Essays in Behavioral Household Finance

Dissertation Advisor: Professor Brigitte C. Madrian William Lee Skimmyhorn

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Abstract

This dissertation investigates some of the factors affecting modern household finance decisions in the United States using natural experimental variation and administrative data.

In Chapter 1 I estimate the effects of financial education on retirement savings decisions. Between 2007 and 2008 the U.S. Army implemented a mandatory 8 hour Personal Financial Management Course (PFMC) for new soldiers. Staggered implementation across locations and time provides quasi-experimental variation in whether an individual received the training. I find that the course has large and lasting effects on individual retirement savings in the Thrift Savings Plan, a tax-deferred account similar to a 401(k). The course doubles savings, has significant effects throughout the distribution of savings and the effects persist out to two years. The mechanism for the effects is likely a combination of both human capital and behavioral assistance.

In Chapter 2 I estimate the effects of financial education on a variety of other economic behaviors. I rely on the same natural experiment as in Chapter 1 but I use individually matched credit data to estimate the effects of financial education on credit scores, credit balances for several types of accounts, monthly payments and adverse legal actions. In some areas I find that the PFMC has beneficial effects, reducing cumulative account balances (especially for automobile accounts) and aggregate monthly payments. In other areas, including credit scores,

the probability of being active in the credit market and the number of adverse legal actions, the PFMC has no statistically significant effects on financial behavior.

In Chapter 3 I estimate the effects of stress on financial decision-making. I use the natural variation in the casualty rates faced by individuals deploying overseas an exogenous source of stress and I measure the effects of this stress on individuals' participation in the Savings Deposit Program (SDP), a risk-free 10% annual percentage rate savings account. I find a modest and statistically significant negative relationship between the stress of casualties and SDP participation on the order of 5%. Some failures of the randomization test and the confounding effects of overall activity levels and rural locations cannot be eliminated as a source of the observed savings differences and as a result, these results should be considered suggestive evidence of the adverse effects of stress on financial decision-making.

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Disclaimer

The opinions expressed herein reflect the personal views of the author and not those of the United States Army or the United States Department of Defense.

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CHAPTER 1

ESTIMATING THE EFFECTS OF FINANCIAL EDUCATION ON RETIREMENT SAVINGS: EVIDENCE FROM THE U.S. ARMY'S PERSONAL FINANCIAL MANAGEMENT COURSE

Estimating the Effects of Financial Education on Retirement Savings: Evidence from the U.S. Army's Personal Financial Management Course

May 2012

ABSTRACT

This paper exploits a natural experiment that occurred in the U.S. Army to estimate the effects of financial education on retirement savings. Between 2007 and 2008 the Army implemented a mandatory 8 hour Personal Financial Management Course for new soldiers. The staggered implementation across locations and time provides quasi-experimental variation in whether an individual received the training. Using event studies and regression discontinuity techniques, I find that the course had large and lasting effects on individual retirement savings in the Thrift Savings Plan, a tax-deferred account available to uniformed service members. The course doubles retirement savings, has positive and significant effects on saving levels throughout the distribution and has persistent effects through at least twenty four months. The mechanism for the effects is likely a combination of both human capital and behavioral assistance and these effects cannot be separated in this data. Nonetheless, this research marks one of the first experimental findings of large and lasting effects from financial education. These findings provide initial evidence to economists and policy makers on the causal effects of such education and motivate additional research on the improvement of the treatment effects and the identification of the mechanisms for behavioral change.

William Skimmyhorn

Harvard Kennedy School

79 JFK Street

Cambridge, MA 02138

bill_skimmyhorn@hksphd.harvard.edu

1.1. INTRODUCTION

Financial literacy is a popular topic among policy-makers, the media and even academics today. The most common discussion in the U.S. media typically involves summarizing academic research on the state of individual financial literacy, whether among high school students (New York Times, 2010), senior citizens (Reuters, 2012) or even the middle aged (New York Times, 2012) as inadequate, then calling for more financial education and occasionally, more consumer protection. The presence of economic hardship in the U.S. since the 2007 economic downturn has only increased the calls for more education and assistance. In response, the Federal government has become more active in this area beginning with President Bush's Financial Literacy Advisory Council in 2008 and President Obama's 2009 financial literacy campaign administered jointly by the Treasury and Education Departments. Yet despite the popularity of the topic and the infrequently contested conclusion that more education is the answer, there exists little robust scientific evidence that financial literacy education improves individuals' economic decisions. Recently, a small but grounded opposition, typified by Willis (2011), has emerged and questioned the efficacy of and advisability of additional formal education. The modern debate over financial education, especially in academic circles, has thus become less about the existence of financial illiteracy and more about whether financial education, especially publicly funded education, offers a cost-effective remedy.

This paper exploits a natural experiment that occurred in the U.S. Army to estimate the effects of financial education on retirement savings. Between 2007 and 2008 the Army implemented a mandatory 8 hour Personal Financial Management Course for new soldiers. The staggered implementation across locations and time provides quasi-experimental variation in whether an individual received the training. Using event studies and regression discontinuity techniques, I find that the course had large and lasting effects on individual retirement savings in the Thrift Savings Plan, a tax-deferred account available to uniformed service members. The course doubles retirement savings, has positive and significant effects on saving levels throughout the distribution and has persistent effects through at least twenty four months. The mechanism for the effects is likely a combination of both human capital and behavioral assistance and these effects cannot be separated in this data. Nonetheless, this research marks one of the first experimental findings of large and lasting effects from financial education. These findings provide initial evidence to economists and policy makers on the causal effects of such education and motivate additional research on the improvement of the treatment effects and the identification of the mechanisms for behavioral change.

For the purposes of this paper, financial management, financial literacy and financial education will be used interchangeably. In addition, the course that includes instruction on these topics will be referred to as both training (since it involves some repetitive clearly identifiable tasks such as balancing a budget) as well as education (since it involves designing solutions and approaches to new situations). In this regard, financial literacy is an area not easily categorized by the training/education taxonomy.

The paper proceeds as follows: Section 1.2 reviews the existing literature and summarizes the contributions of this work. Section 1.3 presents a theoretical model related to financial literacy and education. Section 1.4 describes the financial management course and program implementation. Section 1.5 summarizes the variables and data used in the empirical analysis. Section 1.6 presents the empirical analysis and discusses the results. Section 1.7 conducts robustness checks. Section 1.8 discusses external validity. Section 1.9 summarizes the findings, presents brief policy recommendations and concludes.

1.2. LITERATURE REVIEW & CONTRIBUTIONS

There exists a large and interesting body of research on financial literacy, but unfortunately this literature is largely non-experimental. As a result, there is little robust evidence of a causal relationship between financial education and financial knowledge or financial behavior. This section reviews the relevant existing literature and identifies the contributions of the current research. The extant literature can be broadly summarized by the following four statements: financial illiteracy is widespread in the U.S.; financial literacy affects financial behavior; the effects of financial literacy training on financial behavior are unknown; and there remains disagreement about the advisability of additional financial education efforts. The current research contributes to our understanding of the final two points through the use of experimental variation and observation of behavioral outcomes.

The first important theme in the financial education literature is that there is widespread financial illiteracy in the U.S. For a detailed review of this well-accepted and documented fact, see Lusardi and Mitchell (2007). As examples of this illiteracy, the Jump$tart Coalition for Personal Financial Literacy (2006) surveys high school and college students and finds poor performance (for example, 66% of 12th grade students failed the 2004 test) with little variation in students' scores since then; Mandell (2008) documents annually high school students' poor performance on a financial literacy quiz over several years in the past decade; Lusardi and Mitchell (2006) document similar financial illiteracy among the elderly using the 2004 Household & Retirement Survey; and a 2005 national survey by the National Council on Economic Education revealed an overall score of C for adults, F for high school students and additional gaps for women and minorities.[1] In fact the existence of this illiteracy in the U.S. is accepted by those supporting more education (e.g., Lusardi and Mitchell 2007, 2008) and even those opposing it (e.g., Willis 2008, 2009), who argue among other things, that the levels of illiteracy may be too large for publicly funded programs to address.

The second theme of the financial literacy research is that financial illiteracy correlates with financial behavior. Again, see Lusardi and Mitchell (2007) for a review. Hilgert, Hogarth and Beverly (2003) find that financial knowledge correlates with financial behavior in four areas: cash flow management, credit management, saving and investment. As with the findings of widespread illiteracy described above, the link between illiteracy and poor decision-making is generally accepted. However, there remains little empirical evidence that decomposes poor financial behavior into elements of illiteracy related to knowledge, those related to other behavioral issues like self-control and patience and those related to emotions (Xiao et. al 2011 and Hira 2009). In this sense, the imprecision of the definition of financial literacy may complicate evaluations of education and other policy measures as different programs may affect only some elements or differentially affect the elements of literacy. The link between literacy and behavior is thus correlational and multi-dimensional. To the extent that literacy is

[1] Similar findings hold elsewhere in developed countries. . The 2005 report by the Organization for Economic Cooperation and Development (OECD) entitled “Improving Financial Literacy” surveyed national programs worldwide and found that many countries have not undertaken nationally representative surveys. Among those that have, many consumers lack adequate financial backgrounds and overestimate their financial knowledge.

multi-dimensional, programs desiring immediate impact must address all elements, while those affecting only information related deficiencies might have limited effects or delayed effects, if age and experience eventually curb other behavioral tendencies. Conversely, programs might require complimentary policy interventions such as default options or regulatory approaches.

The next two themes of the financial education research both involve substantial disagreement and reveal unresolved issues. The third finding of the literature is that despite a great deal of research into the efficacy of financial education, the causal effects of education on financial behavior are still unknown. There are at least five reasons that existing research has failed to establish convincing causal estimates for the effects of financial education: the use of self-reported data; the use of knowledge measures as opposed to behavioral outcomes; the potential for improved behavior in one area to be offset by worsened behavior in another area; and most importantly, the lack of experimental design and evaluations. First, many studies have relied principally on self-reports for their evaluation of the effects of education (e.g., Lusardi 2004, Lusardi and Mitchell 2006, 2007, Bell, Gorin & Hogarth 2008, 2009, and Bernheim and Maki 2003). But such self-reports are problematic for at least two reasons: individuals overestimate their knowledge (Agnew and Szykman, 2005) and self-reported measures have proven unreliable measures of financial behavior when compared to administrative data (Collins et. al. 2009).

The second challenge to causal inference is that even increases in financial knowledge do not necessarily translate into improved financial behavior. This follows directly from the correlational nature of the research linking low levels of knowledge and poor financial behavior;

low levels of knowledge might correlate with both poor cognitive and non-cognitive skills and so improving knowledge may not improve behavior. In a stark demonstration of this fact, Madrian and Shea (2001) find that while 100% of work-related retirement seminar participants report that they will save more after receiving information and education, only 14% actually do. Similarly, Choi et al (2011) find that educating individuals about foregone 401(k) matches for vested, penalty-free withdrawal-eligible workers over the age of 59.5 increases contributions by a statistically insignificant amount; in this case, individuals are literally leaving about $507 on average on the sidewalk. As a result, even programs that demonstrate advances in knowledge levels through tests of literacy or announced intentions cannot be assumed to improve actual financial behavior (Coussens 2006).[2] Equally concerning are findings that financial education might improve behavior without affecting knowledge levels as measured on financial literacy exams (Mandell 2009). Thus while financial knowledge is important and may be a goal of some education, these first two facts demonstrate the importance of using behavioral outcomes as a better measure of the effectiveness of financial education.

The third challenge to causal inference is that findings linking financial education with a particular behavior may hide transfers within individual or family budgets and suggest positive behavioral impact when no net improvement in behavior occurred. For example, Bernheim and Garrett (2003) find that financial education in the workplace correlates with 401(k) accumulation but not overall wealth accumulation. One explanation is that employer seminars

[2] In related work in a different setting, Carpena et al (2011) find that while financial literacy in India does not increase some elements of knowledge (numeracy) it does improve awareness of basic financial choices and attitudes toward financial decisions. They formulate financial literacy as multi-dimensional and argue that financial education should aim to improve awareness and attitudes and that more technical skills and knowledge may follow.

simply induce individuals to transfer savings into their 401(k) from other accounts, perhaps their IRA or another savings account. Without more knowledge about the fee structures, tax implications, rates of return and other instrument features, it is unclear if such transfers are net beneficial for individuals. In this regard, data on multiple portions of an individual or family's balance sheet can improve welfare comparisons.

The fourth challenge is the lack of a consensus on the effects of financial literacy even within many non-experimental studies. Both the Jump$tart (2006) and Mandell (2008) results suggest that high school students' financial knowledge levels do not improve even after completing financial education classes. Madrian and Shea (2001) and Choi et al (2011) similarly find little improvement in behavior even among those inclined to enroll in classes. Thus even in self-selected groups financial education may be ineffective. But, there are a number of positive findings in this vein. Bell, Gorin & Hogarth (2008) provide a detailed list of the existing impact evaluations, which are generally positive for financial education.[3] Additional non-experimental research suggesting a positive relationship between financial education and financial behavior exists for credit counseling (Staten 2006), retirement seminars (Lusardi 2004, Bernheim and Garrett 2003), optional high school programs (Boyce & Danes 2004), widespread financial literacy education (Lusardi and Mitchell 2007), state laws mandating financial literacy courses or curriculum (Bernheim, Garrett and Maki 2001) and in the military (Bell, Gorin and Hogarth 2008, 2009[4]). Taken together then, there remains substantial disagreement over the efficacy of

[3] See Appendix A in their work.

[4] The findings of Bell, Gorin & Hogarth are directly related to the work here. They evaluated the effect of the Army's pilot Personal Financial Management Course in 2003 with follow up surveys in 2008 and 2009. They found that financial education positively affected self-reports of a variety of financial behaviors including budgeting,

financial education. And while the most recent reviews and meta analyses of the non-experimental evidence (Collins et al 2009, Gale and Levine 2011) suggest that financial literacy can improve financial behavior, these reviews do not appear to fully discount non-experimental research and its limitations for causal inference.

The fifth and final challenge to any causal claims related to financial education is the general lack of experimental research and the potential for endogenous selection to explain the findings in nearly all of the existing research. Typical selection concerns in this area are that individuals that attend retirement seminars or enroll in economics or personal finance courses differ from those who do not along many dimensions and thus differences in outcomes cannot be attributed to the "treatment effect" of the education / intervention / counseling. Meier and Sprenger (2007) document one example of this problem and find that future oriented individuals are most likely to attend financial education workshops, upward biasing typically measured treatment effects. In fact, nearly all of the previously mentioned work uses non-experimental variation to identify the effects of financial literacy, either by relying on education among self-selected individuals (e.g., Lusardi 2004, Lusardi and Mitchell 2006, 2007), failing to account for differences in the financial conditions faced by control and treatment groups over time (e.g., Bernheim, Garrett and Maki 2001) [5] or the use of non-experimental

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savings, credit card balance payment and bill payment. However, as the authors acknowledge, their research faced several limitations including the lack of an experimental control group (they compared new soldiers in training to other more experienced soldiers on Fort Bliss) and reliance on self-reports of behavior rather than actual behaviors. In addition, their research employed a survey with a very low response rate. The response rate for the first follow-up survey was 4.9% (199/4,061) and these 199 individuals comprised the sample for their results. As the current research hopes to show, military administrative data and the subsequent nationwide implementation of this program makes experimental use of this program feasible.

[5] See Cole and Shastry (2010) for a more robust analysis of the same state laws as Bernheim, Garrett and Maki 2001 that includes additional controls for state time trends. After including these controls, they find no effects of

control/comparison groups (e.g., Bell, Gorin and Hogarth 2008, 2009)[6]. For a host of reasons these studies should be viewed cautiously and causal conclusions seem inappropriate. There are a few studies in the research on financial literacy that employ experimental procedures and random variation, but none provide sufficient evidence for strong conclusions on the causal effects of financial literacy on financial decisions or large scale policy decisions. Duflo & Saez (2003, 2004) use a randomized intervention to measure the effects of information and social interactions on a job benefits fair attendance and subsequent tax deferred account (TDA) savings among employees. Their research provides important evidence on the role of information and social networks in financial savings behavior in a work context, but it does explicitly analyze the effectiveness of the benefits seminar/financial education. Gartner and Todd (2005) evaluate a randomized credit education plan for first year college students but find no statistically significant differences between the control and treatment groups. Servon and Kaestner (2008) used random variation in a financial literacy training and technology assistance program and found virtually no differences between the control and treatment groups, though

state mandates for financial literacy courses on asset accumulation, suggesting that states implemented mandatory education during times of high growth. They also find that laws requiring more math courses (not financial education courses) improve financial behavior for women but not men.

[6] The findings of Bell, Gorin & Hogarth are directly related to the work here. They evaluated the effect of the Army's pilot Personal Financial Management Course in 2003 with follow up surveys in 2008 and 2009. They found that financial education positively affected self-reports of a variety of financial behaviors including budgeting, savings, credit card balance payment and bill payment. However, as the authors acknowledge, their research faced several limitations including the lack of an experimental control group (they compared new soldiers in training to other more experienced soldiers on Fort Bliss) and reliance on self-reports of behavior rather than actual behaviors. In addition, their research employed a survey with a very low response rate. The response rate for the first follow-up survey was 4.9% (199/4,061) and these 199 individuals comprised the sample for their results. As the current research hopes to show, military administrative data and the subsequent nationwide implementation of this program makes experimental use of this program feasible.

they suspect that the program was implemented imperfectly.[7] Carlin and Robinson (2011) evaluate financial education among teenagers at a financial education theme park and find mixed effects of financial education, but their treatment assignment was not randomized Finally, in a small randomized field experiment (n=144), Collins (2010) evaluates a financial education program for low and moderate income families and finds improvements in self-reported knowledge and behaviors, increased savings and small improvements in credit scores twelve months later.

The fourth and final theme of the financial literacy research is the remaining disagreement over whether additional education is the most appropriate policy choice. This disagreement flows naturally from the previous debate over the causal estimates related to financial education. As expected, those who believe that education works favor more education (Lusardi and Mitchell 2007, Hogarth 2006, Martin 2007). Others, optimistic about the promise of financial education despite what they view as little empirical evidence of positive effects, support more targeted and timely education with more emphasis on experimental design and evaluation (Hathaway and Khatiwada 2008, Collins and O'Rourke 2010). Finally, some who do not believe the research demonstrates positive effects support other policy options (Willis 2008, 2009, 2011).

Summary of Contributions

As this review highlights, there is no definitive experimental research on the causal effects of financial education on important financial behaviors. In this regard the literature is incomplete

[7] They also acknowledge that their research also suffers from imperfect randomization and non-random attrition.

and inadequately supports policy development. The current research aims to fill this void and assess the causal effects of financial education using quasi-experimental methods. The contributions and benefits of this research are five-fold. First, the research employs a natural experiment involving implementation of a mandatory Personal Financial Management Course. Variation in course implementation across time and location provides for random assignment of the training, conditional on an individual's job and time of entry into the military. Second, the research uses a robust set of behavioral outcomes, thereby avoiding concerns with surveys, declarations of intentions and the link between test knowledge and behavior. These outcomes are described in more detail in Section 1.5 but include important retirement savings decisions. Third, these outcomes, coupled with those in Chapter 2, reflect financial behavior in a variety of domains and on different portions of individual and family balance sheets, permitting tests of balance shifting within household budgets. Fourth, the research is able to assess the effects of financial education on a large population of independent interest and one whose demographic diversity and education levels are representative of a population of interest for financial literacy programs. Finally, the use of military administrative data affords the use of a rich set of control variables, informed by the existing research on the roles of age, experience, gender, race, socioeconomic status, family characteristics, education, ability, income and other individual and household characteristics. Such data minimizes the risks from omitted variable bias and also permits the testing of heterogeneous treatment effects.

1.3. PERSONAL FINANCIAL MANAGEMENT COURSE PROGRAM IMPLEMENTATION

The U.S. Army initiated its first personal financial management course (PFMC) at Fort Bliss (near El Paso, Texas) in January 2003. The program was developed and administered by the non-profit organization Army Emergency Relief (AER)[8] and executed through a contract with San Diego City College (SDCC).[9] In 2006, after approximately 4 years of field testing and course refinement the Army contracted with SDCC to develop and implement a personal financial management course as part of Advanced Individual Training (AIT) at 12 additional locations. Enlisted soldiers attend AIT immediately following basic training and they learn the skills associated with their specific job (e.g., infantryman, vehicle mechanic, cook, radio operator, etc…) during this course.[10] Courses range in duration from 1-12 months and are typically only offered at one location. In Figure 1.1, I present a map detailing the base locations, course implementation dates and percentages of Army enlistees trained at each location in my sample. The training implementation dates were selected by Army Emergency Relief Headquarters based on discussions with AIT Commanders at each location. As such, these decisions were made without notifying or soliciting information from individual soldiers or the U.S. Army's Recruiting Command. This process created implementation dates orthogonal to any individual's enlistment decision or timing. Conditional on an individual's job (which determines their AIT location) and their entry month, the assignment of financial literacy training is as good as

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[8] For more information on Army Emergency Relief and their mission to assist soldiers and their dependents, see www.aerhq.org.

[9] SDCC was awarded a sole source contract to deliver training at the pilot location (Fort Bliss) from 2002-2006.

[10] 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.

randomly assigned. I discuss identification further in Section VI but as this section illustrates, the program appears to be a valid natural experiment.

