ASSESSING FINANCIAL EDUCATION:
PROMISING EVIDENCE FROM BOOT CAMP[1]

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Abstract: This study estimates the effects of financial education on a variety of economic outcomes using a natural experiment within the U.S. Army. I find that Personal Financial Management Course attendance and enrollment assistance doubles retirement savings, with effects that persist through at least two years. The course has smaller effects on credit market outcomes, reducing account balances and monthly payments in the first year after soldiers finish initial job training. The course has no significant effects on military labor market outcomes. The results suggest that financial education coupled with assistance and advice can affect several financial outcomes.

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I. 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.

In this paper, I estimate the causal effects of financial education and assistance on several financial outcomes using administrative data surrounding the 2007-2008 roll-out of a financial education program in the U.S. Army. The data provide information on retirement savings and credit decisions. Staggered implementation of the course across locations and time provides exogenous variation in financial education. I find that course attendance and its coupled enrollment assistance have substantial effects on retirement savings contributions through at least two years. To estimate course effects on other financial outcomes, I use individually matched credit bureau data and I find that the course causes moderate reductions in combined account balances and aggregate monthly payments in the first year. The course has no significant effects on the probability of having active credit accounts or individuals' credit scores. Finally, the course has no significant effects on adverse employee turnover, current productivity or retention decisions; outcomes of interest to employers considering financial education.

To date, the existing research on financial education has struggled to demonstrate a causal relationship between education and behavior. A recent review (Hastings, Madrian and Skimmyhorn 2013) highlights the challenges for research on this issue and a meta-analysis (Fernandes, Lynch and Netemeyer

2014) suggests a prior expectation of small effects of financial education on behavior. I briefly describe the literature's extant findings here. First, there is widespread evidence of financial illiteracy (Lusardi 2004, Lusardi and Mitchell 2007) and convincing evidence that literacy correlates with financial outcomes (Lusardi & Mitchell 2006). But the evidence on the effects of education is mixed.[2] While a few studies employ experimental or quasi-experimental procedures, their results do not provide convincing evidence on the causal effects of financial education in the U.S.. For example, Choi et. al. (2011) find no effects from employer education about 401(k) matches and Duflo and Saez (2003) find that information on a job benefits fair increases attendance but has small effects on savings. Cole, Paulson and Shastry (2013) find no effects from State mandates for personal finance classes in high school. Outside the U.S., there are positive findings for education in selected contexts including farmers' insurance decisions in India (Gaurav, Cole and Tobacman 2011) and micro-entrepreneurs' accounting behaviors in the Dominican Republic (Drexler, Fischer and Schoar 2014). Recently, a small but grounded opposition (Willis 2011), has questioned the efficacy of additional education. My research contributes to this literature by using plausibly exogenous variation that enables causal estimates of the effects of financial education, a variety of outcomes covering labor market decisions and multiple portions of household balance sheets, and rich administrative data that enables estimation of heterogeneous treatment effects. Given my sample's characteristics and diversity, the results provide important evidence on the effects of financial education for young, moderate-income workers and direct evidence on a population of substantial policy interest. The paper proceeds as follows: Section II describes the program; Section III summarizes the data; Section IV

[2] In the current setting, Bell, Gorin and Hogarth (2008) evaluated the pilot PFMC at Fort Bliss, Texas and found small beneficial effects. Unfortunately, the use of self-reported data, the low survey response rate and the non-experimental comparison group leave open the question of the PFMC's causal effects.

provides the empirical framework; Section V presents the results; and Section VI discusses the findings and concludes.

II. The Army's PFMC Provides a Unique Natural Experiment

Between June 2007 and August 2008 the U.S. Army and its non-profit relief society, Army Emergency Relief (AER), implemented a mandatory eight hour financial education course called the Personal Financial Management Course (PFMC) for new enlistees as part of Advanced Individual Training (AIT).[3] The stated purpose of the 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."[4] AER developed the course with the assistance of a contractor, San Diego City College (SDCC), and staggered its implementation at thirteen locations as depicted in Figure I.[5]

[Insert Figure I about here]

While the PFMC implementation month at each base is known, the exact course start dates are not. In addition, individual level data on course attendance is unavailable. As a result, I impute my treatment variable (PFMC attendance)[6] and define an individual's treatment status using their AIT start date relative to

[3] All enlisted soldiers attend AIT immediately following basic training (10 weeks in length) 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.

[4] 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 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.").

[5] SDCC delivered pilot training at Fort Bliss, TX from 2003-2006 and after four years of course refinement, the Army contracted with SDCC to implement the PFMC at all AIT locations.

[6] My imputation strategy uses administrative data on individual entry dates, basic training durations, and future assignments.

the PFMC start date at their AIT location. Individuals who started AIT after the PFMC began at their location are assigned a value of 1; those who started before are assigned a value of zero. As a result, the treatment and control groups at each location are separated by time, but the staggered implementation creates some common support across these groups at different locations.

Treatment includes education, assistance in signing up for savings plans, and advice provided by the instructors during breaks or in response to specific questions. The course was typically completed in two sessions in which civilian instructors, trained by SDCC, gave lectures on the topics in Table I, following standardized slides and course booklets. The PFMC hours replaced 8 hours of leisure time for new soldiers.[7] The course covered both principles (e.g., the time value of money) and some rules of thumb (e.g., obtain a copy of your credit report annually), and focused on the financial decisions young workers are most likely to face (e.g., buying a car is included; buying a home is not). For the TSP outcomes, treatment should be thought of as education coupled with assistance, since instructors may have assisted with Thrift Savings Plan (TSP) enrollment at some locations.[8] For outcomes related to credit, treatment should be thought of only as education.

[Insert Table I about here]

[7] Whether an 8 hour course is sufficient in length to meet the program's objectives is unclear. On the one hand, this seems far too short given the amount of financial literacy required to succeed in today's economy. On the other hand, time is 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 Sheradden (2002) found that a financial education program for IDAs increased savings for low-income households with diminishing effects after 8-10 hours. Drexler, Fischer, and Schoar (2014) found positive effects from an accounting course lasting only 15 hours. In either event, a course of relatively short duration may have limited effects on behaviors involving complex combinations of analytic skills, life experience, and self-control.

[8] Author interviews with AER, SDCC and PFMC instructors (2011-2012). Enrollment assistance varied by location and time (e.g., at some locations forms were distributed; at others SDCC personnel assisted in form completion and/or submission). Unfortunately, neither AER nor SDCC collected detailed data on the assistance variation and I cannot separately identify the effects of education and assistance for TSP outcomes.

III. Army and Credit Bureau Administrative Data Enable Several Analyses

I use administrative data from the Army and a national credit bureau and focus my analysis on course topics including retirement savings (e.g., the Thrift Savings Plan), credit decisions (e.g., debt levels and payments) and labor market outcomes (e.g., adverse separations and reenlistment decisions). The military administrative data is a repeated cross section and covers all active duty Army soldiers entering service from May 2006-June 2009.[9] I restrict the sample to individuals attending AIT at each location within 12 months of PFMC implementation to minimize time-varying enlistment differences. This process generates an administrative data sample of n=82,211 individuals for my analyses in the first year after an individual starts AIT. Since individuals progressively leave the military, my samples for years 2-4 are reduced to n=70,782, n=59,609 and n=44,655 respectively.[10] To avoid contamination between my experimental groups, I omit individuals starting AIT in the month preceding, month of, and month following PFMC implementation and those individuals whose AIT start date and course length produce overlap with the PFMC implementation date. Since I assume that individuals were treated in the month they began AIT, measurement error arising from outcome observation prior to treatment will make the estimates here lower bounds. The Army demographic data, measured at AIT start, contain a rich set of characteristics potentially related to financial decision-making including demographic data (age, gender, marital status, number of dependents, and race), human capital data (education, Armed Forces Qualification Test [AFQT] percentiles, and enlistment timing), and economic factors (length of AIT, deployments, and compensation).[11]

[9] I obtained data was obtained from the Army's Office of Economic and Manpower Analysis.
[10] Treatment is unrelated to attrition in years 1-4. See Appendix Table A2 for results.
[11] I restrict my sample to those with complete individual characteristic data.

Retirement Savings Outcomes

Since the most significant portion of the curriculum (2 of 8 total hours) is dedicated to retirement savings and the Thrift Savings Plan (TSP), I evaluate TSP decisions (average monthly contributions in each year and the probability of participation in each year) for an individual's first four years in the military.[12] The TSP is a tax-advantaged retirement program available to federal employees, including the military, with participation rules similar to a 401(k). Initial enrollment must occur via a paper form; subsequent contributions must occur via payroll deduction; changes can be made online or at a finance office; and there is a loan option.[13] While military members do not receive matching funds (the Army has a separate defined benefit pension), contributions are tax-deferred,[14] and individuals can select from several funds, all of which have low expense ratios.[15] I observe monthly TSP contributions and measure the TSP outcome horizons relative to an individuals' AIT start month (i.e., Yr 1 outcomes reflect an individual's average monthly TSP contributions from the month after they start AIT through 12 months and the participation indicator reflects any contributions during this same period; Yr 2 covers months 13-24, Yr 3 covers months 25-36; and Yr 4 covers months 37-48). Mean control group participation is 12%, 15%, 16%, and 17% in years 1-4 respectively. While I observe monthly contributions, my view of an individual's retirement portfolio remains incomplete as I cannot observe IRAs or other 401(k) accounts. But the TSP is an important part of Active

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[12] Average annual TSP contributions are winsorized at the 1st and 99th percentiles.

[13] These features and the use of payroll data minimize the chances of unobserved savings or withdrawals.

[14] On Oct 1, 2012 the TSP established a Roth (post income tax, tax free) option for all members. Time fixed effects account for this change.

[15] The default fund is a Government Securities fund. Other funds include: Fixed Income Securities, Common Stock, Small Cap Stock, International Stock and Lifecycle funds. Since 2006, the average net expense ratio has not exceeded 0.031%.

Duty Army members' retirement plans,[16] and the incomplete picture is less concerning for this young population with limited labor market experience.

