefore, I divide the sample into three groups: no risk (RSN 245-366), medium risk (RSN 123-244) and high risk (RSN weakly below 122). Collectively, the last two groups were the men at risk *ex-ante* of being drafted. This leads to

my first hypothesis: *Men with RSNs above 244 should be overrepresented, and men with RSNs weakly below 244 should be underrepresented, in the sample of corporate executives.*

A slight formalization of the draft avoidance decision under heterogeneous draft risk leads to additional testable predictions. Let $f(RSN)$ be the common prior for the probability distribution of the RSN ceiling, with cumulative distribution $F(RSN)$. Define $\pi(RSN) \equiv 1 - F(RSN)$, i.e., the probability that the final lottery ceiling exceeds RSN. Since call-ups were from lowest to highest lottery number, $\pi(RSN)$ is the *ex-ante* probability that a person holding lottery number RSN will be called for induction, with $\pi'(RSN) < 0$.[10] Let $p(a)$ be the probabilty of being drafted conditional on being called, where avoidance activity a reduces draft risk but is subject to diminishing returns. That is, $p'(a) < 0$ and $p''(a) > 0$. Assume that draftees suffer a fixed penalty $D > 0$. Let $C(a)$ be a strictly convex cost function, which would include the opportunity cost of other career-directed effort diverted to a.

Then men exposed to the draft choose a to minimize the expected penalty

$$p(a)\pi(RSN)D + C(a).$$

The first order condition is

$$-p'(a)\pi(RSN)D = C'(a)$$

Observe that $p'(a) < 0$ implies $-p'(a)\pi(RSN)D > 0$. Since $\pi'(RSN) < 0$, a^*

[10] This is a slight oversimplification. The Defense Department projections suggest that the risk profile might have been flat in some areas, implying $\pi'(RSN) \leq 0$.

– the optimal choice of a – is strictly decreasing in RSN. Intuitively, men with lower RSNs have a higher probability of being called for induction, so they have a stronger incentive to take actions that reduce their chance of being drafted if called.[11] To see this even more clearly, consider RSNs 1 and 244. Immediately after the lottery, RSN 244 sees a high probability that the draft ceiling will be below his lottery number and that he won't be called for induction. Under that outcome, any effort invested in avoiding the draft would be wasted. Therefore, he faces strong incentives to "trust to luck." In contrast, RSN 1 knows that the draft ceiling will be above his lottery number and that he will be called for induction if he does not act. In other words, he faces strong incentives to "help fate along," in the words of Baskir and Strauss (1978).

This model has a surprising implication for the relationship between RSNs and actual conscription rates. Note that the choice of avoidance effort, a, is influenced by the density $f(RSN)$, which reflects the *ex-ante* uncertainty about the draft ceiling and, therefore, the risk of being called up for a given lottery number. However, once the ceiling is established to be $\overline{RSN}$, this source of uncertainty evaporates. All lottery numbers weakly below the ceiling are called up with *equal probability* (100 percent), and all numbers above the ceiling are called up with zero probability. Then we can write the *ex-post* probability that a person holding

[11] Similar logic can be found in the literature on patent races. There, followers have relatively strong incentives to invest in advancing the technology, because they gain both the incumbent's monopoly profits and the additional rents associated with the new technology. Incumbents gain only the latter. See, for example, Reinganum (1983).

lottery number RSN is drafted as

$$d(RSN) = \begin{cases} p(a^*(RSN)) & RSN \leq \overline{RSN} \\ 0 & RSN > \overline{RSN} \end{cases}$$

In other words, conditional on holding a lottery number below the ceiling, the probability of being drafted is just the conditional probability $p(a)$, which is decreasing in avoidance effort a. Since $a^*(RSN)$ is decreasing in RSN, $d(RSN)$ is *increasing* in RSN. *Ex-post* draft probabilities are *lower* for low lottery numbers – the ones that, ex-ante, had *higher* draft risk.

While this result may seem counterintuitive, it is a straightforward implication of the maxmizing behavior posited above. Holding avoidance effort fixed, there is no reason to think that the government called up a higher percentage of men with RSN 1 than with RSN 195 (the 1970 ceiling). But within this group, who is more likely to have a deferment or exemption? Clearly, the man with RSN 1, because he had the strongest incentives to invest in "helping fate along."[12] This produces my second hypothesis: *Within the at-risk group, men at high risk for induction (RSNs 1-122) should be overrepresented in the executive sample relative to those at medium risk for induction (RSNs 123-244).*

As discussed above, some draft avoidance activities, such as flight out of the country, were likely to have been substitutes for human capital formation. Others, such as college attendance, were likely to have added to human capital. History shows that each of these routes was taken.[13] Men who avoided the draft through

[12] In principle, one could test this prediction directly using data relating induction rates to RSNs. I was told by the Selective Service System that no such data is available.

