Immigrants, Thank You for Your Service

by

Liesl K. Himmelberger

(Under the Direction of David B. Mustard)

Abstract

I use Army administrative and Current Population Survey (CPS) data to estimate the effects of federal-immigration policy on respective noncitizen enlistment rates, naturalized enlistment rates, and the job-risk selection of noncitizen enlistees using generalized difference-in-differences models. I exploit variation in unanticipated immigration-policy changes.

Chapter 1 estimates the impact of service-for-citizenship incentives on the Army-enlistment propensities of non-US citizens from 1994 to 2007. I find that peacetime enlistment for 17 to 26-year-old high-school equivalents increased by 133% after an asylum-to-green-card policy. I find that wartime enlistment for 17 to 26-year-old more-educated noncitizens 1) increased by 243% just after 9/11; 2) decreased by 367% after the Patriot Act; and 3) increased by 500% after the enactment of a wartime service-for-expedited-citizenship policy. On average, noncitizen enlistment increased by 0.44% relative to US citizens from 1994 to August 2001.

Chapter 2 estimates the impact of the establishment of US Immigration and Customs Enforcement (ICE) on the Army-enlistment propensities of naturalized citizens from 2000 to 2007. I find that enlistment for 17 to 26-year-old, high-school-equivalent or more-educated naturalized citizens increased by 67.7% after the establishment of ICE on March 1, 2003, significant at the 5% level, suggesting that fear of potential family-member deportment likely

drove these enlistment decisions. I find no evidence that naturalized citizens enlisted at different rates than native-born citizens on average over this period, illustrating assimilation of these first-generation Americans.

Chapter 3 estimates the impact of two federal-immigration policies on the job-risk selection of non-US-citizen enlistees from 1994 to 2007. I find that 17 to 26-year-old high-school equivalent noncitizens selected relatively safer military occupations after an asylum-to-green-card policy and selected riskier military jobs following a policy that granted automatic citizenship to green-card-holding children of naturalized citizens. I attribute these choices to a network effect from branches with greater non-white representation, fear of the impact of service-related death on noncitizen family members, and patriotism or role-model effects for children. Despite long-standing service-for-citizenship incentives, I find no evidence that noncitizen enlistees had different job-risk preferences than US-citizen enlistees on average.

INDEX WORDS: Enlistment propensity, Citizenship, Immigrant, Immigration policy, Military service, Naturalization, Section 329, Asylum, ICE, Military occupation, Military job

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Dedication

This dissertation is dedicated to the immigrant soldiers with whom I served in the US Army for over 12 years. Furthermore, it is dedicated to all past, present, and future immigrant-military servicemen and servicewomen who dutifully swore or will swear oaths to support and defend the Constitution of the United States from all enemies, both foreign and domestic.

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Disclaimer

All opinions expressed in this research represent the author's personal views and not those of the US Military Academy, the Army, or the Department of Defense.

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Chapter 1

Immigrants, Thank You for Your Service: Immigration-Policy Impacts on Noncitizen-Enlistment Propensity

1.1 Summary

This paper estimates the impact of service-for-citizenship incentives on the Army-enlistment propensities of non-US citizens from 1994 to 2007. I use Army administrative and Current Population Survey (CPS) data to estimate the effects of federal-immigration policy on noncitizen-enlistment rates using a generalized difference-in-differences model, exploiting variation in unanticipated immigration-policy changes. I find that peacetime enlistment for 17 to 26-year-old high-school equivalents increased by 133% after an asylum-to-green-card policy. I find that wartime enlistment for 17 to 26-year-old more-educated noncitizens 1) increased by 243% just after 9/11; 2) decreased by 367% after the Patriot Act; and 3) increased by 500% after the enactment of a wartime service-for-expedited-citizenship policy. On average, noncitizen enlistment increased by 0.44% relative to US citizens from 1994 to August 2001, significant at the 0.10 level.

1.2 Introduction

The United States historically used conscription to obtain soldiers in wartime, including the Civil War, World Wars I and II, the Korean War, and the Vietnam War. President Nixon established an all-volunteer force in 1973 after support for the draft eroded over the 1960s. Rostker (2006) notes that under this all-volunteer force, the military needed new methods to attract high-quality recruits. These methods included new marketing strategies, better

pay, educational subsidies, enlistment bonuses, varying enlistment-contract lengths, career options with civilian relevance, and better housing, child care, and health benefits. A significant enlistment incentive unique to non-US citizens is an expedited-naturalization pathway based on military service. This paper analyzes whether these enlistment incentives, as well as other immigration-policy changes, served as behavioral mechanisms to prompt non-US citizens to enlist at different rates than those of US citizens.

This is the first and only study of the effect of peacetime citizenship-for-service incentives on noncitizen Army enlistment relative to that of US citizens; additionally, it is the first and only study to examine the impact of an asylum-to-green-card policy on LPR enlistment relative to US citizens. This paper is the first study to find positive, significant effects of wartime citizenship-for-service incentives on noncitizen Army enlistment relative to that of US citizens. It uses Army administrative and basic monthly Current Population Survey (CPS) data, which I collapse into cells by two-age group, sex, citizenship status, and enlistment quarter. I isolate lawful-permanent residents (LPR) from the broader noncitizen population via educational criteria, utilized in part to estimate the service-eligible population. I use a general difference-in-differences (DinD) model to analyze the impact of citizenship-for-service incentives and other immigration-policy changes on noncitizen-enlistment rates in respective peace and wartime periods, exploiting immigration policy changes as sources of exogenous variation.

Over the peacetime period from 1994 to August 2001, I study the effect of a policy that granted green-card status to asylum seekers on noncitizen-enlistment rates relative to those of US citizens. My model shows that 17 to 26-year-old non-US citizen enlistment rates increased after the Nicaraguan Adjustment and Central American Relief Act (NACARA) – a late 1997 reform that enabled green-card status for specific asylee groups. From 1998 to August 2001, LPR enlistment increased by 133% (about 140 more enlistees per quarter) relative to that of US citizens, significant at the 5% level using a two-tailed test based on

my DinD model.[1][2] LPRs had a 0.440% higher average Army-enlistment propensity than US citizens from 1994 to August 2001, significant at the 0.10 level, providing the first evidence confirming the positive impact of a long-standing peacetime expedited-citizenship-for-service incentive.

During the wartime period from 1999 to 2007, I study the dual impact of 9/11 and the Patriot Act on noncitizen enlistment rates relative to those of US citizens. In wartime, 17 to 26-year-old non-US citizen enlistment rates increased relative to those of US citizens after a post-9/11 policy that expedited naturalization for military service. Noncitizen enlistment increased by 500% for those with a high-school-equivalent but less-than-college education and 378% for high-school equivalents, both significant at the 5% level (respectively 51 and 46 more LPR enlistees quarterly). I find higher non-US citizen enlistment rates between 9/11 and this enactment, suggesting anticipation of this expedited-citizenship policy. Noncitizen enlistment increased by 243% for the more-educated specification and 190% for the high-school equivalents, respectively significant at the 5% and 10% levels (about 127 and 111 more LPR enlistees quarterly). The Patriot Act decreased LPR enlistment by 367% for the more-educated specification and 270% for the high-school equivalents, respectively significant at the 5% and 10% levels (275 and 192 less LPR enlistees quarterly).

Expedited citizenship-for-service policies are important to LPRs because they face high-temporal costs in the interim prior to naturalization. Contrary to naturalized citizens, LPRs must petition for their spouses and unmarried children to receive LPR status, but there is a 5-year waiting list and annual quotas for the number and types of green cards.[3] They are subject to inadmissibility grounds based on medical criteria, criminal acts, or likelihood of welfare dependence, and deportation grounds if they commit crimes, violations, or even fail

[1]The 140 quarterly increase reflects an extra 2,104 personnel from a baseline of 1,582.

[2]All subsequent statistical-significance figures for my DinD estimates reflect two-tailed tests based on my DinD model. I will use shorthand stating that DinD estimates are 'significant at the 5% level.'

[3]Cunha et al. (2014) notes that the United States issues 480,000 family-sponsored green cards annually.

to report address changes to US Citizenship and Immigration Services (USCIS). LPRs can lose their status if they leave for over a year, cannot vote, face government job and benefit constraints, and must reapply for a physical card every ten years for a fee.[4] They wait a minimum of five years to apply for naturalization; although, exceptions include a three year wait via marriage, a three (now one) year wait for peacetime-military service, and one day wait for wartime service.[5]

The academic literature largely overlooks immigrant-military service.[6] Three papers conduct anecdotal and descriptive examinations of military service as a path to earned citizenship in Muslim, Latino, and Asian communities (Sandhoff, 2013; Sullivan, 2014; and Harvie, 2014, respectively); whereas, my paper studies a natural experiment using Army-administrative data. Two research reports, Hattiangadi et al. (2005) and McIntosh et al. (2011), analyze performance measures of noncitizen-service members. While I consider the impact of immigration policies on enlistment, McIntosh et al. (2011) examine citizenship impacts on attrition and citizenship attainment by service. Zong and Batalova (2019) summarize Census Bureau information on foreign-born veterans, showing a 28.6% decline in total veterans from 1995 to 2018, while the immigrant share of veterans increased from 2 to 3%. The Congressional Research Service summarizes laws, policies, and issues that affect foreign-born-service members.[7]

My paper most closely aligns with Cunha et al. (2014) – the first and only empirical

[4]https://www.immi-usa.com/green-card-vs-citizenship/.

[5]The marriage exception requires one to reside with the US citizen or be a battered spouse of a US citizen. Other exceptions include automatic naturalization for children under 18 after February 2001 if at least one parent is a citizen and the child is a physically present LPR, in physical custody of the US parent (via the Child Citizenship Act); immediate application for spouses of certain government employees living overseas; all time as a refugee counting toward the five years, and one year of time as an asylee counting toward the five years. Source: https://www.nolo.com/legal-encyclopedia/when-apply-us-citizenship-46704.html.

[6]Immigration status is of pragmatic relevance to the military, in terms of language skills, cultural diversity, and job placement into occupations that require security clearances.

[7]For example, see Congressional Research Service Report RL31884 on citizenship via service, dated February 25, 2009.

study of the impact of the post-9/11 citizenship-for-service policy on military accessions.[8] They use a DinD model to examine the impact of Executive Order 13269 on total noncitizen accessions from 1999 to 2010, using restricted-personnel data from all military services and accessions among US citizens as their control group. Cunha et al. (2014) examine total monthly accessions over the CPS monthly eligible population, defined as high-school educated or more 18 to 29 year olds in the labor force as per Hattiangadi et al. (2005), for all services, minus the Coast Guard. They find no effect of the expedited citizenship policy on total noncitizen accessions; yet, they identify shifts of LPR enlistments out of combat intensive services and into safer ones.

While I use a DinD empirical strategy, my paper is different from Cunha et al. (2014) in several ways. My paper examines Army enlistment while Cunha et al. (2014) examines enlistment across all services. The first portion of my paper examines a period prior to 9/11, analyzing the impact of the peacetime analog of the expedited-citizenship policy on noncitizen enlistment. For the latter wartime portion, differences include a time frame preceding the Great Recession, a tighter service-eligible age group, precursory treatment effects for the anticipation of Executive Order 13269 and the Patriot Act, and immigration, demographic, and immigration-policy controls.[9] I dually treat for the post-9/11 period and the Patriot Act, dividing the post-9/11 period into the anticipatory period before and after the enactment of Executive Order 13269; whereas, Cunha et al. (2014) exclusively treat for Executive Order 13269. Data disaggregated by sex, two-year age group, citizenship status, and enlistment quarter enable the creation of explanatory controls, rather than data based on total monthly accessions over the relative populations that only allow time-based controls. While Cunha et al. (2014) only exclude those with less than a high-school equivalency, I use tighter educational specifications to limit service-ineligible noncitizens within the service-eligible population. I use CPS data to construct birth region and period of US arrival controls,

[8]Cunha et al. (2014) is an updated and published version of Can and Yalcinkaya (2013).

[9]I study a narrower wartime period from 1999 to 2007, excluding confounding factors after 2007, as well as an age group from 17 to 26 rather than 18 to 29.

which are critical to study an immigrant population over time; whereas, Cunha et al. (2014) only use CPS data as the denominator of their dependent variable.

My paper aligns with three themes in the economics literature. First, it fills a gap in immigrant-worker and public sector literature, analyzing noncitizen-military service over time. Second, it touches on occupational choice, since US-military service is an alternative to civilian employment. Third, it contributes to military literature on recruitment incentives and enlistment propensity.[10] The paper proceeds as follows. Section 2 provides background on military enlistment, the military-service sections of the Immigration and Naturalization Act, and US-immigration policies from 1994 to 2007. Section 3 discusses my data and provides descriptive statistics. I describe my empirical strategy in Section 4 and discuss my results in Section 5. Section 6 briefly concludes. For the remainder of the paper, I use the terms 'noncitizen' and 'green-card holder' to identify lawful-permanent residents (LPR), who were born abroad, identify as non-US citizens, and are service eligible.[11] I use the term 'native-born' to identify US citizens who were born in the United States, in a US territory, or to US parents abroad. I use the term 'naturalized' citizens to identify individuals who were born abroad, but identify as naturalized-US citizens.

1.3 Institutional Background

Naturalization can take several years if a non-US citizen lacks a US-citizen relative, US citizen whom they marry, or future employer lobbying for them. Historically, noncitizens can and have used military service as a means to gain US citizenship, although noncitizens can and have served without applying for naturalization.

[10]Political-scientific and historic literature on the 'citizen-soldier' indirectly relates to this research, examining the democratic link between service and earned citizenship. Krebs (2009) argues that the all-volunteer force strengthens the link between US-citizenship ideals and military service, rebutting arguments that ending the draft severed this relationship.

[11]Lawful-permanent residents are aliens admitted to the United States, according to USCIS. They are legally permitted to permanently reside in the United States.

1.3.1 Selective Service and Enlistment Requirements

Modern Selective Service registration requirements for 18 to 25-year-old men started in July 1980.[12] Columns 1 and 2 of Table 1 show a time line of key events from the end of the Vietnam draft to the post-9/11 period. While applicable to all male-US citizens, non-US-citizen and dual-national men residing in the United States must register with the Selective Service System, making it a prerequisite for naturalization.[13] Currently, women are not required to register for the Selective Service; however, in 2017, the Pentagon recommended a change to this policy since the Department of Defense opened combat roles to women in 2015 (Manchester, 2017). My all-volunteer sample excludes birth cohorts that were age-eligible for the draft-era Selective Service.[14]

Voluntary enlistment into the US military falls into one of two categories. Individuals either join the military as enlisted-service members or as officers who serve as leaders and managers.[15] Minimum qualifications to enlist in the US Armed Forces include being a US citizen or LPR, being at least 17-years old (with parental consent), passing a physical-medical exam, and having a high school diploma. Each service permits a limited percentage of recruits with a General Education Development (GED) credential per year, but these

[12]Men born prior to 1958 were eligible for the Vietnam draft. See https://www.sss.gov/.

[13]Exceptions include noncitizens present for less than a year, dual nationals with exemptions between their countries and the United States, and noncitizens who served a year or more in another country's military (with whom the United States shares a defense treaty). Bilateral treaties may exempt specific non-US citizens and dual nationals.

[14]Self-selection into the military complicates causal studies of service outcomes. Angrist (1990) and Angrist and Krueger (1992, 1994) use the draft lottery for exogenous variation to compare earnings, educational, and health outcomes of draft-era veterans relative to those of non-veterans. Goodman and Isen (2017) use this method to examine military-dependent outcomes and Card and Cardoso (2012) and Bingley et al. (2014) use random conscription in Portugal and Denmark to examine veteran versus non-veteran-earnings outcomes; whereas, Heckman (1997) argues that the draft lottery is a poor or invalid instrument for military service. Angrist (1995, 1998) uses AFQT re-norming as an instrument to examine the causal impact of voluntary service. This paper avoids the self-selection issue by analyzing immigration-policy impacts on enlistment rates of the service-eligible population.

[15]Most officer programs require a collegiate degree at a minimum and are very competitive.

exceptions must achieve higher threshold scores on the Armed Forces Qualification Test (AFQT). Each service has slightly different enlistment requirements.[16]

1.3.2 Immigration and Naturalization Act - Sections 328 and 329

The Immigration and Naturalization Act (INA) includes two categories of service eligibility for naturalization, as well as sections granting benefits to military dependents.[17] Section 328 applies to peacetime naturalization, and Section 329 applies to naturalization during designated wartime 'periods of hostility,' which include World War I, World War II, the Korean War, the Vietnam War (February 28, 1961 to October 15, 1978), the Persian Gulf War (August 2, 1990 to April 11, 1991), and 9/11 to the present.[18] Column 3 of Table 1 shows how these two immigration policies aligned with key military events over time.[19] Both sections require that an applicant serve honorably, have fealty to the US Constitution and to lawful order in the United States, show a basic knowledge of US history and government, and be able to read, write, and speak basic English.

Several eligibility requirements become less stringent in wartime. First, the required period of qualifying service for eligibility was three years in peacetime, compared to one day in wartime.[20] Applicants must be 18 years old or older in peacetime, but there is no wartime age limit. Peacetime applicants must display good moral character for at least five years prior to filing until their naturalization, while the requirement shrinks to one year in wartime. An applicant must be a LPR at the time of examination during peacetime, while in wartime, the applicant can also be physically present in the United States or its outlying

[16]For example, an Army recruit is typically required to be no older than 35, achieve an AFQT score of 31, and have no more than two dependents. Any exceptions to these policies require waivers.

[17]INA Section 319 permits spouses of naturalized citizens serving in the Government to apply for naturalization, with some residence and physical presence requirements modified for military spouses stationed overseas with their service members. See https://www.uscis.gov/military.

[18]INA Section 329A covers posthumous naturalization in wartime.

[19]Column 4 illustrates how these policies align with my post-9/11 treatment effect in Section 4.1.

[20]On November 24, 2003, President Bush lowered the peacetime service requirement from three years to one year, backdating previous members' service to allow citizenship application. See Public Law 108-136.

possessions at the time of enlistment, reenlistment, or induction into the military.[21] Finally, applicants must meet residence and physical presence requirements in peacetime, while they are exempt from these requirements in wartime.[22]

President George W. Bush enacted Section 329 on July 3, 2002 in Executive Order 13269 and backdated it to 9/11 —an anticipated event based on historical precedent.[23] On November 24, 2003, the National Defense Authorization Act enhanced Section 329A with posthumous naturalization benefits for spouses, children, and parents of service members who die in combat. In October 2004, this law dropped federal naturalization fees for peace and wartime service members and allowed naturalization application from overseas.

Hattiangadi et al. (2004) examine recruitment incentives to attract Hispanics and several papers examine the impact of monetary recruitment incentives.[24] Apart from INA Sections 328 and 329, no recruitment incentives encouraged or discouraged non-US citizen enlistment relative to their US-citizen counterparts from 1994 to 2007. The US military values pragmatism and mission accomplishment above all else, neither promoting a mercenary-recruitment agenda nor dissuading noncitizen participation. This citizenship-neutral assertion is based on Army recruitment policy.[25]

[21]Physical presence is the number of days that an applicant must physically be present in the United States during the required period until he or she files for naturalization, as per USCIS.

[22]Under Section 328, the applicant must have continuously resided in the United States for at least five years and been physically present for at least 30 months of the five years immediately preceding the date of application, unless he or she filed an application while still in the service or within six-months of separation. See the USCIS Policy Manual / Volume 12 / Part I / Chapter 2 and Chapter 3.

[23]I qualify the Section 329 enactment starting on July 1, 2002, even though it is dated July 3. The most recent, retroactive enactment of Section 329 occurred on November 22, 1994 for veterans who served in the Persian Gulf War. See Executive Order No. 12939, 59 FR 61231.

[24]These papers include Goldberg and Warner (1982), Dertouzos (1985), Polich et al. (1986), Asch and Warner (1995), Payne et al. (2001), Hansen and Wenger (2002, 2005), Hosek and Totten (2002), Hogan et al. (2005), Simon and Warner (2007, 2009, and 2010), Hosek and Martorell (2009), Asch et al. (2010), and Simon et al. (2010).

[25]Former Army recruiter Sergeant First Class (Retired) Gerald Isbell noted the following regarding this period: 'The Army offered no additional incentives for non-US citizens to enlist, even for translators. AFQT waivers were available for in-demand translators, accepting a slightly lower score. The AFQT was available for noncitizens in their native languages, but they would have to attend an English language course either before or after Basic Combat Training and Advanced

1.3.3 Non-Military Immigration Policy in Peace and Wartime

Table 2a outlines federal-immigration policies that occurred during the peacetime period from 1994 to August 2001. Immigration policy remained stable for several years after the Immigration Act of 1990. Congress passed the Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) on September 30, 1996, one month following a law that granted federal benefits to some noncitizen categories.[26] IIRIRA tightened restrictions on legal and illegal US immigration; yet, these policies had a relatively insignificant impact on peacetime-LPR enlistment.

In late 1997, the Nicaragua Adjustment and Central American Relief Act (NACARA) softened the impact of IIRIRA on asylum seekers from civil wars in Nicaragua, Cuba, El Salvador, Guatemala, and Eastern Europe. NACARA Section 202 granted LPR status to Nicaraguan and Cuban asylees up until a deadline of March 31, 2000, if they remained in the United States for five or more years since December 1, 1995. Salvadorian, Guatemalan, and former-Soviet asylees who unsuccessfully pursued LPR applications via other means could apply under NACARA Section 203; NACARA remained an asylum to LPR option after the March 2000 deadline via a more indirect path.[27] Two additional peacetime policies followed NACARA: (1) the Haitian Refugee Immigration Fairness Act (HRIFA) added Haitian asylum seekers to NACARA in October 1998 and (2) the Child Citizenship Act (CCA) granted automatic citizenship to children of naturalized or native-born US citizens if the children were LPRs and aged 17 years or younger in February 2001.

Table 2b lists federal-immigration policies that occurred during the wartime period

Individual Training (vocational training for military occupations). Their initial terms of enlistment could not exceed eight years.'

[26]The Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), enacted on August 22, 1996, opened federal benefits to some refugees, some LPRs, and veterans and their families.

[27]NACARA Section 202 and 203 of Public Law 105-100. Wikipedia states that Section 202 applicants were about 95% Nicaraguan and 5% Cuban (of 70,356 people), and Section 203 were about 65% Salvadorian, 30% Guatemalan, and 5% former-Soviet Union asylees (of 160,102 people) from 1998 to 2007.

from 1999 to 2007.[28] On October 26, 2001, the Patriot Act increased governmental anti-terrorism and surveillance powers, asserting its authority to monitor any immigrant deemed a threat to national security.[29] In late November 2002, the Homeland Security Act (HSA) established the Department of Homeland Security, reorganizing the former Immigration and Naturalization Service (INS) into three subordinate agencies on March 1, 2003 – US Citizenship and Immigration Services (USCIS), US Immigration and Customs Enforcement (ICE), and US Customs and Border Protection (CBP). The National Defense Authorization Act of November 2003 expanded and funded the Iraq War with clauses increasing retroactive posthumous-citizenship rights for family members and increasing Section 329 benefits as of October 1, 2004.[30] Several confounding events after 2007, including the Great Recession, the 2008 expansion of Section 329 to non-LPRs, and Deferred Action for Childhood Arrivals (DACA) in 2012, make it an ideal final year for this study.[31]

Table 3a shows policies during peacetime (1994 to August 2001) and Table 3b shows policies during wartime (1999 to 2007). These tables align each immigration policy with anticipated mechanisms unique to noncitizens that might affect their enlistment decisions. If expedited citizenship-for-service incentives drive choices, I anticipate a positive effect on enlistment rates in peace and wartime. If the fear of potential deportment motivates LPR-enlistment decisions, unfavorable policies have an indeterminate-ex ante sign, as noncitizens might enlist to avoid deportment or recede from government visibility. If a policy increases

[28]Note that the first eleven wartime quarters overlap with the latter eleven peacetime quarters in Table 2. During this overlapping period, the HRIFA and CCA have an insignificant impact on naturalized enlistment propensity; although, I use them as peacetime policy controls to test the stability of my forthcoming NACARA treatment effect.

[29]I use quarterly-enlistment periods to differentiate the post-9/11 period from the Patriot Act.

[30]October 2004 improvements to Section 329 include dropping federal fees for filing naturalization petitions or gaining naturalization certification (minus State-level charges) and availability of applications, interviews, filings, oaths, ceremonies, or other naturalization proceedings for service members through US embassies, consulates, and, as practicable, US military installations overseas.

[31]In 2008, the Department of Defense initiated Military Accessions Vital to the National Interest (MAVNI) as a pilot recruitment program under Section 329 to fill critical shortage occupations, like surgeons and foreign nationals fluent in strategic foreign languages. This policy permitted refugees and asylum seekers to enlist while awaiting their green cards, but became entangled in litigation when some noncitizens failed their background checks. See Bowman (2017) and Horton (2017).

institutional trust or patriotism, I anticipate a positive effect on LPR-enlistment rates. Cunha et al. (2014) suggest that a negative wartime noncitizen-enlistment response might indicate a lower risk threshold for combat death than US citizens, following Christensen (2017).[32] Finally, Section 3.2 acknowledges a potential mechanism driving recruiter behaviors, given additional processing requirements for LPRs that may result in lower noncitizen-enlistment rates.

1.4 Data

The data are bundled cells based on basic monthly CPS (from January 1994 to December 2007) and Army administrative data, including cell-level records generated from all enlistees who entered the Army from 1994 to 2007.[33] Soldiers appear once in the Army administrative data; if they reenlisted, I count them according to their initial enlistment dates so I never see the same person repeatedly. The CPS contains self-identified citizenship designations, birth countries, and periods of US arrival. The CPS and American Community Surveys (ACS) include insufficient data to analyze the small population of immigrant enlistees; thus, I integrate restricted-Army-enlistment data with the CPS data set. The individual-level data contain exact dates of active-duty Army enlistment and entry-citizenship designations.

1.4.1 Descriptive Statistics and Data Model

I supplement Army-enlistment counts with service-eligible population data, since only about 0.5% of the population enlists in the active-duty military.[34] McIntosh et al. (2011) note that an ACS-population supplement has advantages in analyzing noncitizen enlistment given its size and English-proficiency and foreign-language variables; yet, the basic monthly

[32]Christensen (2017) notes that increased-wartime casualties induce fewer people to enlist. These works suggest mechanisms that drive enlistment choice.

[33]I accessed the Census data at IPUMS CPS and a Total Army Personnel Database extract on site at the Office of Economic Manpower Analysis (OEMA) at West Point, NY.

[34]The active-duty Army comprises about 36.6% of this percentage, making it 0.18% of the population. Reynolds and Shendruck (2018).

CPS enables creation of quarterly (as opposed to yearly) and treatment effects for sequential immigration policies. The CPS excludes members of the Armed Forces from its survey of 60,000 households; thus, it automatically excludes those who enlisted in an alternative service, like the Navy or Air Force.[35] I assume that the CPS is representative of service-eligible individuals.

Tables 4a and 4b list sample restrictions by identical criteria, which display respective Army administrative and unweighted-CPS population data.[36] I drop observations missing key variables, including citizenship status, sex, age, Hispanic ethnicity, or educational level. I omit individuals from birth countries with ambiguous-citizenship designations (detailed in Section 3.3) and those where immigration years or current-citizenship designations do not align with respective ages or entry citizenship. The sample excludes enlistment quarters prior to 1994 or after 2007, September 2001, and individuals older than 26 years of age. Finally, I drop observations that have less than a high-school equivalency and those with a college degree or more education.

Figures 1a to 1f compare individual non- and US-citizen characteristics in the Army-administrative data (left-hand columns) to the weighted-CPS population for ages 17 to 26 (right-hand columns) from 1994 to 2007. Figure 1a shows that the number of noncitizen enlistees decreased from 1999 to 2007, while its relative service-eligible population grew; rather, US citizen enlistment and the relative population remained relatively constant over the period. Figure 1b denotes that 18 year olds make up the greatest percentage of enlistees; however, noncitizen enlistees are a bit older than their US citizen counterparts on average. Figure 1c shows that about 91% of enlistees have a high-school equivalency, relative to a more educationally diverse population; hence, I drop college or more educated observations. My secondary specification drops some college and associates-degree holders, leaving the high-school equivalent population most-likely to enlist (detailed in Section 3.2).

[35]Representative cells prevent me from counting the same individual over multiple surveys.

[36]I round each restriction in Table 4a to the nearest 100 to safeguard the immigrant-enlistee data.

Figure 1d displays the percentage of enlistees based on racial and sex categories from 1994 to 2007. Overall, women comprised 18.4% of the active-duty Army, while blacks accounted for 18.9%, Hispanics made up 9.7%, whites constituted 66.6%, and other racial groups comprised 4.7%. Blacks appear to have enlisted more than their relative populations (especially black women); whereas, Hispanic noncitizens and white-female citizens appeared to enlist less than their similar populations. Figure 1e shows marital percentages by Army enlistees and the CPS population. Enlistees are about 10% more likely to remain single, relative to the CPS population. Finally, Figure 1f depicts the percentages of entry contract length for noncitizens versus US citizens, showing that almost half of noncitizens enlist for three years, while US citizens are more likely to enlist for 4 to 6 years.

Table 5 shows summary statistics for the CPS-population sample, showing all variables in my forthcoming model. The mean age of the sample is 21.8 and 6.4% are noncitizens. Rather than displaying a descriptive statistics table of the Army administrative data (given its restricted nature), Figures 1a through 1f illustrate key relationships via count data. I create a count of active-duty Army enlistees, disaggregated by sex, two-age group, citizenship status, and enlistment quarter, which serves as the numerator of my dependent variable. Counts of weighted-CPS demographic variables, divided by the same four categories, serve as the numerators for my control variables. Finally, counts of the weighted-CPS populations disaggregated by sex, two-age group, citizenship status, and enlistment quarter serve as the denominators for my dependent and control variables.

I collapse the individual-level data into cells, sorting the counts of Army enlistments, weighted-CPS populations, and weighted-CPS demographic controls into cells by two-age group, sex, citizenship status, and enlistment quarter (or quarter surveyed). IPUMS-CPS recommends using weights that reflect the 'population represented by each individual in the sample' for person-level analyses; thus, I generate my bundles using weighted samples. As a final step, I divide each count variable by its respective population (except for family size) to generate percentages reflective of each group.

1.4.2 Service-Eligible Sample Selection

I isolate the service-eligible population that chooses between Army enlistment and other alternatives. Given the physically demanding nature of military service, over 93% of individuals enlist between the ages of 17 and 26. Given that about 94% of 17-year olds are still in high school or have less education, the small population with a high-school equivalency is likely biased toward home schoolers and states where children enter high school at a younger age.[37] I cannot just drop 17-year olds from my sample because they comprise about 6% of enlistees.[38] Two-year age groups alleviate this concern, creating a more comprehensive group of high-school-equivalent 17 to 18-year olds, assuming that there is no significant difference between the enlistment choices of 17 and 18-year olds. I use five two-year age groups starting with age 17 and ending with age 26.

I drop observations with less than a high-school equivalency, which Cunha et al. (2014) use to isolate the service-eligible population. GED holders and high-school dropouts, by exception, cannot exceed more than 10% of annual Army enlistment; hence, less than 1% of active-duty enlistees are high-school dropouts. Limitations on recruiting high-school dropouts remained constant from 1994 to 2007, despite a 2005 policy that increased the threshold for GED waivers.[39] Ninety or less noncitizen high-school dropouts enlisted from 1994 to August 2001 and five or less enlisted from 1999 to 2007, making their inclusion not worth the risk of incorrectly assigning CPS-population-level probabilities to representative cells that should be empty.[40] I control for the GED-policy change by using a high-school equivalency variable,

[37] I took the CPS sample from Table 4b, Row 7 (2,771,812) and dropped all non-17-year olds (leaving 319,738). 18,773 had a high-school equivalency or more. 18,773 / 319,738 = 5.9%.

[38] There are 46,927 17-year old enlistees with a high-school equivalency or more education out of 780,195 for 17 to 26-year olds from 1994 to 2007. 46,927 / 780,195 = 6%. If I estimate for all enlistees during this period, I estimate the relative population to be 780,195 / 0.931 = 838,919.4. 46,927 / 838,919.4 = 5.5%.

[39] Discussion with Luke Gallagher, data manager at OEMA, on or about January 15, 2020.

[40] I ultimately run specifications for 'high-school dropouts and equivalents' and 'high-school dropouts to associates-degree holders.' All event studies fail for the peace and wartime 'high-school dropout and equivalent' specifications, indicating pre-treatment trends. The wartime event studies for 'high-school dropouts to associates-degree holders' show insignificant trends, but three of four placebo tests fail. The only specification that shows no event study pre-trends and four

combining GED holders with high-school graduates.

McIntosh et al. (2011) note three additional steps that recruiters face when accepting a LPR enlistee. First, recruiters fill out a waiver verifying proof of Permanent Residence Cards; if missing, recruits must submit a form to USCIS formally requesting status verification. Second, they must observe that recruits understand English sufficiently to meet Military Entrance Processing Station (MEPS) requirements. Third, recruits must provide them with translated copies of any foreign-educational credentials, which recruiters verify with a local community college. These three steps are largely routine and not burdensome according to a recruiter that I interviewed; yet, McIntosh et al. (2011) note that some recruiters find that these steps cause them more work.

These extra steps suggest that recruiters have a disincentive to recruit a noncitizen over a similar US citizen. If they take the additional steps required to enlist a LPR, it is feasible that the average noncitizen enlistee may be more qualified than a similar US citizen given the upfront cost. The third requirement also implies that noncitizen enlistees may hold more educational credentials than they are able to verify, which may make them more qualified than a similar US enlistee – the same applies to all foreign-educated US citizens. My representative-cell technique helps minimize this classification bias, but Armed Services Vocational Aptitude Battery (ASVAB) or AFQT score controls would prove helpful.

I consider whether the remaining omitted service-eligibility criteria in my error term correlate with citizenship or my other explanatory variables, which would be concern for measurement error.[41] Since I use CPS data to generate my service-eligible sample, I lack population-wide service-eligibility data on AFQT scores, English proficiency, medical or fitness histories, and criminal backgrounds; hence, I do not control for them.[42] Already control-

passing placebo tests is 'high-school dropouts to associates-degree holders' from 1994 to August 2001, which I describe in Section 5.1.

[41] I control for citizenship status, age, and educational achievement (minimal high-school equivalency) as per Hattiangadi et al. (2005) and Cunha et al. (2014) (using a modified age group).

[42] I considered using the National Longitudinal Survey of Youth (NLSY) 1997 for population-wide ASVAB scores, but the noncitizen sample is too small.

ling for education, race, Hispanic ethnicity, birth region, and period of arrival, it is unlikely that an AFQT score or English proficiency is correlated to being a LPR. There is little concern that medical histories correlate to citizenship status (or a racial group) due to the young age of the sample. Evidence regarding the link between immigrants and crime points toward a negligible effect on average.[43] If correlation with one of these criterion and the error term is proven, future research can find an instrument to handle the endogeneity. The conditions for this valid instrument are that it has no independent impact on enlistment while inducing change in my explanatory variable of interest, revealing the unbiased enlistment impact of being a noncitizen after the treatment.

1.4.3 Noncitizen versus US-Citizen Criteria

My treatment and control groups are service-eligible noncitizens and US citizens, where an asylum-to-LPR or service-for-citizenship policy should only influence noncitizen-enlistment choices. An indicator variable $Noncitizen_n$ equals zero for naturalized and native-born citizens (my control group), while $Noncitizen_n$ equals one for all LPRs (my treatment group).[44] While residents of the American Samoas, Northern Mariana Islands, Marshall Islands, and other US Pacific territories have several rights or privileges to work stateside, their US-citizenship designation is either ambiguous or inconsistent for service-eligible birth cohorts; thus, I exclude these observations (dropping about 0.02% of the Army administrative sample and 0.58% of the unweighted-CPS sample).[45]

[43] Adelman et al. (2005) find that immigration does not increase crime rates, while it can help lessen metropolitan rates. Bell et al. (2013) find a small positive to no effect on property crime and no link between immigration and violent crime.

[44] The US State Department categorizes the following as native-born citizens: children born to US citizens abroad, Guam residents born after April 11, 1899, Puerto Ricans after April 10, 1899, and US Virgin Island residents born after January 16, 1917. See the USCIS Policy Manual, Volume 12, Part A, Chapter 2.

[45] Residents of the American Samoas are considered US nationals. Anyone born in the Northern Mariana Islands after 1986 is considered a US citizen, with ambiguous status for older residents; thus, I drop these groups. I drop individuals from other ambiguous birth locations, including the US Outlying Areas (not specific), North America or Americas (not specific), Marshall Islands, Micronesia, and Other or Unknown.

A US Census Bureau study on citizenship data highlights the challenge of isolating a noncitizen sub-population. Brown et al. (2018) compare the citizenship data in self-reported survey responses to administrative records, finding significantly lower estimates of the noncitizen share of the population than administrative records depict. Individuals who claim naturalization could be noncitizens, due to possible survey shortcomings, fear of deportation, or misunderstanding one's current-immigrant status. With recruiter confirmation of an enlistee's citizenship designation, my enlistment numerator is accurate; however, population statistics from the CPS may suffer from errant self-identification.

Of greater concern, I accept classification bias due to legal and undocumented immigrants subsumed in the CPS-noncitizen data. Techniques exist to isolate the undocumented immigrant population, but they are less helpful in reverse. Passel and Cohn (2009, 2010) and Amuedo-Dorantes and Antman (2016, 2017) identify undocumented immigrants using Hispanic ethnicity and Mexican origin. Army-administrative data do not denote birth country during this period, which inhibits me from vetting by Mexican origin. While I could drop individuals with Hispanic ethnicity, this would generate biased findings given that 36.9% of immigrant enlistees are Hispanic.

Bachmeier et al. (2014) note that H-1B visa holders likely come from countries with high shares of non-immigrant visas and work in particular occupations for short periods. Borjas (2017) recommends a residual method where he classifies foreign-born individuals as legal if they arrived before 1980, are citizens, receive federal benefits, are veterans, work in the government sector, were born in Cuba, or have legal immigrant or citizen spouses; all others are undocumented. Garcia-Perez (2019) also notes that Cuban-born individuals are likely LPRs. The logic imputation method used by Capps et al. (2018) recommends labeling military, public-sector workers, and public-assistance recipients as LPRs and visa holders by specific circumstances like high-school completion.

I control for undocumented immigrants by dropping individuals with less than a high-school equivalency, which Devadoss et al. (2019) use to differentiate skilled versus low-skilled

Mexicans likely to be migrant-US workers. I further control for H-1B visa holders by dropping 2.5% of individuals with a college or graduate degree, which is a prerequisite for H-1B status. I run a second specification where I drop 5.4% of some college and associates-degree holders to eliminate noncitizens with temporary-student visas, leaving the high-school equivalents most likely to serve. I inevitably retain some classification bias for remaining legal immigrants and high-school-educated, undocumented immigrants in the CPS-noncitizen data.

1.5 Empirical Strategy

Using repeat cross-sectional cellular data, I estimate generalized DinD models with robust standard errors to control for heteroskedasticity. The literature routinely uses DinD to evaluate retroactive policies. My peacetime counter-factual scenario is how enlistment-eligible groups of noncitizens would have enlisted in the absence of the Nicaraguan Adjustment and Central American Relief Act (NACARA), relative to US citizens. My wartime counter-factual scenario is how enlistment-eligible groups of noncitizens would have enlisted in the absence of the events on 9/11, and subsequent Patriot Act and Section 329 enactments, relative to US citizens.