Whether saving for retirement in a tax-deferred account is optimal for new enlistees remains an open question. On the one hand, many have relatively few expenses and unusual job security. The tax advantages of the TSP and the time value of money make early investments powerful. On the other hand, the group has a moderate level of income, a low marginal tax rate,[17] access to the military's DB pension, and some existing debt (mean credit debt for this sample is about \$6,500, See Table III). A simple model of consumer decision-making with uncertainty in two periods suggests that individuals will tradeoff present and future consumption until their expected marginal utilities for an additional dollar in each period equate. But such decisions require numeracy and financial literacy, both of which are costly to obtain.[18] Financial education might affect decision-making by improving numeracy (e.g., computing net present values), increasing literacy (e.g., showing tax advantages of the TSP), or lowering enrollment costs (psychological or time). The latter mechanisms seem most likely since the PFMC sought to improve soldiers' understanding of the benefits of TSP savings and to simplify enrollment. Conversely, financial education might harm financial outcomes if the bundling of education and enrollment assistance leads some soldiers to save in the TSP without changing their spending behavior in other areas, forcing them to use other costlier forms of credit. Overall, I remain agnostic on "optimal" TSP participation decisions and instead evaluate the PFMC's effects against its stated goals of increasing savings and reducing debt.

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[16] As a benchmark, 23.4% of all Active Duty Army individuals participated in the TSP in July 2007 (roughly the midpoint of my control groups). See: http://www.frtib.gov/pdf/minutes/2007Jul.pdf). Accessed May 9, 2014. The lower participation rates in my sample are most likely due to younger ages, shorter tenures, and lower incomes.

[17] Using 2008 Military Basic Pay and tax brackets, an individual of rank E-2 with less than 2 years of service (typical for my sample), would fall in the 15% tax bracket for any filing status.

[18] See Hastings, Madrian and Skimmyhorn (2013) for a more detailed model.

Individual Credit Market Outcomes

Given the cost of credit bureau data, I match a random subsample of individuals to their credit bureau data from April of each year in 2007-2010.[19] The data in my credit subsample contributes to this analysis in several ways.[20] First, it provides a more complete picture of the effects of financial education given the PFMC's topics, even though it does not capture payday loans, auto title loans, or informal lending. Second, the outcomes enable identification of the PFMC effects unconfounded by any direct assistance. Finally, the data enables more precise estimation of the PFMC effects since I can control for individuals' credit outcomes for those with matching records for the year prior to their entry. I focus my analysis on PFMC program goals (i.e., Reducing Debt) and topics (i.e., Develop a Spending Plan, Essentials of Credit and Car Buying)[21] by analyzing four outcomes: cumulative credit account balances (the sum of credit card, finance loan, automobile loan and unpaid account balances), aggregate monthly minimum payments for all credit balances, an adverse legal action index (sum of foreclosures, liens, judgments and bankruptcies), and credit scores. I measure the credit outcome horizons using credit data from the first April after AIT completion (i.e., Yr 1 outcomes reflect data from the first April between the first and twelfth month after an individual finishes AIT; Yr 2 outcomes reflect data between months 13-24 after AIT completion).[22] [23]

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[19] 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 Appendix Table A1for evidence that the probability of match and of having active credit are both unrelated to treatment status.

[20] For a summary of potential uses of credit bureau data, see Avery, Calem and Canner (2003).

[21] 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.

[22] In Figure II, I show the relationship between PFMC dates and credit archive dates. The credit 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 since they completed AIT within 12 months of the final credit bureau data archive (April 2010). I create a

Military Labor Market Outcomes

Since financial stress may undermine job performance (Carrell & Zinman 2014, PFMC Memorandum of Understanding 2003), I use Army data to evaluate three labor market outcomes potentially related to financial decision-making.[24] To evaluate job performance, I observe whether an individual is adversely separated from the military. To evaluate current productivity, I observe whether an individual is rapidly promoted to a supervisory position (Sergeant) during their first term. 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, to evaluate firm attachment, I observe whether or not individuals opt for another term in the Army if they have been offered the opportunity to reenlist. I measure all three labor market outcomes during an individual's first enlistment term in the military. While employer-employee relations in the military differ from other private and public sector jobs, the U.S. military is a volunteer force and these outcomes can provide some insight into whether employer-funded financial education offers a return on investment in lower turnover or higher productivity.

IV. Using the Staggered Roll-Out to Estimate the PFMC's Effects

Recall that at each base, control group (no PFMC attendance) members precede treatment group (PFMC attendance) members. Because the roll-out is

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reduced year 2 sample by removing censored treatment group members and comparable control group members based on their location, training duration and event time. 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). This process ensures the comparability of the control and treatment groups on observable characteristics.

[23] All credit outcomes are winsorized at the 1st and 99th percentiles.

[24] Military data, available through Aug 2013, censors my visibility of the final treatment group (AIT start Aug 2009) to those with initial enlistment terms less than or equal to 4 years. I limit the control group similarly.

staggered over time by location, I can control for different calendar time and location effects on financial outcomes of interest among all sample members. Figure II shows the staggered roll-out, the treatment and control groups at each base, and the data availability for my outcomes of interest.

[Insert Figure II about here]

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

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

In this model Y_{ijt} is a financial or labor market outcome for individual i who started 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 outcome for the previous year.[25] 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 card debt) depend on the PFMC curriculum. I expect positive signs on all TSP outcomes and credit scores. I expect negative signs on cumulative credit account balances, aggregate monthly payments, and adverse legal actions since the course advises soldiers to establish a budget and

[25] For those with matched credit records but missing data, zeros are imputed and a missing indicator is used. The results hold in a robustness check using only active credit records (See Appendix Table A8 Panel A).

reduce their debt levels. However, there is the possibility that the course could increase a soldier's ability to secure better interest rates and this could lead him to take on more debt (e.g., auto loans or credit cards) with comparable payment levels. 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).[26] Identification of causal estimates of the PFMC effects on financial outcomes requires that conditional on an individual's AIT location, start month, and individual characteristics, treatment assignment is unrelated to the individual error terms. I test this assumption below.

Summary Statistics and Covariate Regressions Suggest Valid Identification

Several features of the PFMC implementation plan suggest a potentially valid natural experiment and I argue, justify 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, implementation dates were determined by AER Headquarters and SDCC based on discussions with local military leaders without notifying or soliciting information from individual soldiers or the 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 PFMC start dates.[27] Third, the 8 hour duration of the PFMC is insignificant when compared to the much longer (1-12 month) AIT course and a far more significant career choice to join the military, so selection by individuals is highly unlikely. Fourth, concerns over strategic implementation are mitigated by my use of location (base)

[26] Given the small number of clusters, I complete robustness checks using 10,000 iterations of the wild bootstrap procedure (Cameron et. al. 2008) and nearly all of my results hold. See Appendix Table A3.

[27] Author interviews with AER personnel and the SDCC contract leader (2011-2012). Both parties reported that the PFMC implementation schedule was driven by the ability to recruit and train instructors. 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.

fixed effects in my regression specifications, which remove the average effects for each location. Moreover, there is little reason to believe that the local commanders could affect implementation timing based on any outcome trends, since they have no visibility on TSP savings rates or credit outcomes. Similarly, the use of time (month-year) fixed effects ensures that I remove the effects of any specific time periods from my estimates. To provide additional evidence here, I complete robustness checks of my main specification by adding unique time trends by location and all results hold (see Appendix Table A9). Finally, in Table II, the summary statistics for the individual characteristics reveal covariate balance across control (PFMC) and treatment (No PFMC) groups for both samples.

While there are several statistically significant differences in the means across groups (e.g., Age, AFQT percentile), these univariate differences are due primarily to the large sample size and do not reflect economically significant differences (e.g., the age difference is 0.18 years [=66 days] and the AFQT difference is less than one percentile). The pay differences can be attributed to annual pay increases for the military during this period and the mechanical time difference between the control and treatment groups.[28] More importantly, univariate balance is not required for every characteristic; instead the zero conditional mean assumption requires that the two groups are similar given the conditional expectation function: $E\left(\varepsilon_{ijt} \middle| X_i, \varphi_j, \delta_t\right) = 0$. While I cannot directly test this assumption, I use the relationship between my observable characteristics and treatment to model the relationship between unobservable characteristics and treatment in the spirit of Altonji, Elder and Taber (2005) using Equation 2:

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

[28] Basic pay for an E-1 (Private) increased by an average of 3.22% each year during the sample period. Other cash benefits (e.g., subsistence allowance) increased too.

In this specification I regress my treatment variable ($PFMC_i$) on my individual characteristics (X_i) and fixed effects, and evaluate whether or not these characteristics jointly predict treatment. I report the results from the F-tests of the joint significance of σ in the bottom row of Table II, and they suggest that my observable characteristics are jointly unrelated to treatment in the administrative data sample (Col. 3, p=0.1171) and the credit subsample (Col. 6, p=0.4415).[29]

To validate random sampling for my credit outcomes, I estimate Equation 3 to determine if treatment is related to the probability of an individual being matched by the credit bureau:

$$Z_i = \tau + \pi \cdot PFMC_i + \varphi_j + \delta_t + \epsilon_{ijt} \tag{3}$$

Z_i is a binary indicator for a record being 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 Appendix Table A1 Panel A (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).[30]

Since my labor market outcomes use restricted samples from the full administrative sample (terms less than or equal to 4 years for the involuntary separation and early promotion outcomes; terms less than or equal to 4 years and offered reenlistment for the reenlistment outcome), I evaluate whether treatment is correlated with presence in these samples. I estimate an alternate version of

[29] The individual characteristics explain a trivial portion of the variation in treatment (partial R-squared values for the individual characteristics are 0.0002) in both samples.

[30] To preserve the credit sample size, I assign zeros for records that are matched but coded as inactive since businesses and the credit bureau have the incentive to report all account balances. To ensure that there is no selective imputation based on treatment status, I also estimate Equation 3 using an indicator for active credit as the outcome. The results in Appendix Table A1 (Cols 2 and 4) reveal that conditional on a matching record, treatment is unrelated to the probability of having active credit in year 1 and 2 (p=0.5353 and p=0.9934 respectively). This follows my intuition since the PFMC promoted responsible credit use and not credit avoidance. I complete robustness checks for the assumption that matched but inactive records should be imputed with zeros by completing all credit market regressions with only matched and active records (Appendix Table A8 Panel A). The results are qualitatively similar. Together, these checks confirm that the credit subsample selection was random.

Equation 3 where the outcome (Z_i) is an indicator for presence in each labor market sample. The results in Appendix Table A1 Panel B (Cols 1 and 2) demonstrate that treatment is unrelated to presence in my labor market samples (p=0.528 and p=0.215 respectively), validating my use of these groups.

