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The Effect of the Affordable Care Act Dependency Mandate on Reenlistment in the U.S. Army

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ABSTRACT

One concern with employer-based health insurance is job lock or the inability for employees to leave their current employment for better opportunities for fear of losing benefits. We use the implementation of the Affordable Care Act's dependency mandate as a natural experiment. Data from the United States Army overcome some limitations in previous studies including the ability to examine workers with fixed contract expiration dates, uniform pay, and health coverage. We find that the ACA decreased reenlistment rates by 3.13 percent for enlisted soldiers aged 23–25. We also find that younger veterans who leave the army are more likely to attend college. These findings show that the ACA reduced job lock and increased college-going.

JEL Classification Codes: I13, J22, H56

Key Words: Affordable Care Act, Job Lock, Military Enlistment, GI Bill

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1 Introduction

The United States Congress passed the Affordable Care Act (ACA), colloquially termed "Obamacare," on March 23, 2010. The ACA's proponents in Congress and the White House claimed that the ACA allows more people access to health insurance and reduces their dependence on employer-provided insurance coverage, which in turn creates a more flexible and efficient labor market. In response to a Congressional Budget Office report that the ACA reduces employment, Representative Nancy Pelosi (D-Calif.) stated that "the Affordable Care Act will enable more than 2 million workers to escape job-lock—-the situation where workers remain tied to employers for access to health insurance benefits" (Blake, 2014). One of the ACA's most prominent coverage-expanding features is the dependent mandate, which requires all private insurance plans that offer dependent child coverage to allow that coverage to continue until the dependent's 26th birthday. (Centers for Medicare & Medicaid Services, 2017)

When employees consider leaving a job to find one that better matches their skills, they consider both the benefit their current job provides and the opportunity cost of staying in the job. For young Americans, this option may be another job, an educational opportunity, or some other activity that they perceive increases valuable human capital or lifetime earnings. One opportunity cost of switching jobs is a change in or potential loss of employer-provided health insurance. Workers could choose not to switch to a better match if they perceive that the alternative job's health insurance plan is of lesser quality, more expensive, or nonexistent. In 2016, an individual employee's health insurance coverage cost averaged $5,615, with workers contributing $951 of these costs out of pocket (National Conference of State Legislatures, 2017). The Bureau of Labor Statistics estimates that insurance costs contributed to 7.6 percent of total compensation for private employees and 11.8 percent for state and local employees (Bureau of Labor Statistics, 2017). Because of these high costs, many employees could feel locked into their jobs and do not switch employers because of the cost of losing employer-provided health insurance.

Previous literature provides empirical support for the idea that individuals stay in jobs because of health insurance. Madrian (1994) finds that individuals whose spouses have access to

insurance are 15–25 percent more likely to leave jobs than those without this coverage, and Hamersma and Kim (2009) find that increases in Medicaid eligibility had a statistically significant positive correlation with job turnover for unmarried women. Empirical evidence indicates that the dependent mandate caused a substantial increase in the number of young adults with insurance coverage. Further research also finds that the mandate decreased the participation of young adults in the labor market (Antwi et al., 2013; Depew, 2015).

While these papers show that there is causal evidence that employer-based health insurance may induce job lock, there are some limitations to generally used data sets. First, there is considerable variability of salary and health benefits among jobs that may be unobservable to the researcher.[1] Gruber and Madrian (2004) survey the job lock literature and point out that there is significant unobserved variation in firm size and health insurance plan generosity. For example, small firms are less likely to offer extensive health plans and perhaps charge their workers higher premiums than similar workers at larger firms or in the public sector. One contribution of this paper is that we use data from the U.S. Army which means that all soldiers observed are covered by the same insurance program (TRICARE) and must enroll.

Second, there are endogeneity[2] concerns that the timing of when to leave a job could be influenced by the policy change. Third, it is difficult to ascertain the health status of the worker and ensure that our results are not being driven by differences in health rather than portability of health insurance. Finally, it can be unclear in the data as to whether the worker leaves the firm of their own choice or whether the firm laid off the worker. These unobserved factors could attenuate the effects of the ACA (or policies that increase the portability of health insurance) toward zero.

The ideal experiment in this setting would be a situation where workers were paid equally and have access to the same form of health insurance before the policy change. Also, it would be helpful if the date that a worker must consider leaving the firm was set in advance of the policy

[1]For example, the National Longitudinal Study of Youth and the Current Population Survey have indicators for whether the worker has employer-sponsored health insurance, but no information on coverage or costs.

[2]Bailey and Chorniy (2016) point out a number of concerns from the previous job lock literature such as using married vs. single workers (i.e., Bansak and Raphael (2008) and Adams (2004)) or healthy vs. unhealthy workers (i.e., Stroupe et al. (2001) and Bradley et al. (2012) as treatment and control groups. Bailey and Chorniy (2016) argue that there is unobserved sorting that drives workers into either of these groups.

and there was a clear decision point. Uniformity of worker health would ensure that the worker decided to leave a firm because of insurance and not a negative health shock. Finally, a researcher would want to ensure that workers were given the opportunity to stay but chose to leave, thus signaling that they were locked into a particular job because of the inability to transfer health insurance to their new careers.

U.S. Army data[3] remedy several problems inherent in civilian workforce data. First, all Army personnel have nearly identical compensation at each rank and inside each occupational branch. We can easily control for those differences that may exist. One notable benefit the army provides to soldiers at every rank is their health care network, which allows soldiers full medical coverage at no out of pocket cost. Second, when soldiers enlist into the army, they set a contract that expires usually in three to five years. Soldiers cannot submit a "two-week" notice and move into the civilian sector without facing high penalties. In this study, we consider soldiers who set their enlistment contracts before Congress passed the ACA. Third, all soldiers must pass stringent health and fitness requirements and may be discharged due to poor health, fitness, or disability. We can easily identify these individuals and have dropped them from our sample. Finally, when a soldier's contract expires, the army must first offer the soldier the opportunity to reenlist. If the soldier was unable or unwilling to complete their duties or has performed poorly, the army will not retain them. We condition our data on receiving an offer of reenlistment; thus, if a soldier leaves in our sample it is by her own choice. While the army setting is different from other sectors in the labor market, its unique characteristics allow us to avoid certain limitations that may exist in previous studies and alleviate the concerns of unobserved insurance quality and worker health status pointed out by Gruber and Madrian (2004) and Bailey and Chorniy (2016).

[3]Others have studied the effects of access to health insurance on the labor supply decisions of veterans who have already left the service. Boyle and Lahey (2010) use a policy change in the 1990s that expanded veteran eligibility for access to the Veterans Affairs hospital network. The authors find that this expansion of coverage decreased labor supply among lower education veterans, while college graduates were more likely to be self-employed. Boyle and Lahey (2016) find similar results for spouses of affected veterans. Autor et al. (2016) use the expansion of Veterans Affairs disability compensation to veterans exposed to Agent Orange as a natural experiment and find that veterans with increased benefits reduced their labor supply. Coile et al. (2015) find similar effects to veterans given increases in disability compensation of the early 2000s. Our paper contributes to this literature by exploring the effects of the passage of the ACA on the labor supply decisions of active duty military members as opposed to those who had previously separated from the military.

Prior to the ACA, a young adult's decision to leave the army would mean that they may lose health care coverage. Because fully funded health care was not guaranteed outside the military, soldiers may have assessed a low opportunity cost for reenlisting due to the high cost of health care they may have incurred upon leaving the army. However, soldiers younger than 26 can now leave the military at the end of their contact's term and return to their parents' insurance plans. This paper hypothesizes that the ACA's dependent insurance mandate causes enlisted soldiers below age 26 to leave the army at a higher rate due to the decrease in health care costs they now face outside the army. These lower health care costs increase the value of a soldier's next best option and the opportunity cost of reenlisting.

Our main estimates show that that the ACA reduced reenlistment rates by 1.3 percentage points or 3.14 percent for enlisted soldiers ages 23–25. This result is robust to the addition of various controls and home state of record fixed effects. We estimate various heterogeneous treatment showing that soldiers with increased human capital are the most responsive to the dependent mandate. We also conduct various placebo and robustness tests that use different treatment groups and year specifications to show that our results are a product of the dependent mandate and not contemporaneous year or age effects.

In our sample, the army offered 137,958 soldiers aged 23–26 the option to reenlist, with 54,316, or 53 percent, of those soldiers electing to reenlist. Our findings imply that the dependent mandate reduced reenlistments by 3,255 soldiers. The costs to replace these soldiers include training and recruitment as well as lost human capital within the army that decreases efficiency and preparedness within units. This paper demonstrates that the ACA effectively changed behavior in young adults and provides evidence of job lock within the army.