1.5.1 Linear Model

The peace and wartime model follows, where a signifies a two-year age bracket, s signifies sex, n signifies citizenship status, and t signifies a given period for a particular cell:

$$Enlisted_{asnt} = \beta_1 I(n = Noncitizen) \times Post\_NACARA_t + \beta_2 I(n = Noncitizen) +$$
$$\psi_t + \alpha_s + \rho_a + \beta_3 \mathbf{X}_{asnt} + \beta_0 + \epsilon_{asnt},$$

$$Enlisted_{asnt} = \beta_1 I(n = Noncitizen) \times Post\_911_t +$$
$$\beta_2 I(n = Noncitizen) \times Post\_Patriot\_Act_t + \beta_3 I(n = Noncitizen) \times Post\_Section\_329_t +$$
$$\beta_4 I(n = Noncitizen) + \psi_t + \alpha_s + \rho_a + \beta_5 \mathbf{X}_{asnt} + \beta_0 + \epsilon_{asnt},$$

where $Enlisted_{asnt}$ is the ratio of Army enlistment counts to weighted-CPS population. The coefficients ψ_t estimate the effect of each survey quarter, relative to First Quarter 1994 (Q1 1994). $Noncitizen_n$ indicates whether a cell comprises LPRs, which equals one or zero otherwise and α_s identifies whether a cell contains women, equaling one, or zero otherwise. The coefficients ρ_a estimate the effect of cells containing each two-age group, relative to omitted 17 and 18-year olds.

The peacetime interaction-variable $Noncitizen_n \times Post\_NACARA_t$ is a binary treatment effect, equaling one if the cell contains noncitizens from January 1, 1998 to August 31, 2001 and zero otherwise. The wartime interaction-variable $Noncitizen_n \times Post\_911_t$ is a binary indicator and first treatment effect, which equals one if the cell contains noncitizens from October 1, 2001 to June 30, 2002; $Noncitizen_n \times Post\_Patriot\_Act_t$ is a second wartime-treatment effect, which equals one if the cell contains noncitizens from January 1, 2002 to December 31, 2007; and $Noncitizen_n \times Post\_Section\_329_t$ is a third wartime-treatment effect, which equals one if the cell contains noncitizens from July 1, 2002 to December 31, 2007. The Patriot Act interaction serves a dual function, complicating its interpretation relative to anticipated mechanisms. While it directly subsumes changes in enlistment behaviors after the Patriot Act, the variable also captures the lagged impact of 9/11 on noncitizen enlistment. Additional policy controls comprised of $Noncitizen_n \times Treatment_t$ interaction variables serve as cascading, heterogeneous effects to test the robustness of these treated estimates (see Tables 2 and 2a).

The $\mathbf{X}_{asnt}$ coefficient captures explanatory variables that affect enlistment likelihood. These include birth region, race, Hispanic ethnicity, educational level, marital status, number of children in the household, and period of US-arrival, using weighted-CPS counts over the population. A final control captures the average number of family members per cell, using CPS data. Controls for US-arrival period capture the impact of immigration-policy changes

on newer immigrants.[46] The Army-enlistment civilian education categories align with the CPS educational designations, enabling clear educational specifications.[47]

My treatment and control populations must remain consistent and comparable from 1994 to August 2001 and 1999 to 2007. Birth-region and period-of-US-arrival controls account for changes in the fluctuating, immigrant population over time. I compare birth regions across treatment and control groups thanks to the naturalized population and about 1.5% of US citizens born abroad; thus, limiting colinearity concerns since I use native-born birth cohorts to sort US-arrival periods.[48] I rely on quarterly-time effects to control for national changes over time, which encompass all outside civilian-employment options to military service. I use controls for age, education level, and family size to account for income differences across cells.[49]

In the wartime period, I treat 9/11 and the Patriot Act (which also serves as a lagged 9/11 effect), after which I divide the post-9/11 treatment into two sections – the anticipatory period before and after the enactment of Section 329. In both peace and wartime, my US-citizen control group controls for changes in recruitment over time. Tables 6a and 6b depict respective peace and wartime periods, showing arithmetic DinD estimates of enlistment propensity across treatment versus control and pre- versus post-treatment groups. I use NACARA and 9/11 as my respective peace and initial-wartime treatments. When I solely regress $Noncitizen_n$, the respective post-treatment period, and the respective DinD

[46]CPS year-of-immigration periods vary, making a recent-arrival control unfeasible. I used a heat chart to confirm that sequential immigration periods minimize empty quarterly cells over time.

[47]The Army Total Personnel Database categorizes education as 'high-school dropout;' 'GED' (including GED, home school, National Youth Challenge, distance learning, high-school certificate of attendance, and other non-traditional credentials); 'high-school graduate' (including high-school graduates, high-school seniors, currently in high school, adult-education diploma, and Job Corps); 'some college' (including currently enrolled or completed 15 semester hours or greater); 'associates degree' (including associates or professional nursing degrees); 'college' for a college graduate; and 'graduate' for a graduate degree.

[48]One might argue that birthplace controls might cause endogeneity if a person is born near a US military base or recruiting station; although, I assume that birth region is exogenous since I model enlistment propensity relative to the (non-serving) service-eligible CPS population.

[49]The Census uses family size as a metric to construct its poverty-level variable.

interaction, the enlistment propensity estimates exactly match the arithmetic calculations, verifying the accuracy of my DinD model. The final column of Table 6a depicts the control-less peacetime estimate for NACARA at -0.00018 percentage points (pp), significant at the 5% level. Table 6b shows the similar wartime estimate for 9/11 at 0.00005 pp, which is insignificant.

1.5.2 Assumptions

There are two identifying assumptions required for a DinD model. First, the treatment must be exogenous of the enlistment propensities of noncitizens and US citizens. Immigration policies, such as NACARA, were largely unanticipated events. Similarly, the terrorist attacks on 9/11 were unanticipated national-security shocks. I exploit the unanticipated nature of these policies to generate exogenous variation in enlistment propensity.

Second, enlistment-likelihood trends should remain the same for noncitizens and US citizens in the counterfactual scenario where NACARA and the attacks on 9/11 never occurred. In other words, I assume that no pre-treatment trends indicate that common trends would have remained throughout the post-treatment periods absent the enactment of NACARA and the attacks on 9/11 respectively. I perform event studies for evidence of differential pre-trends in enlistment probability for the treatment and control groups. I estimate a general version of my DinD model by adding interaction terms to the regression for being a noncitizen in each quarter, in lieu of $Noncitizen_n * Treatment_t$ interaction variables. Pre-treatment estimates should be statistically insignificant and close to zero, while estimated post-treatment estimates should remain relatively unchanged.[50]

The event study, generating quarterly difference-in-differences point estimates, generalizes the model as follows:

$$Enlisted_{asnt} = \theta_1 I(n = Noncitizen) \times \pi_t + \theta_2 I(n = Noncitizen) +$$
$$\pi_t + \alpha_s + \rho_a + \theta_3 \mathbf{X}_{asnt} + \theta_0 + \epsilon_{asnt}.$$

[50] Angrist and Pischke (2009).

The coefficients π_t estimate the effects of each quarter, relative to omitted Q4 1997 and Q3 2001 for normalization. The coefficients ρ_a estimate the effect of cells containing each two-age group, relative to omitted 17 and 18-year olds. I regress my generalized DinD model with robust standard errors, noting the magnitudes and significances of the pre-treatment effects. Table 7a and 7b reflect the event studies of dual-aged 17 to 26-year old noncitizens (relative to US citizens) from 1994 to August 2001 and 1999 to 2007 respectively. Columns (1) and (2) show event studies for the main model, while Columns (3) and (4) show event studies for robustness tests that add high-school dropouts to the main-model specifications. All main-model specifications show insignificant pre-treatment trends, except for the more-educated peacetime specification in Column (1) of Table 7a which fails the common trends assumption in Q2 1996 and Q3 1996 with pre-trends significant at the 10% level and in Q2 1997 with a DinD interaction of -0.00031 pp, significant at the 5% level; thus, I drop this specification from further analysis.

Figure 2 plots all $Noncitizen_n \times Quarter_t$ enlistment propensity coefficients with a high-school equivalency from 1994 to August 2001. Similarly, Figure 3a plots quarterly DinD interactions from 1999 to 2007 for the more-educated specification, while Figure 3b displays those with a high-school equivalency. All three figures depict 90% confidence intervals.[51] The trends remain steady and insignificant before and after the treatment events. This is not surprising given that quarterly-point estimates likely do not generate sufficient power to observe statistically significant trends. Evaluating longer post-treatment periods will generate greater variation to evaluate policy-treatment effects.[52]

[51]Currently, these figures reflect pre-treatment trends only with policy treatments and controls, not displaying post-treatment quarterly trends. Here, Q1 1994 and Q1 1999 are omitted for colinearity.

[52]In Sections 5.1 and 5.2, I use multiple placebo tests to confirm my pre-trends against longer periods in the pre-treatment period. See Tables 9, 11a, and 11b.

1.6 Results

I provide estimates for the effects of policies on the peacetime and wartime periods. Both sections include placebo test results that confirm the common-trends assumption.

1.6.1 Peacetime Results for 1994 to August 2001

Table 8a shows peacetime enlistment-propensity estimates for LPR versus US citizen high-school-equivalent 17 to 26-year olds. Given all explanatory variables, Column (1) solely treats for the Nicaraguan Adjustment and Central American Relief Act (NACARA). Columns (2) and (3) add controls for the Haitian Refugee Immigration Fairness Act (HRIFA) and the Child Citizenship Act (CCA), verifying the stability of the NACARA estimate. Table 8b assembles the DinD estimates from Column (3) of Table 8a as Column (1) with no controls (except for HRIFA and CCA) through Columns (2) to (6) with incremental demographic controls.[53]

The DinD estimate for noncitizen Army enlistment after NACARA is 0.00024 pp, significant at the 5% level and constant across policy controls (see Row (3), Column (3) of Table 8a). This reflects a 133% increase in LPR enlistment, dividing the estimate by the pre-treatment, noncitizen-enlistment rate from Table 6a (0.00024/0.00018), for a back-of-the-envelope estimated-quarterly increase of 140 LPR enlistees responding to this policy from January 1998 to August 2001.[54] This means that NACARA increased institutional trust for the immigrant population, directly for eligible asylees and/or indirectly for others. Additionally, LPRs had a higher average Army-enlistment propensity than US citizens from 1994 to August 2001 by 0.440%, significant at the 10% level, suggesting that Section 328

[53]Column (6) highlights the importance of controlling for period-of-US arrival, which captures enlistment responses to an immigration policy relevant to asylum seekers of specific civil wars given physical-presence criteria.

[54]The count of LPR enlistees from January 1998 to August 2001 with an associates degree, some college, or a high-school equivalency (4,001 soldiers) times the percentage of high-school equivalents (92.1%) equals 3,686.7. I divided this by 233%, estimating that 1,582.3 LPRs would have enlisted in the absence of NACARA. 3,686.7 - 1,582.3 = 2,104.4 / 15 quarters = 140.3. I calculate all estimates hereon this way.

was an effective incentive for peacetime-noncitizen enlistment (see Row (6), Column (3) of Table 8a).

Table 9 depicts the results of placebo tests on this peacetime model, applying random treatments to the period from January 1994 to December 1997 to see if the pre-treatment trends remain insignificant when segmented into larger periods. I use April 1995 to December 1997 and April 1996 to December 1997 as respective placebo-treatment effects in Columns (1) and (2), testing quarters with lower p-values from the event study in Table 7a. Both placebo tests fail since the interaction of being a noncitizen in Quarter 4 of 1994 becomes significant at the 10% level with a magnitude of 0.00025 pp, indicating that my previous results are not robust (see Row (3), Columns (1) and (2) of Table 9).[55]

Given these failed tests, I run an alternative model, adding two immigration policies in late 1996 and 1997 as precursory-treatment effects prior and in addition to NACARA (Policies 1 and 2 in Table 2). Columns (1) and (2) of Table 7a show no pre-treatment trends during this period and Table 9 depicts the same previously-failed placebo-treatment effects in Columns (1a) and (2a), achieving no significant pre-trends. I also test placebo treatments from October 1994 and October 1995 to December 1997 respectively – all tests pass showing insignificant pre-treatment trends. Column (3_alt) in Table 8a reflects the resulting coefficient estimates for NACARA. While the additional treatment events add noise to the model, the NACARA estimate remains steady at 0.00027 pp, significant at the 10% level and reflecting a 150% increase in LPR enlistment by an estimated 168 enlistees per quarter on average. The higher average Army-enlistment propensity for LPRs becomes insignificant.

[55] I also run the peacetime model with a high-school dropout, high-school equivalent, some college, and associates-degree specification. Since this educational specification increases undocumented and non-LPR noncitizens within the representative-cellular data (see Section 3.3), it suffers from significant classification bias and relegates the results to supporting evidence. The event study shows no pre-trends in Column (3) of Table 7a and four placebo tests pass. These biased results mimic those of high-school equivalents, with NACARA generating a 125% increase in noncitizen-Army enlistment (0.00005 pp/0.00004) and average LPR enlistment being 0.151% higher than that of US citizens, both significant at the 10% level.

1.6.2 Wartime Results for 1999 to 2007

Table 10a shows wartime enlistment-propensity estimates for more-educated and high-school-equivalent LPR versus US citizen 17 to 26-year olds. Given all explanatory variables, Columns (1) treat for the post-9/11 period. Columns (2) add a treatment effect for the Patriot Act, which dually serves as a lagged post-9/11 effect. Columns (1a & 1b) bisect the post-9/11 treatment into pre and post Section-329-enactment periods, and Columns (3 - 6) add four policy controls. Table 10b assembles the DinD estimates from Columns (3 - 6) of Table 7a as Column (1) with no controls (albeit the four policy controls) through Columns (2) to (6) with incremental demographic controls.

The DinD estimate for noncitizen Army enlistment after 9/11, absent subsequent-related policies, is statistically insignificant; however, the addition of a dual-treatment effect for the Patriot Act and lagged-9/11 impact separates these events. For the more-educated specification, the DinD estimate after 9/11 is 0.00017 pp, significant at the 5% level and constant across policy controls, reflecting a 242.9% increase in LPR enlistment for an estimated-quarterly increase of 54 LPR enlistees from October 2001 to December 2007 (see Row (1), Column (2) of Table 10a). The DinD estimate after the Patriot Act is -0.00016 pp, significant at the 5% level and relatively constant across policy controls, reducing LPR enlistment by 266.7% or about 200 fewer soldiers per quarter from January 2002 to December 2007 (see Row (3), Column (2) of Table 10a).

For high-school equivalents, the DinD estimate after 9/11 is 0.00018 pp, significant at the 10% level, reflecting a 180% increase in LPR enlistment or an estimated-quarterly increase of 46 enlistees from October 2001 until December 2007 (see Row (1), second Column (2) of Table 10a). The DinD estimate after the Patriot Act is -0.00017 pp, significant at the 10% level and relatively constant across policy controls, reducing LPR enlistment by 170% or about 121 fewer soldiers per quarter from January 2002 to December 2007 (see Row (3), second Column (2) of Table 10a). From these two treatments, the net impact of 9/11 and the Patriot Act appears a quarterly decrease of 146 more-educated and 75 high-school-equivalent

LPR enlistees from January 2002 to December 2007.

Amid these opposing forces, I estimate the impact of the Section 329 enactment (the wartime service-for-citizenship policy) by bisecting the post-9/11 period into anticipatory and enactment-treatment effects. I recalculate back-of-the-envelope estimates for the nine-month period prior to Section 329. For the more-educated specification, the post-9/11 anticipatory period reflects an estimated quarterly increase of 127 LPR soldiers who enlisted from October 2001 to June 2002 (see Row (2), Column (3-6) of Table 10a). My Patriot Act estimates become -0.00022 pp, significant at the 5% level, reducing LPR enlistment by 366.7% or about 275 fewer soldiers per quarter from January 2002 to December 2007 (see Row (3), Column (3-6) of Table 10a). The DinD estimate after the enactment of Section 329 is 0.00030 pp, significant at the 5% level, reflecting a 500% increase in LPR enlistment, or about 51 more LPR enlistees each quarter from July 2002 to December 2007 (see Row (4), Column (3-6) of Table 10a). The estimated net impact of 9/11, the Patriot Act, and Section 329 on quarterly-LPR enlistment for the more-educated specification is an increase by 127 LPRs in Quarter 4 of 2001, a decrease by 148 in Quarters 1 and 2 of 2002, and a decrease by 224 from July 2002 to December 2007.

For high-school equivalents, the DinD estimate for the post-9/11 anticipatory period is 0.00019 pp, significant at the 10% level, reflecting an estimated quarterly increase of 111 LPR soldiers who enlisted from October 2001 to June 2002 (see Row (2), second Column (3-6) of Table 10a). My Patriot Act estimate becomes -0.00027 pp, significant at the 10% level and relatively constant across policy controls, reducing LPR enlistment by 270% or about 192 fewer soldiers per quarter from January 2002 to December 2007 (see Row (3), second Column (3-6) of Table 10a). The DinD estimate after the Section 329 enactment is 0.00034 pp, significant at the 10% level and constant across policy controls, reflecting a 377.8% increase in LPR enlistment or about 46 additional LPR soldiers each quarter from July 2002 to December 2007 (see Row (4), second Column (3-6) of Table 10a). The estimated net impact of 9/11, the Patriot Act, and Section 329 on quarterly-LPR enlistment

for high-school equivalents is an increase by 111 LPRs in Quarter 4 of 2001, a decrease by 81 in Quarters 1 and 2 of 2002, and a decrease by 146 from July 2002 to December 2007.

Table 11a depicts placebo-test results for this wartime period, applying four random treatments to the period from January 1999 to August 2001 for the more-education specification. Table 11b shows these same tests for the high-school equivalent specification. All four placebo tests pass for both educational specifications, confirming insignificant pre-treatment trends and robust results. Of note, the policy control for the October 2004 Section 329 enhancement, which dropped federal naturalization application fees and allowed overseas-citizenship application, had a positive value of 0.00012 on the more-educated specification and 0.00017 on the high-school equivalents, both significant at the 10% level. If interpreted as an effect, this would indicate a 240% and 243% respective increase in LPR enlistment; however, I use this policy strictly as a control. My results are delicate given the small nature of the LPR-enlistee population and my use of three treatment effects; at most, I can consider this as additional evidence of a positive response to Section 329.

1.6.3 Average Marginal Effects of Explanatory Variables

I estimate relevant average marginal effects (AME) of my explanatory variables on 17 to 26-year-old enlistment propensities of my peace and wartime models. Figure 4a shows the AME of time on enlistment propensity, highlighting all significant-quarterly coefficients in the left column. The right column shows rolling-yearly averages of these estimates, minimizing enlistment seasonality given recruiting quotas and deadlines that roughly align with fiscal years. The peacetime graphs in the top row show a positive trend in enlistment propensity, while the wartime graphs show a negative enlistment trend over the period.

The left column of Figure 4b shows AME of citizenship, sex, age group, and educational level on enlistment propensity. Noncitizen enlistment propensity is insignificant in wartime, but greater than that of high-school equivalent US citizens in peacetime by 0.468%, statistically significant at the 0.10 level. Bachmann et al. (2000) note that I should expect to

see higher enlistment rates for men.[56] Women have negative enlistment likelihoods relative to men (-0.09% in peace and -0.05% and -0.08% in wartime respectively), as well as older age groups relative to 17 and 18-year olds, all significant at the 0.001 level. The left-middle graph shows that cells with some college are 0.11% less likely to enlist than high-school equivalents in wartime, significant at the 0.001 level, reinforcing Bachmann et al. (2000). The right column depicts period-of-US-arrival AME on enlistment where the later arrival groups are comprised entirely of immigrants.[57] Immigrants arriving from 1996 to August 2001 show a negative AME on peacetime enlistment of -0.07%, significant at the 0.10 level; whereas, the bottom-right two graphs depict positive and significant period-of-US-arrival AME on wartime-enlistment propensities for individuals arriving after 1979.

1.7 Discussion and Conclusion

This study highlights how incentives drive immigrant participation in a new country. For 17 to 26-year-olds, I find that peacetime enlistment for high-school equivalents increased by 133% after an asylum-to-green-card policy. Wartime enlistment for 17 to 26-year-old more-educated noncitizens increased by 243% just after 9/11, decreased by 367% after the Patriot Act, and increased by 500% after the enactment of a wartime service-for-expedited-citizenship policy. Apart from a survey querying why age-eligible individuals serve, it is impossible to gauge the exact mix of motivations that drive enlistment decisions for LPRs versus US citizens. Citizenship incentives, patriotism, fear of combat death, or fear of more rigorous immigration policy changes and enforcement may have motivated LPRs who enlisted before or after 9/11.

Tables 12a and 12b respectively summarize how anticipated peacetime and wartime mechanisms unique to LPR enlistment align with my estimated results. The estimated

[56]Bachman et al. (2000) find lower enlistment rates for those with college-educated parents, high grades, and college plans and higher rates for men, African Americans, Hispanics, and those favorably viewing military service.

[57]Later periods comprised of all foreign-born people are more relevant since the control group reflects birth-cohort-compiled arrival periods.

increase in LPR-Army enlistment following the Nicaraguan Adjustment and Central American Relief Act infers that the peacetime service-for-citizenship policy and institutional trust (or patriotism) likely drove these choices. The estimated increases in LPR-Army enlistment after 9/11 and the enactment of Section 329 suggest that the wartime service-for-citizenship policy likely drove these increases; although, institutional trust or patriotism may play a part. This suggests that LPRs respond to naturalization incentives to an extent that they may risk their lives.[58]

Simultaneously, lower estimated LPR-Army-enlistment rates following the Patriot Act suggest that a lack of institutional trust or a fear of deportment may have driven these behaviors. This estimate captures the lagged impact of 9/11, which may suggest that immigrants have a lower risk threshold to combat; although, this is unlikely given the increased enlistment response of LPRs immediately following 9/11 (prior to the Patriot Act). These results reflect a lower-conservative bound of the impact of these policies, as the US Army comprises about 36% of the active-duty military.[59]

These results have strategic implications for the military and immigration policy. The LPR population increased its Army enlistment following a peacetime asylum-to-LPR policy. Given a resource-constrained wartime military, the immigrant population filled the gaps while Army enlistment declined from 1999 to 2007, adding regional expertise to the ranks.[60] Expedited citizenship via service is a relatively costless incentive. The US military values the skills and pragmatic contributions of immigrant soldiers. The linguistic abilities and cultural knowledge of immigrants are valuable to military forces in the diverse regions where they serve. Whether expedited-citizenship-for-service policies had a direct or indirect effect on LPR-enlistment choices, these results raise questions regarding the representative nature and sustainability of the all-volunteer force, especially if we overlook incentives that motivate participation in the American republic.

[58]Over 95% of LPRs committed to initial enlistment contract lengths of three or more years.

[59]DMDC Active Duty Military Personnel Master File, Chart 1.05, *2007 Demographics Report*.

[60]Baldor (2018) notes that the Army missed its September 2018 recruiting goals by 6,500 recruits for the first time since 2005.

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1.9 Appendix of Figures and Tables

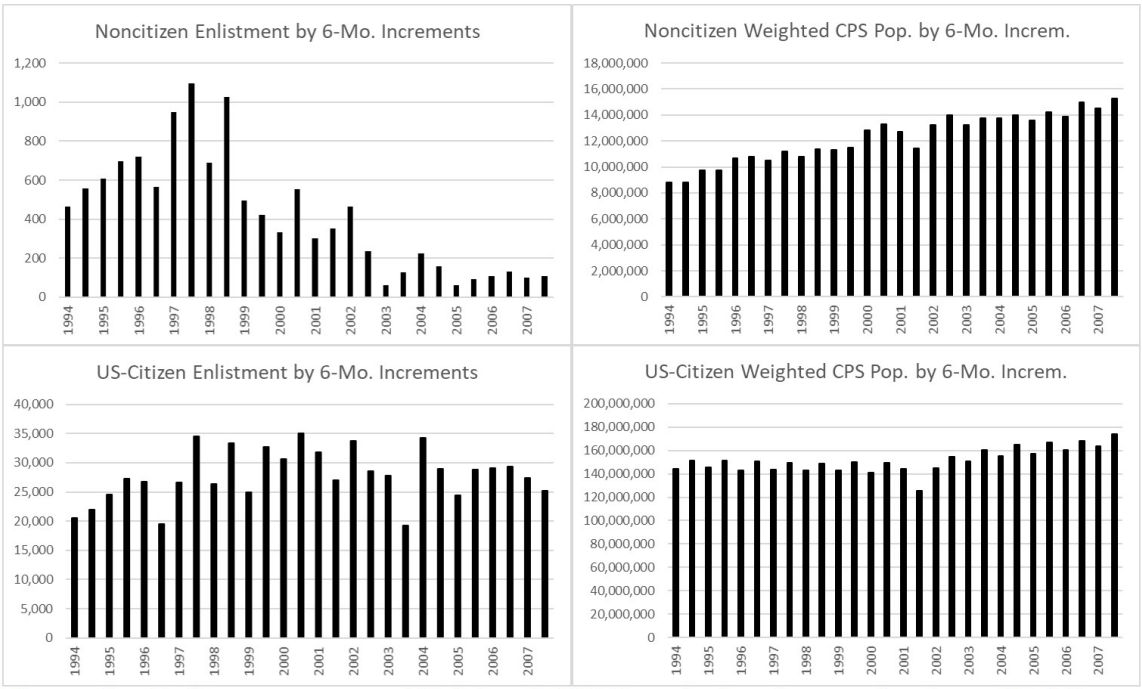

Figure 1a: Enlistment versus Weighted CPS Population Counts for Ages 17-26

Note: The left column reflects the total number of 17 to 26-year old enlistees by six-month intervals from 1994 to 2007 from Army administrative data. The right column reflects the weighted count of individuals from the similar weighted population in the CPS, excluding those with less than a high-school equivalent. US Army administrative data and IPUMS-CPS.

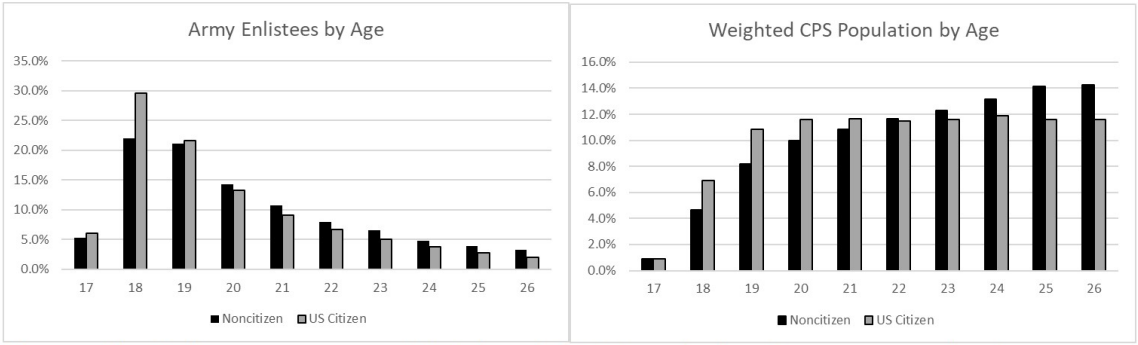

Figure 1b: Enlistment versus Weighted CPS Population Percentages by Age

Note: The left graph reflects the count of 17 to 26-year old, high-school equivalent or more educated enlistees by age over the total number of noncitizen or US citizen enlistees from 1994 to 2007 (11,695 and 768,500 respectively). The right graph shows the same relative ratio from the weighted CPS population. US Army administrative data and IPUMS-CPS.

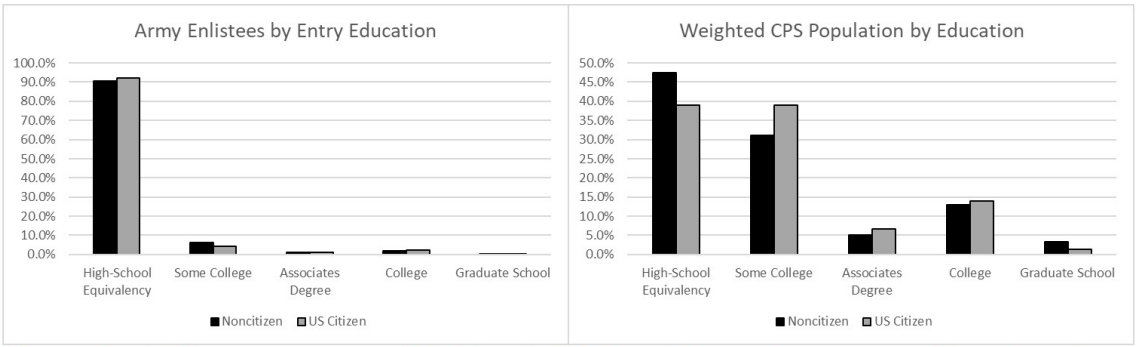

Figure 1c: Enlistment versus Weighted CPS Population Percentages by Education

Note: The left graph reflects the count of 17 to 26-year old enlistees by entry education (excluding high-school dropouts) over the total number of noncitizen or US citizen enlistees from 1994 to 2007 (11,695 and 768,500 respectively). The right graph shows the same relative ratio from the weighted CPS population. US Army administrative data and IPUMS-CPS.

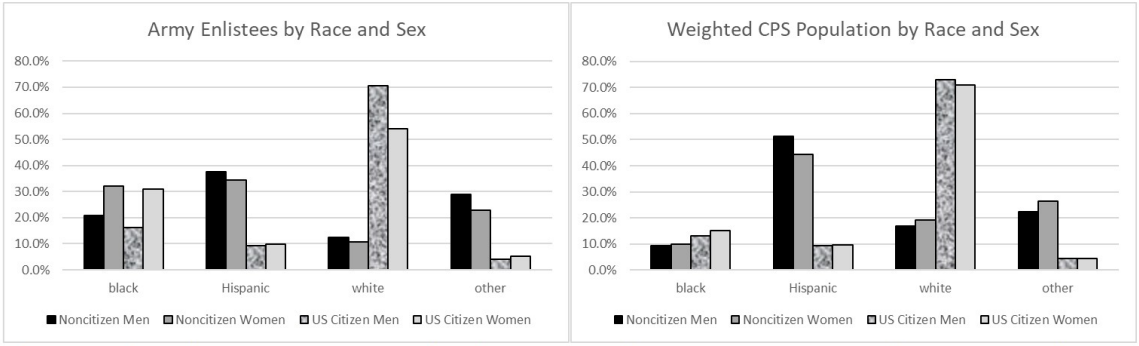

Figure 1d: Enlistment vs. Weighted CPS Population Percentages by Race and Sex

Note: The left graph reflects the count of 17 to 26-year old, high-school equivalent or more educated enlistees by race and sex over the total number of noncitizen or US citizen enlistees from 1994 to 2007 (11,695 and 768,500 respectively). The right graph shows the same relative ratio from the weighted CPS population. US Army administrative data and IPUMS-CPS.

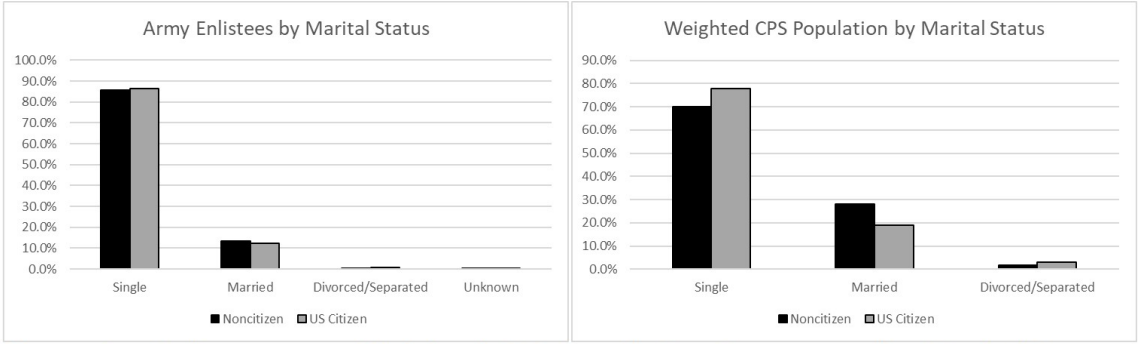

Figure 1e: Enlistment vs. Weighted CPS Population Percentage by Marital Status

Note: The left graph reflects the count of 17 to 26-year old, high-school equivalent or more educated enlistees by marital status over the total number of noncitizen or US citizen enlistees from 1994 to 2007 (11,695 and 768,500 respectively). The right graph shows the same relative ratio from the weighted CPS population. US Army administrative data and IPUMS-CPS.

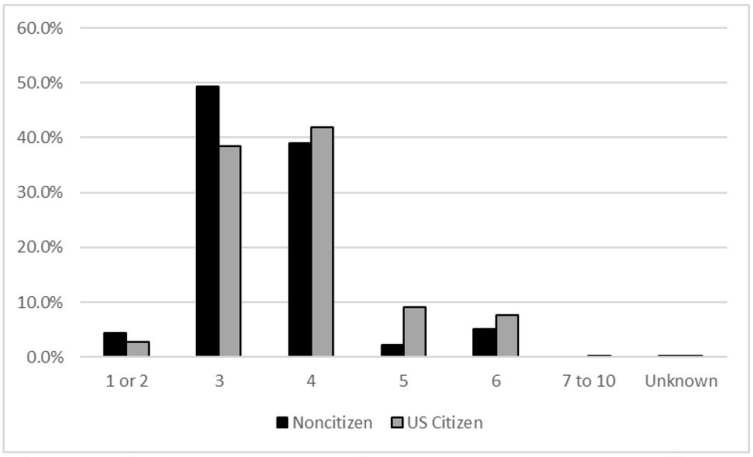

Figure 1f: Percentages of Entry Contract Length

Note: These contract-length percentages show the count of each contract over the total number of 17 to 26-year old noncitizen or US-citizen enlistees from 1994 to 2007 (11,695 and 768,500 respectively). These contract-length counts exclude those with less than a high-school equivalency. With few exceptions, the only ways that contracts can end prematurely is due to a serious injury or illness precluding one from completing his or her commitment, or if a soldier leaves under less-than-honorable circumstances. US Army administrative data.

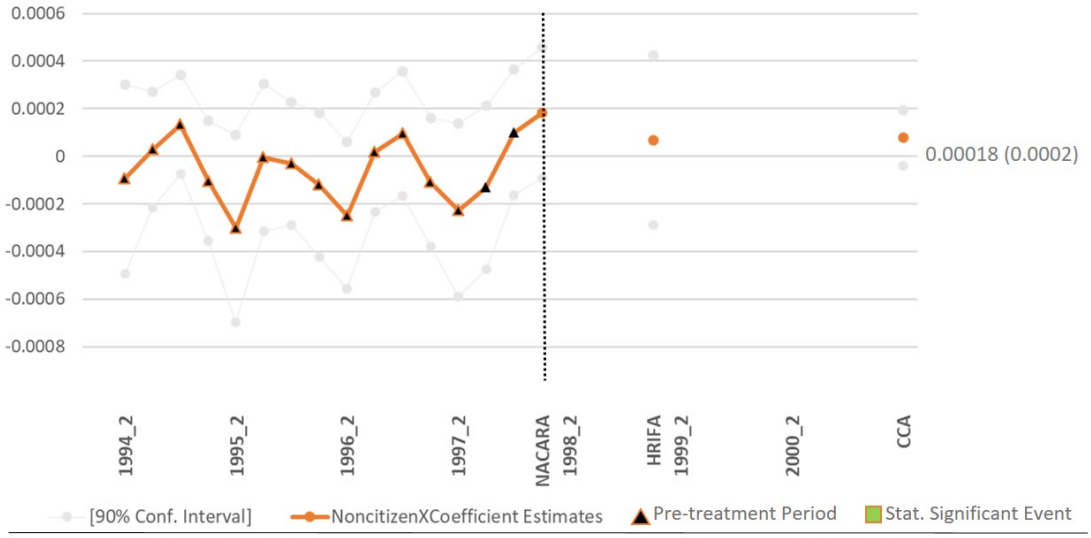

Figure 2: Event Study of Noncitizen-Peacetime Enlistment Propensity from Q1 1994 to Q3 2001 for High-School Equivalents Aged 17 to 26

Note: The right axis shows the baseline noncitizen pre-treatment enlistment propensity (std. dev. in parentheses). The treatment effect, noted by a dotted line, is the Nicaraguan Adjustment and Central American Relief Act (NACARA). The latter two coefficients reflect policy controls for the Haitian Refugee Immigration Fairness Act (HRIFA) and Child Citizenship Act (CCA). I omit Q1 1994 due to collinearity. Robust p-values + $p < 0.10$, * $p < 0.05$.

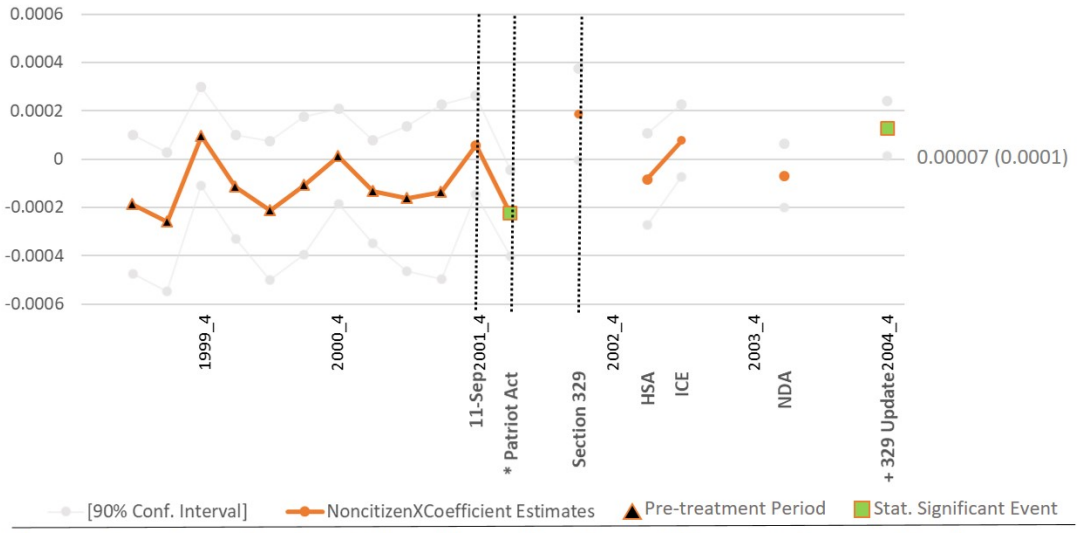

Figure 3a: Event Study of Noncitizen-Wartime Enlistment Propensity from Q1 1999 to Q4 2007 for HS-Equiv. to Assoc. Degree Holders Aged 17 to 26

Note: The right axis shows the baseline noncitizen pre-treatment enlistment propensity (std. dev. in parentheses). Treatment effects, noted by dotted lines, include (1a) the anticipation of Section 329 after 9/11, (2) the Patriot Act, and (1b) Section 329 enactment. The latter four coefficients reflect policy controls. I omit Q1 1999 due to collinearity. Robust p-values + $p < 0.10$, * $p < 0.05$.