[13] Choosing to avoid the draft through activities that detract from human capital is not necessarily inconsistent with a model of optimizing behavior. It can easily explained by individual

activities that substituted for human capital formation are relatively unlikely to have survived into the final sample of executives. On the other hand, men who avoided the draft through avoidance activities that added to their human capital are more likely to appear in the sample. Therefore, conditional on appearing in the sample, the distribution of human capital among men with at-risk lottery numbers (RSNs 1-244) should be skewed upwards. Furthermore, this effect should be stronger for the lower lottery numbers. This is because men with lower lottery numbers had a higher *ex-ante* draft risk and would have invested more heavily in activities that added to human capital. This leads to my third hypothesis: *In the sample of corporate executives, there should be evidence of a negative correlation between human capital formation and RSN.*

In testing the above hypotheses, I focus on the 1970 draft, for several reasons. First, the sample size for men eligible for the 1971 and 1972 drafts is very small: there are only about 200 observations per year, not even enough to cover all the birthdays once. Second, there are many reasons to believe that the effects of the draft would have been strongest in 1970, the first year under the revised draft system. In 1970, the new system was not well understood by the participants or administrators, and inductions began just one month after lottery numbers were drawn. Participants in the later drafts had a year or more to observe the new system and see which avoidance strategies were most effective. They also had at least five months to choose their optimal course of action once their numbers were drawn. Also, after 1970 it was becoming clear that the war was winding down. Manpower calls were declining, and opposition to the war was growing.

heterogeneity in initial conditions, such as wealth or ability. For example, for some men, the monetary or effort costs of attending college might have been prohibitively high.

The likelihood of being drafted was declining, and incentives to take drastic action to avoid the draft were diminishing. Evidence that the draft was losing its effect as a social force after 1970 comes from Card and Lemieux (2000). They observe that, although there was an increase in college enrollments due to the draft, "draft avoidance behavior had little or no effect on the average schooling outcomes of men born after 1950 (p. 6)." Note that this is precisely the population eligible for the draft after 1970.

Analyzing the 1970 draft is complicated by the fact that it covered men born in the years 1944-50 and is not likely to have affected them equally. Only the men born in 1950 were facing the draft for the first time in 1970. Draft-exposed men born before 1948 are unlikely to have escaped one of the earlier drafts and are therefore unlikely to have been in the 1970 draft pool. There are two reasons for this. First, they constitute the older segment of the 1944-1950 birth cohort, and the draft before 1970 took older men first. Second, they are likely to have exhausted any college deferments – the deferment most likely to matter for my sample – by 1969, as they would have been at least 22 years old in that year. In contrast, men in the 1948-50 birth cohort are much more likely to have escaped previous drafts by virtue of age or a college deferment. I therefore examine the 1944-47 and 1948-50 birth cohorts separately.

4 Data

I use an original data set that has three components. The first is the Vietnam draft lottery numbers, which I obtained from the website of the Selective Service System: *www.sss.gov/lotter1*.

The second component is an extract of data used in Hayes et al. (2006) and generously provided by one of the authors (henceforth referred to as the HOS data set). The HOS data set is a listing of executives and their ages keyed to the ExecuComp database distributed by Standard and Poor's. ExecuComp contains compensation data, beginning in 1992, for over 2,500 public companies. The sample is drawn from public filings that, during my sample period (1992-2000), were mandatory for the highest-paid executive employee and, if their pay exceeded $100,000, the next four most highly paid executive employees. Nominally, ExecuComp contains each executive's age, but this data is missing from many of the records. The HOS data set provided to me contains the names and ages of 17,596 individuals appearing in the ExecuComp data set – approximately 90 percent of the total.

The final component is the website *www.anybirthday.com*. This site, now inactive, contained a searchable database of individuals' birth dates drawn from public records. In early 2003, I searched a total of 7,137 names from the HOS data set whose ages indicated that they might have been eligible for any of the draft lotteries conducted from 1970 onward.[14] I found unique matches and birth dates for approximately half this number (3,092).[15] Eliminating individuals not eligible for the 1970 draft (persons born before 1944 or after 1950, women) brought the ultimate sample size to 1,967 observations.