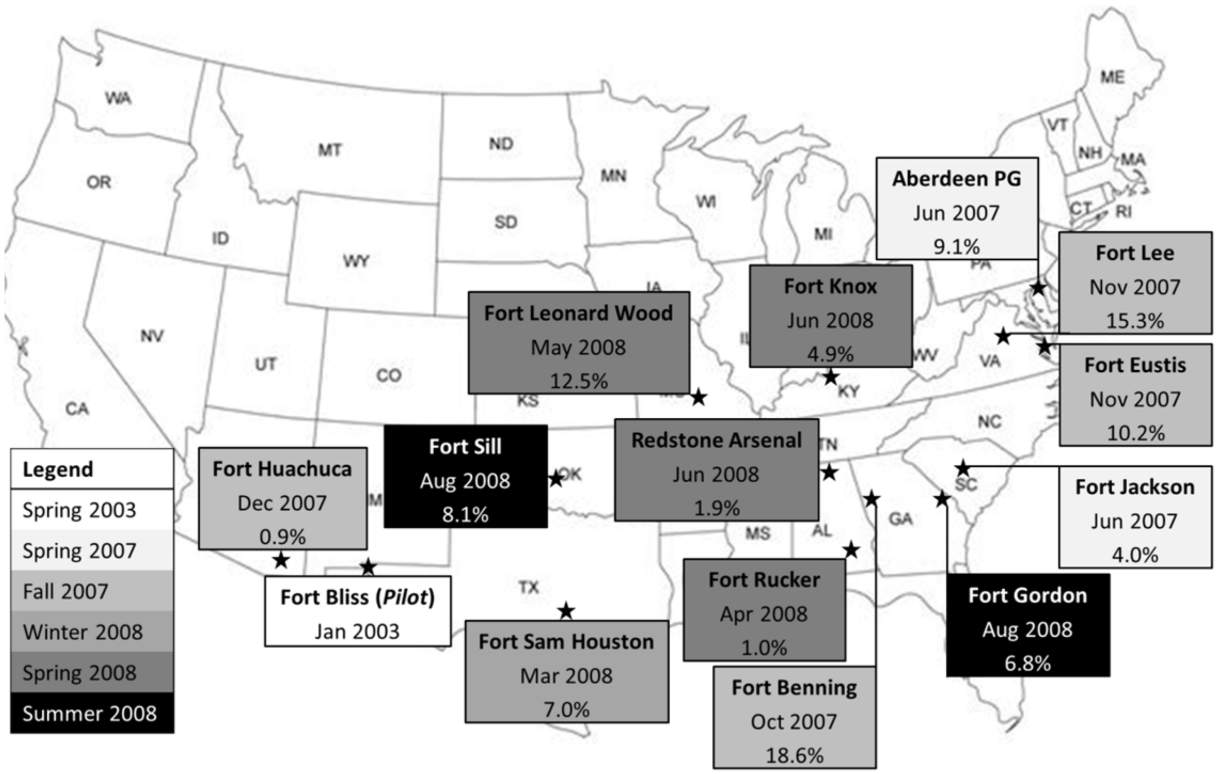

Figure 1.1. U.S. Army Advanced Individual Training locations, Personal Financial Management Course implementation dates and approximate training percentages.

Author complied data using Department of Defense and Army Emergency Relief data. Percentages are calculated for the periods 2006-2009 for individuals who started AIT within 12 months of program implementation at each location excluding those who started in AIT the month before, of and after program implementation, those for whom the treatment category is not determinable based on AIT start dates. The resulting sample size is n=85,879. See Appendix 1 for more information on the sample construction.

The financial management course is ambitious in its scope given its limited duration. The 8 hour course covers a number of important financial subjects with a focus on financial issues and decisions that young soldiers face. In Figure 1.2, I present the course topics and the time devoted to each topic during the period under study.

Whether an 8 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. Topics such as compound interest, the time value of money and portfolio diversification can only be covered briefly given the time limitations above. Similarly, teaching soldiers about how to make better choices in important decisions like buying a car, purchasing insurance and managing a credit card is also a difficult task in such a short time. Even the most straightforward tasks such as teaching a soldier how to read their pay statement and ensure that they are receiving all of their entitlements is difficult given the myriad of military pay processes, benefits and programs.

Lesson	Subject	Topics Covered	Duration (Hours)
1	Financial Ethics	Legal, Moral & Ethical aspects of personal financial management	0.75
2	Leave & 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	1.0
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	1.0
5	Consumer Awareness	Psychology of Advertising, Types of deception, Identity theft recognition and correction, Description of common scams	1.0
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, Investment vehicles	2.0
		Total	**8.0**

Figure 1.2. Personal Financial Management Course Summary

Author compiled data based on discussions with AER Headquarters Staff, the SDCC Staff, from the Personal Financial Management Course classroom slides provided by AER and from the contracting agency's website.[11]

[11] SDCC Program website accessed August 3, 2011 at: http://www.mysdcc.sdccd.edu/Locations/Army_PFM.htm. Since 2011 the program includes an additional 0.4 hour lecture on the mission and operations of AER. These topics were covered informally prior to 2011.

On the other hand, training time is often the commodity in shortest supply for military schools and more time for financial topics may not be justified. Additional time might be wasted if diminishing returns take hold and if soldiers become bored with too much information on any one subject or the course overall. Schreiner, Clancy & Sheradden (2002) found that an education program on individual development accounts increased savings for low-income households, but the effects trailed off after 8-10 hours. The purpose of this discussion is not to settle the debate over the optimal length of a financial education course but to highlight that a course of relatively short duration may have limited effects on behaviors involving complex combinations of analytic skills, life experience and self-control.[12]

1.4. SUMMARY OF VARIABLES AND DATA SOURCES

Demographic and Control Variable Data

The data used in this analysis comes from Department of Defense (DOD) administrative records and 2000 U.S. Census bureau data. This section briefly outlines the data sources and variables that will be used. The DOD data covers all U.S. Army enlisted members entering service from May 2006 through June 2009.[13] The military administrative data merges information from personnel, operational and financial databases to generate a rich set of individual demographic data (age, gender, race, military job, education, Armed Forces Qualification Test (AFQT) percentiles, zip code at entry, etc...), visibility of individual movements

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[12] I am currently working to gather and tabulate the costs of the program. While this research finds large benefits in terms of retirement savings outcomes, these benefits need to be balanced against the costs of the program,

[13] The data was obtained through the cooperation of the Office of Economic & Manpower Analysis (OEMA) at the United States Military Academy, West Point, NY. All personally identifying information has been removed from each observation so as to protect the anonymity of Army members.

(assignment locations, deployment dates and durations) and financial conditions (income and pay deductions).[14] To control for socioeconomic status (SES), I use administrative data on an individual's home of record and matched median household income data from the 2000 U.S. Census Summary File 3.

The Treatment Variable: Personal Financial Management Course (PFMC)

This research investigates the effects of participation in the Army's mandatory Personal Financial Management Course (PFMC) course as part of AIT. I use an indicator variable ($PFMC_i = 1$) to denote individuals who completed the training. 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. I further confine my sample to individuals attending AIT at a given location within 12 months of program implementation at that location. To avoid contamination I omit individuals starting AIT in the month preceding, month of and month following program implementation and individuals whose AIT start date and AIT duration produce overlap with the program implementation at the location.[15] After treatment imputation and sample construction I have a sample of n=85,879 individuals for my year 1 analyses. For analyses in year 2 I have a sample of n=64,017 due to censoring of some individuals in the administrative data. In subsequent analyses I interact treatment with other individual characteristics to explore heterogeneous treatment effects (e.g., AFQT levels, experience, enlistment durations, education levels, SES and marital status).

[14] I employ indicator variables for individuals missing any demographic data and assigned values of zero for the missing data.
[15] For more information on sample selection and imputation of the treatment variable, see Appendix 1.

Outcomes of Interest

This research focuses on the PFMC effects on individual retirement savings. I observe a number of important economic outcomes using administrative and this section briefly identifies the principle outcomes of interest for this research.[16]

First I investigate the effects of education on individual retirement savings decisions. The Thrift Saving Plan (TSP) is a tax-advantaged retirement program available to Federal employees administered by the Federal Retirement Thrift Investment Board (FRTIB).[17] As with a 401(k) plan, members can select from several fund options and contribute via payroll deduction or individual transactions.[18] While military members do not receive matching funds for their contributions, the contributions are tax-deferred or tax-exempt depending on the nature of the contribution.[19] Since I only observe individual contributions that occur via payroll deduction, my picture of an individual or household's retirement savings is incomplete. To the extent that

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[16] In an earlier version of this paper I tested the effects of the Personal Financial Management Course on a number of other outcomes. Military behavioral and performance outcomes included probabilities of punitive (disciplinary) discharges from service, probabilities of being barred or certified as eligible for reenlistment, the probability of promotion to Sergeant in the first term (a sign of good performance) and the mean time to promotion to Sergeant for those who are promoted. Other financial outcomes were the probability of military indebtedness and Montgomery GI Bill contributions. However, in all cases, the censoring among the treatment group in this data made inference using the empirical strategy below impossible and so these analyses were removed.

[17] For additional information on the TSP see https://www.tsp.gov/planparticipation/about/purposeAndHistory.shtml. Accessed on July 12, 2011.

[18] This sample contains only contributions made by payroll deduction. There may be unobserved additional deductions for individuals through personal deposits.

[19] Contributions made while deployed (when income is typically tax exempt) are also tax exempt. Normal contributions are tax-deferred as with most 401(k)s. In addition, tax-exempt contributions do not count towards the individual annual Federal elective deferral limit of $16,500. For more details, see "Your TSP Account: A Guide for Beneficiary Participants" available at: https://www.tsp.gov/PDF/formspubs/tspbk33.pdf. Accessed on July 12, 2011. For more details on the tax implications of different types of TSP contributions, see: https://www.tsp.gov/planparticipation/eligibility/contributionLimits.shtml. Accessed on August 10, 2011.

individuals can also make TSP contributions directly at a Finance Office, the estimates here will underestimate the effects of the program on total TSP contribution levels. In addition, I do not view other sources of individual or household retirement savings and thus my estimates only apply to the TSP. However, the TSP is an important part of many military members' retirement plans and nearly 31% of Active Duty Army members participate in the TSP.[20] In evaluating the PFMC's effects on TSP decisions, I will measure the probability of participation, the unconditional mean contribution level and the effects of the treatment on the TSP contribution distribution.[21] Retirement savings outcomes are of interest as the behavioral economics literature and the financial literacy literature are both filled with documentations of under saving. In addition, retirement planning in general and the TSP in particular comprise the most significant portion of the PFMC curriculum (2.0 hours of 8.0 total hours) and thus TSP account balances provide an important measure of the effectiveness of the program. Because TSP data is available for all military members, this first analysis performed for the full sample of n=87,859. To better understand the distribution of TSP Contributions in the first year, in Figure 1.3 I present a histogram of the Average Monthly TSP Contribution Data for the positive values of the distribution.

[20] The aggregate Army participation statistics are based on the May 2011 FRTIB Monthly Meeting Minutes and Published statistics, available at: http://www.frtib.gov/pdf/minutes/2011May.pdf. Accessed on July 12, 2011.

[21] While these total TSP outcomes can be decomposed into the tax-deferred and tax-exempt contributions, the military pay system automatically categorizes individual contributions based on their deployment and tax status. This makes individual decisions and knowledge of these features largely irrelevant. Once enrolled, individuals do not need to monitor the tax status of their contributions.

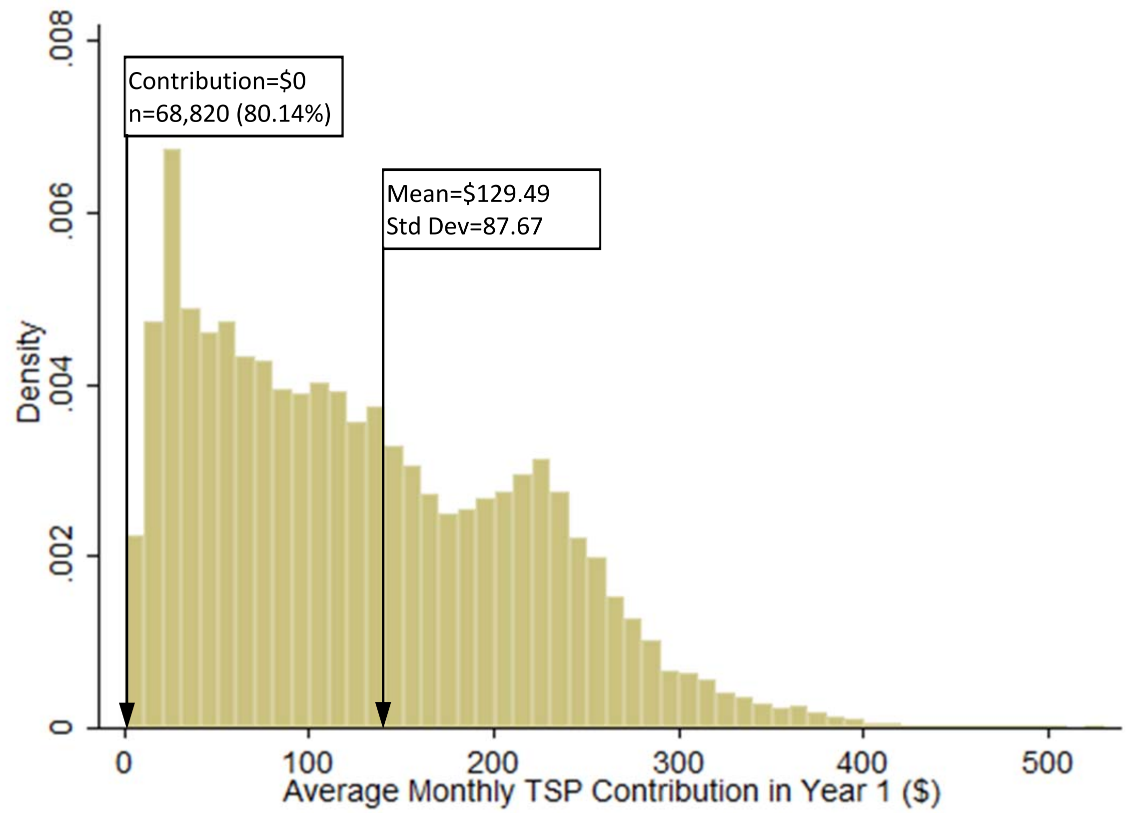

Figure 1.3. Distribution of Positive Average Monthly TSP Contributions in Year 1
Author compiled data using DOD data for 2006-2009. Contributions are winsorized at the 1st and 99th percentiles prior to the construction of averages. n=85,879.

As the histogram reveals, the vast majority (80.14%) of individuals in the full sample do not participate in the TSP during their first year. Among those who do contribute, the average monthly contribution level is approximately $130. Given this information, I will evaluate the effects of the PFMC throughout the average monthly TSP contribution distribution.

Assessing the impact of financial literacy education should involve observing outcomes at multiple horizons. However, given the relatively recent program implementation (most training started in 2007 and 2008) the focus of this paper is to assess financial outcomes in the short term. Future work may revisit this experiment and assess medium or longer term outcomes.

1.5. THEORETICAL MODEL & HYPOTHESES

The empirical estimation in this paper is reduced form in nature and relies on the exogenous variation in financial education completion for identification. While structural estimation of the effects of financial literacy education on financial behaviors is a worthwhile objective, this paper does not develop or test a structural model of financial decision-making. Instead, the basic model employed here assumes that financial decisions and behaviors are the result of individual characteristics and an individual's financial education. In this sense I present reduced form estimates of the effects of financial education. After reviewing the existing literature and the program objectives for the PFMC, I present the following hypotheses:

Hypothesis 1: Participation in Personal Financial Management Course (PFMC) will increase retirement savings in the Thrift Saving Plan (TSP).

In addition to testing the primary hypotheses above, I will attempt to identify the margins on which the PFMC appears to operate. To do so I examine the unconditional averages, probabilities of positive participation and the effects throughout the outcome distribution.

The starting point for my empirical estimation framework is presented below:

$$Y_{ijt} = \alpha + \beta \cdot PFMC_i + \gamma \cdot X_{ijt} + \varphi_j + \delta_t + \varepsilon_{ijt} \tag{1}$$

In this model Y_{ijt} is a measurement of financial decision-making for individual i who attended AIT at location j at in time period t. $PFMC_i$ is an indicator variable that equals 1 if the individual completed the Army's Personal Financial Management Course during AIT in time period t and equals 0 if they did not. X_{ijt} is a vector of individual characteristics including a quadratic in age, a quadratic in experience, marital status, number of children, an indicator for joining the military in the summer, education, gender, race, military income, a fixed effect for an individual's job, their AFQT score, an individual's enlistment term length, the number of months that the individual was deployed during the year and an individual's socioeconomic status. φ_j represents fixed effects for training location j. δ_t is a vector of time fixed effects in period t. Finally, ε_{ijt} is an individual error term assumed to be orthogonal to all other variables. To address potential heteroskedasticity, standard errors are clustered at the training location-month level to capture any unobserved correlation among individuals that experienced the treatment together. In this model β is the coefficient of primary interest and its predicted effects on financial decisions will be based on the nature of the decisions (i.e., save for retirement) and the Personal Financial Management Course curriculum.

To improve my estimation I will conduct utilize a regression discontinuity design and estimate the effects of the Personal Financial Management Course at the month of implementation. To perform this analysis I consolidate individuals across locations into common event month cohorts based on their commencement of AIT relative to the month of Personal Financial Management Course program implementation at their location. In Figure 1.4 I present the details of my event month cohort construction.

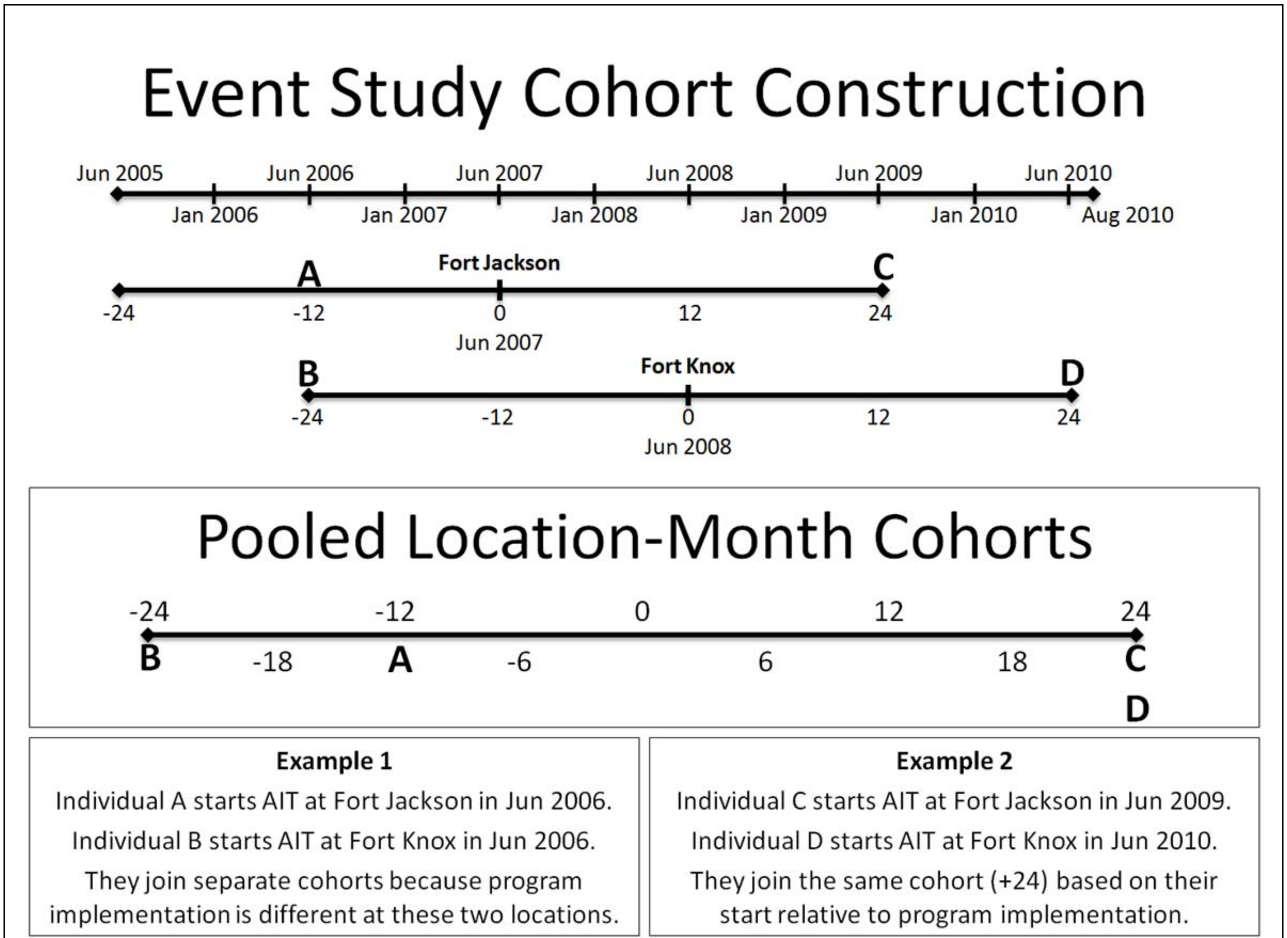

Figure 1.4. Event Month Cohort Construction for Regression Discontinuity (RD) Design
Author compiled data based on AER program implementation dates and Army administrative data on individual military jobs and entry dates.

Using these cohorts enables improved estimation techniques that can account for other unobserved factors influencing the patterns in financial behavior among cohorts. Specifically, the RD framework enables more precise estimation of the treatment effect by controlling for smooth functions of the running variable (here, the event month cohort) on both sides of the discontinuity. Employing this method my estimating equations take the following general form:

$$Y_{ijt} = \alpha + \beta \cdot f(R_i) + \gamma \cdot X_{ijt} + \varphi_j + \delta_t + \varepsilon_{ijt} \tag{2}$$

where $f(R_i)$ reflects the smooth function of the running variable that can take on varying forms (linear, cubic, etc...). In the simplest case, and the one I primarily use in this analysis, the linear RD analysis takes the following form:

$$Y_{ijt} = \alpha + \beta \cdot D_i + \lambda \cdot R_i + \mu \cdot DR_i + \gamma \cdot X_{ijt} + \varphi_j + \delta_t + \varepsilon_{ijt} \quad (3)$$

Here R_i reflects the running variable (event month) and takes on values [-12,12] excluding [-1,0,1]. D_i is an indicator that takes on a value of 1 for $R_i \geq 0$ and a value of 0 otherwise. Thus D_i corresponds exactly to the treatment variable $(PFMC_i)$ in the baseline model. Finally, DR_i reflects the interaction of these two variables and permits the slope of the smooth function to vary on both sides of the discontinuity. I am interested in the estimate β, which reflects the effects of the Personal Financial Management Course on the selected outcome at the month of program implementation.