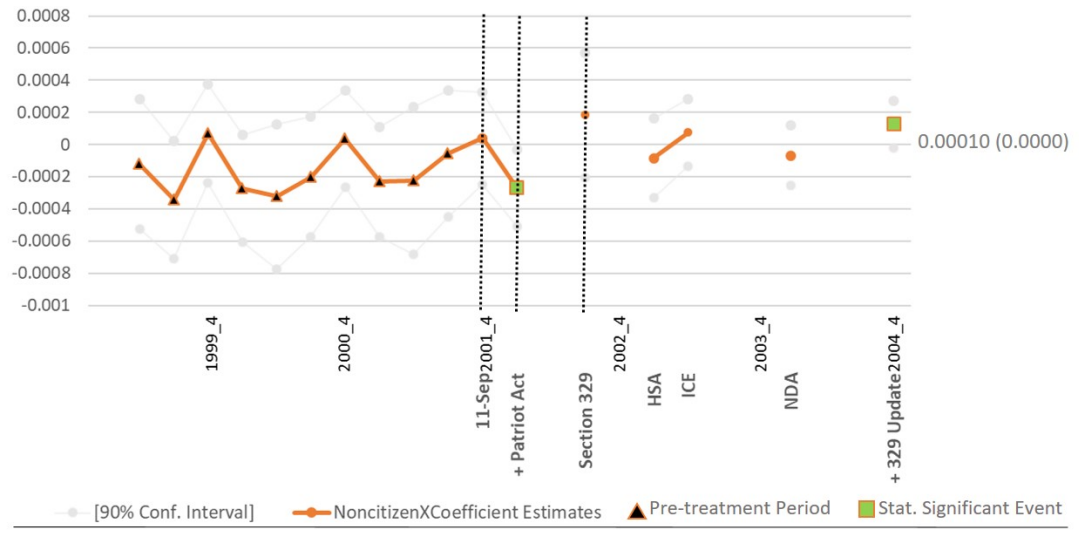

Figure 3b: Event Study of Noncitizen-Wartime Enlistment Propensity from Q1 1999 to Q4 2007 for High-School Equivalents Aged 17 to 26

Note: The right axis shows the baseline noncitizen pre-treatment enlistment propensity (std. dev. in parentheses). Treatment effects, noted by dotted lines, include (1a) the anticipation of Section 329 after 9/11, (2) the Patriot Act, and (1b) Section 329 enactment. The latter four coefficients reflect policy controls. I omit Q1 1999 due to collinearity. Robust p-values + $p < 0.10$, * $p < 0.05$.

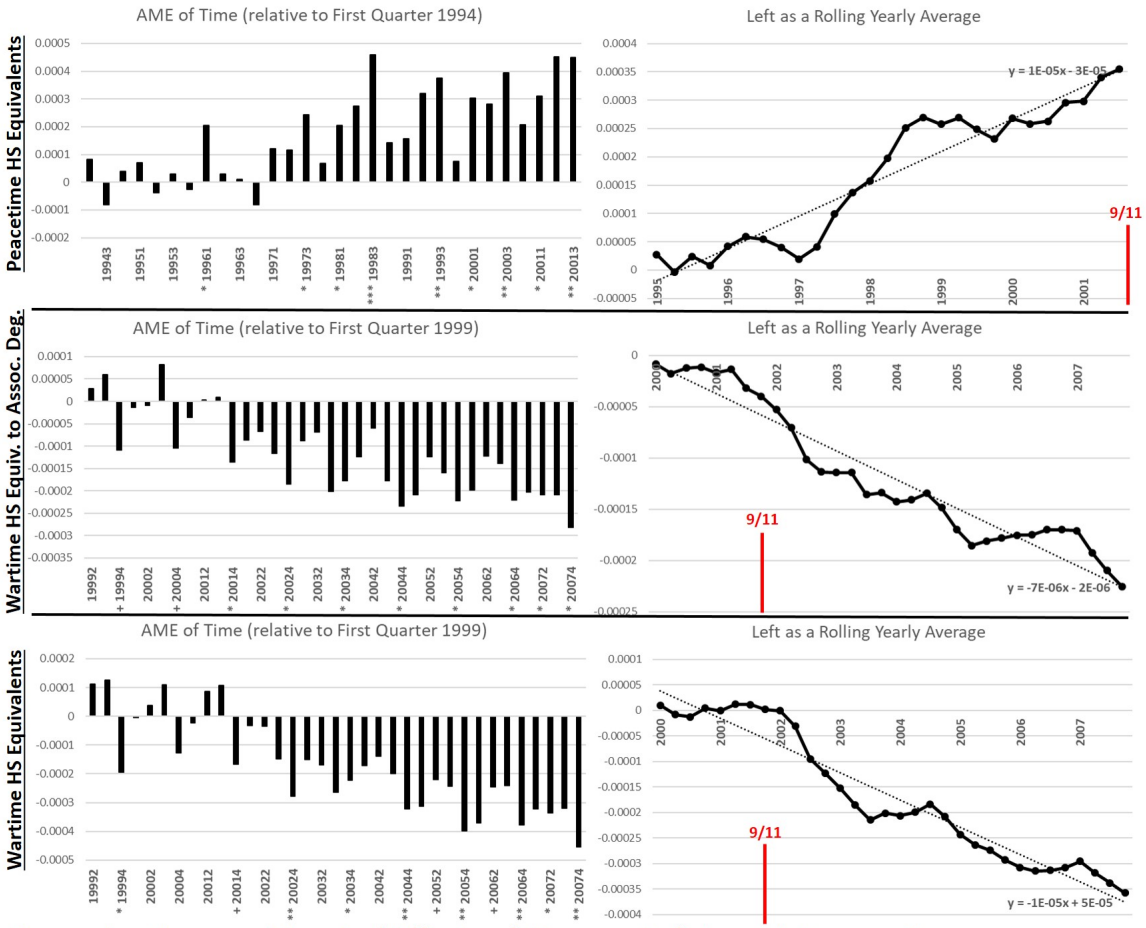

Figure 4a: Average Marginal Effects of Time on Enlistment Propensity

Note: The top row of graphs reflect the peacetime period from 1994 to August 2001 and the remaining graphs reflect the wartime period from 1999 to 2007. The left column reflects average marginal effects (AME) of each quarter on enlistment propensity for ages 17 to 26, showing robust delta-method p-values + p<0.10, * p<0.05, ** p<0.01, *** p< 0.001. The right column reflects rolling yearly averages (with dotted trend lines). The top right-hand graph starts with quarterly AME from Q2 1994 to Q1 1995 and ends with Q4 2000 to Q3 2001. The middle and lower right-hand graphs start with quarterly AME from Q2 1999 to Q1 2000 and ends with Q1 2007 to Q4 2007.

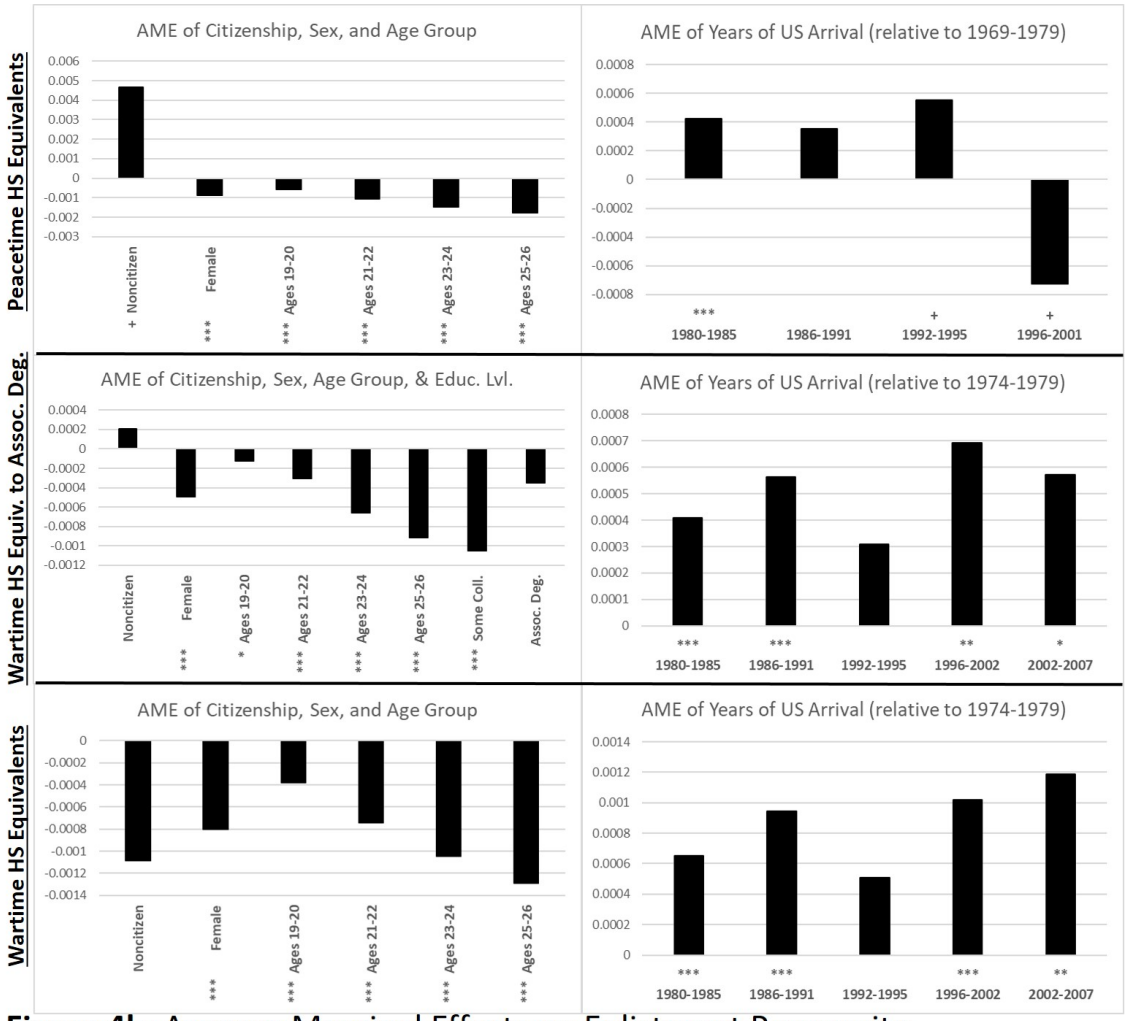

Figure 4b: Average Marginal Effects on Enlistment Propensity

Note: The top row of graphs reflect the peacetime period from 1994 to August 2001 and the remaining graphs reflect the wartime period from 1999 to 2007. The left graphs reflect AME of birth region, relative to US birth, on enlistment propensity for ages 17-26. The left column of graphs reflect AME of being a noncitizen (rel. to a US citizen), female, and a two-year age group (rel. to 17-18 years olds) on enlistment propensity for ages 17 to 26. The middle graph compares educational level to high-school equivalents. Robust delta-method p-values + p<0.10, * p<0.05, ** p<0.01, *** p< 0.001.

Table 1: Time Line of Key Events in `Peacetime' or `Periods of Hostility'

Military & Selective Service Events	Time Line	Immigration and Naturalization Act Citizenship-for-Service Policy	Enlistment Periods (by Quarter)
US Military Draft Ends	27-Jan-73	Section 329: `Period of Hostility'	
Selective Service Registration ends	1-Apr-75		
Selective Service Registration resumes	2-Jul-80	Section 328: `Peacetime'	
Persian Gulf War	2-Aug-90 to 11-Apr-91	Section 329: `Period of Hostility'	
Peacetime Operations	12-Apr-91 to 10-Sep-01	Section 328: `Peacetime'	Jan 1994 to Aug 2001
`War on Terror'	11-Sep-01 to 23-May-13	Section 329: `Period of Hostility'	Oct 2001 to Dec 2007

Note: Column 3 denotes whether Immigration & Naturalization Act Section 328 or 329 applied during each enlistment period, aligning by quarters in Column 4 (with Jul-Aug 2001 as a 2-month period). The Vietnam `period of hostility' lasted from February 28, 1961 to October 15, 1978. The `War on Terror' technically ended on 23-May-13, yet operations continue in the Middle East and Section 329 remains in effect.
Sources: 1) Section D: Designated Periods of Hostilities. *USCIS Policy Manual, Volume 12, Part I, Chapter 3 - Military Service during Hostilities (INA 329),* accessible at https://www.uscis.gov/policymanual/HTML/PolicyManual-Volume12-PartI-Chapter3.html;
2) Selective Service System. History & Records - The Vietnam Lotteries, accessible at https://www.sss.gov/About/History-And-Records/lotter1.

Table 2a: Federal Peacetime Immigration Policies from 1994 to August 2001

Policy	Dates in Effect	1994	1995	1996				1997				1998				1999				2000				2001		
				1	2	3	4	1	2	3	4	1	2	3	4	1	2	3	4	1	2	3	4	1	2	3
1	22-Aug-96 to present							Personal Resp. & Work Opp. Reconcilation Act																		
	30-Sep-96 to present							Illegal Immigration Reform & Immig. Resp. Act signed																		
2	1-Apr-97 to present											IIRIRA enacted														
3	19-Nov-97 to present															**Nicaraguan Adj. & Cent. Am. Relief Act**										
4	21-Oct-98 to present																			Haitian Refugee IRA (HRIFA)						
5	27-Feb-01 to present																							CCA		

Note: Policy (3) reflects the primary peacetime-treatment effect. As a robustness test in Section 5.1, two immigration policies in late 1996 (1) will serve as a precursory-treatment effect, as well as the enactment of IIRIRA (2). Green indicates an anticipated positive effect, yellow indicates an uncertain effect, and red indicates an anticipated negative effect of each policy on enlistment propensity. `HRIFA' reflects the Haitian Refugee Immigration Fairness Act and `CCA' reflects enactment of the Child Citizenship Act.

Table 2b: Federal Wartime Immigration Policies from 1999 to 2007

Policy	Dates in Effect	1999	2000	2001				2002				2003				2004				2005				2006				2007			
		1	1	1	2	3	4	1	2	3	4	1	2	3	4	1	2	3	4	1	2	3	4	1	2	3	4	1	2	3	4
1	11-Sep-01 to present							**Post-9/11**																							
1a	11-Sep-01 to 30-Jun-02							**329 Ant.**																							
2	26-Oct-01 to present											**USA Patriot Act**																			
1b	03-Jul-02 to present															**Section 329 - enacted and backdated to 9/11**															
3	25-Nov-02 to present																			Homeland Security Act (HSA)											
4	01-Mar-03 to present																							US Immigration & Customs Enforcement - stood up							
5	24-Nov-03 to present																											National Defense Authorization (NDA) Act			
6	01-Oct-04 to present																														Section 329 Updates - enacted

Note: Bold policies reflect primary wartime-treatment effects (either 1 & 2 or 1a, 1b, & 2). Green indicates an anticipated positive effect, yellow indicates an uncertain effect, and red indicates an anticipated negative effect of each policy on enlistment propensity. `329 Ant.' reflects the anticipatory period following 9/11 and prior to the enactment of Section 329.

Table 3a: Peacetime Anticipated Mechanisms (1994 to August 2001)

	Peacetime Immigration Policy	Citizenship Incentives	Flight to Safety (Deportment Fear)	Institutional Trust or Patriotism
	Section 328 (Ave. Marginal Effect)	+		
1	Personnel Responsibility & Work Opportunity Reconciliation Act		-	+
	Illegal Immigration Reform & Immigrant Responsibility Act signed		+ or -	-
2	IIRIRA enacted		+ or -	-
3	**Nicaraguan Adjustment & Central American Relief Act (NACARA)**		-	+
4	Haitian Refugee Immigration Fairness Act (HRIFA)		-	+
5	Child Citizenship Act (CCA)		+ (kids) or - (self)	+

Note: Policies (1 and 2) reflect robust-policy controls excluded from my main model. The treatment effect is (3) NACARA, with two additional policy controls (4 and 5). Green indicates an anticipated positive effect and red indicates an anticipated negative effect of a policy on enlistment propensity. An additional potential mechanism is a disincentive for recruiters to accept noncitizen enlistees relative to US citizens given an extra required waiver for LPR enlistees and verification of foreign-educational certification with a local community college; however, these procedures are largely routine and relatively costless for most recruiting stations.

Table 3b: Wartime Anticipated Mechanisms (1999 to 2007)

	Wartime Immigration Policy	Increased Risk of Combat Death	Citizenship Incentives	Flight to Safety (Deportment Fear)	Institutional Trust or Patriotism
	Section 328 (Ave. Marginal Effect)		+		
1a	**9/11 (Section 329 anticipated)**	-	+		
2	**Patriot Act (coincides w/ lagged post-9/11 period)**	-		+ or -	-
1b	**Section 329 enacted**	-	+		
3	Homeland Security Act			+ or -	-
4	Immigration & Customs Enforcement (ICE) stood up			+ or -	-
5	National Defense Authorization (NDA) Act	-	+	+ or -	+ or -
6	Section 329 Update enacted	-	+		

Note: The treatment effects are (1) 9/11 with anticipation of Section 329, (2) the USA Patriot Act, and (3) the enactment of Section 329. Policies 3 to 6 reflect policy controls. Green indicates an anticipated positive effect and red indicates an anticipated negative effect of a policy on enlistment propensity. An additional potential mechanism is a disincentive for recruiters to accept noncitizen enlistees relative to US citizens given an extra required waiver for LPR enlistees and verification of foreign-educational certification with a local community college; however, these procedures are largely routine and relatively costless for most recruiting stations.

Table 4a: Sample Restrictions in Army-Administrative Data

Initial Sample of Enlistees	Drop	Sample
Original Sample (aged 17 to 42)		1,916,351
Drop if missing entry citizenship, sex, entry age, or entry-educ. level, or born in America Samoa, Fed States of Micronesia, Northern Mariana Islands, Palau, and Marshall Islands	100,716	1,815,635
Drop if noncitizen variables incongruent: LPR at entry cannot be a native-born citizen now	5,333	1,810,302
Drop if citizenship variables incongruent: a native-born citizen at entry cannot be a LPR or naturalized citizen now; naturalized at entry cannot be a native-born citizen or LPR now	3,512	1,806,790
Drop if enlistment date is before January 1, 1994	291,759	1,515,031
Drop if enlistment date is after December 31, 2007	669,138	845,893
Drop if enlistment dates occurred in September 2001	4,925	840,968
Drop if entry age is 27 to 42	51,019	789,949
Drop if entry civilian education is graduate, college, or HS dropout, or currently HS dropout	29,397	**760,552**
Drop if entry civilian education is some college or associates degree	42,285	**718,267**

Note: The lower-right-hand numbers of individual, 17 to 26-year old observations show a more-educated and a high-school equivalent specification respectively. I bundle by two-age group, sex, quarter, and citizenship status from 1994 to 2007. Country of birth is largely missing in the data, so I drop individuals who list (i.e.) American Samoa as their birthplace, but I inevitably retain others in the sample.

Table 4b: Sample Restrictions in Unweighted CPS-Population Data

Initial Sample Restrictions	Drop	Sample
Original sample (aged 17 to 42)		8,019,064
Drop if missing Hispanic ethnicity	55,891	7,963,173
Drop if born in American Samoa, Northern Mariana Islands, US outlying areas (ns), North America (ns), Americas (ns), the Marshall Islands, Micronesia, or Other (nec) and Unknown	50,905	7,912,268
Drops foreign-born individuals (not PR, GU, or VI) with no listed immigration year, infeasible immigration years given cohorts after 1957, or labeled as US born	354	7,911,914
Drops US native-born observations mislabeled as immigrating in a particular year, or mislabeled as born in US outlying, but born elsewhere	7	7,911,907
Drop if survey month is September 2001	50,019	7,861,888
Drop if age is 27 to 42	5,090,076	2,771,812
Drop if education is graduate, college, or less than HS equivalency	1,060,443	1,711,369
Drop if education is some college or associates degree	905,603	805,766

Note: The lower-right-hand numbers of individual, 17 to 26-year old observations reflect a more-educated and a high-school equivalent specification. I bundle the weighted data by two-age group, sex, quarter, and citizenship status from 1994 to 2007.

Table 5: Descriptive Statistics for the CPS-Population Sample

Variables	**Mean**	**Std. Dev.**	**Min**	**Max**
Quarter	20008.3	41.1226	19941	20074
Noncitizen	0.0643	0.2452	0	1
Female	0.5119	0.4999	0	1
Ages 17 to 18	0.0943	0.2923	0	1
Ages 19 to 20	0.2591	0.4381	0	1
Ages 21 to 22	0.2465	0.4310	0	1
Ages 23 to 24	0.2047	0.4035	0	1
Ages 25 to 26	0.1953	0.3965	0	1
High-School Equivalency	0.4708	0.4991	0	1
Some College	0.4505	0.4975	0	1
Associates Degree	0.0786	0.2691	0	1
White	0.8144	0.3888	0	1
Black	0.1159	0.3201	0	1
American Indian or Alaska Native	0.0145	0.1194	0	1
Asian or Pacific Islander	0.0423	0.2013	0	1
Other or More than One Race	0.0130	0.1131	0	1
Non-Hispanic	0.8842	0.3200	0	1
Mexican	0.0724	0.2591	0	1
Puerto Rican	0.0118	0.1080	0	1
Cuban	0.0033	0.0572	0	1
Dominican	0.0092	0.0952	0	1
Central or South American	0.0192	0.1373	0	1
Married, Spouse Present	0.1899	0.3923	0	1
Married, Spouse Absent	0.0071	0.0838	0	1
Separated	0.0125	0.1111	0	1
Divorced	0.0194	0.1381	0	1
Widowed	0.0009	0.0295	0	1
Never Married	0.7702	0.4207	0	1
Family Size	3.2045	1.6445	1	16
No Children	0.7926	0.4055	0	1
One Child	0.1241	0.3297	0	1
Two Children	0.0621	0.2414	0	1
Three Children	0.0170	0.1294	0	1
Four or More Children	0.0041	0.0643	0	1
Born in United States	0.9044	0.2941	0	1
Born in PR, GU, or VI	0.0030	0.0548	0	1
Born in Canada or Bermuda	0.0020	0.0443	0	1
Born in Mexico	0.0254	0.157	0	1
Born in Central America	0.0071	0.0842	0	1
Born in the Caribbean	0.0080	0.0892	0	1
Born in South America	0.0070	0.0836	0	1
Born in Europe	0.0132	0.1143	0	1
Born in the Philippines	0.0051	0.0711	0	1
Born in Asia or Oceania	0.0195	0.1384	0	1
Born in Africa or the Middle East	0.0052	0.0722	0	1

Table 5 (cont.): Descriptive Stats for the CPS-Population Sample

Variables	**Mean**	**Std. Dev.**	**Min**	**Max**
US Arrival from Years 1969 to 1979	0.5007	0.5000	0	1
US Arrival from Years 1980 to 1985	0.3504	0.4771	0	1
US Arrival from Years 1986 to 1991	0.1032	0.3042	0	1
US Arrival from Years 1992 to 1995	0.0170	0.1292	0	1
US Arrival from Years 1996 to 2002	0.0204	0.1412	0	1
US Arrival from Years 2002 to 2007	0.0084	0.0912	0	1

The sample drops observations with less than a high-school equivalency, college graduates, and graduate or professional education. 'PR, GU, or VI' indicates Puerto Rico, Guam, or the US Virgin Islands. Each variable contains 1,711,369 observations.

Table 6a: Covariate Balance across Peacetime Treatment & Control Groups

	US-Citizen Change			**Noncitizen Change**			**Final Diff.**
Enlistment Propensity	Pre-Trmt (1)	Post-Trmt (2)	Diff. (2)-(1)	Pre-Trmt (3)	Post-Trmt (4)	Diff. (4)-(3)	(4-3)-(2-1)
	High-School Equivalents Only						
NACARA Treatment	0.00044 (0.0006)	0.00056 (0.0008)	0.00012 (0.0015)	0.00018 (0.0002)	0.00012 (0.0002)	**-0.00006* (0.0005)**	**-0.00018* (0.0022)**
Cellular Observations	160	150	310	160	150	310	620

Note: Specification (for ages 17-26) includes a constant and indicators for noncitizen and quarter effects from Q1 1994 to Q3 2001. The treatment is the Nicaraguan Adjustment and Central American Relief Act (NACARA) as of Q1 1998. In the final column, the R-squared value for the high-school equivalency DinD coefficient is 0.097. Standard deviations are in parentheses. Robust p-values * $p < 0.05$. Villa (2016).

Table 6b: Covariate Balance across Wartime Treatment & Control Groups

	US-Citizen Change			**Noncitizen Change**			**Final Diff.**
Enlistment Propensity	Pre-Trmt (1)	Post-Trmt (2)	Diff. (2)-(1)	Pre-Trmt (3)	Post-Trmt (4)	Diff. (4)-(3)	(4-3)-(2-1)
	High-School Equivalents to Associates-Degree Holders						
9/11 Treatment	0.00036 (0.0006)	0.00026 (0.0004)	-0.00009 (0.0012)	0.00007 (0.0001)	0.00002 (0.0000)	**-0.00005*** (0.0002)**	0.00005 (0.0018)
Cellular Observations	110	250	360	110	250	360	720
Patriot Act Treatment				0.00006 (0.0001)			
Section 329 Treatment				0.00006 (0.0001)			
Section 329 Update				0.00005 (0.0001)			
	High-School Equivalents Only						
9/11 Treatment	0.00058 (0.0008)	0.00044 (0.0006)	-0.00014 (0.0017)	0.00010 (0.0000)	0.00003 (0.0002)	**-0.00007*** (0.0003)**	0.00006 (0.0024)
Cellular Observations	110	250	360	110	250	360	720
Patriot Act Treatment				0.00010 (0.0002)			
Section 329 Treatment				0.00009 (0.0002)			
Section 329 Update				0.00007 (0.0001)			

Note: All specifications (for ages 17-26) include a constant and indicators for noncitizen and quarter effects from Q1 1999 to Q4 2007. The treatment is the anticipatory period following 9/11, the Patriot Act in late October 2001, and the Section 329 enactment in July 2002. In the final column, the R-squared value for the high-school equivalent to associates-degree holder DinD coefficient is 0.130 and that of the high-school (HS) equivalent coefficient is 0.181. Standard deviations are in parentheses. Robust p-values $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Villa (2016).

Table 7a: Event Study for Peacetime Enlistment Propensity (1994-Aug 01)

Variables	**HS Equiv. to Assoc. Deg.** (1)	**HS Equivalent Only** (2)	**HS Dropout to Assoc. Deg.** (3)	**HS Dropout to HS Equiv.** (4)
$Noncitizen \times Q2\_1994$	-0.00003	-0.00010	-0.00001	-0.00000
	(0.830)	(0.691)	(0.832)	(0.947)
$Noncitizen \times Q3\_1994$	0.00002	0.00003	-0.00003	-0.00003
	(0.877)	(0.846)	(0.420)	(0.656)
$Noncitizen \times Q4\_1994$	0.00006	0.00013	-0.00002	0.00002
	(0.556)	(0.293)	(0.685)	(0.738)
$Noncitizen \times Q1\_1995$	0.00001	-0.00010	-0.00001	-0.00002
	(0.915)	(0.499)	(0.866)	(0.813)
$Noncitizen \times Q2\_1995$	-0.00015	-0.00030	-0.00001	0.00005
	(0.362)	(0.205)	(0.796)	(0.442)
$Noncitizen \times Q3\_1995$	-0.00006	-0.00000	-0.00002	0.00000
	(0.666)	(0.980)	(0.661)	(0.946)
$Noncitizen \times Q4\_1995$	0.00006	-0.00003	-0.00001	-0.00003
	(0.610)	(0.845)	(0.837)	(0.675)
$Noncitizen \times Q1\_1996$	0.00002	-0.00012	-0.00003	**-0.00015+**
	(0.849)	(0.515)	(0.472)	**(0.073)**
$Noncitizen \times Q2\_1996$	**-0.00023+**	-0.00025	-0.00003	-0.00005
	(0.092)	(0.186)	(0.507)	(0.421)
$Noncitizen \times Q3\_1996$	**-0.00021+**	0.00002	0.00000	0.00002
	(0.067)	(0.912)	(0.993)	(0.812)
$Noncitizen \times Q4\_1996$	0.00003	0.00010	0.00003	0.00003
	(0.843)	(0.545)	(0.478)	(0.639)
$Noncitizen \times Q1\_1997$	-0.00007	-0.00011	-0.00002	-0.00008
	(0.534)	(0.504)	(0.701)	(0.294)
$Noncitizen \times Q2\_1997$	**-0.00031***	-0.00023	-0.00004	-0.00006
	(0.046)	(0.305)	(0.380)	(0.372)
$Noncitizen \times Q3\_1997$	-0.00028	-0.00013	-0.00007	-0.00008
	(0.123)	(0.532)	(0.261)	(0.379)
$Noncitizen \times Q4\_1997$	-0.00009	0.00010	0.00003	0.00006
	(0.514)	(0.533)	(0.581)	(0.413)
$Noncitizen \times NACARA$	-0.00008	0.00018	0.00004	0.00006
01Jan98_31Aug01	(0.515)	(0.279)	(0.438)	(0.367)
$Noncitizen \times HRIFA$	0.00006	0.00007	0.00002	0.00004
01Jan99_31Aug01	(0.477)	(0.573)	(0.491)	(0.292)
$Noncitizen \times CCA$	-0.00001	0.00008	0.00003	0.00006
01Apr01_31Aug01	(0.965)	(0.607)	(0.394)	(0.168)
Cellular Observations	620	620	620	620
R-squared	0.693	0.689	0.758	0.740

Note: All studies include a constant and indicators for noncitizen, quarter effects, sex, two-year age-group effects, birth-region effects, race, ethnicity, marital status, number of children, family size, and period of US-arrival effects. Column 1 shows coefficient estimates for a sample with a HS equivalency, some college, or an associates degree. Column 2 shows estimates from a sample with a HS equivalency only. Column 3 shows estimates from a sample with HS dropouts to associates degree holders. Column 4 shows estimates from a sample with HS dropouts to HS equivalents. Robust p-values in parentheses: $^{+}$ $p < 0.10$, * $p < 0.05$.

Table 7b: Event Study for Wartime Enlistment Propensity (1999-2007)

Variables	**HS Equiv. to Assoc. Deg.** (1)	**HS Equivalent Only** (2)	**HS Dropout to Assoc. Deg.** (3)	**HS Dropout to HS Equiv.** (4)
$Noncitizen \times Q2\_1999$	-0.00019	-0.00012	-0.00002	-0.00001
	(0.280)	(0.620)	(0.525)	(0.882)
$Noncitizen \times Q3\_1999$	-0.00026	-0.00034	-0.00009	-0.00014
	(0.136)	(0.124)	(0.179)	(0.118)
$Noncitizen \times Q4\_1999$	0.00009	0.00007	0.00006	0.00009
	(0.449)	(0.721)	(0.119)	(0.136)
$Noncitizen \times Q1\_2000$	-0.00012	-0.00028	-0.00004	-0.00005
	(0.379)	(0.175)	(0.288)	(0.411)
$Noncitizen \times Q2\_2000$	-0.00021	-0.00032	-0.00001	-0.00001
	(0.223)	(0.238)	(0.828)	(0.807)
$Noncitizen \times Q3\_2000$	-0.00011	-0.00020	-0.00004	-0.00005
	(0.531)	(0.380)	(0.475)	(0.477)
$Noncitizen \times Q4\_2000$	0.00001	0.00004	0.00002	0.00007
	(0.931)	(0.840)	(0.610)	(0.230)
$Noncitizen \times Q1\_2001$	-0.00014	-0.00023	-0.00002	0.00001
	(0.300)	(0.266)	(0.574)	(0.845)
$Noncitizen \times Q2\_2001$	-0.00016	-0.00023	0.00002	0.00006
	(0.370)	(0.420)	(0.642)	(0.260)
$Noncitizen \times Q3\_2001$	-0.00014	-0.00006	-0.00002	0.00002
	(0.536)	(0.811)	(0.801)	(0.842)
$Noncitizen \times 329\_anticipated$	0.00006	0.00004	0.00004	**0.00012***
01Oct01_30Jun02	(0.641)	(0.815)	(0.319)	**(0.044)**
$Noncitizen \times Patriot\_Act$	**-0.00022***	**-0.00027+**	-0.00003	-0.00007
01Jan02_31Dec07	**(0.039)**	**(0.060)**	(0.250)	(0.111)
$Noncitizen \times 329\_enacted$	0.00018	0.00019	0.00005	**0.00015***
01Jul02_31Dec07	(0.288)	(0.417)	(0.331)	**(0.038)**
$Noncitizen \times HSA$	-0.00008	-0.00013	0.00002	0.00009
01Jan03_31Dec07	(0.472)	(0.406)	(0.571)	(0.130)
$Noncitizen \times ICE$	0.00008	0.00017	**0.00006***	**0.00014****
01Apr03_31Dec07	(0.406)	(0.179)	**(0.037)**	**(0.003)**
$Noncitizen \times NDA$	-0.00007	-0.00008	**-0.00007****	**-0.00012****
01Jan04_31Dec07	(0.394)	(0.468)	**(0.006)**	**(0.010)**
$Noncitizen \times 329\_updated$	**0.00013+**	**0.00018+**	**0.00006****	**0.00013*****
01Oct04_31Dec07	**(0.068)**	**(0.051)**	**(0.002)**	**(0.000)**
Cellular Observations	720	720	720	720
R-squared	0.652	0.681	0.771	0.771

Note: All studies include a constant and indicators for noncitizen, quarter effects, sex, two-year age-group effects, birth-region effects, race, ethnicity, marital status, number of children, family size, and period of US-arrival effects. Column 1 shows coefficient estimates for a sample with a HS equivalency, some college, or an associates degree. Column 2 shows estimates from a sample with a HS equivalency only. Column 3 shows estimates from a sample with HS dropouts to associates degree holders. Column 4 shows estimates from a sample with HS dropouts to HS equivalents. Robust p-values in parentheses: $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 8a: Results for Peacetime Enlistment Propensity from 1994 to Aug 2001

	HS Equivalents Only **Main Model**			**HS Equivalents Only** **Robust Test with NACARA as 3rd of 3 Treated Effects**				
Variables	(1) NACARA	(2) + HRIFA	(3) + CCA	(1_alt) PRWORA	(2_alt) + IIRIRA	(3_alt) + NACARA	(4_alt) + HRIFA	(5_alt) + CCA
$Noncitizen \times PRWORA$				0.00008	0.00006	0.00006	0.00006	0.00006
01Oct96_31Aug01				(0.328)	(0.529)	(0.515)	(0.503)	(0.487)
$Noncitizen \times IIRIRA$					0.00003	-0.00008	-0.00008	-0.00008
01Apr97_31Aug01					(0.754)	(0.505)	(0.498)	(0.492)
$Noncitizen \times NACARA$	**0.00024***	**0.00023+**	**0.00024***			**0.00027***	**0.00025+**	**0.00027+**
01Jan98_31Aug01	**(0.033)**	**(0.059)**	**(0.042)**			**(0.049)**	**(0.073)**	**(0.056)**
$Noncitizen \times HRIFA$		0.00007	0.00006				0.00007	0.00006
01Jan99_31Aug01		(0.578)	(0.604)				(0.573)	(0.599)
$Noncitizen \times CCA$			0.00008					0.00008
01Apr01_31Aug01			(0.605)					(0.599)
$Noncitizen$	**0.00445+**	**0.00456+**	**0.00440+**	**0.00484+**	**0.00480+**	0.00446	**0.00457+**	0.00440
	(0.096)	**(0.089)**	**(0.095)**	**(0.075)**	**(0.082)**	(0.104)	**(0.097)**	(0.103)
Cellular Observations	620	620	620	620	620	620	620	620
R-squared	0.683	0.683	0.683	0.681	0.681	0.683	0.683	0.684

Note: All specifications include a constant and indicators for noncitizen, quarter effects, sex, two-year age groups, birth-region effects, race, ethnicity, marital status, number of children, family size, and period of US-arrival effects. Robust p-values $^{+}$ $p < 0.10$, * $p < 0.05$.

Table 8b: Peacetime Results with Gradual Controls

Variables	(1)	(2)	(3)	(4)	(5)	(6)
Sex & Age-Group Effects	No	Yes	Yes	Yes	Yes	Yes
Birth Region Effects	No	No	Yes	Yes	Yes	Yes
Race and Hispanic	No	No	No	Yes	Yes	Yes
Family Controls	No	No	No	No	Yes	Yes
US-Arrival Period Effects	No	No	No	No	No	Yes
	High-School Equivalents Only					
$Noncitizen \times NACARA$	-0.00006	-0.00006	-0.00014	-0.00008	-0.00005	**0.00024***
01Jan98_31Aug01	(0.667)	(0.550)	(0.225)	(0.488)	(0.625)	**(0.042)**
$Noncitizen \times HRIFA$	-0.00014	-0.00014	-0.00009	-0.00011	-0.00011	0.00006
01Jan99_31Aug01	(0.365)	(0.220)	(0.432)	(0.341)	(0.325)	(0.604)
$Noncitizen \times CCA$	-0.00008	-0.00008	-0.00012	-0.00014	-0.00009	0.00008
01Apr01_31Aug01	(0.744)	**(0.656)**	(0.498)	(0.440)	(0.592)	(0.605)
$Noncitizen$	**-0.00026*** **	**-0.00026*** **	**0.0118*** **	**0.0113*** **	**0.00875** **	**0.00440+**
	(0.000)	**(0.000)**	**(0.000)**	**(0.000)**	**(0.002)**	**(0.095)**
Cellular Observations	620	620	620	620	620	620
R-squared	0.124	0.547	0.583	0.592	0.663	0.683

Note: All specifications include a constant and indicators for noncitizen and quarter effects from 1994 to August 2001. 'Family controls' include those for marital status, number of children, and family size. Robust p-values $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 9: Placebo Tests for Peacetime Enlistment Propensity

Variables	(1)	(1)	(2)	(2)	(1a)	(1a)	(2a)	(2a)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
	High-School Equivalents Only							
$Noncitizen \times 01Apr95\_31Dec97$	0.00001	-0.00000						
Placebo Test 1	(0.923)	(0.979)						
$Noncitizen \times 01Apr96\_31Dec97$			0.00002	0.00000				
Placebo Test 2			(0.818)	(0.996)				
$Noncitizen \times 01Apr95\_30Sep96$					-0.00006	-0.00007		
Robust Placebo Test 1					(0.751)	(0.613)		
$Noncitizen \times 01Apr96\_30Sep96$							-0.00002	-0.00012
Robust Placebo Test 2							(0.933)	(0.500)
$Noncitizen \times Q2\_1994$	-0.00010	-0.00003	-0.00011	-0.00002	-0.00016	-0.00015	-0.00016	-0.00013
	(0.765)	(0.920)	(0.750)	(0.935)	(0.673)	(0.543)	(0.676)	(0.587)
$Noncitizen \times Q3\_1994$	0.00011	0.00011	0.00010	0.00011	0.00005	-0.00003	0.00005	-0.00002
	(0.560)	(0.465)	(0.597)	(0.447)	(0.820)	(0.852)	(0.822)	(0.884)
$Noncitizen \times Q4\_1994$	0.00010	**0.00025+**	0.00009	**0.00025+**	0.00004	0.00009	0.00004	0.00010
	(0.619)	**(0.052)**	(0.656)	**(0.052)**	(0.869)	(0.503)	(0.870)	(0.440)
$Noncitizen \times Q1\_1995$	-0.00001	0.00003	-0.00002	0.00002	-0.00006	0.00000	-0.00006	0.00001
	(0.972)	(0.851)	(0.940)	(0.885)	(0.798)	(0.997)	(0.800)	(0.969)
$Noncitizen \times Q2\_1995$			-0.00011	-0.00011			-0.00016	-0.00008
			(0.721)	(0.607)			(0.644)	(0.732)
$Noncitizen \times Q3\_1995$			0.00000	0.00011			-0.00005	0.00008
			(0.992)	(0.565)			(0.880)	(0.719)
$Noncitizen \times Q4\_1995$			0.00005	0.00005			-0.00000	-0.00000
			(0.789)	(0.742)			(0.999)	(0.989)
$Noncitizen \times Q1\_1996$			-0.00008	-0.00007			-0.00013	-0.00019
			(0.743)	(0.709)			(0.652)	(0.325)
Cellular Observations	220	220	220	220	220	220	220	220
R-squared	0.075	0.687	0.076	0.688	0.079	0.738	0.081	0.740

Note: All specifications include a constant and indicators for noncitizen and quarter effects from 1994 to August 2001. 'Controls' include sex, two-year age groups, birth-region effects, race, ethnicity, marital status, number of children, family size, and period of US-arrival effects. Robust p-values $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 10a: Results for Wartime Enlistment Propensity from 1999 to 2007

	HS Equiv. to Assoc. Degrees Main Model				**HS Equivalents Only** Main Model			
Variables	(1) 9/11	(2) + Patriot A.	(1a & 1b) + Sect. 329	(3 - 6) + Controls	(1) 9/11	(2) + Patriot A.	(1a & 1b) + Sect. 329	(3 - 6) + Controls
Noncit. × *Post_9/11*	0.00004	**0.00017***			0.00004	**0.00018+**		
01Oct01_31Dec07	(0.612)	**(0.043)**			(0.674)	**(0.091)**		
Noncit. × 329_*antic.*			**0.00017***	**0.00017***			**0.00018+**	**0.00019+**
01Oct01_30Jun02			**(0.042)**	**(0.038)**			**(0.089)**	**(0.080)**
Noncit. × *Patriot_Act*		**-0.00016***	**-0.00023***	**-0.00022***		**-0.00017+**	**-0.00028+**	**-0.00027+**
01Jan02_31Dec07		**(0.034)**	**(0.036)**	**(0.038)**		**(0.079)**	**(0.053)**	**(0.063)**
Noncit. × 329_*enact*			**0.00027***	**0.00030***			**0.00034+**	**0.00034+**
01Jul02_31Dec07			**(0.048)**	**(0.039)**			**(0.051)**	**(0.072)**
Noncit. × *HSA*				-0.00008				-0.00013
01Jan03_31Dec07				(0.465)				(0.372)
Noncit. × *ICE*				0.00007				0.00018
01Apr03_31Dec07				(0.410)				(0.165)
Noncit. × *NDA*				-0.00006				-0.00008
01Jan04_31Dec07				(0.444)				(0.485)
Noncit. × 329_*update*				**0.00012+**				**0.00017+**
01Oct04_31Dec07				**(0.072)**				**(0.052)**
Noncitizen	0.00095	0.00106	0.00098	0.00058	-0.00070	-0.00066	-0.00089	-0.00138
	(0.714)	(0.680)	(0.706)	(0.824)	(0.790)	(0.800)	(0.736)	(0.607)
Cellular Observations	720	720	720	720	720	720	720	720
R-squared	0.643	0.644	0.645	0.646	0.672	0.673	0.674	0.676

Note: All specifications include a constant and indicators for noncitizen, quarter effects, sex, two-year age groups, birth-region effects, race, ethnicity, educational level (HS equiv. to assoc. deg.), marital status, number of children, family size, and period of US-arrival effects. Robust p-values $^{+}$ $p < 0.10$, * $p < 0.05$.