[Insert Table II about here]

V. Empirical Evidence Suggests Important Effects from the PFMC

In Table III, I present the summary statistics for all outcomes by 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. These means reflect mixed results, but they do not account for potential differences in the groups based on location or time. The time effects may be especially important given that PFMC implementation occurred from June 2007-August 2008 and treatment group members are typically observed, about one calendar year later than the control group. If the U.S. recession disproportionately affected members of the treatment group, then reliable estimates of the PFMC's effects must account for the effects of time.

[Insert Table III about here]

The preferred estimates for the PFMC effects come from my multivariate regressions (Equation 1). In Table IV, 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) with standard errors clustered at the location level (N=13).[31] [32] The Panel A results suggest that the PFMC has large effects on TSP outcomes in all 4 years.[33] The course increased average monthly savings by $19.93 (115%) in year 1 (Col.1, p=0.029) and $14.02 (49%) in year 2 (Col.2, p=0.038). The effects in years 3-4 ($9.75 and $7.17, Cols. 3, 4) remain positive but they are statistically insignificant. The PFMC also increased the probability of TSP participation in all 4 years, by 15.04 percentage points(pp) (125%) in year 1 (Col.5, p=0.015), 13.46 pp (89%) in year 2 (Col. 6, p=0.014), 11.56 pp (71%) in year 3 (Col. 7, p=0.015), and 8.23 pp (47%) in year 4 (Col. 8, p=0.071). These effects persist through the 90th percentile of the TSP contribution distribution in both years.[34] The results suggest a "catch up" effect for the control group and not contribution decreases by the treatment group, potentially explained by the stickiness of the original contribution rates among the treated. Overall, the bundled intervention substantially increased retirement savings for treated members; the 2 year differences amount to a future balance difference of over $4,200.[35]

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[31] Given the small number of clusters and their unequal sizes (See Figure I), I complete robustness checks using 10,000 iterations of the wild bootstrap procedure (Cameron, Gelbach and Miller 2008) in Appendix Table A3. For seven of nine outcomes, the statistical significance levels are identical. The exceptions are the probability of TSP participation in Yr 4 (from p=0.063 to p=0.102) and the adverse legal action index in Yr 1 (from p=0.078 to p=0.149).

[32] I complete two types of robustness checks for my TSP estimates and present the results in Table A8. The Panel A results reveal that Tobit estimates (for average TSP contributions) are larger and equal in their statistical significance to the main results and Logit estimates (for probability of participation) are comparable in size but have higher levels of statistical significance. The Panel B results reveal that my sample is not sensitive to potential outliers (event month group +2).

[33] To provide visual evidence for my identification strategy and to rule out concerns that there are common trends in my outcomes across locations, I complete event studies for my outcomes of interest (See Appendix Figures A1-A3). The event studies suggest that common trends across locations do not explain my results and they suggest a discontinuous change in the TSP and some credit market outcomes at the time of PFMC implementation. In robustness checks, I omit event month group +2 and find nearly identical results (See Table A8).

[34] In Appendix Table A4, I present a detailed analysis of the PFMC effects on the TSP contribution distributions for Years 1-4. The positive effects are statistically significant through at least the 90th percentile of the contribution distributions in years 1-2.

[35] Assuming a 6% real rate of return, withdrawal at 60 years of age and no contribution differences after 2 years, the future value difference of $19.93 for year 1 and $13.75 for year 2 is $4,207.

[Insert Table IV about here]

The Panel B estimates suggest important but limited effects on credit outcomes. The PFMC reduced cumulative credit balances by $635 (10%) in year 1 (Col. 1, p=0.028) but the year 2 estimate of -$235 (3%) is statistically insignificant (Col. 2, p=0.668). The course reduced aggregate monthly payments by $37 (17%) in year 1 (Col. 3, p=0.009), but the year 2 estimate of -$1.02 is statistically insignificant (Col. 4, p=0.970). The course reduced the adverse legal actions in year 1 by 0.057 (36%) with marginal statistical significance (Col. 5, p=0.078) and the year 2 estimate of a 0.086 (36%) decrease is statistically insignificant (Col. 6, p=0.317).[36] The PFMC has no effects on credit scores in year 1 (Col. 7, p=0.946) or year 2 (Col. 8, p=0.467).[37]

The Panel C results suggest no significant effects of the PFMC on the labor market outcomes. The estimates are economically small (0.82pp [0.79%], 0.09pp [0.10%], and 1.37pp [0.76%]) and statistically insignificant (p=0.4182, p=0.8751 and p=0.7821 respectively).

To provide additional visual evidence of the PFMC effects, I complete event studies for all eighteen outcomes in Appendix Figures A1-A3. To address concerns over the potential for differential outcome trending at the different locations, I complete robustness checks for all specifications that include unique time trends by location. The results (Appendix Table A9) are nearly identical to my main specifications and suggest slightly larger PFMC effects.

[36] In the case of balances and legal actions, mean effects of zero may reflect the large number of individuals with zeros. See Table III (Cols. 2, 6) for the fraction of individuals with zeros for each outcome. In Appendix Table A8 (Panel B), I complete Tobit estimates for credit outcomes and find larger and more statistically significant results. In an alternate specification for adverse legal actions I using an adverse legal action indicator (vs. index) and find that the PFMC reduces the probability of an action by 1.1pp (25%), a statistically significant (p=0.004) effect.

[37] In Appendix Tables A5 and A6, I present more detailed analyses of the PFMC effects on the credit outcome distributions for Years 1 and 2 respectively. Of note, while the PFMC effect on the number of adverse legal actions is marginally significant, the effect on the probability of any adverse legal action (Table A5, Panel G Col. 2) is statistically significant and suggests that the course reduces the probability of an adverse legal action in year 1 by about 1.03pp (22%).

PFMC Effects Differ for Some Groups

In Table V, I present OLS estimates of the PFMC effects on two outcomes (average monthly TSP contributions and aggregate monthly credit payments) by several individual characteristics of interest: gender, minority status, human capital levels (AFQT score), marital status, age, and prior year credit activity (for the aggregate monthly payment). I summarize the differential effects here by comparing the effect magnitudes (regression coefficient divided by the control mean) in the main sample (Col. 1) and each subsample (Cols. 2-7). For the Average Monthly TSP Contributions (Panel A), the magnitudes for the course effects for females (54%, Col. 2), minorities (96%, Col. 3), and married individuals (94%, Col. 5) remain large, but they are smaller than the main effects (115%, Col. 1). While I cannot identify the mechanism for these effects, potential explanations include: these groups may be less susceptible to peer effects for saving; they may come from lower socioeconomic status families and have less experience with retirement savings; or they may deliberate more and not submit the TSP enrollment forms during the course. The effect magnitudes for individuals with high AFQT scores (132%, Col. 4) are larger than the main effects, suggesting that these individuals can better process the tax advantages and time value of money principles inherent in the course. Finally, the effect magnitudes for older individuals (109%, Col. 6) are slightly smaller than the main effects. Habit formation may explain this result if these individuals' maturity, financial and labor market exposure, and the salience of retirement.

[Insert Table V about here]

In Table V Panel B, I present separate sample estimates for the aggregate monthly credit payments. The effect magnitudes for females (-29%, Col. 2), minorities (-21%, Col. 3), and married individuals (-21%, Col. 5) are larger than the main effect (-17%, Col. 1). I cannot identify the effect mechanism, but these groups might identify more with the instructors or material or their socioeconomic status might mean they have had more exposure to the credit market and potential problems prior to the course. The effect magnitudes for individuals with high AFQT scores (-23%, Col. 4) are larger than the main effects and suggest that these individuals are better able to learn and apply course concepts related to credit and debt. The estimates for older individuals (-15%, Col. 6) are nearly identical to the main estimates, but these individuals have larger baseline levels of debt and so the effect magnitudes are slightly smaller. While older individuals might have been expected to change their outcomes more than younger individuals given their maturity or greater credit market experience, habit formation in credit behaviors or higher original knowledge levels (with less opportunity for learning) might explain the smaller effects for this group. Finally, the effect magnitudes for individuals with previous credit activity (-20%, Col. 7) are larger than the main effects and again may suggest that market experience motivates additional learning and application of course concepts.

VI. Discussion and Lessons Learned from the PFMC

Benchmarking the PFMC Results

The observed retirement savings effects are economically significant (50-100%) and endure through at least two years. While there is little experimental evidence on financial education's effects on retirement savings, I review related results. Lusardi and Mitchell (2007) estimate that workplace education correlates with 18% increases in wealth; Duflo and Saez (2003) find that exposure to an employee benefit fair increases tax deferred account saving by 3-4%; and Cole and Shastry (2010) find that exposure to additional high school math (but not finance) courses increases investment income by 3-11% for women. My estimated effects are much larger, but this is unsurprising as the PFMC combined education and enrollment assistance. Given this bundling, I also compare my effect magnitudes to relevant choice architecture interventions: Madrian and Shea

(2001) find that automatic enrollment increases 401(k) participation by 103% and Carroll et. al. (2009) find that an active decision enrollment regime increases 401(k) participation by 68%. Combining education and assistance appears to achieve results as large as other leading policy options aimed at promoting retirement savings.

The PFMC effects on credit market outcomes are important but 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 (10%) on combined credit balances (i.e., credit cards, finance loans, auto loans and unpaid balances) and required monthly payments (17%). The effects on adverse legal actions (i.e., bankruptcies, foreclosures, liens and judgments) are larger (36%) but marginally statistically significant. In related wok, financial education has been linked to 10-20% changes in desired financial behaviors that include accounting behaviors for micro-entrepreneurs (Drexler, Fischer and Schoar 2014) and rainfall insurance purchases for farmers (Gaurav, Cole and Tobacman 2011). With these imperfect benchmarks in mind, the PFMC appears to be about as successful as other programs in improving financial decision-making and more successful than previous findings of no effects (e.g., Choi et. al. 2011).[38]

The PFMC has no statistically significant effects on the military labor market outcomes (separations, promotions and reenlistment). Similarly, the economics literature contains little evidence on the causal effects of financial education on labor market decisions. And while the military remains interested in financial education for reasons that include lost security clearances and readiness, it's unclear whether the observed effects reflect a lack of effectiveness or if there

[38] Short education programs might also be compared to information interventions, which have generated increased demand for better schools by 23% (Hastings and Weinstein 2008) and shown potential to mitigate consumption losses up to 1% (Stango and Zinman 2011).

is no sizable empirical link between financial distress and military job performance.[39]

Mechanism for PFMC Effects: Human Capital and Behavioral Assistance

The large effects documented above and the lack of similar findings in the literature motivates brief consideration of the program's effect mechanism. A traditional explanation suggests that the PFMC develops financial human capital that translates into improved decisions while a behavioral explanation arises from the PFMC's TSP enrollment assistance. Bettinger et. al. (2009) find large effects from similar assistance on financial aid applications and the behavioral economics literature (e.g., Carroll et. al. 2009) provides evidence on the power of enrollment policies. For the Army, such attribution may be second order, but for educators and policy makers, identification of the role of human capital versus behavioral elements improves our ability to design programs.[40] In addition, to my knowledge, there have been no previous findings in this literature demonstrating the effects of combining education and assistance for retirement savings decisions, so even a finding of bundling effects is new. Since some organizations that provide financial education may not have the authority to change policy defaults (e.g., non-profit organizations) and other organizations that have the authority have chosen not to implement opt-out defaults (e.g., the Department of Defense for servicemembers), this bundling strategy may represent the leading policy choice for increasing retirement savings.

