

ASSESSING FINANCIAL EDUCATION: EVIDENCE FROM A PERSONAL FINANCIAL MANAGEMENT COURSE¹

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Abstract: This research estimates the effects of financial education on a variety of economic outcomes using a large natural experiment within the U.S. Army. I find that Personal Financial Management Course (PFMC) attendance and enrollment assistance doubles retirement savings, with significant effects throughout the contribution distribution that persist through at least two years. The course has smaller but suggestive effects on credit market outcomes including combined account balances and aggregate monthly credit payments in the first year after soldiers finish their initial job training. The PFMC has no significant effects on military labor market outcomes including measures of performance, productivity and reenlistment early in their service. Overall, the results suggest that financial education coupled with assistance and advice can improve financial outcomes in a number of areas.

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New Evidence on an Important Topic with High Visibility

Financial literacy and education remain popular topics among the media, policy-makers and academics. In the U.S., slow economic growth, increasing personal responsibility for retirement planning and concerns over savings rates have all generated calls for more education. Federal government responses have included President Bush's 2008 Financial Literacy Advisory Council, President Obama's 2009 financial literacy campaign and no less than 16 federal programs among 14 agencies (GAO 2012). Yet there exists little robust scientific evidence that financial education improves individuals' economic decision-making.

The existing research on financial education has struggled to convincingly demonstrate a causal relationship between financial education and financial behavior. The principle challenges to identifying causal effects include the use of self-reported data;² the use of knowledge measures as opposed to behavioral outcomes;³ the potential for household intrabudget reallocations;⁴ and most importantly, the lack of experimental design and evaluation. As a result, endogenous selection may explain many of the positive findings related to financial education since those who select into treatment differ from those who do not along important unobserved dimensions including motivation, cognitive abilities, future orientation, etc....⁵ These endogeneity concerns are pervasive and challenge existing work that relies on education among self-selected individuals (Lusardi 2004, Lusardi and Mitchell 2007); that fails to account for differences in

² Studies relying on self-reports for their program evaluations include Lusardi (2004, 2006) Lusardi and Mitchell (2007), Bell, Gorin and Hogarth (2008, 2009) and Bernheim and Garrett (2003), but self-reports are problematic since individuals overestimate their knowledge (Agnew and Szykman, 2005) and they may be unreliable when compared to administrative data (Collins et. al. 2009).

³ If low levels of knowledge correlate with poor cognitive and non-cognitive skills, then improving knowledge may not improve behavior (Madrian and Shea 2001b, Choi et al 2011). If programs demonstrating advances in knowledge levels through literacy tests or declared intentions do not improve financial behavior (Coussens 2006), then financial outcomes provide better evidence than knowledge measures.

⁴ Bernheim and Garrett (2003) offer such an explanation for their findings that financial education in the workplace correlates with 401(k) accumulation but not overall wealth accumulation. Without knowledge of the fees, taxes, rates of return, etc., it remains unclear if intra-portfolio transfers are welfare-enhancing. Data on multiple portions of an individual or family's balance sheet enable more useful comparisons.

⁵ As examples, see Meier and Sprenger 2007, Hastings and Mitchell 2011.

the financial conditions faced by control and treatment groups over time (Bernheim, Garrett and Maki 2001); or fails to use experimental comparison groups (Bell, Gorin and Hogarth 2008, 2009). As a result, causal interpretations of many existing estimates are inappropriate.⁶

There are a few financial education studies that employ experimental or quasi-experimental procedures, but none provide generalizable evidence on the causal effects of financial education in the U.S. Choi et. al. (2011) find that employer education about 401(k) matches has no significant effects on employee contributions and Duflo and Saez (2003, 2004) find that information on a job benefits fair increases attendance but has marginal effects on subsequent savings.⁷ Cole, Paulson and Shastry (2013) find that State mandates for personal finance classes in high school schools have no effects on financial outcomes. Outside the U.S., there are positive findings in selected contexts including insurance education for farmers in India (Gaurav, Cole and Tobacman 2011) and accounting education for micro-entrepreneurs in the Dominican Republic (Drexler, Fischer and Schoar 2012). See Hastings, Madrian and Skimmyhorn (2012) for a review.

As a result of the extant findings on financial education, a small but grounded opposition, typified by Willis (2008, 2009, 2011), has questioned the efficacy of additional formal education. The modern debate in the U.S. over financial education, especially in academic circles, has become less about financial literacy and more about whether education offers an effective remedy.

⁶ Quasi-experimental studies remain divided on the effects of financial education. Studies suggesting minimal effects include JumpStart (2006) and Mandell (2008) in a high school setting; and Madrian and Shea (2001b) and Choi et. al. (2011) in a workplace setting. Non-experimental studies suggesting beneficial effects include Staten (2006) for credit counseling; Lusardi (2004) and Bernheim and Garrett (2003) for retirement savings; Boyce and Danes (2004) for optional high school programs; Bernheim, Garrett and Maki (2001) for state laws mandating financial education; and Bell, Gorin and Hogarth (2008, 2009) for financial education in the military. Even the most recent reviews and meta analyses of the non-experimental evidence remain divided, with Collins et. al. (2009) and Gale and Levine (2011) suggesting positive effects and Hastings, Madrian and Skimmyhorn (2012) concluding that there exists little robust positive evidence for financial education.

⁷ Experimental approaches confounded by implementation issues include research on education for low and moderate income families (Collins 2010), credit education plans for college students (Gartner and Todd 2005) and financial education coupled with technology assistance (Servon and Kaestner 2008).

I estimate the causal effects of financial education on a variety of important economic outcomes using administrative data covering multiple portions of household balance sheets and the largest quasi-experimental approach to date. Between 2007 and 2008 the Army implemented large scale mandatory eight hour Personal Financial Management Course (PFMC) for new soldiers. Staggered implementation across locations and time provides quasi-experimental variation in course attendance. I find that course attendance and enrollment assistance have large effects on retirement savings contributions in the Thrift Savings Plan, a tax-deferred account similar to a 401(k) in the first two years. Course. To estimate the PFMC effects on other financial choices I use credit bureau data on individuals' combined account balances, monthly payments, legal actions and credit scores. I find that the PFMC has moderately sized beneficial effects in some areas, reducing combined account balances (credit cards, auto loans, finance loans and accounts in an unpaid status) and aggregate monthly payments in the first year. The PFMC has no significant effects on the probability of being active in the credit market or credit scores. Finally, the PFMC has no significant effects on adverse employee turnover, current productivity, or retention decisions, outcomes of interest to private and public sector firms interested in determining if there is a return on their investment. The paper proceeds as follows: Section II describes program implementation; Section III summarizes the data; Section IV provides the empirical framework; Section V presents the results; and Section VI discusses the findings and concludes.

II. The PFMC Provides a Unique Natural Experiment

Between October 2007 and August 2008 the U.S. Army and the non-profit Army Emergency Relief (AER)⁸ implemented a mandatory eight hour financial education course for new enlisted members as part of Advanced Individual

⁸ For information on AER and its mission to assist soldiers and their dependents, see www.aerhq.org.

Training (AIT).⁹ The purpose of the Personal Financial Management Course (PFMC) was “to assist Service men and women and their immediate families in their efforts to building personal wealth through reducing debt and establishing savings goals.”¹⁰ AER developed the course with the assistance of a contractor, San Diego City College (SDCC), and implemented it at thirteen locations.¹¹ In Figure 1, I present the PFMC implementation dates for each location.

[Insert Figure 1 about here]

While PFMC delivery methods might have varied slightly by location, the course was typically conducted in a large classroom or auditorium with 1-2 civilian instructors hired and trained by SDCC. Course delivery combined lectures, Q&A opportunities and informal advice during breaks, which occurred about once per hour. The course was normally taught in two four-hour sessions on separate days. Course content included standardized slides and a course book provided to each student. The course content included both principles (e.g., the time value of money) and rules of thumb (e.g., obtain a copy of your credit report annually). AER and SDCC designed a curriculum focused the financial decisions that young workers are likely to face (e.g. buying a car and managing credit are included; buying homes is not). In Table 1, I present the PFMC syllabus.¹² In one

⁹ All enlisted soldiers attend AIT immediately following basic training where they learn the skills associated with their specific job (e.g., infantryman, vehicle mechanic, cook, radio operator, etc.). AIT courses range in duration from 1-12 months and are typically only offered at one location. Some Army jobs require an intermediate school between Basic Training and AIT (e.g., language school for translators). I omit these atypical jobs from this analysis. See Appendix 1 for more details.

¹⁰ Memorandum of Understanding between DoD and AER for the pilot PFMC dated June 5, 2003. The MOU went on to state that the program goals were focused on soldier welfare (e.g., “Building wealth affords Service members and their families an opportunity to achieve goals such as maintaining an emergency cash reserve, buying a house, or paying for college.”) and military readiness (e.g., “Personal financial management is also seen as an integral part of personal readiness to accomplish the DoD mission. Poor money management skills may cause a Service member more than financial problems and may also interfere with his or her ability to focus on the mission of defending the nation’s interests.”).

¹¹ SDCC delivered pilot training at Fort Bliss, TX from 2003-2006 under a sole source contract. After four years of course refinement, the Army contracted with SDCC to implement the PFMC at all AIT locations.

¹² Whether an eight hour course is sufficient in length to meet the program’s objectives is unclear. On the one hand, this course length seems far too short given the amount of financial knowledge required to succeed in today’s economy. On the other hand, time is often the commodity in shortest supply for schools and more time for financial topics may not be justified if diminishing returns take hold. Schreiner, Clancy and

area (retirement savings) at some locations, the PFMC was coupled with enrollment assistance (i.e., completing forms and submitting them to military finance offices) and as such, the program effects measured here evaluate a bundled intervention.¹³ Prior to the PFMC's implementation, the eight hours were unassigned and were likely leisure hours for new soldiers.

[Insert Table 1 about here]

Several features of the PFMC implementation suggest a potentially valid natural experiment. First, the program was implemented at the thirteen locations across the U.S. at eight different unannounced points in time. This staggered implementation enables identification of the program effects separate from other time factors affecting individual's financial decisions. Second, training implementation dates were determined by Army Emergency Relief Headquarters and San Diego City College based on discussions with local military leaders without notifying or soliciting information from individual soldiers or the U.S. Army's Recruiting Command. As a result, there is little reason to believe that potential enlistees had any knowledge of the PFMC or an ability to change their enlistment timing or their job based on start dates.¹⁴ In addition, the eight hour duration of the PFMC is insignificant when compared to the much longer (1-12 month) AIT course and an even more significant career choice to join the military. As a result, I argue here that conditional on an individual's AIT location, the

Sheradden (2002) found that a financial education program on individual development accounts increased savings for low-income households, but the effects trailed off after 8-10 hours. As a result, a course of relatively short duration may have limited effects on behaviors involving complex combinations of analytic skills, life experience and self-control.

¹³ Author interviews with AER Headquarters, SDCC leadership and instructors at several locations (2011-2012). Enrollment assistance varied by location and time (e.g., at some locations forms were distributed; at others SDCC personnel assisted in forms completion; still at others, SDCC personnel were allowed to submit the forms on behalf of soldiers). Unfortunately, neither AER nor SDCC collected detailed data on exactly when, where and how these various assistance methods were used and separately identifying the effects of the information/education and the assistance is impossible.

¹⁴ Author interviews with AER Headquarters personnel responsible for the PFMC and the SDCC contract leader (2011-2012). Both parties reported that the program implementation timeline was driven by the ability to recruit and train instructors and was unrelated to soldier characteristics at any AIT location. In fact, neither AER nor SDCC had any data on soldier characteristics, further minimizing concerns over non-random implementation on the basis of individual characteristics or financial conditions.

assignment of financial literacy training should be as good as randomly assigned. I discuss and test my identification assumptions in Section IV but upon initial review, PFMC implementation details suggest plausibly exogenous variation.

III. Army Administrative and Credit Bureau Data Enable Several Analyses

This study uses a combination of Army administrative data and individually-matched national credit bureau data to evaluate the effects of financial education on a diverse set of financial and labor market outcomes. I focus my analysis on the outcomes for which the PFMC curriculum focused directly, including retirement savings (e.g., the Thrift Savings Plan), several important credit decisions (e.g., debt levels and payments) and military labor market outcomes (e.g., adverse separations and rapid promotions). I briefly summarize the individual characteristics and outcomes by data source.