Table 10b: Wartime Results with Gradual Controls

Variables	(1)	(2)	(3)	(4)	(5)	(6)
Sex & Age-Group Effects	No	Yes	Yes	Yes	Yes	Yes
Birth Region Effects	No	No	Yes	Yes	Yes	Yes
Race, Hispanic, & Educ. Effects	No	No	No	Yes	Yes	Yes
Family Controls	No	No	No	No	Yes	Yes
US-Arrival Period Effects	No	No	No	No	No	Yes
	HS Equivalent, Some College, or Associates Degrees					
Noncitizen × *Sect329_antic.*	0.00012	0.00012	**0.00014+**	**0.00013+**	**0.00016***	**0.00017***
01Oct01_30Jun02	(0.185)	(0.137)	**(0.063)**	**(0.093)**	**(0.026)**	**(0.038)**
Noncitizen × *Patriot_Act*	-0.00015	-0.00015	-0.00019	**-0.00021+**	**-0.00025***	**-0.00022***
01Jan02_31Dec07	(0.307)	(0.213)	(0.120)	**(0.069)**	**(0.017)**	**(0.038)**
Noncitizen × *Sect329_enact*	0.00021	0.00021	0.00023	**0.00025+**	**0.00028***	**0.00030***
01Jul02_31Dec07	(0.264)	(0.168)	(0.136)	**(0.087)**	**(0.042)**	**(0.039)**
Noncitizen × *HSA*	-0.00005	-0.00005	-0.00006	-0.00011	-0.00008	-0.00008
01Jan03_31Dec07	(0.764)	(0.705)	(0.604)	(0.325)	(0.455)	(0.465)
Noncitizen × *ICE*	0.00008	0.00008	0.00005	0.00010	0.00007	0.00007
01Apr03_31Dec07	(0.555)	(0.465)	(0.618)	(0.325)	(0.482)	(0.410)
Noncitizen × *NDA*	-0.00011	-0.00011	-0.00008	-0.00010	-0.00012	-0.00006
01Jan04_31Dec07	(0.322)	(0.217)	(0.371)	(0.257)	(0.142)	(0.444)
Noncitizen × *Sect329_update*	0.00007	0.00007	0.00005	0.00006	0.00005	**0.00012+**
01Oct04_31Dec07	(0.448)	(0.334)	(0.509)	(0.402)	(0.407)	**(0.072)**
Noncitizen	**-0.00029*** **	**-0.00029*** **	**0.0105*** **	**0.00905** **	**0.00545***	0.00058
	(0.000)	**(0.000)**	**(0.000)**	**(0.001)**	**(0.034)**	(0.824)
Cellular Observations	720	720	720	720	720	720
R-squared	0.156	0.454	0.487	0.545	0.616	0.646
	High-School Equivalents Only					
Noncitizen × *Sect329_antic.*	0.00018	0.00018	0.00011	0.00009	0.00014	**0.00019+**
01Oct01_30Jun02	(0.194)	(0.125)	(0.331)	(0.412)	(0.177)	**(0.080)**
Noncitizen × *Patriot_Act*	-0.00025	-0.00025	-0.00025	-0.00025	**-0.00031***	**-0.00027+**
01Jan02_31Dec07	(0.246)	(0.144)	(0.145)	(0.123)	**(0.046)**	**(0.063)**
Noncitizen × *Sect329_enact*	0.00037	**0.00037+**	0.00033	0.00026	0.00029	**0.00034+**
01Jul02_31Dec07	(0.156)	**(0.072)**	(0.114)	(0.205)	(0.145)	**(0.072)**
Noncitizen × *HSA*	-0.00016	-0.00016	-0.00019	-0.00017	-0.00012	-0.00013
01Jan03_31Dec07	(0.481)	(0.357)	(0.256)	(0.309)	(0.456)	(0.372)
Noncitizen × *ICE*	0.00018	0.00018	0.00020	0.00022	0.00023	0.00018
01Apr03_31Dec07	(0.379)	(0.259)	(0.194)	(0.149)	(0.107)	(0.165)
Noncitizen × *NDA*	-0.00019	-0.00019	-0.00017	-0.00016	**-0.00022***	-0.00008
01Jan04_31Dec07	(0.222)	(0.124)	(0.154)	(0.183)	**(0.049)**	(0.485)
Noncitizen × *Sect329_update*	0.00013	0.00013	0.00009	0.00007	0.00006	**0.00017+**
01Oct04_31Dec07	(0.361)	(0.239)	(0.364)	(0.496)	(0.452)	**(0.052)**
Noncitizen	**-0.00048*** **	**-0.00048*** **	**0.00577***	**0.00492+**	0.00294	-0.00138
	(0.000)	**(0.000)**	**(0.046)**	**(0.088)**	(0.282)	(0.607)
Cellular Observations	720	720	720	720	720	720
R-squared	0.208	0.505	0.525	0.543	0.628	0.676

Note: All specifications include a constant and indicators for noncitizen and quarter effects from 1999 to 2007. 'Family controls' include those for marital status, number of children, and family size.
Robust p-values $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 11a: Placebo Tests for Wartime Enlistment Propensity

Variables	(1)	(1a)	(2)	(2a)	(3)	(3a)	(4)	(4a)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
	HS Equivalent, Some College, or Associates Degrees							
$Noncitizen \times 01Jul99\_31Aug01$	-0.00015	-0.00005						
Placebo Test 1	(0.207)	(0.727)						
$Noncitizen \times 01Jan00\_31Aug01$			-0.00017	-0.00008				
Placebo Test 2			(0.191)	(0.556)				
$Noncitizen \times 01Jul00\_31Aug01$					-0.00016	-0.00006		
Placebo Test 3					(0.266)	(0.716)		
$Noncitizen \times 01Jan01\_31Aug01$							-0.00019	-0.00004
Placebo Test 4							(0.273)	(0.798)
$Noncitizen \times Q2\_1999$	-0.00017	-0.00022	-0.00017	-0.00023	-0.00017	-0.00023	-0.00017	-0.00021
	(0.497)	(0.221)	(0.499)	(0.219)	(0.501)	(0.224)	(0.503)	(0.243)
$Noncitizen \times Q3\_1999$			-0.00023	-0.00018	-0.00023	-0.00017	-0.00023	-0.00015
			(0.412)	(0.331)	(0.415)	(0.350)	(0.417)	(0.404)
$Noncitizen \times Q4\_1999$			0.00003	0.00015	0.00003	0.00016	0.00003	0.00018
			(0.785)	(0.378)	(0.786)	(0.362)	(0.787)	(0.305)
$Noncitizen \times Q1\_2000$					-0.00016	-0.00006	-0.00016	-0.00003
					(0.380)	(0.715)	(0.383)	(0.831)
$Noncitizen \times Q2\_2000$					-0.00023	-0.00014	-0.00023	-0.00013
					(0.383)	(0.401)	(0.386)	(0.455)
$Noncitizen \times Q3\_2000$							-0.00019	-0.00008
							(0.496)	(0.666)
$Noncitizen \times Q4\_2000$							-0.00003	0.0009
							(0.815)	(0.665)
Cellular Observations	220	220	220	220	220	220	220	220
R-squared	0.124	0.733	0.128	0.739	0.129	0.739	0.131	0.741

Note: All specifications include a constant and indicators for noncitizen and quarter effects from 1999 to 2007. 'Controls' include sex, two-year age groups, birth-region effects, race, ethnicity, marital status, number of children, family size, and period of US-arrival effects. Robust p-values $^{+}$ $p < 0.10$, * $p < 0.05$.

Table 11b: Placebo Tests for Wartime Enlistment Propensity

Variables	(1)	(1a)	(2)	(2a)	(3)	(3a)	(4)	(4a)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
	High-School Equivalents Only							
$Noncitizen \times 01Jul99\_31Aug01$	-0.00021	-0.00003						
Placebo Test 1	(0.261)	(0.902)						
$Noncitizen \times 01Jan00\_31Aug01$			-0.00025	-0.00001				
Placebo Test 2			(0.223)	(0.972)				
$Noncitizen \times 01Jul00\_31Aug01$					-0.00022	0.00008		
Placebo Test 3					(0.310)	(0.734)		
$Noncitizen \times 01Jan01\_31Aug01$							-0.00027	0.00007
Placebo Test 4							(0.269)	(0.747)
$Noncitizen \times Q2\_1999$	-0.00024	0.00001	-0.00024	0.00002	-0.00024	0.00002	-0.00024	0.00002
	(0.518)	(0.979)	(0.520)	(0.951)	(0.522)	(0.933)	(0.524)	(0.942)
$Noncitizen \times Q3\_1999$			-0.00028	-0.00019	-0.00028	-0.00017	-0.00028	-0.00016
			(0.421)	(0.468)	(0.423)	(0.530)	(0.426)	(0.543)
$Noncitizen \times Q4\_1999$			0.00007	0.00020	0.00007	0.00022	0.00007	0.00024
			(0.721)	(0.470)	(0.723)	(0.411)	(0.724)	(0.390)
$Noncitizen \times Q1\_2000$					-0.00030	-0.00010	-0.00030	-0.00008
					(0.321)	(0.691)	(0.324)	(0.731)
$Noncitizen \times Q2\_2000$					-0.00034	-0.00013	-0.00034	-0.00013
					(0.391)	(0.642)	(0.393)	(0.657)
$Noncitizen \times Q3\_2000$							-0.00021	0.00001
							(0.559)	(0.956)
$Noncitizen \times Q4\_2000$							-0.00004	0.00022
							(0.835)	(0.426)
Cellular Observations	220	220	220	220	220	220	220	220
R-squared	0.162	0.729	0.167	0.733	0.168	0.735	0.170	0.736

Note: All specifications include a constant and indicators for noncitizen and quarter effects from 1999 to 2007. 'Controls' include sex, two-year age groups, birth-region effects, race, ethnicity, marital status, number of children, family size, and period of US-arrival effects. Robust p-values $^{+}$ $p < 0.10$, * $p < 0.05$.

Table 12a: Summarized Peacetime Results from 1994 to August 2001

	Peacetime Immigration Policy	Estimated DinD Effects: HS-Equiv. from 1994 to Aug 01 (Main Model)	Estimated DinD Effects: HS-Equiv. from 1994 to Aug 01 (Robustness Test)	Anticipated Mechanisms: Citizenship Incentives	Anticipated Mechanisms: Flight to Safety (Deportment Fear)	Anticipated Mechanisms: Institutional Trust or Patriotism
Section 328 (Ave. Marginal Effect)		**0.440% (+)**	+	+		
1	PRWORA		+		-	+
	IIRIRA signed				**+ or -**	-
2	IIRIRA enacted		-		**+ or -**	-
3	**NACARA (asylees to LPR)**	**133% (*)**	**150% (+)**		-	+
4	HRIFA (asylees to LPR)	+	+		-	+
5	Child Citizenship Act	+	+		**+ (kids) or - (self)**	+

Note: Green indicates an estimated positive effect and red indicates an estimated negative effect of each policy on enlistment propensity. The NACARA estimates equate to a respective-quarterly increase of 140 and 168 LPR enlistees. Statistically significant DinD estimates with robust p-values in parentheses: + p<0.10, * p<0.05.

Table 12b: Summarized Wartime Results from 1999 to 2007

	Wartime Immigration Policy	Estimated DinD Effects: More Educated from 1999 to 2007	Estimated DinD Effects: HS-Equiv. from 1999 to 2007	Anticipated Mechanisms: Increased Risk of Combat Death	Anticipated Mechanisms: Citizenship Incentives	Anticipated Mechanisms: Flight to Safety (Deportment Fear)	Anticipated Mechanisms: Institutional Trust or Patriotism
Section 328 (Ave. Marginal Effect)		+	+		+		
1a	**9/11 (Antic. of Section 329)**	**243% (*)**	**190% (+)**	-	+		
2	**USA Patriot Act**	**-367% (*)**	**-270% (+)**	-		**- (or +)**	-
1b	**Section 329**	**500% (*)**	**378% (+)**	-	+		
3	Homeland Security Act	-	-			**+ or -**	-
4	ICE stood up	+	+			**+ or -**	-
5	National Defense Auth. Act	-	-	-	+	**+ or -**	**+ or -**
6	Section 329 Update	**(+)**	**(+)**	-	+		

Note: Green indicates an estimated positive effect and red indicates an estimated negative effect of each policy on enlistment propensity. The 9/11 estimates (1a) equate to a respective-quarterly increase of 127 and 111 LPR enlistees; the Patriot Act estimates (2) equate to a quarterly decrease of 275 and 192; and the Section 329 estimates equate to quarterly increases of 51 and 46. Statistically significant DinD estimates with robust p-values in parentheses: + p<0.10, * p<0.05.

Chapter 2

Immigrants, Thank You for Your Service: Immigration-Policy Impacts on Naturalized-Enlistment Propensity

2.1 Summary

This paper estimates the impact of the enactment of US Immigration and Customs Enforcement (ICE) on the Army-enlistment propensities of naturalized citizens from 2000 to 2007. I use Army administrative and Current Population Survey (CPS) data to estimate the effects of federal-immigration policy on naturalized-enlistment rates using a generalized difference-in-differences model. I exploit variation in an immigration policy change and leverage service-eligibility criteria. I find that enlistment for 17 to 26-year-old, high-school-equivalent or more-educated naturalized citizens increased by 67.7% after the establishment of ICE on March 1, 2003, significant at the 5% level, suggesting that fear of potential family-member deportment likely drove these enlistment decisions. I find no evidence that naturalized citizens enlisted at different rates than native-born citizens on average over this period, illustrating assimilation of these first-generation Americans.

2.2 Introduction

Recruitment of the modern all-volunteer force relies on marketing campaigns and various incentives, including enlistment bonuses, educational subsidies, potential to 'see the world,' occupational training that translates into civilian jobs, health benefits, child care, and quality housing.[1] US-citizen service members with alien spouses, children, or parents (in

[1] Rostker (2008).

some cases) uniquely enjoy citizenship benefits that facilitate and can potentially expedite naturalization for their family members. Familial citizenship is important to naturalized citizens as their lawful-permanent resident (LPR)-family members face high time-based costs while awaiting naturalization. This paper examines whether a policy that increased the stringency of immigration-law enforcement motivated naturalized-US citizens to enlist at different rates than those of native-born US citizens, in light of these long-standing familial benefits.

This paper is the first study to find positive, significant effects of increased immigration enforcement on the military enlistment of naturalized citizens (relative to their native-born counterparts). It uses basic monthly Current Population Survey (CPS) and Army administrative data, which I collapse into cells by sex, two-year age group, citizenship status, and enlistment quarter. I use a general difference-in-differences (DinD) model to examine the effect of long-standing citizenship benefits for military families, immigration-policy changes, and key events on naturalized-enlistment rates. I constrain my sample based on service-eligibility criteria and use unanticipated immigration policy changes as a source of exogenous variation.

I examine the impact of the enactment of US Immigration and Customs Enforcement (ICE) on naturalized-enlistment rates relative to those of native-born citizens from 2000 to 2007. My model estimates that 17 to 26-year-old naturalized-enlistment rates increased after the establishment of ICE, an agency that increased the stringency of immigration law enforcement. Naturalized citizens exhibited a 67.7% increase in enlistment relative to native-born citizens, significant at the 5% level using a two-tailed test based on my DinD model.[2][3] This suggests that fear of potential family-member deportment by ICE likely drove those considering to enlist. Naturalized enlistment was not statistically different than native-born enlistment on average, suggesting assimilation by first-generation citizens.

[2]This equates to 81 more soldiers per quarter from a baseline of 2,273 naturalized citizens (over First Quarter 2000 to First Quarter 2003).

[3]All subsequent statistical-significance figures for my DinD estimates reflect two-tailed tests based on my DinD model. I will use shorthand stating that DinD estimates are 'significant at the 5% level.'

My paper fills a gap in immigrant-worker literature by analyzing naturalized-military service – a topic that has been largely overlooked by the academic literature.[4] Zong and Batalova (2019) describe Census Bureau statistics on foreign-born veterans, while the Congressional Research Service compiles information on policies, laws, and topics that affect foreign-born soldiers.[5] Hattiangadi et al. (2005) and McIntosh et al. (2011) are research reports that study the performance measures of LPR-service members, including the impact of citizenship on their attrition rates and the relative amount of LPRs who gain naturalization in each service (like the Air Force or Navy). Sandhoff (2013), Sullivan (2014), and Harvie (2014) are anecdotal and descriptive studies of respective Muslim, Latino, and Asian communities using military service as a means to earn citizenship; in contrast, I study a natural experiment on naturalized citizens using Army administrative data.

My paper most closely aligns with Cunha et al. (2014) and Himmelberger (2020), which are the first empirical studies of the effect of a citizenship-for-service policy on the military accessions of noncitizens.[6] They use DinD models to examine the July 2002 impact of Executive Order 13269 on total noncitizen accessions (over the eligible population), using CPS and military-personnel data. They use respective monthly and quarterly citizen accessions as their control groups and follow Hattiangadi et al. (2005), defining the service-eligible population as high-school or more educated who are in the labor force (and not currently serving).[7] Cunha et al. (2014) find no impact of the policy on total noncitizen accessions and Himmelberger (2020) finds a positive effect. Differently, I study the impact of an immigration policy on naturalized-enlistment propensity; as well, I study the impact of an immigration policy that affected the families of service-eligible naturalized citizens.

My paper explores enlistment propensity and recruitment incentives that affect the

[4]Immigrants have language skills and regional expertise that are invaluable in the US Army.

[5]Congressional Research Service Report RL31884 on February 25, 2009 is an example.

[6]Cunha et al. (2014) is an updated and published version of Can and Yalcinkaya (2013).

[7]Himmelberger (2020) incorporates the impact of post-9/11 and Patriot Act and studies the peacetime effect of an asylum-to-LPR policy on the enlistment propensity of service-eligible noncitizens.

occupational choice of military service versus civilian employment. Hattiangadi et al. (2004) studies how recruitment incentives attract Hispanic enlistees and many papers explore how monetary incentives impact accessions.[8] Bachman et al. (2000) find that high-school students with high-grades, college-educated parents, and college plans are less likely to enlist, while men, African Americans, and Hispanics are more likely. Christensen (2017) notes that increased-wartime casualties induce fewer people to enlist. These papers suggest mechanisms that impact enlistees regardless of citizenship; whereas, Section 2.2 introduces mechanisms unique to naturalized citizens.

The paper proceeds as follows. Section 2 provides background on US-immigration policies and events affecting the enlistment choices of naturalized citizens from 2000 to 2007. Section 3 depicts summary statistics and describes my data method. I outline my empirical strategy and results in Sections 4 and 5 respectively. I discuss my findings and conclude in Section 6. For the rest of the paper, I use the term 'naturalized' citizens to denote individuals who were born abroad and identify as naturalized-US citizens. I use the term 'native-born' to identify US citizens who were born in the United States, in a US territory (including Puerto Rico, Guam, or the US Virgin Islands), or to a US parent abroad. I use the term 'noncitizens' to identify individuals who were born abroad and identify as non-US citizens. 'Green-card holders' (or LPRs) are a subcategory of noncitizen.[9]

2.3 Institutional Background

Familial citizenship is important to naturalized citizens since their LPR family members face high temporal costs while awaiting naturalization. Naturalized citizens consider long-standing provisions that impact familial citizenship statuses and changes to immigration policy while deciding whether to enlist.

[8]These studies include Goldberg and Warner (1982), Dertouzos (1985), Polich et al. (1986), Asch and Warner (1995), Payne et al. (2001), Hansen and Wenger (2002, 2005), Hosek and Totten (2002), Hogan et al. (2005), Simon and Warner (2007, 2009, and 2010), Hosek and Martorell (2009), Asch et al. (2010), and Simon et al. (2010).

[9]USCIS defines LPRs as aliens admitted to and legally allowed to reside in the United States.

2.3.1 Familial Immigration and Naturalization Act Provisions

Different than US citizens, LPR family members can be deported if they commit violations or crimes, which can be as simple as failing to update a current address with US Citizenship and Immigration Services (USCIS). They face inadmissibility constraints based on criminal acts, medical conditions, or their potential for welfare dependency. LPRs must reapply for their green cards every ten years for a fee and they can lose their statuses if they leave the country for over a year. They cannot vote and face federal benefit and job constraints. While most LPRs wait a minimum of five years for naturalization, INA 319(a) provides a marriage exception that shortens the wait to three years if a LPR marries and resides with a US citizen.[10]

The Immigration and Naturalization Act (INA) has provisions that grant citizenship benefits to the spouses, children, and parents of citizen military-service members. While most LPRs wait a minimum of five years to apply for naturalization, INA 319(a) provides a marriage exception that shortens the wait to three years if a LPR marries and resides with a US citizen.[11] LPR-military spouses that file for naturalization under general or marriage provisions may count the time living abroad with their citizen service member toward residency and physical presence; furthermore, they may complete the entire naturalization

[10]The marriage exception requires one to reside with the US citizen or be a battered spouse of a US citizen. Other exceptions include automatic naturalization for children under 18 after the Child Citizenship Act in February 2001 if at least one parent is a citizen and the child is a physically present LPR, in physical custody of the US parent and immediate application for spouses of certain government employees living overseas. See https://www.nolo.com/legal-encyclopedia/when-apply-us-citizenship-46704.html.

[11]The marriage exception requires one to reside with the US citizen or be a battered spouse of a US citizen. Other exceptions include automatic naturalization for children under 18 after the Child Citizenship Act in February 2001 if at least one parent is a citizen and the child is a physically present LPR, in physical custody of the US parent and immediate application for spouses of certain government employees living overseas. See https://www.nolo.com/legal-encyclopedia/when-apply-us-citizenship-46704.html.

process from overseas.[12][13] INA Section 319(b) permits LPR-military spouses to forego residency and physical presence requirements entirely given that they live in marital union with the citizen spouse, but they must conduct their naturalization interviews and oaths in the United States.

LPR children of service members, aged 17 or younger, enjoy additional citizenship benefits. INA 320 states that LPR children residing in the United States with their citizen service members may acquire automatic naturalization, but they must take their oaths in the United States. INA 322 asserts that a citizen service member living abroad may apply for his or her biological, legitimated, or adopted children (born or residing outside of the United States) to gain naturalization.[14] Under INA 319(d), if a citizen active-duty member dies during his or her service, the surviving spouse, child, or parent may be eligible for naturalization as the surviving relative of the service member.[15]

2.3.2 Federal-Immigration Policies and Key Events

Table 1 outlines immigration policy changes and key events from 2000 to 2007 that impacted the enlistment choices of naturalized versus native-born citizens.[16] The attacks on

[12]US Citizenship and Immigration Services defines physical presence as the number of days that an applicant must physically be present in the United States during the required period until he or she files for naturalization.

[13]Under INA Sections 316(a) or 319(a)) respectively, spouses of US citizens must complete five or three years of residency respectively spending half of that time physically present in the United States. See https://www.uscis.gov/policy-manual/volume-12-part-i-chapter-9.

[14]The parent must have been physically present in the United States or its outlying possessions for at least five years, with at least two after the age of 14. If the citizen parent died within the past five years, a citizen grandparent or citizen legal guardian can submit for naturalization on behalf of the children.

[15]The surviving spouse must have been legally married to the member at the time of the service-member's death, even if he or she remarries later.

[16]The Child Citizenship Act (CCA) on February 17, 2001 (not depicted in Table 1) granted automatic citizenship to children of naturalized or native-born US citizens if the dependents were LPRs and aged 17-years or younger; although, this policy had a relatively insignificant impact on naturalized enlistment. The CCA reinforced the precedent that children stationed overseas with their service-member parents qualify as resident and physically present in the United States for citizenship applications, formalized by an amendment to INA 322 in January 28, 2008. See https://www.uscis.gov/policy-manual/volume-12-part-i-chapter-9.

9/11 marked the start of the War on Terror, which added the potential of combat injury or death to the enlistment decision. In late October 2001, the Patriot Act increased the monitoring authority of the US government to fight domestic and international terrorism, permitting government agencies to surveil any immigrant who might pose a threat to national security.[17]

The Homeland Security Act (HSA) on November 25, 2002 established the Department of Homeland Security (DHS), which divided the Immigration and Naturalization Service (INS) into three subordinate agencies on March 1, 2003. These agencies included ICE, US Customs and Border Protection (CBP), and USCIS. The National Defense Authorization (NDA) Act on November 24, 2003 expanded and funded the Iraq War; as well, it granted posthumous-naturalization rights for spouses, children, and parents of service members who die in combat and backdated it to 9/11.[18] I end my study in 2007 to avoid the confounding events of the Great Recession.

Table 2 aligns each policy or event with hypothesized mechanisms that may affect the enlistment choices of naturalized citizens, displaying the expected signs of their effects. If familial-citizenship benefits motivated naturalized enlistment, I anticipate a positive average marginal effect for naturalized citizenship. If fear of the potential deportment of family members drives naturalized-enlistment decisions, unfavorable policies have an uncertain sign since naturalized citizens with undocumented family members might enlist to avoid their family members' deportment or recede from government visibility.

If a policy enhances institutional trust or patriotism, I anticipate a positive effect on naturalized-enlistment rates; although, naturalized citizens could either embrace or oppose stronger enforcement of immigration laws, so the sign is undetermined in these cases. Cunha

[17]Quarterly-enlistment periods allow me to differentiate the post-9/11 period from the effect of the Patriot Act on October 26, 2001 for a forthcoming robustness test in Section 5.1.

[18]Title XVII, Section 1703 of the NDA states that an alien spouse (not legally separated), child, or parent of a US citizen at the time of his or her death, given honorable service and a death in or aggravated by military service, are considered immediate relatives and eligible for benefits and naturalization if filed within two years of the death. See https://www.congress.gov/bill/108th-congress/house-bill/1588.

et al. (2014) conjecture that a negative noncitizen-enlistment response in wartime might suggest a greater fear of combat death than US citizens. The same risk differential could exist for naturalized versus native-born citizens. Additionally, Section 3.2 recognizes that an extra step by recruiters to validate foreign education might be a mechanism that results in lower naturalized-enlistment rates.

2.4 Data

I use data divided into cells compiled from basic monthly CPS (January 2000 to December 2007) and Army administrative data, including records generated from all Army enlistees from 2000 to 2007.[19] While both provide citizenship designations, the CPS uniquely contains detailed nativity data and the Army administrative data have exact enlistment dates; thus, I merge Army-enlistment counts with service-eligible CPS population data. Since the CPS excludes members of the active-duty military from its 60,000 household survey, it can represent the service-eligible population. This automatically prevents an individual from being double-counted in the data or enlisting in another service, like the active-duty Air Force or Marine Corps. Soldiers appear once in the Army administrative data; if they reenlisted, I count them according to their initial enlistment dates so I never see the same person repeatedly.

2.4.1 Descriptive Statistics and Data Method

Table 3a shows sample restrictions in the Army administrative data and Table 3b shows identical restrictions imposed on the unweighted CPS-population sample.[20] I omit observations lacking information on citizenship status, sex, age, Hispanic ethnicity, or civilian education. I drop observations from birth countries where US-citizenship status is uncertain.

[19]IPUMS CPS generated my CPS data extract and I accessed a Total Army Personnel Database extract on site at the Office of Economic Manpower Analysis (OEMA) at West Point, NY during multiple trips from October 2018 to January 2020.

[20]I rounded each restriction in Table 3a to the nearest 100 to safeguard the immigrant-enlistee data.

I omit cases where citizenship status does not align with entry citizenship, as well as where immigration year is incongruent with age. I drop service-eligible individuals from before 2000, September 2001, and after 2007, noncitizens, people older than 26, and those with less than a high-school equivalency from the sample.

Table 4 shows descriptive statistics for the CPS-population sample from 2000 to 2007. I summarize all variables in my forthcoming model, including individual and familial-demographic traits. 2.4% of the sample self-identifies as naturalized citizens, 52.1% are women, and the average age is 22.1. I display comparable count data for enlistees relative to the service-eligible population in Figures 1a through 1f. These figures compare the individual characteristics of 17 to 26-year old naturalized versus native-born US citizens across the Army enlistment data (left graphs) and weighted CPS-population data (right graphs).

The left column of Figure 1a displays the counts of naturalized and native-born enlistments over time and the right column shows similar weighted CPS-population counts. The number of naturalized and native-born enlistees remained relatively constant over the period, showing a small increase for naturalized citizens around 2004 (by about 75 enlistees per quarter). The relative population of naturalized and native-born citizens increased gradually, with the naturalized population growing at slightly steeper rate.

The left columns of Figures 1b through 1e depict variable counts over the total number of 17 to 26-year old naturalized or native-born enlistees, while the right columns show the relative percentages in the weighted CPS population.[21] Figure 1b shows that naturalized enlistees were older on average than their native-born counterparts. Figure 1c illustrates that 83.8% of naturalized and 91% of native-born enlistees had a high-school equivalency relative to a more diversely educated population. It also highlights that naturalized enlistees were more educated on average than native-born enlistees at every educational level during

[21]These percentages match the averages from Table 4 albeit the racial data in Figure 1d, which aligns the CPS data with the singular $race_i$ variable from the Army administrative data.

this period.[22]

Figure 1d illustrates the percentage of enlistees by sex and racial group. Relative to their proportion in the population, black women and Hispanic-naturalized women enlisted at higher ratios relative to their CPS populations; meanwhile, white women and 'other'-naturalized women enlisted at lower ratios than their relative populations. During this period, women made up 21.7% of the active-duty Army. Army enlistees were 68.8% white, 16.4% black, 10.4% Hispanic, and 4.4% of another race. Figure 1e depicts the marital statuses of enlistees relative to the CPS population. Army enlistees were about 9% more likely to remain single than their civilian counterparts. Also, Figure 1f shows the average contract length for naturalized versus native-born enlistees. About 78% of enlistees signed initial contracts committing them to three or four years of service. Naturalized enlistees were about 1% more likely to sign up for four years or less relative to native-born citizens.

Identical to the data-compilation technique in Himmelberger (2020), I disaggregate counts of active-duty Army enlistees by sex, two-year age group, citizenship status, and enlistment quarter, which provide the numerators for my dependent variable. Using the same four groups, I disaggregate counts of weighted-CPS demographic variables to be my control-variable numerators. Finally, I disaggregate counts of the weighted-CPS populations by the same four categories to serve as the denominators for my dependent and control variables. I collapse and sort the counts of Army enlistments, weighted-CPS demographic controls, and weighted-CPS populations into cells by sex, two-year age group, citizenship status, and enlistment quarter. While collapsing the data, I weight the CPS samples. Finally, I divide the numerators by the respective denominators, albeit family size.

[22]Relative to native-born enlistees, naturalized enlistees were 0.2% less high-school dropouts, 7.2% less high-school equivalents, 2% more with some college, 0.9% more with an associates degree, and 4.5% more with college or more education.

2.4.2 Service-Eligible Sample Selection

Individuals have voluntarily entered the US military as enlisted personnel or commissioned officers since the end of the draft-era Selective Service on April 1, 1975.[23][24] An estimated 0.5% of the population enlists in the active-duty military, of which 36.6% enlist in the active-duty Army.[25] To enlist in the US Army, an individual must be a US citizen or LPR, pass a physical-medical exam, be at least 17-years old (given parental consent), and have a high-school diploma. The US Army grants exceptions for a limited annual percentage of recruits with General Education Development (GED) credentials; however, these individuals require waivers and must exceed minimum-threshold scores on the Armed Forces Qualification Test (AFQT). Army recruits typically must be younger than 36, achieve a minimum AFQT score of 31, and have no more than two dependents.

Individuals between the ages of 17 and 26 account for about 93.14% of Army enlistees from 2000 to 2007; therefore, I isolate the 17 to 26-year old service-eligible population over this period. I use five two-year age groups starting with 17 to 18-year olds to create a recent high-school-graduate age bracket since 6.1% of native-born and 5.2% of naturalized enlistees are 17 years old and 5.9% of 17-year olds have a high-school equivalency or more education.[26] I assume that there is no significant difference between the enlistment choices of 17 versus 18-year olds and so forth.

High-school dropouts and GED holders can comprise no more than 10% of annual

[23]The Vietnam draft ended on January 27, 1973 and the modern Selective Service System resumed in July 1980, requiring all 18 to 25-year old male US citizens and dual-nationals to register. Registration is a naturalization prerequisite; although, there are bilateral-treaty exceptions for specific dual nationals. Women do not have a Selective Service registration requirement; although, with the 2015 opening of combat-arms occupations to women, the Pentagon recommended registration for women in 2017. See (1) https://www.sss.gov/history-and-records/ & (2) Manchester (2017).

[24]Commissioning sources for officers include service academies, like the United States Military Academy, Reserve Officers' Training Corps (ROTC) programs, the Green to Gold program for enlisted soldiers to become officers by completing their undergraduate education, and direct commissions for those with professional degrees.

[25]Reynolds and Shendruck (2018).

[26]I take the CPS sample from Table 3a, Row 8 (1,470,993) and drop all non-17-year olds (leaving 178,587). 10,496 had a high-school equivalency or more. 10,496 / 178,587 = 5.9%.

Army enlistment.[27] By exception, high-school dropouts account for 0.63% of naturalized and 0.86% of native-born enlistees from 2000 to 2007; however, their inclusion invalidates my main service-eligible criterion to minimize classification bias in the CPS population. Following Hattiangadi et al. (2005), Cunha et al. (2014), and Himmelberger (2020), I drop observations with less than a high-school equivalency to isolate the service-eligible population currently in the labor force (and not serving in the active-duty military), while preventing the assignment of CPS-population-level probabilities to cells that should be empty. My main specification captures those with a high-school equivalency or more education. I run a robust specification on all educational levels (including high-school dropouts), as well as a robustness test constrained to high-school equivalents.

McIntosh et al. (2011) describes an additional requirement for foreign-born recruits that attended foreign schools – they must provide recruiters with translated copies of their foreign-educational credentials. Recruiters verify these credentials with a local community college or accrediting institution. Some recruiters note that this step increases their workloads as per McIntosh et al. (2011); although, another recruiter described this step as relatively costless and routine given standing relationships with local community colleges.[28] Depending on the perceived inconvenience, recruiters might be less likely to recruit a naturalized citizen over a similar native-born one; however, if this bias exists, it is likely negligible given wartime demands on recruiters to meet their quotas.[29]

Given this upfront recruiter cost, it is feasible that the average naturalized enlistee is more qualified than a similar US citizen. In part, this may explain why a greater percentage of naturalized citizens enlist with more civilian education than native-born citizens across

[27] A 2005 policy raised the threshold for GED waivers; yet, restrictions on high-school-dropout recruitment remained unchanged from 2000 to 2007. I control for high-school equivalency, which includes high-school graduates and GED holders, making this policy change irrelevant to my model. Discussion with Luke Gallagher, data manager at OEMA, on or about January 15, 2020 at West Point, NY.

[28] Discussion with SFC Raymond Theriot, former US Army recruiter, on or about January 15, 2020 at West Point, NY.

[29] Army recruiters failed to meet their enlistment goals in 2005. Baldor (2018).

every educational category. Furthermore, naturalized enlistees may have educational credits that they are unable to verify via incomplete or untranslated documentation. Armed Services Vocational Aptitude Battery (ASVAB) or AFQT score controls could help to minimize potential recruiter and/or classification bias from an imperfect service-eligible population. Without sizable data on population-level ability scores for naturalized and native-born citizens, I must accept some classification bias.[30]

I consider whether the remaining omitted service-eligibility criteria in my error term correlate with citizenship or my other explanatory variables, which would be concern for measurement error.[31] Since I use CPS data to generate my service-eligible sample, I lack population-wide service-eligibility data on AFQT scores, English proficiency, medical or fitness histories, and criminal backgrounds; thus, I do not control for them.[32] Already controlling for education, race, Hispanic ethnicity, birth region, and period of arrival, it is unlikely that an AFQT score is correlated to being a naturalized citizen. An English proficiency test is part of the naturalization process, which negates this potential source of error. There is little concern that medical histories correlate to citizenship status (or a racial group) due to the young age of the sample. Evidence regarding the link between immigrants and crime points toward a negligible effect on average.[33] If correlation with one of these criterion and the error term is proven, future research can find an instrument to handle the endogeneity. The conditions for this valid instrument are that it has no independent impact on enlistment while inducing change in my explanatory variable of interest, revealing the unbiased enlistment impact of being a naturalized citizen after the treatment.

[30]The National Longitudinal Survey of Youth (NLSY) 1997 has population-wide ASVAB scores; although, the naturalized citizen sample is relatively small.

[31]I control for citizenship status, age, and educational achievement (minimal high-school equivalency) as per Hattiangadi et al. (2005) and Cunha et al. (2014) (using a different age group).

[32]I considered using the National Longitudinal Survey of Youth (NLSY) 1997 for population-wide ASVAB scores, but the noncitizen sample is too small.

[33]Adelman et al. (2005) find that immigration does not increase crime rates, while it can help lessen metropolitan rates. Bell et al. (2013) find a small positive to no effect on property crime and no link between immigration and violent crime.