In addition, to better understand the extent of job lock in the military context, we test whether soldiers leaving the army move to better opportunities in the labor force. To test this hypothesis, we merge Post 9/11 GI Bill usage data to our enlistment records and find that veterans that have access to their parents' health insurance are more likely to use their GI Bill benefits. These findings are important since turnover due to health insurance portability may increase a

firm's costs, but increased education and labor market opportunities are beneficial to the worker. We find that a veteran with access to health insurance because of the dependent mandate is three percent more likely to attend college on the GI Bill.

Following this introduction, Section 2 provides institutional details. Section 3 provides background on job lock and the ACA's effect on labor markets and explains the contribution this paper makes to existing literature. Section 4 discusses the data, how it helps to overcome issues in other data sets, and its limitations. Section 5 details our empirical model. Section 6 discusses results including the heterogeneous treatment effects. Section 7 presents a host of placebo and robustness checks. Section 8 summarizes results from our model for the effect of the ACA on the uptake of the Post 9-11 GI Bill. Section 9 discusses the results and their generalizability to a broader context of national security and labor market policy. Finally, Section 10 concludes.

2 Institutional Details Regarding Enlistment in the U.S. Army

The U.S. Army is composed of two distinctly different groups: officers and enlisted soldiers. the army requires that officers hold bachelor's degrees before receiving a commission, while enlisted soldiers must be between 17 and 35 years old when they join and hold a high school diploma or GED. Both officers and enlisted soldiers must meet baseline physical and medical requirements before entering the military. These requirements create a population with similar health upon entry. Those with chronic health conditions such as cancer, asthma, or diabetes are not admitted or retained in the army.

Enlisted soldiers have two basic divisions: junior enlisted (comprised from ranks of E1 to E3) and noncommissioned officers (E5 and above). Upon entering the army, they will be privates (E1 to E3), after which they will receive a promotion to specialist (E4). Privates and specialists are known as junior enlisted. After showing competence and leadership potential, qualified junior-enlisted soldiers are promoted to the ranks of noncommissioned officers as a sergeant (E5 and above). Noncommissioned officers supervise lower-enlisted soldiers and ensure the soldiers

carry out officers' orders. Enlisted soldiers cannot advance into officer ranks without leaving and attending Officer Candidate School, an ROTC detachment at a university, or the United States Military Academy at West Point. One difference between enlisted soldiers and officers is that officers do not sign the same contracts as enlisted soldiers. For this reason, we exclude officers from our data. With rare exceptions, the army cannot hire laterally from the civilian workforce; meaning that a new recruit needs to start at the bottom of the rank structure regardless of outside experience. Thus, an increase in separation from the military comes at an increased training and retention cost.

Branches, or occupational lanes, exist in both officer and enlisted ranks. There are 17 branches ranging from combat arms, such as armor and infantry, to logistic, such as quartermaster and transportation. With relatively few exceptions, soldiers maintain membership in their branches for the duration of their careers. While civilian jobs vary from employer to employer, jobs within each branch are relatively uniform across the army for a given rank. Thus while the tasks that infantry soldiers and helicopter mechanics perform certainly differ, the designators assigned to each soldier's file allow us to create controls that account for the differences in careers across the military. Furthermore, the branch in which each soldier serves helps control for many of the differences in compensation that exist within the army due to hazard or flight pay.

When enlisted soldiers join the army, they sign contracts that are set for specific terms of service ranging from two to six years. Upon completion of the contract, the army will either discharge the soldiers or give them an offer of reenlistment for another term of service. With rare exceptions for issues such as medical problems, poor performance, or misbehavior, soldiers cannot leave the army prior to finishing their contracts. These term contracts offer a unique way to examine soldiers' labor market decisions in a way that most private sector data cannot replicate. Because soldiers must decide to reenlist at dates determined several years in advance, term contracts force soldiers to uniformly evaluate compensation and opportunity costs before continuing their military careers. These contracts eliminate problems inherent in civilian workforce data where employees may never have to make a decision to leave or stay, and timing

for leaving or quitting jobs is endogenous.

While on active duty, soldiers enroll in TRICARE or the military health insurance plan. TRICARE is available to active duty military, soldiers retired from the service[4] and activated Guard/Reserve members. TRICARE also covers dependents.[5] While active duty service members pay nothing out of pocket, however, choice may be limited to the military health care network. For example, seeing a civilian specialist requires a referral by a military physician. Unlike many jobs in the private sector, if a soldier does separate from the army her TRICARE benefits continue premium-free for 180 days (a program called Transitional Assistance Management Program [TAMP]). Thus the dependent mandate potentially offers younger soldiers health coverage after their TAMP benefits expire.

3 Background on Job Lock and the ACA Dependent Mandate

3.1 Job Lock

Job lock affects labor supply decisions in three ways. First, it discourages individuals from entrepreneurial ventures due to their consequent need for individually purchased insurance. Second, it encourages employees to stay at a job even if a job for which they are a better match is available in the labor market. This effect may also drive people into the labor force who would otherwise retire or choose not to participate in the labor force. Third, it may increase individuals' participation in the labor market on the intensive margin because they must obtain full time status to qualify for employer-provided health insurance. This paper does not analyze job lock's effect on the intensive margin because the army is not a profession that offers hourly rates.

The findings in the literature regarding health insurance and job lock are mixed. Madrian (1994) examines differences in job turnover rates conditional on whether the worker's spouse has

[4]Service members must have completed 20 years on active duty to be considered retired.

[5]Regarding TRICARE, one interesting aspect of tort law is that active duty personal cannot sue the government for malpractice for procedures covered by TRICARE, but their dependents can sue due to negligence in their own health care. Frakes and Gruber (2018) show that liability immunity toward active duty patients reduces inpatient spending by 5 percent without harming active duty patients.

access to employer-provided health insurance. The author finds that employees with insurance coverage through their spouses are between 12 and 16 percentage points more likely to leave their jobs than those whose spouses do not provide coverage. Monheit and Cooper (1994) use variables such as job experience and education to estimate the likelihood that individuals will be able to find a job that provides health insurance and then regress job turnover on this likelihood variable. The authors show that the likelihood of gaining insurance is negatively correlated with the likelihood of staying in a job. Gruber and Madrian (1994) study rollouts of state laws that gave workers access to COBRA or continuation of health benefits for a short amount of time. The authors find that worker mobility increased when state governments enacted these laws, showing that health insurance portability is important for short-run transitions.

Other scholars' findings suggest that employer-provided health insurance does not affect labor supply. Holtz-Eakin (1994) examines one- and three-year transition rates using spousal coverage strategy of Madrian (1994) but find no significant difference between those whose spouses have insurance and those whose spouses do not. However, several issues exist with workforce data that pose problems for scholars measuring job lock. Bailey and Chorniy (2016) use data from the Current Population Survey to measure the effect of the dependent mandate on job mobility of young adult workers using a method similar to ours. The authors find that the ACA had no effect on job switching. The authors also point out a number of issues of studying job lock that we hope to remedy by using the unique institutional setting of Army enlistments.

3.2 The ACA Dependent Mandate

The ACA's dependent mandate was a large increase in coverage for young adults because it allowed children up to the age of 26 to return to their parent's health care plans at no extra cost. The quickness and popularity of this aspect of the ACA have made the dependent mandate a useful natural experiment to study the effect of health insurance coverage on a variety of health and labor market outcomes. Barbaresco et al. (2015) find that despite the increase in insurance coverage, the dependent mandate did not increase young adults' use of preventative care. The

mandate did, however, increase the probability that young adults reported excellent self-assessed health. However, Depew and Bailey (2015) find that the dependency mandate did increase premiums for the young adult's parents' health insurance by 2.5–2.8 percent. Thus, while evidence suggests that the mandate does not increase young adults' actual health care use, it may nonetheless cause young adults to feel more confident about their personal health. Colman and Dave (2018) show that while overall health care usage may not have increased, young adults substituted away from Emergency Department usage toward primary care physician visits. McClellan (2017), using the Multiple Cause Mortality database, shows that the ACA dependent mandate reduced mortality among young adults, while Daw and Sommers (2018) shows similar results for infants.

Empirical evidence also suggests that the dependent mandate influenced young adults behaviors beyond health care and health insurance. Abramowitz (2016) finds that the dependent mandate influenced young adults to forgo marriage and cohabitate at higher rates because young adults no longer depend on a potential spouse's insurance plan to obtain health care coverage, which consequently decreases cohabitation's opportunity cost. Abramowitz (ming) finds that the ACA dependent mandate also reduces the likelihood of giving birth and increases the use of long-term hormonal contraception. If expanded insurance coverage options drive individuals to change marriage and fertility decisions, it is likely that increased coverage may have affected other decisions such as remaining in a job or furthering one's education.[6] Jung and Shrestha (2018) find that the dependent mandate encourages more young adults to enroll in college full time. We extend our findings in this paper to GI Bill usage and find similar results.