The military administrative data is a repeated cross section and covers all active duty Army soldiers entering military service from May 2006-June 2009.¹⁵ I restrict the sample to individuals attending AIT at each location within 12 months of PFMC implementation to minimize time-varying differences in military enlistments. Since AIT durations vary from 1-12 months and micro data on treatment is unavailable, I briefly discuss the implications of potential measurement error. I assume that individuals were treated in the month they began AIT. To avoid contamination between control (pre-PFMC implementation) and treatment (post-PFMC implementation) groups, I omit individuals starting AIT in the month preceding, month of and month following program implementation and individuals whose start date and AIT duration produce

¹⁵ The data was obtained through the cooperation of the Office of Economic and Manpower Analysis (OEMA) at the United States Military Academy, West Point, NY. Individuals missing demographic data and/or non-standard AIT timelines (e.g., linguists and Special Forces soldiers) are omitted.

overlap with the program implementation date.¹⁶ Still, measurement error may be an issue if I assume that individuals receive the PFMC immediately and they actually received it later in their AIT course. If I assign someone treatment as of their AIT start month (e.g., Jan 2007) and begin measuring their outcomes for year 1 in the next month (e.g., Feb 2007) but they do not receive the PFMC until later (e.g., Mar 2007 or Jun 2007) then their early AIT month outcomes levels will be similar to control group members even though I have them recorded as being treated. My treatment imputation strategy is thus conservative and my estimates should serve as lower bounds of the PFMC effects.¹⁷ My sample selection procedures generate an administrative data sample of n=82,211 individuals for my analyses in the first year after AIT (i.e., year 1).¹⁸ Since these individuals progressively leave the military, my samples for years two through four are reduced to n=70,782, n=59,609 and n=44,655 respectively.¹⁹

The military administrative data contains a rich set of characteristics, measured at AIT start, potentially related to financial decision-making including demographic data (age, gender, marital status, number of dependents and race); human capital data (education level, Armed Forces Qualification Test (AFQT) percentiles and enlistment timing); and economic factors (length of AIT and compensation). I also include the number of months deployed overseas during the

¹⁶ Since individual-level data on program participation is unavailable I impute an individual's treatment status using administrative data on individual entry dates, basic training durations, unit assignments and location assignments. For more information on sample selection and imputation of the treatment variable, see Appendix 1. This process leaves room for contamination across control and treatment groups. For example, if a soldier began AIT to become a Food Service Specialist (cook) at Fort Lee in Sep 2007, he would be scheduled to complete his 9 week course in early Dec 2007. Since the PFMC began in Nov 2007 at Fort Lee, it is unclear if this soldier received the training. I omit individuals in these "bridge" cohorts to avoid contamination. Earlier work demonstrated that these cohorts reflected average outcome levels between the control and treatment groups, as would be expected if these cohorts suffered from contamination.

¹⁷ There are several other reasons that my estimates serve as lower bounds, which I discuss in Section VI.

¹⁸ Year 1 outcomes cover individuals from the month after they start AIT through their twelfth month after starting AIT; Year 2 outcomes cover months 13-24, Year 3 outcomes cover months 25-36; and Year 4 outcomes cover months 37-48 after AIT start.

¹⁹ Treatment is unrelated to attrition in years 1-4. See Appendix Table 5 for results.

appropriate horizon (i.e., the number in the first year for year 1 TSP outcomes).²⁰

The data also provides several outcomes of interest in evaluating the PFMC.

Retirement Savings Outcomes

Given that the most significant portion of the PFMC curriculum (2.0 of 8.0 total hours) is dedicated to retirement savings and the Thrift Savings Plan (TSP), I evaluate TSP decisions annually for an individual's first four years in the military. The two primary outcomes are the Average Monthly TSP Savings, which measures the mean monthly contribution levels for the year and the Probability of TSP Participation, which measures any contribution.²¹ Given the importance of the TSP in the curriculum and this analysis, I provide a brief program summary. The TSP is a tax-advantaged retirement program available to federal employees, including the military.²² Initial enrollment must occur via a hardcopy form, subsequent contributions must occur via payroll deduction, changes can be made online or at a finance office, and individuals can take loans against their balances.²³ While military members do not receive matching funds (the military has a separate defined benefit pension), contributions are tax-deferred²⁴ and individuals can select from several fund options (the default is a Government securities index fund).²⁵ Mean control group member participation levels are 12%, 15%, 16% and 17% in years 1-4 respectively. Importantly, while the PFMC presented soldiers with an earlier opportunity to enroll in the TSP, soldiers must opt-in and select the amount that they will contribute. But the PFMC also assisted

²⁰ I omit individuals with incomplete individual characteristic data.

²¹ In Appendix Table 2 I present more detailed analyses of the PFMC effects on the TSP contribution distributions for Years 1 and 2. The positive effects remain large and statistically significant through at least the 90th percentile of the contribution distributions in both years.

²² For additional information on the TSP see: <https://www.tsp.gov/>.

²³ These features and the use of payroll data minimize the chances of unobserved savings or withdrawals.

²⁴ On October 1, 2012 the TSP established tax-free (post income tax) contributions in the Roth TSP Program. None of the sample individuals had access to this option during AIT or during their first several years in the Army. All individuals (control and treatment group) had the opportunity to change their contributions freely after October 1, 2012. Time fixed account for this change and eliminate any impact on my estimates.

²⁵ Fund options include: Government Securities(G), Fixed Income Securities(F), Common Stock(C), Small Cap Stock(S), International Stock(I) and Lifecycle funds(L).

soldiers with form completion and submission and as a result, program effects in this area represent the combined effects of education and assistance.

While I observe administrative data on TSP contribution levels monthly, my view of an individual's retirement savings portfolio remains incomplete as I cannot observe non-TSP retirement savings contributions (e.g., Roth IRAs or other 401(k) accounts). However, the TSP is an important part of many military members' retirement plans, as nearly 31% of Active Duty Army members participate in the TSP.²⁶ The lack of visibility on alternate retirement plans is also less concerning for this young population with limited labor market experience.²⁷

Whether saving for retirement in a tax-deferred account is optimal for a new enlistee remains an open question. On the one hand many have relatively few expenses, unusual job security and the time value of money makes early investments powerful. On the other hand, the group has a moderate level of income, a low marginal tax rate,²⁸ and access to the military's defined benefit pension. A simple model of consumer financial decision-making with uncertainty in two periods suggests that individuals will tradeoff present and future consumption to maximize total utility. But such decisions require numeracy and financial literacy, both of which are costly to obtain.²⁹ Financial education might affect decision-making by improving numeracy (e.g., computing net present values), increasing literacy (e.g., demonstrating the time value of money or making salient the tax advantages of participation), or lowering the enrollment costs (psychic or time). In the current setting the latter two mechanisms seem

²⁶ Aggregate participation statistics based on the May 2011 FRTIB Monthly Meeting Minutes, available at: <http://www.frtib.gov/pdf/minutes/2011May.pdf>. Accessed on July 12, 2011.

²⁷ Finding a relevant benchmark for the retirements savings levels this population is challenging; military members are eligible for a defined benefit pension, but this option only vests at 20 years. It seems reasonable to assume that these young members have limited savings outside of their work related accounts.

²⁸ Using 2008 Military Basic Pay and Tax brackets, an individual of rank E-2 with less than 2 years of service (typical for this sample), would fall in the 15% tax bracket regardless of the filing status (Single, Married filing jointly, etc...).

²⁹ See Hastings, Madrian and Skimmyhorn (2013) for a simple model of consumer financial decision-making and the role of financial literacy.

most likely as the PFMC sought to improve soldiers' understanding of the benefits of retirement savings and the TSP and to simplify enrollment.

Military Labor Market Outcomes

I also evaluate three military labor market outcomes potentially related to financial decision-making. Since financial stress may undermine job performance (Carrell & Zinman forthcoming, PFMC Memorandum of Understanding 2003), I evaluate whether or not an individual is adversely separated from the military during their first term of service using the outcome Prob(Adverse Separation).³⁰ The separations include but are not limited to financial separations as the effects of financial problems may manifest themselves in other ways including poor job performance or absenteeism. To evaluate current productivity, I use the Prob(Promotion to Sergeant), which measures if an individual is promoted to the first significant supervisory position in their first term of service. Such promotions are uncommon (control mean is less than 5%) and could reflect an individual's ability to focus more on job performance with a better financial situation. Finally, I evaluate one measure of firm attachment using the outcome Prob(Reenlisted | Eligible), which measures whether or not individuals opt for another term in the military if they have been offered the opportunity to reenlist. While employer-employee relations in the military differ from other private and public sector jobs, the U.S. military is nonetheless an All-Volunteer Force and these outcomes should provide some insight into whether employer-provided financial education provides a return on investment in reduced turnover or increased productivity.

Individual Credit Market Outcomes

To expand the PFMC evaluation in terms of outcomes and individual characteristics, I utilize individually-matched de-identified credit bureau data.

³⁰ All three labor market outcomes are restricted to individuals with initial terms of 4 years or less since military administrative data was only available through August 2013. I only observe the final treatment group (AIT start in Aug 2009) with 4 year terms or less and I limit the control group members similarly to ensure comparability of the groups.

Since the data is costly, I elected to match a random subsample of my full administrative data set.³¹ I match selected individuals to their credit bureau data from April of each year from 2007-2010. The credit bureau data provides reliable information on a rich set of consumer decisions in several domains and contributes to this analysis in several ways.³² First, the data provides a much more complete picture of the potential effects of financial education given the range of topics covered by the PFMC, including auto loans/leases, credit card balances, unpaid account balances and an overall credit score.³³ Still, the data do not capture payday loans, informal lending arrangements, or formal arrangements from agencies not reporting to the credit bureau. Second, the credit outcomes enable identification of the PFMC effects unconfounded by direct assistance. Finally, the data enables more precise estimation of the PFMC effects since many individuals have matching records not only for their first year in the military, but for the year prior to their entry. The covariate Prior Credit Score measures the bureau's proprietary credit score for individuals in the year prior to AIT.³⁴ In Figure 1 I depict the relationship between PFMC implementation dates and credit archive dates. The available archives enable evaluation of all subsample members during their first year after AIT completion. However, for many of the treatment group members, year 2 outcomes are unavailable as their AIT completion occurred within 12 months of the final credit archive (April 2010). As a result, I create a reduced year 2 credit subsample by removing censored treatment group members and comparable control group members based on their location, training

³¹ I submitted n=39,484 records for matching in year 1 and 84% were matched. For year 2 credit outcomes, I submitted n=28,496 records for matching and 85% were matched. See Table 3 for evidence that the probability of match and of having active credit are both unrelated to treatment status.

³² For a summary of credit bureau data and its potential uses, see Avery, Calem and Canner (2003).

³³ The outcomes used here capture approximately 75% of credit accounts and 20% of credit balances based on Avery, Calem and Canner (2003), who examine a nationally representative sample. Mortgage balances account for the large difference and given their infrequency in my sample, I omit them from this analysis.

³⁴ All credit outcome regression specifications include the individual's credit score in the year prior to military entry (i.e., year 0). The account balance outcomes (e.g., credit card balances) control for the year 0 account balance. Individuals with missing values are given a zero and a missing indicator is used.

duration and event time.³⁵ This process ensures the comparability of the control and treatment groups on observable characteristics.

I focus my credit analysis on PFMC program goals (i.e., Reducing Debt) and curriculum topics (i.e., Develop a Spending Plan [1.0 hour], The Essentials of Credit [1.0 hour] and Car Buying [1.5 Hour]).³⁶ I analyze four outcomes: cumulative credit account balances (the sum of credit card, finance loans, automobile loans and unpaid account balances), required aggregate monthly minimum payments for all credit balances, an adverse legal action index (foreclosures, liens, judgments and bankruptcies) and the credit score.

IV. Identification Strategy Relies on Quasi-Experimental Variation

The estimates from this research are reduced form in nature and reflect the average effect of the PFMC on individual financial outcomes at a given time horizon. Equation 1 presents the primary specification:

$$Y_{ijt} = \alpha + \beta \cdot PFMC_i + X_i \cdot \gamma + \varphi_j + \delta_t + \varepsilon_{ijt} \quad (1)$$

In this model Y_{ijt} is a financial or labor market outcome for individual i who attended AIT at location j in time period t . $PFMC_i$ is the binary treatment variable that equals 1 if the individual completed the course and equals 0 otherwise. X_i is a vector of individual characteristics that affect financial decision-making including a quadratic in age, gender, race, marital status, number of dependents, education level, AFQT score, a summer enlistment indicator, enlistment term length, AIT course length, average monthly income and the number of months the individual was deployed during the year. For the credit market outcomes, X_i also includes the credit score from the pre-AIT year and the appropriate credit balance/outcome

³⁵ For example, since the last treatment group members from Fort Sill, OK who started AIT in Aug 2009 (event time +12) are not observed in their second year after AIT, I remove them and I remove their control group counterparts from Fort Sill, OK in August 2007 (event time -12).