Nonetheless, this research cannot separately evaluate these mechanisms for the TSP outcomes. However, the PFMC effects on credit outcomes, separated in time from the instruction, suggest that the behavioral explanation is at best

[39] In the latter case, these results might be thought of as falsification tests.
[40] What may be of interest to policy-makers and academics is the optimal mix of education and assistance. Behavioral assistance might be fruitless if introduced without some education on the decision at hand, but several hours of education may be unnecessary to motivate action when assistance is provided.

incomplete. The course appears to be generating human capital for some financial decisions. It seems unlikely that it would generate human capital for one set of decisions and not the others given the similarity of the instruction and the audience. Finally, the short duration of the PFMC effects on credit decisions (significant in year 1 but not in year 2) might suggest the need for additional periodic education on these topics.

Suggestive Evidence Discounting Intrabudget Transfers

The combined effects of the PFMC across financial domains provide reason for some optimism. The absence of any intrabudget transfer evidence, wherein individuals could have financed retirement savings with credit spending, is noteworthy. The year 2 results (more savings and equivalent debt levels) are encouraging and the year 1 results (more saving and less credit use) are doubly indicative that the course affects decisions in accordance with the curriculum.[41]

PMFC Effects are Likely Lower Bound Estimates for this Population

Several institutional factors suggest that my results are likely lower bound estimates of the PFMC effects. Absences among the treatment group and AIT training delays among the control group would generate contamination and bias my estimates downward. Interactions between control and treatment members after AIT at first assignments (e.g., as roommates or friends) could reduce any differences in knowledge and motivation that the PFMC imparted. Such spillovers are desirable from the military's perspective but they pose a challenge to empirical estimation.[42] Military leaders routinely help soldiers facing financial problems through counseling or requiring training after AIT. These efforts and/or

[41] The PFMC could have encouraged TSP savings for those with credit debt instead of paying off that debt first. The best counterfactual is not perfect education but what the control group did (save less and spend more on credit).

[42] This may also explain the differences in the magnitude of the treatment effects for retirement savings (likely a one-time decision) and subsequent credit outcomes after mixing has occurred.

control group members' voluntary attendance at other Army courses will also reduce the observed outcome differences.[43]

External Validity Concerns for Program Effects

As a captive group, new soldiers are likely an unusually receptive audience. Course timing seems uniquely suited for influencing financial behavior for a young group, new to the labor force, and often living alone for the first time. In addition, while individuals could not plausibly select into the military for the PFMC, they may be selecting into the military for career goals that include securing a better financial future, making these individuals "better compliers" than the average individual. Finally, the course instructors, often retired military personnel, might be trusted role models for the students, increasing their motivation to learn.[44] As a result, my findings might be most usefully applied to other groups of new workers (e.g., other service members, those in apprenticeship or union programs, and public sector workers).

PFMC Proves to be an Inexpensive Program

In addition to the PFMC's promising results, I estimate that the course costs approximately $22 per soldier.[45] While choice architecture interventions may be cheaper methods for increasing retirement savings, the PFMC's broader curriculum and the military's reluctance thus far to support opt-out defaults for military members' defined contribution (TSP) programs make the course a reasonably inexpensive alternative.

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[43] 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 other Army courses do not explain my observed effects since they were not initiated concurrently with the PFMC. If the other Army courses are compliments to the PFMC then my estimates could be upward biased, but this seems unlikely given the substantial overlap in course content.

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

[45] SDCC cost estimates are roughly half this value, but my estimates and theirs differ in the time horizons and estimated student throughput.

Explaining the PMFC's Success

While the program did not employ experimental variation in its methods or content, I briefly offer 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). Finally, the course combines teaching with assistance, generating actionable education.

In light of the financial crisis of the late 2000s and the 2009 implementation of the Credit Card Accountability Responsibility and Disclosure (CARD) Act, which may have reduced credit limits for many consumers, my observed credit effects might be driven by the law disproportionately reducing account balances for treatment group members (who are systematically observed later in calendar time than their control group counterparts). In robustness checks, I analyze a restricted sample preceding the law and my main estimates hold.[46]

Suggestions for Future Research

The key contribution of this work is demonstrating that financial education can affect short term financial outcomes, an update to the existing literature with few robust findings of beneficial effects. But several issues warrant further research. First, there are no doubt potential improvements in the curriculum design and methods for teaching financial literacy topics. Second, more attention might be devoted to the difficult task of isolating the mechanisms through which this education works (e.g., knowledge, rules of thumb, appreciation of complexity, time preferences, peer effects, or other policy nudges). The isolation of the mechanisms will be difficult but there are opportunities for learning if

[46] See Panel C of Appendix Table A8.

program administrators commit to experimental approaches. Finally, outcome evaluations over longer time horizons are important but will likely prove challenging, requiring large samples (especially for voluntary programs) and administrative outcome data.

New Evidence on the Effectiveness of Financial Education

This study estimates the effects of financial education on a variety of economic outcomes using a natural experiment within the U.S. Army. I find that Personal Financial Management Course 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, reducing 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 measures of performance, productivity and retention for soldiers early in their service. Overall, the results suggest that education, coupled with assistance and advice, can directly affect individuals' financial outcomes.

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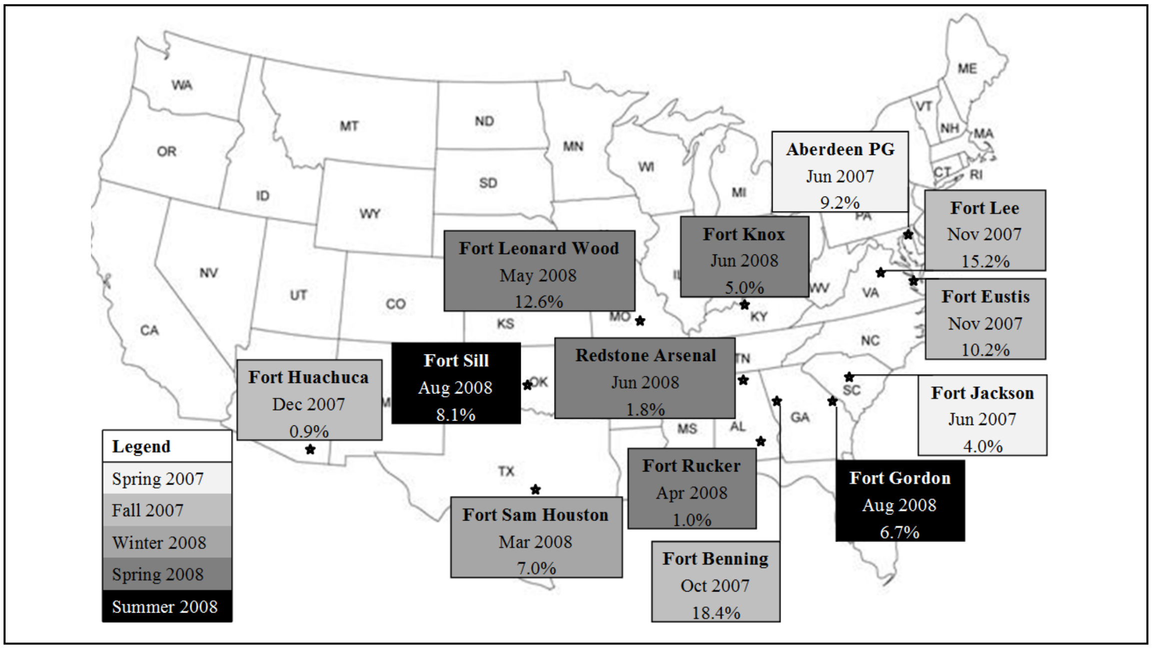

Figure I. PFMC Implementation Schedule

Source: Author compilation using Department of Defense (DOD) and Army Emergency Relief (AER) data. Percentages reflect the fraction of the administrative data sample (n=82,211) from each location.