In this paper, we study how insurance portability affects young adults' labor supply. Previous research on this topic finds evidence that the higher uptake in parental health insurance is associated with greater flexibility in the labor market. Depew (2015) studies dependent mandates enacted on the state level before the ACA. He finds that state-dependent mandate laws

[6]Various short-run effects of the ACA dependent mandate include increases in self-employment among disabled workers (Bailey, 2017), lowered precautionary savings but increased savings in illiquid assets (Lee, 2016), taking on lower wage jobs (Heim et al., 2018), increased leisure time (Lenhart and Shrestha, 2017), reduced drug and alcohol usage Breslau et al. (2017), and increased contraception usage Trudeau and Conway (2018).

increased health insurance coverage of young adults and decreased the number of hours that young adults worked. Antwi et al. (2013) use the ACA's dependent mandate as a natural experiment and also find that the dependent mandate reduces the probability that a young adult works full time by 2.21 percentage points. They also find that the ACA reduced the number of hours worked by 4.75 percent.

While previous literature analyzes the ACA's effect on individual behaviors ranging from health care utilization to cohabitation rates, this paper adds to existing literature with a unique ability to assess the magnitude of job lock in the labor market. It measures the value that soldiers place on health care benefits when they calculate the opportunity cost for staying in the army using administrative data. However, the paper's contributions go beyond the army. While the army's unique demographic and setting limit our ability to translate our findings to the civilian population as a whole, evidence of job lock in the army suggests that job lock could possibly exist outside the army as well. Because the army's term contracts force soldiers to assess whether they will choose to stay in or leave the service, they provide a compelling population to study how employees value their compensation and provide further support for policymakers looking to increase access to health care as a way to decrease friction in the labor market.

4 Data

This paper uses U.S. Army administrative data from the Office of Economic and Manpower Analysis at West Point. In these data, we observe demographic and enlistment contract characteristics for every soldier and the decision whether to reenlist in the army once their initial contract ends. We limit our sample to soldiers aged 23–25 and 27–29 from years 2006 to 2013 who are on their first enlistment contract. Army ranks included in our sample are private (E1-E3), specialist (E4), corporal (also E4), and sergeant (E5). Our sample excludes all officers and enlisted soldiers above E5 due to differences in contracts, opportunity costs, and motivations to reenlist. We also exclude anyone from our sample to whom the army did not offer a reenlistment

option to ensure that we can distinguish labor supply from demand.

For our empirical model, we use the same age ranges and year groups as Barbaresco et al. (2015) and Abramowitz (2016). We limit our sample to soldiers between aged 23–29 to mitigate unobservable differences between treatment and control groups. Furthermore, some states already had dependent mandate laws for individuals younger than 23. We also exclude soldiers who are 26 years old to clearly separate treatment and control groups. We chose the years 2006–2013 for the same reasons as Barbaresco et al. (2015), by excluding data prior to 2006 to decrease the likelihood that macroeconomic shocks will sway our sample's job decisions. Depew (2015) cautions against using data for years prior to 2007 because the Great Recession affected younger people more than older people and thus increases the risk of introducing contemporaneous time trends into the data. One limitation to our data, however, is that we cannot observe whether the soldier's parents are insured, which would mean that she would not have access to her parents' plan under the ACA if she left the army. We want to avoid recession years because parents would be less likely to be employed and have health insurance.

the army first offers reenlistment conditional on a soldier showing competence in her duties and remaining physically fit. We can observe whether the army offered the soldier reenlistment and we drop observations throughout the paper of those to whom the army decided not to offer. This feature is advantageous to us because we can clearly distinguish demand side effects (a firm firing or laying off an employee) from supply side effects (the employee choosing to leave the firm for another opportunity). By conditioning our sample on those the army offers reenlistment, we can clearly identify a labor supply effect. Also, in our data, we can observe the exact date that the enlistment contract expires and the soldier leaves the army, thus we can precisely code whether the contract expired before or after the ACA's enactment and/or implementation.

To avoid selection and compositional effects, we only consider soldiers who are completing their first enlistment contract and thus are deciding whether to remain in the army after an initial introduction. While our control group is a few years older, this sample selection

allows us to compare soldiers who remain on the same career trajectory—that is, having achieved the same rank in the same amount of service time or chosen the same occupational branch. Comparing soldiers who are older but have completed multiple enlistment terms and remained in the army would be inappropriate because of selection on unobservables. Thus, controlling for rank and branch should allow us to compare equally rated soldiers, with the only difference being that those in the control group are a few years older and thus not eligible for parental health coverage. However, not controlling for rank and branch could cause us to compare soldiers who did not advance as quickly or have a higher taste for military-specific tasks which would bias our estimates upwards.

Table 1 displays the summary statistics for the soldiers in our sample. We observe, conditional on being offered reenlistment, that 54 percent of soldiers reenlist. We also find that a majority of our sample is of rank E4 (specialist or corporal) who are the most junior noncommissioned officers. Another 23 percent of our sample is of rank E5 (sergeant). This distribution of rank is not surprising because we observe soldiers at the end of their enlistment contract and since they were offered reenlistment, they were promoted in a timely manner.

Not surprisingly, given our identification strategy, the average age of our sample is 24.86 years. Our sample is also representative of the army as a whole: 67 percent of the soldiers are white, 15 percent are black, and 12 percent are Hispanic. The soldiers in our sample are also 87 percent male. The education level of our sample is also representative of the army given that a soldier must have at least a high school degree to enlist, but those with a four-year college degree are more likely to receive a commission as an officer. In our sample, 74 percent of soldiers have only a high school degree and 13 percent hold a GED. However, our sample does show that a little more than 10 percent of enlisted soldiers have either attended some college or completed a four-year degree.

Figure 1 and Table 2 provide preliminary evidence that our assumption of parallel trends holds. Figure 1 displays reenlistment rates for soldiers who were offered a choice to reenlist, and shows a distinct downward shift starting in 2010 for younger soldiers. Table 2 shows covariate

balance for our treatment and control groups before and after the passage of the ACA. Standard deviations are included in parentheses under the means. Both groups have relatively similar reenlistment rates, with 56 percent of soldiers reenlisting before the ACA in both treatment and control groups. Enlistment rates drop to 52 percent for the treatment group and 55 percent for the control group after the ACA's passage. All means for control variables and standard deviations are similar across time periods and especially among demographic variables. The difference in college graduates is notable: almost no one in the treatment group has a college degree, while over 10 percent of the soldiers in the control group do. This difference is understandable since the control group is older.

5 Econometric Methods

We estimate the causal effect of the ACA's dependent mandate using the following difference in differences model:

$$Reenlist_{igst} = \beta_1 Treat_g + \beta_2 PostACA_t + \beta_3 (Treat_g \times PostACA_t) + X_{igst}\beta + \zeta_s + \varepsilon_{igst} \qquad (1)$$

where *Reenlist* is whether individual, i, of treatment group, g, with home state of record state, s, in year t reenlists for a second contract. $Treat_g$ is a dummy variable that indicates whether a soldier is in the 23–25 year old treatment group. $Post_t$ is a dummy variable that indicates whether the soldier's enlistment contract expires after the ACA's enactment.[7] β_3 is the difference-in-differences coefficient of interest that estimates the causal effect of the dependent mandate on the treatment group's reenlistment rates.

The vector X_{igst} contains exogenous demographic and military controls such as gender, race, and education level. All these factors influence the soldier's opportunity cost of staying in

[7] In Section 7.2, we follow the logic of Antwi et al. (2013) and separate the enactment and implementation of the ACA. We find our results are robust to this specification.

the army. We also include various military-related controls such as branch and rank. Occupational branches within the army are assigned when soldiers first enlist and determine the type of work a soldier will do within the army. Jobs range from frontline combat tasks—such as infantry, armor, or field artillery—to cyber operations and logistical supply chain support. A soldier's branch may influence his labor market opportunity outside the army because the human capital and skills that soldiers receive in each branch vary.

We also include state home of record fixed effects (ζ_s) in our model because states have varying policies on whether a young adult could return to her parents' health insurance.[8] Also, there is some variation in the maximum age limit for a young adult to be eligible to be covered. Since many veterans return to their home state of record, we want to control for any variation in labor market opportunities. We also choose to cluster at the state level because of the various state-level policies that could explain enlistment and reenlistment. These policies include generosity in public health insurance and opportunities for higher education. We feel that if these policies are creating a correlation between soldiers from the same state, then clustering at the home state level would be appropriate.