³⁶ The PFMC lessons on Financial Ethics [0.75 hour], Consumer Awareness [1.0 hour] and Meeting Your Insurance Needs [0.5 hour] are also related to the observed credit outcomes, albeit indirectly.

for the previous year.³⁷ φ_j is a vector of AIT location fixed effects, δ_t is a vector of unique time (month-year) fixed effects and ε_{ijt} is an individual error term. β is the coefficient of interest and the predicted effects on financial decisions (i.e., more retirement savings or less credit debt) depend on the PFMC curriculum. Unless otherwise specified, I report only the main treatment effect estimates (β) and I cluster the standard errors at the treatment location level (N=13 clusters).

My analysis includes event studies and regression estimates for each outcome. The event studies employ event time, which is the difference between an individual's AIT start month and their location's PFMC implementation date. See Figure 1 for the event times by location and calendar time. Identification of causal estimates of the PFMC effects on financial outcomes requires that conditional on an individual's location and the month they entered AIT, treatment assignment is unrelated to the individual error terms. I test this assumption below.

Experimental Validation Using Summary Statistics and Randomization Tests

Several facts suggest that the PFMC implementation justifies causal inference of my estimates. First, the details of the program implementation, unannounced and staggered across locations and time, support an expectation of exogenous variation. Second, the use of location (base) fixed effects in my regression specifications ensures that my identifying variation comes from individuals at a given location and the use of time (month-year) fixed effects ensures that my identifying variation comes from individuals starting AIT in the same month. Third, in Table 2 I present summary statistics for the individual characteristics, which reveal covariate balance across control (PFMC) and treatment (No PFMC) groups for both samples. While there are many statistically significant differences in the means across groups (e.g., Age, AFQT percentile), the differences are due primarily to the large sample size and do not reflect economically significant

³⁷ For those with matched credit records but missing data, zeros are imputed and a missing indicator is used.

differences (i.e., the age difference is 0.18 years =66 days and the AFQT difference is less than one percentile). These mean comparisons also do not account for potential time trends or location differences in the groups. Third, univariate differences in means are not required for the identification of my parameter of interest. Instead the critical assumption is the zero conditional mean assumption: $E(\varepsilon_{ijt}|X_i, \varphi_j, \delta_t) = 0$. While this assumption itself cannot be tested, I use the relationship between treatment and my observable characteristics to model the relationship between treatment and my unobservable characteristics as in Altonji, Elder and Taber (2005) using the following equation:

$$PFMC_i = \rho + X_i \cdot \sigma + \varphi_j + \delta_t + \mu_{ijt} \quad (2)$$

I report the results from the F-tests of the joint significance of σ in the bottom row of Table 2. The results suggest that my observable characteristics (X_i) are jointly unrelated to my treatment variable ($PFMC_i$) in the administrative data sample (Col. 3, p=0.1171) and the credit subsample (Col. 6, p=0.4415).³⁸

[Insert Table 2 about here]

In Table 3 I present evidence that my credit subsample is random and that the probability of the credit bureau matching a record is unrelated to treatment. Specifically I estimate the following equation:

$$Z_i = \tau + \pi \cdot PFMC_i + \varphi_j + \delta_t + \varepsilon_{ijt} \quad (3)$$

The outcome Z_i is a binary indicator for whether or not a submitted record was matched by the credit bureau and I include the same structural controls (location and time fixed effects) as in Equations 1 and 2. The results in Table 3 (Cols 1 and 3) reveal that treatment is unrelated to the probability of having a record matched in the year 1 and 2 subsamples (p=0.7656 and p=0.2852 respectively). To preserve the credit sample size, I assign zeros for records that are matched but

³⁸ For more complete results of these covariate regressions, see Online Appendix Table 1. The results reveal that the individual characteristics are jointly unrelated to treatment in both samples (administrative and credit) and that they explain a trivial portion of the variation in treatment. The partial R-squared values for the individual characteristics are 0.0002 for both samples.

coded as inactive since businesses and the credit bureau have the incentive to report all account balances.³⁹ To ensure that there is no selective imputation based on treatment status I estimate Equation 3 using an indicator for active credit as the outcome. The results in columns 2 and 4 reveal that conditional on a record being matched, treatment is unrelated to the probability of having active credit in year 1 and year 2 ($p=0.5353$ and $p=0.9934$ respectively). This follows my intuition since the PFMC curriculum promoted responsible credit use not credit avoidance. Overall these results suggest that the credit subsample selection was random.

[Insert Table 3 about here]

V. Empirical Evidence Suggests Important Effects for the PFMC

In this section I provide the empirical results for the effects of the PFMC on retirement savings decisions, credit market outcomes and military labor market outcomes. In Table 4, I present the summary statistics for all outcomes by sample and treatment status. The results in Panel A reveal large differences in TSP contributions between the groups with those attending the course participating at higher rates and higher average levels than those who did not. The credit market outcomes in Panel B reveal mixed results, with large negative differences for cumulative account balances; marginal differences for the aggregate monthly payments and credit scores; and some counter-intuitive positive differences for the adverse legal action indexes. The results in Panel C suggest minor potential differences in the labor market outcomes. Taken together, these means reflect mixed results, but they do not account for potential differences in the two groups based on location or decision time. The time effects may be especially important given that program implementation occurred from October 2007 through August 2008 and treatment group members are observed, on average, about one year later

³⁹ I complete robustness checks for this assumption by completing all credit market regressions with only matched and active records. The results are qualitatively similar to the main results in Table 5, suggesting that the results are not driven by the use of the matched but inactive records. See Appendix Table 6.

than their control group counterparts.⁴⁰ If the economic hardship experienced in the U.S. disproportionately affected members of the treatment group, then reliable estimates of the PFMC's effects must account for the effects of time. As a result, I proceed with event studies and regression adjusted estimates of the PFMC.

[Insert Table 4 about here]

Large and Lasting Retirement Savings Results

My empirical strategy, which pools individuals across locations by their common event time, enables event study analyses of the outcomes of interest. In Figure 2, I present event studies for four selected outcomes: the average monthly TSP savings in year 1, the average monthly TSP savings in year 4, the probability of an adverse separation from the military in the first enlistment term and the aggregate monthly payment for credit debts.⁴¹ Panel A of Figure 2 presents a striking result: at the time of program implementation, there is a large, discontinuous increase in the average monthly TSP contribution in year 1 across locations. The increase appears to be \$15-20, nearly doubling contributions; Panel B reveals large but less precise increases for year 4 contributions.⁴² Panel C suggests that Aggregate Monthly Payments are slightly lower among the treatment group in year 1 and Panel D reveals minimal differences in the Probability of an Adverse Separation in the First Term between the two groups. But while the event studies provide suggestive evidence that the PFMC effects TSP and credit market outcomes, they do not fully account for the entry cohort time differences. As a result, I turn to a multivariate regression framework.

[Insert Figure 2 about here]

⁴⁰ The median month for AIT completion for the control (treatment) group is August 2007 (December 2008).

⁴¹ In Figures 3-5 (Appendix) I provide event studies for all of the outcomes analyzed in the paper.

⁴² Cohort month +3 appears to be a large outlier in its average outcome values. In robustness checks I omit cohort month +3 and find nearly identical OLS results (\$18.65 p=0.040 vs. \$19.90 p=0.030).

In Table 5 I present ordinary least squares estimates for the PFMC effects on TSP contributions in years 1-4 (Panel A), credit market outcomes in years 1-2 (Panel B) and labor market outcomes in the first term (Panel C).

[Insert Table 5 about here]

The Table 5 Panel A results suggest that the PFMC has large effects on TSP outcomes across all four years. The PFMC increased the average monthly contributions by approximately \$19.93 (over 100%) in year 1 (Col.1, $p=0.029$) and \$13.75 (about 50%) in year 2 (Col.3, $p=0.038$). The average effects in years 3 and 4 (\$10.33 and \$6.79, Cols. 5, 7) are also positive, but smaller and statistically insignificant ($p=0.136$ and $p=0.328$ respectively). The PFMC also increased the probability of TSP participation in all 4 years, by 15.04 percentage points (over 100%) in year 1 (Col.2, $p=0.015$), 13.46 percentage points (about 100%) in year 2 (Col. 4, $p=0.014$), 11.55 percentage points (about 75%) in year 3 (Col. 6, $p=0.015$) and 8.09 percentage points (about 50%) in year 4 (Col. 8, $p=0.071$).⁴³ The PFMC, together with the enrollment assistance policies, generated substantial increases in the retirement savings for treated members. The year 1-2 contribution differences amount to a future balance difference of over \$4,200 under conservative assumptions.⁴⁴

In Table 5 Panel B I provide estimates suggesting important but more limited effects among the credit outcomes in both magnitude and horizon. Here the PFMC reduced cumulative credit balances by \$636 (about 10%) in the first year (Col.1, $p=0.028$) but the second year reduction estimate of \$284 (3%) is statistically insignificant (Col. 5, $p=0.620$). The course reduced the aggregate monthly credit payment by \$37 (about 17%) in year 1 (Col. 2, $p=0.008$) but the second year estimate of \$1.67 is economically and statistically insignificant (Col.

⁴³ In Appendix Table 2, I analyze the PFMC effects on the TSP contribution distributions for all 4 years. The effects are statistically significant through at least the 90th percentiles of the distributions in years 1-2.

⁴⁴Using conservative assumptions (6% real rate of return; withdrawal at 60 years of age and no TSP contribution differences after 2 years) the future value difference of \$19.93 for 12 months in year 1 and \$13.75 for 12 months in year 2 is \$4,207.

6, $p=0.949$). The course reduced the number of adverse legal actions in year 1 by 0.057 actions (about 35%) but the statistical significance is marginal ($p=0.078$). The second year estimate of a 0.084 (about 30%) action decrease is statistically insignificant (Col. 7, $p=0.3286$). The PFMC has no statistically significant effects on overall credit scores in year 1 (Col. 4, $p=0.9318$) or year 2 (Col. 8, $p=0.3079$).

Finally, the Table 5 Panel C results suggest no significant effects of the PFMC on military labor market outcomes. The estimates are all economically small (0.79%, 0.10% and 0.76% respectively) and statistically insignificant ($p=0.4182$, $p=0.8751$ and $p=0.7821$ respectively).

Given the large samples and the detailed nature of the military administrative data, I evaluate the PFMC for differential treatment effects along several dimensions. In Table 6, I present OLS estimates of the effects of the PFMC on two year 1 outcomes (average monthly TSP contributions and aggregate monthly credit payments) by gender, minority status, human capital levels (AFQT score), marital status and prior year credit activity (for the credit payments). In the retirement saving domain (Panel A), all of the individual characteristics have statistically significant interactions with course attendance. Females (Col. 2) on average contributed less after the PFMC than their male counterparts (interaction coefficient $-\$14.27$, $p=0.007$),⁴⁵ minorities (Col. 3) on average contributed less than their white counterparts (interaction coefficient $-\$8.54$, $p=0.011$) and married individuals (Col. 5) on average contributed less than their single counterparts (interaction coefficient $-\$5.75$, $p=0.039$). While the current research design is unable to identify the mechanism for the observed effects, I provide some potential explanations. These groups may be less susceptible to peer effects for saving; they may identify less with the PFMC

⁴⁵ This finding, that males benefit more from the training, stands in contrast to some previous results suggesting greater gains for females from randomized interventions for education assistance (Bettinger et. al. 2009) and housing assistance (Sanbonmatsu et. al. 2011). However, Grinstein-Weiss et. al. (2013) find that males benefitted more than females from financial education on IDAs in their educational attainment.

instructors; or they may deliberate more and not submit the TSP enrollment forms during the course. Individuals with higher levels of human capital (AFQT interactions, Col. 4) on average contributed more when treated (interaction coefficient=0.36, $p=0.002$) suggesting that these individuals can better process the tax advantages and time value of money principles inherent in the course. The age interactions with retirement savings (Col. 6) are mixed (interaction coefficient for age is -1.09 and for age squared is 0.01, p -values 0.096 and 0.093) and when combined with the main effect estimates for age and age squared suggest that older individuals contribute more to the TSP. Maturity, financial and labor market exposure and the salience of retirement may explain these results.