1.6. EMPIRICAL ESTIMATION

The primary purpose of this research is to estimate the effects of an 8-hour mandatory course of instruction in financial management on financial decision-making. Identification of these causal effects of Personal Financial Management Course on the outcomes of interest requires exogenous assignment of the course to individuals. I discuss and test this assumption below. For now I assume that this program was exogenously assigned to individuals conditional on their job and entry date into the Army.

1.6.A. Summary Statistics

I begin the empirical analysis with a summary of my data. The research design relies on variation in financial literacy education during AIT. In Table 1, I present the summary statistics for the data by full sample, control and treatment groups. This analysis restricts its attention to the financial outcomes in an individual's first year of service, beginning with their time in AIT and ending 12 months later.

As Table 1.1 reveals, sample individuals are young, predominantly male and unmarried and almost universally educated at or above the high school level. While this sample is not nationally representative of the U.S. population or even the U.S. population with mean age 21, it is nonetheless large and demographically diverse. In addition, given the size of the U.S. Army and its importance to U.S. policymakers, the sample is of independent interest as a segment of the population for whom the government takes an active role in developing and protecting as part of the All-Volunteer Force (AVF).

As Panel A. reveals, the outcomes of interest (Probability of Participation and Average Monthly TSP Contributions) in the treatment group are substantially larger than those in the control group. While the outcome variables are statistically different from one another and consistent with the hypotheses above[22], simple t-tests are inadequate for causal inference given the other demographic differences evident in Panel B that could explain the outcome patterns and their omission of other time-varying effects. As a result I will proceed with event studies and multivariate regression estimates below.

[22] The coefficients, (standard errors), t-statistics and p-values for the Probability of Participation and Average Monthly TSP Contribution (Column 3 – Column 2) are: 20.68 (0.4360) t=-47.43 p=0.0000 and 16.92 (0.27) t=-63.59 p=0.0000 respectively.

Table 1.1. Summary Statistics by Treatment Condition for Administrative Data Sample

	(1)		(2)		(3)	
	Full Sample		**No Training**		**Received Training**	
	N=85,879		*N=42,467*		*N=43,412*	
Variable	**Mean**	**(Std Dev)**	**Mean**	**(Std Dev)**	**Mean**	**(Std Dev)**
Panel A. Outcomes						
Prob (TSP Participation), %	19.86	(39.90)	11.31	(31.67)	28.23	(45.01)
Avg Monthly TSP Savings, $	25.68	(64.71)	15.23	(52.15)	35.91	(73.56)
Panel B. Individual Characteristics						
Age, years	21.67	(4.11)	21.59	(4.10)	21.74	(4.12)
Experience, years	3.80	(4.02)	3.72	(3.99)	3.88	(4.06)
Female, %	0.16	(0.37)	0.15	(0.36)	0.16	(0.37)
Married, %	0.19	(0.40)	0.19	(0.39)	0.20	(0.40)
Number of dependents	0.87	(1.20)	0.89	(1.21)	0.85	(1.19)
Less than high school education, %	0.01	(0.08)	0.00	(0.01)	0.01	(0.11)
High school graduate, %	0.90	(0.30)	0.91	(0.28)	0.88	(0.32)
Some college, %	0.06	(0.25)	0.06	(0.24)	0.07	(0.25)
College graduate or more, %	0.03	(0.16)	0.02	(0.15)	0.03	(0.17)
Minority, %	0.32	(0.47)	0.31	(0.46)	0.33	(0.47)
AFQT Score, percentile	55.91	(19.56)	55.77	(19.42)	56.04	(19.71)
Summer accession, %	0.37	(0.48)	0.38	(0.48)	0.36	(0.48)
Enlistment term, years	3.81	(0.99)	3.84	(0.98)	3.78	(1.00)
AIT length, months	3.16	(1.12)	3.16	(1.13)	3.15	(1.10)
Monthly basic pay, $	1,553	(307)	1,608	(329)	1,498	(273)
Median HH Income in Zip Code, $	41,922	(14,060)	41,968	(14,079)	41,334	(13,806)
Months deployed during the year	1.16	(2.31)	1.04	(2.16)	1.28	(2.45)

Source: Department of Defense and Census Bureau Data. Notes: All data is for U.S. Army enlisted soldiers who enlisted between June 2006 and August 2009 and completed AIT at a given location within the 12 month period before and after program implementation, excluding the month before, of and after program implementation (n=85,879). All outcome variables (Panel A) in this table are measured for the period beginning during Advanced Individual Training and ending 12 months later. Average monthly TSP savings is the monthly average of the amount of the total TSP savings (tax-deferred and tax-exempt) during the 12 month period. Experience is an approximate measure of labor force experience at the time of enlistment and is calculated using age minus education minus 6 years. For this calculation, education is imputed using the following values: 10 years for high school dropouts; 11 years for GED holders; 12 years for high school graduates; 13 years for some college; 14 years for associate's degrees; 16 years for college graduates; 18 years for post graduate. The less than high school graduate variable includes dropouts and GED holders. The some college variable includes those with an Associate's Degree. The greater than or equal to college graduate variable includes those with Bachelor's, Master's and Doctorate degrees. The married variable represents formal and common law marriages for anyone who has ever been married. Average monthly pay represents the average monthly base pay during the 12 month period. The enlistment term variable represents the length of service that an individual has agreed to serve upon joining the military or reenlisting and typically varies from 2-6 years and the enlistment term during the 12 month observation period is used. The median household income data reflects the median household income from the 2000 U.S. Census (Sample File 3) for those individuals not missing zip code data. Sample sizes apply to all variables with the following exceptions: the education data is restricted to those not missing their education level (n=85,307); the monthly pay data is restricted to the individuals for whom this data was not missing (n=84,447); the median household income is restricted to the individuals for whom this data was not missing (n=41,995).

1.6.B. Event Studies

I turn now to more powerful visual evidence of the differences in the control and treatment groups. In Figures 1.5 and 1.6, I present non-parametric event studies of the Probability of TSP Participation and the Average Monthly Level of TSP Contributions for individuals in their first year in the Army. I note initially that these are not traditional event studies that follow an individual over time. Instead, I use the cohorts described above that pool individuals from 12 different AIT locations based on the month that they started AIT relative to the month when the financial management course was implemented at their AIT location. For example, an Infantry soldier who started AIT at Fort Benning, GA in July 2007 (3 months prior to program implementation at Fort Benning) and an Engineer soldier who started AIT at Fort Leonard Wood, MO in February 2008 (3 months prior to program implementation at Fort Leonard Wood) are both assigned to the event study cohort of -3. Once pooled into month cohorts, I average the outcomes of interest (probability of participation and the average monthly TSP contribution during the first year in the Army) for each cohort.

Figure 1.5 presents a striking result: there is a large, discontinuous increase in the probability of TSP participation at the time of program implementation across locations. The increase in participation appears to be roughly 15 percentage points, approximately doubling the probability of participation. Formal tests of this difference are completed below.

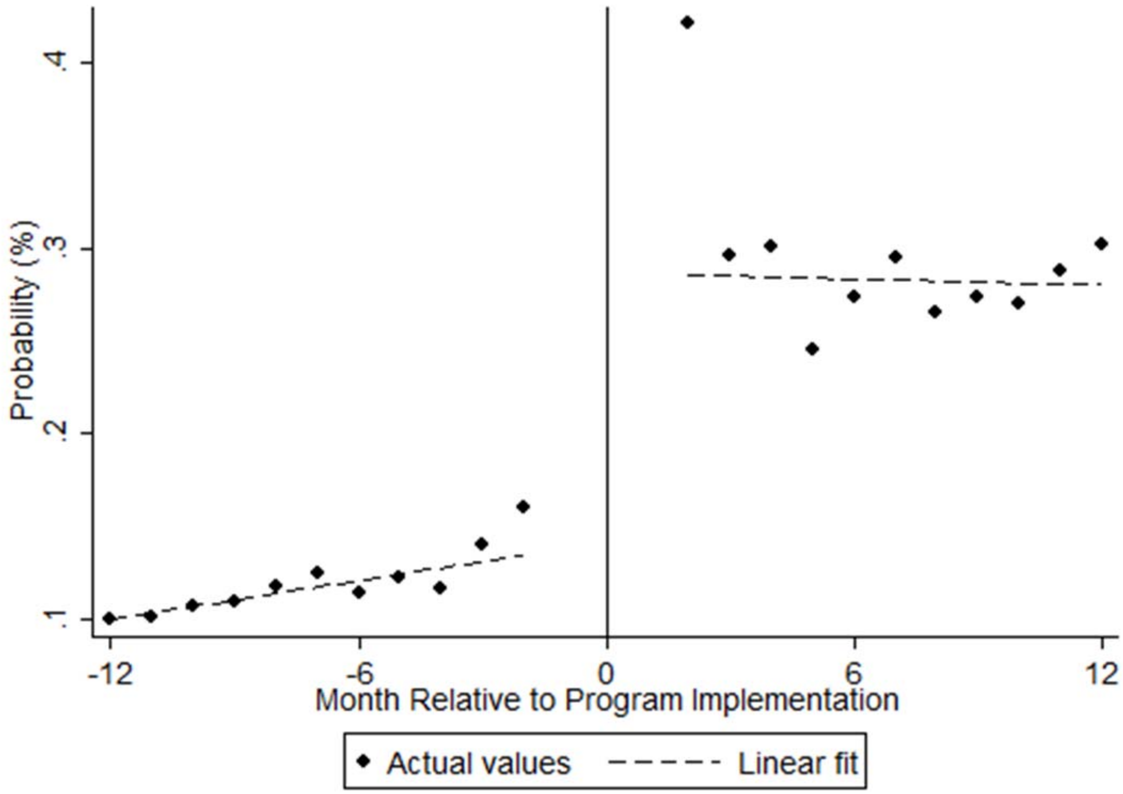

Figure 1.5. Event Study for the Probability of TSP Participation in Year 1
Author compiled data using DOD and AER data for 2006-2009. The participation indicator takes on a value of one for any participation in year 1 and zero for no participation. The values at each x-axis value depict the average probability of participation for all individuals in each cohort. The dashed lines depict an unweighted linear fit of these values to visually demonstrate the patterns in participation among cohorts. n=85,879.

The second event study, displaying the average TSP contribution levels, depicts a similar pattern. Figure 1.6 reveals that the average monthly level of contribution also nearly doubles at the time of program implementation from around \$20 to \$40. Once again this is a large effect. To address concerns over other possible explanations for the patterns depicted in these event studies, I complete balance of covariate tests and additional event studies using predicted outcomes that omit treatment in Section 1.7.

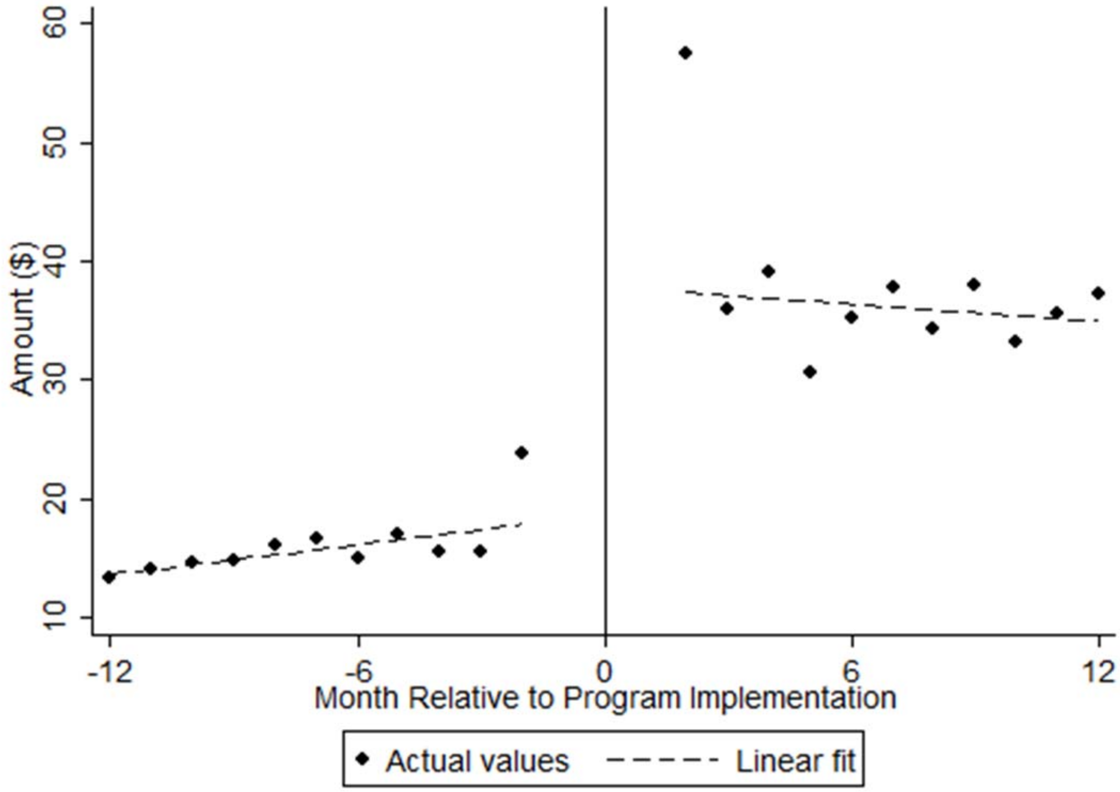

Figure 1.6. Event Study for the Average Monthly TSP Contributions in Year 1
Author compiled data using DOD and AER data for 2006-2009. The monthly average is the mean of all contributions made by month in Year 1. The values at each x-axis value depict the average of the monthly average contributions for all individuals in each cohort. The dashed lines depict an unweighted linear fit of these values to visually demonstrate the patterns in participation among cohorts. n=85,879.

These event studies support a few conclusions. First, there appear to be large discontinuities in the outcomes of interest at the time of program implementation (month 0). Second, the patterns in the data appear to support the use of a linear functional form in controlling for the cohort participation patterns over time. Finally, while the event studies provide suggestive evidence that the PFMC increased TSP participation, these methods do not account for the differences in cohort demographics or time periods that might also explain the increases in participation. As a result I now complete several analyses utilizing multivariate regression in a regression discontinuity framework.

1.6.C. Regression Estimates

My use of regression discontinuity techniques relies on the sharp nature of the policy change to estimate the average treatment effects among all students in the PFMC relative to their counterparts in the control group cohorts. Given the mandatory nature of the course, all individuals who started AIT at a location after the program was implemented can be assumed to have completed the course and therefore treatment assignment is sharp.[23] Concerns over fuzzy treatment assignment generated by individuals who started AIT at a location before the program was implemented but finished AIT after the program was implemented motivated my omission of such individuals from the sample.[24] In addition, the event studies suggest that the constructed sample does not suffer from contamination. As a result, this data set and the program design support the use of RD analysis. I am interested in the mean outcome (Probability of TSP Participation or Average Monthly TSP Contribution for individuals in their first year of service) on either side of the implementation threshold and the difference between these means. In this case the implementation threshold varies by AIT location but pooling the individuals at different locations into monthly cohorts relative to the program implementation enables unbiased estimation of the differences in the mean outcomes at the discontinuity. In Table 1.2 I present the main effect estimates of the PFMC on average monthly TSP contributions in Years 1 and 2.

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[23] While the course is mandatory, it is neither a graded event nor a requirement for completing AIT. As a result, some individuals may have missed the course or paid little attention. I discuss violations of the attendance assumption below. Briefly, imperfect attendance or inattention would attenuate my results.

[24] See Appendix 1.2 for more details on the sample construction.

Table 1.2. RD Estimates of PFMC Effects on Average Monthly TSP Contributions, by Year											
	(1)		(2)		(3)		(4)		(5)		(6)
Outcome	Y		Pr(Y>$0)		Pr(Y≥$100)		Pr(Y≥$200)		Pr(Y≥$300)		Pr(Y≥$400)
Panel A: Year 1 Outcomes											
PFMC Effect	16.38	***	16.15	***	6.47	***	1.85	**	0.50	*	0.07
Std Err	(3.75)		(2.70)		(1.69)		(0.79)		(0.26)		(0.08)
Control Mean	15.23		11.31		6.59		3.04		0.41		0.04
Adj R^2	0.1200		0.1244		0.1000		0.0718		0.0096		0.0015
N	85,879		85,879		85,879		85,879		85,879		85,879
Clusters	266		266		266		266		266		266
Panel B: Year 2 Outcomes											
PFMC Effect	33.91	***	29.57	***	12.91	***	6.90	**	2.22		0.56
Std Err	(7.01)		(4.31)		(2.97)		(2.81)		(1.89)		(0.72)
Control Mean	21.91		16.82		9.05		4.03		0.84		0.12
Adj R^2	0.1073		0.1115		0.0854		0.0681		0.0120		0.0007
N	54,933		54,933		54,933		54,933		54,933		54,933
Clusters	186		186		186		186		186		186

Source: Department of Defense and Census Bureau Data. Notes: All data is for U.S. Army enlisted soldiers who enlisted between June 2006 and August 2009 and completed AIT at a given location within the 12 month period before and after program implementation, excluding the month before, after and of program implementation. The coefficient reported is for the discontinuity at the month of implementation. All regressions include the following covariates: a quadratic in age, quadratic in experience, indicators for female, married, minority, number of dependents and a summer entry, indicators for education levels less than or equal to high school, some college, and greater than or equal to college (high school graduate is the omitted category), AFQT score, enlistment term, average monthly base pay, median household income in the individual's zip code of record, AIT length, number of months deployed in the year and fixed effects for an individuals' job, military branch, AIT location, and AIT start month. Indicator variables capture individuals missing pay or zip code income data and these individuals are assigned values of zero for these variables. Heteroskedasticity robust standard errors, clustered at the AIT location-month level, are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

Table 1.2 provides significant evidence of both the distributional and longitudinal effects of the PFMC on TSP contributions.[25] On average, the PFMC more than doubles the average monthly TSP contribution relative to the control group mean in both years ($16.42 vs. $15.23 in year 1 and $33.88 vs. $21.91in year 2) and both results are highly significant (p<0.001). On the extensive margin, the program increases contributions significantly, by 16.17% in year 1 relative to a control group mean of 11.31% and by 29.68% in year 2 relative to a control group mean of 16.82%, with both effects highly statistically significant (p<0.001). Since treatment may induce

[25] For complete regression results for the Average Monthly TSP Contributions in year 1, see Table 2.13.

changes in the composition of TSP contributors, I evaluate the intensive margin effects by looking at the program's effects on the probability of contributions greater than or equal to the levels specified in columns (3) through (6) of Table 1.2. As the results show, the program has positive effects throughout the distribution. In year 1, the course has a large and statistically significant effect on the probability of contribution levels up to and including $300 per month (99th percentile is $282). Similarly, in year 2, the course has a large and statistically significant effect on the probability of contribution levels up to and including $200 per month (95th percentile is $206). Taken together, the results are large, pervasive and persistent.

1.6.D. Distributional Effects

In addition to the regression table I present graphs of the distributional and longitudinal effects of the PFMC. In Figure 1.7, I present the effects of the PFMC on the distribution of TSP savings for the 0-95th percentiles. The graph reveals that the PFMC effects are large and statistically significant throughout the distribution. Thus the PFMC affects those unlikely to save for retirement on their own in year 1 and increases among those who were likely to save.

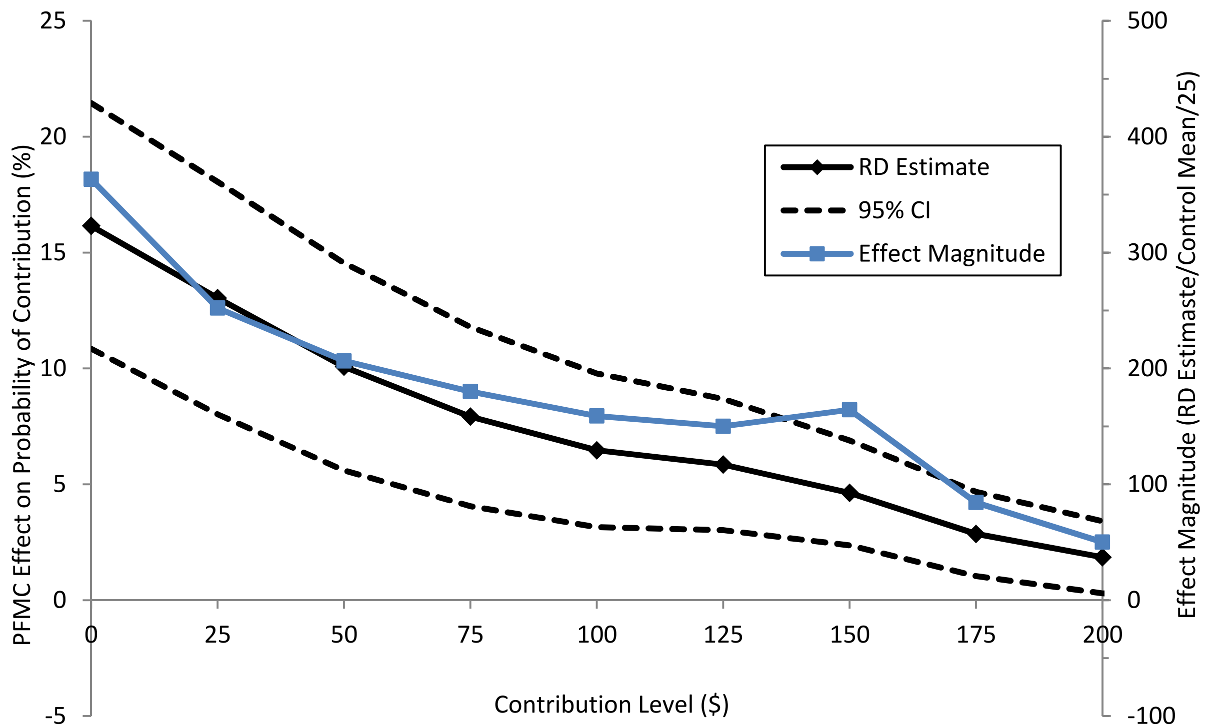

Figure 1.7. PFMC Effects on the Distribution of Average Monthly TSP Contributions in Year 1
Author compiled data using DOD and AER data for 2006-2009. The x-axis values reflect the 0-95th percentiles of the distribution for the Average Monthly TSP Contributions ($0-$195).The RD estimates and 95% confidence intervals reflect a series of regressions on indicator variables for Average Monthly TSP Contribution levels that correspond to the x-axis. For each bin (k) of binwidth $25 on the x-axis [0,200], I generate an indicator for contribution levels in the $25 bin [i.e., I=1 if k<TSP≤k+25 and I=0 Otherwise]. I then complete RD regressions on the series of indicators and identify the effects of the PFMC in each bin. These estimates correspond to the left axis. To convert these probability estimates into a dollar effect magnitude I divide the RD estimate by f(k)/25 where f(k)=Prob(k<TSP≤k+25). These estimates are depicted in blue and correspond to the right axis. n=85,879.