Finally, one threat to our identification could be the use of bonuses to encourage soldiers to reenlist, particularly those that the army would like to retain. Borgschulte and Martorell (ming) and Greenstone et al. (2017) show that soldiers are quite responsive to bonus, especially when faced with economic downturns or the in the face of increased mortality risk. Swings in reenlistment could be caused by changes in bonuses that could have happened contemporaneously to the enactment of the ACA. Given monthly recruiting targets, bonuses are equal for any soldier of the same rank, branch, and year-month of contract expiration. To understand what effects bonuses may have on our findings, we estimate an alternative model to the one above by including rank by branch by year-month fixed effects. Figure 2 shows that even at this cell size, there is considerable variation in the percentage of soldiers in our treatment group.

[8] Illinois has a provision that allows young veterans to return to their parents' health insurance up to age 30.

6 Results

Table 3 presents the difference-in-differences estimates for the effects of the ACA's passage on the enlistment rates of soldiers aged 23–25. We use several models that include various soldier characteristics to analyze the interaction coefficient's stability and ensure that our estimates are not dependent on other factors that may affect soldiers reenlistment decisions. Following a discussion of our results, we conduct several placebo tests and robustness checks to show that our results are stable to alternative specifications and events independent from the ACA's passage.

6.1 Primary Results

Table 3 shows the results for four models that include various controls. Column 1 is a difference-in-differences estimator with no exogenous controls. Column 2 introduces state home of record fixed effects to account for any state-level policies that may affect health care coverage or a soldier's reenlistment decision. Column 3 adds the vector of demographic and military controls. This model controls for the soldier's race, gender, education level, rank, and occupational branch. Finally, Column 4 addresses concerns about the influence of bonuses on reenlistment rates by including branch by rank by year-month fixed effects.

All regressions in Table 3 are conditioned on whether a soldier had a choice to reenlist in the army at the end of their contracts. The interaction term in our model provides the causal estimate of the dependent mandate's effect on the reenlistment decisions of soldiers younger than 26. Column 1 shows a baseline estimate of -0.026, interpreted as a 2.6 percentage point decrease in reenlistment rates for the treatment group. The coefficient on the interaction is highly stable across the first three models, decreasing only by 0.2 percentage points across in the third column with the addition of exogenous controls. Since roughly 52 percent of our sample reenlists when offered, our results translate into a 5 percent decrease in the reenlistment rate because of the dependent mandate in the ACA. If we adjust our results for potential changes in the bonus structure with the addition of branch by rank by year-month fixed effects, our estimates shrink to

a coefficient of -0.017. This estimate represents a 1.7 percentage point reduction in the probability of reenlistment or 3.2 percent. While the magnitude of the coefficient does decrease, it remains statically significant.

The other advantage to using branch and rank fixed effects is that according to Army doctrine, soldiers of the same rank, occupational branch, and term of service should be treated equally. the army uses this principle to decide who should deploy, which soldiers should be let go during a troop drawdown, any changes in recruitment standards, and which incentives to offer during a troop build up. These controls, combined with our specification with branch by rank by year-month fixed effects should control for any of these fluctuations. Thus, in each specification, we are comparing soldiers of the same rank and same occupational branch, so any of these troop level changes or probability of deployment should not bias our results.

6.2 Heterogeneous Treatment Effects

One limitation of our data is that we cannot observe the health insurance status of a soldier's parents.[9] Therefore, we conduct subsample analysis across various demographic groups whose parents are most likely to have health insurance. We test for heterogeneous treatment effects across gender, race, and education levels to examine whether specific populations within our data respond differently to the ACA dependent mandate, since racial minorities and lower-income families are less likely to be covered by employer-sponsored insurance. (Lillie-Blanton and Hoffman, 2005; Card et al., 2008), and Breslau et al. (2018) show that white parents are more likely to have health insurance than black parents. Higher levels of education may allow individuals to use their parents' health insurance to pursue better options outside the military (Courtemanche et al., 2016; Grossman, 1972). The opportunity cost of reenlistment may also be

[9]This data limitation is not confined just to our data, it is present in other data sources that researchers have used to examine the effects of the ACA. For example, the American Community Survey only asks about the source of the individual's health coverage (i.e., employer-sponsored, publicly provided, or from the military). Interestingly, the American Community Survey treats TRICARE as employer-sponsored health insurance while the Current Population Survey treats TRICARE as publicly provided. Holder and Day (2017) use the American Community Survey to show that after Congress passed the ACA, younger veterans are less likely to be uninsured and more likely to be covered by employer-sponsored health insurance.

lower for soldiers with lower education levels since fewer opportunities are available to them outside the army. To test for results specific to these populations we estimate regression models with all controls and use subsample analysis with regards to race, gender, and education level.

First, we reestimate our model given a soldier's AFQT scores. The AFQT is a helpful measure of a soldier's cognitive ability and can be a good indicator for whether the soldier's parents have health insurance (since high ability, young workers may come from a background with higher-ability parents) and the extent of a soldier's options outside the military. Military occupations that have high value in the civilian workforce are generally reserved for those soldiers enlisting with a higher AFQT score. Table 4 displays results from reestimating our primary model for soldiers with AFQT scores below the 50th percentile, above the 50th percentile, and above the 75th percentile. We find that soldiers with higher cognitive ability are more willing to exit the army when they have the opportunity to use their parents' health insurance. For soldiers whose AFQT scores are below the median we find a small but statistically insignificant increase in reenlistment after the passage of the ACA. However, when we consider soldiers above the median AFQT score, reenlistment drops by 2.4 percentage points, or 4.42 percent. The magnitude of the effect increases as AFQT scores increase. When we consider only soldiers with an AFQT score of greater than the 75th percentile, we estimate a 3.9 percentage point decrease in reenlistment, translating to a loss of 7.19 percent of very high ability soldiers. However, it should be noted that these two coefficients are not statistically significant from each other.

Table 5 displays the results of our primary regression model with all controls using race, gender, and educational subsamples. The interaction term's coefficient shows the responsiveness to the ACA's dependent mandate within each subpopulation. Conditioning on race only provides a statistically significant result of a decrease of 3.20 percentage points in the probability of reenlistment for white soldiers, although the coefficient estimate for Hispanics is almost identical but not statistically significant. The coefficient for black soldiers shrinks to a -0.016 and is no longer statistically significant. Along gender lines, the confidence intervals for the estimates for male and female soldiers overlap enough that we cannot claim that the coefficients are statistically

different from each other. However, the estimates do suggest that the soldier's education level will affect their responsiveness to the dependent mandate. Specifically, they suggest that individuals are more responsive to the dependent mandate as their educational attainment increases. When we limit the regression to only students with at least some college experience, the effect is a 4.1 percentage point decrease in reenlistment rates, while among high school graduates the effect is only 1.3 percentage points. These results should be treated with caution, however, because there are many reasons that white males with higher education attainment may leave the army at higher rates other than access to their parent's health insurance (for example, better labor market opportunities or college access).

7 Placebo and Robustness Checks

7.1 Placebo Tests—Examining Year and Age Effects

We conduct placebo tests to ensure our result is only identified in the presence of the policy change and the treatment group. These placebo tests are designed to determine whether there are contemporaneous effects with the years and age brackets drive our results. Threats to our identification include contemporaneous policies such as changes in troop levels due to the military draw-down in Iraq and Afghanistan or macroeconomic trends. One could also claim that younger soldiers are always less likely to reenlist, causing age to drive our results. We address these concerns by constructing multiple placebo regressions. First, we change the enactment year to 2008 and only use data from 2006–2009, years before the passage of the ACA. Second, we code 27–30-year-olds as "treated" and 32–34-year-olds as "control" to follow the method proposed by Slusky (2017).[10] Last, we compare soldiers aged 19–20 with those aged 21–22. The logic behind these placebo tests is that if just the year or age range drive our results, and not the union between the two, then it is highly unlikely that our results are the product of the dependent

[10]Barbaresco et al. (2015), Depew (2015), and Abramowitz (2016) all use a similar placebo method to test the robustness of their respective estimates.

mandate of the ACA as opposed to trends not related to the policy.

The results from the placebo tests are shown in Table 6. First, we show that our results are not a product of comparing early years (when combat activity was highest) against later years (when combat activity and deployments had slowed) by limiting our sample to observations from 2009 to 2012, with years post 2010 as our treatment. This specification also ensures that we have similar power in our baseline analysis with our pre-ACA placebo test. We estimate that the dependent mandate reduced reenlistment rates by 2.5 percentage points for those eligible for their parents' health insurance. Next, we consider years strictly before 2010 to see if our result is primarily driven by age differences and not the ACA. We have a few observations before 2006 but do not include them in this robustness check for fear of having compositional changes in the types of enlistees before and after the terrorist attacks of 2001. We split these four years evenly, using years after 2007 as our "treated" years. We find that our placebo policy year had no effect on reenlistment rates, giving evidence for our primary results. Third, we consider older soldiers only and pretend that soldiers aged 27–30 were treated by the ACA and use soldiers aged 32–34 as the control. We find that the placebo age requirement "caused" a 0.4 percentage point, statistically insignificant increase in the reenlistment rate. Finally, we compare soldiers aged 18–20 to those 21–22 and pretend that 18–20-year-olds were eligible for the dependent mandate and those 21–22 were not. One limitation of this test is that there are very few soldiers in the "treatment" group compared to the baseline results. Since we are considering reenlistment and the average enlistment contract is four years, the only soldiers who would be in our treatment group are those who joined the army when they were either 17 or 18 and just graduated from high school and agreed to a two-year enlistment contract. Also, the bulk of this treatment group (77.53 percent) is 20 years old. For these reasons, this placebo treatment group is very different from the 23–25-year-olds in our baseline specification. However, for completeness, we estimate this placebo test and find that soldiers who are 18 to 20 years old at the end of their contracts are 5.7 percentage points *more* likely to reenlist (but not statistically significant), showing that we cannot replicate our baseline findings for 23–25-year-olds using data from younger soldiers.