[Insert Table 5 about here]

In the credit domain (Panel B) of Table 5, only three characteristics have statistically significant interactions with PFMC attendance. Treated individuals with higher AFQT scores (Col. 4) have lower required monthly payments (interaction coefficient=-0.59, $p=0.003$), suggesting that they are better able to learn course concepts related to credit and debt. The age interactions (Col. 6) are mixed (interaction coefficient for age is 8.36 and for age squared is -0.17, p -values 0.048 and 0.026) and when combined with the main effect estimates for age and age squared suggest that older treated individuals are more able to reduce their required monthly credit payments.⁴⁶ Finally, treated individuals with prior credit market activity (Col. 7) have lower required monthly payments (interaction coefficient=-15.20, $p=0.019$), suggesting that experience motivated additional learning or that experienced individuals could better apply course concepts.⁴⁷

⁴⁶ The age interactions for both TSP contributions and required monthly credit payments in year 1 are consistent with the results of Agarwal, Driscoll, Gabaix and Laibson (2009) who find that financial mistakes are u-shaped in age with cost-minimizing performance around age 53. Since my sample ages vary from 17-49, the monotonically increasing (decreasing) effects for retirement savings (required credit payments) appear to follow a similar pattern.

⁴⁷ I also tested and found no significant interaction effects for course attendance and prior credit scores (interaction coeff=-0.07, $p=0.146$) using a restricted sample of individuals with prior scores ($n=18,054$).

VI. Discussion and Lessons Learned from the PFMC

These results suggest that the PFMC has large effects on treated individuals' retirement savings decisions through at least two years and moderate effects on routine financial decisions including total credit balances, monthly credit payments and adverse legal actions in the first year after initial job training. The course does not appear to have significant effects on these credit market outcomes in the following year nor does it appear to affect individual's labor productivity or attachment to the military. This section benchmarks the empirical results, discusses explanations for the program's effectiveness and provides recommendations for future research and policy design.

The observed retirement savings effects are large in economic magnitude (50-100%) and they persist through at least two years.⁴⁸ While there is little empirical evidence on the effects of financial education on retirement savings decisions, I highlight a few relevant results: Lusardi and Mitchell (2007) estimate that financial education in the workplace increases financial wealth by approximately 18%⁴⁹; Duflo and Saez (2003) find that exposure to an employee benefit fair increases tax deferred account saving by 3-4%⁵⁰; Cole and Shastry (2010) find that exposure to additional math (but not financial) education in high school increases investment income (self-reported) by 11% for black women and 3% for white women⁵¹. The PFMC effects appear to be much larger than for existing studies in the retirement savings domain, but this is not surprising as the PFMC combined education and enrollment assistance for these decisions. As a result, I compare my observed effect magnitudes (100%+) to other policy options designed to improve retirement savings: Madrian and Shea (2001a) find that

⁴⁸ For evidence on the effects of the course on the TSP contribution distributions, see Appendix Table 2.

⁴⁹ Whether their estimates should be interpreted causally remains unclear; the authors use HRS data and correlate financial education attendance with wealth levels, making selection a serious concern.

⁵⁰ Their effect size is 1.25 percentage points on a control mean of 34 percent.

⁵¹ They find no statistically significant effects of exposure to math education on investment income for men. They also find no effects of exposure to financial education on investment income for men or women.

automatic enrollment increases 401(k) participation rates by 103%; Benartzi and Thaler (2004) estimate that the Save More Tomorrow™ program could double the overall saving rate (from 5 to 9.7 percent) within 5 years; and Carroll et. al. (2009) find that an active decision enrollment regime increases 401(k) participation rates by 68% relative to opt-in enrollment. My findings suggest that the PFMC achieves savings results as large as other leading policy options.

The PFMC effects on credit market outcomes are important but more limited. The course has no significant effects on the most routine outcomes (probability of active credit and credit score), but it has moderately sized effects on combined credit balance (i.e., credit cards, finance loans, auto loans and balances in an unpaid status) levels (nearly 10%) and total required monthly payments (17%). The effects on adverse legal actions (i.e., bankruptcies, foreclosures, liens and judgments) are larger (about 36%) but marginally statistically significant. Several studies of the return to financial education provide benchmarks for comparison. Drexler, Fischer and Schoar find that rule-of-thumb based education for entrepreneurs improved self-reported desirable financial accounting behaviors by 6-12%; Gaurav, Cole and Tobacman (2011) find that customized financial education increases rainfall insurance purchases by 8-16% in India; and Grinstein-Weiss et. al. (2013) find that financial education about Individual Development Accounts increased educational attainment by 7%. With these limited benchmarks in mind, the PFMC appears to be at least as successful as previous programs in improving financial decision-making and more successful than previous findings of no effects (e.g., Gartner and Todd 2005, Servon and Kaestner 2008, Carlin and Robinson 2011 and Choi et. al. 2011).

The PFMC has no statistically significant effects on any of the military labor market outcomes (adverse separations, rapid promotions and reenlistment decisions). The economic literature provides little evidence on the causal effects of employer provided financial education on labor market decisions (Bernheim

and Garrett 2003), though employers remain interested in such education for reasons that include managing liability risks, improving productivity, lowering worker stress and reducing turnover (Garman et. al. 1999). Other research highlights additional reasons that the services may be interested in financial education including operational costs (Garman 1998) and threats to security clearances (Carrell and Zinman forthcoming). While the current research does not provide any direct evidence on these outcomes, there will likely remain continued interest by the military in providing such education.

The results above provide convincing evidence that individuals who completed the PFMC have higher rates of TSP participation, higher average contribution levels, reduced credit account balances and lower required monthly credit payments early in their military service. The lack of similar findings in the literature motivates consideration of the program effect mechanism. A traditional human capital explanation suggests that given the PFMC's emphasis on retirement savings (25% of the course) improved individuals' knowledge of the importance of retirement savings, the benefits of tax-advantaged savings and the TSP program itself. The observed increases in early TSP contributions could be a result of the financial education improving individuals' financial literacy, thereby improving their financial decision-making and jumpstarting retirement savings. An equally likely explanation, behavioral in nature, results from distributing enrollment forms, assisting in form completion, answering specific enrollment questions and at certain locations, assisting in form submission. As a result, the observed effects could demonstrate the power of behavioral assistance in the retirement savings context similar to the effects identified by Bettinger et. al. (2009) for financial aid applications and the general role of institutional features widely demonstrated in the behavioral economics literature.⁵² From the military

⁵² See for example, Madrian and Shea (2001a), Carroll et. al. (2009) and Beshears et. al. (2006a, 2006b).

perspective, such attribution may be second order,⁵³ but for the economics literature, identification of the role of financial human capital versus behavioral elements improves our understanding of the effects of financial education and policy design. While there is no ability to separate these mechanisms for the observed TSP outcomes,⁵⁴ the PFMC effects on credit outcomes suggest that the education appears to be generating human capital in some areas.

In addition, the combination of the TSP and credit results provides more reason for optimism since it can potentially rule out intrahousehold budget transfers as an explanation for the increased retirement savings. Recall that on average, the TSP results suggest an increase in retirement savings of about \$14-\$20 each month in the first two years of service. Analysis of subsequent credit behavior reveals that treated individuals also have lower credit balances and required monthly payments in year 1. The absence of any intrabudget transfer evidence, wherein individuals could have financed retirement savings with credit spending, is a noteworthy result. The year 1 results are doubly indicative (more saving and less credit use) that the PFMC affects economic decision-making.

Several program and institutional factors suggest that my results are likely lower bound estimates of the PFMC effects. First, while the course is mandatory for all new soldiers, it is not graded nor is it an explicit requirement for completing AIT; absences would attenuate the results of treatment. Second, military training delays (e.g., from deficiencies or injuries) might force control group members to experience treatment in a later month, again attenuating the measured treatment effects.⁵⁵ Third, interactions between control and treatment

⁵³ What may be of interest to the military in this case and educators more widely, is the optimal mix of education and behavioral assistance. One can imagine behavioral assistance being fruitless if introduced without some education or context for the decision being prompted. Conversely, several hours of education may be unnecessary to motivate action when assistance is provided. These combined policies have received little attention in the financial literacy literature to date and warrant further attention.

⁵⁴ I attempted to exploit variation in the assistance by location to separately estimate the effects of human capital and enrollment assistance, but the data was anecdotal and unavailable for most of the sample period.

⁵⁵ There is no mechanism that would accelerate treatment members' AIT completion.

group individuals after AIT at subsequent assignments (e.g., as roommates, colleagues or friends) could result in discussions about the contents of the course and reduce any differences in knowledge and motivation that the PFMC imparted. From the military's perspective, such spillovers are desirable. From an experimental standpoint though, this contamination will reduce the observed outcome differences.⁵⁶ Fourth, military leaders may act to help soldiers facing financial problems through counseling or requiring financial training after AIT; these institutional efforts will act to mitigate differences between the groups. Finally, since the PFMC is not the Army's only financial education program, control group individuals' attendance at other courses (voluntary or command directed) will also mitigate the observed outcome differences.⁵⁷

While the internal validity considerations above suggest that these results are likely lower bound estimates for the PFMC itself, external validity considerations suggest that these results could be difficult to replicate in other contexts. As a captive audience and recent military enlistees, PFMC students are generally in a mindset to receive and follow instructions. The timing of the course may also be uniquely suited for influencing individual financial behavior given that these individuals are often living alone for the first time. This young population, new to the labor force, may not have developed bad habits yet and might be able to build better financial habits based on the course. In addition, while individuals could not plausibly select into the military for the PFMC, they may be selecting into the military for more general career/life goals, which include securing a better financial future. Such selection may make these individuals "better compliers" than the average individual.⁵⁸ Finally, peer effects

⁵⁶ This feature would also explain the differences in the magnitude of the treatment effects for retirement savings (likely a one-time decision during AIT) and subsequent credit outcomes after mixing has occurred.

⁵⁷ Diminishing returns among the treatment group attending more training and/or any "John Henry" effects among control group members who seek to "catch up" will mitigate positive findings. Importantly, these courses do not explain my observed effects since they were not initiated concurrently with the PFMC.

⁵⁸ I thank Antoinette Schoar for bringing this concern to my attention.

and/or role model effects may influence individuals in this population more than in the general population given selection into the military. The instructors in the course, often retired military personnel, are likely trusted role models for the students, potentially increasing the motivation to listen and learn.⁵⁹ As a result, the findings might be most usefully applied to other new working populations: employees in apprenticeship or union programs, public sector employees, non-traditional college students and other service members.

In addition to the PFMC's promising results, its costs are relatively low. Using expenditure and student throughput data by location, I estimate that the course costs approximately \$22 per soldier.⁶⁰ At this price, the PFMC appears to more than pay for itself in increased retirement savings;⁶¹ any benefits in reduced credit balances or soldier financial welfare make the program even more cost-effective. While choice architecture interventions (e.g., defaults) may be less expensive methods for increasing retirement savings, the PFMC's wider program goals and the military's reluctance thus far to support default savings options make the course a reasonable and inexpensive alternative.⁶²

The key contribution of this work is establishing that financial education can improve short term financial outcomes. But additional research might focus on several issues. First, there are no doubt improvements in the curriculum design and teaching of financial literacy topics. Systematic examination of these approaches holds promise for improving student and program outcomes. Second, more attention might be devoted to the difficult task of isolating the mechanisms through which this education works (e.g., knowledge, appreciation of complexity,

⁵⁹ These comments are based on author conversations with the Program Director at SDCC, observation of PFMC instruction at Fort Lee, VA in July 2012 and interviews with lead instructors at a number of locations.

⁶⁰ SDCC cost estimates are roughly half this value, though their estimates are done for a longer duration (2008-2011); my estimates rely on the costs at each location in the first year of implementation (2007-2009).

⁶¹ These differences amount to about \$400 in present dollars and this is a conservative estimate (6% real rate of return compounded monthly) as it assumes no contribution differences past two years.