1.6.E. Longitudinal Effects

In Figure 1.8, I present the longitudinal effects of the PFMC on TSP contributions by month for the first two years of an individual's service. Using my RD framework I estimate the effects of the PFMC on TSP contributions for each month in the individual's first two years. As the graph reveals, the effects vary from approximately $5-$30 each month. In addition, the estimates are statistically different from zero at α=0.05 for 17 of 24 months. However, in year two there are two noticeable changes in the estimates. First, the standard errors increase and several of the year 2 estimates are statistically indistinguishable from zero. Second, the main effect estimates also decrease from month 12 to month 18 and 19. These changes result from censoring in the data. Since some treatment group members are censored in the administrative data in year 2 they and their control group counterparts by cohort month and location are removed from the analysis. Clearly this censoring reduces the estimated program effects, implying that the omitted individuals come from AIT locations and cohorts where there were strong PFMC effects. While this graph provides additional evidence of the persistent effects of the PFMC on TSP contributions, it also reveals that the largest program effects appear to take place in the first six months starting with AIT.

If inertia plays a large role in individual savings decisions, as the large behavioral economics literature on the question suggests that it does (Choi et. al., 2004), then the financial management course may simply motivate individuals to enroll in the TSP with payroll deduction and once they do, to continue to use payroll deduction for their contributions. As a result, the reductions in program effects are likely to due to control group members starting their contributions later in their service. If the payroll deduction has less lasting effects then the decline in program effects might also be due to treated individuals reducing their contributions.

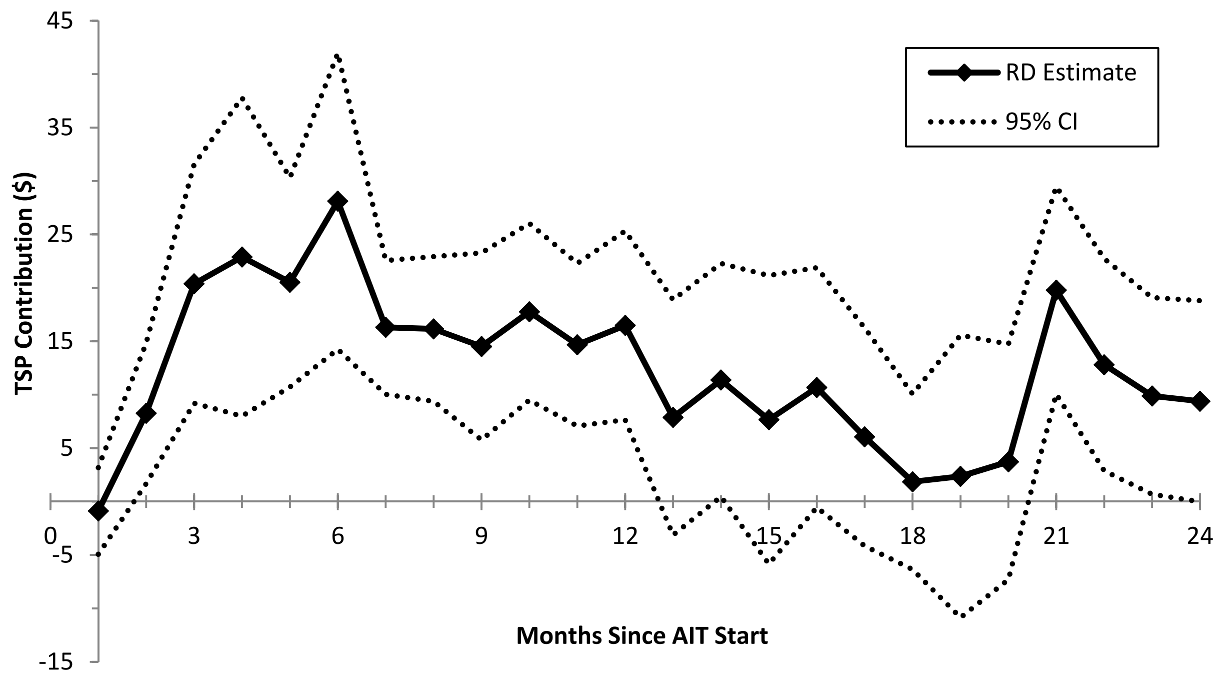

Figure 1.8. PFMC Effects on Average Monthly TSP Contributions in Years 1-2
Author compiled data using DOD and AER data for 2006-2009. Each point reflects the RD estimates of the PFMC effect on the TSP Contribution in the respective month. n=85,879 for months 1-12; n=64,017 for months 13-24.

In Figure 1.9, I present the longitudinal effects of the PFMC on cumulative TSP contributions by month for the first two years of an individual's service. First I create a TSP cumulative account balance for each month in the first two years and then I find estimates of the effects of the PFMC on these balances at each month using my RD framework. As the graphs shows, the main effects of the PFMC are persistent and increasing with time. However, as described above, the year two estimates have larger standard errors and lower point estimates than in year one. Even so, all estimates remain statistically different from zero at α=0.10. As a result, while the censoring is unfortunate, the first year results provide suggestive evidence of a trend that the PFMC establishes and sustains larger cumulative TSP contribution balances.

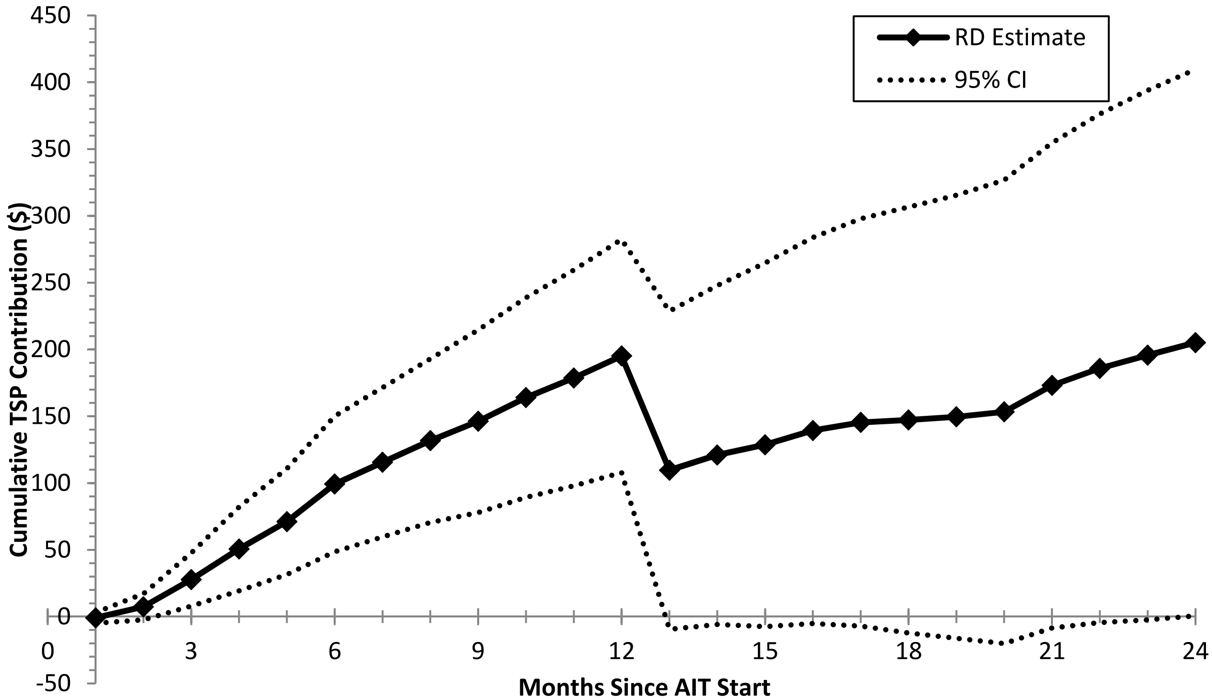

Figure 1.9. PFMC Effects on Cumulative Monthly TSP Contributions in Years 1-2

Author compiled data using DOD and AER data for 2006-2009. Each point reflects the RD estimates of the PFMC effect on the Cumulative TSP Contribution in each month. n=85,879 for months 1-12; n=64,017 for months 13-24.

1.6.F. Heterogeneous Treatment Effects

The evidence presented thus far suggests large average treatment effects of the PFMC across individuals. However, the course may have different effects on different individuals. The rich nature of the military administrative data enables evaluation of heterogeneous treatment effects by a host of demographic variables. In Table 1.3 I present RD estimates for the heterogeneous treatment effects of the PFMC by gender, human capital (education and AFQT scores), socioeconomic status (SES) and marital status.

Table 1.3. Heterogeneous Treatment Effects of PFMC on Average TSP Contributions in Year 1

	(1)		(2)		(3)		(4)		(5)		(6)	
Interaction:	None		Female		AFQT		Education		SES		Married	
PFMC Main Effect	16.51	***	18.43	***	15.93	***	10.13	***	23.55	***	17.28	***
	(3.75)		(3.93)		(3.74)		(3.76)		(5.13)		(3.94)	
Female	-1.73	**	3.78		-1.73	**	-1.72	**	-1.74	**	-1.72	**
	(0.77)		(2.84)		(0.77)		(0.76)		(0.77)		(0.77)	
Female × PFMC			-13.60	***								
			(4.49)									
AFQT Quartile 2 (Q2)	-0.16		-0.11		-0.15		-0.16		-0.13		-0.15	
	(0.59)		(0.59)		(0.59)		(1.74)		(0.59)		(0.60)	
AFQT Q2 × PFMC					0.90							
					(2.40)							
AFQT Q3	-0.25		-0.25		-0.23		-4.54	*	-0.22		-0.22	
	(0.86)		(0.86)		(0.86)		(2.73)		(0.86)		(0.86)	
AFQT Q3 × PFMC					9.33	***						
					(3.19)							
AFQT Q4	8.15	***	8.17		8.19	***	2.02		8.19	***	8.16	***
	(1.21)		(1.21)		(1.21)		(3.38)		(1.21)		(1.21)	
AFQT Q4 × PFMC					20.20	***						
					(4.64)							
Educ ≥ Some College(SMC)	1.85		1.92		-6.36	**	2.08		1.85		1.89	
	(1.27)		(1.28)		(3.12)		(1.27)		(1.27)		(1.28)	
SMC × PFMC							7.35					
							(4.88)					
SES Q3	-0.11		-0.02		-0.08		-0.33		3.76		-0.11	
	(1.48)		(1.47)		(1.48)		(1.49)		(4.89)		(1.47)	
SES Q3 × PFMC									-14.42	*		
									(7.85)			
SES Q4	-0.14		0.12		-0.12		0.15		4.22		-0.05	
	(1.47)		(1.46)		(1.47)		(1.48)		(4.89)		(1.46)	
SES Q4 × PFMC									-16.00	*		
									(8.56)			
Married	-0.65		-0.65		-0.65		-0.69		-0.63		0.34	
	0.63		(0.64)		(0.63)		(0.64)		(0.64)		(2.11)	
Married × PFMC											-4.05	
											(3.18)	
Control Mean	15.23		15.23		15.23		15.23		15.23		15.23	
Adj R^2	0.1203		0.1217		0.1204		0.1226		0.1207		0.1206	
N	85,879		85,879		85,879		85,879		85,879		85,879	
Clusters	266		266		266		266		266		266	

Table 1.3, Continued

Source: Department of Defense, Census Bureau and Credit Bureau Data. Notes: All data is for U.S. Army enlisted soldiers who enlisted between June 2006 and August 2009 and completed AIT at a given location within the 12 month period before and after program implementation, excluding the month before, of and after program implementation. The aggregate balance outcome excludes mortgage debt. The coefficient reported is for the discontinuity at the month of implementation. All regressions include the following covariates: a quadratic in age, quadratic in experience, indicators for female, married, minority, number of dependents and a summer entry, indicators for education levels less than or equal to high school, some college, and greater than or equal to college (high school graduate is the omitted category), AFQT score, enlistment term, average monthly base pay, median household income in the individual's zip code of record, AIT length, number of months deployed in the year and fixed effects for an individuals' job, military branch, AIT location and AIT start month. Indicator variables capture individuals missing pay, or zip code income data and these individuals are assigned values of zero for these variables. Heteroskedasticity robust standard errors, clustered at the AIT location-month level, are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

As the regression results show, there are only a few statistically significant heterogeneous treatment effects. First, the treatment interacts negatively with being female (or positively with male), with the PFMC reducing TSP contributions for treated females by $13.60 on a control mean of $15.23. These effects might occur due to greater peer effects by males in the military and/or greater levels of risk aversion among females.[26] Second, the upper two quartiles of AFQT have a positive interaction with treatment. These results suggest that the PFMC increases average monthly TSP contributions for students in the third (fourth) quartile of the AFQT distribution by $9.33 ($20.20) on a control mean of $15.23 and both effects are highly significant (p<0.005) relative to students in the first quartile. The interpretation is that individuals with higher AFQT scores respond more positively to the PFMC lesson on retirement savings and the TSP and subsequently increase their contributions more. Second, the upper two quartiles of SES, measured by the median household income of the individual's zip code of record, have a negative interaction with treatment. These results suggest that the PFMC decreases average monthly TSP contributions for students in the third (fourth) quartile of the

[26] See Xiao et al. 2011 for a detailed review of differential risk preferences by gender and their potential explanations.

SES distribution by $14.22 ($16.00) on a control mean of $15.23 relative to the lower two quartiles, but both effects are marginally significant (p=0.067 and p=0.063 respectively). Here the interpretation is less clear but the results imply that individuals with high SES, controlling for a variety of other factors, respond less favorably to the treatment relative to those individuals in the lower two quartiles of SES. It may be that individuals with lower SES have less previous experience with information on the time value of money and the role and importance of saving for retirement relative to individuals with higher SES. Finally, the remainder of the groups: women, individuals with some college or more and married individuals, have no statistically distinguishable interactions with the PFMC.

1.6.G. Discussion of Results

Treatment Effect Mechanism

The evidence presented in Sections 1.6.A through 1.6.F strongly suggests that the PFMC increases individual contributions to the Thrift Savings Plan. The mechanism for this increase is not identified though and might be increased financial literacy and sophistication or it might be a reduction in the costs of TSP enrollment. That is, the PFMC might have taught individuals the importance of retirement savings and the value of the TSP or it might simply have assisted them with enrollment, or a combination of both. In fact, the PFMC does discuss TSP enrollment procedures and requirements and it also provides assistance in actual TSP enrollment, both through form completion and form submission.[27] Other research has shown the positive effects of enrollment assistance in the context of education decisions (Clayton and Dynarski 2006,

[27] Based on author discussions with AER program director and SDCC personnel, Fall 2011.

Bettinger et al. 2009) and retirement savings (Beshears et al. 2006a, 2006b) and such assistance can also be expected to increase enrollment in the TSP. However, the presence of potential behavioral effects should not completely undermine the case for financial literacy development. In this case, TSP enrollment still requires an active decision and comparison of the costs and benefits of making TSP contributions. Enrollment does not occur via a default rule change and individuals must take active steps to enroll. Since the decisions under consideration require some reflection and effort by students, behavioral assistance seems unable to account for the full effect. While AER program administrators might be results oriented and be primarily concerned with the total effects, Army administrators and other public policy-makers might desire more detailed information on the mechanism at work, both to tailor this course and design others in the future. Ideally, the two effects could be separately identified and measured. In the context of the PFMC, while there may have been variation in the enrollment assistance provided at each location, there is no reliable data on where and when this variation occurred.[28] Nonetheless, in this sample, the roles of human capital and behavioral assistance cannot be separated and the large program effects documented above should be thought of as the combination of human capital (financial literacy) development and assistance. Additional work is required to identify the effects of financial literacy separate from behavioral policy assistance. Chapter 2 of this dissertation explores the effects of the PFMC in the context of other financial behaviors where behavioral confounds are absent or less present.

[28] AER and SDCC personnel confirmed that the enrollment assistance provided has and does vary by location, but that the precise timing of the policy changes driving these differences is not documented.

Threats to Identification

Typical concerns when employing a regression discontinuity (RD) design include concerns over other changes occurring at the threshold value (in this case, something else changed at a location at the same time that the PFMC course was implemented) and concerns over individual manipulation to obtain or avoid the treatment (in this case, individuals attending AIT early or delaying their AIT start based on the course implementation). Neither concern seems valid in this case given the nature of this policy change, where a month-location combination is the forcing variable. I first note that the course implementation dates were selected by the executing agency, Army Emergency Relief, external to the Army generally and the Army Recruiting Command, which manages the accession and distribution of new enlistees by jobs for the entire enlisted force, specifically. Locations and dates of implementation were based on the ability to field instructors at an AIT location and the course was implemented at 12 different locations (nearly all with differing mixes of all other covariates) nationwide at 8 different points in time spanning 2 years. It seems unlikely and almost impossible that AER could have systematically determined the course implementation dates with respect to any observable characteristics of enlisting individuals.[29] Second, I present the results of a series of randomization tests in Table 1.4 that formally establish that the covariates employed in this analysis are systematically unrelated to the treatment/discontinuity.[30] As the results show, none of the observable covariates are systematically related to treatment, validating the experimental design and justifying causal inference for the PFMC effects.

[29] This intuition is confirmed by author conversations with the AER Program Director from February through March 2011 and discussions with the contractor in July and August 2011. Soldiers' observable characteristics were never identified as a consideration in the timing of the program implementation.

[30] For additional balance of covariate tests (event studies), see the Section 1.7. Robustness Checks.

Table 1.4. Randomization Tests for Administrative Data Sample

Variable	Coeff	(Std Err)	p-value	Sig	Adj-R2	Obs	Clust.	Control Mean
Age, Years	18.91	(24.05)	0.4324		0.0271	85,879	266	21.59
Experience, Years	16.89	(21.48)	0.4325		0.0295	85,879	266	3.72
Female, %	4.83	(6.69)	0.4703		0.0151	85,879	266	15.32
Married, %	1.46	(2.08)	0.4849		0.0062	85,879	266	18.62
Number of Dependents	0.07	(0.06)	0.1990		0.0069	85,879	266	0.89
Education ≤ High School Graduate, %	0.32	(0.37)	0.3863		0.0226	85,879	266	0.02
Education = High School Graduate, %	3.23	(2.10)	0.1265		0.0133	85,879	266	91.38
Education = Some College, %	0.59	(1.02)	0.5599		0.0023	85,879	266	6.22
Education ≥ College Graduate, %	-0.09	(0.73)	0.9030		0.0012	85,879	266	2.38
Minority, %	4.95	(5.84)	0.3974		0.0112	85,879	266	30.77
AFQT Score, Percentile	-2.28	(2.10)	0.2773		0.0336	85,879	266	55.77
Summer Enlistment, %	-6.51	(4.83)	0.1784		0.6906	85,879	266	37.74
Enlistment Term Length, Years	-0.01	(0.14)	0.9531		0.0416	85,879	266	3.84
AIT Length, Months	0.10	(0.30)	0.7381		0.1086	85,879	266	3.16
Monthly Basic Pay, $	5.01	(14.91)	0.7373		0.1637	85,879	266	1,608
Median HH Income in Home Zip, $	-1,136	(921)	0.2187		0.7536	85,879	266	41,968
Months Deployed	0.07	(0.19)	0.7185		0.0338	85,879	266	1.04

Source: Department of Defense, Census Bureau and Credit Bureau Data. Notes: All data is for U.S. Army enlisted soldiers who enlisted between June 2006 and August 2009 and completed AIT at a given location within the 12 month period before and after program implementation, excluding the month before, of and after program implementation. Each cell reports the RD estimate for the effect of the discontinuity at the month of program implementation on the covariate specified in the respective row. All regressions include the running variable (event month relative to program implementation), an indicator for positive values of the running variable, an interaction between the running variable and the discontinuity indicator, and month fixed effects. The education data, monthly pay data and household income control means are calculated for those observations not missing this data. Heteroskedasticity robust standard errors, clustered at the AIT location-month level, are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively and are depicted in the column labeled Sig.

With respect to the second concern, individual selection into or out of treatment also seems unlikely. The program was managed and implemented by AER but the program was not integrated with U.S. Army recruiting efforts. As a result, recruiters, who are the primary source of information for individuals considering enlistment, were not informed about the course, its contents, or the dates of implementation for each AIT location. As a result, individual decisions on whether and when to enlist were made exogenously with respect to course implementation. Even if individuals somehow knew about the course and its scheduled implementation date at a given location, delaying or accelerating enlistment into the military, a significant life event, is not likely to be influenced by an 8 hour course in financial management that is part of an initial entry training phase (Basic Training and Advanced Individual Training) that for most individuals is several months long. Second, the randomization test and balance of covariate tests in the robustness checks both suggest that there was no selection based on any observable characteristics.

Interpretation of Estimates

The interpretation of the magnitude of the effects of the PFMC requires consideration of several program features and other institutional features facing new enlistees during the period under consideration. Taken together, five features suggest that the estimates presented here ought to be considered lower bound estimates of the effects of the PFMC. The first two features deal with contamination, the third with peer effects, the fourth with role model effects and the fifth with additional and simultaneous financial education in the U.S. Army. First, while the course is required for all individuals in AIT, there course is not graded nor is it an explicit requirement for completing AIT. As a result, absences, either deliberate or as a result of competing demands for soldiers' time, would result in “treated” individuals foregoing treatment and attenuate the results of treatment. This parallels the role of the first stage in traditional instrumental variables (IV) analyses where ordinary least squares (OLS) estimates are “scaled up” by the treatment non-compliance rate. In this experimental design I have imputed and assumed a first stage of 100% compliance. To the extent that this assumption is violated, my estimates will serve as lower bounds and the actual effects of the PFMC will be even larger.