7.2 Robustness Check-Enactment and Implementation of the ACA

One question from the ACA literature is how to handle 2010 since Congress passed the ACA in March of 2010 and implemented the dependent mandate in September of 2010. To ensure, that we treat 2010 correctly and ensure that this transition year is not biasing or driving our estimates, we conduct two robustness checks in line with Antwi et al. (2013). In our data, we can see the exact day that the soldier's enlistment contract expired, allowing us to explore the effects of the timing of enactment and implementation of the ACA. First, we drop 2010 from our sample. Second we follow the example of Antwi et al. (2013) by estimating:

$$\begin{aligned} Reenlist_{igst} = \quad & \gamma Treat_g + \sigma Enact_t + \delta Implement_t + \\ & \beta_1(Treat_g \times Implement_t) + \beta_2(Treat_g \times Enact_t) \quad + X_{igst}\beta + \zeta_s + \varepsilon_{igst} \end{aligned}$$

where $Enact_t$ is an indicator for whether a soldier's contract expired after Congress passed the ACA but before the dependent mandate was implemented, and $Implement_t$ is an indicator for whether the soldier's enlistment contract expired after the date that the parental mandate took effect.

Table 7 displays the results from both of these checks. As a reference, we include the baseline results in Column (1). Column (2) displays results for the baseline specification with the exception that we have omitted all enlistment contracts that expired during 2010 from our sample. We find that omitting 2010 does not affect our results in any significant way. Column (3) shows the estimates for splitting 2010 into pre-ACA, postenactment, and postimplementation periods. We find that the period after the dependent mandate's implementation is driving our baseline results, while the coefficient associated with the time period between enactment and implementation is slightly smaller in magnitude and statistically insignificant. These results are in line with estimates by Antwi et al. (2013).

7.3 Robustness Check-Controlling for Youth Unemployment, War on Terror Casualties, and Medicaid Expansion States

One concern with our baseline estimates is that Congress passed the ACA in 2010 during the Great Recession and the tail-end of the surge in Iraq and the beginning of a similar troop surge in Afghanistan. These two events could explain the drop in reenlistment for younger soldiers after 2010 instead of the availability of health insurance through the soldier's parents. Also, the passage of the ACA made funds available from the federal government to states to expand Medicaid. While most states did not expand Medicaid until after our sample window, Medicaid expansion is a good proxy for the presence of other publicly provided health insurance programs. To address these concerns, we merge the annual, state level, average unemployment rate for workers aged 18–25 and the total number of troop casualties in Iraq and Afghanistan (as a proxy for combat intensity and danger) to our data. We also created an indicator for whether the soldier's state home of record expanded Medicaid. We include these three indicators with our baseline specification and additionally interact the treatment effect with these indicators to allow for the possibility that younger soldiers were more highly influenced by any of these events.

Table 8 presents results for controlling for the unemployment rate for younger adults and for troop causalties from Iraq and Afghanistan. In Column (1), we include the annual, average unemployment rate for each year for workers aged 18–25. We find that while the unemployment rate is positively correlated with reenlistment, the estimate is imprecise and has no effect on our baseline findings. In column (2) we interact the treatment group with the unemployment rate to allow for the possibility that younger soldiers react differently to increased unemployment than older soldiers and find similar results. Next, we add combat-related causalities from the wars in Iraq and Afghanistan to control for decreased reenlistments due to increased risk. Column (3) displays the results from this specification. We find that annual combat deaths had no effect on reenlistment rates, and the addition of an interaction term with our treated age group interacted with combat deaths only increased the magnitude and precision of our main estimate. Finally, Column (5) indicates whether the soldier is from a Medicaid expansion state. We find that neither

Medicaid expansion nor its interaction with the treatment group affects our results.

7.4 Robustness Check- Pre-ACA Parental Mandate States

Finally, we consider whether state-level dependent mandates that existed before the passage of the ACA affected our results. Gamino (2018) shows that failure to consider preexisting, state-level mandate laws can bias results. He also shows that there exists large variation in the generosity of these laws and the timing of their enactment. For example, many states have different maximum ages that a child can remain on their parent's health insurance, and many states vary regarding whether the child has to be single, a full-time student, or disabled. Pertinent to this study, Illinois allowed veterans leaving the military to remain on their parent's health insurance plans as long as they were under the age of 30. We conduct a similar robustness check as Gamino (2018) and Antwi et al. (2013) by dropping states with preexisting laws; the only difference is we code states with laws that require a dependent to be a full-time student or disabled as a zero since these laws would not apply to active duty personal considering exiting the military. However, our results are similar when we use the same states as the previously cited literature.

Table 9 displays results from each of these robustness checks. Column (1) displays the baseline model for comparison. In Column (2), we drop all soldiers from Illinois since before the ACA, veterans could return to the parents' health insurance. We find that dropping Illinois only slightly increased the magnitude of our result without affecting precision. Next, we drop states that had preexisting dependent mandate laws to ensure that our results are not driven by soldiers from these states. Column (3) shows the results from this specification. We find that our baseline result does shrink to a decrease of 2.0 percentage points, but this result is still statistically significant. Column (4) shows our specification using a subsample of soldiers only from states that had preexisting laws. Here, we find that our ACA effect increases such that the ACA decreased reenlistment by 3.0 percentage points. However, when comparing the two specifications, neither coefficient is statistically significant. One reason that our effect is found in both the subsample with or without states with preexisting laws is that soldiers are not necessarily

(and in most cases are not) living in their home states of record. Active duty military can also declare residency in either their home state or any state they have lived in while serving in the military. The active duty military population therefore is probably less influenced by preexisting state laws than other populations studied in the literature.

8 Results for Post 9/11 GI Bill Uptake

Our results show that the dependent mandate made it more difficult for the army to retain young adults. While this result may show that recruitment and retainment costs may increase for the military (and perhaps other firms), it is unclear whether the portability of health insurance will lead veterans to better long-run outcomes. Since the passage of the ACA is still relatively recent, it will be difficult to measure its effects of some outcomes such as career satisfaction and mid-career wages. One outcome that could be a good proxy for an enhanced career path is whether a soldier attends college. Since most enlisted soldiers have only high school diplomas, college attendance would indicate whether the availability of health insurance increases education opportunities and thus, arguably, increased labor market outcomes.

One good indicator of college enrollment among veterans is whether the veteran used her Post 9/11 GI Bill benefit. Signed by President George W. Bush in 2008, the Post 9/11 GI Bill was one of the largest expansions of federal financial aid. It eliminated the need for veterans to opt into the GI Bill and removed requirements of service members to make contributions to the GI Bill while on active duty. The bill also made every military member who had served at least three years after September 11, 2001, eligible for four years of college tuition including a monthly stipend and housing costs. Also, the Post 9/11 GI Bill allowed veterans to transfer these benefits to a spouse or children.

We test this hypothesis by merging our sample with data from the VA (which administers the GI Bill) and reestimate the same specification as before. In these data, we can observe which soldiers used the GI Bill and the amounts that the VA paid on their behalf to institutions. We code

GI Bill usage as a one if the VA expended positive amounts of aid and zero otherwise. As before, we include various demographic information and state fixed effects to control for state-level, time in-varying characteristics such as scholarships for veterans or generous merit aid programs.

We use the same sample as in our previous estimates to avoid selection and compositional effects of those soldiers leaving the army. Soldiers who are active duty can still enroll in college and use their Post 9/11 GI Bill; however, we worry that conditioning our sample on those leaving the service may cause the interpretation of our results to be unclear. We do not want to extend our causal claims too far and present these results for the Post 9/11 GI Bill as descriptive.