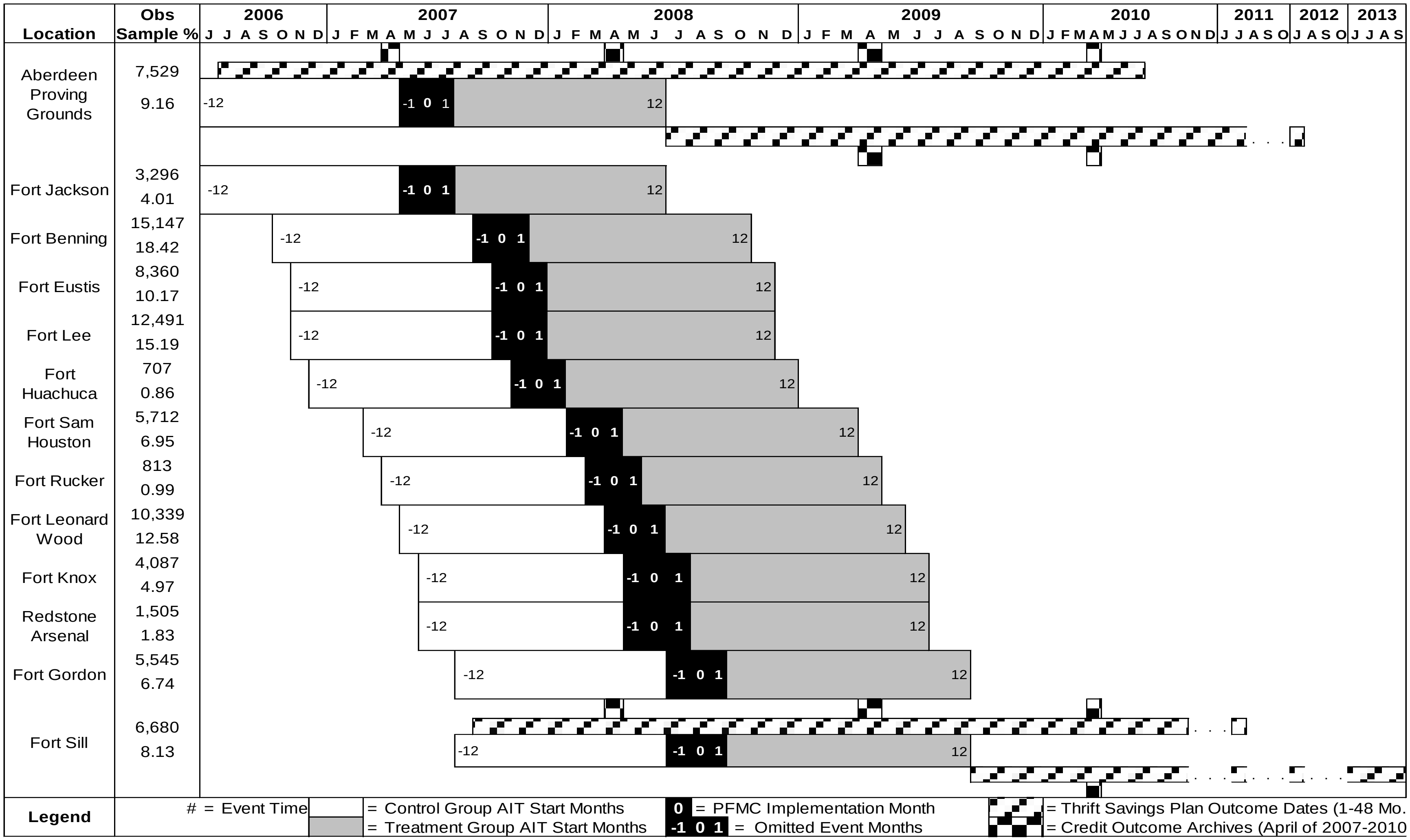

Figure II. PFMC Implementation Schedule and Outcome Data Horizons for Selected Locations

Author compiled using DOD and AER data. Percentages calculated for the administrative sample (n=82,211). Thrift Savings Plan (TSP) Outcome data (hashed bars) is available for 4 years for all 13 locations, but I only depict the outcome data horizons for the first (Aberdeen Proving Grounds) and the last (Fort Sill) locations for clarity. Credit outcome data is only available for April of 2007-2010.

Table I. 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, 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 II. Individual Characteristics by Sample and Treatment Status				
	Panel A Full Administrative Data Sample *N=82,211*		Panel B Matched Credit Subsample *N=33,178*	
	(1)	(2)	(3)	(4)
	No PFMC *N=40,843*	**PFMC** *N=41,368*	**No PFMC** *N=16,740*	**PFMC** *N=16,438*
Variable	**Mean** **(SD)**	**Mean** **(SD)**	**Mean** **(SD)**	**Mean** **(SD)**
Age, years	21.35	21.53	21.40	21.61
	(4.05)	(4.12)	(3.98)	(4.16)
Female, %	14.91	15.89	11.37	12.14
	(35.62)	(36.56)	(31.75)	(32.66)
Married, %	17.60	19.05	17.86	19.04
	(38.08)	(39.27)	(38.30)	(39.26)
Dependents	0.43	0.47	0.43	0.46
	(0.93)	(0.97)	(0.94)	(0.96)
Minority, %	30.84	33.63	29.17	31.76
	(46.18)	(47.25)	(45.46)	(46.56)
< HS education, %	28.77	24.46	29.79	24.92
	(45.27)	(42.98)	(45.74)	(43.26)
HS graduate, %	62.61	65.89	61.61	65.32
	(48.38)	(47.41)	(48.63)	(47.60)
Some college, %	6.20	6.72	6.20	6.94
	(24.11)	(25.04)	(24.12)	(25.42)
≥ College grad, %	2.42	2.93	2.40	2.82
	(15.36)	(16.87)	(15.29)	(16.56)
AFQT, percentile	55.89	56.14	56.35	57.25
	(19.45)	(19.78)	(19.26)	(19.02)
Joined in summer, %	38.10	35.99	37.10	33.48
	(48.56)	(48.00)	(48.31)	(47.19)
Enlistment term, yr	3.85	3.79	3.86	3.79
	(0.98)	(1.00)	(0.99)	(1.00)
AIT length, months	3.16	3.15	3.18	3.18
	(1.13)	(1.11)	(1.11)	(1.11)
Monthly pay, $	1,757	1,882	1,758	1,880
	(542.27)	(578.99)	(542.02)	(576.15)
Months deployed	1.18	1.51	1.19	1.56
	(2.26)	(2.66)	(2.27)	(2.69)
Prior Credit Score	-	-	557	554
			(105)	(108)
Missing Prior Credit Score, %	-	-	47.35	43.79
			(49.93)	(49.61)
Joint test of significance		**p=0.1171**		**p=0.4415**

Source: DOD Data. All demographic data is measured at AIT start with one exception: months deployed is the number of months deployed in the first year after AIT start. Married represents formal and common law marriages. 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). 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 III. Retirement Saving, Credit Market, and Labor Market Outcomes

	No PFMC				PFMC			
	N	**% at 0**	**Mean**	**(SD)**	**N**	**% at 0**	**Mean**	**(SD)**
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Retirement Savings Outcomes								
Avg Monthly TSP Savings in Yr 1, $	40,843	87.96	17.27	(57.46)	41,368	70.24	40.03	(79.86)
Avg Monthly TSP Savings in Yr 2, $	34,874	84.91	28.52	(83.68)	35,908	69.10	45.62	(91.15)
Avg Monthly TSP Savings in Yr 3, $	29,255	83.84	28.89	(81.98)	30,354	69.22	45.80	(92.77)
Avg Monthly TSP Savings in Yr 4, $	22,865	82.66	30.28	(85.00)	21,790	69.82	46.87	(97.89)
Prob (TSP Participation) in Yr 1, %	40,843	87.96	12.04	(32.54)	41,368	70.24	29.76	(45.72)
Prob (TSP Participation) in Yr 2, %	34,874	84.91	15.09	(35.79)	35,908	69.10	30.90	(46.21)
Prob (TSP Participation) in Yr 3, %	29,255	83.84	16.16	(36.81)	30,354	69.22	30.78	(46.16)
Prob (TSP Participation) in Yr 4, %	22,865	82.66	17.34	(37.86)	21,790	69.82	30.18	(45.90)
Panel B. Credit Market Outcomes								
Cumulative Credit Balance in Yr 1, $	16,740	15.24	6,668	(8,585)	16,438	16.70	6,326	(8,391)
Cumulative Credit Balance in Yr 2, $	12,328	10.22	8,882	(9,466)	11,907	12.25	7,863	(9,115)
Aggregate Monthly Payment in Yr 1, $	16,740	27.33	214.38	(261.70)	16,438	28.98	217.82	(266.95)
Aggregate Monthly Payment in Yr 2, $	12,328	23.46	282.43	(296.95)	11,907	24.73	273.76	(294.71)
Adverse Legal Action Index in Yr 1, #	16,740	95.34	0.16	(1.11)	16,438	95.28	0.24	(1.74)
Adverse Legal Action Index in Yr 2, #	12,328	95.48	0.24	(1.66)	11,907	96.09	0.28	(2.09)
Credit Score in Yr 1, #	15,130	0.00	581	(88.55)	14,713	0.00	584	(89.24)
Credit Score in Yr 2, #	11,603	0.00	587	(92.59)	11,063	0.00	587	(90.06)
Panel C. Labor Market Outcomes								
Prob (Adverse Separation in 1st Term), %	32,585	77.89	22.11	(41.50)	33,251	76.33	22.67	(41.87)
Prob (Promoted to Sgt in 1st Term), %	32,585	95.40	4.60	(20.95)	33,251	93.49	6.65	(24.92)
Prob (Reenlisted | Eligible), %	21,875	33.82	66.18	(47.31)	21,207	38.05	61.95	(48.55)

Source: DOD and Credit Bureau Data. Notes: The table reports the sample sizes, percentage of the sample with an outcome value equal to zero, means and standard deviations for each outcome in the corresponding row 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. 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 IV. 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, $	Y2=Avg Monthly TSP Contr Yr 2, $	Y3=Avg Monthly TSP Contr Yr 3, $	Y4=Avg Monthly TSP Contr Yr 4, $	Pr (Y1>0) %	Pr (Y2>0) %	Pr (Y3>0) %	Pr (Y4>0) %
PFMC Effect	19.93**	14.02**	9.745	7.17	15.04**	13.46**	11.56**	8.23*
Std Err	(8.06)	(5.98)	(6.19)	(6.58)	(5.31)	(4.66)	(4.56)	(4.02)
Control Mean	17.27	28.51	28.90	30.26	12.04	15.09	16.17	17.34
Observations	82,212	70,786	59,766	44,947	82,212	70,786	59,766	44,947
Adj. R-Squared	0.0971	0.0611	0.0612	0.0659	0.1037	0.0815	0.0742	0.0685
Panel B. Credit Outcomes in Years 1-2								
	Cumulative Credit Account Balance in Yr 1, $	Cumulative Credit Account Balance in Yr 2, $	Aggrgeate Monthly Payment in Yr 1, $	Aggrgeate Monthly Payment in Yr 2, $	Adverse Legal Action Index in Yr 1, #	Adverse Legal Action Index in Yr 2, #	Credit Score in Yr 1, #	Credit Score in Yr 2, #
PFMC Effect	-634.77**	-234.71	-37.17***	-1.02	-0.057*	-0.086	-0.20	-3.75
Std Err	(254.61)	(534.94)	(11.850)	(26.62)	(0.03)	(0.08)	(2.957)	(5.00)
Control Mean	6,668	8,882	214.38	282.43	0.16	0.24	581	587
Observations	33,178	24,235	33,178	24,235	33,178	24,235	29,843	22,666
Adj. R-Squared	0.3312	0.1785	0.2716	0.1278	0.4472	0.2656	0.3680	0.2863
Panel C. Labor Market Outcomes in First Term								
	Prob (Adverse Separation) in 1st Term, %	Prob (Promoted to Sergeant) in First Term, %	Prob (Reenlisted | Eligible), %					
PFMC Effect	0.82	0.09	-1.37					
Std Err	(0.94)	(0.66)	(1.37)					
Control Mean	22.10	4.60	66.18					
Observations	65,838	65,838	43,082					
Adj. R-Squared	0.0496	0.0954	0.0467					