Second, I also assume that there are no delays in individual training and that individuals proceed through AIT as scheduled. If individuals are delayed in their AIT training, either due to training deficiencies, injuries or other unobserved institutional reasons, individuals in the control group (no PFMC) might be delayed until treatment is initiated at a given location. While my removal of the month cohorts on both sides of program implementation (cohorts -1 and 1) from the sample should alleviate these concerns, any other delays greater than one month might result in control group members receiving treatment, again attenuating my results and marking them as lower bounds.

Third, control group individuals might interact with treatment group individuals after AIT. These interactions are likely given the mixing of new soldiers of varying job skills into units at Army locations worldwide. If individuals in the control group and individuals in the treatment group are subsequently assigned to the same unit (as roommates, barracks mates, squad mates, colleagues or friends) then they may discuss the contents of the course and reduce any differences in knowledge and motivation that the financial management course imparted. From the military's perspective, such spillovers are desirable. From an experimental standpoint though, this contamination will reduce the measured differences in control and treatment group members in the future.

Fourth, role model effects and Army leadership efforts may bias findings downward. With role model effects, unit leaders (Officers or Non-Commissioned Officers) may act to help soldiers facing financial problems through voluntary counseling or even requiring financial training at the duty location (post-AIT). If control group members are more likely to encounter such problems than treatment group members then these efforts may act to mitigate differences between the groups.

Finally, the PFMC is not the Army's only financial education program. To the extent that other financial training is available to control group individuals at their AIT location (outside the AIT program) or at their first duty assignment location, control group individuals may receive training and experience treatment similar to the PFMC.[31] While this training is also available to treatment group members, if there are diminishing returns or any "John Henry" effects among control group members who feel as if they need to obtain more financial literacy, then this condition works against finding positive effects of training. Even so, the basic assumption of monotonicity holds; there is no condition in which control group members can receive more financial education than treatment group members; all optional training opportunities are equally is available to both groups and mandatory training requirements outside of AIT apply to both groups, regardless of treatment status.[32] Fortunately for the research design, all five of these factors work against any findings of beneficial effects of the Personal Financial Management Course.

1.7. ROBUSTNESS CHECKS

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[31] Many Army installations have implemented mandatory financial literacy training for first-term soldiers. This training aims to improve the financial capabilities of individuals and their families. As an example, see the policy directive for Fort Sill, OK from 2004 at: http://www.sillmwr.com/Forms/acs/financialReadiness/608-1.pdf.

[32] The situation in which some members were receiving training in AIT , others at their first duty location and still others at both locations leaves open the possibility for further research into optimal training design by evaluating the effects of those with zero, one or multiple courses. Unfortunately, the first assignment training requirements are idiosyncratic to different post locations and the data is difficult to obtain. Nonetheless, I am investigating this alternate course and its potential for additional analyses.

In this section I complete a series of robustness checks to validate the assumptions, analyses and findings above. Specifically, I complete five types of checks: regression estimates with and without covariates, balance of covariate event studies, predicted outcome event studies, functional form validation for my regression discontinuity estimates and longitudinal treatment effect estimates comparing the year 1 and year 2 samples. Together these checks validate the identification strategy for this research and support causal inference for my PFMC estimates.

Table 1.5. RD Estimates of PFMC Effects with and without Covariates

		(1)		(2)		(3)	
Panel A: Average Monthly TSP Contribution ($)							
PFMC Effect		19.33	***	16.55	***	16.38	***
	Std Err	(6.20)		(5.26)		(3.75)	
	Control Mean	15.23		15.23		15.23	
	Covariates	N		Y		Y	
	Fixed Effects	N		N		Y	
	Adj R^2	0.0257		0.0896		0.1200	
	N	85,879		85,879		85,879	
	Clusters	266		266		266	
Panel B: Probability of TSP Participation (%)							
PFMC Effect		14.85	***	14.34	***	16.15	***
	Std Err	(4.12)		(3.75)		(2.70)	
	Control Mean	11.31		11.31		11.31	
	Covariates	N		Y		Y	
	Fixed Effects	N		N		Y	
	Adj R^2	0.0452		0.0967		0.1244	
	N	85,879		85,879		85,879	
	Clusters	266		266		266	

Source: Department of Defense and Census Bureau Data. Notes: All data is for U.S. Army enlisted soldiers who enlisted between June 2006 and August 2009 and completed AIT at a given location within the 12 month period before and after program implementation, excluding the month before, after and of program implementation. The coefficient reported is for the discontinuity at the month of implementation. All regressions include the following covariates: a quadratic in age, quadratic in experience, indicators for female, married, minority, number of dependents and a summer entry, indicators for education levels less than or equal to high school, some college, and greater than or equal to college (high school graduate is the omitted category), AFQT score, enlistment term, average monthly base pay, median household income in the individual's zip code of record, AIT length, number of months deployed in the year and fixed effects for an individuals' job, military branch, AIT location, and AIT start month. Indicator variables capture individuals missing pay or zip code income data and these individuals are assigned values of zero for these variables. Heteroskedasticity robust standard errors, clustered at the AIT location-month level, are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

1.7.A. Regression Estimates with and without Covariates

To rule out differences in observable characteristics as the source of my estimated treatment effects, I complete a series of regressions for my main outcomes (Average Monthly TSP Contributions and Probability of TSP Participation) in Year 1 in which I estimate the program effects using only the RD framework, the RD framework and covariates and the complete specification that uses the RD framework, covariates and fixed effects. In Table 1.5 I present the results of these regressions. As the table reveals, the estimated effects are largely consistent and robust to these variations in specification. As a result it seems unlikely that differences in observable characteristics are driving the results.

1.7.B. Balance of Covariate Event Studies

In Figure 1.10 I present a series of event studies that depict the patterns in each observable characteristic that I use in my regression estimation. While there is substantial variation in the patterns across these characteristics and even a few apparent discontinuities, there are no large discontinuities that might explain the primary findings here and these event studies do not account for differences in participation patterns over time. In addition, as the randomization test results in Section 1.6.G. (Table 1.4) revealed, none of these differences are statistically significant when tested against the RD framework. To fully explore the role of these differences, in the next section I use all of the potential differences in these observable characteristics to predict the TSP outcomes among the treatment group and compare that to the actual TSP outcomes.

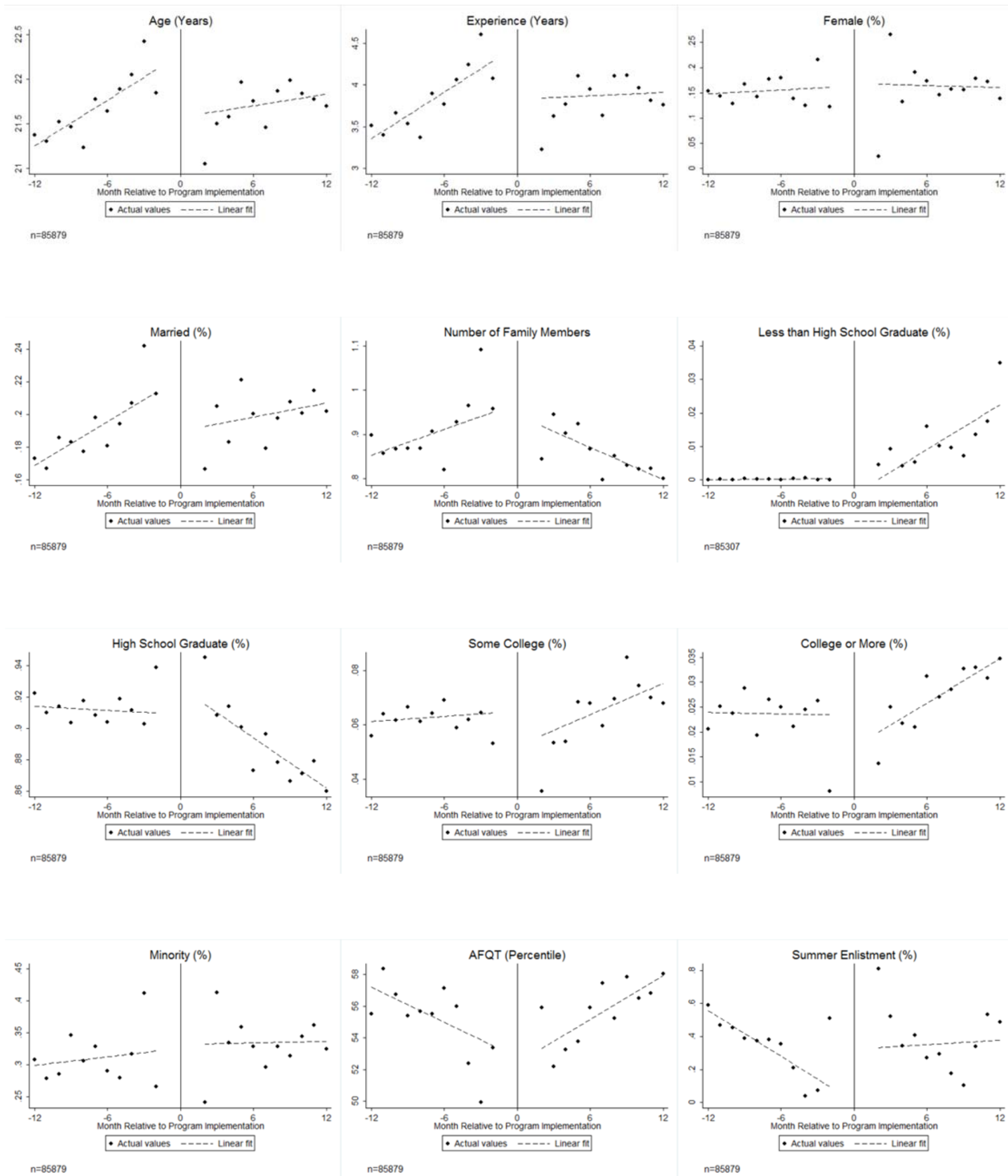
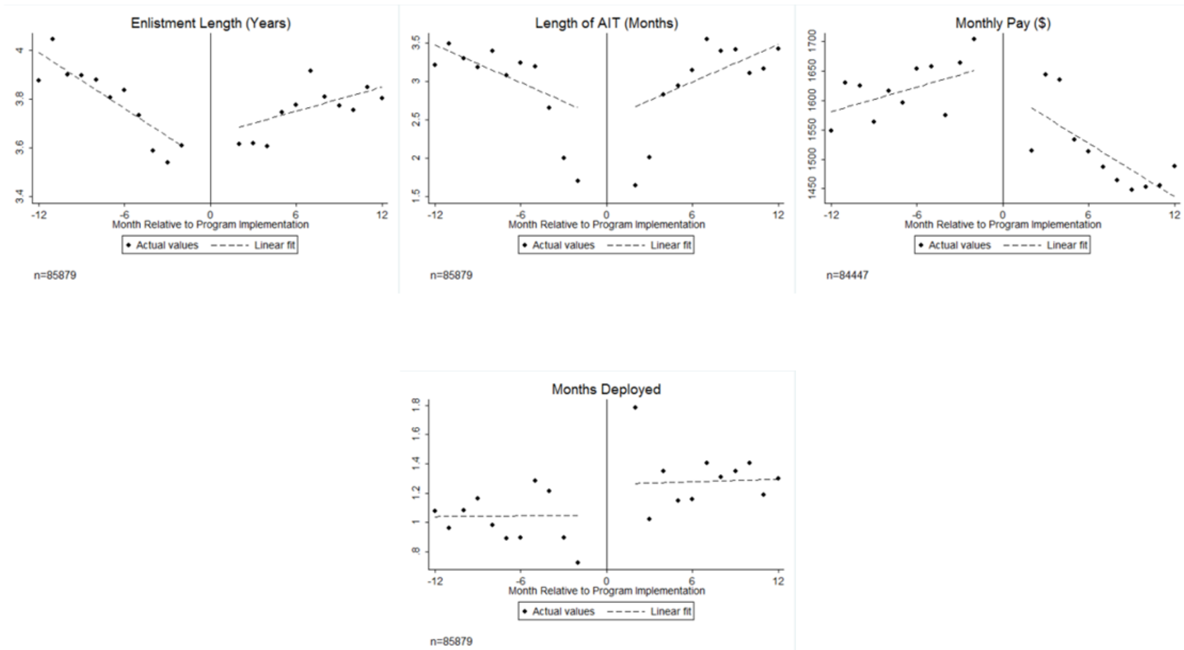


Figure 1.10. Event Studies for Covariate Balance in Year 1

Author compiled data using DOD and AER data for 2006-2009. These event studies provide a non-parametric summary of the average covariate value for each month cohort but do not account for time varying effects. The dashed lines depict an unweighted linear fit of the mean values for each month cohort. Sample sizes for each variable are listed on the graph below the y-axis.

Figure 1.10, Continued



1.7.C. Predicted Outcome Event Studies

To complete these event studies I regress the outcome of interest (Probability of TSP Participation or Average Monthly TSP Contribution) on the covariates listed in Table 1. (Panel B) but I omit the treatment variable from the regression. I then generate a predicted outcome for each individual and average these outcomes for all individuals in a given month cohort. These estimates capture the variation present in all covariates and should present the best prediction for the outcome variables that the observable characteristics can provide. If the program was implemented exogenously to individual characteristics then these plots should not reveal a significant change in the Predicted Probability of Participation and Predicted Average Monthly Contribution Levels at the time of program implementation. Figure 1.11 reveals two facts. First, the predicted TSP outcomes are actually lower for the treatment group than the control group, strongly suggesting that the results are not due to differences in observable characteristics. Second, the apparent discontinuities in the predicted outcomes are small (approximately $2 for the Average Monthly Contribution and 2% for the Probability of Participation) relative to the treatment magnitudes estimated above for the main RD specifications ($16.42 and 16.17% respectively).

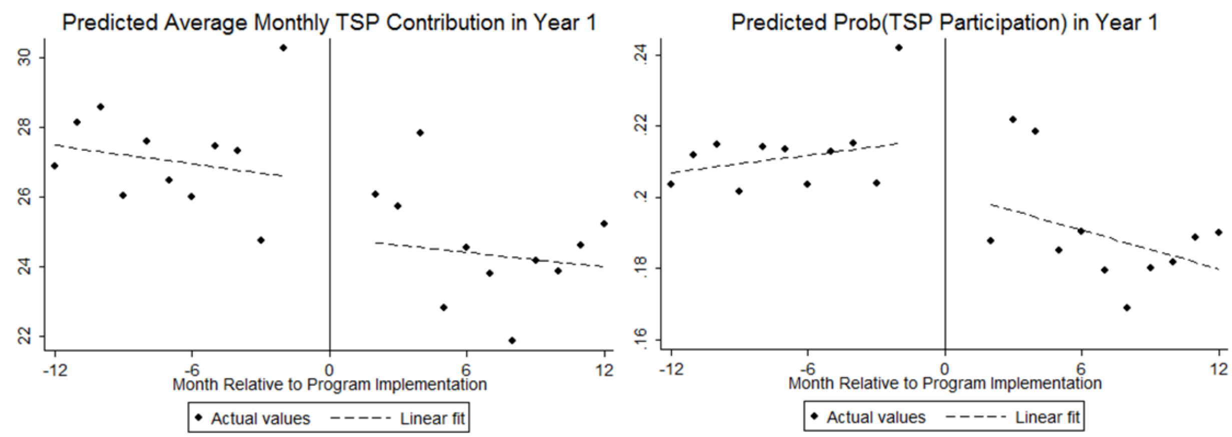


Figure 1.11. Event Studies for Predicted TSP Outcomes in Year 1
Author compiled data using DOD and AER data for 2006-2009. The left graph uses the Average Monthly TSP Contribution in Year 1 as an outcome; the right graph uses the Probability of TSP Participation in Year 1 as an outcome. The points reflect the mean predicted outcome at each cohort month from a regression using all covariates in Table 1 and omitting the RD framework, including treatment. The dashed lines depict an unweighted linear fit of the predicted values on each side of the discontinuity. n=85,879.

1.7.D. Regression Discontinuity Functional Form

As noted in Section 1.6.B the non-parametric event studies for the TSP outcomes of interest suggest that the use of a linear functional form is appropriate for this analysis. In addition, there is no reason to suggest that the outcomes here should have a polynomial relationship with the month cohorts used in the RD analysis. Nonetheless, in this section I complete robustness checks for alternate RD functional forms to ensure that the estimates are not the result of a particular functional form. That is, I vary the functional form of $f(R_i)$ in equation (2) above. In Table 1.6 I present the RD estimates by linear, quadratic and cubic functional forms and for several local linear specifications with varying bandwidths.[33] As the table reveals, the alternate functional form estimates are consistently large, statistically significant and always greater than the linear estimates for all specifications. In this regard the linear estimates presented above are likely conservative estimates of the treatment effects.

Table 1.6. RD Estimates of PFMC Effects on TSP Outcomes in Year 1, by Functional Form

	Linear		Quadratic		Cubic		Local Linear BW=4 Mo		Local Linear BW=6 Mo		Local Linear BW=8 Mo	
Form:	(1)		(2)		(3)		(4)		(5)		(6)	
Panel A: Average Monthly TSP Contribution												
PFMC Effect	16.38	***	19.35	**	36.32	***	35.14	***	39.30	***	31.24	***
	(3.75)		(7.50)		(12.76)		(9.90)		(13.14)		(5.33)	
Control Mean	15.23		15.23		15.23		15.23		15.23		15.23	
Adj R^2	0.1200		0.1200		0.1202		0.1522		0.1374		0.1318	
N	85,879		85,879		85,879		10,467		27,796		46,628	
Clusters	266		266		266		58		110		162	
Panel B: Probability (TSP Participation)												
PFMC Effect	16.15	***	18.52	***	28.98	***	23.72	***	24.98	***	20.33	***
	(2.70)		(5.76)		(9.61)		(7.44)		(9.52)		(4.23)	
Control Mean	11.31		11.31		11.31		11.31		11.31		11.31	
Adj R^2	0.1244		0.1244		0.1246		0.1721		0.1478		0.1361	
N	85,879		85,879		85,879		10,467		27,796		46,628	
Clusters	266		266		266		58		110		162	

Source: Department of Defense and Census Bureau Data. Notes: All data is for U.S. Army enlisted soldiers who enlisted between June 2006 and August 2009 and completed AIT at a given location within the 12 month period before and after program implementation, excluding the month before, after and of program implementation. The coefficient reported is for the discontinuity at the month of implementation. All regressions include a polynomial of the order specified by the column in the running variable and the interaction of the running variable and the discontinuity indicator. All regressions also include the following covariates: a quadratic in age, quadratic in experience, indicators for female, married, minority, number of dependents and a summer entry, indicators for education levels less than or equal to high school, some college, and greater than or equal to college (high school graduate is the omitted category), AFQT score, enlistment term, average monthly base pay, median household income in the individual's zip code of record, AIT length, number of months deployed in the year and fixed effects for an individuals' job, military branch, AIT location, and AIT start month. Indicator variables capture individuals missing pay or zip code income data and these individuals are assigned values of zero for these variables. Heteroskedasticity robust standard errors, clustered at the AIT location-month level, are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

1.7.E. Longitudinal Treatment Effects by Sample

[33] I have also completed the robustness checks for quartic functional form and the results hold.

The PFMC effects on monthly and cumulative monthly TSP contributions discussed in Section 1.6.E appear to show discontinuous declines in the PFMC effects between months 12 and 13.

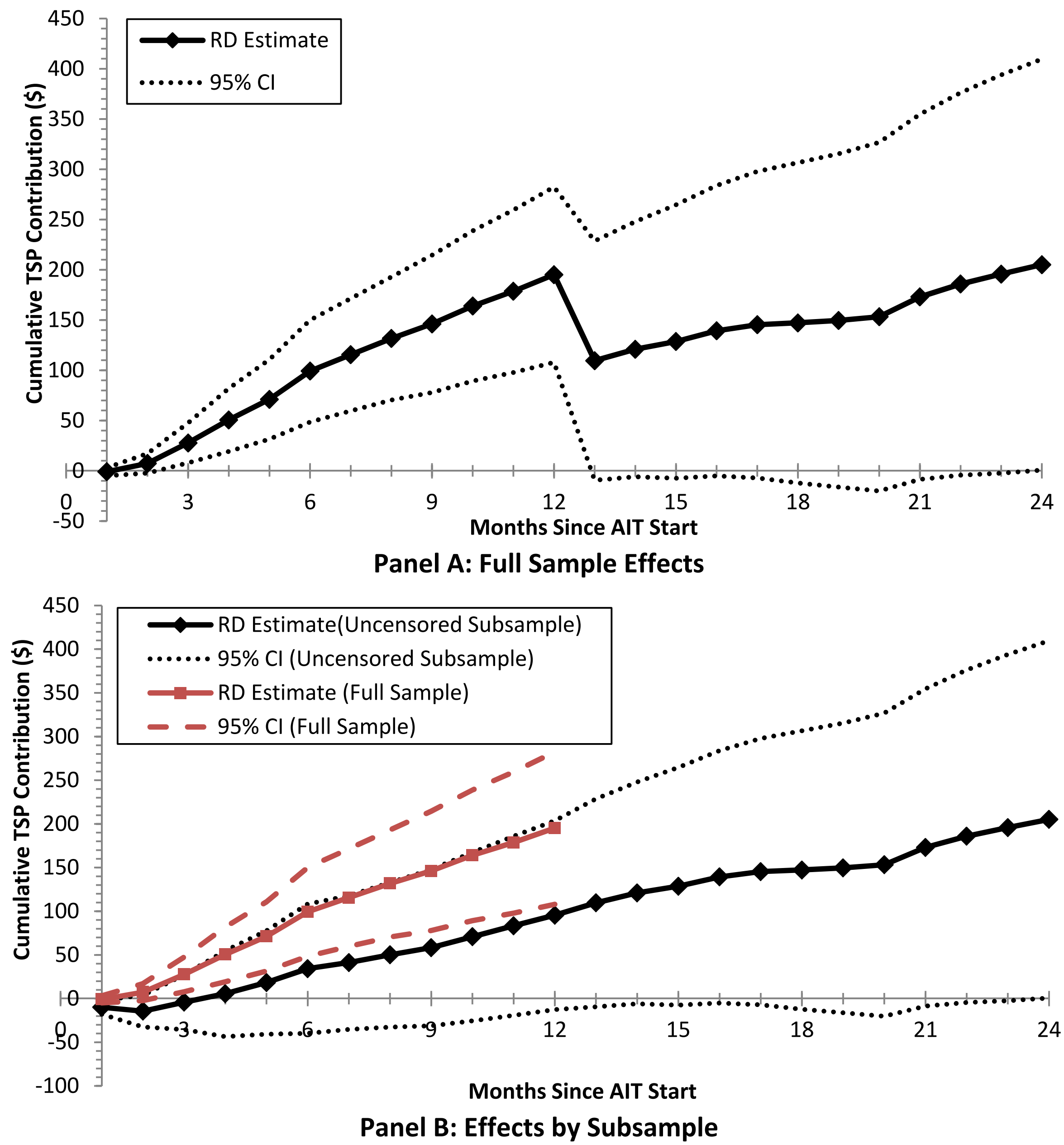


Figure 1.12. PFMC Effects on Monthly TSP Contributions in Years 1-2 by Subsample

Author compiled data using DOD and AER data for 2006-2009. Each point reflects the RD estimates of the PFMC effect on the TSP Contribution in the respective month. Panel A depicts the original plot (Figure 1.9) as a reference. In Panel B (lower graph), the black lines depict the RD estimates for the subsample for which observations are available for both years (n=64,017). The red lines depict the estimates for the full sample in Year 1. (n=85,879).