2.4.3 Naturalized Versus Native-Born US Citizens

My treatment and control groups are service-eligible naturalized and native-born US citizens, where an immigration-enforcement policy should only influence naturalized-citizens' households and their subsequent enlistment choices. The indicator variable $Naturalized_n$ equals one for all naturalized citizens (treatment group) and zero for native-born US citizens (control group).[34] I drop observations from the American Samoas, Northern Mariana Islands, Marshall Islands, and other US Pacific territories since their citizenship designations vary by birth cohort and location.[35]

I accept classification bias given potential noncitizens in the CPS naturalized-citizen data if they incorrectly self-identified as naturalized citizens due to fears of deportation or survey misunderstandings. Brown et al. (2018) compare administrative records to the self-reported portion of noncitizens in the CPS population, finding lower estimates than records substantiate; thus, while the Army administrative data is likely accurate given citizenship-record verification, the relative service-eligible population may suffer from classification bias. I drop individuals with less than a high-school equivalency for my main specification, which mitigates potential undocumented immigrants; however, my all-education robustness test includes high-school dropouts, which increases potential classification bias.[36]

[34]The US State Department categorizes 'children born to US citizens abroad,' Guam residents born after April 11, 1899, Puerto Ricans after April 10, 1899, and US Virgin Island residents born after January 16, 1917 as native-born citizens. See USCIS Policy Manual, Volume 12, Part A, Chapter 2.

[35]American Samoan residents qualify as US nationals and Northern Mariana Islands residents born after 1986 are US citizens; hence, I drop these observations, as well as those from the US Outlying Areas (not specific), North America or Americas (not specific), Marshall Islands, Micronesia, and Other or Unknown.

[36]Himmelberger (2020) uses high-school equivalency to segment service-eligible green card holders from undocumented immigrants and Devadoss et al. (2019) uses it to separate skilled from low-skilled Mexicans to denote likely migrant-US workers.

2.5 Empirical Strategy

I use repeat cross-sectional cellular data to estimate a generalized DinD model with robust standard errors to mitigate heteroskedasticity. My counter-factual scenario is how service-eligible naturalized citizens would have enlisted relative to native-born ones absent the establishment of ICE. Event studies and placebo-test results confirm the common-trends assumption.

2.5.1 Linear Model

The model follows, where s signifies sex, a signifies a two-year age group, n signifies citizenship status, and t signifies a quarterly period for a particular cell:

$$Enlisted_{asnt} = \beta_1 I(n = Naturalized) \times Post\_ICE_t + \psi_t +$$
$$\beta_2 I(n = Naturalized) + \alpha_s + \rho_a + \beta_3 \mathbf{X}_{asnt} + \beta_0 + \epsilon_{asnt},$$

where $Enlisted_{asnt}$ is the ratio of Army enlistment counts to the weighted-CPS population. The coefficients ψ_t estimate quarterly-time effects relative to the omitted First Quarter 2000 (Q1 2000). $Naturalized_n$ indicates whether a cell contains naturalized citizens, equaling one and zero otherwise. $Post\_ICE_t$ indicates whether a cell is from April 1, 2003 to December 31, 2007 and zero otherwise. α_s identifies gender effects, where a cell equals one if it contains women and zero otherwise. The coefficients ρ_a estimate age-group effects, relative to omitted 17 and 18-year olds. The interaction-variable $Naturalized_n \times Post\_ICE_t$ is a binary treatment effect, equaling one if the cell contains naturalized citizens from April 1, 2003 to December 31, 2007 and zero otherwise.[37]

The $\mathbf{X}_{asnt}$ coefficient contains explanatory variables that impact enlistment propensity. These variables include race, Hispanic ethnicity, marital status, number of children, birth

[37]To test the stability of my DinD-coefficient estimate, I add a policy control $Naturalized_n \times Post\_NDA_t$, which equals one if the cell contains naturalized citizens from January 1, 2004 to December 31, 2007 and zero otherwise.

region, US-arrival period, and civilian education, constructed from weighted-CPS counts over the weighted-CPS population. A final control denotes the average number of family members per representative cell. Entry-civilian education in the Army administrative data align with the CPS educational categories.[38]

The controls for birth region and period-of-US arrival account for changes in the foreign-born population from 2000 to 2007, ensuring consistency across the treatment and control populations. I compare birth regions across treatment and control groups since about 1.5% of US citizens are born abroad; whereas, I use birth cohorts to determine periods-of-US arrival for native-born citizens (including those born in Puerto Rico, Guam, and the US Virgin Islands).[39] US-arrival-period controls account for the impact of recency of the arrival of foreign-born citizens on their enlistment likelihoods.[40] I control for national changes with quarterly-time effects, which account for outside civilian-employment options. I indirectly account for income via controls for two-year age group, educational level, and family size.

Table 5 shows arithmetic DinD estimates of enlistment propensity for high-school equivalent or more educated citizens across treatment and control groups for pre and post-treatment periods, given the establishment of ICE as the treatment event. The native-born control group captures changes in recruitment over time. My enlistment propensity estimates match those in Table 5 where I regress $Enlisted_{asnt}$ on $Naturalized_n \times Post\_ICE_t$, the post-treatment period, and $Naturalized_n$, excluding all other controls.

[38]The Army Total Personnel Database defines the following educational categories: 'high-school dropout;' 'GED' (including GED, home school, National Youth Challenge, distance learning, high-school certificate of attendance, and other non-traditional credentials); 'high-school graduate' (including high-school graduates, high-school seniors, currently in high school, adult-education diploma, and Job Corps); 'some college' (including currently enrolled or completed 15 semester hours or greater); 'associates degree' (including associates or professional nursing degrees); 'college' for a college graduate; and 'graduate' for a graduate degree. There are no high-school seniors or current high-schoolers in the Army data.

[39]Five additional variables at the bottom of Table 4 (not included in my model) depict arrival periods for all foreign-born individuals not born in the United States, Puerto Rico, Guam, or the US Virgin Islands. A comparison of these percentages relative to my control variables (which include native-born individuals) reveals the exact proportion of immigrants relative to the control group.

[40]The CPS changed the year-of-immigration periods over this period, making a uniform control for recent arrival unfeasible.

2.5.2 Assumptions

A DinD model has two identifying assumptions. First, the treatment must be exogenous of naturalized versus native-born US citizen enlistment propensities. I use a largely unanticipated immigration policy to generate exogenous variation; as well, the attacks on 9/11 were national-security shocks (for a forthcoming robustness test), enabling my use of these policies and events as sources of exogenous variation in enlistment propensity.

Second, enlistment-likelihood trends should remain the same for naturalized and native-born US citizens in the counterfactual scenario where the establishment of ICE never occurred. In other words, I assume that no pre-treatment trends indicate that common trends would have remained throughout the post-treatment period absent the initiation of ICE on March 1, 2003. I conduct an event study for significant pre-trends in enlistment propensity for my treatment and control groups, adding naturalized-quarterly interaction terms to my DinD model rather than treatment effects. Angrist and Pischke (2009) note that pre-treatment estimates should be statistically insignificant and close to zero with estimated post-treatment estimates remaining unchanged.

The event study generalizes the model as follows:

$$Enlisted_{asnt} = \theta_1 I(n = Naturalized) \times \psi_t + \theta_2 I(n = Naturalized) + \psi_t +$$
$$\alpha_s + \rho_a + \theta_3 \mathbf{X}_{asnt} + \theta_0 + \epsilon_{asnt}.$$

The coefficients ψ_t estimate quarterly-time effects relative to the omitted First Quarter 2003 (Q1 2003) for normalization. I regress DinD models with robust standard errors, noting the significances of the pre-treatment effects. Table 6 reflects the quarterly DinD point estimates of dual-aged 17 to 26-year old naturalized-US citizens (relative to native-born citizens) from 2000 to 2007, adding controls to demonstrate coefficient stability.[41] All specifications

[41]The event studies in Table 6, as well as Figure 2, reflect pre-treatment trends with the policy treatment and control, not showing post-treatment quarterly trends. Here, I omit Q1 2000 for colinearity.

show insignificant pre-trends, including my main model (Column (1)) and two robust educational specifications (Columns (2) and (3)). Figure 2 plots all $Naturalized_n \times \psi_t$ enlistment propensity coefficients with a high-school equivalency or more from 2000 to 2007 (with 90% confidence intervals). The trends remain steady and insignificant before the treatment events. This is not surprising given that quarterly-point estimates may not generate enough power to observe statistically significant trends; thus, I use placebo tests to evaluate longer pre-treatment periods to confirm the common-trends assumption.

Columns (1), (2), and (3) of Table 7a show the placebo-test results for my main-model specification (with a high-school equivalency or more education), applying three placebo-treatment events to the period from January 2000 to March 2003 to ensure insignificant pre-treatment trends. I use the respective periods from April 2001 to March 2003, October 2001 to March 2003, and April 2002 to March 2003 as averaged-placebo treatments with and without controls. All three tests remain insignificant before and after the placebo treatments, confirming the common-trends assumption.

Table 7b shows the same placebo tests on two alternative-robust specifications: high-school equivalents in Column (1a) and all educational levels in Columns (1b, 2b, & 3b). Column (1a) shows that the high-school-equivalent specification fails the placebo test from April 2002 to March 2003, as the interaction denoting a naturalized citizen in Q2 2000 is 0.00071 percentage points (pp), significant at the 10% level. This indicates that forthcoming results on this specification in Section 5.1 are not robust; however, the three placebo tests in Columns (1b, 2b, & 3b) for the all educational specification pass, as the pre-treatment estimates remain statistically insignificant.

2.6 Results

I estimate the impact of federal-immigration policies and events on naturalized enlistment from 2000 to 2007 for my main specification and three robustness tests. Additionally, I estimate average marginal effects of my controls on naturalized enlistment.

2.6.1 Results for 2000 to 2007

Table 8a shows enlistment-propensity estimates for naturalized versus native-born 17 to 26-year-old citizens with three different educational specifications, including my main model of high-school equivalents or more education in Columns (1) and (2) and two robust specifications of high-school equivalents (Columns (1a) and (2a)) and all educational categories (Columns (1b) and 2b)). Odd columns treat for the initiation of ICE given all explanatory variables and even columns add a NDA policy control to confirm the stability of the ICE estimate. Table 8b incrementally builds the estimates for the main model, starting with no controls (except for the NDA) in Column (1) and adding demographic controls in Columns (2 to 6), showing the stability of the treatment effect in Row (1).[42]

The DinD estimate for naturalized Army enlistment by high-school equivalent or more educated individuals after the implementation of ICE is 0.00015 pp, significant at the 5% level (see Row (1), Column (2) of Table 8a).[43] This reflects a 67.7% increase in naturalized enlistment, or a back-of-the-envelope estimated-quarterly increase of 81 naturalized enlistees from April 2003 to December 2007.[44] This suggests that ICE increased fear of potential family-member deportment and/or increased institutional trust by the naturalized population for greater governmental-immigration enforcement. Naturalized enlistment on average was statistically insignificant, providing no evidence that they enlisted for familial citizenship benefits (in general) and exhibiting assimilation by first-generation citizens.

Given multiple confounding events following 9/11, I run an alternative model adding the post-9/11 period, Patriot Act (also serving as a lagged 9/11 effect), and Homeland Security Act (which announced the pending enactment of ICE) as three treatment effects prior

[42] As I add controls to Rows (1) and (3) in Table 8b, the coefficient estimates increase, stabilizing only after the addition of the immigrant controls. The $Naturalized_n$ coefficient in Row (3) only becomes statistically insignificant because the standard errors increase so much.

[43] I divide the estimate by the pre-treatment, naturalized enlistment rate from Table 5 (0.00015/0.00022).

[44] I take the count of naturalized enlistees from Q2 2003 to Q4 2007 with a high-school equivalency or more (3,811 soldiers) and divide this by 167.7%, estimating that 2,272.5 naturalized citizens would have enlisted absent the initiation of ICE. 3,811 - 2,272.5 = 1,538.5 / 19 quarters = 80.97.

to and including ICE (Policies 1, 2, and 3 in Table 1). Column (1) of Table 6 shows no pre-treatment trends for the quarterly-DinD interactions from Q2 2000 to Q3 2001. Column (5) of Table 8c displays the resulting coefficient estimate for the establishment of ICE, which remains relatively stable at 0.00013 pp given the additional treatment events, significant at the 10% level. This estimate reflects a 59.1% increase in naturalized-citizen enlistment by an average of 75 enlistees per quarter.

I estimate the impact of ICE on naturalized-enlistment propensities for high-school equivalents and all educational levels as robustness tests. The DinD estimate for naturalized Army enlistment by high-school equivalents in Row (1), Column (2a) of Table 8a is statistically insignificant; although, this result is not robust given a failed placebo test, outlined in Section 4.2. The DinD estimate for the all educational specification complements my main results at 0.00004 pp, significant at the 5% level (see Row (1), Column (2b) of Table 8a). It reflects a 40% increase in naturalized enlistment, or a back-of-the-envelope estimated-quarterly increase of 58 naturalized enlistees from April 2003 to December 2007. This estimate increases classification bias since it omits a service-eligibility criterion that applies to over 99% of enlistees and only affects 41 naturalized enlistees over this period.

2.6.2 Average Marginal Effects of Explanatory Variables

I display relevant average marginal effects (AME) from all three specifications in Figures 3a and 3b; although, the middle rows depict my main model (the high-school equivalent or more educated service-eligible population). Naturalized enlistment on average (not depicted) was insignificant relative to that of native-born citizens, suggesting assimilation. While the left column of Figure 3a shows that AME for birth region is insignificant, the right-hand-side column shows positive and significant US-arrival AME on enlistment. High-school-equivalent or more-educated cells arriving from 1980 to 1985 were 0.03% more likely to enlist than those from 1974 to 1979, significant at the 0.001 level. Those who arrived from 1986 to 1991, 1992 to 1995, and 1996 to 2007 were respectively 0.03%, 0.07%, and 0.07%

more likely to enlist than cells arriving from 1974 to 1979, all significant at the 0.01 level.[45] These positive enlistment trends for recent US arrival suggest that immigrants respond to enlistment incentives more than native-born citizens.

Figure 3b reflects AME of marital status, number of children, and family size. Enlistment propensities for separated people are 0.24% higher than their never married counterparts, significant at the 0.01 level for my main specification. Enlistment propensities for all educational levels are 0.02% higher for married cells with their spouses present and 0.07% higher for divorced cells than never married ones, both significant at the 0.05 level. All specifications show that more children in the household reflects positive AME on enlistment propensity to varying levels of significance. One child increases enlistment by 0.07%, two children by 0.15%, three by 0.2%, and four or more by 0.01%, respectively significant at 0.05, 0.01, 0.001, and 0.05 levels for my main model. This main specification shows that greater family size increases enlistment propensity by 0.01%, significant at the 0.10 level. These household-oriented AME reflect the importance of family in enlistment decisions.

2.7 Discussion and Conclusion

Table 9 summarizes how the hypothesized mechanisms unique to naturalized-citizen enlistment align with my estimated results. It is impossible to determine what combinations of motivations drive individuals to voluntarily enlist in the Army, as well as distill recruiter incentives to recruit one population over another. However, there are mechanisms unique to the service-eligible naturalized population that are testable via natural experiments. Naturalized enlistees in the active-duty Army from 2000 to 2007 may have enlisted due to citizenship benefits for family members, fear of potential deportation of family members via stricter immigration enforcement, institutional trust or distrust for stronger immigration enforcement, or different risk preferences for combat than native-born citizens.

[45] Given that the native-born control group reflects arrival periods based on birth cohort, only the two final periods are comprised of all foreign-born people.

An estimated 67.7% increase in naturalized Army enlistment following the establishment of ICE (significant at the 5% level) suggests that fear of potential family-member deportment likely drove decision-makers. While increased immigration enforcement may have inspired some naturalized enlistees, the policy likely decreased institutional trust for others, especially those with family members in precarious citizenship categories. Reduced institutional trust would likely decrease naturalized-enlistment propensity, making this mechanism unlikely. This suggests that naturalized citizens respond to immigration policies that affect their families, enlisting to secure benefits that protect their households given more stringent immigration enforcement; however, there is no evidence that naturalized citizens enlisted for these benefits on average from 2000 to 2007.[46]

These results highlight demographic considerations that affect enlistment propensities, especially for those with established households. The AME associated with separation is 0.24% greater enlistment than the never married service-eligible population (significant at the 0.01 level) and AME associated with marriage and divorce are respectively 0.02% and 0.07% greater, both significant at the 0.05 level. Family size has a positive and significant impact on enlistment, as well as an incrementally greater number of children in the household (relative to none), regardless of educational background.

The importance of these household AME on enlistment rates draw attention to an August 2019 reinterpretation of the Child Citizenship Act, wherein USCIS categorized children stationed overseas with their service-member parents as not 'residing in the United States.' This reinterpretation delays citizenship and green-card applications for naturalized and native-born adoptive parents serving overseas, delaying automatic-US-citizenship transmittal until service-members and their families return stateside.[47] This is the first disincentive that directly targets naturalized citizens via their children; yet while this only impacts a few hundred service members per year, it targets a small sub-population inherently endowed with

[46]These results reflect a lower bound of the response to ICE since the Army comprised about 36% of the active-duty military as of 2007. DMDC Active Duty Military Personnel Master File, Chart 1.05, *2007 Demographics Report*.

[47]See USCIS Policy Alert dated August 28, 2019.

diversity, regional knowledge, and language skills that are critical for US Army operations worldwide. This study illustrates that familial considerations drive enlistment propensities; thus, I recommend a reassessment of this policy (and similar ones in the future) since the reduced costs in manpower and morale outweigh the benefits.

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2.9 Appendix of Figures and Tables

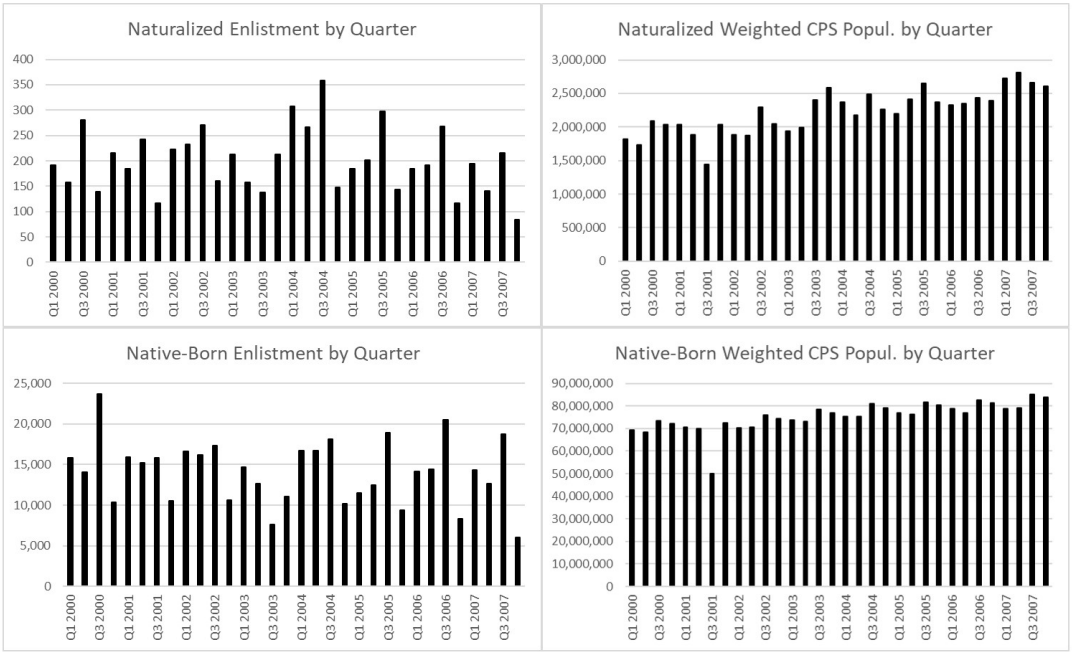

Figure 1a: Enlistment versus Weighted CPS Population Counts by Quarter

Note: The left column reflects the counts of 17 to 26-year old, high-school equivalent or more educated enlistees by quarter from 2000 to 2007. The right column shows the same relative ratio from the weighted CPS population. US Army administrative data and IPUMS-CPS.

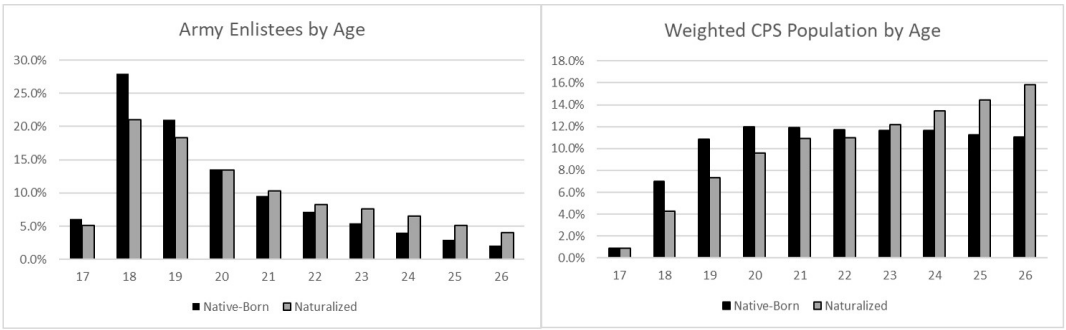

Figure 1b: Enlistment versus Weighted CPS Population Percentages by Age

Note: The left graph reflects the count of 17 to 26-year old, high-school equivalent or more educated enlistees by age over the total number of native-born or naturalized enlistees from 2000 to 2007 (451,388 and 6,441 respectively). The right graph shows the same relative ratio from the weighted CPS population. US Army administrative data and IPUMS-CPS.

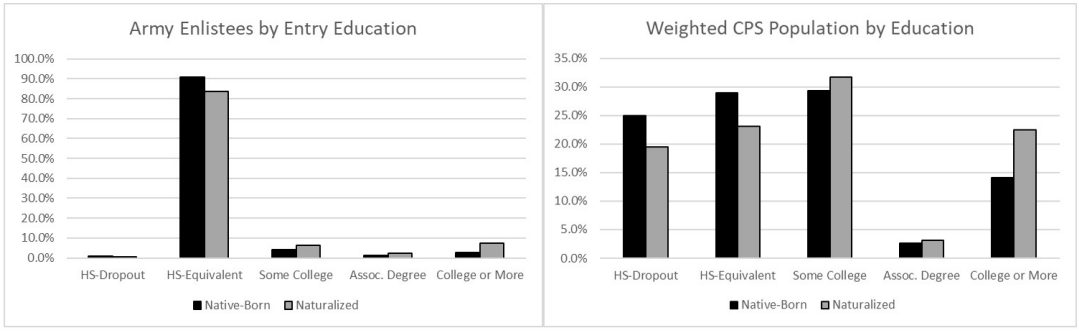

Figure 1c: Enlistment versus Weighted CPS Population Percentages by Education

Note: The left graph reflects the count of 17 to 26-year old enlistees by entry education (including high-school dropouts) over the total number of native-born or naturalized enlistees from 2000 to 2007 (455,289 and 6,482 respectively). The right graph shows the same relative ratio from the weighted CPS population. US Army administrative data and IPUMS-CPS.

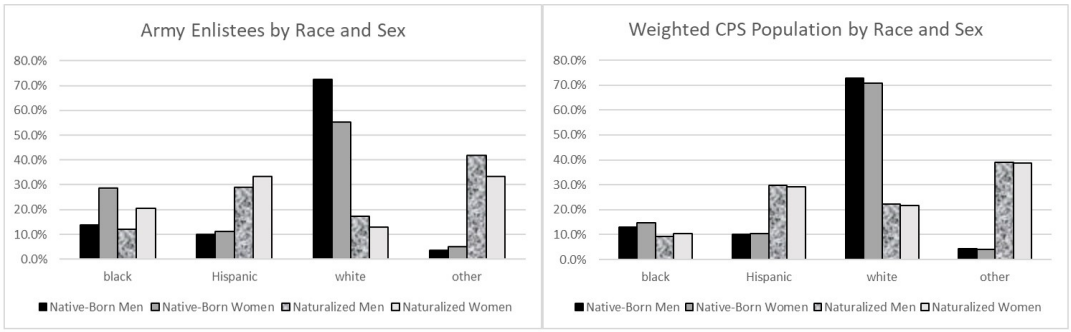

Figure 1d: Enlistment vs. Weighted CPS Population Percentages by Race and Sex

Note: The left graph reflects the count of 17 to 26-year old, high-school equivalent or more educated enlistees by race and sex over the total number of native-born or naturalized, male or female enlistees from 2000 to 2007 (371,086 native men, 80,302 native women 4,993 naturalized men, and 1,448 naturalized women respectively). The right graph shows the same relative ratio from the weighted CPS population. US Army administrative data and IPUMS-CPS.

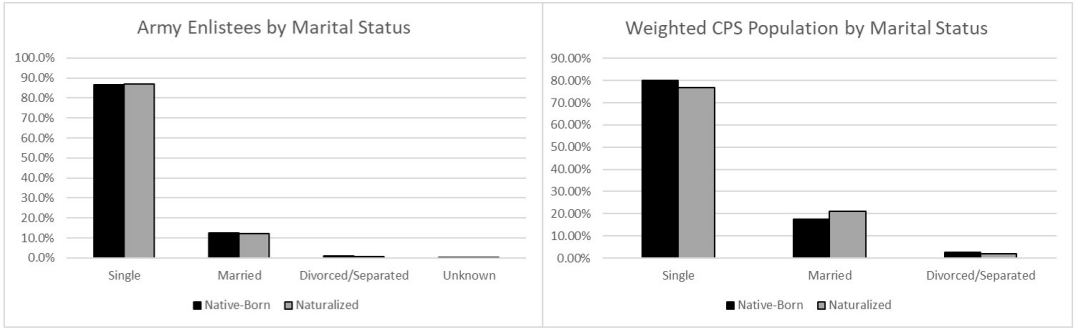

Figure 1e: Enlistment vs. Weighted CPS Population Percentages by Marital Status

Note: The left graph shows the count of 17 to 26-year old, HS equivalent or more educated enlistees in each marital group over the total number of native-born or naturalized enlistees from 2000 to 2007 (451,388 and 6,441 respectively). The right graph shows the same relative ratio from the weighted CPS population. US Army administrative data and IPUMS-CPS.

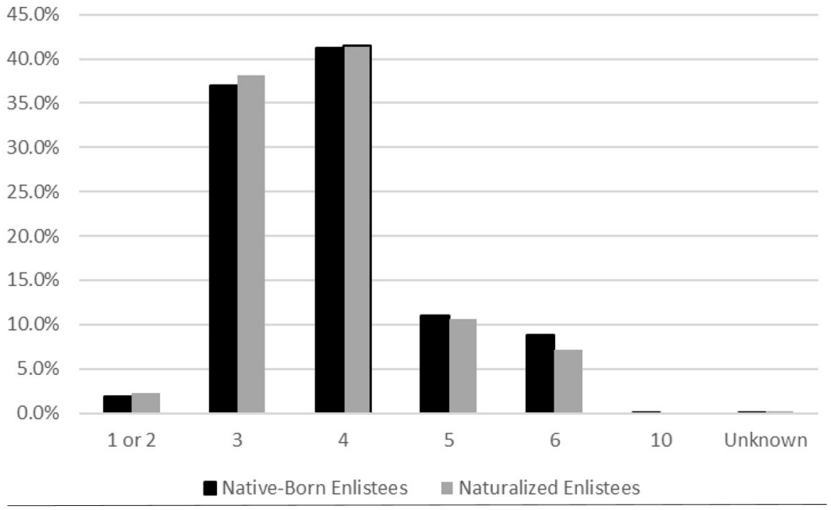

Figure 1f: Percentages of Entry Contract Lengths in Years

Note: These contract-length percentages reflect the count of contract length over the total number of 17 to 26-year old native-born or naturalized enlistees from 2000 to 2007 (451,388 and 6,441 respectively). I exclude those with less than a high-school equivalency. These contracts can end early due to a serious injury or illness that inhibits further active-duty service, or if a soldier leaves the service under less-than-honorable circumstances. US Army administrative data.

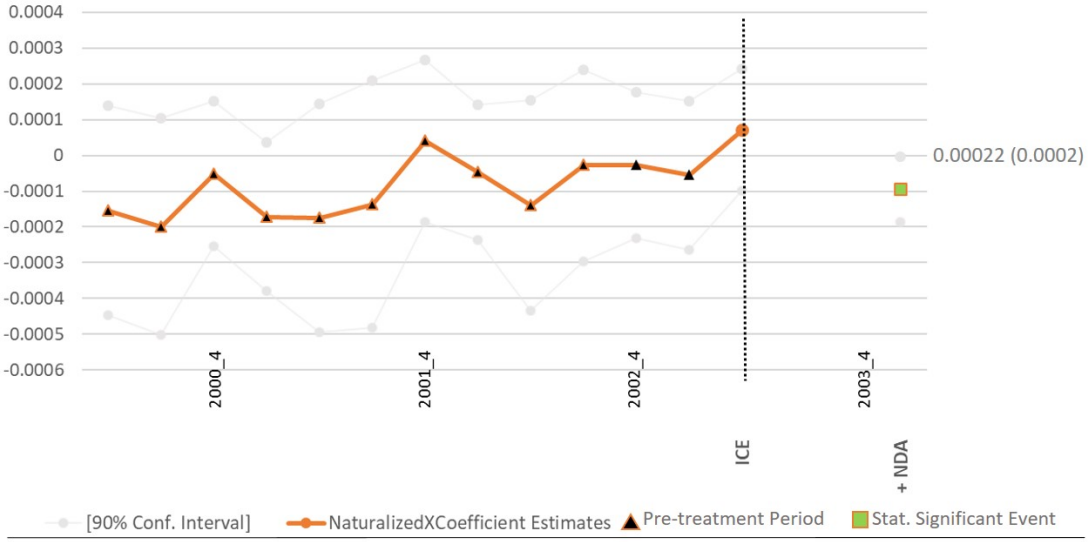

Figure 2: Event Study of Naturalized Enlistment Propensity from First Quarter 2000 to Fourth Quarter 2007 for High-School Equivalents or More Aged 17 to 26

Note: The right axis shows the baseline naturalized pre-treatment enlistment propensity (std. dev. in parentheses). The treatment effect, noted by a dotted line, is when Immigration and Customs Enforcement (ICE) stood up on March 1, 2003. The latter coefficient reflects a policy control for the National Defense Authorization (NDA) Act of late November 2003. I omit First Quarter 2000 due to collinearity. Robust p-values + $p < 0.10$, * $p < 0.05$.

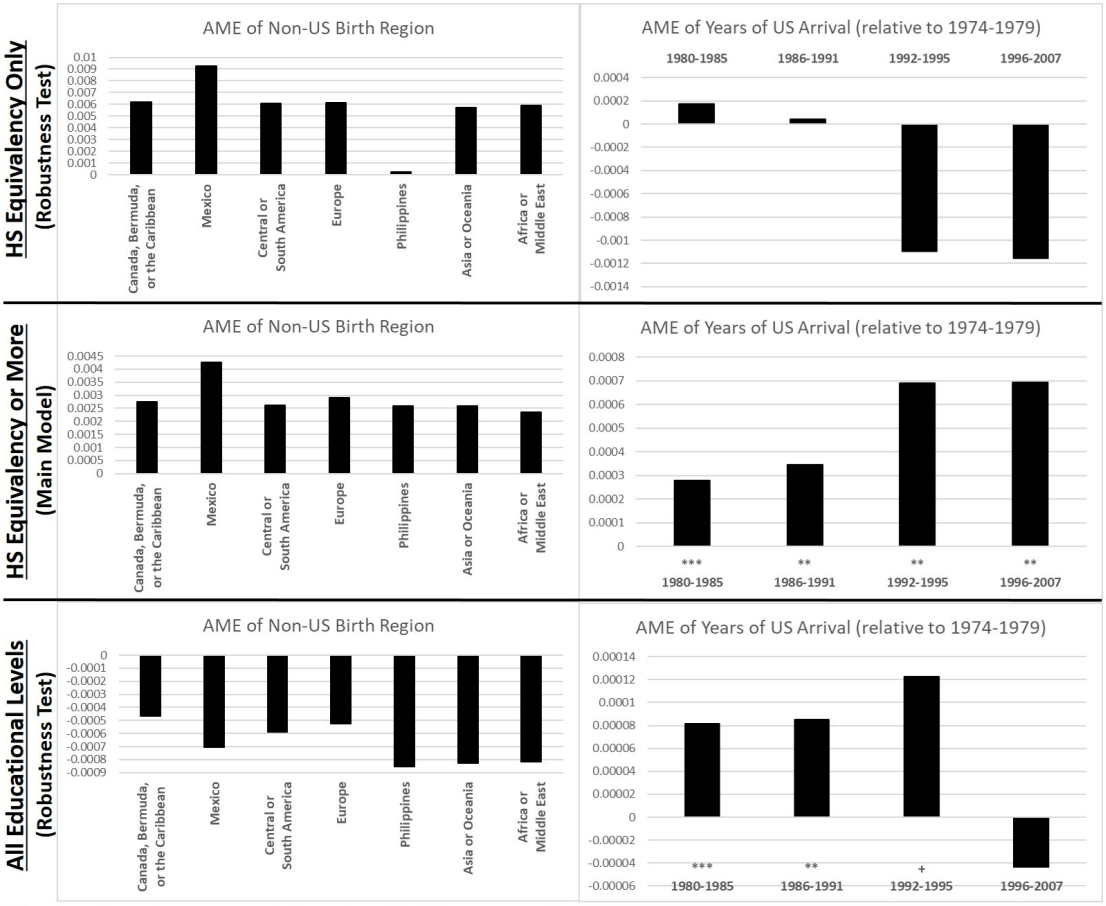

Figure 3a: Average Marginal Effects on Enlistment Propensity (2000 to 2007)

Note: The middle row depicts AME for the main model. The top and bottom rows reflect AME for the high-school- equivalent and all-education robustness tests respectively. The left column shows AME of birth region, relative to US birth (including Puerto Rico, Guam, and the US Virgin Islands), on enlistment propensity for ages 17-26. All are insignificant. The right column reflects AME of immigration periods for naturalized citizens (treatment group), US citizens born abroad (control group), and US citizens born stateside via birth cohort (control group). Robust delta-method p-values + p<0.10, * p<0.05, ** p<0.01, *** p< 0.001.

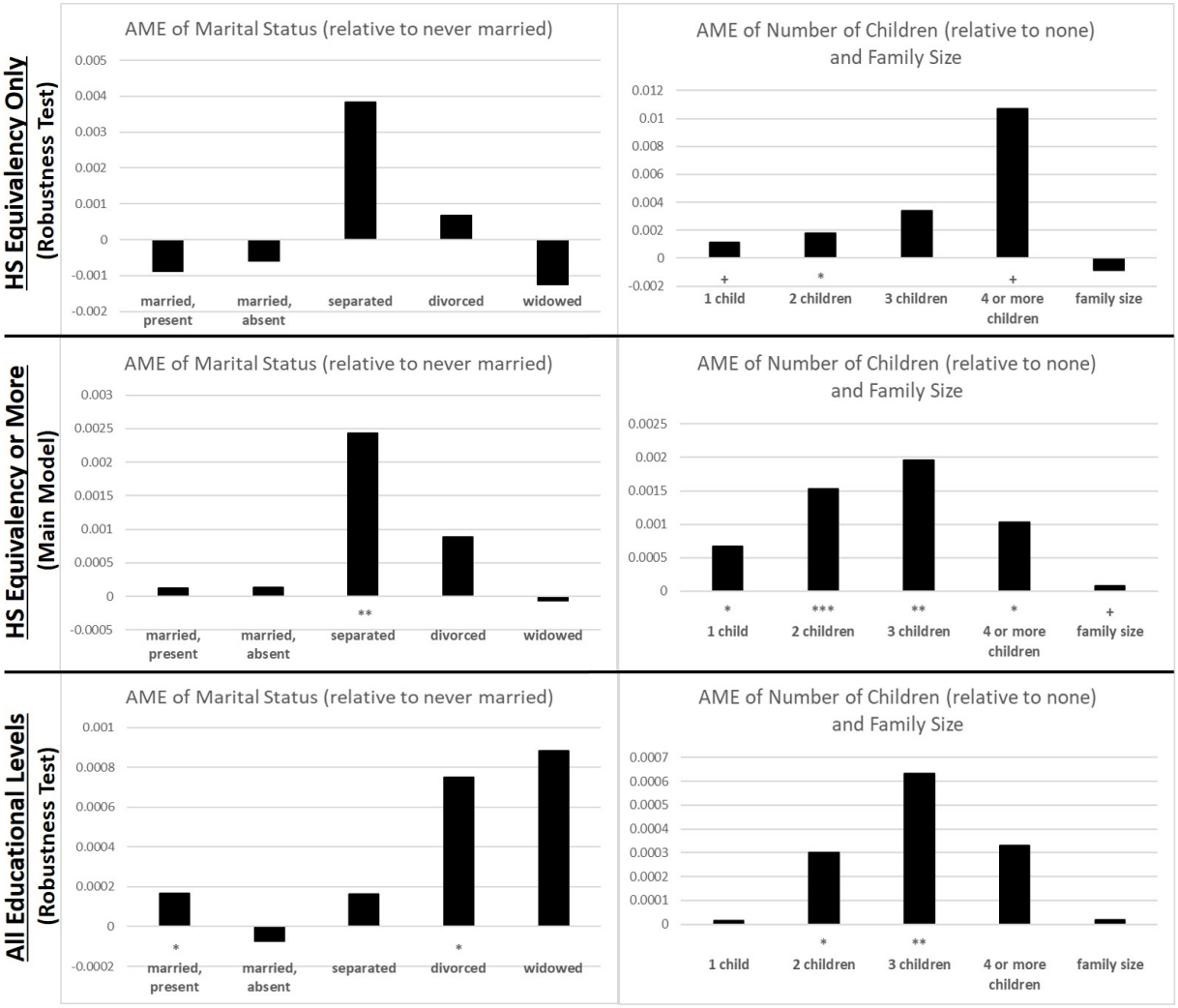

Figure 3b: Average Marginal Effects on Enlistment Propensity (2000 to 2007)

Note: The middle row depicts AME for the main model. The top and bottom rows reflect AME for the high-school- equivalent and all-education robustness tests respectively. All graphs reflect 17 to 26-year olds. Robust delta-method p-values + p<0.10, * p<0.05, ** p<0.01, *** p< 0.001.

Table 1: Federal Immigration Policies or Events from 2000 to 2007

Policy / Event	Dates in Effect	2000	2001				2002				2003				2004				2005				2006				2007			
			1	2	3	4	1	2	3	4	1	2	3	4	1	2	3	4	1	2	3	4	1	2	3	4	1	2	3	4
1	11-Sep-01 to present						Post-9/11																							
2	26-Oct-01 to present							USA Patriot Act																						
3	25-Nov-02 to present										Homeland Security Act (HSA)																		
4	01-Mar-03 to present										**US Immigration & Customs Enforcement - enacted**																		
5	24-Nov-03 to present														National Defense Authorization (NDA) Act														

Note: Policy (4) reflects the primary treatment effect and Policy (5) is a policy control. As a robustness test in Section 5.1, I add 9/11 (1), the Patriot Act (2), and the Homeland Security Act (3) as precursory-treatment effects. Green indicates an anticipated positive effect, yellow indicates an uncertain effect, and red indicates an anticipated negative effect of each event on enlistment propensity.

Table 2: Hypothesized Mechanisms with Expected Signs of Policies from 2000 to 2007

	Federal Policy or Event	Increased Risk of Combat Death	Flight to Safety (Deportment Fear for Family)	Institutional Trust or Patriotism
	INA 319, 320, & 322 (Average Marginal Effect)		+	+
1	Post-9/11	-	+ or -	+
2	Patriot Act (coincides w/ lagged post-9/11 period)	-	+ or -	-
3	Homeland Security Act (anticipation of ICE)		+ or -	+ or -
4	**Immigration & Customs Enforcement (ICE) enacted**		+ or -	+ or -
5	National Defense Authorization (NDA) Act	-	+ or -	+ or -

Note: The primary treatment effect is the enactment of ICE on March 1, 2003 (4), with one additional policy control (5). Events (1, 2, and 3) reflect precursory-treatment effects for a robustness test in Section 5.1. Green indicates an anticipated positive effect and red indicates an anticipated negative effect of an event on enlistment propensity. An additional potential mechanism is a disincentive for recruiters to accept naturalized enlistees relative to native-born US citizens given an extra step to verify foreign-educational certification with a local community college; however, these procedures are largely routine for most recruiting stations.