Source: DOD and Credit Bureau data. The table reports OLS estimates of Equation 1. All regression specifications (Cols 1-8) 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, the number of months deployed in the year, and fixed effects for location and month. 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 outcomes are measured relative to the month an individual started AIT. Standard errors are clustered at the AIT location level (N=13). ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

Table V. OLS Estimates of Heterogeneous Treatment Effects in Year 1							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Full Sample (Equation 1)	Females	Minorities	AFQT Score > Median	Married	Age > Median	Prior Year Credit Activity
Panel A. Average Monthly TSP Contributions in Year 1							
PFMC Effect	19.93**	8.29*	17.14**	28.25**	15.83*	20.53**	-
	(8.06)	(4.29)	(7.47)	(9.83)	(8.19)	(8.87)	-
Control Mean	17.27	15.31	17.92	21.45	16.79	18.87	-
Effect Mag. (Coeff/Mean)	115%	54%	96%	132%	94%	109%	-
Observations	82,212	12,662	26,510	39,943	15,068	36,902	-
Clusters	13	11	13	13	13	13	-
Adj. R-Squared	0.0971	0.0506	0.0765	0.1018	0.0859	0.1005	-
Panel B. Aggregate Monthly Payment for Credit Balances in Year 1							
PFMC Effect	-37.17***	-59.92*	-48.13**	-49.54***	-69.24***	-37.55**	-53.44**
	(11.85)	(29.46)	(16.39)	(9.07)	(16.99)	(15.88)	(17.78)
Control Mean	214.38	208.30	226.35	215.11	324.97	251.01	265.27
Effect Mag. (Coeff/Mean)	-17%	-29%	-21%	-23%	-21%	-15%	-20%
Observations	33,178	3,900	10,104	16,544	6,119	15,333	18,054
Clusters	13	11	13	13	13	13	13
Adj. R-Squared	0.2716	0.2990	0.2734	0.3198	0.3313	0.3524	0.3186

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 treatrment 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, and fixed effects for location and month. 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 outcomes are measured relative to the month an individual started AIT. 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 A. Additional Results

ASSESSING FINANCIAL EDUCATION: PROMISING EVIDENCE FROM BOOT CAMP

William L. Skimmyhorn

Table A1. OLS Estimates of Sample Credit Matching and Activity

	(1)	(2)	(3)	(4)
Panel A. Credit Sample Matching and Activity				
	Pr (Matched Record) in Yr 1, %	Pr (Active Credit) in Yr 1, %	Pr (Matched Record) in Yr 2, %	Pr (Active Credit) in Yr 2, %
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
Panel B. Labor Market Sample Indicators				
	Pr (Term≤4 Years), %	Pr (Term≤4 Years & Offered Reenlistment), %		
PFMC Effect	-0.61	-1.54		
Std Err	(0.94)	(1.18)		
Location fixed effects	Y	Y		
Time fixed effects	Y	Y		
Control Mean	79.78	53.56		
Observations	82,212	82,212		
Clusters	13	13		
Adj R-Squared	0.7370	0.2534		

Source: DOD and Credit Bureau data. The table reports OLS estimates of Equation 3. Panel A outcomes are measured relative to the month an individual started AIT. Panel B outcomes are measured for an individual's first enlistment term. Standard errors are clustered at the AIT location level. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

Table A2. OLS Estimates of PFMC Effects on Retention, Years 1-4				
Outcome	Prob (Serving at End of Yr 1), %	Prob (Serving at End of Yr 2), %	Prob (Serving at End of Yr 3), %	Prob (Serving at End of Yr 4), %
	(1)	(2)	(3)	(4)
PFMC Effect	-1.09	0.74	0.14	0.37
(Std Err)	(1.36)	(1.12)	(1.01)	(1.32)
Location fixed effects	Y	Y	Y	Y
Time fixed effects	Y	Y	Y	Y
Control Mean	86.19	72.40	75.30	97.02
Observations	65,794	65,270	47,078	17,385
Clusters	13	13	13	13
Adj R-Squared	0.0812	0.0683	0.1825	0.0214

Source: DOD and Credit Bureau data. The table reports OLS estimates of Equation 1 with an outcome indicator for whether or not an individual is still serving at the end of each year. 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.

Table A3. OLS Estimates of the PFMC Effects with Alternate Standard Error Methods

	(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, $	Y2=Avg Monthly TSP Contr Yr 2, $	Y3=Avg Monthly TSP Contr Yr 3, $	Y4=Avg Monthly TSP Contr Yr 4, $	Pr (Y1>0) %	Pr (Y2>0) %	Pr (Y3>0) %	Pr (Y4>0) %
PFMC Effect	19.93	14.02	9.75	7.17	15.05	13.46	11.56	8.23
p-value for clustered (N=13) Std Errs	0.029	0.037	0.141	0.298	0.015	0.014	0.026	0.063
p-value from Wild Bootstrap Std Errs	0.009	0.018	0.213	0.587	0.018	0.020	0.042	0.102
Control Mean	17.27	28.51	28.90	30.26	12.04	15.09	16.17	17.34
Observations	82,212	70,786	59,766	44,947	82,212	70,786	59,766	44,947
Adj. R-Squared	0.0971	0.0611	0.0612	0.0659	0.1037	0.0815	0.0742	0.0685
Panel B. Credit Outcomes in Years 1-2								
	Cumulative Credit Account Balance in Yr 1, $	Cumulative Credit Account Balance in Yr 2, $	Aggrgeate Monthly Payment in Yr 1, $	Aggrgeate Monthly Payment in Yr 2, $	Adverse Legal Action Index in Yr 1, #	Adverse Legal Action Index in Yr 2, #	Credit Score in Yr 1, #	Credit Score in Yr 2, #
PFMC Effect	-634.77	-234.72	-37.17	-1.02	-0.06	-0.09	-0.20	-3.75
p-value for clustered (N=13) Std Errs	0.028	0.669	0.009	0.970	0.078	0.316	0.946	0.467
p-value from Wild Bootstrap Std Errs	0.035	0.817	0.013	0.962	0.149	0.412	0.937	0.545
Control Mean	6,668	8,882	214.38	282.43	0.16	0.24	581	587
Observations	33,178	24,235	33,178	24,235	33,178	24,235	29,843	22,666
Clusters	13	13	13	13	13	13	13	13
Adj. R-Squared	0.3312	0.1785	0.2716	0.1278	0.4472	0.2656	0.3680	0.2863
Panel C. Labor Market Outcomes in First Term								
	Prob (Adverse Separation) in First Term, %	Prob (Promoted to Sergeant) in First Term, %	Prob (Reenlisted | Eligible), %					
PFMC Effect	0.82	0.09	-1.37					
p-value for clustered (N=13) Std Errs	0.400	0.883	0.335					
p-value from Wild Bootstrap Std Errs	0.444	0.889	0.406					
Control Mean	22.10	4.60	66.18					
Observations	65,838	65,838	43,082					
Clusters	13	13	13					
Adj. R-Squared	0.0496	0.0954	0.0467					

Source: DOD and Credit Bureau data. The table reports OLS estimates of Equation 1. The table reports OLS estimates of Equation 1. All regression specifications (Cols 1-8) include the treatrment 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, and fixed effects for location and month. 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. The p-values are reported for two different methods of calculating heteroskedasticity robust standard errors. The first p-value reflects standard errors clustered at the AIT location level (N=13). The second p-value reflects standard errors computed using 10,000 iterations of the Wild Bootstrap method suggested by Cameron et. al. (2008) for small and/or unequally sized clusters.

Table A4. 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.

Table A5. 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 **	-1.93	-1.51	-653.13 **
	(253.68)	(1.13)	(0.85)	(280.58)
Control Mean	6,667.82	84.76	10.52	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 *	-4.59	-1.72	-74.63
	(54.57)	(2.68)	(1.05)	(99.69)
Control Mean	937.04	55.05	10.85	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	-2.02	-0.87	-386.42
	(248.85)	(1.55)	(1.14)	(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 *	-4.64 **	-2.20	-110.31
	(62.24)	(2.09)	(1.53)	(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	-1.55	0.58	-184.24
	(104.04)	(1.50)	(1.24)	(232.21)
Control Mean	1,796.32	48.48	9.18	3,705
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 ***	-5.77 **	-1.81	-31.58 **
	(11.78)	(2.08)	(1.07)	(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 *	-1.03 ***		-0.09
	(0.03)	(0.31)		(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		0.37	
	(2.95)		(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.

Table A6. PFMC Effects on Credit Outcomes in Year 2				
	(1)	(2)	(3)	(4)
Outcome Variable	Y = Outcome	Pr (Y>0)	Pr (Y>90th %ile)	Y | Y>0
Panel A: Cumulative Credit Balance			90th %ile = $ 22,195	
PFMC Effect	-277.84	5.90 *	0.63	-358.32
	(391.60)	(3.30)	(1.15)	(416.89)
Control Mean	8,852	89.98	10.85	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	2.17	1.90	205.20
	(88.74)	(2.72)	(1.27)	(139.24)
Control Mean	1,097	56.59	11.41	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	-1.25	0.72	-227.26
	(410.78)	(3.20)	(0.86)	(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	1.59	0.84	-181.08
	(72.23)	(1.90)	(1.06)	(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	-1.00	-0.19	-192.15
	(168.95)	(1.49)	(1.08)	(320.43)
Control Mean	2,527	56.34	9.93	4,470
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	0.16	0.72	-0.54
	(21.36)	(2.32)	(1.43)	(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	-0.38		-1.11
	(0.06)	(0.46)		(1.08)
Control Mean	0.22	4.25		5.24
Adj R2	0.2595	0.1178		0.3530
Observations	28,053	28,053		1,177
Panel H: Credit Score			90th %ile = 720	
PFMC Effect	-5.58		-0.96	
	(3.45)		(0.54)	
Control Mean	588		10.69	
Adj R2	0.2963		0.1994	
Observations	26,527		26,527	

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.