Since the administrative data is censored for individuals at this point in time the decreases may be the result of data availability and not actual reductions in the program's effects. To demonstrate that the decreases are due to data censoring, in Figure 1.12 I show the same longitudinal graphs as above but I plot the estimates separately for subsample that is censored in year two and the subsample that is not censored. As the graph show, the apparent reductions in the monthly and cumulative contributions appear to be the result of data censoring and not a reduction in the PFMC program effects. The lines depicting the effects for the uncensored subsample (for whom data is available in both years) are lower than for the full sample in year 1. In addition, these estimates smoothly transition to the year two estimates for the full sample (n=64,017) evident in the original plot, Figure 1.9 since this subsample is the full sample in year 2. Based on these plots, it is reasonable to assume that the estimates for the censored sample would similarly transition smoothly to higher monthly and cumulative monthly levels in year two.

1.8. EXTERNAL VALIDITY

This section briefly discusses the external validity of these findings. There are two primary threats to the external validity: differences in the nature of this course vs. other potential financial literacy courses and differences in the sample vs. other potential student populations.

With respect to the concerns over the nature of this course, the curriculum and the instructor population warrant attention. Concerns over the curriculum seem less significant in that, while nearly all financial education programs will vary, this program contains elements which at first glance seem both reasonable and appropriate. A basic understanding of the organization an individual works in, its pay system and knowledge of benefits and entitlements all seem appropriate for employer-designed courses. Courses offered to students in a more educational setting might omit these topics. Other topics such as budgeting, managing credit, making important purchases and retirement savings seem relevant to nearly all audiences. However, the external validity of the PFMC is concerning from a faculty perspective. The instructors in the course are typically retired military personnel and likely serve as role models for the students, thereby increasing their motivation to learn and to heed the teachers' advice.[34]

However, the key contribution of this work is establishing that financial literacy education does positively impact financial decision-making, at least in the short term. With this finding, additional research might focus on two related issues. First, there are no doubt potential improvements in the curriculum design, course content and teaching of financial literacy topics. Systematic examination of these approaches using experimental design holds promise for improving student and outcomes and program objectives. Second, more attention might be devoted to the difficult task of isolating the specific mechanisms through which this education works. Does this training instill knowledge that remains with the individuals and allows them to make better decisions? Does it increase their appreciation of the complexity of financial decisions and increase the time they devote to these topics, either through their own efforts or through seeking out help from others? Or, does the program simply provide a nudge to impressionable young adults at an opportune time in their life? Similarly, until the mechanisms are better understood, there is the possibility for overconfidence among trained individuals

[34] The comments on the nature of the instructors for the PFMC are based on author conversations with the Program Director at SDCC and direct communications with lead instructors at a number of locations.

that may adversely affect financial outcomes. These detailed questions will be difficult to answer but the growth of financial literacy education in the U.S. and other countries presents enormous opportunities for learning if program administrators and educators remain committed to scientific approaches in their implementation. This research provides a start point for this expanded understanding of retirement savings behavior by demonstrating that education can influence these behaviors and that the effects appear to operate on both margins.

The second concern with respect to external validity deals with the student population. At a minimum, the findings presented here seem applicable to all other members of the military or all individuals that have served as enlisted soldiers in the military, which makes the findings useful as support for continued financial literacy training for new military members and previous military members. As noted previously, this population is large and of independent interest for promoting financial literacy. However, there may be important differences in this population and the average population for which financial literacy might also be more relevant. Given that the students are new recruits they are generally in a mindset to receive instruction and are especially likely to be influenced by perceived leaders given their stage in the Army initial entry training program. In addition, given that these individuals are now typically living on their own and gaining their independence, the timing of the course might be uniquely suited for influencing individual financial behavior. Since the population is young they may not have developed bad habits yet and might be able to build better financial habits based on the course instruction. Finally, peer effects and/or role model effects may influence individuals in this population more than in the general population given the proximity of colleagues in military life. Even with these concerns though, the size and the diversity of the sample used in this research establish that financial literacy can have large positive effects on retirement savings decisions in the short term under these conditions. To generalize these results though, the course effects might be viewed as somewhat larger than could be typically expected among civilian student populations.

1.9. SUMMARY

This paper exploits a natural experiment that occurred in the U.S. Army to estimate the effects of financial education on retirement savings. Between 2007 and 2008 the Army implemented a mandatory 8 hour Personal Financial Management Course for new soldiers. The staggered implementation across locations and time provides quasi-experimental variation in whether an individual received the training. Using event studies and regression discontinuity techniques, I find that the course had large and lasting effects on individual retirement savings in the Thrift Savings Plan, a tax-deferred account available to uniformed service members. The course doubles retirement savings with an estimated effect of \$16.38 on a control mean of \$15.23 for the Average Monthly TSP Contribution in Year 1. In addition, the course also has positive and significant effects on saving levels throughout the distribution, with statistically significant effects at all savings levels from \$0 per month (p=0.000) where the course increases the probability of contributing by 15.15% through the 95th percentile of \$200 per month (p=0.02) where the course increases the probability of contributing is 1.85%. Finally, the course has persistent effects on TSP contributions, increasing monthly contributions through at least twenty four months. The mechanism for these treatment effects is likely a combination of both human capital and behavioral assistance since the course provides traditional education and assistance in program enrollment. Unfortunately, these effects cannot be separately identified and measured in this data. Nonetheless, this research marks one of the first experimental findings of large and lasting effects from financial education. These findings provide initial evidence to economists and policy makers on the causal effects of such education and motivate additional research on the improvement of the treatment effects and the identification of the mechanisms for behavioral change.

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Appendix 1.1
Sample Selection and Treatment Variable Imputation

Step 1. I define the full sample of interest as Active Duty U.S. Army enlistees in selected Military Occupation Specialties (MOSs) who started AIT at one of 13 locations (see Figure 1) within 12 months of the Personal Financial Management Course (PFMC) program implementation at their respective. Given that the Army's recruiting and training pipelines do not change significantly in such a period, this restriction creates reasonably comparable control and treatment groups, conditional on time fixed effects. This method enables the pooling of individuals across locations by common month cohorts relative to program implementation and the use of regression discontinuity techniques. The sample is thus initially defined as individuals in month cohorts [-12, 12] and numbers 104,393.[35]

Step 2. Second I impute the treatment variable. Since micro data on attendance and completion of the financial management course are not available, the imputation of treatment variable ($PFMC_i$) status relies on Army administrative data on unit assignment, data on locations and knowledge of the duration of training requirements before Advanced Individual Training (AIT). The rules below describe in detail the imputation process for the Personal Financial Management Course variable.

2.1. The first sub-objective of the second step is to establish the month that an individual started AIT. The Personal Financial Management Course is completed in Advanced Individual Training (AIT), which individuals complete immediately following Basic Combat Training (BCT).[36] AIT durations vary from 4-52 weeks and BCT is 9-10 weeks long. For some

[35] I elected to submit a reduced sample to the credit bureau based on the cost of the data. I submitted a 50% sample (n=47,798) of the originally constructed administrative sample (n=96,689, based on Steps 1-3.B.). After submission of this sample to the credit bureau the full sample was adjusted based on two changes to the sample (one involved use of incorrect PFMC program implementation dates at 3 locations and the other involved the need to remove some individuals from the treatment group as described here in Step 3.C.). The first change has a potential effect on the estimates by preventing some individuals at these three locations in affected month cohorts from being included in the sample submitted for matching. The estimated effect of this change is small (n~1,000 matched individuals, balanced across the control and treatment groups for each location). The second change has no effects on any estimates except that it reduces the sample size of the matched sample. The corrected full sample size for the full administrative data for cohorts [-12,-2] and [2,12], which includes all locations except the pilot location at Fort Bliss, TX, is n=85,879. The result is that some matched records were unusable (n=5,297, about 11% of the submitted sample). However, in the final matched sample employed in this analysis, there is no bias in AIT lengths or any other observable characteristics and all program implementation dates are correct. In addition, given the relatively small numbers affected by this error and the mixed effects of the omissions, there does not appear to be any resulting bias in my estimates. I conducted robustness checks by omitting all individuals from the 3 affected locations and found similar results, (for example, the main results for the Average Monthly TSP Contribution in Year 1 are Coeff=$16.38, Std Err=3.75, p=0.000 and the reduced sample estimates are Coeff=$19.35, Std Err=4.27, p=0.000).

[36] A small minority of Army jobs require attendance at other schools (often language school) prior to completing AIT. These observations are omitted from the sample as they are atypical of Army enlistees and their treatment status is indeterminate.

military jobs BCT and AIT are completed together as part of a program called One Source Unit Training (OSUT) where new recruits complete BCT and AIT with the same set of instructors and soldiers. Finally, a small minority of Army jobs require AIT at locations not covered by the Personal Financial Management Course studied here.[37]

A. Individuals who change both units and locations in the same month after entry into the Army are assigned an AIT start month based on this common month. (Approximately 64% of the sample was assigned based on this criterion)

B. Unassigned individuals who have unit assignment data but do not have common location data are assigned an AIT start month based on the unit assignment data. (Approximately 23% of the sample assigned based on this criterion)

C. The remaining unassigned individuals who are missing unit data or who do not change units are assigned their AIT start month based on their location change. (Approximately 0.01% of the sample assigned based on this criterion)

D. The remaining unassigned individuals who are missing unit and location data are assigned their AIT start month based on the scheduled duration of BCT (9-10 weeks=2.5 months). Their start date is assigned as their entry month +3 months. (12.79% of the sample had the AIT commencement month adjusted based on this criterion)

E. Finally, individuals who have Army jobs that requires OSUT have their AIT start month adjusted down by one month. For these individuals the unit change and/or location change marks completion of BCT and AIT. But these individuals started the AIT portion of OSUT one month prior to their OSUT completion. (26.7% of the sample was affected by this adjustment)

2.2. The second sub-objective of this step is to assign a treatment status based on an AIT start month. After the five steps above are completed each individual has an AIT start month. This variable serves as the basis for assignment to the control or treatment group. I use the dates of program implementation provided by the program agency, Army Emergency Relief (AER), to assign individuals their control or treatment status. The AER data, reflected in Figure 1, identifies the start month of the training at each of the Army's 13 AIT locations (12 used in this sample, which excludes the pilot at Fort Bliss, TX in January 2003). Individuals who completed AIT at a location prior to program implementation at that location are assigned to the control group ($PFMC_i = 0$). Individuals who started AIT during or after program implementation at that location are assigned to the treatment group ($PFMC_i = 1$).

[37] Some examples are Special Forces (AIT at Fort Bragg, NC) and firefighters (Goodfellow Air Force Base, TX). I omit these individuals from the sample as they are also atypical of Army enlistees, they are few in number (les that 2% of sample), and they do not experience treatment, preventing an experimental comparison.

Step 3. I refine and edit the sample to clarify the differences between the control and treatment groups and avoid contamination issues. After the assignment process of Step 2.2, every individual has a value of 0 or 1 assigned to the variable $PFMC_i$. To further avoid any contamination of control and treatment group members I adjust the sample in three ways.

A. First, I omit from the sample individuals who started AIT in the month preceding, month of and month following program implementation at a given location (month cohorts -1, 0 and 1). This removes individuals from the sample for whom treatment status is the most uncertain and the potential for contamination is high.

B. Second, I omit individuals whose AIT start month and AIT length result in an overlap with the month of program implementation (e.g., an individual with an AIT length≥2 months who started AIT at a location 2 months prior to program implementation [month cohort -2]).

C. Third, to avoid a systematic bias in the selection of individuals with longer AIT lengths in the treatment group based on this second rule, I remove individuals from the treatment group to parallel the removal of the control group individuals described above (e.g., I remove individuals from the treatment group in the second month after program implementation [month cohort 2] who have an AIT length ≥2 months). The second and third rules can be summarized together as omitting any individual whose AIT length is greater than or equal to the absolute value of their month cohort.[38]

After these adjustments the full administrative data sample contains n=85,879 observations.

[38] Note that the length of an individual's AIT course is a covariate of interest included in all regression specifications. The assignment rule specified here effectively removes any bias between control and treatment groups related to AIT lengths. For evidence refer to the randomization test completed in the Robustness Checks section. The coefficient on the discontinuity in AIT length variable regression is statistically insignificant (p=0.8564).

Appendix 1.2
Complete Regression Results for PFMC Effects on Average Monthly TSP Contribution in Year 1

Table 1.7. Complete RD Estimates of PFMC Effects on Average Monthly TSP Contribution in Year 1

Variable	Coeff	(Std Err)	p-value	Sig
Panel A: Parameter Estimates				
PFMC Effect (Discontinuity)	16.38	(3.75)	0.000	***
Cohort Month	0.17	(0.58)	0.772	
PFMC*Cohort Month	-1.30	(0.51)	0.011	**
Age	4.07	(1.23)	0.001	***
Age2	-0.07	(0.02)	0.001	***
Experience	-1.35	(0.57)	0.018	**
Experience2	0.07	(0.02)	0.001	***
Female	-1.66	(0.77)	0.032	**
Married	-0.70	(0.63)	0.271	
Number of Dependents	-1.54	(0.22)	0.000	***
Education ≤ High School Graduate, %	0.39	(2.61)	0.882	
Education = Some College, %	1.56	(1.28)	0.225	
Education ≥ College Graduate, %	7.70	(2.51)	0.002	***
Minority, %	2.85	(0.45)	0.000	***
AFQT Score, Percentile	0.18	(0.02)	0.000	***
Enlistment Term Length, Years	16.75	(1.26)	0.000	***
Monthly Basic Pay, $	0.02	(0.00)	0.000	***
Socioeconomic Status	0.0000	(0.0000)	0.071	*
Months Deployed	1.50	(0.16)	0.000	***
Summer enlistment indicator	-2.55	(1.42)	0.075	*
Missing Pay Indicator	8.35	(2.04)	0.000	***
Missing HH Income Indicator	-1.32	(1.71)	0.443	
AIT Length, Months	21.52	(23.09)	0.352	
Constant	-169.47	(46.60)	0.000	***
Panel B: Regression Statistics				
Observations	85,879		Adjusted R^2	0.1200
Clusters	257		F-Statistic	28.23

Source: Department of Defense, Census Bureau and Credit Bureau Data. Notes: Sample as described in Table 1.2. The table reports the regression coefficients for all parameters except the fixed effects, specifically, time (month) fixed effects, location fixed effects, job category (branch) fixed effects and job specific fixed effects. The omitted category for education is high school graduate. Heteroskedasticity robust standard errors, clustered at the AIT location-month level, are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

CHAPTER 2

ESTIMATING THE EFFECTS OF FINANCIAL EDUCATION ON FINANCIAL BEHAVIOR: EVIDENCE FROM THE U.S. ARMY'S PERSONAL FINANCIAL MANAGEMENT COURSE

Estimating the Effects of Financial Education on Financial Behavior: Evidence from the U.S. Army's Personal Financial Management Course

May 2012

ABSTRACT

This paper estimates the effects of financial education on economic behavior. I employ a natural experiment in the U.S. Army that implemented a mandatory 8 hour personal financial management course (PFMC) in a quasi-experimental manner. Previous research has shown that this course has large, pervasive and persistent effects on retirement savings but the mechanism for these effects is unclear. In this paper I use administrative data and individually matched credit data to estimate the effects of financial education on a variety of financial outcomes including credit scores, credit balances for several types of accounts, monthly payments and adverse legal actions. In some areas I find that the PFMC has positive effects, reducing cumulative account balances (especially for automobile and credit card accounts) and aggregate monthly payments. In other areas, including credit scores, the probability of being active in the credit market and the number of adverse legal actions, the PFMC has no statistically significant effects on financial behavior.

William Skimmyhorn
Harvard Kennedy School
79 JFK Street
Cambridge, MA 02138
bill_skimmyhorn@hksphd.harvard.edu

2.1. INTRODUCTION

This paper estimates the effects of financial education on economic behavior. I employ a natural experiment in the U.S. Army that implemented a mandatory 8 hour personal financial management course (PFMC) in a quasi-experimental manner. Previous research has shown that this course has large, pervasive and persistent effects on retirement savings but the mechanism for these effects is unclear. In this paper I use administrative data and individually matched credit data to estimate the effects of financial education on a variety of financial outcomes including credit scores, credit balances for several types of accounts, monthly payments and adverse legal actions. In some areas I find that the PFMC has positive effects, reducing cumulative account balances (especially for automobile and credit card accounts) and aggregate monthly payments. In other areas, including credit scores, the probability of being active in the credit market and the number of adverse legal actions, the PFMC has no statistically significant effects on financial behavior.

Since this research uses the same natural experiment and much of the same data as the previous chapter, many sections of this paper will be brief and refer the reader to the previous chapter for more detail. The paper proceeds as follows: Section 2.2 reviews the existing literature and summarizes the contributions of this work. Section 2.3 summarizes the financial management course and program implementation. Section 2.4 discusses the relevant theory for this analysis. Section 2..5 summarizes the variables and data used in the empirical analysis. Section 2.6 presents the empirical analysis and discusses the results. Section 2.7 conducts robustness checks. Section 2.8 discusses external validity. Section 1.9 summarizes the findings, presents brief policy recommendations and concludes.

2.2. LITERATURE REVIEW & CONTRIBUTIONS

Section 1.2 thoroughly reviews the relevant literature and extant findings for financial education. Taken together, there is little experimental evidence of beneficial effects of financial education and a host of zero or small findings even in endogenous settings.

One additional contribution of this work, in addition to those described in Chapter 2, is the use of detailed credit bureau data to analyze financial behavior. While this data has existed for a number of years (see Avery et al. 2003 for a summary) and a growing number of economists are turning to credit data as a rich source of information on financial behavior (e.g., Agarwal et al. 2009 and Finkelstein et al. forthcoming), little of the financial education research has used this detailed data. One important exception is Staten et al. 2006, who used credit histories to evaluate the effects of targeted credit counseling. They find that credit counseling improves credit behavior, albeit in a self-selected group. Given the general concerns with self-reported data described in Section 1.2 and the potential for financial education to impact a variety of financial behaviors, credit data seems particularly useful in this context.

2.3. PERSONAL FINANCIAL MANAGEMENT COURSE PROGRAM IMPLEMENTATION

Section 1.3 reviews the implementation of the PFMC and the content of the course. As Figure 1.2 reveals, several elements of the PFMC might impact an individual's financial behavior as measured by credit data. First and most directly, there is an entire one hour lesson entitled "The Essentials of Credit," that surveys the types of credit, advises students about the factors affecting creditworthiness, reviews the role of debt and discusses proper credit usage, credit and debt assistance, consumer protection laws and credit reporting. In addition to this lesson, since the course also covers Car Buying, Consumer Awareness and Spending Plans (each one hour lessons), the credit data should serve as a useful measure of the diverse and potentially wide-ranging effects of the course.

2.4. THEORETICAL MODEL & HYPOTHESES

The theoretical model and estimation strategy remains the same as in Chapter 1. I provide reduced form estimates of the effects of PFMC on selected financial behaviors. Based on a review of the PFMC curriculum I provide the following additional hypotheses:

Hypothesis 2: Participation will reduce the cumulative debt for individuals.

- **Hypothesis 2a: Participation will reduce the size of auto loans for individuals.**
- **Hypothesis 2b: Participation will reduce the amount of credit card debt for individuals.**
- **Hypothesis 2c: Participation will reduce the amount of finance debt for individuals.**
- **Hypothesis 2d: Participation will reduce the amount of unpaid debt for individuals.**

Hypothesis 3: Participation will reduce the incidence of negative financial outcomes (liens, foreclosures, judgments, repossessions and collections) for individuals.

Hypothesis 4: Participation will reduce the aggregate monthly payment for individuals.

Given the uncertainty over the use of credit score as a measurement of financial literacy provided in Section 2.4, I do not provide a hypothesis for this outcome. Similarly I do not have a hypothesis for the effects of the PFMC on the probability of having active credit conditional on having a matched credit record. Instead, for these two outcomes, this research provides empirical estimates of the relationship between financial literacy and credit activity and financial literacy and credit bureau scoring procedures.