As the *anybirthday.com* data base was not comprehensive, one might be concerned about potential selection problems. The website provided little information

[14] The HOS data set only allows me to identify that an executive was a certain age in a certain year, so I can only pinpoint the birth year within a range of three years. For example, an executive listed as 50 years old in Fiscal Year 2000 (which may include calendar year 2001) could have been born in any of the years 1949-1951.

[15] The matching process is described in Appendix A.

on its data sources, but Jones (2004) has concluded that most of the data came from state motor vehicle departments. Release of this data varies from state to state, and even in states that release data, individuals can block release of their records. I maintain the assumption throughout that these potential selection effects do not bias my sample. A more serious concern is that the data set might have included military records, which would clearly introduce a bias. However, release of federal records is proscribed by the Freedom of Information Act and Privacy Act of 1974 (Jones 2002), so I assume my sample is not biased in this way.

5 Statistical Inference

I test the above hypotheses by partitioning the set of lottery numbers into cells and comparing the predicted and observed proportions of executives in each cell. The simplest analysis uses a two-cell partition. Consider for illustration a partition of lottery numbers into at-risk (RSN ≤ 244) and not-at-risk numbers. An individual born in 1950 had probability $p = 244/365$ of being born on a date placing him at risk for induction.[16] Therefore, the population of men born in 1950 and classified as either at risk or not at risk has a binomial distribution.

Under the null hypothesis that the draft had no effect on the pool of future executives, by the Central Limit Theorem, in my sample the proportion of those with at-risk birthdays, $\widehat{p}$, is asymptotically normal with mean p and variance $\frac{1}{n}p(1-p)$, where n is the sample count of individuals born in 1950.[17] The null hypothesis, $\widehat{p} = p$, is tested using the standard normal distribution[18]. If the draft had a

[16] This assumes that birthdays are randomly distributed. The expression for p is altered when a leap year is included among the birth years. I address the inclusion of leap years below.

[17] See for example Amemiya (1985).

[18] Technically, the hypothesis is $E[\widehat{p}] = p$, but I will abuse notation in the interest of a less

negative effect on career achievement, one should see $\widehat{p} < p$ (individuals born on at-risk birthdays are underrepresented in the sample of executives).

For partitions of greater than two cells, the inference is less straightforward. With two cells, the vector of predicted cell proportions, p, has only one degree of freedom and can thus be treated as a scalar.[19] This permits unambiguous directional comparisons of $\widehat{p}$ and p. With more than two cells, comparing predicted and observed cell proportions involves comparing vectors. Even if it is possible to reject the null hypothesis that $\widehat{p} = p$, one cannot generally show that $\widehat{p} > p$ or $\widehat{p} < p$.

Inference therefore consists of testing the null hypothesis that $\widehat{p} = p$ using the fact that in large samples, Pearson's Chi-squared statistic,

$$\chi^2 \equiv \sum_{i=1}^{N} \frac{(n_i - m_i)^2}{m_i},$$

has a Chi-squared distribution with $N-1$ degrees of freedom, where n is the sample size, $m_i = np_i$ is the expected cell frequency, n_i is the cell count, and N is the dimensionality of p (Agresti 1990). Although the test statistic is calculated using counts, for consistency I present and discuss the results in terms of proportions.

Note that this analysis makes no strong assumptions about the distribution of birth dates throughout the year. In particular, I am not assuming that birth dates are uniformly distributed. The only assumption is that the lottery randomly assigned birth dates to RSNs, meaning a given birth date is equally likely to fall into any of the broad RSN "buckets" I define. The only caveat is that the lottery

cluttered exposition.

[19] The elements of the vector p must sum to one and so $N-1$ elements completely determine the Nth element.

was not perfectly random. This opens up the possibility that certain groups of birthdays are associated with certain outcomes for reasons unrelated to the draft and that the non-random 1970 draft accidentally sorted men into one of these groups. Recall from above that statisticians found that late-year birthdays were overrepresented among low RSNs in the 1970 draft lottery. If late-year birthdays were disadvantageous for reasons unrelated to the Vietnam draft, then this would result in finding that men with low lottery numbers had worse career outcomes for purely spurious reasons. I control for this possibility in the analysis that follows.

Another potential concern is the very real possibility that I have mischaracterized the draft risk of a nontrivial fraction of my sample. This could arise, for example, through false matches in *anybirthday.com*, which would lead me to assign incorrect birthdays and, therefore, lottery numbers. Even if birthdays are correctly assigned, there are certainly men in my sample who were never at risk for being drafted, such as foreign nationals and men with permanent exemptions. Assuming that it is randomly distributed, such sample contamination will tend to bias against finding any effect of the draft. This is because the presence of men apparently but not actually in danger of induction will dilute the sample and cause the observed sample proportions to move closer to the values predicted under the hypothesis that the draft had no effect.