⁶² The DOD relies on the TSP as the principal 401(k) for most of its civilian employees and it has instituted "opt-out" defaults for their contributions. However, neither the DOD nor the military services have instituted defaults for uniformed service members in the TSP.

changing time preferences, peer effects, or other policy nudges). The isolation of the mechanisms will be difficult but there are enormous opportunities for learning in the U.S. and elsewhere if program administrators commit to experimental approaches. This research demonstrates that financial education can influence behavior and that the effects appear to operate on multiple margins. Evaluations at longer time horizons are also important but they will likely prove challenging, requiring large samples and detailed administrative outcome data.

I briefly discuss some potential explanations for the program's success. First, the PFMC has a targeted curriculum that covers the most relevant topics for the students. Second, the course is well-timed in reaching individuals who are increasingly responsible for their financial welfare. Third, the course generally provides practical advice (e.g., avoid variable rate mortgages) as opposed to broad principles (e.g., how to complete a net-present-value analysis of mortgage options). Finally, the course combines education with assistance (e.g., TSP enrollment assistance) and advice, generating actionable education.

New Evidence on the Effectiveness of Financial Education

This research estimates the effects of financial education on a variety of economic outcomes using a large natural experiment within the U.S. Army. I find that Personal Financial Management Course (PFMC) attendance and enrollment assistance doubles retirement savings, with significant effects that persist through at least two years. The course has smaller but suggestive effects on credit market outcomes including account balances (credit cards, automobile loans and finance loans) and aggregate monthly credit payments in the first year after soldiers finish their initial job training. The PFMC has no significant effects on military labor market outcomes including measures of performance, productivity and retention early in their service. Overall, the results suggest that financial education coupled with assistance and advice can improve financial outcomes in a number of areas.

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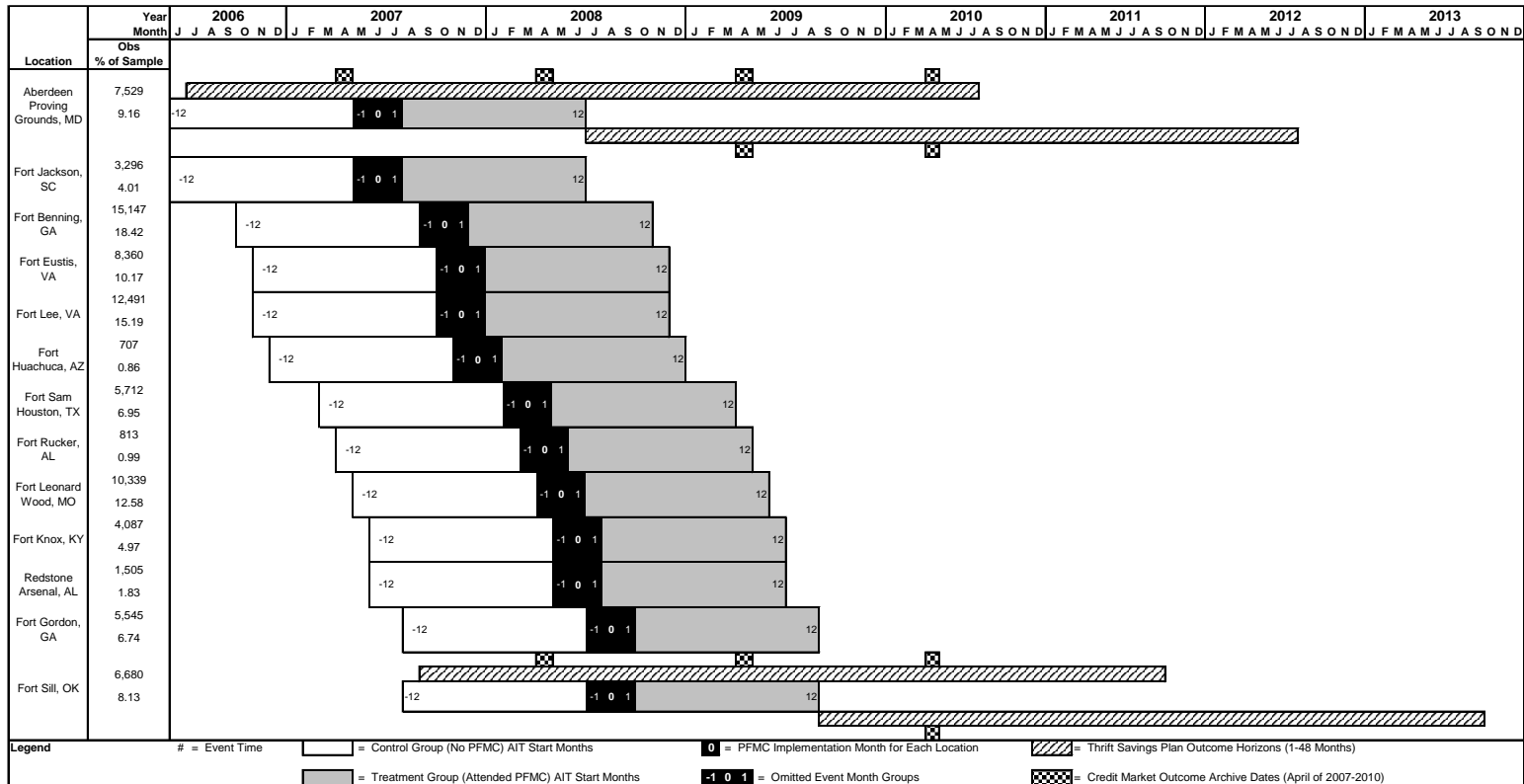


Figure 1. PFMC Implementation Schedule and Outcome Data Horizons for Selected Locations

Author compiled data using Department of Defense (DOD) and Army Emergency Relief (AER) data. Percentages are calculated for the administrative data sample (n=82,211).

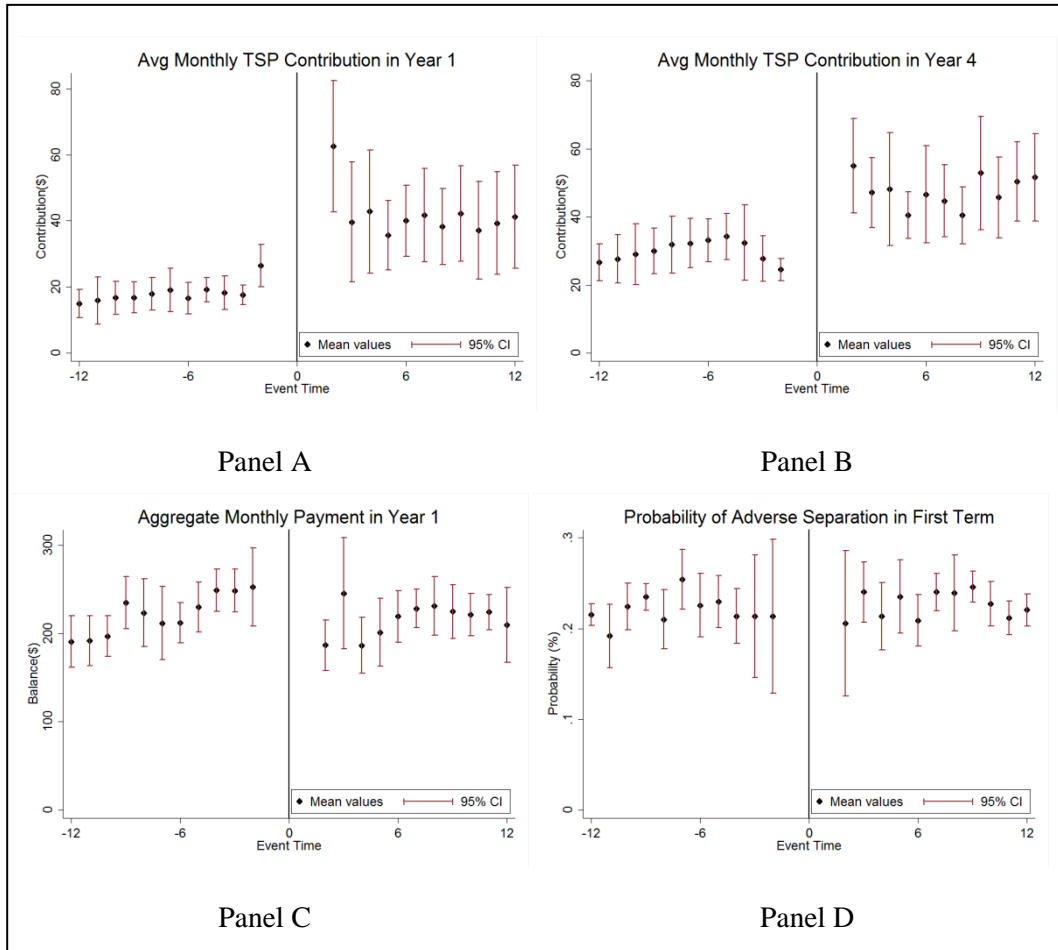


Figure 2. Event Studies for Select Economic Outcomes

Source: DOD and credit bureau data. In Panel A, the y-values depict the mean of the average monthly TSP contributions in year 1 for all individuals by event month (n=82,211). In Panel B, the y-values depict the mean of the average monthly TSP contributions in year 4 for all individuals by event month (n=44,655). In Panel C, the y-values depict the mean of the average aggregate monthly credit payments in year 1 for all individuals by event month (n=33,167). In Panel D, the y-values depict the mean of the probabilities of adverse first term separations from the military by event month (n=65,837).

Table 1. Personal Financial Management Course (PFMC) Syllabus

Lesson	Subject	Topics Covered	Hours
1	Financial Ethics	Legal, moral and ethical aspects of personal financial management	0.75
2	Leave and Earnings (Pay) Statement	Understanding pay statements, Military benefits and Insurance coverage, Educational benefits, Payroll deductions and Resolving pay problems	0.25
3	Developing a Spending Plan	Net worth, Debt to income ratios, Discretionary vs. Non-discretionary spending	1
4	The Essentials of Credit	Types of credit, Factors affecting credit worthiness, Proper credit usage, Warning signs of too much debt, Credit and debt assistance, Consumer protection laws, Credit reports	1
5	Consumer Awareness	Psychology of advertising, Types of deception, Identity theft recognition and correction, Description of common scams	1
6	Car Buying	Personal budget review, Contract tips, Determining fair price, Negotiation tips, Effects of car ownership in the military, Financing, Consumer protection	1.5
7	Meeting your Insurance Needs	Renters and Homeowners, Automobile, Life, Health, Insurance frauds and scams, Consumer protection tips	0.5
8	Thrift Savings Plan and Investing	Retirement concepts, the Thrift Savings Plan, Military retirement programs, Compound interest, Investments	2
Total			8

Source: Army Emergency Relief and San Diego City College.