2.5. SUMMARY OF VARIABLES AND DATA SOURCES

This research uses military administrative data as described in Section 1.4 and this data affords a significant amount of information about individual demographics, education levels, pay, work and home locations. While the outcome data for the retirement savings analysis in Chapter 2 also relied on administrative data, this research exploits individually matched credit data from one of the three national credit bureaus for a random subsample of the full sample employed in Chapter 1.[1]

I selected the evaluation outcomes based on three criteria. First, the outcomes relate directly or indirectly to the PFMC topics and implicitly, financial behaviors of interest to the program administrators. Second, the outcomes capture a large portion of the economic activity of most individuals. In Figure 2.1 I present a comparison of the expected coverage of economic activity by categories for my sample and a nationally representative subsample of individuals analyzed by Avery et al. in 2003. Unfortunately, there is little data available on demographically

[1] For more information on matching results, see Appendix 2.1. For additional information on the sample selection, see Appendix 1.1.

comparable groups to this sample. As a result, since my sample is not nationally representative (they are among other features, younger, less female and less married on average), their financial behavior and debt profiles might be expected to differ from the national averages in a few ways (e.g., fewer mortgages, less overall borrowing). Nonetheless, this table shows the national averages based on both the number of accounts (columns 1 and 3) and the dollar weighted value of accounts (columns 2 and 4) and serve as a reasonable first approximation. While the balances and distributions of financial behavior in Table 2.2 should not be taken literally, they should provide confirmation that the selected attributes provide visibility on a significant portion of individual and household economic behavior.

Table 2.1. Credit Accounts and Balances, by Type of Account

		Share of all open accounts			
		National Averages (2003)		Estimates for Current Data	
		#	$ Wtd	#	$ Wtd
Account Type	**Definition and/or examples**	**(1)**	**(2)**	**(3)**	**(4)**
Open End	Repeated discretionary borrowing with a limit.	78.5%	15.3%	69.8%	9.8%
Non-Revolving	Borrowing for a short period with full repayment.	4.2%	4.0%	-	-
Revolving	Unsecured accts with flexible borrowing amounts.	74.3%	11.3%	69.8%	9.8%
Bank Card	VISA, Capital One, Discover, Citibank, some Amex	37.0%	8.4%	37.0%	8.4%
Retail	Macy's, Sears, Target	28.1%	0.5%	28.1%	0.5%
Finance	Sales Fin. Co. Loan or CC, pers. loans, credit union	4.7%	0.9%	4.7%	0.9%
Check Credit	Overdraft accounts, personal lines of credit	1.9%	1.4%	-	-
Other	National oil & gas, government entities, utilities	2.5%	0.1%	-	-
Closed End	Lump sum loans repaid on a schedule.	21.6%	84.9%	10.6%	76.9%
Mortgage	Special installment accts secured with real estate.	5.0%	66.5%	5.0%	66.5%
Installment	Fixed payments that fully amortize the loan amount.	16.6%	18.4%	5.6%	10.4%
Automobile	From banks, credit unions or other finance co.	4.6%	7.8%	4.6%	7.8%
Banking	Short term loans from banks.	3.2%	1.4%	-	-
Finance	Short term loans from finance companies.	1.0%	2.6%	1.0%	2.6%
Other	Recreational vehicles & equipment, household items	7.8%	6.6%	-	-
Total		**100.1%**	**100.2%**	**80.4%**	**86.7%**

Source: Avery et al. 2003. The account totals above exclude accounts in a major derogatory status and those in dispute. Notes: Percentages within categories do not add perfectly due to rounding. The estimates for the percentages of credit balances observed in columns 3 and 4 assume that the national balance averages by account type are the same for my sample as the nationally representative sample in 2003.

Third, I tried to select and evaluate outcomes that can separately identify the effects of the PFMC through human financial capital. Since the large effects documented in Chapter 2 reflect the combined effects of human capital and behavioral assistance, one of the principle goals of this analysis is to evaluate behavior in domains where the effects can be attributed to the creation of improved human capital.

Since the matched credit data is organized by archive dates (April of each year in 2005 through 2011) but my analysis requires comparison of control and treatment group individuals at comparable time horizons, I convert the archive dates to the relevant analysis horizons (e.g., year 1 and year 2).[2] In addition, I observe the majority of individuals prior to their entry into the Army (year 0) and will use credit attributes from this "baseline" year, including the credit score and the outcome variables as additional controls in my analyses.[3]

Next I review the data matching process, which generates several potential outcomes.[4] Records are either matched or attrited and then the matched records are either active (valid and present balances and scores) or inactive (no balances and scores due to lack of activity). The first outcome is important for experimental validity and the second outcome is a meaningful measure of the financial behavior. These are discussed and evaluated below in Section 2.6.H and Section 2.6.C respectively.

[2] In my analysis, year 0 consists of the 12 months preceding an individual's entry to the Army. Year 1 consists of the 12 months beginning with AIT and year 2 consists of the 12 months beginning with the month 13 after beginning AIT and ending at month 24.

[3] For individuals missing year 0 data but matched in year 1 or year 2 I will assign them an indicator variable for missing data and impute a zero for the outcome variable in year 0.

[4] See Appendix 2.1 for more details on the specific match statistics.

Here I briefly summarize the match rate statistics for my year 1 and year 2 samples. Of my n=41,303 submitted records, I match n=33,931 (82.2%) records for year 1 and n=33,754 (81.7%) records for year 2. Of those that are matched, n=29,318 (86.4%) have active credit records for year 1 and n=31,196 (94.2%) for have active credit records for year 2. I will test the credit activity outcome below.

In addition, given that the inactive records are matched, I can reliably impute zeros for balances and trade counts for the matched records. The reason is that a matched record reflects unique identification of an individual by the credit bureau from reporting institutions (bank, credit card, credit union, etc…) and these institutions have an incentive to not only know the account types, balances and statuses for active accounts but also to report these accounts to the credit bureau. A matched but inactive record implies balances of zero for a given individual. Robustness checks in Section 2.7 validate this assumption.

As with the previous research, the recent implementation of this program limits the analysis horizon to short term financial behavior. In addition, the censoring of some individuals in my year 2 administrative data means that the actual sample sizes will differ from the match statistics above. My adjusted sample sizes are n=33,931 for year 1 and n=31,104 for year 2. As a result, I focus primarily on year 1 outcomes to maximize statistical power. In contrast to the previous research on TSP outcomes, in which I observe individuals in every month each year, in this analysis I only observe each individual once per year in April. As a result, an individual's "Year 1" outcome is a one-time monthly snapshot of that individual during their first year and the month of observation depends on the relationship between the individual's AIT commencement month and April of the year. This means that while year 1 observations vary between month 1 and month 12, on average I observe individuals at their six and a half month since beginning AIT.

This research will focus initially on five outcomes: the probability of having active credit (conditional on being matched), cumulative balances, aggregate monthly payments, an adverse legal action index and an individual's credit score.

First I evaluate the probability of having active credit, conditional on having a matched credit record. This outcome reflects extensive margin participation in the credit market. For this outcome there is no clear prediction for the effects of the PFMC. The course promotes responsible credit use and generally encourages participation in the credit market, subject to accumulating small balances, paying more than the minimum payment and restricting the use of credit by the type of purchase (routine vs. large purchases). Since financially illiterate individuals are likely users of credit, we expect them to have active credit conditional on being matched. But since the PFMC is not anti-credit in its curriculum, even if students gain financial literacy, there is no reason to assume that they will participate less frequently in the market.

Second, since the primary financial behaviors of interest are those that reflect the levels, margins and types of credit used, I generate a variable that reflects financial behavior in a variety of areas. I generate the outcome titles cumulative balance, that represents the total balance that individual i has in the following areas: automobile credit (loans and leases), credit cards (bankcards and retail credit), finance trades (short term loans) and unpaid balances (collections and charge offs).[5] [6] The cumulative balance is not comprehensive and omits some types of accounts (e.g., mortgages) but captures a host of important and routine economic transactions. I evaluate the cumulative balances and the components of this balance to determine the effects of the PFMC on a variety of important behaviors.

Third, since the PFMC aims to reduce the incidence of very poor financial behavior by individuals I will evaluate the PFMC effects on behaviors that results in a legal action or decision against an individual. Here I generate an index of adverse legal actions that is the total of the following actions: liens, foreclosures, repossessions and judgments. While these incidents are relatively rare, they are extremely adverse to an individual or household's current and future financial standing and represent an important objective of the PFMC.

Fourth, I will evaluate the effects of the PFMC on an individual's aggregate monthly payment. This outcome represents total of all of the scheduled payments for all trades (accounts) with outstanding balances. The trades involved in this outcome subsume those in the cumulative balance outcome above and include other items like mortgages and personal lines of credit. For closed-end credit (e.g., an auto loan or a mortgage), the monthly payment is typically constant and amortizes the value of the debt throughout the loan duration. For open-

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[5] Finance loans include loans provided by banks, credit unions and other financing companies. They generally do not include payday loans. While payday loans may occasionally be captured in credit data (e.g., if a payday lending company is also a bank (say, Wells Fargo) and this institution is reporting on an individual), payday lenders generally do not report to credit bureaus. These facts are derived from conversations with credit bureau personnel in Fall 2011 and other large financial institution representatives in Spring 2012.

[6] In the credit data, a collection refers to an unpaid balance that has been referred to an external company for processing and collection or to an institution's own agency for the same purpose. Such balances are rarely repaid in full, adversely affect credit ratings and may be subject to legal actions. A charge off is similar in that it reflects an unpaid balance and adversely affects an individual's credit rating. These balances have typically been "written off" by the reporting institution but they may still be referred to collection or legal action at a future date.

end credit, individual balances vary by month and the monthly payment is a function of the balance, the interest rate and the agreed upon terms for minimum payments. As a result of these differences, the aggregate monthly payment reflects a weighted combination of open end and closed end credit and is a monotonically increasing function of an individual's total debt. Since this variable reveals the minimum payment in a given month it provides some visibility into the cash flow patterns for individuals and households.

Finally, I evaluate the effects of the PFMC on an individual's credit score. While credit scores are generally associated with access to better credit and better credit rates, whether or not the score correlates directly with financial literacy is less clear. The scores are an industry measure used to guide lending decisions, which are driven both by risk and by profitability. The scores are proprietary but credit bureaus acknowledge that scores are a function of credit use, the number of trades (accounts) and payment history, among other factors. If more literate consumers use credit less or open fewer trades their scores might decline relative to the less literate. Conversely, if more literate consumers use credit more responsibly and have better repayment patterns, their scores may be higher relative to the less literate. As a result, it is not clear that a more financially literate individual will necessarily have a higher credit score. As a result, the individual's credit score will be used as an outcome of interest but there is no clear theoretical prediction for the effects of the PFMC on the score.

2.6. EMPIRICAL ESTIMATION

2.6.A. Summary Statistics

In Table 2.2 I present the summary statistics for my principle outcomes and covariates for the full sample and by treatment status. While there are apparent outcome variable differences in Table 2.2, these observed patterns are inadequate for causal inference given the other demographic differences evident in Panel B that could explain the outcome patterns and their omission of other time-varying effects. As a result I will proceed with event studies and multivariate regression estimates below.

Table 2.2. Summary Statistics by Treatment Condition for Administrative Data Sample

	(1)		(2)		(3)	
	Full Sample		**No Training**		**Received Training**	
	N=33,931		*N=17,053*		*N=16,878*	
Variable	**Mean**	**(Std Dev)**	**Mean**	**(Std Dev)**	**Mean**	**(Std Dev)**
Panel A. Outcomes						
Prob (Active Credit), %	86.40	(34.27)	86.67	(34.00)	86.14	(34.55)
Cumulative Balance, $	6,081	(8881)	6,117	(8887)	6,044	(8875)
Adverse Legal Action Index	0.24	(1.50)	0.19	(1.20)	0.28	(1.76)
Aggregate Monthly Payment, $	183	(254)	179	(249)	187	(259)
Credit Score	578	(91.3)	577	(90.2)	578	(92.4)
Panel B. Individual Characteristics						
Age, years	21.80	(4.09)	21.70	(4.04)	21.91	(4.14)
Experience, years	3.90	(3.94)	3.82	(3.91)	3.98	(3.96)
Female, %	0.12	(0.33)	0.12	(0.32)	0.13	(0.33)
Married, %	0.20	(0.40)	0.19	(0.39)	0.20	(0.40)
Number of dependents	0.91	(1.21)	0.94	(1.23)	0.88	(1.20)
Less than high school education, %	0.01	(0.08)	0.00	(0.01)	0.01	(0.12)
High school graduate, %	0.90	(0.30)	0.91	(0.28)	0.89	(0.32)
Some college, %	0.07	(0.25)	0.06	(0.24)	0.07	(0.26)
College graduate or more, %	0.03	(0.16)	0.02	(0.15)	0.03	(0.17)
Minority, %	0.30	(0.46)	0.29	(0.45)	0.32	(0.47)
AFQT Score, percentile	56.66	(19.13)	56.20	(19.26)	57.12	(19.00)
Summer accession, %	0.35	(0.48)	0.37	(0.48)	0.34	(0.47)
Enlistment term, years	3.81	(0.99)	3.85	(0.98)	3.78	(0.99)
AIT length, months	3.17	(1.11)	3.17	(1.11)	3.17	(1.10)
Monthly basic pay, $	1,551	(304)	1,605	(325)	1,496	(270)
Median HH Income in Zip Code, $	42,138	(13,804)	42,204	(13,851)	41,425	(13,272)
Months deployed during the year	1.19	(2.33)	1.06	(2.17)	1.33	(2.48)
Credit Score in Year 0	554	(106.3)	555	(104.9)	553	(107.6)

Table 2.2, Continued

Source: Department of Defense and Census Bureau Data. Notes: All data is for U.S. Army enlisted soldiers who enlisted between June 2006 and August 2009 and completed AIT at a given location within the 12 month period before and after program implementation, excluding the month before, of and after program implementation and who had a matched credit record in year 1 (n=33,931). All outcome variables (Panel A) in this table are measured for the period beginning during Advanced Individual Training and ending 12 months later. Experience is an approximate measure of labor force experience at the time of enlistment and is calculated using age minus education minus 6 years. For this calculation, education is imputed using the following values: 10 years for high school dropouts; 11 years for GED holders; 12 years for high school graduates; 13 years for some college; 14 years for associate's degrees; 16 years for college graduates; 18 years for post graduate. The less than high school graduate variable includes dropouts and GED holders. The some college variable includes those with an Associate's Degree. The greater than or equal to college graduate variable includes those with Bachelor's, Master's and Doctorate degrees. The married variable represents formal and common law marriages for anyone who has ever been married. Average monthly pay represents the average monthly base pay during the 12 month period. The enlistment term variable represents the length of service that an individual has agreed to serve upon joining the military or reenlisting and typically varies from 2-6 years and the enlistment term during the 12 month observation period is used. The median household income data reflects the median household income from the 2000 U.S. Census (Sample File 3) for those individuals not missing zip code data. Sample sizes apply to all variables with the following exceptions: the monthly pay data is restricted to the individuals for whom this data was not missing (n=33,344); the median household income is restricted to the individuals for whom this data was not missing (n=14,325); the year 0 credit score is restricted to those with credit data in year 0 (n=18,947).

In addition, since the distributions for several of the outcome variables are heavily influenced the presence of zeros, in Table 2.3 I present the summary statistics for the outcome variables by unconditional and conditional levels for the full sample. I also include the elements of the cumulative balance variable in this table.

Table 2.3. Selected Summary Statistics for Credit Outcomes in Year 1

	Unconditional		% at	Conditional on Y>0	
Variable (Y)	Mean	(Std Dev)	Zero	Mean	(Std Dev)
Prob (Active Credit), %	86.40	(34.27)	13.60	1	(0.0)
Cumulative Balance, $	6,081	(8,881)	18.66	7,472	(9,302)
Auto Loan Balance, $	2,759	(6,008)	78.33	12,731	(6,292)
Credit Card Balance, $	767	(1,632)	53.37	1,644	(2,066)
Finance Trade Balance, $	356	(1,606)	88.49	3,098	(3,733)
Delinquent Balance, $	1,986	(4,635)	51.70	4,112	(5,977)
Adverse Legal Action Index	0.24	(1.50)	94.56	4.35	(4.87)
Aggregate Monthly Payment, $	183	(254)	34.82	280	(267)
Credit Score	578	(91.3)	-	578	(91.3)

Source: Department of Defense and Census Bureau Data. Notes: See Table 2.2 for sample and variable details.

2.6.B. Event Studies

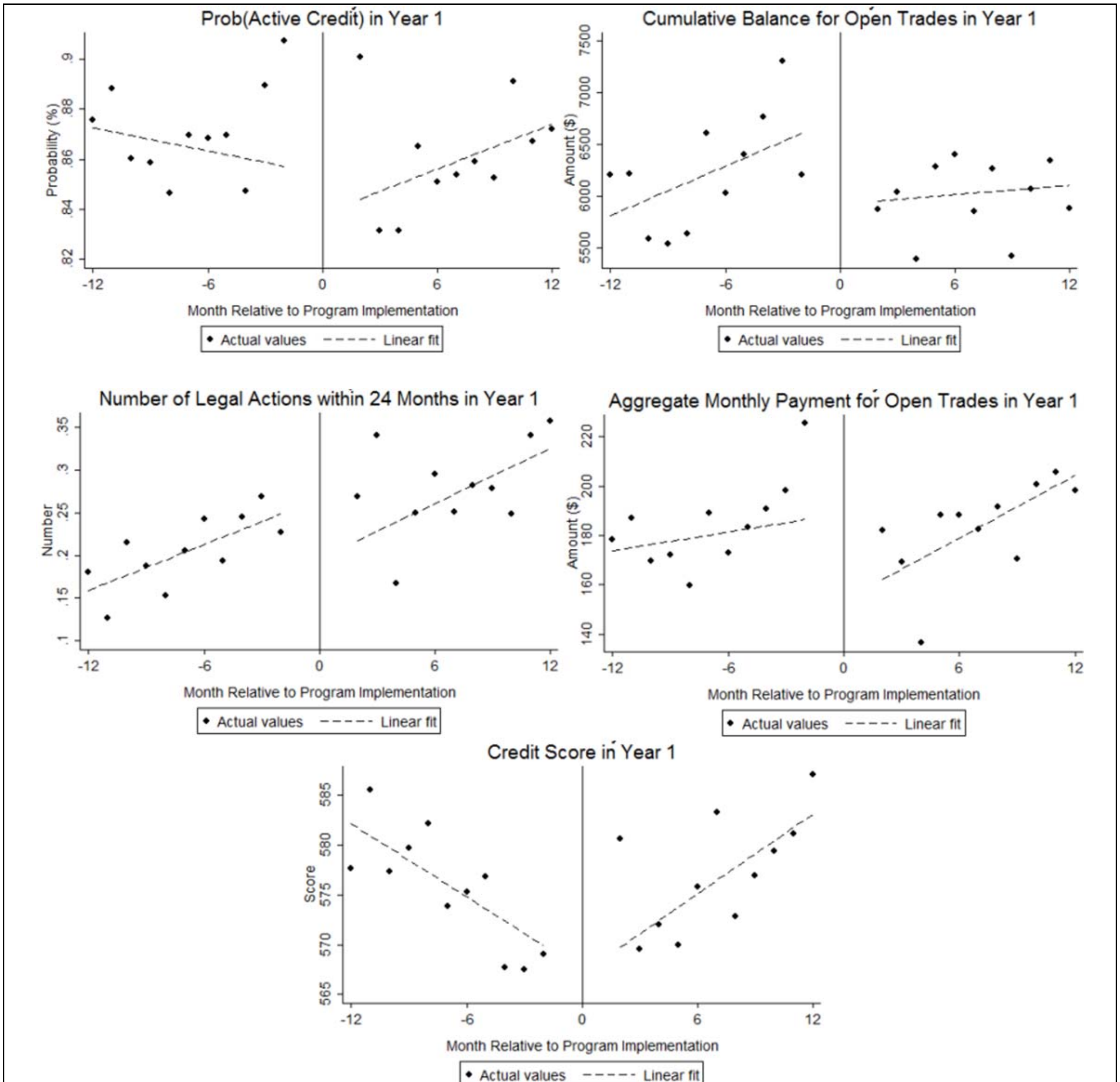

Figure 2.1. Event Studies for Financial Outcomes of Interest in Year 1

Author compiled data using DOD and AER data for 2006-2009. In each graph, the points at each x-axis value depict the average of the outcome for all individuals in the x cohort. The dashed lines depict an unweighted linear fit of these values to demonstrate the cohort patterns. n=29,318 for credit score; n=33,931 for all other outcomes.

In Figure 2.1 I present event studies for the five outcomes of interest from Table 2.2. In many cases, the event studies do not indicate a significant discontinuity at the month of PFMC implementation. For several outcomes however, including the cumulative balance (and its component parts, auto trades and credit cards) and the aggregate monthly payment, there appears to be a discontinuity at the month of program implementation. I explore the discontinuity estimates in more detail for all of these outcomes in the next sections.

2.6.C. PFMC Effects: Main Regression Estimates

In Table 2.4 I present RD estimates for the five principle outcomes of interest. In addition to the average effects (column 1), these estimates reveal the influence of the course on the extensive (column 2) an intensive (columns 3 and 4) margins. The regression results reveal that the PFMC has mixed effects on the selected outcomes. The course has statistically insignificant effects on the probability of having active credit, conditional on an individual record being matched (p=0.546), on the adverse legal action index (p=0.957) and on an individual’s credit score (p=0.147) in Panels A, C and E respectively. The course does appear to have an effect on the cumulative balance and the aggregate monthly payment. On average, the PFMC reduces an individual’s cumulative balance by $585 on a control mean of $6,117, a modest (10%) and marginally statistically significant effect (p=0.081). This effect appears to operate on the intensive margin as the effect estimates for the extensive margin (column 2) are negative but statistically insignificant (p=0.328). The course also has an effect on individuals' aggregate monthly payments. Here, the PFMC reduces monthly payments on average by $28 on a control mean of $179, also a modest (16%) and statistically significant effect (p=0.018) and the effects also appear to operate on the intensive margin. These results suggest that the effects of the

PFMC operate primarily through the total use of credit, which is typically not identifiable using only credit reports and credit scores, highlighting the value of the detailed credit bureau data.