6 Results

6.1 Effects of the Draft on Career Outcomes

I first investigate the basic hypothesis that the draft had no lasting effect on the career outcomes of young men of the Vietnam generation. In Panel A of Table 2, I examine predicted versus observed representation in the executive sample for at-risk and not-at-risk lottery numbers. For the 1948-50 birth cohort, I find that the at-risk group is significantly underrepresented in my sample, as predicted. The group comprises 0.634 of the sample versus a predicted share of 0.668.[20] In contrast, there is no effect for the 1944-47 birth cohort.

As noted above, the 1970 lottery was criticized on the grounds that the low RSNs fell disproportionately on birthdays in the second half of the year. This could affect my results if late birthdays are associated with lower career attainment for some reason other than the draft. The results for the 1944-47 birth cohort suggest that this is not the case, however. If the negative effect were for reasons other than the draft, this cohort should be affected in the same way as the 1948-50 cohort. For further comparison, I applied the 1970 lottery numbers to executives born in 1940-42 and performed the same analysis.[21] This birth cohort was ineligible for the 1970 draft; furthermore it is similar to the 1948-50 cohort in that it covers three years including a leap year and has roughly the same number of observations. As Panel A shows, the observed and predicted fractions do not differ significantly.

[20] The predicted share is not $\frac{2}{3}$ because 1948 was a leap year. The lottery number corresponding to February 29 – RSN 285 – falls in the not-at-risk category. With three years represented in the 1948-50 birth cohort, it receives one-third the weight of the other lottery numbers in the computation of the predicted share.

[21] This sample was collected after the main sample. Its 735 observations are not included in the totals discussed in Section 4.

Next, I examine the hypothesis that the men at medium draft risk (RSNs 123-244) should be most underrepresented in the selection of executives. The results are in Panel B of Table 2. First, this partition is significant only for the 1948-1950 cohort but not the 1944-47 or 1940-42 comparison groups.[22] Second, for the 1948-50 cohort, men in the low risk category appear at close to the predicted frequency, while men in the medium risk category constitute only 0.301 of the observations versus the predicted fraction of 0.334.

Despite the rationale discussed above for dividing the 1944-50 birth cohort as I have, there still might be concerns that the division at birth year 1948 is arbitrary. One might wonder if these results are robust to other partitions of the 1944-50 birth years. I repeated the analysis in Panel B using birth cohorts 1946-50, 1947-50 and 1949-50. For the 1947-50 and 1949-50 cohorts the observed cell proportions are virtually identical to those reported in Table 2. The only difference to the reported results is that the Chi-squared statistic for the 1949-50 cohort is not significant, perhaps due to the small sample size. The 1946-50 results are qualitatively similar to the reported results but closer to the cell proportions predicted by the hypothesis that the draft had no effect. As discussed above, men born in 1946 were unlikely to have been exposed to the 1970 draft. If they were not exposed, adding them to the 1947-50 cohort dilutes the sample and hides the effect of the draft.

[22] One might object that rejecting the hypothesis $p = \widehat{p}$ for the 1948-50 cohort but not rejecting for the 1940-42 cohort obscures the fact that the Chi-squared statistics are actually fairly close to one another. However, $\widehat{p}_{40-42}$ and $\widehat{p}_{48-50}$ differ from p in very different ways. In the 1940-42 cohort, the RSN 123-244 group is actually overrepresented, whereas it is underrepresented in the 1948-50 cohort. It is important to recall here the purpose for using the 1940-42 cohort as a control: to rule out the possibility that the the RSN 123-244 group is underrepresented because these are somehow "bad" birthdays for reasons unrelated to the draft. Since this group is overrepresented in the 1940-42 cohort, this is clearly not the case. Finally, fixing $\widehat{p}_{40-42}$ and testing the hypothesis that $\widehat{p}_{48-50} = \widehat{p}_{40-42}$, I reject with p-value 0.0009.

Summarizing Table 2, the draft appears to have had an effect on the long-term career outcomes of the younger members of the 1970 draft cohort, regardless of the year chosen to distinguish "younger" and "older." The men in the at-risk group are underrepresented in the selection of corporate executives decades later. This is consistent with the argument that these men were constrained by the presence of the draft in making choices that influenced their long-term career outcomes. Furthermore, the negative effect is strongest for men who were at medium risk for induction. This is consistent with their relatively low incentives – compared with men in the high-risk group – to invest in draft avoidance activities.