Table 2. Individual Characteristics by Sample and Treatment Condition

Variable	Panel A Full Administrative Data Sample <i>N</i> =82,211			Panel B Matched Credit Subsample <i>N</i> =33,178		
	(1)	(2)	(3)	(4)	(5)	(6)
	No PPMC <i>N</i> =40,843	PPMC <i>N</i> =41,368	Diff	No PPMC <i>N</i> =16,740	PPMC <i>N</i> =16,438	Diff
	Mean (SD)	Mean (SD)	(1)-(2) <i>p</i> -value	Mean (SD)	Mean (SD)	(1)-(2) <i>p</i> -value
Age, years	21.35 (4.05)	21.53 (4.12)	-0.18 <i>0.0000</i>	21.40 (3.98)	21.61 (4.16)	-0.22 <i>0.0000</i>
Female, %	14.91 (35.62)	15.89 (36.56)	-0.98 <i>0.0001</i>	11.37 (31.75)	12.14 (32.66)	-0.77 <i>0.0298</i>
Married, %	17.60 (38.08)	19.05 (39.27)	-1.45 <i>0.0000</i>	17.86 (38.30)	19.04 (39.26)	-1.19 <i>0.0054</i>
Dependents	0.43 (0.93)	0.47 (0.97)	-0.04 <i>0.0000</i>	0.43 (0.94)	0.46 (0.96)	-0.03 <i>0.0051</i>
Minority, %	30.84 (46.18)	33.63 (47.25)	-2.79 <i>0.0000</i>	29.17 (45.46)	31.76 (46.56)	-2.59 <i>0.0000</i>
< HS education, %	28.77 (45.27)	24.46 (42.98)	4.32 <i>0.0000</i>	29.79 (45.74)	24.92 (43.26)	4.87 <i>0.0000</i>
HS graduate, %	62.61 (48.38)	65.89 (47.41)	-3.28 <i>0.0000</i>	61.61 (48.63)	65.32 (47.60)	-3.71 <i>0.0000</i>
Some college, %	6.20 (24.11)	6.72 (25.04)	-0.52 <i>0.0023</i>	6.20 (24.12)	6.94 (25.42)	-0.74 <i>0.0065</i>
≥ College grad, %	2.42 (15.36)	2.93 (16.87)	-0.52 <i>0.0000</i>	2.40 (15.29)	2.82 (16.56)	-0.43 <i>0.0147</i>
AFQT, percentile	55.89 (19.45)	56.14 (19.78)	-0.25 <i>0.0698</i>	56.35 (19.26)	57.25 (19.02)	-0.91 <i>0.0000</i>
Joined in summer, %	38.10 (48.56)	35.99 (48.00)	2.11 <i>0.0000</i>	37.10 (48.31)	33.48 (47.19)	3.61 <i>0.0000</i>
Enlistment term, yr	3.85 (0.98)	3.79 (1.00)	0.06 <i>0.0000</i>	3.86 (0.99)	3.79 (1.00)	0.06 <i>0.0000</i>
AIT length, months	3.16 (1.13)	3.15 (1.11)	0.01 <i>0.1303</i>	3.18 (1.11)	3.18 (1.11)	0.00 <i>0.8020</i>
Monthly pay, \$	1,757 (542.27)	1,882 (578.99)	-124.50 <i>0.0000</i>	1,758 (542.02)	1,880 (576.15)	-121.46 <i>0.0000</i>
Months deployed	1.18 (2.26)	1.51 (2.66)	-0.34 <i>0.0000</i>	1.19 (2.27)	1.56 (2.69)	-0.37 <i>0.0000</i>
Prior Credit Score	-	-	-	557 (105)	554 (108)	2.41 <i>0.1276</i>
Missing Prior Credit Score, %				47.35 (49.93)	43.79 (49.61)	3.56 <i>0.0000</i>
Joint test of significance	<i>p</i>= 0.1171			<i>p</i>= 0.4415		

Source: DOD Data. Married represents formal and common law marriages for anyone ever married. Less than high school variable includes dropouts and GED holders. Mean AFQT percentiles exceed 50 due to enlistment prohibitions for low scores. Average monthly pay represents the mean base pay, subsistence pay, and housing allowance during the first year. Months deployed variable reflects the number of months that an individual received hostile fire pay during the first year. Prior credit score data is restricted to individuals with a pre-treatment score (*n*=18,054). Columns 3 and 6 report the differences in the means and the *p*-values from a *t*-test of equality. The joint test of significance row reports the *p*-value from an *F*-test for the joint significance of all individual characteristics (omitting high school grad indicator and adding a quadratic term in age) from an OLS regression of Equation 2 with standard errors clustered at the location level (*N*=13). The *p*-values suggest that treatment is unrelated to individual characteristics.

Table 3. OLS Estimates of Sample Credit Matching and Activity

Outcome	Pr (Matched Record) in Yr 1, %	Pr (Active Credit) in Yr 1, %	Pr (Matched Record) in Yr 2, %	Pr (Active Credit) in Yr 2, %
	(1)	(2)	(3)	(4)
PFMC Effect	-0.44	-0.67	2.24	0.01
Std Err	(1.43)	(1.05)	(2.00)	(1.14)
Location fixed effects	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y
Control Mean	85.08	90.38	87.65	94.12
Observations	39,484	33,178	28,496	24,235
Clusters	13	13	13	13
Adj R-Squared	0.0132	0.0096	0.0758	0.0055

Source: DOD and Credit Bureau data. The table reports OLS estimates of Equation 3. Standard errors are clustered at the AIT location (base) level. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

Table 4. Retirement Saving, Credit Market, and Labor Market Outcomes

Outcome	Panel A Full Administrative Data Sample <i>N</i> =82,211						Panel B Matched Credit Subsample <i>N</i> =33,178					
	(1)			(2)			(3)			(4)		
	No PFMC			PFMC			No PFMC			PFMC		
	N	Mean	(SD)	N	Mean	(SD)	N	Mean	(SD)	N	Mean	(SD)
Panel A. Retirement Savings Outcomes												
Prob (TSP Participation) in Yr 1, %	40,843	12.04	(32.54)	41,368	29.76	(45.72)	16,740	12.07	(32.58)	16,438	30.53	(46.06)
Avg Monthly TSP Savings in Yr 1, \$	40,843	17.27	(57.46)	41,368	40.03	(79.86)	16,740	17.37	(57.69)	16,438	41.75	(81.82)
Prob (TSP Participation) in Yr 2, %	34,874	15.09	(35.79)	35,908	30.90	(46.21)	14,375	14.82	(35.54)	14,304	31.26	(46.36)
Avg Monthly TSP Savings in Yr 2, \$	34,874	28.52	(83.68)	35,908	45.62	(91.15)	14,375	28.06	(83.26)	14,304	46.48	(91.77)
Prob (TSP Participation) in Yr 3, %	29,255	16.16	(36.81)	30,354	30.78	(46.16)	12,048	15.72	(36.40)	12,062	30.84	(46.19)
Avg Monthly TSP Savings in Yr 3, \$	29,255	28.89	(81.98)	30,354	45.80	(92.77)	12,048	28.26	(81.27)	12,062	45.57	(92.21)
Prob (TSP Participation) in Yr 4, %	22,865	17.34	(37.86)	21,790	30.18	(45.90)	9,402	17.02	(37.58)	8,629	30.43	(46.01)
Avg Monthly TSP Savings in Yr 4, \$	22,865	30.28	(85.00)	21,790	46.87	(97.89)	9,402	30.45	(86.35)	8,629	47.49	(98.75)
Panel B. Credit Market Outcomes												
Cumulative Credit Balance in Yr 1, \$							16,740	6,668	(8,585)	16,438	6,326	(8,391)
Aggregate Monthly Payment in Yr 1, \$							16,740	214	(262)	16,438	218	(267)
Adverse Legal Action Index in Yr 1, #							16,740	0.16	(1.11)	16,438	0.24	(1.74)
Credit Score in Yr 1, #							15,130	581	(89)	14,713	584	(89)
Cumulative Credit Balance in Yr 2, \$							12,328	8,882	(9,466)	11,907	7,863	(9,115)
Aggregate Monthly Payment in Yr 2, \$							12,328	282	(297)	11,907	274	(295)
Adverse Legal Action Index in Yr 2, #							12,328	0.24	(1.66)	11,907	0.28	(2.09)
Credit Score in Yr 2, #							11,603	587	(93)	11,063	587	(90)
Panel C. Labor Market Outcomes												
Prob (Adverse Separation in 1st Term), %	32,585	22.11	(41.50)	33,251	22.67	(41.87)	13,268	22.22	(41.57)	13,168	22.65	(41.86)
Prob (Promoted to Sergeant in 1st Term), %	32,585	4.60	(20.95)	33,251	6.65	(24.92)	13,268	4.65	(21.06)	13,168	6.99	(25.51)
Prob (Reenlisted Eligible), %	13,996	74.00	(43.87)	4,759	86.66	(34.01)	5,619	74.53	(43.57)	1,856	87.72	(32.83)

Source: DOD and Credit Bureau Data. Notes: The table reports the sample sizes, means and standard deviations for each outcome in the corresponding row, the sample specified by the panel, and the control/treatment group specified in the column. Credit outcomes (except Credit Score) are set to zero for individuals with a matched credit record but missing data. Since credit scores cannot be imputed this way the sample for this outcome is reduced by $n=3,335$ ($n=33,178$ for full credit subsample and $n=29,843$ for those with matched records). The labor market outcomes (adverse separation, promotion, and reenlistment) are limited to those with initial terms ≤ 4 years to ensure comparable term lengths between the control and treatment groups.

Table 5. OLS Estimates of the PFMC Effects on Retirement Savings, Credit and Labor Market Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Thrift Savings Plan Outcomes in Years 1-4								
Outcome	Y1=Avg Monthly TSP Contr Yr 1, \$	Pr (Y1>0) %	Y2=Avg Monthly TSP Contr Yr 2, \$	Pr (Y2>0) %	Y3=Avg Monthly TSP Contr Yr 3, \$	Pr (Y3>0) %	Y4=Avg Monthly TSP Contr Yr 4, \$	Pr (Y4>0) %
PFMC Effect	19.93**	15.04**	13.75**	13.46**	10.33	11.55**	6.79	8.09*
Std Err	(8.06)	(5.31)	(5.88)	(4.68)	(6.47)	(4.60)	(6.67)	(4.08)
Control Mean	17.27	12.04	28.52	15.09	28.89	16.16	30.28	17.34
Observations	82,211	82,211	70,782	70,782	59,609	59,609	44,655	44,655
Clusters	13	13	13	13	13	13	13	13
Adj. R-Squared	0.0971	0.1037	0.0602	0.0815	0.0603	0.0744	0.0613	0.0676
Panel B. Credit Outcomes in Years 1-2								
	Cumulative Credit Account Balance in Yr 1, \$	Aggrgeate Monthly Payment in Yr 1, \$	Adverse Legal Action Index in Yr 1, #	Credit Score in Yr 1, #	Cumulative Credit Account Balance in Yr 2, \$	Aggrgeate Monthly Payment in Yr 2, \$	Adverse Legal Action Index in Yr 2, #	Credit Score in Yr 2, #
PFMC Effect	-636.23**	-37.17***	-0.057*	-0.25	-284.12	-1.67	-0.084	-3.66
Std Err	(253.68)	(11.80)	(0.030)	(2.95)	(558.24)	(25.67)	(0.083)	(3.45)
Control Mean	6,667.82	214.38	0.16	580.76	8,881.63	282.43	0.24	586.84
Observations	33,178	33,178	33,178	29,843	24,235	24,235	24,235	22,666
Clusters	13	13	13	13	13	13	13	13
Adj. R-Squared	0.3311	0.2710	0.4472	0.3676	0.1770	0.1279	0.2659	0.2679
Panel C. Labor Market Outcomes								
	Prob (Adverse Separation) in 1st Term, %	Prob (Promoted to Sergeant) in First Term, %	Prob (Reenlisted Eligible), %					
PFMC Effect	0.79	0.10	0.76					
Std Err	(0.95)	(0.66)	(2.71)					
Control Mean	22.11	4.60	74.00					
Observations	65,837	65,837	18,755					
Clusters	13	13	13					
Adj. R-Squared	0.0494	0.0953	0.1513					

Source: DOD and Credit Bureau data. Sample as specified in Appendix 1. The table reports OLS estimates of Equation 1. Standard errors are clustered at the AIT location (base) level. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

Table 6. OLS Estimates of PFMC Heterogeneous Treatment Effects in Year 1

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Main Specification (Equation 1)	Female	Minority	AFQT Score	Married	Age + Age Squared	Prior Year Credit Activity
Panel A. Average Monthly TSP Contributions in Year 1							
PFMC Effect	19.93** (8.06)	22.00** (8.16)	22.71** (8.40)	0.388 (7.37)	21.01** (8.22)	34.71*** (10.47)	
Individual characteristic from column		3.73 (2.27)	7.48*** (1.35)	0.07 (0.09)	-0.44 (2.67)	1.41 (0.82)	N/A
Individual characteristic from column×PFMC		-14.27*** (4.39)	-8.54** (2.84)	0.36*** (0.09)	-5.75** (2.49)	-1.09* (0.61)	N/A
Control Mean	17.27	17.27	17.27	17.27	17.27	17.27	
Observations	82,211	82,211	82,211	82,211	82,211	82,211	
Clusters	13	14	15	16	17	18	
Adj. R-Squared	0.0971	0.0984	0.0979	0.0996	0.0973	0.0971	
Panel B. Aggregate Monthly Payment for Credit Balances in Year 1							
PFMC Effect	-37.17*** (11.80)	-37.87*** (11.77)	-40.64*** (12.74)	-4.729 (11.66)	-35.11*** (11.48)	-130.88** (51.65)	-28.79** (11.76)
Individual characteristic from column		-20.42** (9.35)	7.65 (5.80)	-0.59*** (0.13)	31.29** (11.29)	-14.40*** (6.90)	-17.11** (6.87)
Individual characteristic from column×PFMC		6.49 (11.11)	11.02 (6.44)	-0.59*** (0.16)	-10.95 (6.65)	8.36** (3.81)	-15.20** (5.58)
Control Mean	214.38	214.38	214.38	214.38	214.38	214.38	214.38
Observations	33,178	33,178	33,178	33,178	33,178	33,178	33,178
Clusters	13	13	13	13	13	13	13
Adj. R-Squared	0.2710	0.2710	0.2710	0.2714	0.2710	0.2711	0.2711