Table 3a: Sample Restrictions in Army-Administrative Data

Initial Sample of Enlistees	Drop	Sample
Original Sample (aged 17 to 42)		1,916,400
Drop if missing entry citizenship, sex, entry age, or entry-educ. level, or born in America Samoa, Fed States of Micronesia, Northern Mariana Islands, Palau, and Marshall Islands	100,700	1,815,700
Drop if noncitizen variables incongruent: LPR at entry cannot be a native-born citizen now	5,300	1,810,400
Drop if citizenship variables incongruent: a native-born citizen at entry cannot be a LPR or naturalized citizen now; naturalized at entry cannot be a native-born citizen or LPR now	3,500	1,806,900
Drop if enlistment date is before January 1, 2000	632,800	1,174,100
Drop if enlistment date is after December 31, 2007	669,100	505,000
Drop if enlistment dates occurred in September 2001	4,900	500,100
Drop if entry citizenship status is noncitizen (or LPR)	4,100	496,000
Drop if entry age is 27 to 42	34,000	462,000
Drop if entry civilian education is HS dropout, or current status listed as HS dropout	900	460,912

Note: The lower-right-hand number of individual, 17 to 26-year old observations reflects a high-school equivalent or more educated specification. I round all other numbers to the nearest 100 to safeguard the Army personnel data. I bundle and weight the data by sex, two-age group, enlistment quarter, and citizenship status from 2000 to 2007. Birth country is missing throughout the data so I drop those who list (i.e.) American Samoa as their birthplace, retaining some in the sample.

Table 3b: Sample Restrictions in Unweighted CPS-Population Data

Initial Sample Restrictions	Drop	Sample
Original sample (aged 17 to 42)		4,553,761
Drop if missing Hispanic ethnicity	30,146	4,523,615
Drop if born in American Samoa, Northern Mariana Islands, US outlying areas (ns), North America (ns), Americas (ns), the Marshall Islands, Micronesia, or Other (nec) and Unknown	27,800	4,495,815
Drops foreign-born individuals (not PR, GU, or VI) with no listed immigration year, infeasible immigration years given cohorts after 1957, or labeled as US born	199	4,495,616
Drops US native-born observations mislabeled as immigrating in a particular year, or mislabeled as born in US outlying, but born elsewhere	7	4,495,609
Drop if survey month is September 2001	50,019	4,445,590
Drop if citizenship status is noncitizen	445,388	4,000,202
Drop if age is 27 to 42	2,529,209	1,470,993
Drop if education is less than HS equivalency	371,875	1,099,118

Note: The lower-right-hand numbers of individual, 17 to 26-year old observations show a high-school equivalent or more educated specification. I bundle and weight the data by sex, two-age group, enlistment quarter, and citizenship status from 2000 to 2007.

Table 4: Descriptive Statistics for the CPS-Population Sample

Variables	**Mean**	**Std. Dev.**	**Min**	**Max**
Quarter	20038.7	22.5636	20001	20074
Naturalized	0.0238	0.1525	0	1
Female	0.5214	0.4995	0	1
Ages 17 to 18	0.0824	0.2750	0	1
Ages 19 to 20	0.2277	0.4194	0	1
Ages 21 to 22	0.2311	0.4215	0	1
Ages 23 to 24	0.2298	0.4207	0	1
Ages 25 to 26	0.2290	0.4202	0	1
High-School Equivalency	0.3885	0.4874	0	1
Some College	0.3862	0.4869	0	1
Associates Degree	0.0695	0.2543	0	1
College Graduate or More	0.1558	0.3627	0	1
White	0.8282	0.3772	0	1
Black	0.1066	0.3086	0	1
American Indian or Alaska Native	0.0132	0.1140	0	1
Asian or Pacific Islander	0.0349	0.1835	0	1
Other or More than One Race	0.0171	0.1298	0	1
Non-Hispanic	0.9103	0.2857	0	1
Mexican	0.0547	0.2274	0	1
Puerto Rican	0.0123	0.1100	0	1
Cuban	0.0027	0.0514	0	1
Dominican	0.0082	0.0900	0	1
Central or South American	0.0119	0.1084	0	1
Married, Spouse Present	0.1771	0.3817	0	1
Married, Spouse Absent	0.0063	0.0794	0	1
Separated	0.0101	0.1001	0	1
Divorced	0.0166	0.1278	0	1
Widowed	0.0009	0.0307	0	1
Never Married	0.7889	0.4081	0	1
Family Size	3.0270	1.6131	1	16
No Children	0.8200	0.3842	0	1
One Child	0.1086	0.3111	0	1
Two Children	0.0533	0.2246	0	1
Three Children	0.0145	0.1197	0	1
Four or More Children	0.0036	0.0602	0	1
Born in US (including PR, GU, or VI)	0.9662	0.1806	0	1
Born in Canada, Bermuda, or the Caribbean	0.0038	0.0615	0	1
Born in Mexico	0.0039	0.0620	0	1
Born in Central or South America	0.0039	0.0620	0	1
Born in Europe	0.0077	0.0877	0	1
Born in the Philippines	0.0026	0.0512	0	1
Born in Asia or Oceania	0.0123	0.1103	0	1
Born in Africa or the Middle East	0.0022	0.0468	0	1

Table 4 (cont.): Descriptive Statistics for the CPS-Population Sample

Variables	**Mean**	**Std. Dev.**	**Min**	**Max**
US Arrival from 1974 to 1979	0.2781	0.4481	0	1
US Arrival from 1980 to 1985	0.5738	0.4945	0	1
US Arrival from 1986 to 1991	0.1382	0.3451	0	1
US Arrival from 1992 to 1995	0.0048	0.0689	0	1
US Arrival from 1996 to 2007	0.0051	0.0716	0	1
Foreign-Born Birth Only (not in model):				
US Arrival from 1974 to 1979 (non-US, PR, GU, or VI)	0.0032	0.0563	0	1
US Arrival from 1980 to 1985 (non-US, PR, GU, or VI)	0.0102	0.1005	0	1
US Arrival from 1986 to 1991 (non-US, PR, GU, or VI)	0.0105	0.1017	0	1
US Arrival from 1992 to 1995 (non-US, PR, GU, or VI)	0.0048	0.0689	0	1
US Arrival from 1996 to 2007 (non-US, PR, GU, or VI)	0.0051	0.0716	0	1

The sample drops observations with less than a high-school equivalency. 'PR, GU, or VI' indicates Puerto Rico, Guam, or the US Virgin Islands. Each variable contains 1,099,118 observations.

Table 5: Covariate Balance across Treatment & Control Groups

	Native-Born Change			**Naturalized Change**			**Final Diff.**
Enlistment Propensity	Pre-Trmt (1)	Post-Trmt (2)	Diff. (2)-(1)	Pre-Trmt (3)	Post-Trmt (4)	Diff. (4)-(3)	(4-3)-(2-1)
	High-School Equivalency or More Education Main Model						
ICE Treatment	0.00034	0.00025	-0.00009	0.00022	0.00015	-0.00006+	0.00002
	(0.0006)	(0.0004)	(0.0011)	(0.0003)	(0.0003)	(0.0006)	(0.0015)
Cellular Observations	130	190	320	130	190	320	640
	All Educational Levels Robustness Test						
ICE Treatment	0.00016	0.00013	-0.00003+	0.00010	0.00008	-0.00002*	0.00001
	(0.0002)	(0.0001)	(0.0003)	(0.0001)	(0.0001)	(0.0002)	(0.0006)
Cellular Observations	130	190	320	130	190	320	640

Note: All specifications (aged 17 to 26) include a constant and indicators for naturalized and quarter effects. The treatment is the enactment of ICE on March 1, 2003 and the period is from 2000 to 2007. In the final column, the R-squared value for the high-school-equivalency or more education DinD coefficient is 0.028 and that of the all-educational-level coefficient is 0.050. Standard deviations are in parentheses. Robust p-values $^{+}$ $p < 0.10$, * $p < 0.05$. Villa (2016).

Table 6: Event Study for Enlistment Propensity from 2000 to 2007

	HS Equivalency or More Main Model	**HS Equivalency Only** Robustness Test	**All Educational Levels** Robustness Test
Variables	(1)	(2)	(3)
$Naturalized \times Q2\_2000$	-0.00015	0.00053	0.00000
	(0.389)	(0.386)	(0.959)
$Naturalized \times Q3\_2000$	-0.00020	-0.00004	-0.00007
	(0.279)	(0.918)	(0.329)
$Naturalized \times Q4\_2000$	-0.00005	0.00046	0.00002
	(0.679)	(0.324)	(0.577)
$Naturalized \times Q1\_2001$	-0.00017	-0.00018	0.00001
	(0.174)	(0.779)	(0.889)
$Naturalized \times Q2\_2001$	-0.00018	0.00009	0.00002
	(0.367)	(0.838)	(0.695)
$Naturalized \times Q3\_2001$	-0.00014	0.00017	0.00003
	(0.518)	(0.695)	(0.699)
$Naturalized \times Q4\_2001$	0.00004	0.00043	0.00001
	(0.774)	(0.441)	(0.791)
$Naturalized \times Q1\_2002$	-0.00005	-0.00029	0.00001
	(0.680)	(0.616)	(0.825)
$Naturalized \times Q2\_2002$	-0.00014	0.00000	0.00002
	(0.434)	(0.998)	(0.643)
$Naturalized \times Q3\_2002$	-0.00003	0.00044	0.00002
	(0.864)	(0.296)	(0.788)
$Naturalized \times Q4\_2002$	-0.00003	-0.00006	0.00004
	(0.831)	(0.905)	(0.386)
$Naturalized \times Q1\_2003$	-0.00006	0.00026	0.00001
	(0.662)	(0.589)	(0.819)
$Naturalized \times ICE$	0.00007	0.00072	0.00005
01Apr03_31Dec07	(0.491)	(0.140)	(0.153)
$Naturalized \times NDA$	**-0.00009+**	-0.00036	-0.00000
01Jan04_31Dec07	**(0.093)**	(0.151)	(0.972)
Cellular Observations	640	640	640
R-squared	0.684	0.371	0.722

Note: All studies include a constant and indicators for naturalized, quarter effects, sex, two-year age-group effects (for ages 17-26), birth-region effects, race, ethnicity, educational level, marital status, number of children, family size, and period of US-arrival effects. Columns 1-2 show coefficient estimates for a sample with a high-school equivalency or more. Columns 1a-2a show estimates from a sample with a high-school equivalency only. Columns 1b-2b show estimates with all educational levels. Robust p-values in parentheses: $^{+}$ $p < 0.10$, * $p < 0.05$.

Table 7a: Placebo Tests for Enlistment Propensity from 2000 to 2007

	High-School Equivalency or More Education Main Model					
Variables	(1)	(1)	(2)	(2)	(3)	(3)
Controls	No	Yes	No	Yes	No	Yes
$Naturalized \times 01Apr01\_31Mar03$	0.00004	-0.00013				
Placebo Test 1	(0.821)	(0.296)				
$Naturalized \times 01Oct01\_31Mar03$			0.00005	-0.00009		
Placebo Test 2			(0.782)	(0.439)		
$Naturalized \times 01Apr02\_31Mar03$					0.00003	-0.00016
Placebo Test 3					(0.870)	(0.226)
$Naturalized \times Q2\_2000$	0.00005	-0.00009	0.00005	-0.00010	0.00005	-0.00011
	(0.894)	(0.623)	(0.894)	(0.591)	(0.895)	(0.565)
$Naturalized \times Q3\_2000$	-0.00006	-0.00014	-0.00006	-0.00015	-0.00006	-0.00016
	(0.868)	(0.443)	(0.868)	(0.429)	(0.869)	(0.378)
$Naturalized \times Q4\_2000$	0.00007	0.00002	0.00007	0.00002	0.00007	0.00001
	(0.725)	(0.897)	(0.726)	(0.905)	(0.727)	(0.965)
$Naturalized \times Q1\_2001$	-0.00002	-0.00018	-0.00002	-0.00018	-0.00002	-0.00019
	(0.935)	(0.228)	(0.935)	(0.225)	(0.935)	(0.211)
$Naturalized \times Q2\_2001$			-0.00001	-0.00020	-0.00001	-0.00021
			(0.974)	(0.326)	(0.974)	(0.296)
$Naturalized \times Q3\_2001$			0.00004	-0.00021	0.00004	-0.00023
			(0.911)	(0.323)	(0.911)	(0.282)
$Naturalized \times Q4\_2001$					0.00016	0.00003
					(0.497)	(0.866)
$Naturalized \times Q1\_2002$					0.00003	-0.00006
					(0.886)	(0.702)
Cellular Observations	260	260	260	260	260	260
R-squared	0.047	0.721	0.047	0.722	0.048	0.724

Note: All specifications include a constant and indicators for naturalized and quarter effects. 'Controls' include sex, two-year age-group effects (for ages 17-26), birth-region effects, race, ethnicity, educational level, marital status, number of children, family size, and period of US-arrival effects. Robust p-values $^{+}$ $p < 0.10$, * $p < 0.05$.

Table 7b: Placebo Tests for Enlistment Propensity from 2000 to 2007

	HS Equivalency Only Robustness Test		**All Educational Levels** Robustness Test					
Variables	(1a)	(1a)	(1b)	(1b)	(2b)	(2b)	(3b)	(3b)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
$Naturalized \times 01Apr01\_31Mar03$			0.00003	0.00002				
Placebo Test 1b			(0.664)	(0.695)				
$Naturalized \times 01Oct01\_31Mar03$					0.00002	0.00002		
Placebo Test 2b					(0.694)	(0.696)		
$Naturalized \times 01Apr02\_31Mar03$	0.00001	0.00008					0.00003	0.00001
Placebo Test 1a/3b	(0.981)	(0.741)					(0.675)	(0.753)
$Naturalized \times Q2\_2000$	0.00067	**0.00071+**	0.00002	0.00002	0.00002	0.00002	0.00002	0.00002
	(0.367)	**(0.098)**	(0.745)	(0.735)	(0.746)	(0.741)	(0.747)	(0.741)
$Naturalized \times Q3\_2000$	-0.00004	0.00025	-0.00002	-0.00006	-0.00002	-0.00006	-0.00002	-0.00006
	(0.931)	(0.398)	(0.854)	(0.411)	(0.855)	(0.415)	(0.855)	(0.434)
$Naturalized \times Q4\_2000$	0.00011	0.00031	0.00003	0.00006	0.00003	0.00006	0.00003	0.00006
	(0.757)	(0.276)	(0.655)	(0.292)	(0.656)	(0.294)	(0.658)	(0.284)
$Naturalized \times Q1\_2001$	0.00013	0.00035	0.00000	0.00001	0.00000	0.00002	0.00000	0.00002
	(0.782)	(0.290)	(0.984)	(0.764)	(0.984)	(0.764)	(0.984)	(0.760)
$Naturalized \times Q2\_2001$	0.00025	0.00011			0.00002	0.00001	0.00002	0.00001
	(0.677)	(0.760)			(0.793)	(0.803)	(0.794)	(0.801)
$Naturalized \times Q3\_2001$	0.00020	0.00031			0.00005	0.00002	0.00005	0.00002
	(0.698)	(0.315)			(0.733)	(0.822)	(0.74)	(0.819)
$Naturalized \times Q4\_2001$	0.00038	0.00049					0.00002	0.00004
	(0.477)	(0.149)					(0.698)	(0.504)
$Naturalized \times Q1\_2002$	0.00010	0.00031					0.00001	0.00001
	(0.810)	(0.244)					(0.899)	(0.921)
Cellular Observations	260	260	260	260	260	260	260	260
R-squared	0.058	0.691	0.101	0.725	0.101	0.725	0.101	0.726

Note: All specifications include a constant and indicators for naturalized and quarter effects. 'Controls' include sex, two-year age-group effects (for ages 17-26), birth-region effects, race, ethnicity, educational level, marital status, number of children, family size, and period of US-arrival effects. Robust p-values $^{+}$ $p < 0.10$, * $p < 0.05$.

Table 8a: Results for Enlistment Propensity from 2000 to 2007

	HS Equivalency or More Main Model		**HS Equivalency Only** Robustness Test		**All Educational Levels** Robustness Test	
Variables	(1) ICE	(2) + NDA	(1a) ICE	(2a) + NDA	(1b) ICE	(2b) + NDA
Naturalized × *ICE*	**0.00009+**	**0.00015***	0.00031	0.00058	**0.00004***	**0.00004***
01Apr03_31Dec07	**(0.070)**	**(0.020)**	(0.287)	(0.162)	**(0.022)**	**(0.037)**
Naturalized × *NDA*		-0.00009		-0.00036		-0.00000
01Jan04_31Dec07		(0.104)		(0.148)		(0.901)
Naturalized	-0.00231	-0.00220	-0.00254	-0.00185	0.00105	0.00106
	(0.717)	(0.732)	(0.738)	(0.809)	(0.525)	(0.524)
Cellular Observations	640	640	640	640	640	640
R-squared	0.680	0.680	0.366	0.368	0.719	0.719

Note: All specifications include a constant and indicators for naturalized, quarter effects, sex, two-year age-group effects (from ages 17-26), birth-region effects, race, ethnicity, educational level, marital status, number of children, family size, and period of US-arrival effects. Columns 1-2 show coefficient estimates for my main model with a high-school equivalency or more education. Columns 1a-2a show estimates for a robustness test with high-school equivalents only. Columns 1b-2b show estimates for a robustness test with all educational levels. Robust p-values $^{+}$ $p < 0.10$, * $p < 0.05$.

Table 8b: Main Results with Gradual Controls from 2000 to 2007

	High-School Equivalency or More Education Main Model					
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Sex & Two-Year Age-Group Effects	No	Yes	Yes	Yes	Yes	Yes
Race & Hispanic	No	No	Yes	Yes	Yes	Yes
Marital St., # Kids, Family Size	No	No	No	Yes	Yes	Yes
Birth-Region & Arrival-Period Effects	No	No	No	No	Yes	Yes
Educational Level	No	No	No	No	No	Yes
Naturalized × *ICE*	0.00009	0.00009	**0.00014***	**0.00016***	**0.00017****	**0.00015***
01Apr03_31Dec07	(0.397)	(0.183)	**(0.037)**	**(0.015)**	**(0.009)**	**(0.020)**
Naturalized × *NDA*	-0.00008	-0.00008	-0.00009	**-0.00012***	-0.00008	-0.00009
01Jan04_31Dec07	(0.408)	(0.192)	(0.144)	**(0.049)**	(0.148)	(0.104)
Naturalized	**-0.00012***	**-0.00012****	**-0.00017+**	**-0.00017+**	-0.00219	-0.00220
	(0.038)	**(0.002)**	**(0.065)**	**(0.068)**	(0.747)	(0.732)
Cellular Observations	640	640	640	640	640	640
R-squared	0.062	0.588	0.598	0.629	0.659	0.680

Note: All specifications include a constant and indicators for naturalized and quarter effects. Two-year age-group effects range from ages 17 to 26. Robust p-values $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$.

Table 8c: Results for Enlistment Propensity from 2000 to 2007

Variables	**High-School Equivalency or More Education** Robustness Test using ICE as the 4th of 4 Treated Effects (1) 9/11	(2) + Patriot A.	(3) + HSA	(4) + ICE	(5) + NDA
$Naturalized \times Post\_911$ 01Oct01_31Dec07	0.00011 (0.110)	0.00016 (0.176)	0.00016 (0.171)	0.00016 (0.172)	0.00016 (0.172)
$Naturalized \times Patriot\_Act$ 01Jan02_31Dec07		-0.00006 (0.605)	-0.00010 (0.424)	-0.00010 (0.426)	-0.00010 (0.423)
$Naturalized \times HSA$ 01Jan03_31Dec07			0.00006 (0.332)		
$Naturalized \times ICE\_anticipated$ 01Jan03_31Mar03				0.00001 (0.933)	0.00000 (0.961)
$Naturalized \times ICE$ 01Apr03_31Dec07				0.00007 (0.284)	**0.00013+ (0.095)**
$Naturalized \times NDA$ 01Jan04_31Dec07					-0.00009 (0.107)
$Naturalized$	**-0.00271 (0.669)**	**-0.00265 (0.677)**	-0.00266 (0.676)	**-0.00279 (0.662)**	-0.00268 (0.677)
Cellular Observations	640	640	640	640	640
R-squared	0.680	0.680	0.681	0.682	0.682

Note: All specifications include a constant and indicators for naturalized, quarter effects, sex, two-year age-group effects (from ages 17-26), birth-region effects, race, ethnicity, marital status, number of children, family size, and period of US-arrival effects. Robust p-values $^{+}$ $p < 0.10$, * $p < 0.05$.

Table 9: Summarized Results from 2000 to 2007

		Estimated DinD Effects			Hypothesized Mechanisms		
	Federal Policy or Event	HS-Equiv. or More from 2000 to 2007	HS-Equiv. or More from 2000 to 2007 (Robustness Test)	All Educ. Levels from 2000 to 2007 (Robustness Test)	Increased Risk of Combat Death	Flight to Safety (Deportment Fear for Family)	Institutional Trust or Patriotism
	INA 319, 320, & 322 (AME)	-	-	+		+	+
1	Post-9/11		+		-	+ or -	+
2	USA Patriot Act		-		-	+ or -	-
3	Homeland Security Act		+			+ or -	+ or -
4	**ICE enacted**	**67.7% (*)**	**59.1% (+)**	**40.1% (*)**		**+ (or -)**	**+ (or -)**
5	National Defense Auth. Act	-	-	-	-	+ or -	+ or -

Note: The estimated results for the high-school-equivalency robustness test are all insignificant; although, a failed placebo test indicates that these estimates are not robust. Policy (4) reflects the primary treatment effect. Policies (1), (2), and (3) serve as precursory-treatment events as a robustness test in Section 5.1. Green indicates an estimated positive effect of the enactment of ICE on enlistment propensity, equating to a respective increase of 81, 75, and 58 naturalized enlistees per quarter. Black indicates policies that are not applicable to a particular specification or hypothesized mechanism. Statistically significant DinD estimates with robust p-values in parentheses: + p<0.10, * p<0.05, ** p<0.01, *** p< 0.001.

Chapter 3

Skin in the Game: Immigration-Policy Impacts on Job-Risk Selection by Noncitizen Enlistees

3.1 Summary

This paper estimates the impact of two federal-immigration policies on the job-risk selection of non-US-citizen enlistees from 1994 to 2007. I use Army administrative and Current Population Survey (CPS) data to estimate the propensity for noncitizen enlistees to choose relatively more or less risky Army occupations using a generalized difference-in-differences model. I exploit variation in unanticipated immigration-policy changes. I find that 17 to 26-year-old high-school equivalent noncitizens selected relatively safer military occupations after an asylum-to-green-card policy and selected riskier military jobs following a policy that granted automatic citizenship to green-card-holding children of naturalized citizens. I attribute these choices to a network effect from branches with greater non-white representation, fear of the impact of service-related death on noncitizen family members, and patriotism or role-model effects for children. Despite long-standing service-for-citizenship incentives, I find no evidence that noncitizen enlistees had different job-risk preferences than US-citizen enlistees on average.

3.2 Introduction

Of the 0.5% of Americans who volunteer to serve in the US Armed Forces, about 36.6% of those individuals enlist in the US Army.[1] Minimum qualifications to enlist include

[1]The active-duty Army comprises 0.18% of the population. Reynolds and Shendruck (2018).

being a US citizen or lawful-permanent resident (LPR), being at least 17-years old, having a high-school diploma, and passing a physical-medical exam.[23] Framed by the 'needs of the Army,' eligible enlistees choose their desired military occupations. Three key factors constrain the set of job options: threshold Armed Services Vocational Aptitude Battery (ASVAB) scores by-job, sex (for combat arms occupations prior to 2015), and citizenship status. The potential risk inherent to Army occupations depends largely on their peace and wartime missions. For example, riskier jobs entail direct engagement with enemy forces, like those serving in the Infantry, while relatively safer jobs support these combat forces, like logistical support soldiers serving in the Quartermaster Corps. It is likely that immigration policies affect the likelihoods of noncitizen enlistees to choose more or less-risky jobs relative to US citizens, driven by mechanisms unique to their citizenship statuses.

This paper is the first study to find immigration-policy impacts on the job-risk preferences of noncitizen Army enlistees relative to similar US-citizen enlistees. I use a general difference-in-differences (DinD) model to analyze the impact of two immigration-policy changes on the propensities of noncitizen enlistees to choose safer or riskier jobs, exploiting unanticipated policy changes as my sources of exogenous variation. This paper uses individual-level Army administrative data, appending basic monthly Current Population Survey (CPS) data collapsed into cells by sex, two-year age group, citizenship status, and survey quarter. I leverage service-eligibility criteria and isolate lawful-permanent residents (LPR) from the broader noncitizen population to build immigration controls.

I anticipate that enlistees will select riskier jobs given policies that affect their children and safer jobs given policies that impact their broader households. Over 1994 to 2007, I study the impact of a policy that granted green cards to asylum seekers on job-risk

[2]Enlistment into the modern all-volunteer force falls into one of two categories: enlisted-service members and officers, who complete a commissioning source like West Point or a Reserve Officers' Training Corps (ROTC) program.

[3]Each service permits a limited percentage of recruits with a General Education Development (GED) credential per year, but these cases require waivers and higher Armed Forces Qualification Test (AFQT) scores. Army recruits are typically 35-years old or younger, achieve a minimum AFQT score of 31, and have no more than two dependents.

selection of noncitizen enlistees relative to US-citizen enlistees. My model shows that 17 to 26-year-old noncitizen enlistees selected relatively safer Army occupations after the Haitian Refugee Immigration Fairness Act (HRIFA) – a late 1998 reform that granted LPR status to Haitian asylees. This 0.0786 percentage point (pp) shift, significant at the 0.1% level using a two-tailed test based on my DinD model, was likely motivated by network effects or fear for family members with precarious citizenship statuses.[4] With several African Americans serving in support branches, former Haitian asylees may anticipate better opportunities for mentoring and career progression in these fields. I also study the effect of a policy that granted automatic citizenship to children of US citizens after February 2001. I document that 17 to 26-year-old noncitizen enlistees chose riskier Army occupations after the Child Citizenship Act (CCA) at -0.0993 pp (significant at the 0.1% level), suggesting that patriotism or a role-model effect likely drove decision-makers. I find no evidence that service-for-citizenship incentives influenced noncitizen-enlistee job choices on average.

LPR enlistees are mindful of their families' citizenship statuses because the wait for naturalization can be precarious. Noncitizen soldiers must complete three years (now one) of peacetime-military service, or one day of wartime service to be eligible for service-based naturalization.[5] Different than US citizens, LPRs and their family members face inadmissibility constraints based on criminal acts, medical conditions, or potential welfare dependency. They can be deported if they commit crimes or violations, which can include an oversight like failing to register an updated address with US Citizenship and Immigration Services (USCIS). LPRs must reapply for their green cards every ten years for a fee and if they leave

[4]All subsequent statistical-significance figures for my DinD estimates reflect two-tailed tests based on my DinD model. I will use shorthand stating that DinD estimates are 'significant at the 0.1% level.'

[5]In general, LPRs wait a minimum of five years to apply for naturalization; although, exceptions include a three year wait via marriage, requiring residence with a US citizen or being a battered spouse of a US citizen. Other exceptions include automatic naturalization for children under 18 after February 2001 if at least one parent is a citizen and the child is a physically present LPR, in physical custody of the US parent (via the CCA); immediate application for spouses of certain government employees living overseas; all time as a refugee counting toward the five years, and one year of time as an asylee counting toward the five years. See https://www.nolo.com/legal-encyclopedia/when-apply-us-citizenship-46704.html.

the country for over a year, they can lose their green cards. They face federal benefit and job constraints and are ineligible to vote. Upon achieving naturalization, service members with alien spouses, children, or parents (in some cases) gain benefits that facilitate familial naturalization.[6][7]

This paper adds to immigrant-worker literature by analyzing job risk preferences of noncitizen enlistees. Research reports by Hattiangadi et al. (2005) and McIntosh et al. (2011) study the impact of citizenship on the attrition rates of LPR-service members, as well as naturalization rates by-service.[8] Some papers study the extrinsic choice of military enlistment versus civilian employment. Sandhoff (2013), Sullivan (2014), and Harvie (2014) conduct anecdotal and descriptive studies of respective Muslim, Latino, and Asian communities to examine service as a means to earn citizenship. Christensen (2017) notes that increased-wartime casualties induce fewer people to enlist. Many papers explore how monetary incentives impact accessions.[9] Recruiters use monetary incentives to encourage enlistees to choose certain in-demand jobs over others. My paper looks at the intrinsic selection of one job over another given that an individual has already chosen to enlist.

[6]The Immigration and Naturalization Act (INA) has provisions that grant citizenship benefits to the spouses, children, and parents of citizen military-service members. LPR-military spouses that file for naturalization under general or marriage provisions may count the time living abroad with their citizen service member toward residency and physical presence; furthermore, they may complete the naturalization process from overseas. INA Section 319(b) permits LPR-military spouses to forego residency and physical presence requirements given that they live in marital union with the citizen spouse, but they must conduct their naturalization interviews and oaths in the United States. LPR children of service members, aged 17 or younger, enjoy additional citizenship benefits under INA 320 and 322. Under INA 319(d), if a citizen active-duty member dies during his or her service, the surviving spouse, child, or parent may be eligible for naturalization as the surviving relative of the service member. The surviving spouse must have been legally married to the member at the time of the service-member's death.

[7]USCIS defines physical presence as the number of days that an applicant must physically be present in the United States during the required period until he or she files for naturalization.

[8]Zong and Batalova (2019) compile Census Bureau statistics on foreign-born veterans, while the Congressional Research Service outlines information on policies and topics that impact foreign-born soldiers. Congressional Research Service Report RL31884 on February 25, 2009 is an example.

[9]These studies include Goldberg and Warner (1982), Dertouzos (1985), Polich et al. (1986), Asch and Warner (1995), Payne et al. (2001), Hansen and Wenger (2002, 2005), Hosek and Totten (2002), Hogan et al. (2005), Simon and Warner (2007, 2009, and 2010), Hosek and Martorell (2009), Asch et al. (2010), and Simon et al. (2010).

My paper most closely aligns with Can and Yalcinkaya (2013), Cunha et al. (2014), and Himmelberger (2020a), which are empirical studies of the effect of a citizenship-for-service policy on noncitizen military accessions.[10] These papers use DinD models to examine the July 2002 impact of Executive Order 13269 on total noncitizen accessions (over the eligible population), using CPS and military-personnel data. Cunha et al. (2014) identify shifts of LPR enlistments out of combat-intensive services, like the Army, and into safer ones, like the Navy, due to this wartime service-for-citizenship incentive. My paper conducts a similar analysis, but it examines shifts in risk preference for specific jobs within one service (the Army). Himmelberger (2020a) uniquely studies the peacetime effect of an asylum-to-LPR policy on the enlistment propensity of service-eligible noncitizens; rather, my paper examines the intrinsic effect of such a policy on noncitizen military-job choices.

The paper proceeds as follows. Section 2 provides background on the military-service sections of the Immigration and Naturalization Act and immigration policies from 1994 to 2007. Section 3 discusses my data and provides summary statistics. I describe my empirical strategy in Section 4 and discuss my initial results in Section 5. Section 6 briefly concludes. For the remainder of the paper, I use the terms 'noncitizen' and 'green-card holder' to identify lawful-permanent residents (LPR), who were born abroad and are service eligible.[11] I use the terms 'naturalized' citizens to identify individuals who were born abroad, but are naturalized-US citizens and 'native-born' to identify US citizens who were born in the United States (including US territories) or to US parents abroad.

3.3 Institutional Background

Noncitizens historically can and have used military service to gain US citizenship. Himmelberger (2020a) shows that service-for-citizenship incentives increase the likelihood that noncitizens enlist; although, my study examines the intrinsic impact of federal-immigration

[10]Cunha et al. (2014) is an updated and published version of Can and Yalcinkaya (2013).

[11]Lawful-permanent residents are aliens admitted to the United States, according to USCIS. They are legally permitted to permanently reside in the United States.

policies on noncitizen-enlistee selection into more or less risky military professions, controlling for the impact of these incentives.

3.3.1 Immigration and Naturalization Act - Sections 328 and 329

The Immigration and Naturalization Act (INA) includes two categories of expedited service-for-citizenship eligibility, as well as sections outlining benefits for military dependents.[12] Section 328 affects peacetime naturalization and Section 329 applies to wartime naturalization, including 9/11 to the present.[13][14] Both sections require that an applicant serve honorably, support the US Constitution and lawful order in the United States, have a basic knowledge of US government and history, and be able to speak, read, and write basic English. Several eligibility requirements become less strict in wartime given the enactment of Section 329 – the most notably being a reduced period of qualifying service from three years in peacetime to one day in wartime.[15][16]

[12]INA Sections 319, 320, and 322 permit spouses and children of naturalized-citizen government employees to apply for naturalization with modified residence and physical presence requirements. See https://www.uscis.gov/military.

[13]Since July 1980, 18 to 25-year-old men are required to register for the modern Selective Service. US citizen, noncitizen, and dual-national men residing in the United States must register with the Selective Service. Registration is a prerequisite for naturalization. Exceptions include noncitizens present for less than a year, dual nationals with exemptions between their countries and the United States, and noncitizens who served a year or more in another country's military (with whom the United States shares a defense treaty). Bilateral treaties may exempt specific non-US citizens and dual nationals.

[14]Section 329 also enacted during World War I, World War II, the Korean War, the Vietnam War (February 28, 1961 to October 15, 1978), and the Persian Gulf War (August 2, 1990 to April 11, 1991). INA Section 329A covers posthumous naturalization in wartime.

[15]President Bush lowered the peacetime requirement to one year on November 24, 2003.

[16]Applicants must be at least 18-years old in peacetime, but there is no wartime age limit. Peacetime applicants must display good moral character for at least five years prior to filing until their naturalization, while the requirement becomes one year in wartime. An applicant must be a LPR at the time of peacetime examination, while in wartime, the applicant can be physically present in the United States or its outlying possessions at the time of military enlistment, reenlistment, or induction. Finally, applicants must meet residence and physical presence requirements in peacetime, while they are exempt from them in wartime. Physical presence is the number of days that an applicant must physically be present in the United States during the required period until he or she files for naturalization. See Public Law 108-136; USCIS Policy Manual / Vol. 12 / Part I / Ch. 2 & 3.

President George W. Bush enacted Section 329 via Executive Order 13269 on July 3, 2002, backdating it to 9/11 given historical precedent.[17] On November 24, 2003, the National Defense Authorization Act enhanced Section 329A with posthumous naturalization benefits for spouses, children, and parents of service members who die in combat. In October 2004, this law dropped federal naturalization fees for peace and wartime service members and allowed naturalization application from overseas. Apart from INA Sections 328 and 329, there were no other recruitment incentives for noncitizens relative to similar US citizens from 1994 to 2007.[18]

3.3.2 Non-Military Immigration Policy from 1994 to 2007

Table 1 outlines federal-immigration policies from 1994 to 2007. Immigration policy remained relatively constant following the Immigration Act of 1990. Congress passed the Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) on September 30, 1996, one month after the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) granted federal benefits to some noncitizen categories.[19] The enactment of IIRIRA on April 1, 1997 tightened restrictions on legal and illegal US immigration; however, these early policies had little impact on LPR-enlistee job selection.

The Nicaragua Adjustment and Central American Relief Act (NACARA) on November 19, 1997 targeted asylum seekers fleeing civil wars in Nicaragua, Cuba, El Salvador, Guatemala, and Eastern Europe. NACARA Section 202 gave LPR status to Nicaraguan

[17] My model depicts the Section 329 enactment starting on July 1, 2002. A previous retroactive enactment of Section 329 occurred on November 22, 1994 for Persian Gulf veterans via Executive Order 12939.

[18] Former Army recruiter Sergeant First Class (Retired) Gerald Isbell stated the following about this period: 'The Army offered no additional incentives for non-US citizens to enlist, even for translators. AFQT waivers were available for in-demand translators, accepting a slightly lower score. The AFQT was available for noncitizens in their native languages, but they would have to attend an English language course either before or after Basic Combat Training and Advanced Individual Training (vocational training for military occupations). Their initial terms of enlistment could not exceed eight years.'

[19] Enacted on August 22, 1996, the PRWORA offered federal benefits to some refugees and LPRs, as well as veterans and their families. I exclude the PRWORA and IIRIRA from Table 1.

and Cuban asylees until March 31, 2000 if they remained in the United States for five or more years since December 1, 1995. It remained an indirect asylum-to-LPR option after this deadline. NACARA Section 203 gave Salvadorian, Guatemalan, and former-Soviet asylees, who were previously denied LPR applications, an alternative method to achieve LPR status.[20] Two key policies followed NACARA: (1) the Haitian Refugee Immigration Fairness Act (HRIFA), which added Haitian asylum seekers to NACARA Section 202 on October 21, 1998, and (2) the Child Citizenship Act (CCA) of February 27, 2001, which granted automatic citizenship to children of naturalized or native-born US citizens if they were LPRs and less than 18-years old. These two policies comprise my treatment events, generating significant military-occupation shifts in Section 5.1.[21]

The Patriot Act increased governmental anti-terrorism and surveillance authority on October 26, 2001, increasing its power to monitor immigrants deemed as national-security threats.[22] The Homeland Security Act (HSA) established the Department of Homeland Security on November 25, 2002, reorganizing the former Immigration and Naturalization Service (INS) into three agencies on March 1, 2003: US Citizenship and Immigration Services (USCIS), US Immigration and Customs Enforcement (ICE), and US Customs and Border Protection (CBP). On November 24, 2003, The National Defense Authorization (NDA) Act expanded and funded the Iraq War with clauses increasing posthumous citizenship rights for family members and expanding Section 329 benefits in October 1, 2004.[23] The Great Recession, an expansion of Section 329 in 2008, and Deferred Action for Childhood Arrivals

[20]NACARA Section 202 and 203 of Public Law 105-100. According to Wikipedia, about 95% of Section 202 applicants were Nicaraguan and 5% were Cuban (of 70,356 people), and about 65% of Section 203 applicants were Salvadorian, 30% were Guatemalan, and 5% were former-Soviet Union asylees (of 160,102 people) from 1998 to 2007.

[21]I use all subsequent policies as controls due to the small size of the noncitizen-enlistee population and forthcoming delicate DinD estimates in Section 5.1.

[22]I use quarterly-enlistment periods to differentiate the post-9/11 period from the Patriot Act.

[23]This expansion of Section 329 includes no federal service-member fees for filing naturalization petitions or gaining naturalization certification and their ability to apply, interview, file, and conduct other naturalization proceedings from US embassies, consulates, and US military installations overseas.

(DACA) in 2012 make 2007 a good year to end this study.[24]

Table 2 displays and aligns each immigration policy to anticipated mechanisms that could affect the job-risk preferences of noncitizen enlistees. If network effects (or familial legacies of service) drive choices, I anticipate an indeterminate-ex ante sign, as noncitizens will select jobs recommended by their cultural or familial networks. If the fear of service-related death hurting ones family motivates noncitizen job choices, LPRs will select a safer military occupation. If a policy increases patriotism or inspires a role-model effect for children, I anticipate a likely shift toward riskier jobs; although, individuals show patriotism by selecting relatively safer jobs as well. Cunha et al. (2014) suggest that if noncitizens were to enlist at lower rates than US citizens, they could have a lower risk threshold for combat death. Himmelberger (2020a) finds minimal evidence of this extrinsic effect, but an intrinsic wartime shift by noncitizen enlistees into less risky jobs relative to US citizens is feasible. It is more likely that enlistees will select riskier jobs in response to policies focused on their children, like the Child Citizenship Act, and safer jobs given policies that impact their broader households.