Table A7. Estimates of the PFMC Effects on Retirement Savings Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Full Sample (Tobit and Logit)								
Outcome	Y1=Avg Monthly TSP Contr Yr 1, $	Y2=Avg Monthly TSP Contr Yr 2, $	Y3=Avg Monthly TSP Contr Yr 3, $	Y4=Avg Monthly TSP Contr Yr 4, $	Pr (Y1>0) %	Pr (Y2>0) %	Pr (Y3>0) %	Pr (Y4>0) %
PFMC Effect	106.94***	95.26***	77.42**	55.90*	14.46***	13.53***	11.90***	8.50**
Std Err	(31.38)	(30.45)	(30.45)	(29.76)	(4.19)	(29.76)	(4.20)	(3.85)
Control Mean	17.27	28.51	28.90	30.26	12.04	15.09	16.17	17.34
Observations	82,212	70,786	59,766	44,947	82,212	70,786	59,766	44,947
Clusters	13	13	13	13	13	13	13	13
R2 Measure	0.0322	0.0204	0.0192	0.0191	0.1050	0.0779	0.0702	0.0650
Panel B. Sample Omitting Event Month Group = 2 (OLS)								
	Y1=Avg Monthly TSP Contr Yr 1, $	Y2=Avg Monthly TSP Contr Yr 2, $	Y3=Avg Monthly TSP Contr Yr 3, $	Y4=Avg Monthly TSP Contr Yr 4, $	Pr (Y1>0) %	Pr (Y2>0) %	Pr (Y3>0) %	Pr (Y4>0) %
PFMC Effect	18.03**	13.04**	8.042	5.71	13.81***	12.34***	10.21**	6.78**
Std Err	(6.52)	(5.19)	(4.81)	(5.44)	(4.31)	(3.80)	(3.59)	(3.03)
Control Mean	17.27	28.51	28.90	30.26	12.04	15.09	16.17	17.34
Observations	81,360	70,011	59,095	44,463	81,360	70,011	59,095	44,463
Adj. R-Squared	0.0956	0.0611	0.0608	0.0656	0.1020	0.0809	0.0737	0.0679

Source: DOD data. Panel A reports Tobit (Col.1-4) and Logit (Col. 5-8) estimates of Equation 1. Panel B reports OLS estimates for a sample that excludes individuals from event group +2, which may be an outlier (See Event Studies in Figure A1). All regression specifications (Cols 1-8) include the treatrment 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, the number of months deployed in the year, and fixed effects for location and month. Standard errors are clustered at the AIT location (base) level. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

Table A8. Robustness Checks for OLS Estimates of the PFMC Effects on Credit Market Outcomes in Years 1-2

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Active Records Only Sample (OLS)								
	Cumulative Credit Account Balance in Yr 1, $	Cumulative Credit Account Balance in Yr 2, $	Aggrgeate Monthly Payment in Yr 1, $	Aggrgeate Monthly Payment in Yr 2, $	Adverse Legal Action Index in Yr 1, #	Adverse Legal Action Index in Yr 2, #	Credit Score in Yr 1, #	Credit Score in Yr 2, #
PFMC Effect	-721.03**	-264.82	-42.10***	-2.21	-0.060	-0.097	-0.25	-3.66
Std Err	268.41	598.37	12.72	26.66	0.04	0.09	2.95	3.45
Control Mean	6,668	9,370	237.07	300.01	0.17	0.25	581	587
Observations	29,843	22,666	29,843	22,666	29,843	22,666	29,843	22,666
Clusters	13	13	13	13	13	13	13	13
Adj. R-Squared	0.3131	0.1657	0.2578	0.1171	0.4465	0.2644	0.3676	0.2679
Panel B. All Records (Tobit Specification)								
	Cumulative Credit Account Balance in Yr 1, $	Cumulative Credit Account Balance in Yr 2, $	Aggrgeate Monthly Payment in Yr 1, $	Aggrgeate Monthly Payment in Yr 2, $	Adverse Legal Action Index in Yr 1, #	Adverse Legal Action Index in Yr 2, #	Credit Score in Yr 1, #	Credit Score in Yr 2, #
PFMC Effect	-773.68**	-233.46	-53.24***	-5.49	-0.858***	-1.826***	-0.20	-3.75
Std Err	306.41	600.42	17.00	31.93	0.24	0.02	2.95	4.99
Control Mean	6,668	9,370	237.07	300.01	0.17	0.25	581	587
Observations	33,178	24,235	33,178	24,235	33,178	24,235	29,843	22,666
Clusters	13	13	13	13	13	13	13	13
Adj. R-Squared	0.0212	0.0105	0.0244	0.0109	0.1937	0.1210	0.0390	0.0286
Panel C. Pre-CARD Act Sample (OLS)								
	Cumulative Credit Account Balance in Yr 1, $	Cumulative Credit Account Balance in Yr 2, $	Aggrgeate Monthly Payment in Yr 1, $	Aggrgeate Monthly Payment in Yr 2, $	Adverse Legal Action Index in Yr 1, #	Adverse Legal Action Index in Yr 2, #	Credit Score in Yr 1, #	Credit Score in Yr 2, #
PFMC Effect	-525.46*	-289.86	-30.96*	-2.29	-0.057*	-0.089	-0.46	-4.27
Std Err	270.39	559.19	14.58	28.58	0.03	0.09	3.18	5.03
Control Mean	6,222	8,555	213.81	284.82	0.15	0.31	586	596
Observations	28,138	23,269	28,138	23,269	28,138	23,269	25,256	21,960
Clusters	13	13	13	13	13	13	13	13
Adj. R-Squared	0.3334	0.1744	0.2722	0.1241	0.4519	0.2652	0.3699	0.2925

Source: DOD and Credit Bureau data. Panel A reports OLS Estimates of Equation 1 but the sample is restricted to individuals with matched and active credit records. Panel B reports Tobit Estimates of Equation 1. Panel C reports OLS Estimates of Equation 1 but the sample is restricted to individuals with credit outcomes prior to 2009 (pre-CARD Act). All regression specifications (Cols 1-8) include the treatrment 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, the credit score, the credit outcome from the previous year, the number of months deployed in the year, and fixed effects for location and month. Individuals missing the credit outcome data are given a zero value and a missing indicator is used. Standard errors are clustered at the AIT location (base) level. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

Table A9. OLS Estimates of the PFMC Main Effects (With Time Trends by Location)								
	(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, $	Y2=Avg Monthly TSP Contr Yr 2, $	Y3=Avg Monthly TSP Contr Yr 3, $	Y4=Avg Monthly TSP Contr Yr 4, $	Pr (Y1>0) %	Pr (Y2>0) %	Pr (Y3>0) %	Pr (Y4>0) %
PFMC Effect	22.25***	16.61***	12.11**	10.5*	14.87***	13.72***	11.83***	8.99**
Std Err	(7.08)	(5.12)	(4.70)	(4.90)	(4.44)	(3.98)	(3.80)	(3.11)
Control Mean	17.27	28.51	28.90	30.26	12.04	15.09	16.17	17.34
Observations	82,212	70,786	59,766	44,947	82,212	70,786	59,766	44,947
Adj. R-Squared	0.1080	0.0673	0.0657	0.0699	0.1197	0.0930	0.0833	0.0762
Panel B. Credit Outcomes in Years 1-2								
	Cumulative Credit Account Balance in Yr 1, $	Cumulative Credit Account Balance in Yr 2, $	Aggrgeate Monthly Payment in Yr 1, $	Aggrgeate Monthly Payment in Yr 2, $	Adverse Legal Action Index in Yr 1, #	Adverse Legal Action Index in Yr 2, #	Credit Score in Yr 1, #	Credit Score in Yr 2, #
PFMC Effect	-838.36***	35.8072	-44.10***	22.81	-0.045	0.1470	-0.50	2.005
Std Err	(269.83)	(373.59)	(13.999)	(19.79)	(0.03)	(0.14)	(2.862)	(4.23)
Control Mean	6,668	8,882	214.38	282.43	0.16	0.24	581	587
Observations	33,178	24,235	33,178	24,235	33,178	24,235	29,843	22,666
Adj. R-Squared	0.3320	0.1787	0.2723	0.1292	0.4472	0.2656	0.3682	0.2865
Panel C. Labor Market Outcomes in First Term								
	Prob (Adverse Separation) in 1st Term, %	Prob (Promoted to Sergeant) in First Term, %	Prob (Reenlisted | Eligible), %					
PFMC Effect	0.94	-0.1	-0.15					
Std Err	(1.04)	(0.37)	(1.25)					
Control Mean	22.10	4.60	66.18					
Observations	65,838	65,838	43,082					
Adj. R-Squared	0.0506	0.0974	0.0473					

Source: DOD and Credit Bureau data. The table reports OLS estimates of Equation 1 but adds unique time trend indicators by location. All regression specifications (Cols 1-8) 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, the number of months deployed in the year, and fixed effects for location and month. 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. TSP outcomes are measured relative to the month an individual started AIT. Credit outcomes are measured relative to the month an individual finished AIT. Labor market outcomes are measured for an individuals' first term. Standard errors are clustered at the AIT location level (N=13). ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