Source: DOD Data. Notes: This table reports OLS coefficients for the main effect of the PFMC (first row, Cols 1-7) and the heterogeneous treatment effect characteristics (second and third rows, Cols 2-7). All regression specifications (Cols 1-7) include the treatment effect indicator (PFMC) and the following covariates: a quadratic in age, number of dependents, indicators for female, married, minority, a summer entry and education levels (high school grad is omitted), AFQT score, enlistment term, average monthly pay in the first year, AIT length, and the number of months deployed in the year. Panel B specifications also include the credit score and the aggregate monthly payment amount from the previous year. Individuals missing this data are given a zero value and a missing indicator is used. All regressions include fixed effects for location and month. Heteroskedasticity robust standard errors, clustered at the AIT location level (N=13), are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

Appendix

Appendix Table 1. Covariate Regression Results				
Outcome: Treatment Indicator	(1)	(2)	(3)	(4)
Variable	Baseline Specification	Add Demographics	Add Education & Ability	Add Economic Exposure
Location fixed effects	Y	Y	Y	Y
Month fixed effects	Y	Y	Y	Y
Demographic controls	N	Y	Y	Y
Education & ability controls	N	N	Y	Y
Economic exposure controls	N	N	N	Y
Panel A. Administrative Data Sample				
Observations	81,631	81,631	81,631	81,631
R-Squared	0.9145	0.9146	0.9147	0.9147
Partial R-Squared	-	0.00001	0.00012	0.00006
Clusters (location level)	13	13	13	13
p-value of Joint F-Test	-	0.9606	0.2235	0.2815
Panel B. Matched Credit Subsample				
Observations	33,167	33,167	33,167	33,167
R-Squared	0.9203	0.9204	0.9205	0.9205
Partial R-Squared	-	0.00001	0.00014	0.00005
Clusters (location level)	13	13	13	13
p-value of Joint F-Test	-	0.7131	0.3989	0.8835

Source: DOD and Credit Bureau Data. This table reports the results of OLS regression estimates of Equation 2. The partial R-squared reflects the difference in the R-squared for each column and the preceding column. The p-values reflect the results of an F-test of the joint significance of all the included individual characteristics in each column on the treatment variable. Standard errors are clustered at the AIT location level (N=13). ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

Appendix Table 2. PFMC Effects on TSP Contributions in Years 1-4					
Outcome	(1) Y=Avg Mo. Contribution	(2) Pr(Y>\$0)	(3) Pr(Y≥\$100)	(4) Pr(Y≥\$200)	(5) Pr(Y≥\$300)
Panel A: Year 1 Thrift Savings Plan Contribution Decisions					
PFMC Effect	19.93 **	15.05 **	8.41 **	3.66 *	0.93 **
Std Err	(8.06)	(5.31)	(3.64)	(1.77)	(0.34)
Control Mean	17.27	12.04	7.27	3.66	0.68
Adjusted R ²	0.0971	0.1037	0.0817	0.0614	0.0115
Observations	82,211	82,211	82,211	82,211	82,211
Panel B: Year 2 Thrift Savings Plan Contribution Decisions					
PFMC Effect	14.03 **	13.46 **	5.28 *	2.50 **	0.27
Std Err	(5.98)	(4.66)	(2.91)	(1.06)	(0.39)
Control Mean	28.52	15.09	10.49	6.33	2.88
Adjusted R ²	0.0611	0.0815	0.0567	0.0438	0.0172
Observations	70,782	70,782	70,782	70,782	70,782
Panel C: Year 3 Thrift Savings Plan Contribution Decisions					
PFMC Effect	9.86	11.52 **	3.66	1.10	0.17
Std Err	(6.33)	(4.61)	(2.84)	(1.42)	(0.46)
Control Mean	28.89	16.16	10.96	6.36	2.52
Adjusted R ²	0.0614	0.0743	0.0542	0.0440	0.0192
Observations	59,609	59,609	59,609	59,609	59,609
Panel D: Year 4 Thrift Savings Plan Contribution Decisions					
PFMC Effect	6.94	8.08 *	2.18	1.25	0.65
Std Err	(6.74)	(4.10)	(2.66)	(1.40)	(0.61)
Control Mean	30.28	17.34	11.09	6.38	2.88
Adjusted R ²	0.0662	0.0687	0.0577	0.0461	0.0304
Observations	44,655	44,655	44,655	44,655	44,655

Source: DOD data. Notes: This table reports the results of OLS estimates of Equation 1 and the coefficient reported (PFMC Effect) is for the binary treatment variable. The outcomes for each specification are listed in the columns. Heteroskedasticity robust standard errors, clustered at the AIT location level (N=13 clusters), are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

Appendix Table 3. PFMC Effects on Credit Outcomes in Year 1				
Outcome Variable	(1) Y = Outcome	(2) Pr (Y>0)	(3) Pr (Y>90th %ile)	(4) Y Y>0
Panel A: Cumulative Credit Balance				
			90th %ile =	\$ 19,421
PFMC Effect	-636.24 ** (253.68)	-1.93 (1.13)	-1.51 (0.01)	-653.13 ** (280.58)
Control Mean	6,667.82	84.76	0.11	7,867.16
Adj R2	0.3311	0.0929	0.1852	0.3077
Observations	33,178	33,178	33,178	27,881
Panel B: Credit Card Balance				
			90th %ile =	\$2,919
PFMC Effect	-101.68 * (54.57)	-4.59 (2.68)	-1.72 (0.01)	-74.63 (99.69)
Control Mean	937.04	55.05	0.11	1,702.23
Adj R2	0.2230	0.0586	0.1292	0.2593
Observations	33,178	33,178	33,178	17,152
Panel C: Automobile Loan Balance				
			90th %ile =	\$14,560
PFMC Effect	-314.93 (248.85)	-2.02 (1.55)	-0.87 (1.14)	-386.42 (542.79)
Control Mean	3,529	26.87	11.31	13,136
Adj R2	0.1883	0.1767	0.0972	0.0648
Observations	33,178	33,178	33,178	8,431
Panel D: Finance Loan Balance				
			90th %ile =	\$817
PFMC Effect	-124.36 * (62.24)	-4.64 ** (2.09)	-2.20 (1.53)	-110.31 (143.41)
Control Mean	405	14.27	8.13	2,838
Adj R2	0.2096	0.0903	0.0914	0.2820
Observations	33,178	33,178	33,178	5,346
Panel E: Credit Balances in an Unpaid Status				
			90th %ile =	\$5,763
PFMC Effect	-62.51 (104.04)	-1.55 (1.50)	0.58 (1.24)	-184.24 (232.21)
Control Mean	404.98	14.27	64.82	2,838
Adj R2	0.5611	0.3794	0.3996	0.4981
Observations	33,178	33,178	33,178	16,203
Panel F: Aggregate Monthly Payment				
			90th %ile =	\$561
PFMC Effect	-37.25 *** (11.78)	-5.77 ** (2.08)	-1.81 (1.07)	-31.58 ** (12.06)
Control Mean	214	72.67	9.82	295
Adj R2	0.2710	0.0637	0.1645	0.2684
Observations	33,178	33,178	33,178	23,840
Panel G: Adverse Legal Actions				
			90th %ile =	0.00
PFMC Effect	-0.06 * (0.03)	-1.03 *** (0.31)		-0.09 (0.60)
Control Mean	0.16	4.77		3.44
Adj R2	0.4472	0.2101		0.4948
Observations	33,178	33,178		1,556
Panel H: Credit Score				
			90th %ile =	695
PFMC Effect	-0.26 (2.95)		0.37 (0.76)	
Control Mean	581		9.60	
Adj R2	0.3676		0.2484	
Observations	29,843		29,843	

Source: DOD and Credit Bureau Data. Notes: This table reports the results of OLS estimates of Equation 1 and the coefficient reported (PFMC Effect) is for the binary treatment variable. The outcomes for each specification are listed in the columns. Heteroskedasticity robust standard errors, clustered at the AIT location level (N=13 clusters), are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

Appendix Table 4. PFMC Effects on Credit Outcomes in Year 2				
Outcome Variable	(1) Y = Outcome	(2) Pr (Y>0)	(3) Pr (Y>90th %ile)	(4) Y Y>0
Panel A: Cumulative Credit Balance				
			90th %ile =	\$ 22,195
PFMC Effect	-277.84 (391.60)	5.90 * (3.30)	0.63 (0.01)	-358.32 (416.89)
Control Mean	8,851.54	89.98	0.11	9,805.92
Adj R2	0.1710	0.5377	0.0917	0.1550
Observations	28,053	33,178	33,178	25,178
Panel B: Credit Card Balance				
			90th %ile =	\$3,461
PFMC Effect	138.92 (88.74)	2.17 (2.72)	1.90 (0.01)	205.20 (139.24)
Control Mean	1,096.76	56.59	0.11	1,931.86
Adj R2	0.1078	0.0396	0.0654	0.1246
Observations	28,053	28,053	28,053	15,376
Panel C: Automobile Loan Balance				
			90th %ile =	\$15,816
PFMC Effect	-205.45 (410.78)	-1.25 (3.20)	0.72 (0.86)	-227.26 (468.65)
Control Mean	4,639	35.19	11.32	13,143
Adj R2	0.0840	0.0848	0.0446	0.0237
Observations	28,053	28,053	28,053	9,293
Panel D: Finance Loan Balance				
			90th %ile =	\$1,710
PFMC Effect	-16.36 (72.23)	1.59 (1.90)	0.84 (1.06)	-181.08 (278.81)
Control Mean	589	22.17	9.70	2,646
Adj R2	0.0797	0.0554	0.0426	0.1303
Observations	28,053	28,053	28,053	6,694
Panel E: Credit Balances in an Unpaid Status				
			90th %ile =	\$8,455
PFMC Effect	-140.71 (168.95)	-1.00 (1.49)	-0.19 (1.08)	-192.15 (320.43)
Control Mean	588.58	22.17	86.66	2,646
Adj R2	0.3423	0.2696	0.2228	0.2810
Observations	28,053	28,053	28,053	16,010
Panel F: Aggregate Monthly Payment				
			90th %ile =	\$668
PFMC Effect	-0.27 (21.36)	0.16 (2.32)	0.72 (1.43)	-0.54 (22.52)
Control Mean	287	77.41	10.34	369
Adj R2	0.1260	0.0386	0.0772	0.1226
Observations	28,053	28,053	28,053	21,616
Panel G: Adverse Legal Actions				
			90th %ile =	0.00
PFMC Effect	-0.06 (0.06)	-0.38 (0.46)		-1.11 (1.08)
Control Mean	-277.84	5.90		-358.32
Adj R2	0.2595	0.1178		0.3530
Observations	28,053	28,053		1,177
Panel H: Credit Score				
			90th %ile =	0
PFMC Effect	13.00 (0.00)		1300.00 *** (0.00)	
Control Mean	0		0.00	
Adj R2	0.0000		0.0000	
Observations	0		0	

Source: DOD and Credit Bureau Data. Notes: This table reports the results of OLS estimates of Equation 1 and the coefficient reported (PFMC Effect) is for the binary treatment variable. The outcomes for each specification are listed in the columns. Heteroskedasticity robust standard errors, clustered at the AIT location level (N=13 clusters), are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

Appendix Table 5. PFMC Effects on Retention, Years 1-4

Outcome	Pr (Serving at End of Yr 1), %	Pr (Serving at End of Yr 2), %	Pr (Serving at End of Yr 3), %	Pr (Serving at End of Yr 4), %
	(1)	(2)	(3)	(4)
PFMC Effect	-1.07	0.55	-0.10	0.39
Std Err	(1.36)	(1.11)	(0.96)	(1.34)
Location fixed effects	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y
Control Mean	86.19	72.18	75.00	94.12
Observations	65,792	65,263	46,897	17,038
Clusters	13	13	13	13
Adj R-Squared	0.0809	0.0675	0.1834	0.0596

Source: DOD and Credit Bureau data. Sample as specified in Appendix 1. The table reports OLS estimates of Equation 1. Each outcome is conditioned on those with term lengths less than or equal to the outcome horizon. Standard errors are clustered at the AIT location (base) level. ***, **, * represent statistical significance at the 1%, 5% and 10% level.