This explanation depends in part on the notion that men who invested less in draft avoidance were actually drafted with greater frequency. Thus, within the medium-risk group, RSN 195 – the lottery ceiling – is a critical value. Above this number, men were not called for induction, so they were not penalized by conscription for their low investment in avoidance activities. Below the draft ceiling, conscription was a real possibility. In Table 3, I further partition the medium-risk group at RSN 195. This table shows that the results for the medium-risk group in Table 2 are driven by men with RSNs below the draft ceiling. The observed fraction of this subset – 0.175 – is significantly less than the predicted fraction of 0.200. While the observed fraction in the subset above the draft ceiling is also less than the predicted fraction, the difference is much smaller and it is not significant. One caveat here is that introducing the additional category robs me of statistical power. While tests of individual proportions show significant differences to the predicted values, I cannot decisively reject the hypothesis that the overall vector of observed proportions equals the vector of predicted values. The Chi-squared statistic for this 4-way partition of the lottery numbers is not

significant at conventional levels. Subject to this caveat, the results suggest that actual conscription contributed to persistent differences in career outcomes for men of this generation.[23] One might alternatively attempt to explain this result as follows: Because their ex-ante draft risk was higher, the men with RSNs 123-195 chose higher levels of avoidance activities that had human capital penalties, and this is why they are underrepresented relative to men with RSNs 196-244. But if this is true, it is doubly so for men with RSNs below 123, and that group should be most underrepresented in the executive sample, which it is not. Also, it would suggest that there should be evidence of a negative correlation between ex-ante draft risk and human capital accumulation. As I show in the next section, this correlation is positive, not negative.

6.2 Evidence of Differential Human Capital Acquisition

To investigate human capital acquisition within my sample, I look at three measures: executive rank, compensation and age of entry into the ExecuComp database. Table 4 contains the results for the first two measures. Executive rank is the maximum rank attained by an individual in the sample period. I define three categories: CEO, high executive (President, Chief Operating Officer and Chief Financial Officer) and low executive (all other titles). Compensation is the maximum annual compensation observed for an individual during the sample period and includes salary, bonus, option and restricted stock grants, and long-term incentive payouts.[24] I define three categories, corresponding with the bottom fourth,

[23] An alternative, but qualitatively similar, explanation is that large numbers of men with RSNs in the range 123-195 made insufficient investments in avoidance activities and, when they were called for induction, took rash measures to avoid service that imposed heavy career penalties. In either case, the mechanism is that they were "caught" by the draft.

[24] Amounts are 1992 equivalents.

middle half and top fourth of this distribution.

For each class of categories, I construct two-way contingency tables. These are simply matrices that partition the sample into cells according to the row and column categories. For both tables, the rows are the three RSN categories – low, medium and high – corresponding to the Defense Department's projections of ex-ante draft risk. The columns are the executive rank categories in Panel A and the compensation categories in Panel B.

In interpreting these contingency tables, it is helpful to think of the RSN category as the "treatment" and the column categories as the "response." The null hypothesis is that the rows and columns are independent of one another, i.e., that the response is independent of the treatment, or that the distribution of executives across the column categories is the same for low, medium and high RSNs. The test statistic is calculated as follows:

$$\chi^2 \equiv \sum_i^I \sum_j^J \frac{(n_{ij} - \widehat{m}_{ij})^2}{\widehat{m}_{ij}},$$

where i indexes the rows, j indexes the columns, n_{ij} is the number of observations in cell (i, j), and $\widehat{m}_{ij}$ is the estimated expected cell frequency obtained by multiplying the sample size n by the product of the observed marginal row i and column j sample proportions. This statistic has a Chi-squared distribution with $(I-1)(J-1)$ degrees of freedom (Agresti 1990).

For the executive rank measure, I cannot reject the null hypothesis of independence. There is no apparent relationship between RSN and these career outcomes. However, when comparing RSN with maximum annual compensation, the test statistic is highly significant for the 1948-50 cohort – the p-value is 0.054 – while

it is insignificant for the 1944-47 cohort. These results show that, although men in the highest RSN category are overrepresented in the executive sample, within this category, the distribution is skewed toward the lower compensation. This is consistent with the explanation that men with the highest RSNs were not exposed to the draft and – relative to men with lower RSNs – had less incentive to invest in human-capital-enhancing draft avoidance activities, leading them to end up in lower-paying jobs.