Table 10 contains estimates for the effect of the dependency mandate on Post 9/11 GI Bill usage. Column (1) displays a baseline regression with only an indicator for a soldier between 23 and 25 years old, a dummy variable for the policy change, and the interaction effect. Using this specification, we estimate that soldiers who have access to their parents' health insurance are one percentage point more likely to use their GI Bill benefits. In our sample, we observe that 53 percent of soldiers use these benefits, so our point estimates translate to around a 2.1 percent increase. Columns 2–4 show that our estimate is robust to adding state home of record fixed effects, exogenous demographic controls, rank fixed effects, and occupational branch fixed effects.

While longer-run outcomes would be helpful, these outcomes are currently not feasible since the ACA is still relatively new. However, our results show that the ACA did encourage more soldiers to attend college. These results are also helpful for assessing the social welfare of the dependent mandate of the ACA. Since our job lock results show that the ACA did reduce the number of soldiers remaining in the army, it does appear that the policy change helps veterans attain more education after leaving. Thus the dependent mandate (and the corresponding increased portability in health insurance) may increase recruiting and retention costs for employers, but increase potential wages and educational outcomes for the worker.

9 Discussion

In this paper, we estimate the effect of the dependent mandate of the ACA on job lock in the U.S. Army. By comparing young soldiers aged 23–25 who are eligible to return to their parents' health care plans to those just barely above the maximum age cutoff, we show that the ACA reduced reenlistment by 2.4 percentage points, or around 4.4 percent. After controlling for possible reenlistment bonuses, this magnitude drops to 1.7 percentage points, or around 3.13 percent. This reduction in reenlistment is more dramatic for those soldiers who are white, with higher AFQT scores, or at least some college experience.

One question regarding this study is, How applicable are these findings to the general population, particularly low income, less healthy, and underrepresented minorities whom job lock might affect the most? While everyone in our sample is healthy enough for military service (since they were all offered reenlistment), the soldiers in our sample are less white and more male than the U.S. population. Also, enlisted soldiers in the army tend to come from lower socioeconomic backgrounds (Kleykamp, 2006). Thus our sample could be seen as a lower-bound estimate because the presence of job lock among a younger, healthy population would only be greater among workers with illness or other challenges. If we can find job lock in a population of workers that are healthy and enjoy generous health benefits, then it is probable that job lock exists in populations that are less healthy and thus more dependent on maintaining employer-based health insurance.

Our results are similar, however, to other papers that use a more general population. Antwi et al. (2013) find that the probability of working full time drops by 2.21 percentage points, and the number of hours worked also drops by 4.75 percent after the implementation of the ACA. These results show that our findings are not too far removed from the previous literature, and are somewhat lower when compared to a paper that uses a more representative sample like the Survey of Income and Program Participation. However, our findings are helpful for future research in this area because the nature of the enlistment contract and the nature of Army allows us to avoid some potential concerns of endogeneity and clearly identify supply effects as opposed to an employer

reducing its workforce because of increased health care costs as a result of the ACA.

Another difference between a military and civilian population is risk tolerance. There is actually a significant literature regarding the risk preferences and discount rates of service members and their civilian counterparts. While active duty, military members are less risk averse and have higher discount rates than civilians Warner and Pleeter (2001); Simon et al. (2015), a soldier's risk preferences do follow general population trends. Bell et al. (2018) solicit risk preferences in a variety of hypothetical scenarios among West Point cadets. They find that West Point cadets exhibit risk-averse behavior, particularly when considering financial risk. These results could imply that military members are still risk averse regarding their own health and financial well-being. Ault (2003) argues that risk aversion has been growing in the military and could actually prove to be detrimental in future conflicts. Overall, however, military members are less risk averse than civilians, so we would expect that our results may be smaller than civilians with similar incentives and could be considered a lower bound when compared to the general population.

10 Conclusion

The ACA's dependent provision was meant to increase social welfare and decrease financial threats to young adults (Goldman, 2013). While research shows that insurance coverage among young adults increased, it is less clear that this coverage improved health care utilization and outcomes (Barbaresco et al., 2015). In this study, we estimate the effect of the dependent mandate on job switching by using the unique characteristics of the U.S. Army's reenlistment process. This scenario allows us to avoid certain sources of endogeneity such as unobserved health status, variation in salary and health insurance generosity, the timing of job changing, and differentiation or labor demand or supply shocks. We avoid these potential pitfalls since all soldiers in our sample have passed rigorous physical fitness qualifications, are paid equally conditional on rank and occupational branch, are covered by TRICARE, set contract expiration dates before the

passage of the ACA, and have been offered the opportunity to reenlist.

We employ a difference-in-differences identification strategy that compares enlisted soldiers aged 23–25 with those aged 27–29 and who are completing the first enlistment at the ranks of E1 to E5 before and after President Obama signed the ACA in 2010. We find that the ACA's dependent mandate decreases soldier reenlistments among 23–25-year-olds by 4.4 percent. We also find that these results are strongest among populations whose parents are most likely to have health insurance and among soldiers with the highest opportunity cost of reenlistment such as those with some college experience. For robustness, we include branch by rank by year-month fixed effects to control for changes in reenlistment bonuses. After these controls, our estimates show a reduction of 1.7 percentage points or 3.13 percent. Given our sample size of 137,958 enlistment contracts, our results suggest that 3,255 soldiers left the army when given access to their parents' health insurance via the ACA.

While the army has a very distinct labor structure and attracts a different type of worker than traditional firms, our findings show that the ACA did increase turnover rates among workers which can be costly to the firm. We show that an economically significant number of soldiers, whom the army wants to retain, left because of outside access to health insurance. Beyond losing valuable human capital, the army may also expend resources to train new recruits. These results show that the army needs to adjust its appeal now that health insurance is no longer a strong selling point to many young adults. While the dependent mandate was intended to improve social welfare among young Americans, it has unintended consequences for the Department of Defense that affect the nation's national security and budgetary considerations. Firms in the private sector will arguably face similar recruitment and retention costs.

While our results may be discouraging for the army, they may be positive for individuals and for the labor market as a whole. We provide evidence that the ACA decreased labor market frictions from job lock. To test whether portability of health insurance is welfare enhancing for the individual, we use data on Post 9/11 GI Bill usage as a proxy to whether a veteran attended college after leaving the army. We find that younger veterans who have access to their parents'

health insurance are 3 percent more likely to use the Post 9/11 GI Bill. These results, combined with the 3.13 percent reduction in soldiers remaining in the army, show that the dependent mandate may be costly for employers, with increased turnover and decreased retention. However, the policy change may be beneficial for workers who may attend college at higher rates and pursue better job matches and more satisfying career paths.

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Table 1: Summary Statistics

	Mean	Standard dev	Min	Max
Reenlisted	0.54	0.50	0.00	1.00
E1	0.00	0.07	0.00	1.00
E2	0.01	0.10	0.00	1.00
E3	0.08	0.27	0.00	1.00
E4	0.68	0.47	0.00	1.00
E5	0.23	0.42	0.00	1.00
Age	24.86	1.89	23.00	29.00
White	0.67	0.47	0.00	1.00
Black	0.15	0.36	0.00	1.00
Hispanic	0.12	0.33	0.00	1.00
Male	0.87	0.34	0.00	1.00
GED	0.13	0.34	0.00	1.00
High school	0.74	0.44	0.00	1.00
Some college	0.07	0.25	0.00	1.00
College grad	0.03	0.17	0.00	1.00
Graduate degree	0.00	0.03	0.00	1.00
Ever married	0.48	0.50	0.00	1.00
No. of dependents	0.96	1.16	0.00	10.00
Observations	146,458			

NOTE: Data come from the Office of Economic and Manpower Analysis and reflect soldiers aged 23–29 whose first enlistment contracts expired between 2007–2013, and who the army offered reenlistment.

Figure 1: **Reenlistments from Sample 2007–2013.** This figure shows the parallel trends of reenlistment around the policy change. Our treatment group is soldiers aged 23–25 and our control group contains soldiers aged 27–29. This figure shows that reenlistment rates were very similar before the passage of the Affordable Care Act, but diverged afterward.