Appendix Table 6. OLS Estimates of the PFMC Effects on Credit Market Outcomes (Active Credit Records Only)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Credit Outcomes in Years 1-2								
	Cumulative Credit Account Balance in Yr 1, \$	Aggrgeate Monthly Payment in Yr 1, \$	Adverse Legal Action Index in Yr 1, #	Credit Score in Yr 1, #	Cumulative Credit Account Balance in Yr 2, \$	Aggrgeate Monthly Payment in Yr 2, \$	Adverse Legal Action Index in Yr 2, #	Credit Score in Yr 2, #
PFMC Effect	-721.03**	-42.10***	-0.060	-0.25	-264.82	-2.21	-0.097	-3.66
Std Err	(268.41)	(12.72)	(0.035)	(2.95)	(598.37)	(26.66)	(0.085)	(3.45)
Control Mean	7,317.71	237.07	0.17	580.76	9,370.13	300.01	0.25	586.84
Observations	29,843	29,843	29,843	29,843	22,666	22,666	22,666	22,666
Clusters	13	13	13	13	13	13	13	13
Adj. R-Squared	0.3131	0.2578	0.4465	0.3676	0.1657	0.1171	0.2644	0.2679

Source: DOD and Credit Bureau data. The table reports OLS estimates of Equation 1 as in Table 5 but the sample is restricted to individuals with matched and active credit records. Standard errors are clustered at the AIT location (base) level. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

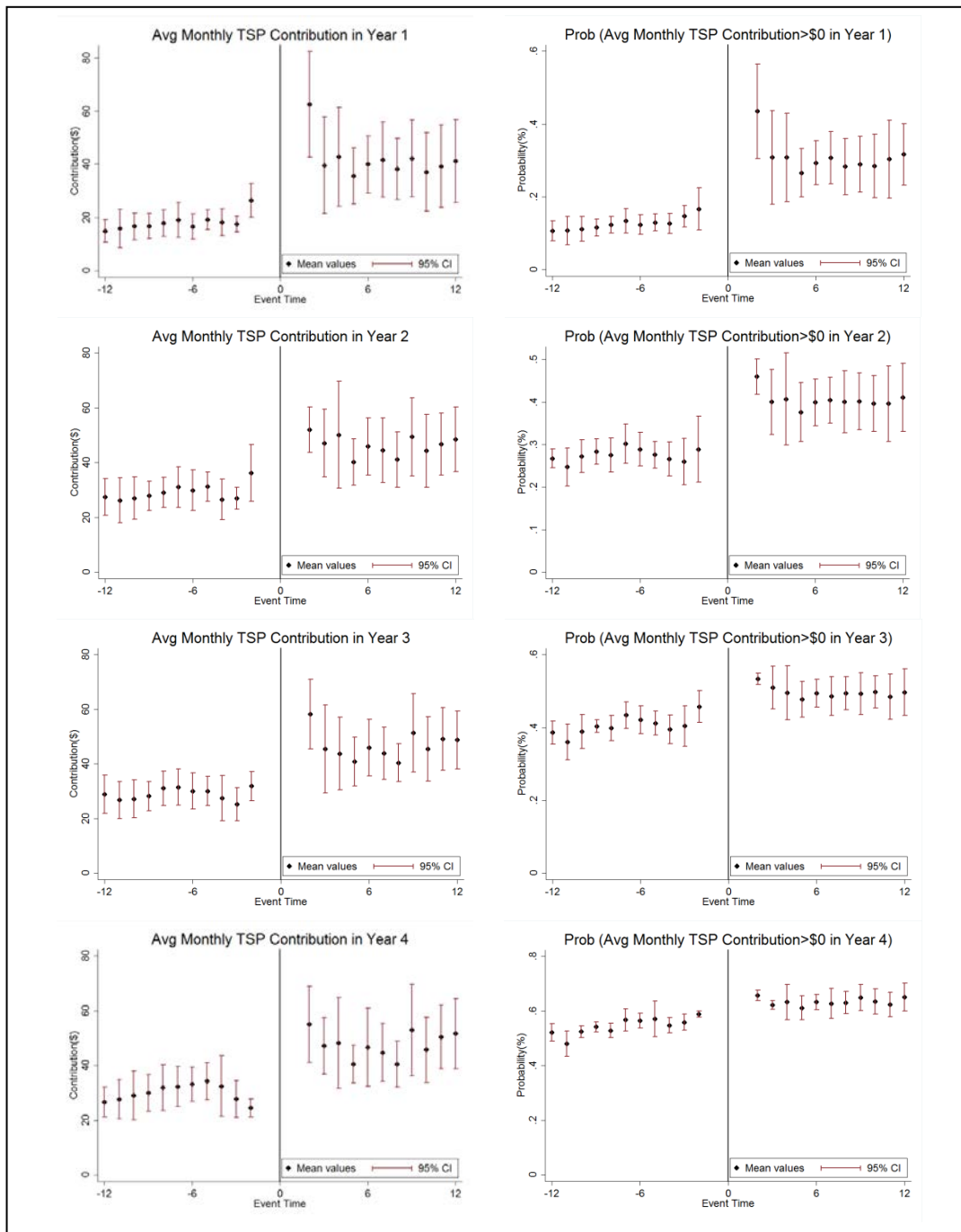


Figure 3. Event Studies for Retirement Savings (Table 5 Panel A) Outcomes

Source: DOD and credit bureau data. In the left panels, the y-values depict the mean of the average monthly TSP contributions in years 1-4 for all individuals by event month. In the right panels, the y-values depict the mean of the probabilities of TSP participation in years 1-4.

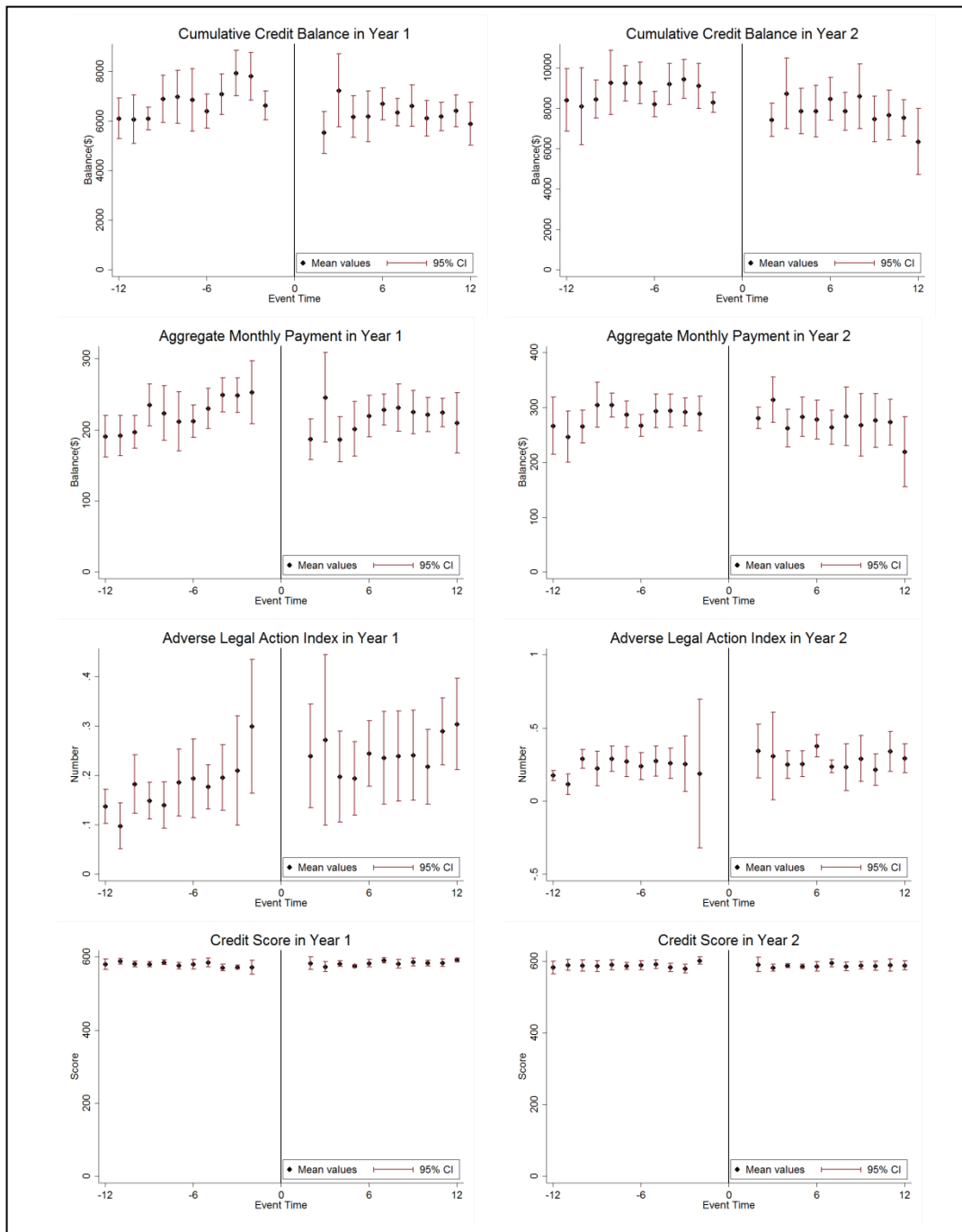


Figure 4. Event Studies for Credit (Table 5 Panel B) Outcomes

Source: DOD and credit bureau data. In the left panels, the y-values depict the mean of the cumulative account balance, aggregate monthly payment, adverse legal action index and credit score in year 1 for all individuals by event month. The right panels depict the same values for year 2 credit outcomes.

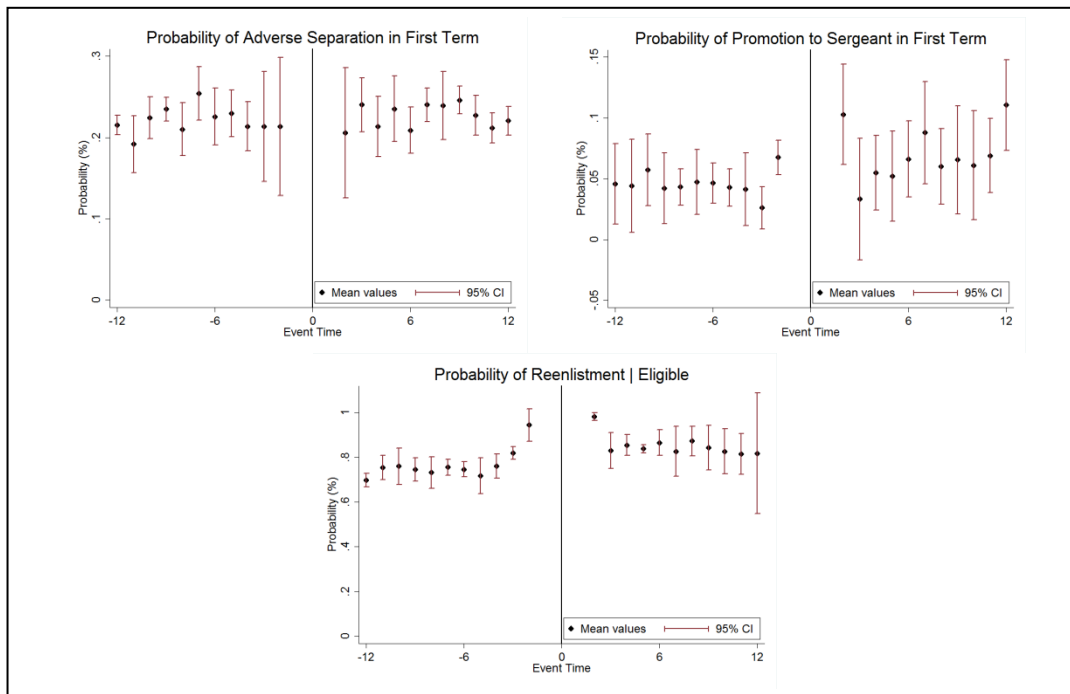


Figure 5. Event Studies for Labor Market (Table 5 Panel C) Outcomes

Source: DOD and credit bureau data. The y-values depict the mean of the probability of an adverse first term separation, early promotion to Sergeant and the probability of reenlistment (conditional on eligibility) for all individuals with initial terms ≤ 4 years by event month.