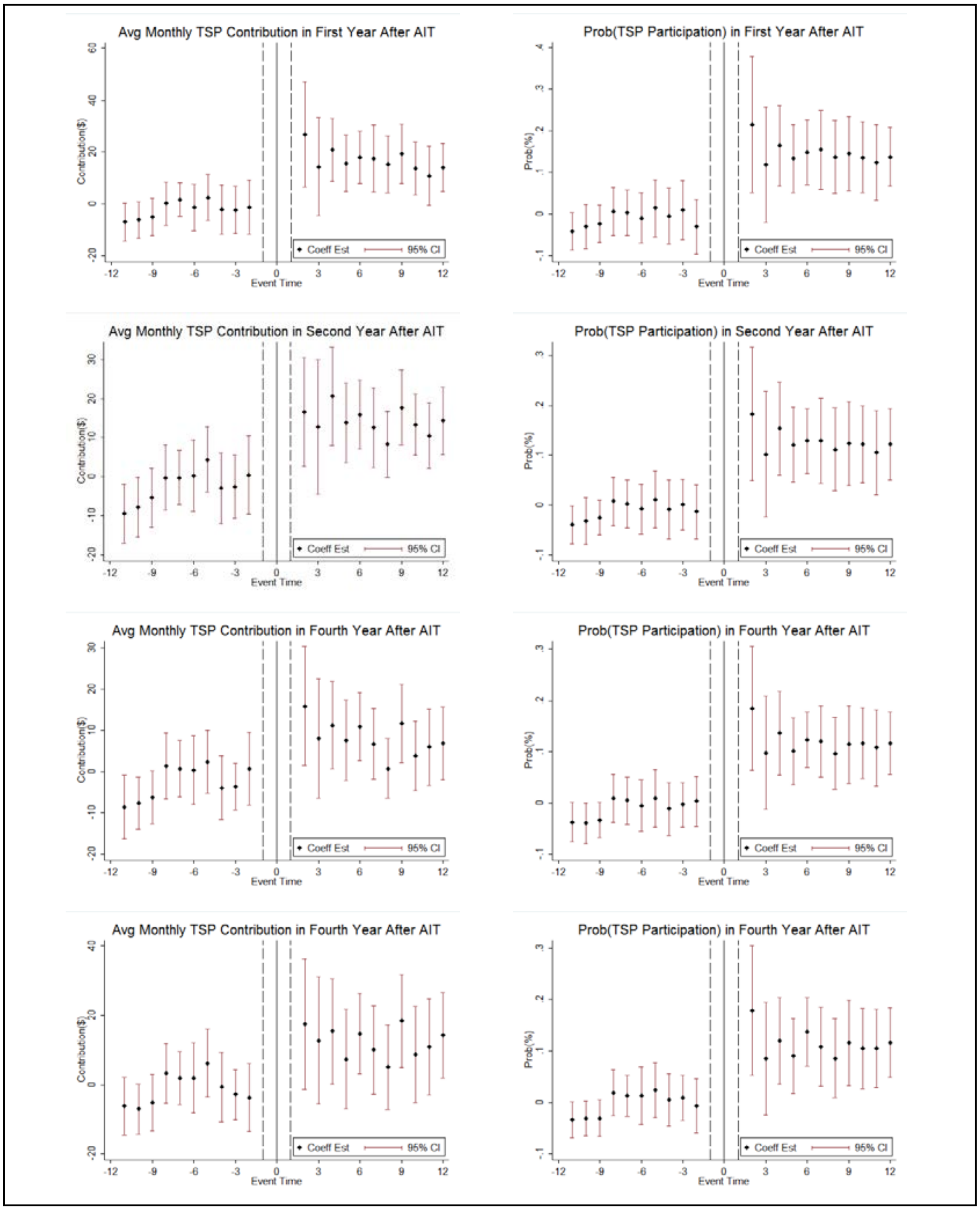

Figure A1. Event Studies for Retirement Savings (TSP) Outcomes

Source: DOD data. The events studies reflect alternate estimates of Equation 1, replacing the binary treatment indicator ($PFMC_i$) with indicators for each event time group (omitting -12). Event time equals an individual's AIT start month minus the month the PFMC began at their location. The y-values depict the OLS estimate for each event time indicator. The outcomes (e.g., Year 1) are measured relative to the month an individual started AIT.

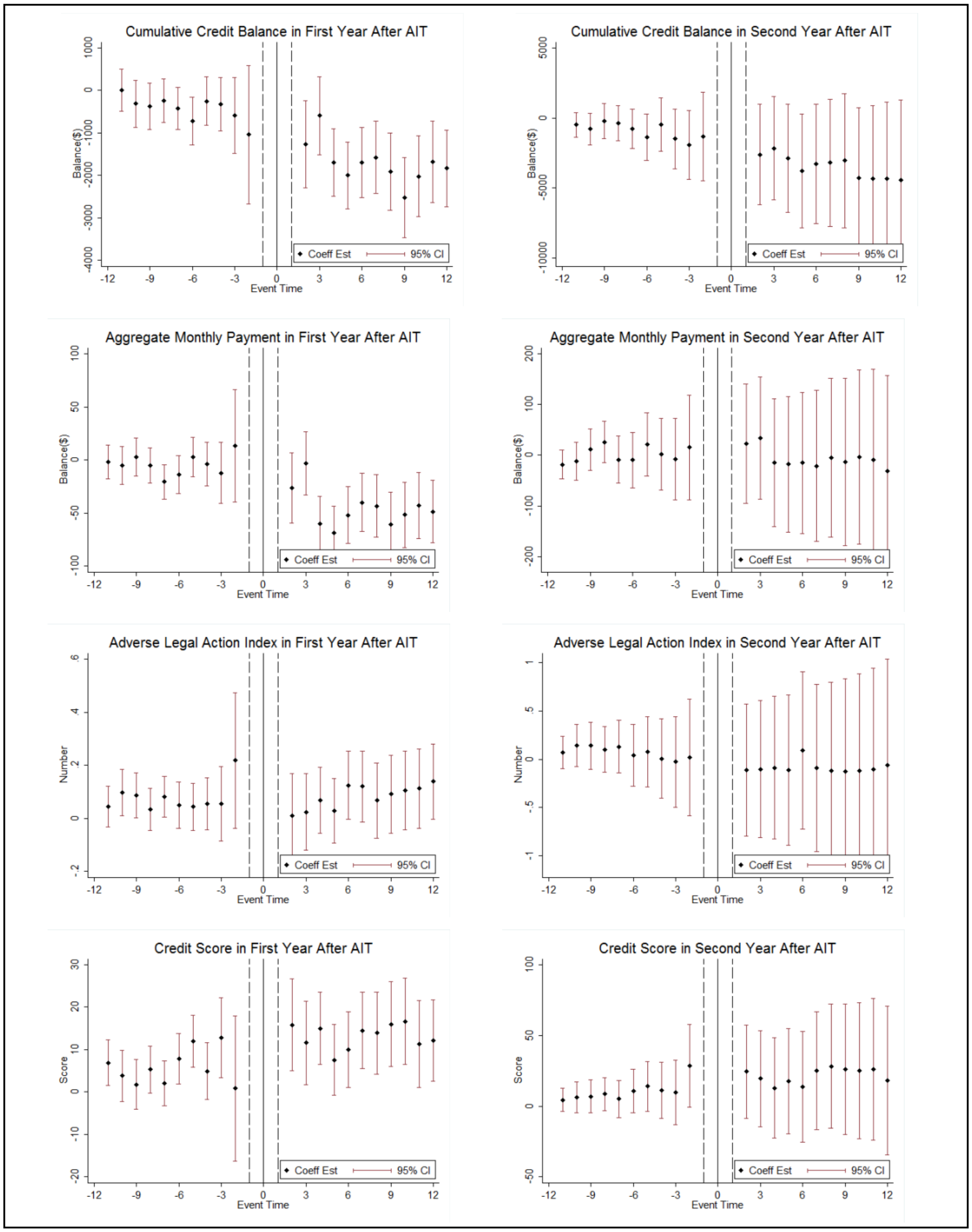

Figure A2. Event Studies for Credit Outcomes

Source: DOD and credit bureau data. The events studies reflect alternate estimates of Equation 1, replacing the binary treatment indicator ($PFMC_i$) with indicators for each event time group (omitting -12). Event time equals an individual's AIT start month minus the month the PFMC began at their location. The y-values depict the OLS estimate for each event time indicator. The outcomes (e.g., Year 1) are measured relative to the month an individual started AIT.

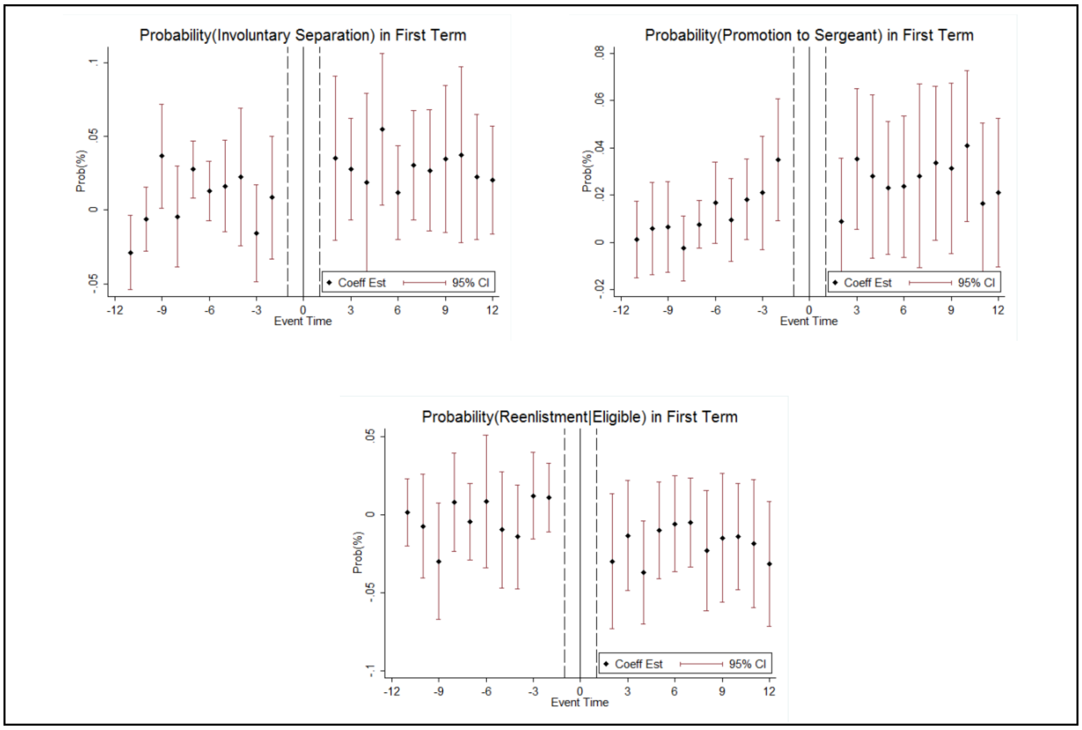

Figure A3. Event Studies for Labor Market Outcomes

Source: DOD data. The events studies reflect alternate estimates of Equation 1, replacing the binary treatment indicator ($PFMC_i$) with indicators for each event time group (omitting -12). Event time equals an individual's AIT start month minus the month the PFMC began at their location. The y-values depict the OLS estimate for each event time indicator. The outcomes (e.g., Year 1) are measured relative to the month an individual started AIT.