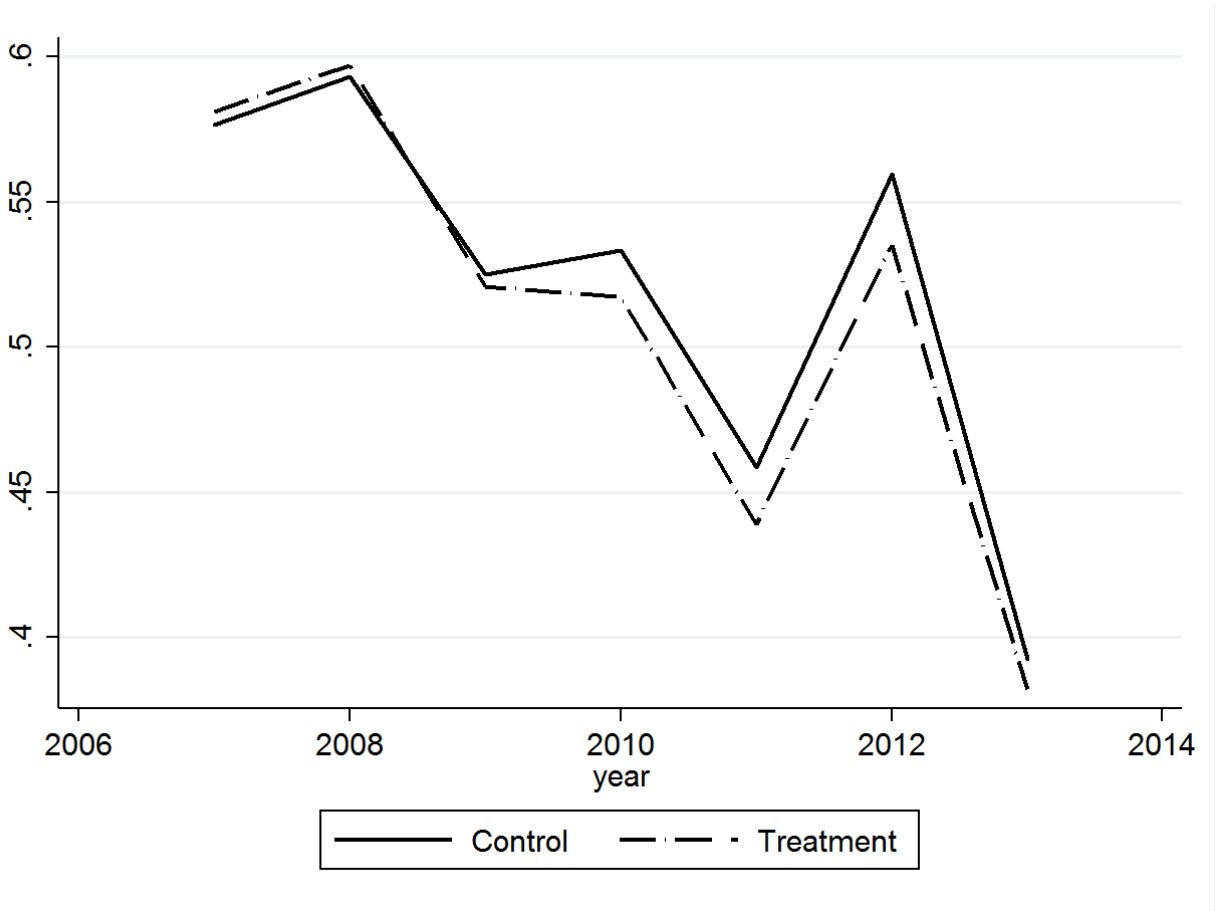

Table 2: Covariate Balance across Treatment and Control Groups

Variable	Pre-ACA			Post-ACA			Final
	23–25 years old	27–29 years old	Diff.	23–25 years old	27–29 years old	Diff.	Diff.
	(1)	(2)	(2) - (1)	(3)	(4)	(4) - (3)	(4 - 3) - (2 - 1)
Reenlistments	0.56	0.56	0.00	0.52	0.55	0.03	−0.03
	(0.49)	(0.49)		(0.50)	(0.50)		
Age	23.86	27.81	3.95	23.85	27.8	3.95	0.00
	(0.80)	(0.80)		(0.80)	(0.80)		
Male	0.87	0.87	0.00	0.87	0.87	0.00	0.00
	(0.33)	(0.33)		(0.34)	(0.34)		
White	0.69	0.68	−0.01	0.67	0.66	0.00	0.00
	(0.46)	(0.47)		(0.47)	(0.47)		
Black	0.13	0.13	0.00	0.16	0.16	0.00	0.00
	(0.34)	(0.34)		(0.37)	(0.37)		
Hispanic	0.13	0.12	−0.01	0.12	0.12	−0.01	0.00
	(0.33)	(0.33)		(0.33)	(0.32)		
College graduate	0.00	0.11	0.10	0.00	0.10	0.10	0.00
	(0.06)	(0.31)		(0.07)	(0.30)		
High school graduate	0.77	0.57	−0.20	0.79	0.58	−0.21	−0.01
	(0.42)	(0.50)		(0.41)	(0.49)		
AFQT score	59.25	64.81	5.56	58.49	63.66	5.17	−0.39
	(19.27)	(20.98)		(19.03)	(20.33)		

NOTE: Columns 1, 2, 3, and 4 display means for treatment and control groups in the pre and posttreatment periods with standard deviation in parentheses. Final "difference" column reports the differences in the differences in means for groups aged 23–25 and 27–29 over the period of 2008–2012. The first time period is prior to the Affordable Care Act (2008–2009) and the second is after the Affordable Care Act (2011–2012). We exclude 26-year-olds to create a clear separation between treatment and control groups.

Figure 2: **Histogram of Branch by Rank by Year-Month Fixed Effects** This figure shows variation in percentage of soldiers in each branch by rank by year-month fixed effects who were aged 23–25, meaning that they are considered "treated" in our quasi-experimental design. This variation is important to show that we can still identify an effect even with this very specific fixed effect.

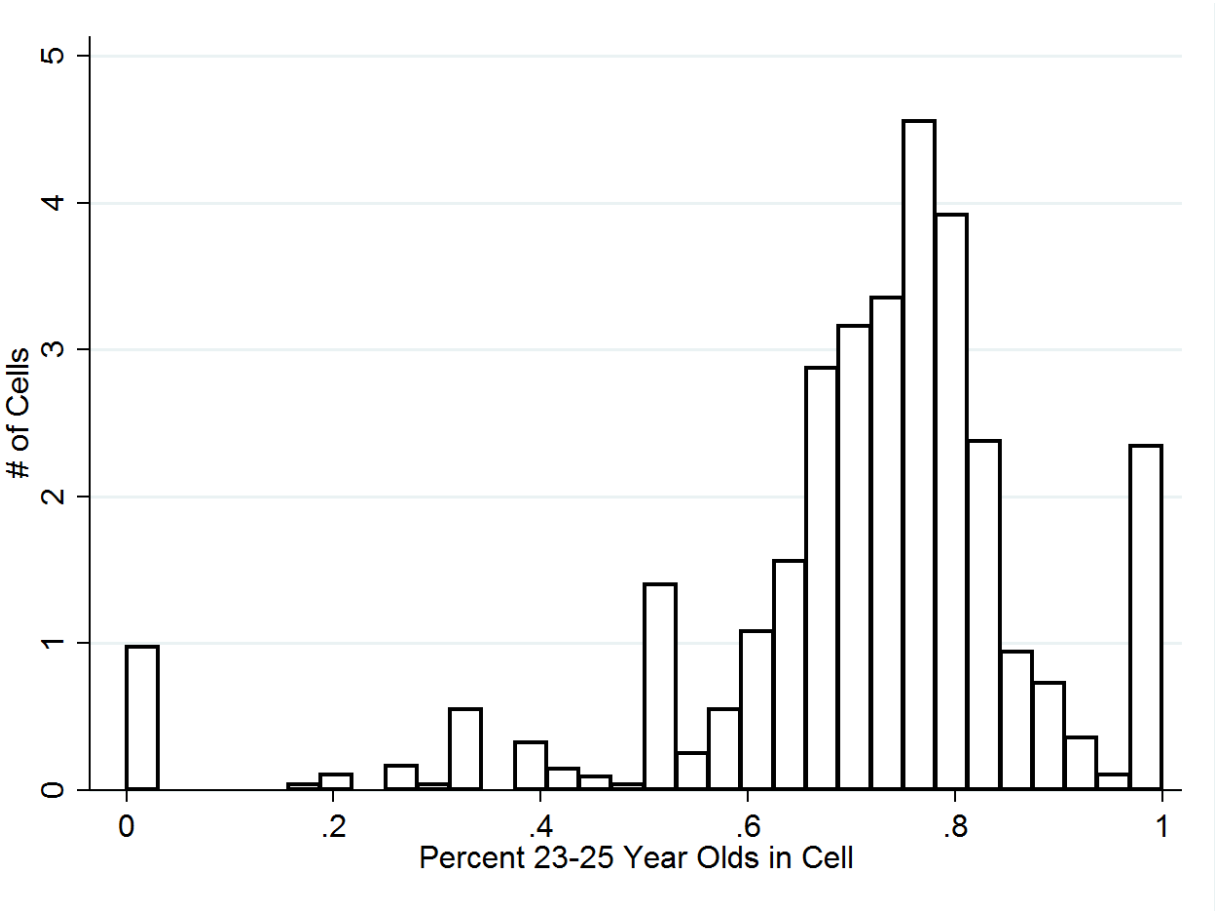

Table 3: Regression Results for Soldier Reenlistment

Variable	(1) Reenlisted	(2) Reenlisted	(3) Reenlisted	(4) Reenlisted
23–25 years × Post ACA	-0.026^{***}	-0.026^{***}	-0.024^{***}	-0.017^{***}
	(0.006)	(0.006)	(0.006)	(0.006)
State FEs	No	Yes	Yes	Yes
Exog. controls	No	No	Yes	Yes
Rank by branch by month FEs	No	No	No	Yes
Observations	138,216	138,216	138,040	137,958
R^2	0.006	0.022	0.070	0.196

Standard Errors Clustered at the State Home of Record Level

$^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

NOTE: Regression results in the interaction row estimate the dependent mandate's effect on reenlistment rates for soldiers aged 23–25 after the law passed in March, 2010. Demographic controls include ethnicity, gender, marital status and number of dependents. Education controls are for level of education attained. Army controls include soldiers' rank, contract terms, and occupational branch. In our sample, 54.27 percent of soldiers reenlist conditional on the army offering reenlistment.