3.4 Data

The data are comprised of basic monthly CPS (from January 1994 to December 2007) and Army administrative data, including individual-level records from all enlistees who entered the Army from 1994 to 2007.[25] The individual-level Army data reflect exact dates of active-duty Army enlistment, entry citizenship designations, ability-test scores, and entry military occupations. Soldiers appear once in the Army administrative data; if they reenlisted, I count them according to their initial enlistment dates so I never see the same

[24] In 2008, the Department of Defense launched Military Accessions Vital to the National Interest (MAVNI) as a trial recruitment program under Section 329 to fill critical shortage occupations, like surgeons and foreign nationals fluent in strategic foreign languages. This policy allowed refugees and asylee enlistment while they awaited their green cards, but it became highly-contested in litigation when some participants failed their background checks. See Bowman (2017) and Horton (2017).

[25] I used IPUMS CPS to access the Census data and a Total Army Personnel Database extract on site at the Office of Economic Manpower Analysis (OEMA) at West Point, NY.

person repeatedly. The CPS contains self-identified citizenship designations, birth countries, and periods of US arrival, which I bundle into representative cells and use to generate controls for birth region and US-arrival periods.

3.4.1 Descriptive Statistics and Data Appending

I use individual-level Army administrative data on all enlistees for my dependent and control variables; however, the Army data lacks variables on birth region and US-arrival period so I use service-eligible population data as a supplement. The basic monthly CPS data facilitate controls segmented by quarter (in part) to account for close immigration-policy changes over time; thus, I assume that the CPS population is representative of the population of enlistees. The CPS excludes active-duty military members from its survey of 60,000 households so there is no danger of double-counting the same enlistee or including one serving in the active-duty Air Force, Navy, Marine Corps, or Coast Guard.

Tables 3a and 3b list sample restrictions by identical criteria, which describe the Army administrative and unweighted-CPS data respectively.[26] I drop observations lacking key variables, including sex, entry age, entry citizenship status, or entry educational level. I exclude individuals born in countries that have unique citizenship categorizations and those where immigration year or current citizenship do not correspond with age or entry citizenship respectively. The sample contains soldiers that enlisted from 1994 to 2007, dropping September 2001 and those older than 26.[27] I only keep observations with a high-school equivalency, including high-school graduates and GED-holders in the Army administrative data.[28] Finally, I drop observations without an entry career branch or those serving in career

[26] I round each restriction in Table 3a to the nearest 100 to safeguard the immigrant-enlistee data.

[27] I eliminate dates from September 2001 given the confounding events surrounding 9/11 and individuals older than 26 given that about 94% of Army enlistees are 17 to 26-years old.

[28] At present, I only have results for high-school equivalents. I want to return to the Army administrative data to run the model on a more educationally diverse sample.

branches that are not open to LPR enlistees (detailed in Section 3.3).[29]

Figures 1a to 1f compare individual characteristics in the Army-administrative data (left columns) to the weighted-CPS population for noncitizens and US citizens aged 17 to 26 (right columns) from 1994 to 2007.[30] Figure 1a depicts a decline in noncitizen enlistment from about 1999 to 2007; meanwhile, the similar service-eligible population increased.[31] US-citizen enlistment over this period remained relatively steady. Figure 1b shows that the average noncitizen enlistee was slightly older than a US citizen enlistee. Figure 1c illustrates that 91% of enlistees had a high-school equivalency, while the similar population had more education; thus, I only retain high-school equivalents to isolate the CPS-population most likely to enlist for my birth-region and arrival-period control variables.

Figure 1d shows the percentage of enlistees based on sex and racial demographics from 1994 to 2007. About 18.4% of the active-duty Army were women. Whites comprised 66.6% of the active-duty Army, while blacks, Hispanics, and other racial groups made up 18.9%, 9.7%, and 4.7% respectively. Blacks (especially women) enlisted more than their population ratios; although, white-female US citizens and Hispanic noncitizens enlisted less than their population ratios. Figure 1e depicts marital percentages of Army enlistees and the CPS population, showing that enlistees were 10% more likely than the population to remain single. Finally, Figure 1f shows contract length percentages for noncitizen versus US-citizen enlistees, showing that most noncitizens enlisted for three years while most US citizens enlisted for four during this period.[32]

[29]The omitted career-branch categories include Air Defense Artillery (ADA), Civil Affairs (CA), Military Intelligence Corps (MI), Military Police Corps (MP), Public Affairs (PAO), Psychological Operations (PO), Signal Corps (SC), Special Forces (SF), Warrant Officer Candidates (WC), and a non-branch category identified as BI (only containing US citizens).

[30]These figures depict a broader enlistee population than the sample in Table 3a (prior to all educational and career-branch restrictions). The noncitizen enlistee counts are close estimates to my tighter sample, but the US citizen counts include military occupations in career branches that are off-limits to noncitizens.

[31]I attribute this difference to the net negative noncitizen-enlistment impact of the Patriot Act relative to the positive impacts of 9/11 and Section 329 identified in Himmelberger (2020a).

[32]Most active-duty Army jobs require a four-year contract, but there are also two, three, five, and six-year options. Two and three-year contracts are for jobs requiring less training time. There

Over 93% of enlistees are 17 to 26-years old, narrowing the service-eligible population in the CPS data. I create five two-year age groups, starting with age 17 and ending with 26, to create a high-school-graduating group of 17 to 18-year olds given that 94% of 17-year olds have yet to complete a high-school equivalency.[33] This assumes that there are no significant differences between the military-occupational and enlistment choices of 17 and 18-year olds. Ninety or less noncitizen enlistees were high-school dropouts from 1994 to 1998 and five or less were dropouts from 1999 to 2007 (my pre and post-treatment periods); thus, I drop observations with less than a high-school equivalency to prevent the assignment of population-level probabilities to control variables that should be empty.[34]

Table 4 shows summary statistics for the CPS sample (depicted in Table 3b) for my forthcoming birth region and US-arrival-period controls.[35] I create counts of weighted-CPS demographic variables for birth region and period-of-US arrival, disaggregated by sex, two-year age group, citizenship status, and survey quarter, which serve as the numerators of my control variables. Similar counts of the weighted-CPS populations, divided by the same four categories, serve as the denominators of these controls. I weight and collapse the individual-level CPS data into cells, sorting the counts of weighted-CPS demographic controls and

are a handful of individuals that claim a one-year contract, which are either exceptions or errant entries; I bundle these with the two-year contracts. There are also a few US citizens that claim more than six-year contracts, which may include their post-active-duty commitment in the inactive reserves. For example, an individual who serves in the active-duty for four years usually incurs an additional four-year requirement in the inactive reserves; thus, if the nation goes to war, he or she could be recalled to active-duty service during this time. https://www.acteonline.org/what-the-recruiter-never-told-you/.

[33]I drop non-17-year olds from the CPS sample from Table 3b, Row 7 (2,771,812), leaving 319,738. 18,773 had a high-school equivalency or more education. 18,773 / 319,738 = 5.9%. 6% of enlistees are 17-years old.

[34]By exception, high-school dropouts and GED holders cannot surpass 10% of annual Army enlistment; thus, under 1% of active-duty enlistees are high-school dropouts. Recruiter limitations on enlisting high-school dropouts remained unchanged from 1994 to 2007. A 2005 policy increased the threshold for GED waivers; however, I account for this by isolating the high-school-equivalent population in the CPS and using separate variables for high-school graduates and GED holders in the Army data. Discussion with Luke Gallagher, data manager at OEMA, on or about January 15, 2020.

[35]The sample is 7.7% comprised of noncitizens, 48.3% are women, and the average age is 21.7. It is not yet determined if and how I can display similar summary statistics for the Army administrative data.

populations by sex, two-year age group, citizenship status, and quarter of survey.[36] I divide the count variables by the respective populations to generate representative percentages of each group. Finally, I match these two sets of controls with the Army administrative data based on the sex, two-year age group, citizenship status, and enlistment quarter of each individual enlistee.

3.4.2 LPR versus US-Citizen Criteria

My treatment and control groups are LPR and US-citizen enlistees, where an asylum-to-LPR should only influence noncitizen military-occupational choices. Similarly, a policy that automatically confers citizenship for children of US citizen parents should only affect the occupational choices of LPRs, given concurrent citizenship-for-service policies. The indicator variable $Noncitizen_i$ equals one for all LPR enlistees (my treatment group), and zero for naturalized and native-born citizen enlistees (my control group).[37] While residents of the American Samoas, Northern Mariana Islands, Marshall Islands, and other US Pacific territories have several rights similar to those of US citizens, their citizenship categorizations are inconsistent across birth cohorts or locations; thus, I exclude these observations (dropping 0.02% of the Army administrative sample and 0.58% of the unweighted-CPS sample for the two immigration controls).[38]

The citizenship designations in the Army administrative data are accurate (absent data-entry errors) given recruiter confirmation of enlistees' citizenship documentation; however, I accept classification bias since I add two sets of 'immigration controls' from the

[36] IPUMS-CPS recommends using weights that reflect the 'population represented by each individual in the sample' for person-level analyses.

[37] The US State Department categorizes children born to US citizens abroad, Guam residents born after April 11, 1899, Puerto Ricans after April 10, 1899, and US Virgin Island residents born after January 16, 1917 as native-born citizens. See the USCIS Policy Manual, Volume 12, Part A, Chapter 2.

[38] American Samoans are considered US nationals. Post-1986 birth cohorts from the Northern Mariana Islands are considered US citizens, with ambiguous status for older individuals; thus, I drop these groups. I drop observations from additional ambiguous birth locations, including the US Outlying Areas (not specific), North America or Americas (not specific), Marshall Islands, Micronesia, and Other or Unknown.

CPS data and service-ineligible non-LPRs remain in its noncitizen population. I control for undocumented immigrants in the CPS data by dropping enlistees with less than a high-school equivalency. Hattiangadi et al. (2005), Cunha et al. (2014), and Himmelberger (2020a, 2020b) use this technique (in part) to isolate the service-eligible population.[39][40] Following Himmelberger (2020a), I further drop 2.5% of individuals with a college or graduate degree to exclude H-1B visa holders and 5.4% of some college and associates-degree holders to eliminate student-visa holders from the CPS-noncitizen population. I retain some bias given remaining high-school-equivalent non-LPR immigrants in the CPS-noncitizen data.[41]

There are further sources of potential bias introduced by recruiters and the 'immigration controls' due to an imprecise service-eligible population. McIntosh et al. (2011) note that recruiters incur three additional steps to enlist a noncitizen.[42] While one recruiter described these steps as routine and not burdensome, McIntosh et al. (2011) find that some recruiters view these steps as additional work; thus, these extra steps could increase the cost for recruiters to enlist a noncitizen over a similar US citizen, making it feasible that an

[39]Devadoss et al. (2019) use high-school equivalency to differentiate between high and low-skilled Mexicans likely to be migrant-US workers.

[40]Several other techniques distill the undocumented immigrant population. Passel and Cohn (2009, 2010) and Amuedo-Dorantes and Antman (2016, 2017) identify undocumented immigrants using Hispanic ethnicity and Mexican origin, but 36.9% of immigrant enlistees are Hispanic. Borjas (2017) recommends a residual method where he classifies foreign-born individuals as legal if they arrived before 1980, are citizens, receive federal benefits, are veterans, work in the government sector, were born in Cuba, or have legal immigrant or citizen spouses; all others are undocumented. Garcia-Perez (2019) uses a similar technique. Capps et al. (2018) use a logic imputation method of labeling military, public-sector workers, and public-assistance recipients as LPRs and visa holders by specific circumstances like high-school completion. Bachmeier et al. (2014) isolate H-1B visa holders via countries with high shares of non-immigrant visas.

[41]Additionally, Brown et al. (2018) compare the citizenship data in self-reported survey responses to administrative records, finding a lower estimated share of the noncitizen population than administrative records substantiate. Self-reported naturalized citizens could be noncitizens given survey shortcomings.

[42]1) Recruiters must submit a waiver to verify proof of a recruit's Permanent Residence Cards. If they lack this documentation, recruits must send a form to USCIS to formally request verification. 2) LPRs must demonstrate that they are sufficiently proficient in English to meet Military Entrance Processing Station (MEPS) requirements. 3) Recruits must show translated copies of any foreign-educational credentials, which recruiters verify with a local community college or accrediting source.

average noncitizen enlistee may be more qualified than a US-citizen enlistee.[43] The ability of a recruit determines which occupations are open to them given minimum ASVAB-score requirements for each job. I control for AFQT scores in the Army data, but do not control for them in the service-eligible CPS population comprising the 'immigration controls,' nor English proficiency, medical background, or criminal history.

3.4.3 Less Versus More-Risky Job Criteria

An enlistee must meet a minimum ASVAB score to be eligible for each Military Occupational Specialty (MOS).[44] The ASVAB includes several sub-tests that rate a soldier's vocational aptitude in various skills required for each job, including arithmatic reasoning, electronics knowledge, and part-assembly aptitude. More technically-intricate jobs require higher ASVAB scores. I include a control for the AFQT score in my forthcoming model, which is a composite of the ASVAB scores. Additional restrictions constrained the set of jobs open to enlistees from 1994 to 2007. Women were excluded from combat-arms jobs, except for those in Aviation, Air Defense Artillery, and a few Field Artillery positions. Similarly, noncitizens could not select jobs that required security clearances. Finally, the 'needs of the Army' largely dictated the military occupations that recruiters sought this period, such as an increased push for Infantry recruitment in wartime. Recruitment incentives align with current or foreseeable personnel shortages by MOS.

There are 17 career branches that comprise these Army occupations, which fall under three main categories. 1) Combat Arms Branches include Infantry, Armor, Field Artillery*, Aviation*, *Air Defense Artillery*, *Special Forces*, and the Corps of Engineers. 2) Combat Support branches include the Corps of Engineers*, Chemical Corps, Military Police Corps*, *Military Intelligence Corps*, and *Signal Corps*. 3) Combat-Service Support Branches include the

[43]Further, foreign-educated enlistees may hold more educational credentials than they are able to verify.

[44]I use the terms MOS, job, and military occupation interchangeably.

Ordnance Corps*, Transportation Corps, Quartermaster Corps, Finance Corps, and Adjutant General's Corps*. Additionally, there are Specialty Branches, including the Medical Service Corps, Chaplain Corps, and Judge Advocate General Corps. The preceding italicized branches are not open to LPRs. Branches designated by an asterisk have select jobs not open to LPRs.

My dependent variable sorts the riskiness of Army jobs into a less and more risky category given occupations that were open to LPR enlistees from 1994 to 2007. Column (2) of Table 5 depicts the indicator variable Low_Risk_i, which equals one for military jobs that were accessible to noncitizens in the combat-service-support or specialty branches (excluding the Ordnance Corps). Low_Risk_i equals zero for military jobs in the combat-arms, combat-support, or Ordinance Corps branches that were open to noncitizen enlistees.[45] I omit all career branches that were off-limits to noncitizen enlistees due to security clearance requirements, including Air Defense Artillery (ADA), Civil Affairs (CA), the Military Intelligence Corps (MI), the Military Police Corps (MP), Public Affairs (PAO), Psychological Operations (PO), the Signal Corps (SC), and Special Forces (SF).

Sorting military occupations into high versus low-risk categories is challenging because the risk of each job (or career branch) changes over time. Some jobs have very different peace versus wartime missions. For example, soldiers in Military Intelligence jobs largely manage unit-security clearances in peacetime; however, the post-9/11 mission of these soldiers shifts to analysis and anticipation of enemy activities in support of all combat-arms branches. Similarly, the Transportation Corps became one of the more dangerous career branches after 9/11

[45]My dependent variable currently suffers from bias because Field Artillery, Aviation, the Corps of Engineers, the Ordnance Corps, and the Adjutant General's Corps contain some jobs that were not open to noncitizen enlistees. I must drop the jobs that were off-limits to noncitizens and sort these branches into the dependent variable by-job, or MOS. I list a count of the job restrictions in Column (4) of Table 5, which currently remain in the Army administrative data. I dropped the Military Police Corps, but there was one job open to noncitizens that I could retain. I will run a revised dependent variable to clarify risk according to Army doctrine, depicted in Column (5) (Combat Arms vs. all other branches).

since traveling on roads in Iraq and Afghanistan became highly dangerous due to improvised explosive devices (IEDs).[46]

3.5 Empirical Strategy

I use repeat cross-sectional individual-level data to estimate a generalized DinD model with robust standard errors to limit heteroskedasticity. My counter-factual scenario is how noncitizen enlistees would have selected lower-risk branches over higher-risk ones relative to US-citizen enlistees absent two immigration policies: 1) the HRIFA in late 1998 and 2) CCA in early 2001. Event studies confirm the common-trends assumption.

3.5.1 Linear Model

The model follows, where i signifies an individual and t signifies a given period:

$$Low\_Risk_{it} = \beta_1 I(i = Noncitizen) \times Post\_HRIFA_t + \beta_2 I(i = Noncitizen) \times Post\_CCA_t + \beta_3 I(i = Noncitizen) + \psi_t + \beta_4 \mathbf{X}_{it} + \beta_0 + \epsilon_{it},$$

where Low_Risk_{it} is the likelihood of selection into a non-risky occupation relative to a risky one. The coefficients ψ_t estimate quarterly-time effects relative to the omitted First Quarter 1994 (Q1 1994). $Noncitzen_i$ indicates whether an individual is a noncitizen, equaling one and zero otherwise. The interaction-variable $Noncitizen_i \times Post\_HRIFA_t$ is a binary treatment effect, equaling one if the individual is a noncitizen enlistee from January 1, 1999 to December 31, 2007 and zero otherwise. The interaction-variable $Noncitizen_i \times Post\_CCA_t$ is a binary treatment effect, equaling one if the individual is a noncitizen enlistee from April 1, 2001 to December 31, 2007 and zero otherwise. The addition of seven policy controls, such as $Noncitizen_i \times Post\_ICE_t$, tests the robustness of my DinD-coefficient estimates.

[46] I need to incorporate a control that determines the annual number of combat deaths per branch. Alternate future dependent variables, like including the Transportation Corps in the risky-jobs category, will help tease out nuanced differences over time.

The $\mathbf{X}_{it}$ coefficient contains explanatory variables that impact the likelihood of less-risky job selection. These variables include sex, two-year age group, race (including Hispanic ethnicity), entry civilian education, entry AFQT score, entry marital status, and number of dependents at entry, included in the Army administrative data. I derive two additional controls for birth region and US-arrival period from weighted-CPS counts over the weighted-CPS population, assigning each individual a representative likelihood based on his or her sex, two-year age group, citizenship status, and enlistment quarter. Civilian education in the Army administrative data align with the CPS educational categories.[47]

Birth-region and US-arrival-period controls account for fluctuations in the immigrant population from 1994 to 2007, instituting consistency across my treatment and control populations. I compare birth regions across treatment and control groups via the naturalized population and about 1.5% of native-born US citizens born abroad. US-arrival-period variables control for the temporal impact of physical presence in the United States on enlistees' career-branch choices.[48] I use quarterly-time effects to control for national changes, including changes in the riskiness of military occupations over time. The US citizen control group captures changes in military-occupation recruitment over time.

3.5.2 Assumptions

A DinD model has two identifying assumptions. First, the treatment must be exogenous of noncitizen versus US citizen propensities to select lower-risk Army occupations. I leverage two unanticipated immigration policies to generate exogenous variation – the

[47]The Army Total Personnel Database includes the following educational categories: 'high-school dropout;' 'GED' (including GED, home school, National Youth Challenge, distance learning, high-school certificate of attendance, and other non-traditional credentials); 'high-school graduate' (including high-school graduates, high-school seniors, currently in high school, adult-education diploma, and Job Corps); 'some college' (including currently enrolled or completed 15 semester hours or greater); 'associates degree' (including associates or professional nursing degrees); 'college' for a college graduate; and 'graduate' for a graduate degree. There are no high-school seniors or current high-schoolers in the Army data.

[48]The CPS year-of-immigration variable changed over this period, preventing a singular control for recent arrival. I assign native-born citizens US-arrival periods based on their birth cohorts (including those born in Puerto Rico, Guam, and the US Virgin Islands).

HRIFA and CCA; as well, the NACARA asylum-to-LPR policy was largely unanticipated, enabling my use of these policies as sources of exogenous variation in job-risk selection of Army occupation for my forthcoming model and a robustness test.

Second, lower versus higher-risk job selection should remain the same for noncitizens and US citizens in the counterfactual scenario where the establishment of ICE never occurred. I assume that no pre-treatment trends indicate that common trends would have remained throughout the post-treatment period in the absence of the HRIFA in late 1998 and CCA in early 2001. I conduct an event study for significant pre-trends in job-risk propensity for my treatment and control groups, adding noncitizen-quarterly interaction terms to my DinD model rather than treatment effects. Angrist and Pischke (2009) note that pre-treatment estimates should be statistically insignificant and estimated post-treatment estimates should remain unchanged.

The event study generalizes the model as follows:

$$Low\_Risk_{it} = \theta_1 I(i = Noncitizen) \times \psi_t + \theta_2 I(i = Noncitizen) + \psi_t + \theta_3 \mathbf{X}_{it} + \theta_0 + \xi_{it},$$

The coefficients ψ_t estimate quarterly-time effects relative to omitted the Fourth Quarter 1998 (Q4 1998) for normalization. I run DinD models with robust standard errors, denoting the significances of all pre-treatment effects. Table 6 reflects the quarterly DinD point estimates of dual-aged 17 to 26-year old noncitizens (relative to US citizens) from 1994 to 2007, adding incremental controls to highlight coefficient stability.[49] The high-school equivalent specification shows insignificant pre-trends across Columns (1) through (6), with pre-treatment periods depicted above the dotted line. Figure 2 plots all $Noncitizen_i \times \psi_t$ lower-risk-job propensity coefficients with a high-school equivalency from 1994 to 2007.[50] The trends remain steady and insignificant before the treatment events. The post-treatment trends initially become significant in Q1 2000. Several post-treatment trends are insignifi-

[49]The event studies in Table 6, as well as Figure 2, depict Q1 1994 omitted for colinearity.

[50]Given p-values and having used robust standard errors, I must return to the Army administrative data to depict confidence intervals in Figure 2.

cant since quarterly-point estimates may not generate enough power to observe statistically significant trends.

3.6 Preliminary Results

I interpret estimates for the effect of federal-immigration policies on job-risk choice by noncitizen enlistees from 1994 to 2007 via a robustness-test specification. Additionally, I estimate average marginal effects of my controls on job-risk selection.

3.6.1 Preliminary Results for 1994 to 2007

Table 7 shows job-risk-likelihood estimates for noncitizen versus US-citizen 17 to 26-year-old enlistees with a high-school equivalency, showing a robust specification of three precursory treatment events in Column (1), my first (intended) treatment effect HRIFA in Column (2), and my second (intended) treatment effect CCA in Column (3).[51] Columns (4) through (6) reflect the addition of several policy controls to confirm the stability of the HRIFA and CCA coefficients.

I run a robust specification where I add (1) two immigration policies from Q3 1996, (2) the enactment of IIRIRA on April 1, 1997, and (3) NACARA as three treatment effects prior to and including HRIFA and the CCA (Policy 1 in Table 1 – the first two are not listed). Row (4), Column (6) of Table 7 shows the coefficient estimate for the enactment of the HRIFA, which remains steady at 0.0786 pp, significant at the 0.1% level.[52] This estimate indicates a shift in noncitizen enlistee career selection toward safer jobs after this Haitian asylee-to-LPR policy. It is likely that network effects motivated these choices, as career branches like Quartermaster or Transportation Corps have a high proportion of black

[51] Once I re-access the Army administrative data, I will run the main model with just the two treatment effects and policy controls. I will re-run this robustness test, solely adding NACARA as a precursory treatment event. I currently do not have results starting with HRIFA as my first treatment. This treatment may change once I modify my dependent variable.

[52] Column (6) of Table 6 shows no significant pre-treatment trends from Q2 1994 to Q3 1996.

relative to white soldiers.[53] An alternate (or concurrent) mechanism may be a fear for family given a higher chance of combat death in riskier jobs if some family members have yet to apply for naturalization.

Row (5), Column (6) of Table 7 displays the coefficient estimate for the enactment of the CCA at -0.0993 pp, significant at the 0.1% level. This estimate reflects noncitizen-enlistee selection toward riskier jobs, suggesting that patriotism or a role-model effect to inspire family members likely drove decision-makers. It is less likely that network effects or familial legacies encouraged individuals to join riskier jobs given that this policy only impacts those with children. The policy control coefficient for the enactment of ICE provides weak evidence in support of this argument, reflecting -0.130 pp, significant at 10%. Both of these family oriented policies result in a shift toward riskier jobs.

3.6.2 Average Marginal Effects of Explanatory Variables

Figure 3 depicts relevant average marginal effects (AME) of my explanatory variables on the job-risk propensities of 17 to 26-year old high-school equivalents from 1994 to 2007. Noncitizen enlistee job-risk propensity (not depicted) was insignificant relative to that of US-citizen enlistees, showing no impact of the peacetime citizenship-for-service policy. The top-center graph shows that enlistees of color were more likely to choose less-risky jobs relative to white enlistees. Black, Hispanic, and other raced enlistees were respectively 22.4%, 8.1%, and 6.9% more likely to choose lower-risk jobs than their white counterparts, significant at the 0.001 level. Mechanisms likely driving these choices are network effects, given that branches like logistics have historically been comprised of many non-white soldiers, fostering opportunities for mentorship and leadership in those career branches. The middle-left graph shows that individuals born in Puerto Rico, Guam, or the US Virgin Islands were 116.9% more likely to choose riskier jobs than those born in the 50-US states, significant at the 0.001 level. Individuals born in Africa were 31.8% more likely to choose riskier jobs than

[53]This is a statistic that I need to go back and retrieve from the Army administrative data.

those born in the United States, significant at the 0.10 level.

The middle-right graph of Figure 3 shows the AME of immigrant waves after 1979. The latter two immigration periods comprised entirely of immigrants were insignificant, reflecting assimilation. The bottom row reflects AME of marital status and number of dependents. Separated individuals were 8.3% more likely and married individuals were 3.7% more likely to select safer jobs than single enlistees, both significant at the 0.001 level. Individuals with one dependent, who may be a spouse or child for a single parent, were 0.83% more likely to select safer jobs; however, individuals with three dependents were 1.51% more likely to select riskier jobs, both significant at the 0.01 level. This reinforces the impact of patriotism or a role-model effect of a parent for his or her family. These family-oriented AME reflect the importance of family in job-risk selection.

3.7 Discussion and Conclusion

It is impossible to distill all of the factors that affect enlistees' choices to select more versus less-risky jobs in the active-duty Army, especially when there are life and death stakes in the balance. Table 8 summarizes how the anticipated mechanisms for noncitizen-enlistee job-risk choice align with my estimates. Possible mechanisms unique to noncitizen enlistees include selection of riskier jobs than US citizens due to patriotism (or a role model effect for their families), inspired by favorable immigration policies. Noncitizen enlistees may choose more or less risky jobs due to networking effects, either through cultural ties or familial legacies of serving in a particular role. They may choose less-risky jobs due to fears of their families losing spouses or parents. Noncitizen enlistees could also have a different risk preference than US-citizen enlistees for assuming more hazardous jobs in wartime.

The estimated shift in noncitizen enlistees to select safer jobs after the HRIFA suggests that network effects from career branches with a large proportion of non-white soldiers (like the Quartermaster Corps) may have influenced their decisions. With more African Americans than in many other branches, former Haitian asylees may anticipate better opportuni-

ties for mentoring, leadership, and career progression within these fields. Two other possible motivations for this shift include 1) patriotism or 2) a greater fear for family in case of service-related death given the precarious nature of some family-members' citizenship statuses. The estimated shift in noncitizen enlistees toward riskier jobs after the enactment of the CCA suggests that patriotism or a role-model effect (to inspire family members) likely drove decision-makers. There is no evidence that noncitizen job-risk preferences were influenced by expedited citizenship-for-service policies on average.[54]

The AME associated with race and family demographics reinforce these likely mechanisms. Black, Hispanic, and other raced enlistees are more likely to choose 'less-risky' jobs relative to white enlistees by 22.4%, 8.1%, and 6.9% respectively. Having controlled for AFQT scores (dictating threshold requirements per job), these choices are likely driven by network effects, showing avenues where mentors and leadership opportunities are perceived greater than in other career branches. The AME associated with marriage and separation have significant impacts on selection toward safer jobs relative to single enlistees. While one dependent reinforces this trend toward safer occupations, three dependents sway the trend toward riskier jobs, reinforcing the role-model effect for children. This study highlights that cultural and familial considerations drive the job-risk choices of noncitizen enlistees.

[54]These results form a lower bound of the response to these two treatments since the Army comprised about 36% of the active-duty military in 2007. See DMDC Active Duty Military Personnel Master File, Chart 1.05, *2007 Demographics Report*.

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3.9 Appendix of Figures and Tables

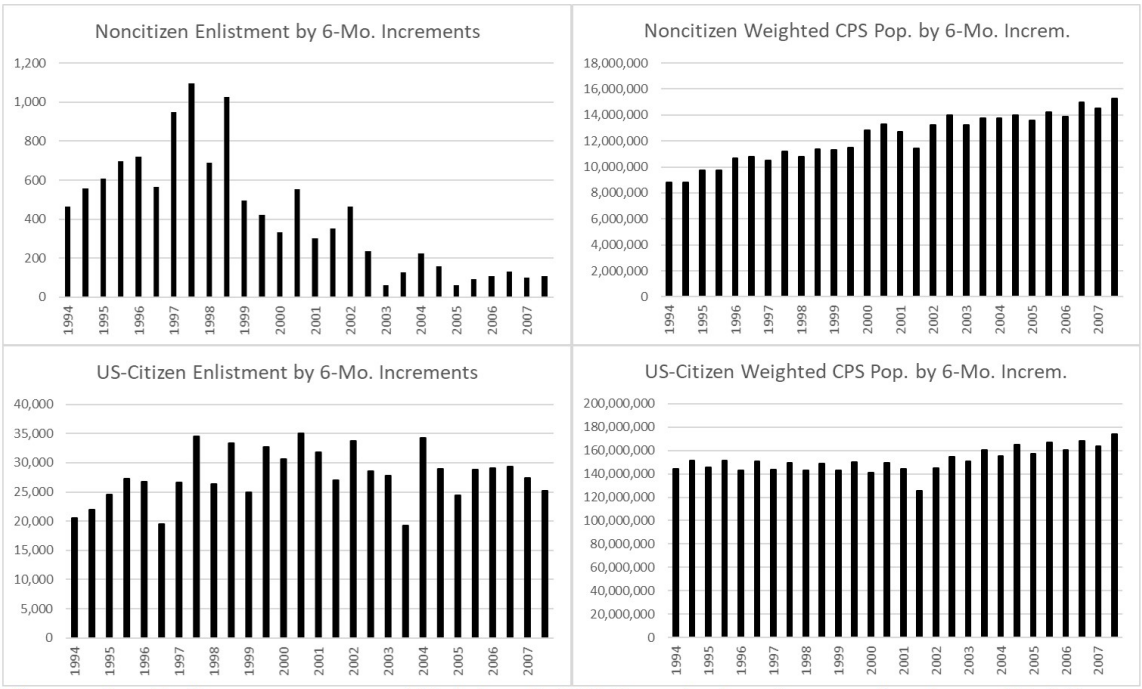

Figure 1a: Enlistment versus Weighted CPS Population Counts for Ages 17-26

Note: The left column reflects the total number of 17 to 26-year old enlistees by six-month intervals from 1994 to 2007 from Army administrative data. The right column reflects the weighted count of individuals from the similar weighted population in the CPS, excluding those with less than a high-school equivalent. US Army administrative data and IPUMS-CPS.

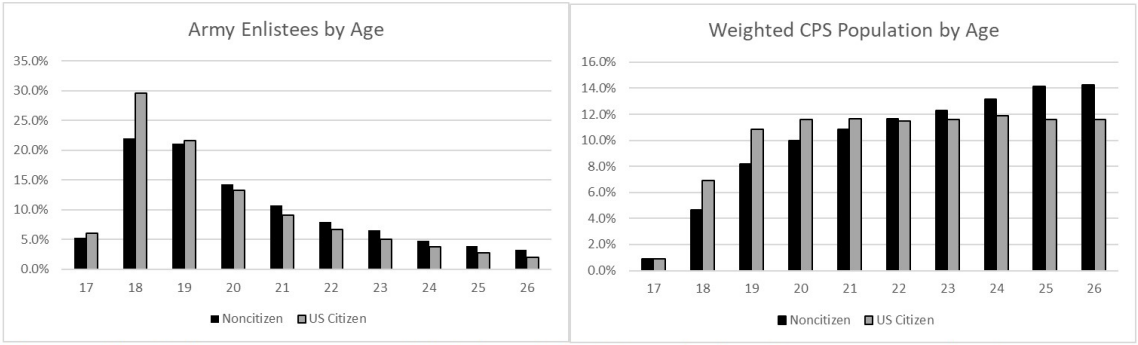

Figure 1b: Enlistment versus Weighted CPS Population Percentages by Age

Note: The left graph reflects the count of 17 to 26-year old, high-school equivalent or more educated enlistees by age over the total number of noncitizen or US citizen enlistees from 1994 to 2007 (11,695 and 768,500 respectively). The right graph shows the same relative ratio from the weighted CPS population. US Army administrative data and IPUMS-CPS.

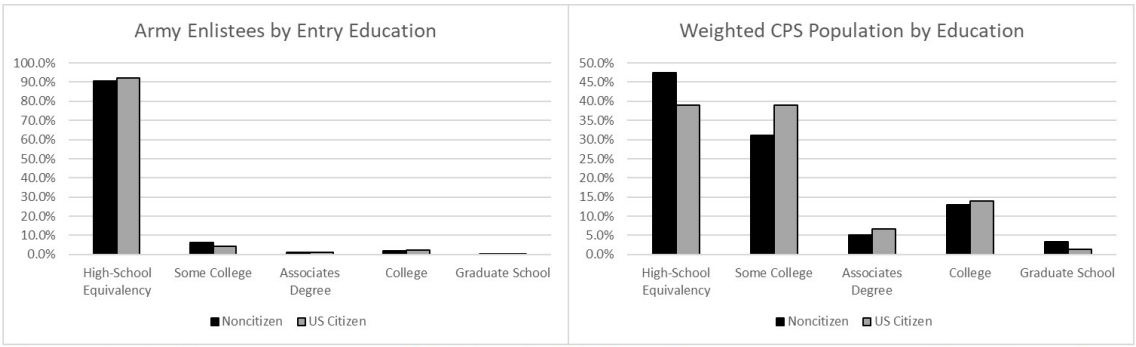

Figure 1c: Enlistment versus Weighted CPS Population Percentages by Education

Note: The left graph reflects the count of 17 to 26-year old enlistees by entry education (excluding high-school dropouts) over the total number of noncitizen or US citizen enlistees from 1994 to 2007 (11,695 and 768,500 respectively). The right graph shows the same relative ratio from the weighted CPS population. US Army administrative data and IPUMS-CPS.

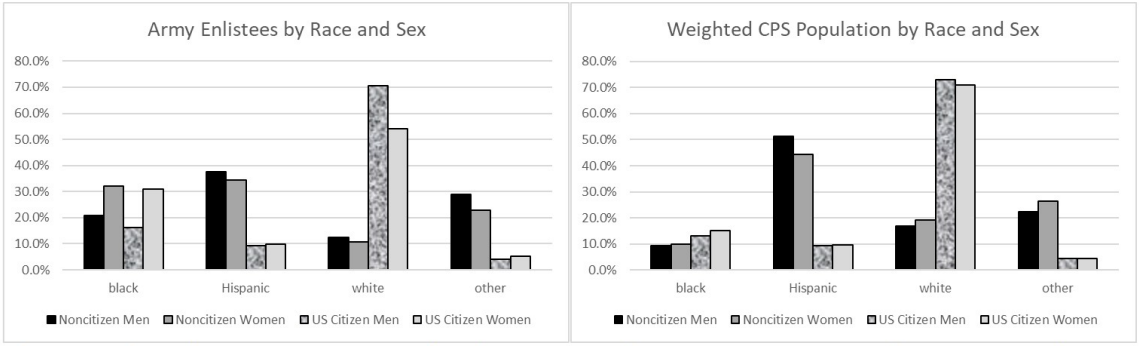

Figure 1d: Enlistment vs. Weighted CPS Population Percentages by Race and Sex

Note: The left graph reflects the count of 17 to 26-year old, high-school equivalent or more educated enlistees by race and sex over the total number of noncitizen or US citizen enlistees from 1994 to 2007 (11,695 and 768,500 respectively). The right graph shows the same relative ratio from the weighted CPS population. US Army administrative data and IPUMS-CPS.

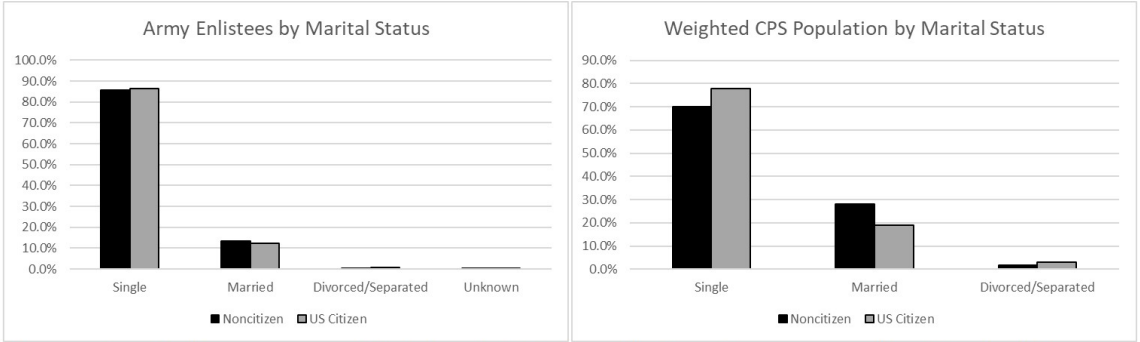

Figure 1e: Enlistment vs. Weighted CPS Population Percentage by Marital Status

Note: The left graph reflects the count of 17 to 26-year old, high-school equivalent or more educated enlistees by marital status over the total number of noncitizen or US citizen enlistees from 1994 to 2007 (11,695 and 768,500 respectively). The right graph shows the same relative ratio from the weighted CPS population. US Army administrative data and IPUMS-CPS.

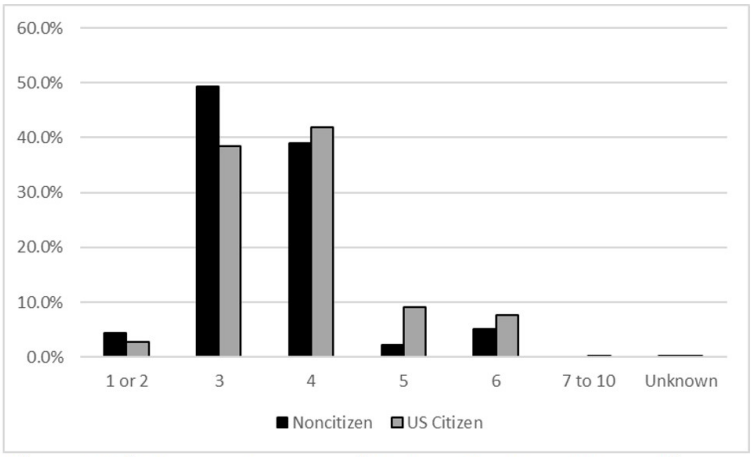

Figure 1f: Percentages of Entry Contract Length

Note: These contract-length percentages show the count of each contract over the total number of 17 to 26-year old noncitizen or US-citizen enlistees from 1994 to 2007 (11,695 and 768,500 respectively). These contract-length counts exclude those with less than a high-school equivalency. With few exceptions, the only ways that contracts can end prematurely is due to a serious injury or illness precluding one from completing his or her commitment, or if a soldier leaves under less-than-honorable circumstances. US Army administrative data.