Finally, in Table 5, I present the results for the relationship between lottery number and age of entry into the ExecuComp sample. If early human capital acquisition is positively correlated with rapid career progress, and if the draft produced heterogeneous incentives for human capital acquisition in the manner discussed above, then men with lower lottery numbers should reach the executive ranks more quickly. In other words, there should be a positive correlation between lottery number and age of entry into the executive sample.

Testing this hypothesis is complicated by the fact that the sample is censored. Many individuals were already in top executive positions prior to 1992, which is the earliest year in which they can be observed. Furthermore, the censoring point is random, because the individuals already present in 1992 were of different ages. With censored data, ordinary least squares estimation is biased. Therefore, I use a censored normal regression. This is a tobit-style regression that is estimated using maximum likelihood with the assumption of normally distributed errors. The symmetry of the normal distribution permits estimation with a random censoring point.[25] The dependent variable is the age at which an individual enters the sample of top executives.

[25] See for example Amemiya (1985).

In Table 5, I interact the RSN and RSN squared terms with an indicator for the 1944-47 birth cohort, meaning that the uninteracted RSN coefficient is the marginal effect on the omitted category – the 1944-48 cohort. As predicted, age of entry into the executive sample is positively correlated with lottery number. In Column 1, the coefficient is 0.00805, meaning that an increase of 100 in the lottery sequence corresponds with a delay of 0.8 years in entering the executive ranks. Furthermore, the interaction terms for the 1944-47 cohort are negative, significant, and of similar magnitude to the corresponding uninteracted coefficients. Evaluated at the sample mean, the marginal effect of RSN on the 1944-47 cohort is weakly negative and insignificant (point estimate, -0.0006; standard error, 0.0009).

In Column 2, I introduce a control for the fiscal year-end market value, in 1992 dollars, of the executive's employer in the year in which he entered the ExecuComp database. The sign of this coefficient is counterintuitive – apparently men are younger on average when they enter the top executive ranks of larger firms, although one might expect large firms to have deeper hierarchies that take longer to ascend. In any event, the estimates in Column 1 are essentially unaffected by this additional control.

Summing up these results, they provide modest support for the hypothesis that the draft influenced human capital acquisition. Two of the three measures show evidence of increasing human capital acquisition with decreasing RSN. These results echo Angrist and Krueger's (1992) evidence of a draft-induced increase in college attendance, and they lend support to the argument that some men were able to partially offset the negative effects of the draft through activities that enhanced their human capital.

7 Conclusion

Overall, the evidence suggests that the Vietnam draft lottery did produce persistent career effects. Men with lottery numbers placing them at risk of induction are underrepresented among top U.S. executives more than 20 years after the war ended. This effect is concentrated in the middle of the lottery sequence. This somewhat counterintuitive result is actually a natural implication of a simple model of optimizing behavior among young men facing the draft, which predicts that relatively weak incentives to invest in draft avoidance activities lead to a higher actual incidence of conscription. Finally, I find that differences in lottery numbers explain differences in executives' compensation and age of entry into the top executive ranks in a way that is consistent with draft-induced differences in incentives to acquire human capital early in their careers.

This work complements existing work on the economic consequences of the Vietnam war by broadening the scope of analysis to include all men of this generation, not just those who served in the military. My results, particularly those on human capital formation, suggest that the draft had economic effects simply by distorting economic incentives to invest in human capital.

My results also add to the broader literature on the persistence of early labor market shocks. Existing empirical work is divided over just how persistent these shocks are. I show evidence that the shock of the Vietnam draft lottery explains differences in labor market outcomes more than 20 years after the initial shock. In addition, I contribute to this literature by studying a truly random shock that is unquestionably exogenous to unobservable determinants of labor market outcomes. This is an issue that looms large in existing empirical work, especially work fea-

turing longitudinal variation in the shocks. Furthermore, although my findings cannot be generalized in the same way as those from studies of macroeconomic shocks to labor demand, the Vietnam draft lottery is perhaps a better analogue for other important shocks displaying contemporaneous variation. Other accidents of birth – such as race, gender, family income and access to public amenities – might also affect labor market outcomes in important ways whose policy implications are quite different from those related to cyclical, macroeconomic shocks.