Table 4: Regression Results for Soldier Reenlistment by AFQT Score

	(1) AFQT $<$ 50th Reenlisted	(2) AFQT$\geq$50th Reenlisted	(3) AFQT$\geq$75th Reenlisted
23–25 years $\times$ post ACA	0.012	-0.024^{***}	-0.039^{***}
	(0.011)	(0.007)	(0.011)
State FEs	Yes	Yes	Yes
Exog. controls	Yes	Yes	Yes
Observations	44,443	90,728	35,018
R^2	0.219	0.159	0.178

Standard Errors Clustered at the State Home of Record Level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

NOTE: Regression results in the interaction row estimate the dependent mandate's effect on reenlistment rates for soldiers aged 23–25 after the law passed in March 2010. Demographic controls include ethnicity, gender, and education level. In our sample, 54.27 percent of soldiers reenlist conditional on the army offering reenlistment. We use data from 2007–2013 unless otherwise noted.

Table 5: Heterogeneous Treatment Effects

Variable	(1) White	(2) Black	(3) Hispanic	(4) Male	(5) Female	(6) Some college	(7) High school
Post-ACA $\times$	-0.032^{**}	-0.016	-0.031	-0.023^{**}	-0.022	-0.041^{**}	-0.013^{*}
23–25 years	(0.007)	(0.014)	(0.016)	(0.006)	(0.016)	(0.016)	(0.006)
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exog. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean reenlist	0.504	0.700	0.563	0.540	0.558	0.575	0.530
R^2	0.079	0.046	0.081	0.106	0.082	0.093	0.096
N	98,221	21,967	18,103	126,965	19,493	36,091	109,377

Standard Errors Clustered at the State Home of Record Level

*** $<$ 0.01, ** p$<$ 0.05, * p$<$ 0.1

NOTE: Regression results in the interaction row estimate the dependent mandate's effect on reenlistment rates for soldiers aged 23–25 after the law passed in March 2010. Demographic controls include ethnicity, gender, and education level. In our sample, 54.27 percent of soldiers reenlist conditional on the army offering reenlistment. We use data from 2007–2013 unless otherwise noted.

Table 6: Placebo Test Results

	(1) Reenlisted 2009–2012	(2) Reenlisted 2006–2009	(3) Reenlisted 27–30 years vs. 32–34 years	(4) Reenlisted 19–20 years vs. 21–23 years
23–25 years × post ACA	-0.025^{***}			
	(0.009)			
23–25 years × post 2007		-0.000		
		(0.008)		
27–30 years × post ACA			0.004	
			(0.012)	
18–20 years × post ACA				0.057
				(0.049)
State FEs	Yes	Yes	Yes	Yes
Exog. controls	Yes	Yes	Yes	Yes
Rank & branch FEs	Yes	Yes	Yes	Yes
Observations	56,643	66,029	20,190	42,884
R^2	0.089	0.092	0.098	0.176

Standard Errors Clustered at the State Home of Record Level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

NOTE: Regression results in the interaction row estimate the dependent mandate's effect on reenlistment rates for soldiers aged 23–25 after the law passed in March 2010. Demographic controls include ethnicity, gender, and education level. In our sample, 54.27 percent of soldiers reenlist conditional on the army offering reenlistment. We use data from 2007–2013 unless otherwise noted.

Table 7: Robustness-Enactment and Implementation of ACA

	(1) Reenlisted full sample	(2) Reenlisted w/o 2010	(3) Reenlisted enact vs. implement
23–25 years × post ACA	-0.024^{***}	-0.021^{***}	
	(0.006)	(0.007)	
23–25 years × enact			−0.020
			(0.012)
23–25 years × implement			-0.025^{***}
			(0.006)
State FEs	Yes	Yes	Yes
Exog. controls	Yes	Yes	Yes
Rank & branch FEs	Yes	Yes	Yes
Observations	138,043	118,600	138,043
R^2	0.070	0.065	0.071

Standard Errors Clustered at the State Home of Record Level

$^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Note: Regression results in the interaction row estimate the dependent mandate's effect on reenlistment rates for soldiers aged 23–25 after the law passed in March 2010. Demographic controls include ethnicity, gender, and education level. In our sample, 54.27 percent of soldiers reenlist conditional on the army offering reenlistment. We use data from 2007–2013 unless otherwise noted.

Table 8: Robustness-Controlling for Youth Unemployment, War on Terror Casualties, and Medicaid Expansion

	(1) Reenlisted	(2) Reenlisted	(3) Reenlisted	(4) Reenlisted	(5) Reenlisted	(6) Reenlisted
23–25 years × post ACA	−0.024***	−0.024***	−0.024***	−0.030***	−0.024***	−0.024***
	(0.006)	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)
Unemployment	0.107	0.136				
	(0.071)	(0.116)				
23–25 years × unemployment		−0.039				
		(0.090)				
Casualties			0.000***	−0.000		
			(0.000)	(0.000)		
23–25 years × casualties				−0.000		
				(0.000)		
Expanded Medicaid					0.047***	0.052***
					(0.002)	(0.005)
23–25 years × Medicaid						−0.005
						(0.007)
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Exog. controls	Yes	Yes	Yes	Yes	Yes	Yes
Rank & branch FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	135,184	135,184	138,043	138,043	138,043	138,043
R^2	0.069	0.069	0.070	0.070	0.069	0.069

Standard Errors Clustered at the State Home of Record Level

$^{*}\ p < 0.10,\ ^{**}\ p < 0.05,\ ^{***}\ p < 0.01$

NOTE: Regression results in the interaction row estimate the dependent mandate's effect on reenlistment rates for soldiers aged 23–25 after the law passed in March 2010. Demographic controls include ethnicity, gender, and education level. In our sample, 54.27 percent of soldiers reenlist conditional on the army offering reenlistment. We use data from 2007–2013 unless otherwise noted.

Table 9: Robustness-States with Preexisting Dependent Mandates

	(1) Reenlisted full sample	(2) Reenlisted w/o Illinois	(3) Reenlisted w/o Depend. Mandate States	(4) Reenlisted w/ Depend. Mandate States
23–25 years × post ACA	-0.024^{***}	-0.025^{***}	-0.020^{**}	-0.030^{***}
	(0.006)	(0.006)	(0.008)	(0.007)
State FEs	Yes	Yes	Yes	Yes
Exog. controls	Yes	Yes	Yes	Yes
Rank & branch FEs	Yes	Yes	Yes	Yes
Observations	138,043	133,107	85,658	52,385
R^2	0.070	0.070	0.073	0.066

Standard Errors Clustered at the State Home of Record Level

$^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

NOTE: Regression results in the interaction row estimate the dependent mandate's effect on reenlistment rates for soldiers aged 23–25 after the law passed in March 2010. Demographic controls include ethnicity, gender, and education level. In our sample, 54.27 percent of soldiers reenlist conditional on the army offering reenlistment. We use data from 2007–2013 unless otherwise noted.

Table 10: Post 9-11 GI Bill Usage

	(1) PGIB Use	(2) PGIB Use	(3) PGIB Use	(4) PGIB Use
23-25 Years × Post ACA	0.010**	0.011**	0.011**	0.011**
	(0.005)	(0.005)	(0.005)	(0.005)
State FEs	No	Yes	Yes	Yes
Exog. Controls	No	No	Yes	Yes
Rank & Branch FEs	No	No	No	Yes
Observations	146,639	146,639	146,639	146,453
R^2	0.003	0.009	0.024	0.028

Standard Errors Clustered at the State Home of Record Level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Regression results in the interaction row estimate the Dependent Mandate's effect on reenlistment rates for soldiers ages 23-25 after the law passed in March, 2010. Demographic controls include ethnicity, gender, and education level. In our sample, 54.27 percent of soldiers reenlist conditional on the army offering reenlistment. We use data from 2007 to 2013 unless otherwise noted.