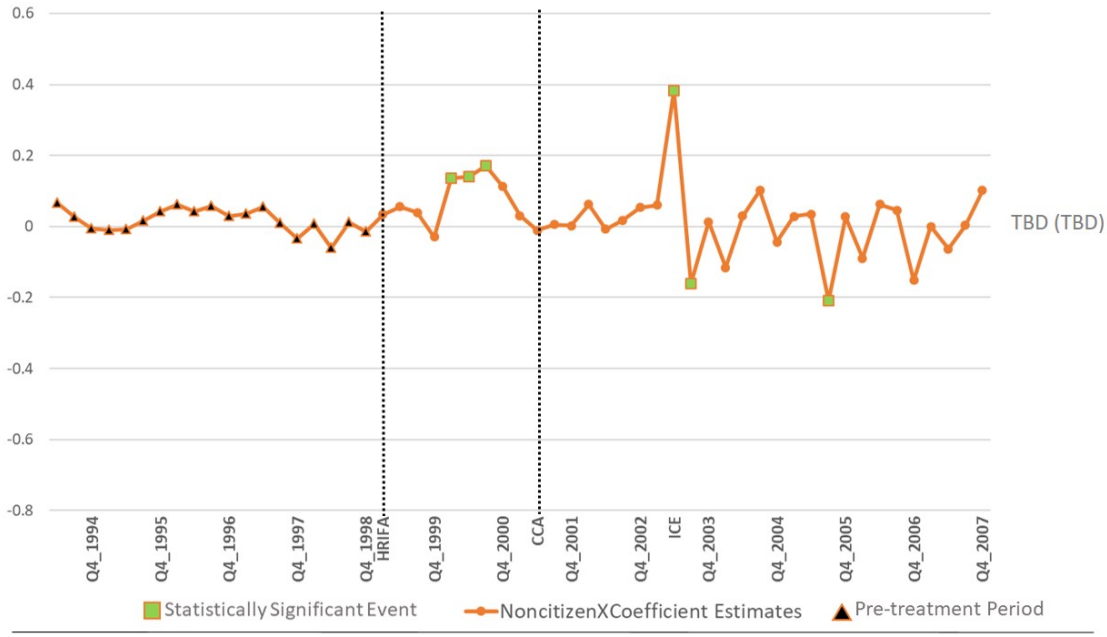

Figure 2: Event Study of Noncitizen Lower-Risk Job Propensity from First Quarter 1994 to Fourth Quarter 2007 for High-School-Equivalents

Note: The two treatment effects, noted by dotted lines, are the enactment of the Haitian Refugee and Immigration Fairness Act (HRIFA) on October 21, 1998 and the Child Citizenship Act (CCA) on February 27, 2001. The highlighted policy control for the enactment of US Immigration and Customs Enforcement (ICE) is on March 1, 2003. The right axis shows the baseline noncitizen pre-treatment lower-risk job propensity (std. dev. in parentheses). I omit First Quarter 1994 due to collinearity. Robust p-values + $p < 0.10$, * $p < 0.05$.

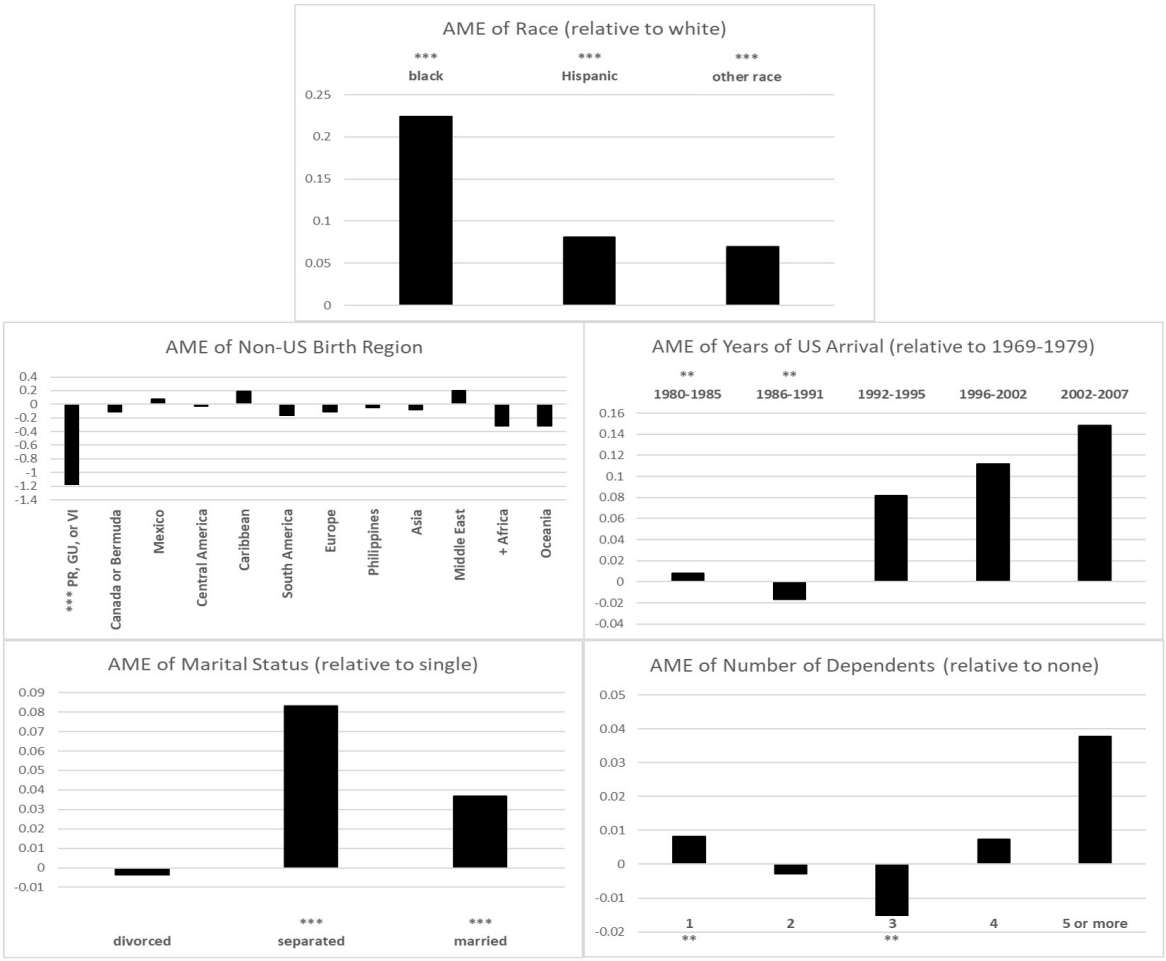

Figure 3: Average Marginal Effects on Lower-Risk Job Propensity

Note: All graphs reflect AME for high-school equivalents only from 1994 to 2007 for 17 to 26-year olds. `PR, GU, or VI' stands for Puerto Rico, Guam, and the US Virgin Islands, which are US territories. The middle-right graph reflects AME of immigration periods for noncitizens citizens (treatment group), naturalized citizens (control group), US citizens born abroad (control group), and US citizens born stateside via birth cohort (control group). Robust delta-method p-values + p<0.10, * p<0.05, ** p<0.01, *** p< 0.001.

Table 1: Federal Immigration Policies or Events from 1994 to 2007

Policy	Dates in Effect	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007
				1 2 3 4	1 2 3 4	1 2 3 4	1 2 3 4	1 2 3 4	1 2 3 4	1 2 3 4	1 2 3 4	1 2 3 4	1 2 3 4	1 2 3 4	1 2 3 4
1	19-Nov-97 to present					Nicaraguan Adjustment & Central American Relief Act (NACARA)									
2	**21-Oct-98 to present**						**Haitian Refugee Immigration Fairness Act (HRIFA)**								
3	**27-Feb-01 to present**								**Child Citizenship Act (CCA)**						
4a	11-Sep-01 to 30-Jun-02								329 Ant.						
5	26-Oct-01 to present									USA Patriot Act					
4b	03-Jul-02 to present									Section 329 - enacted and backdated to 9/11					
6	25-Nov-02 to present										Homeland Security Act (HSA)				
7	01-Mar-03 to present										US Immigration & Customs Enforcement - enacted				
8	24-Nov-03 to present											National Defense Authorization Act (NDA)			
9	01-Oct-04 to present												Section 329 Updates - enacted		

Note: The two treatment effects are the Haitian Refugee Immigration Fairness Act (HRIFA) and the Child Citizenship Act, denoted as Policy (2) and (3) respectively. Policies (4a) through (9) are policy controls. As a robustness test, I add the Nicaraguan Adjustment and Central American Relief Act (NACARA) (1) as a precursory-treatment effect. Green indicates an anticipated positive effect, yellow indicates an uncertain effect, and red indicates an anticipated negative effect of each event on lower-risk job propensity.

Table 2: Anticipated Mechanisms from 1994 to 2007

	Immigration Policy	Increased Risk of Combat Death	Network Effects (incl. Familial Legacy)	Flight to Safety (Fear for Family)	Patriotism (Role Model Effect)
1	Nicaraguan Adjustment & Central American Relief Act (NACARA)		+ or -	+	- or +
2	**Haitian Refugee Immigration Fairness Act (HRIFA)**		+ or -	+	- or +
3	**Child Citizenship Act (CCA)**		+ or -	+	- or +
4a	9/11 (Section 329 anticipated)	+	+ or -	+	- or +
5	Patriot Act (coincides w/ lagged post-9/11 period)	+	+ or -	+	- or +
4b	Section 329 enacted	+	+ or -	+	- or +
6	Homeland Security Act		+ or -	+	- or +
7	US Immigration & Customs Enforcement (ICE) stood up		+ or -	+	- or +
8	National Defense Authorization (NDA) Act	+	+ or -	+	- or +
9	Section 329 Update enacted	+	+ or -	+	- or +

Note: The two treatment effects are the Haitian Refugee Immigration Fairness Act (HRIFA) and the Child Citizenship Act (CCA), denoted as Policy (2) and (3) respectively. Policies (4a) through (9) are policy controls. As a robustness test, I add the enactment of the Nicaraguan Adjustment and Central American Relief Act (NACARA) (1) as a precursory-treatment effect. Green indicates an anticipated positive effect and red indicates an anticipated negative effect of each event on lower-risk job propensity.

Table 3a: Sample Restrictions in Army-Administrative Data

Initial Sample of Enlistees	Drop	Sample
Original Sample (aged 17 to 42)		1,916,400
Drop if missing entry citizenship, sex, entry age, or entry-educ. level, or born in America Samoa, Fed States of Micronesia, Northern Mariana Islands, Palau, and Marshall Islands	100,700	1,815,700
Drop if noncitizen variables incongruent: LPR at entry cannot be a native-born citizen now	5,300	1,810,400
Drop if citizenship variables incongruent: a native-born citizen at entry cannot be a LPR or naturalized citizen now; naturalized at entry cannot be a native-born citizen or LPR now	3,500	1,806,900
Drop if enlistment date is before January 1, 1994	291,800	1,515,100
Drop if enlistment date is after December 31, 2007	669,100	846,000
Drop if enlistment dates occurred in September 2001	4,900	841,100
Drop if miscoded entry number of dependents, or missing entry number of dependents, marital status, or contract terms (418 observations)	1,500	839,600
Drop if entry age is 27 to 42	50,800	788,800
Drop if entry civilian education is HS dropout or some college or more, or current HS dropout	71,600	717,200
Drop if entry branch is missing	13,800	703,400
Drop if entry branch requires a security clearance or includes only US citizens, including Public Affairs (PAO), Air Defense (AD), BI (unknown - only US citizens), Civil Affairs (CA), Psychological Operations (PO), Military Intelligence (MI), Military Police (MP), Signal Corps (SC), Special Forces (SF), and Warrant Officer Candidates (WC)	134,200	568,981

Note: The lower-right-hand number of individual, 17 to 26-year old observations reflects a high-school equivalent only specification. I round all other numbers to the nearest 100 to safeguard the personnel data. Birth country is missing throughout the data so I drop those who list (i.e.) American Samoa as their birthplace, retaining some in the sample. Source: Army administrative data.

Table 3b: Sample Restrictions in Unweighted CPS-Population Data

Initial Sample Restrictions	Drop	Sample
Original sample (aged 17 to 42)		8,019,064
Drop if missing Hispanic ethnicity	55,891	7,963,173
North America (ns), Americas (ns), the Marshall Islands, Micronesia, or Other (nec) and Unknown	50,905	7,912,268
Drops foreign-born individuals (not PR, GU, or VI) with no listed immigration year, infeasible immigration years given cohorts after 1957, or labeled as US born	354	7,911,914
Drops US native-born observations mislabeled as immigrating in a particular year, or mislabeled as born in US outlying, but born elsewhere	7	7,911,907
Drop if survey month is September 2001	50,019	7,861,888
Drop if age is 27 to 42	5,090,076	2,771,812
Drop if education is graduate, college, or less than high-school equivalency	1,060,443	1,711,369
Drop if education is some college or associates degree	905,603	805,766

Note: The lower-right-hand number of individual, 17 to 26-year old observations reflects a high-school equivalent only specification. I bundle and weight the data by two-age group, sex, citizenship status, and quarter from 1994 to 2007 to create controls for birth region and period-of-US arrival.

Table 4: Descriptive Statistics for the CPS-Population Sample

Variables	**Observations**	**Mean**	**Std. Dev.**	**Min**	**Max**
Quarter	805,766	20007.9	41.0082	19941	20074
Noncitizen	805,766	0.0768	0.2663	0	1
Female	805,766	0.4830	0.4997	0	1
Ages 17 to 18	805,766	0.1468	0.3539	0	1
Ages 19 to 20	805,766	0.2348	0.4239	0	1
Ages 21 to 22	805,766	0.2060	0.4044	0	1
Ages 23 to 24	805,766	0.2050	0.4037	0	1
Ages 25 to 26	805,766	0.2074	0.4055	0	1
Born in US	805,766	0.8955	0.3059	0	1
Born in PR, GU, or VI	805,766	0.0038	0.0618	0	1
Born in Canada or Bermuda	805,766	0.0015	0.0391	0	1
Born in Mexico	805,766	0.0389	0.1933	0	1
Born in Central America	805,766	0.0095	0.0971	0	1
Born in the Caribbean	805,766	0.0086	0.0922	0	1
Born in South America	805,766	0.0076	0.0866	0	1
Born in Europe	805,766	0.0119	0.1082	0	1
Born in the Philippines	805,766	0.0040	0.0635	0	1
Born in Asia	805,766	0.0134	0.1150	0	1
Born in Oceania	805,766	0.0009	0.0298	0	1
Born in Africa	805,766	0.0024	0.0492	0	1
Born in the Middle East	805,766	0.0020	0.0447	0	1
US Arrival from 1969 to 1979	805,766	0.4927	0.4999	0	1
US Arrival from 1980 to 1985	805,766	0.3394	0.4735	0	1
US Arrival from 1986 to 1991	805,766	0.1133	0.3169	0	1
US Arrival from 1992 to 1995	805,766	0.0191	0.1369	0	1
US Arrival from 1996 to 2002	805,766	0.0247	0.1553	0	1
US Arrival from 2002 to 2007	805,766	0.0108	0.1034	0	1

The sample includes observations with a high-school equivalency. 'PR, GU, or VI' indicates Puerto Rico, Guam, or the US Virgin Islands. I use this sample to create controls for birth region and period-of-US arrival, bundled by sex, two-year age group, citizenship status, and enlistment quarter.

Table 5: Enlisted Active-Duty Army Occupations Open to Noncitizens (LPRs)

Career Branch Military Occupational Specialty (MOS) - Job Title	*Low_Risk* **(Dependent Variable)**	**Current Sample Count**	**Jobs Restricted to LPRs**	*Combat_Arms* **(Better DV)**
Combat-Arms Branches				
Infantry (IN)	0	125,268	None	1
11B - Infantryman				
11C - Indirect Fire Infantryman				
Armor (AR)	0	42,111	None	1
19D - Cavalry Scout				
19K - M1 Armor Crewman				
Field Artillery (FA)	0	**52,034**	**8**	**(by MOS)**
13B - Cannon Crewmember				1
13S - Field Artillery Surveyor				1
Aviation (AV)	0	**25,682**	**5**	**(by MOS)**
15A - Aviation Life Support Systems Repair				1
15B - Aircraft Powerplant Repairer				1
15D - Aircraft Powertrain Repairer				1
15F - Aircraft Electrician				1
15G - Aircraft Structural Repairer				1
15H - Aircraft Pneudraulics Repairer				1
15M - UH-1 Helicopter Repairer				1
15R - AH-64 Attack Helicopter Repairer				1
15S - OH-58D/ARH Helicopter Repairer				1
15T - UH-60 Helicopter Repairer				1
15U - CH-47 Helicopter Repairer				1
15V - Observation/Scout Helicopter Repairer				1
15X - AH-64A Armament/Elect./Avionics Syst. Rep.				1
15Y - AH-64D Armament/Elect./Avionics Syst. Rep.				1
Air Defense Artillery (AD)	N/A	0	All	N/A
Special Forces (SF)	N/A	0	All	N/A
Corps of Engineers (EN)	0	**33,794**	None	**(by MOS)**
21B - Combat Engineer				1
21D - Diver				1
Combat-Support Branches				
Corps of Engineers (EN)	0	**see above**	**5**	**(by MOS)**
21C - Bridge Crewmember				0
21E - Construction Equipment Operator				0
21G - Quarrying Specialist				0
21J - General Construction Equipment Operator				0
21K - Plumber				0
21M - Firefighter				0
21P - Prime Power Production Specialist				0
21Q - Powerline Distribution Specialist				0
21R - Interior Electrician				0
21S - Topographic Surveyor				0
21T - Technical Engineer				0
21V - Concrete and Asphalt Equipment Operator				0
21W - Carpentry and Masonry Specialist				0
44B - Metal Worker				0
44E - Machinist				0
62B - Construction Equipment Repairer				0
Chemical Corps (CM)	0	10,533	None	0
74D - Chemical, Biological, Radiol., Nuclear Specialist				
Military Police Corps (MP)	N/A	0	2	**(by MOS)**
31E - Internment/Resettlement Specialist				0
Military Intelligence Corps (MI)	N/A	0	All	N/A
Signal Corps (SC)	N/A	0	All	N/A

Table 5 (cont.): Enlisted Active-Duty Army Occupations Open to Noncitizens

Career Branch Military Occupational Specialty (MOS) - Job Title	*Low_Risk* (Dependent Variable)	Current Sample Count	Jobs Restricted to LPRs	*Combat_Arms* (Better DV)
Combat-Service-Support Branches				
Ordnance Corps (OD)	0	**92,249**	**25**	**(by MOS)**
45B - Small Arms/Artillery Repairer				0
45K - Armament Repairer				0
52C - Utilities Equipment Repairer				0
52D - Power-Generation Equipment Repairer				0
63A - M1 Abrams Tank System Maintainer				0
63B - Wheeled Vehicle Mechanic				0
63D - Artillery Mechanic				0
63H - Track Vehicle Repairer				0
63J - Quartermaster and Chemical Equipment Repairer				0
63M - M2/3 Bradley Fighting Vehicle System Maintainer				0
94H - Test, Measurement, & Diagn. Eqmt. Maint. Spt. Spec.				0
Transportation Corps (TC)	1	32,678	None	0
88H - Cargo Specialist				
88K - Watercraft Operator				
88L - Watercraft Engineer				
88M - Motor Transport Operator				
88N - Transportation Management Coordinator				
Quartermaster Corps (QM)	1	81,962	None	0
92A - Automated Logistical Specialist				
92F - Petroleum Supply Specialist				
92G - Food Service Specialist				
92L - Petroleum Laboratory Specialist				
92M - Mortuary Affairs Specialist				
92S - Shower/Laundry & Clothing Repair Spec.				
92W - Water Treatment Specialist				
92Y - Unit Supply Specialist				
Finance Corps (FI)	1	2,601	None	0
44C - Financial Management Technician				
Adjutant General's Corps (AG)	1	**22,756**	**5**	**(by MOS)**
42R - Army Bandsperson				0
79R - Recruiter				0
Special Branches				
Medical Service Corps (MS)	1	43,992	None	0
68A - Biomedical Equipment Specialist				
68D - Operating Room Specialist				
68E - Dental Specialist				
68G - Patient Administration Specialist				
68H - Optical Laboratory Specialist				
68J - Medical Logistics Specialist				
68K - Medical Laboratory Specialist				
68M - Nutrition Care Specialist				
68P - Radiology Specialist				
68Q - Pharmacy Specialist				
68R - Veterinary Food Inspection Specialist				
68S - Preventive Medicine Specialist				
68T - Animal Care Specialist				
68V - Respiratory Specialist				
68W - Health Care Specialist				
68X - Mental Health Specialist				
Chaplain Corps (CH)	1	1,711	No Listing	0
56M - Chaplain Assistant				
Judge Advocate General Corps (CH)	1	1,610	No Listing	0
27D - Paralegal Specialist				
09L - Interpreter/Translator (no clearance)	N/A	N/A	N/A	N/A

Note: The sample includes 17 to 26-year olds with a high-school equivalency. *Low_Risk* is my current dependent variable. The first column shows every Military Occupational Specialty (MOS) open to noncitizens, given that they meet threshold AFQT scores and sex-based requirements. US Army PERSCOM MOS Smart Book, http://www.apd.army.mil/Home/Links/PDFFiles/MOS-Book.pdf, copied from Appendix D of McIntosh et al. (2011).

Table 6: Event Study of Lower-Risk Job Propensity (1994 to 2007)

	High-School Equivalency Only Main Model					
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Sex & Two-Year Age-Group Effects	No	Yes	Yes	Yes	Yes	Yes
Race (including Hispanic)	No	No	Yes	Yes	Yes	Yes
Educational Level & AFQT Score	No	No	No	Yes	Yes	Yes
Marital Status & Number of Children	No	No	No	No	Yes	Yes
Birth-Region & Arrival-Period Effects	No	No	No	No	No	Yes
$Noncitizen \times Q2\_1994$	0.0717	0.0671	0.0640	0.0719	0.0710	0.0666
	(0.158)	(0.144)	(0.155)	(0.111)	(0.115)	(0.149)
$Noncitizen \times Q3\_1994$	0.0226	0.0300	0.0313	0.0317	0.0316	0.0260
	(0.644)	(0.504)	(0.476)	(0.470)	(0.470)	(0.564)
$Noncitizen \times Q4\_1994$	-0.0125	0.0026	0.0011	0.0030	0.0020	-0.0054
	(0.781)	(0.950)	(0.977)	(0.940)	(0.959)	(0.898)
$Noncitizen \times Q1\_1995$	0.0003	0.0113	0.0070	0.0106	0.0095	-0.0108
	(0.994)	(0.766)	(0.852)	(0.779)	(0.801)	(0.780)
$Noncitizen \times Q2\_1995$	-0.0075	0.0121	-0.0075	-0.0007	0.0001	-0.0079
	(0.878)	(0.788)	(0.867)	(0.987)	(0.998)	(0.865)
$Noncitizen \times Q3\_1995$	-0.0035	0.0119	0.0019	0.0073	0.0060	0.0162
	(0.934)	(0.757)	(0.960)	(0.846)	(0.875)	(0.694)
$Noncitizen \times Q4\_1995$	0.0287	0.0351	0.0344	0.0403	0.0404	0.0425
	(0.516)	(0.376)	(0.380)	(0.305)	(0.304)	(0.304)
$Noncitizen \times Q1\_1996$	0.0582	0.0547	0.0499	0.0526	0.0519	0.0608
	(0.159)	(0.140)	(0.174)	(0.151)	(0.157)	(0.117)
$Noncitizen \times Q2\_1996$	0.0350	0.0542	0.0430	0.0467	0.0459	0.0418
	(0.456)	(0.204)	(0.313)	(0.274)	(0.281)	(0.348)
$Noncitizen \times Q3\_1996$	0.0456	0.0373	0.0434	0.0478	0.0479	0.0571
	(0.311)	(0.366)	(0.288)	(0.241)	(0.241)	(0.191)
$Noncitizen \times Q4\_1996$	0.0379	0.0374	0.0398	0.0285	0.0260	0.0295
	(0.445)	(0.398)	(0.365)	(0.515)	(0.552)	(0.533)
$Noncitizen \times Q1\_1997$	0.0400	0.0469	0.0323	0.0359	0.0356	0.0353
	(0.333)	(0.220)	(0.393)	(0.341)	(0.345)	(0.389)
$Noncitizen \times Q2\_1997$	0.0277	0.0525	0.0453	0.0496	0.0496	0.0546
	(0.521)	(0.179)	(0.241)	(0.199)	(0.200)	(0.190)
$Noncitizen \times Q3\_1997$	-0.0313	0.0120	0.0085	0.0130	0.0120	0.0094
	(0.411)	(0.732)	(0.805)	(0.706)	(0.727)	(0.806)
$Noncitizen \times Q4\_1997$	-0.0594	-0.0148	-0.0228	-0.0177	-0.0179	-0.0334
	(0.177)	(0.705)	(0.557)	(0.648)	(0.644)	(0.442)
$Noncitizen \times Q1\_1998$	0.0225	0.0448	0.0224	0.0272	0.0265	0.0064
	(0.586)	(0.237)	(0.550)	(0.467)	(0.478)	(0.878)
$Noncitizen \times Q2\_1998$	-0.0645	-0.0231	-0.0305	-0.0280	-0.0286	-0.0605
	(0.159)	(0.583)	(0.467)	(0.504)	(0.495)	(0.203)
$Noncitizen \times Q3\_1998$	0.0308	0.0423	0.0387	0.0429	0.0426	0.0124
	(0.423)	(0.233)	(0.269)	(0.220)	(0.223)	(0.769)
$Noncitizen \times Q4\_1998$	-0.0039	0.0081	0.0048	0.0059	0.0057	-0.0141
	(0.934)	(0.852)	(0.911)	(0.890)	(0.894)	(0.782)
$Noncitizen \times Q1\_1999$	0.0535	0.0558	0.0410	0.0421	0.0431	0.0335
	(0.237)	(0.174)	(0.313)	(0.299)	(0.289)	(0.477)
$Noncitizen \times Q2\_1999$	0.0565	**0.0816+**	0.0707	0.0727	0.0730	0.0561
	(0.260)	**(0.068)**	(0.111)	(0.101)	(0.100)	(0.274)
$Noncitizen \times Q3\_1999$	0.0368	0.0547	0.0523	0.0571	0.0568	0.0390
	(0.388)	(0.155)	(0.170)	(0.134)	(0.136)	(0.417)
$Noncitizen \times Q4\_1999$	-0.0086	-0.0021	0.0080	0.0090	0.0103	-0.0297
	(0.928)	(0.980)	(0.923)	(0.915)	(0.901)	(0.736)
$Noncitizen \times Q1\_2000$	**0.204*** **	**0.159*** **	**0.149** **	**0.148** **	**0.150** **	**0.136***
	(0.000)	**(0.001)**	**(0.001)**	**(0.002)**	**(0.001)**	**(0.021)**
$Noncitizen \times Q2\_2000$	**0.225*** **	**0.163*** **	**0.159** **	**0.161*** **	**0.161*** **	**0.140***
	(0.000)	**(0.001)**	**(0.001)**	**(0.001)**	**(0.001)**	**(0.018)**
$Noncitizen \times Q3\_2000$	**0.232*** **	**0.189*** **	**0.185*** **	**0.188*** **	**0.188*** **	**0.171*** **
	(0.000)	**(0.000)**	**(0.000)**	**(0.000)**	**(0.000)**	**(0.001)**
$Noncitizen \times Q4\_2000$	**0.186** **	**0.167** **	**0.167** **	**0.162** **	**0.163** **	0.113
	(0.005)	**(0.008)**	**(0.006)**	**(0.007)**	**(0.007)**	(0.123)
$Noncitizen \times Q1\_2001$	0.0963	0.0794	0.0813	0.0785	0.0774	0.0310
	(0.104)	(0.155)	(0.141)	(0.155)	(0.162)	(0.651)
$Noncitizen \times Q2\_2001$	0.0313	0.0323	0.0320	0.0335	0.0319	-0.0107
	(0.536)	(0.464)	(0.461)	(0.439)	(0.461)	(0.862)

Table 6 (cont.): Event Study of Lower-Risk Job Propensity (1994 to 2007)

	High-School Equivalency Only Main Model					
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Sex & Two-Year Age-Group Effects	No	Yes	Yes	Yes	Yes	Yes
Race (including Hispanic)	No	No	Yes	Yes	Yes	Yes
Educational Level & AFQT Score	No	No	No	Yes	Yes	Yes
Marital Status & Number of Children	No	No	No	No	Yes	Yes
Birth-Region & Arrival-Period Effects	No	No	No	No	No	Yes
$Noncitizen \times Q3\_2001$	0.0171	0.0138	0.0223	0.0251	0.0251	0.0061
	(0.709)	(0.739)	(0.589)	(0.543)	(0.543)	(0.917)
$Noncitizen \times Q4\_2001$	**0.110+**	0.0486	0.0428	0.0392	0.0380	0.0026
	(0.091)	(0.437)	(0.492)	(0.529)	(0.543)	(0.973)
$Noncitizen \times Q1\_2002$	**0.187*** **	**0.126** **	**0.113** **	**0.112** **	**0.110** **	0.0624
	(0.000)	**(0.002)**	**(0.006)**	**(0.007)**	**(0.008)**	(0.313)
$Noncitizen \times Q2\_2002$	0.0719	0.0511	0.0399	0.0418	0.0418	-0.0073
	(0.203)	(0.318)	(0.434)	(0.410)	(0.409)	(0.920)
$Noncitizen \times Q3\_2002$	**0.117***	0.0452	0.0539	0.0592	0.0582	0.0177
	(0.049)	(0.390)	(0.306)	(0.261)	(0.271)	(0.801)
$Noncitizen \times Q4\_2002$	**0.210*** **	**0.104+**	**0.0996+**	**0.0995+**	**0.0982+**	0.0542
	(0.000)	**(0.062)**	**(0.071)**	**(0.070)**	**(0.075)**	(0.456)
$Noncitizen \times Q1\_2003$	**0.157***	**0.134+**	**0.123+**	**0.123+**	**0.126+**	0.0604
	(0.044)	**(0.059)**	**(0.079)**	**(0.076)**	**(0.069)**	(0.550)
$Noncitizen \times Q2\_2003$	**0.576*** **	**0.408***	**0.423***	**0.429***	**0.437** **	**0.382***
	(0.000)	**(0.031)**	**(0.013)**	**(0.010)**	**(0.009)**	**(0.040)**
$Noncitizen \times Q3\_2003$	-0.0723	**-0.103+**	**-0.106***	**-0.103+**	**-0.102+**	**-0.161+**
	(0.287)	**(0.060)**	**(0.049)**	**(0.056)**	**(0.057)**	**(0.068)**
$Noncitizen \times Q4\_2003$	0.0751	0.0740	0.0666	0.0678	0.0689	0.0136
	(0.324)	(0.264)	(0.318)	(0.309)	(0.302)	(0.890)
$Noncitizen \times Q1\_2004$	-0.0656	-0.0306	-0.0399	-0.0407	-0.0420	-0.115
	(0.229)	(0.510)	(0.388)	(0.378)	(0.363)	(0.137)
$Noncitizen \times Q2\_2004$	**0.142***	**0.101+**	0.0873	0.0887	0.0877	0.0293
	(0.026)	**(0.083)**	(0.132)	(0.126)	(0.131)	(0.718)
$Noncitizen \times Q3\_2004$	**0.242*** **	**0.187*** **	**0.171** **	**0.176*** **	**0.172** **	0.102
	(0.000)	**(0.001)**	**(0.001)**	**(0.001)**	**(0.001)**	(0.228)
$Noncitizen \times Q4\_2004$	0.140	0.0330	0.0302	0.0253	0.0236	-0.0431
	(0.103)	(0.705)	(0.729)	(0.771)	(0.786)	(0.683)
$Noncitizen \times Q1\_2005$	0.111	0.117	0.107	0.107	0.110	0.0281
	(0.274)	(0.170)	(0.208)	(0.209)	(0.197)	(0.795)
$Noncitizen \times Q2\_2005$	**0.256***	0.134	0.129	0.126	0.129	0.0355
	(0.016)	(0.176)	(0.203)	(0.210)	(0.201)	(0.782)
$Noncitizen \times Q3\_2005$	**-0.176** **	**-0.114***	**-0.124***	**-0.124***	**-0.122***	**-0.209***
	(0.004)	**(0.026)**	**(0.021)**	**(0.021)**	**(0.022)**	**(0.028)**
$Noncitizen \times Q4\_2005$	0.117	0.123	0.125	0.123	0.121	0.0276
	(0.283)	(0.237)	(0.219)	(0.224)	(0.230)	(0.834)
$Noncitizen \times Q1\_2006$	0.0334	0.0407	0.0138	0.0100	0.0102	-0.0891
	(0.737)	(0.685)	(0.893)	(0.922)	(0.921)	(0.481)
$Noncitizen \times Q2\_2006$	**0.172***	**0.167** **	**0.141***	**0.135***	**0.135***	0.0627
	(0.014)	**(0.008)**	**(0.027)**	**(0.034)**	**(0.034)**	(0.539)
$Noncitizen \times Q3\_2006$	**0.140***	**0.137***	**0.129***	**0.129***	**0.128***	0.0459
	(0.024)	**(0.021)**	**(0.030)**	**(0.031)**	**(0.032)**	(0.641)
$Noncitizen \times Q4\_2006$	-0.0202	-0.0366	-0.0469	-0.0517	-0.0568	-0.152
	(0.862)	(0.683)	(0.596)	(0.553)	(0.515)	(0.183)
$Noncitizen \times Q1\_2007$	0.129	0.123	0.0937	0.0901	0.0902	0.0002
	(0.138)	(0.110)	(0.213)	(0.231)	(0.230)	(0.999)
$Noncitizen \times Q2\_2007$	0.0426	0.0441	0.0355	0.0358	0.0342	-0.0631
	(0.600)	(0.529)	(0.608)	(0.606)	(0.620)	(0.580)
$Noncitizen \times Q3\_2007$	0.114	0.106	0.0910	0.0908	0.0859	0.0035
	(0.153)	(0.140)	(0.197)	(0.196)	(0.219)	(0.975)
$Noncitizen \times Q4\_2007$	**0.253** **	**0.229** **	**0.197***	**0.194***	**0.194***	0.102
	(0.002)	**(0.004)**	**(0.011)**	**(0.012)**	**(0.012)**	(0.397)
Cellular Observations	568,341	568,341	568,341	568,341	568,341	568,341
R-squared	0.007	0.184	0.217	0.218	0.219	0.219

Note: All specifications include a constant and indicators for noncitizen, quarter effects, sex, two-year age-group effects (from ages 17-26), birth-region effects, race, educational level, AFQT score, marital status, number of dependents, and period of US-arrival effects. All columns show coefficient estimates fora robustness check with high-school equivalents only. Robust p-values $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 7: Preliminary Results for Lower-Risk Job Propensity from 1994 to 2007

Variables	High-School Equivalency Only (Robustness Test) (1) PRWORA, IIRIRA, & NACARA	(2) + HRIFA	(3) + CCA	(4) + 9/11, Patriot Act, & 329	(5) + HSA & ICE	(6) + NDA & 329 Update
$Noncitizen \times PRWORA$ 01Oct96_31Dec07	0.00028 (0.990)	0.00088 (0.968)	0.00150 (0.946)	0.000392 (0.986)	0.00061 (0.978)	0.00078 (0.972)
$Noncitizen \times IIRIRA$ 01Apr97_31Dec07	-0.0130 (0.581)	-0.0146 (0.536)	-0.0177 (0.451)	-0.0171 (0.467)	-0.0171 (0.467)	-0.0168 (0.476)
$Noncitizen \times NACARA$ 01Jan98_31Dec07	-0.00135 (0.949)	-0.00278 (0.896)	-0.0327 (0.145)	-0.0289 (0.203)	-0.0300 (0.185)	-0.0258 (0.260)
$Noncitizen \times HRIFA$ 01Jan99_31Dec07		**0.0686*** (0.000)**	**0.0742*** (0.000)**	**0.0760*** (0.000)**	**0.0756*** (0.000)**	**0.0786*** (0.000)**
$Noncitizen \times CCA$ 01Apr01_31Dec07			**-0.0864*** (0.000)**	**-0.101*** (0.000)**	**-0.102*** (0.000)**	**-0.0993*** (0.000)**
$Noncitizen \times Post9/11$ 01Oct01_30Jun02				0.00942 (0.872)	0.00929 (0.874)	0.00992 (0.866)
$Noncitizen \times PatriotAct$ 01Jan02_31Dec07				0.0302 (0.612)	0.0297 (0.618)	0.0310 (0.602)
$Noncitizen \times Section329$ 01Jul02_31Dec07				-0.0151 (0.825)	0.00763 (0.914)	0.00771 (0.914)
$Noncitizen \times HSA$ 01Jan03_31Dec07					0.00950 (0.909)	-0.0214 (0.810)
$Noncitizen \times ICE$ 01Apr03_31Dec07					-0.0800 (0.210)	**-0.130+** (0.075)
$Noncitizen \times NDA$ 01Jan04_31Dec07						0.0795 (0.114)
$Noncitizen \times 329Update$ 01Oct04_31Dec07						-0.0313 (0.431)
$Noncitizen$	-0.0201 (0.816)	-0.0314 (0.717)	-0.0184 (0.832)	-0.0183 (0.833)	-0.0126 (0.885)	-0.00895 (0.918)
Cellular Observations	568,341	568,341	568,341	568,341	568,341	568,341
R-squared	0.219	0.219	0.219	0.219	0.219	0.219

Note: All specifications include a constant and indicators for noncitizen, quarter effects, sex, two-year age-group effects (from ages 17-26), birth-region effects, race, educational level, AFQT score, marital status, number of dependents, and period of US-arrival effects. All columns show coefficient estimates for a robustness check with high-school equivalents only. Robust p-values $^{+}$ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 8: Summarized Results from 1994 to 2007

	Immigration Policy	Est. DinD Effects: HS-Equiv. from 1994 to 2007 (Robustness Test)	Anticipated Mechanisms: Increased Risk of Combat Death	Network Effects (incl. Familial Legacy)	Flight to Safety (Fear for Family)	Patriotism (Role Model Effect)
1	Nicaraguan Adjustment & Central American Relief Act (NACARA)	-		+ or -	+	- or +
2	**Haitian Refugee Immigration Fairness Act (HRIFA)**	**+ (0.1%)**		**+ (or -)**	**+**	**+ (or -)**
3	**Child Citizenship Act (CCA)**	**- (0.1%)**		**- (or +)**	**+**	**- (or +)**
4a	9/11 (Section 329 anticipated)	+	+	+ or -	+	- or +
5	Patriot Act (coincides w/ lagged post-9/11 period)	+	+	+ or -	+	- or +
4b	Section 329 enacted	+	+	+ or -	+	- or +
6	Homeland Security Act	-		+ or -	+	- or +
7	US Immigration & Customs Enforcement (ICE) stood up	- (10%)		- (or +)	+	- (or +)
8	National Defense Authorization (NDA) Act	+	+	+ or -	+	- or +
9	Section 329 Update enacted	-	+	+ or -	+	- or +

Note: Policies (2) and (3) reflects the two treatment effects. Policy (1) serves as precursory-treatment event for a robustness test. The remaining policies serve as controls. Green indicates an estimated positive effect of these policies on lower-risk job propensity. Statistically significant DinD estimates with robust p-values in parentheses.