Finally, my analysis has some important limitations, stemming mainly from the fact that the data set is is not a comprehensive sample. It is a selection of men who experience a specific career outcome, and it does not contain complete work histories. This leaves a wide range of interesting questions beyond its scope. First, although I am able to draw indirect inferences about some of the mechanisms that might be producing the observed differences in career outcomes, I cannot examine these mechanisms in great detail. Second, it is natural to ask what the relative outcomes are for women of this generation. Women constitute a natural control group, since they were not drafted. However, women are simply too scarce in the sample of top corporate executives to be of use in this way. Finally, using attainment of an executive position as the measure of career "success" is perhaps overly normative. It is possible that the draft led men to choose other careers that were equally fulfilling. But even if this were shown to be true, it would simply be further evidence that the draft had a significant long-term impact on this generation's career outcomes.

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A Matching Names to Birth Dates

I matched the records returned by *anybirthday.com* against the searched names from the HOS data set to determine individuals' birth dates. There were two sources of uncertainty in matching. First, I only knew the individuals' birth years within a range of three years, so the possibility of finding multiple birthdays for the same name was not insignificant, especially for common names. Second, *anybirthday.com* does not allow middle initial as a search term and returns many records with blank middle initials. Therefore, there was the possibility that the true matching record in *anybirthday.com* had a blank middle initial while the corresponding record in the HOS data set had an identified middle initial.

I adopted what I consider a conservative set of matching criteria. There were two ways for a record to qualify as a unique match:

1. It was the only record for which the name matched exactly (first, last and middle initial) and the birth year was in the correct range.

2. It was the only record among those containing a blank middle initial for which the first and last names matched exactly and the birth year was in the correct year range. In addition, there were no other records in the correct year range with the same first name.

To clarify the second criterion, if the HOS data set contains a "John P. Smith" born in one of the years 1948-50, then if *anybirthday.com* returned the following records:

JOHN		SMITH	01-FEB-48
JOHN	Q	SMITH	25-JUL-47
JOHN		SMITH	03-AUG-51

the first John Smith in the list would be considered a unique match. If, however, the returned records were:

JOHN		SMITH	01-FEB-48
JOHN	Q	SMITH	25-JUL-47
JOHN		SMITH	03-AUG-51
JOHN	B	SMITH	21-JUL-49

then the first John Smith would not be accepted as a unique match. In this case, the presence of another John Smith born in the years 1948-50 indicates that this name is not sufficiently unique to conclude that John Smith and John P. Smith are the same person.

Table 1: Draft Outcomes for the Vietnam War Generation

							% of Total	% of Men
Vietnam Generation	53,100							
Women		26,300					49.53	
Men		26,800					50.47	100.00
Enlisted			8,720				16.42	32.54
Drafted			2,215				4.17	8.26
Never Served			15,980				30.09	59.63
Deferred, Exempted, or Disqualified			15,410				29.02	57.50
Disqualified				6,641			12.51	24.78
High Lottery Number					3,376		6.36	12.60
Lottery Loophole					633		1.19	2.36
Under Age, Death Before Induction					2,632		4.96	9.82
Deferred or Exempted				8,769			16.51	32.72
Failed Physical					5,276		9.94	19.69
Conscientious Objectors					172		0.32	0.64
Student Deferments					371		0.70	1.38
Graduate School						184	0.35	0.69
College						187	0.35	0.70
Occupational Deferments and Exemptions					483		0.91	1.80
Personal Status Deferments and Exemptions					2,467		4.65	9.21
Hardship, Marriage, or Fatherhood						2,420	4.56	9.03
Other						47	0.09	0.18
Apparent Draft Offenders			570				1.07	2.13

Source: Baskir and Strauss (1978). The Vietnam Generation is defined as anyone born between July 1, 1939 and June 30, 1954. Figures in thousands. Subcategories of men do not add up to the total due to rounding and extrapolation errors.

Table 2: 1970 Vietnam Draft: Predicted vs. Observed Cell Frequencies; Lottery Numbers Partitioned by Defense Department Call-up Projections

A. At-risk vs. Not-at-risk

RSN	Draft risk	p-pred.	p-obs.		
			1940-42	1944-47	1948-50
1-244	at risk	0.668	0.672	0.667	0.634
245-366	not at risk	0.332	0.328	0.333	0.366
n			735	1200	767
z			0.231	-0.074	-1.95*
			(0.82)	(0.94)	(0.05)

B. High-, Medium- and Low-risk

RSN	Draft risk	p-pred.	p-obs.		
			1940-42	1944-47	1948-50
1-122	high	0.334	0.306	0.337	0.332
123-244	medium	0.334	0.366	0.330	0.301
245-366	low/zero	0.332	0.328	0.333	0.366
n			735	1200	767
Chi-squared			4.00	0.09	5.18*
			(0.14)	(0.96)	(0.08)

Notes: p-pred. is the proportion of executives under the null hypothesis that the draft had no effect (birthdays randomly distributed among the population of executives); p-obs. is the proportion observed in the sample. For 1944-47, p-pred. differs in the fourth decimal place because only 1/4 of years were leap years vs. 1/3 for 1940-42 and 1948-50. Cells containing RSN 285 (corresponding to birthdate Feb. 29) have p-pred. adjusted to reflect the fact that this RSN was irrelevant for men not born in a leap year. In Panel A, z is the test statistic for the null hypothesis that p-obs.=p-pred.; two-tailed probabilities are in parentheses. In Panel B, Chi-squared is the chi-squared statistic for the null hypothesis that p-obs.=p-pred.; one-tailed probabilities are in parentheses. *Statstically significant at the .10 level; ** at the .05 level; *** at the .01 level.

Table 3: 1970 Vietnam Draft: Representation in Executive Sample, 1948-50 Cohort

RSN	Draft risk	p-pred.	p-obs.
1-122	high	0.334	0.332
123-195	medium	0.200	0.175 *
196-244	medium	0.134	0.126
245-366	low/zero	0.332	0.366 *
n			767
Chi-squared			5.27
			(0.15)

Notes: p-pred. is the proportion of executives under the null hypothesis that the draft had no effect (birthdays randomly distributed among the population of executives); p-obs. is the proportion observed in the sample. Chi-squared is the test statistic for the null hypothesis that p-obs. = p-pred.; one-tailed probabilities are in parentheses. Stars to the right of each reported fraction represent the significance level for the test that p-pred = p-obs for that individual proportion. *Statistically significant at the .10 level; ** at the .05 level; *** at the .01 level.

Table 4: 1970 Vietnam Draft: Career Outcome vs. Risk Category; Contingency Tables

A. Maximum Executive Rank Attained

	1944-47				1948-50			
RSN	**CEO**	**High Exec.**	**Low Exec.**	**n**	**CEO**	**High Exec.**	**Low Exec.**	**n**
1-122	0.191	0.205	0.604	404	0.101	0.156	0.374	255
123-244	0.194	0.255	0.551	396	0.086	0.154	0.343	231
245-366	0.165	0.243	0.593	400	0.115	0.200	0.388	281
Total	0.183	0.234	0.583	1200	0.101	0.170	0.368	767
Chi-squared		4.480				1.429		
		(0.345)				(0.839)		

B. Maximum Annual Compensation

	1944-47				1948-50			
RSN	**bottom fourth**	**middle half**	**top fourth**	**n**	**bottom fourth**	**middle half**	**top fourth**	**n**
1-122	0.233	0.505	0.262	404	0.231	0.514	0.255	255
123-244	0.258	0.490	0.253	396	0.204	0.541	0.255	231
245-366	0.253	0.540	0.208	400	0.313	0.477	0.210	281
Total	0.248	0.512	0.241	1200	0.253	0.509	0.239	767
Chi-squared		4.440				9.308*		
		(0.350)				(0.054)		

Notes: Cell entries are proportion of executives observed in the sample. Within an RSN category, proportions sum to one. Chi-squared is the test statistic for the null hypothesis that the rows of each table are independent of the columns; one-tailed probabilities are in parentheses. CDFs were computed using 1992 dollars. "Maximum" refers to the maximum reported value over the years in which each executive appeared in the ExecuComp database. *Statistically significant at the .10 level; ** at the .05 level; *** at the .01 level.

Table 5: 1970 Vietnam Draft: Effect of Lottery Number on Time to Reach Executive Level

Coefficient (Censored Normal Regressions)	**Dependent Variable: Entry Age**	
	(1)	(2)
RSN	0.00805*	0.00845*
	(0.00478)	(0.00493)
RSN * (1944-47 cohort)	-0.01083*	-0.01121*
	(0.00610)	(0.00629)
RSN squared	0.00002	-0.00002
	(0.00001)	(0.00001)
RSN squared * (1944-47 cohort)	0.00003*	0.00003*
	(0.00002)	(0.00002)
CDF of firm market value		-1.42725***
		(0.3000)
Observations	1967	1889
Censored Observations	764	743

Notes: The dependent variable is the age at which the individual entered the ExecuComp database, calculated by subtracting the birth year from the fiscal year of the first record. This variable is randomly left-censored because the dataset begins in 1992. All regressions include birth year dummies. P-values in parentheses. *Statistically significant at the .10 level; ** at the .05 level; *** at the .01 level.

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