Table 2.4. RD Estimates of the Effects of PFMC on Financial Outcomes in Year 1

Outcome Variable	(1) Y	(2) Pr (Y>0)	(3) Pr (Y>75 %ile)	(4) Y | Y>0
Panel A: Probability (Active Credit)			75%ile = 1	
PFMC Effect	1.20			
Std Err	(1.99)			
Control Mean	54.64			
Adj R^2	0.1764			
N	33,391			
Clusters	257			
Panel B: Cumulative Balance			75%ile = $9,033	
PFMC Effect	-585.43 *	-1.85	-3.46 **	-757.88 *
Std Err	(334.66)	(1.89)	(1.69)	(400.01)
Control Mean	6,116.89	81.73	25.33	7,483.95
Adj R^2	0.4516	0.1194	0.2831	0.4309
N	33,931	33,931	33,931	27,614
Clusters	257	257	257	255
Panel C: Legal Action Index			75%ile = 0	
PFMC Effect	0.002	-0.114	-0.114	-0.009
Std Err	(0.039)	(1.015)	(1.015)	(0.449)
Control Mean	0.193	5.20	5.20	3.71
Adj R^2	0.5341	0.2653	0.2653	0.5155
N	33,931	33,931	33,931	1,845
Clusters	257	257	257	215
Panel D: Aggregate Monthly Payment			75%ile = $308	
PFMC Effect	-28.03 **	-1.25	-2.96	-40.16 ***
Std Err	(11.79)	(2.87)	(2.52)	(13.82)
Control Mean	178.62	65.58	24.59	272.36
Adj R^2	0.3795	0.1368	0.2083	0.3618
N	33,931	33,931	33,931	22,117
Clusters	257	257	257	254
Panel E: Credit Score			75%ile = 641	
PFMC Effect	-5.47	1.21	-1.69	-5.47
Std Err	(3.76)	(2.01)	(2.05)	(3.76)
Control Mean	577.50	86.67	22.00	577.50
Adj R^2	0.4349	0.1758	0.2629	0.4349
N	29,318	33,931	33,931	29,318
Clusters	256	257	257	256

Table 2.4, Continued

Source: Department of Defense and Census Bureau Data. Notes: Sample as defined in the text and restated here. All data is for U.S. Army enlisted soldiers who enlisted between June 2006 and August 2009 and completed AIT at a given location within the 12 month period before and after program implementation, excluding the month before, after and of program implementation. The cumulative balance outcome is the sum of individual account balances from automobile trades (loans and leases), credit card trades (bankcard and retail), finance trades and unpaid trades (charge offs and collection status codes). The legal action index outcome is the sum of an individual's liens, judgments, repossessions, foreclosures and bankruptcies. The aggregate monthly payment outcome is the sum of an individual's scheduled payments for all account types. The regression coefficients reported are for the discontinuity at the month of implementation. All regressions include the following covariates: a quadratic in age, quadratic in experience, indicators for female, married, minority, number of dependents and a summer entry, indicators for education levels less than or equal to high school, some college, and greater than or equal to college (high school graduate is the omitted category), AFQT score, enlistment term, average monthly base pay, median household income in the individual's zip code of record, AIT length, number of months deployed in the year, the credit score in the year prior to entry, the outcome value in the year prior to entry and fixed effects for an individuals' job, military branch, AIT location, and AIT start month. Indicator variables capture individuals missing pay, zip code income, credit score and lagged outcome variable data and these individuals are assigned values of zero for these variables. Heteroskedasticity robust standard errors, clustered at the AIT location-month level, are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

Having identified the primary effects of the PFMC on economic behavior, I now turn to more detailed analyses of the effects of the course on individuals' cumulative balances throughout the distribution, over time, by balance type (auto, credit, finance and unpaid), by previous balance levels and through heterogeneous treatment effects.[7]

2.6.D. PFMC Distributional Effects

In Figure 2.2., I present the effects of the PFMC on cumulative balances throughout the majority (0-95th percentile) of the balance distribution. As the graph shows, the PFMC point estimates are negative throughout the distribution and typically between a 1% and 6% reduction in the probability of a given balance level. However, the 95% confidence interval bands reveal that these point estimates are only statistically distinguishable from zero between

[7] Since the monthly payments are a function of an individual's balances, the aggregate monthly payment outcome is of secondary interest. Nonetheless, it reveals an important effect of the course: individuals who attended the course on average have smaller scheduled payments to creditors.

$8,000 and $14,000. The blue line (with square points) on Figure 2.2 depicts the scaled effect magnitude. At each $2000 interval along the distribution the blue line represent a scaled effect size for the PFMC on that balance level and these effects vary from a $500 to $5500 reduction. In the region of the distribution where the PFMC has statistically significant effects at $\alpha=0.05$ ($8,000-15,000), the scaled effect size is on average $2,303, implying that the PFMC reduces balances by this amount for these balance levels. While the effects of the PFMC do not appear to operate on the extensive margin, the intensive margin effects also vary substantially with the most identifiable effects in the upper part of the distribution (roughly the 75th-85th percentiles).

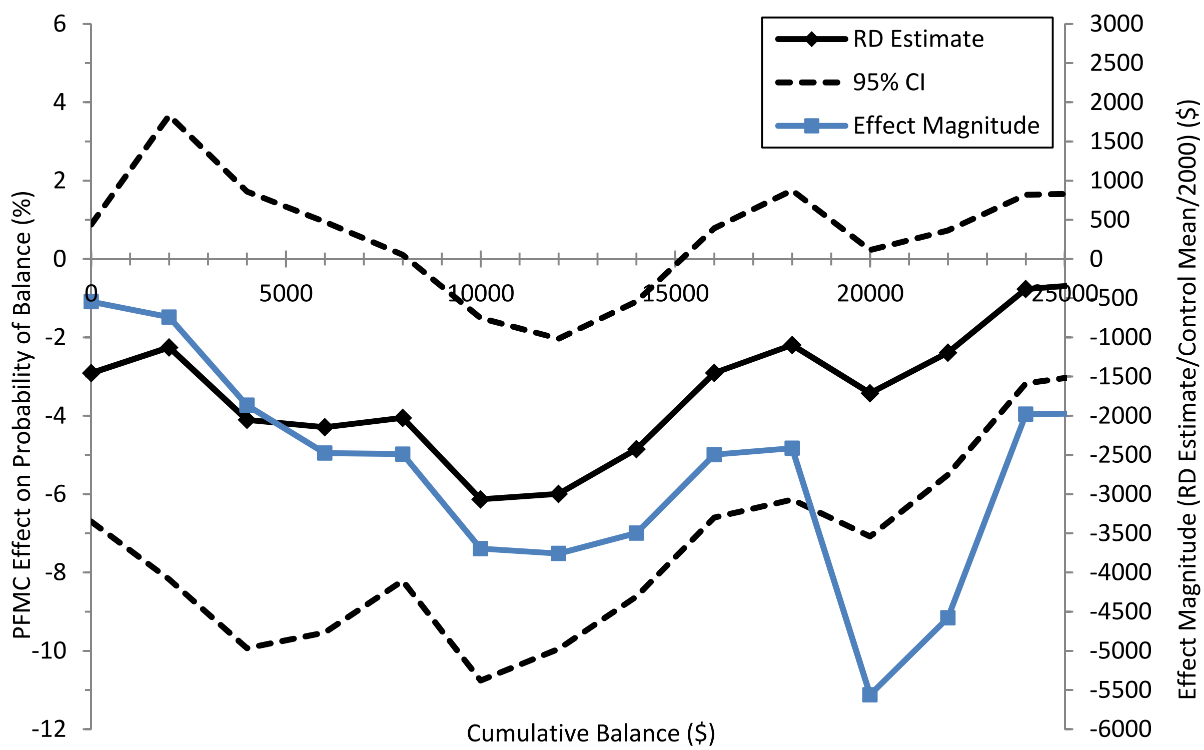

Figure 2.2. PFMC Effects on the Distribution of Cumulative Balances in Year 1

Author compiled data using DOD and AER data for 2006-2009. The x-axis values reflect the 0-95th percentiles of the distribution for the Cumulative balances ($0-$24,778).The RD estimates and 95% confidence intervals reflect a series of regressions on indicator variables for the balance levels that correspond to the x-axis. For each bin (k) of binwidth $2000 on the x-axis [0,24000], I generate an indicator for balance levels in the $2000 bin [i.e., I=1 if k<TSP≤k+2000 and I=0 Otherwise]. I then complete RD regressions on the series of indicators and identify the effects of the PFMC in each bin. These estimates correspond to the left axis. To convert these probability estimates into a dollar effect magnitude I divide the RD estimate by f(k)/2000 where f(k)=Prob(k<TSP≤k+2000). These estimates are depicted in blue and correspond to the right axis. n=33,931.

2.6.E. PFMC Longitudinal Effects

While the military administrative data is censored in part during year 2, the size of the data set still affords evaluation of the cumulative monthly balances at this horizon. In Table 2.5 I present the RD estimates for the effects of the PFMC on cumulative balances for years 1 and 2.

Table 2.5. RD Estimates of the Effects of PFMC on Cumulative Balance, by Year

	(1)		(2)	(3)		(4)		(5)
Outcome:	Y		Pr (Y>0)	Pr (Y>75 %ile)		Y | Y>0		Δ No. Trades
	Panel A: Year 1 Outcomes			75%ile = $9,033				
PFMC Effect	-585.43	*	-1.85	-3.46	**	-757.88	*	0.032
Std Err	(334.66)		(1.89)	(1.69)		(400.01)		(0.102)
Control Mean	6,116.89		81.73	25.33		7,483.95		1.66
Adj R^2	0.4516		0.1194	0.2831		0.4309		0.0548
N	33,931		33,931	33,931		27,614		33,931
Clusters	257		257	257		255		257
	Panel B: Year 2 Outcomes			75%ile = $14,098				
PFMC Effect	-889.79	*	-0.40	-1.88		-604.09		0.131
Std Err	(465.12)		(1.96)	(2.04)		(369.47)		(0.185)
Control Mean	8,535.52		88.82	27.36		9,609.49		1.64
Adj R^2	0.2389		0.0769	0.1351		0.2201		0.1074
N	31,014		31,014	31,014		27,188		31,014
Clusters	222		222	222		222		222

Source: Department of Defense, Census Bureau and Credit Bureau Data. Notes: Sample as defined in the text. Regression specifications as described in Table 2.4. Heteroskedasticity robust standard errors, clustered at the AIT location-month level, are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

The estimates in Table 2.5 reveal that the balance reductions caused by the PFMC carry over to year 2. On average, the PFMC reduces an individual's cumulative balance in year 2 by $889 on a control mean of $8,535, a similar size (10%) effect as in year 1, with marginal statistical significance (p=0.082). Once again the effect appears to operate on the intensive margin and at relatively high balance levels (the point estimate at the 75th percentile is insignificant [col 3] but the conditionally positive point estimate approaches significance [col4], p=0.104). Overall, the results in Table 2.5 suggest that the PFMC reduces cumulative balances through at least year 2.

2.6.F. PFMC Effects by Types of Balances

Table 2.6. RD Estimates of the Effects of PFMC on Cumulative Balance in Year 1, by Type

	(1)	(2)	(3)	(4)	(5)
Outcome Variable	Y = Balance	Pr (Y>0)	Pr (Y>75 %ile)	Y | Y>0	Δ No. Trades
Panel A: Cumulative Balance for Open Trades					
			75%ile = $9,033		
PFMC Effect	-585.43 *	-1.85	-3.46 **	-757.88 *	0.032
Std Err	(334.66)	(1.89)	(1.69)	(400.01)	(0.102)
Control Mean	6,116.89	81.73	25.33	7,483.95	1.664
Adj R^2	0.4516	0.1194	0.2831	0.4309	0.0548
N	33,931	33,931	33,931	27,614	33,931
Clusters	257	257	257	255	257
Panel B: Cumulative Balance for Open Automobile Trades					
			75%ile = $0		
PFMC Effect	-656.06 **	-4.55 **	-4.55 **	-453.38	-0.035 *
Std Err	(272.04)	(1.82)	(1.82)	(566.41)	(0.021)
Control Mean	2,950.15	41.83	22.60	13,053.69	0.176
Adj R^2	0.2632	0.2404	0.2404	0.1012	0.0432
N	33,931	33,931	33,931	7,353	33,931
Clusters	257	257	257	246	257
Panel C: Cumulative Balance for Credit Card Trades					
			75%ile = $853		
PFMC Effect	-147.22	-2.35	-4.73 **	-187.99	-0.011
Std Err	(91.49)	(2.83)	(2.17)	(145.25)	(0.068)
Control Mean	826.61	49.39	26.84	1,673.74	0.795
Adj R^2	0.2807	0.0935	0.1144	0.3155	0.0753
N	33,931	33,931	33,931	15,822	33,931
Clusters	257	257	257	253	257
Panel D: Cumulative Balance for Open Finance Trades					
			75%ile = $0		
PFMC Effect	-84.59	-3.14 **	-3.14 **	-70.91	-0.046 *
Std Err	(60.93)	(1.54)	(1.54)	(435.68)	(0.026)
Control Mean	325.57	9.62	9.62	3,385.34	0.096
Adj R^2	0.2931	0.1281	0.1281	0.3861	0.0721
N	33,931	33,931	33,931	3,904	33,931
Clusters	257	257	257	226	257
Panel E: Cumulative Balance for Open Unpaid Trades					
			75%ile = $1,342		
PFMC Effect	180.51	1.25	4.44 **	451.08 *	0.115 *
Std Err	(114.29)	(2.29)	(2.11)	(242.07)	(0.069)
Control Mean	1,786.21	47.36	23.53	3,771.69	0.597
Adj R^2	0.6192	0.3340	0.3898	0.5706	0.0504
N	33,931	33,931	33,931	16,389	33,931
Clusters	257	257	257	253	257

Source: Department of Defense, Census Bureau and Credit Bureau Data. Notes: Sample as defined in the text. Regression specifications as described in Table 2.5. Heteroskedasticity robust standard errors, clustered at the AIT location-month level, are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

Since the PFMC effect estimates on the balance levels are large (over $1,000 in year 1), implying a relatively fast effect and since there exists more detailed evidence on the composition of these balances, I now turn to analyzing the effects of the PFMC on cumulative balances by the type of account. In Table 2.6 I present the estimates of the PFMC on account balances in year 1. In addition to the average (column 1), extensive margin (column 2) and intensive margin (columns 3 and 4) estimates, I also analyze another intensive margin, the number of new trades (accounts) in each type of account during year 1 in column 5.

The most significant finding from Table 2.6 is that the only balance type that the PFMC significantly affects is automobile trade balances (Panel B). The Panel B estimates reveal that the PFMC reduces auto trade (loans and leases) balances in year 1, on average, by $656 on a control mean of $2,950, a moderate (22%) and statistically significant effect (p=0.017) effect. In addition, the effects appear to operate on the extensive margin, reducing the probability of having an auto trade balance by 4.55%, a modest (11%) and statistically significant effect (p=0.013). This extensive margin effect has a few potential sources: the PFMC might discourage some soldiers from purchasing or leasing an automobile in year 1 who might have otherwise; it might encourage soldiers with existing auto balances to sell their car, perhaps through highlighting the complete costs of owning a car or the limited need for a car if deploying; finally, the PFMC might encourage soldiers with existing auto balances to reduce their balances, through higher monthly payments, renegotiation of interest rates that facilitate repayment prior to the year 1 observation, selling the automobile or foregoing new vehicle purchases. From a review of the PFMC course materials, all of these possibilities are potential outcomes from the car buying lesson. I explore the automobile trade effects in more detail below.

The second conclusion from Table 2.6 is that the PFMC does not appear, on average, to have statistically significant effects on credit card trade (Panel B) balances (p=0.109), finance trade (Panel D) balances (p=0.166) or unpaid trade (Panel E) balances (p=0.116), though all of these estimates approach statistical significance at the α=0.10 level.

2.6.F. PFMC Effects by Previous Balance Levels

The results presented in Table 2.6 suggest that the main effects of the PFMC operate via automobile trade decisions. In this section I analyze the effects of the PFMC on individual financial decisions in this area by individual account balance levels in the year prior to entry into the Army (year 0). In Table 2.7 I present the results of this analysis for automobile trades. The results in Panel A restate the primary effects of the PFMC on automobile trade balances, with more detailed analyses of the distributional effects of the PFMC. These results suggest that the average effects are concentrated at the lower end of the automobile balance distribution (columns 2 and 3).

The results in Panel B suggest that for individuals with no existing auto balances, the PFMC reduces the total auto balances, on average, by $360 on a control mean of $2,132, a moderate (17%) effect that is not significant at conventional levels (p=0.273). This is surprising given that the PFMC encourages more frugal car buying for students and a substantive number with no existing balances in year 0 buy a car in year 1 (n=4,165). However, given the reduced sample size and the indicative p-value (0.273) there might be an effect on this group as well. In addition, most individuals do not buy a car in year 1, suggesting that the PFMC effects might not be visible for this group at this time horizon.[8]

Table 2.7. RD Estimates of the Effects of PFMC on Automobile Trade Balances in Year 1

	(1)		(2)		(3)		(4)		(5)	(6)	
Outcome:	Y = Balance		Pr (Y>0)		Pr (Y>$5K)		Pr (Y>$15K)		Pr(Y>$25K)	Δ # Trades	
Panel A: All Automobile Trades											
PFMC Effect	-656.06	**	-4.55	**	-4.87	***	-0.86		-0.01	-0.035	*
Std Err	(272.04)		(1.82)		(1.66)		(1.41)		(0.75)	(0.021)	
p-value	*0.017*		*0.013*		*0.004*		*0.542*		*0.990*	*0.092*	
Control Mean	2,950.15		22.60		20.36		7.68		1.33	0.176	
Adj R^2	0.2632		0.2404		0.2279		0.1354		0.0730	0.0432	
N	33,931		33,931		33,931		33,931		33,931	33,931	
Clusters	257		257		257		257		257	257	
Panel B: Subsample with No Year 0 Auto Trade Balance											
PFMC Effect	-359.81		-3.52		-3.64	*	1.12		0.68	-0.035	
Std Err	(327.48)		(2.13)		(2.00)		(1.27)		(0.68)	(0.024)	
p-value	*0.273*		*0.100*		*0.069*		*0.376*		*0.317*	*0.144*	
Control Mean	2,131.54		15.76		14.92		5.56		0.68	0.175	
Adj R^2	0.0480		0.0505		0.0479		0.0217		0.0060	0.0495	
N	29,629		29,629		29,629		29,629		29,629	29,629	
Clusters	256		256		256		256		256	256	
Panel C: Subsample with Positive Year 0 Auto Trade Balance											
PFMC Effect	-1,590.40	**	-6.70		-2.55		-9.05	**	-2.82	0.003	
Std Err	(659.65)		(4.55)		(4.76)		(3.75)		(2.80)	(0.056)	
p-value	*0.017*		*0.142*		*0.592*		*0.017*		*0.316*	*0.956*	
Control Mean	9,094.03		73.92		61.20		23.64		6.18	0.186	
Adj R^2	0.3647		0.0757		0.1922		0.2850		0.1122	0.0367	
N	4,302		4,302		4,302		4,302		4,302	4,302	
Clusters	244		244		244		244		244	244	

Source: Department of Defense, Census Bureau and Credit Bureau Data. Notes: Sample as defined in the text with caveats noted in each panel title. Regression specifications as described in Table 2.5. Heteroskedasticity robust standard errors, clustered at the AIT location-month level, are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

The results in Panel C suggest that for individuals with an existing auto balance, the PFMC reduces the total auto balances in year 1, on average, by $1,590 on a control mean of $9,094, a moderate (17%) effect that is significant (p=0.017). Here the PFMC effects might operate

[8] The results for the Panel B subsample in year two suggests that the PFMC reduces automobile balances for those without year 0 balances by $514 on a control mean of $3,748, a modest (14%) reduction that is marginally significant (p=0.071). The effect is divided between the extensive margin (about 35% of the effect) and the probability of automobile trade balances greater than $5K (about 65% of the effect), suggesting that the PFMC encourages both a delay in car buying and improved car buying (better negotiations or more affordable car selection).

through several channels: treated individuals could pay down their balances at higher rates than their control group counterparts; treated individuals might obtain balance reductions through negotiating for reduced interest rates or other fees; treated individuals might sell their automobiles at higher rates than their control counterparts, if the course raises the awareness of the total costs of car ownership; finally, treated individuals might lower their balances through trading in their current car for a less expensive one, by foregoing or delaying the purchase or lease of new and more expensive cars, or by upgrading their current automobiles for more affordable automobiles relative to the control group. I explore each of these below.

The first mechanism (paying larger portions of the balance) is possible given the PFMC emphasis on the costs of interest or the total costs of late payments. However, this mechanism requires considerable flexibility in an individual's monthly disposable income, which is less likely for military members on a fixed income and in particular, for married individuals or individuals with children. I discuss and discount this possibility below, since the subsample in Panel C is on average older, more likely to be married and has more dependents (spouse plus children) than the subsample in Panel B.

The second mechanism (renegotiated terms) is a potential outcome of the course's coverage of the Servicemember's Civil Relief Act (SCRA), a federal consumer protection law for military service members.[9] The relevant portion of the SCRA for this analysis is the mandated maximum 6% interest rates that creditors can charge military members on debt that existed at their time of entry into the military. As part of the lesson on credit, the PFMC reviews the SCRA and

[9] The SCRA, also known as the "Soldiers & Sailors Act" is a federal law that mandates a number of protections for individuals who join the Active Duty military. For more information on SCRA provisions, see: http://www.uscourts.gov/FederalCourts/Bankruptcy/BankruptcyBasics/SCRA.aspx.

highlights its protections for soldiers. It also provides students with an example letter in the student handbook that students can complete and submit to creditors to obtain interest rate reductions.[10] If the PFMC increases the probability that individuals obtain SCRA protection then it should reduce individual account balances and payments as evidenced in this section. Unfortunately, the credit bureau data contains no indications of SCRA protection and without monthly panel data the interest rates cannot be reliably computed and compared to determine if rates decreased in any given area. This element of the SCRA requires an understanding of the law and its requirements by individuals and additional action by them to improve their financial standing. As such, the SCRA rewards informed and motivated individuals. But the PFMC information about the SCRA and provision of a sample creditor notification letter clearly lower the costs of obtaining protection. In this way, claiming SCRA protection is a combination of human financial capital (awareness and action) and behavioral assistance (lowering the costs of action). As with the retirement savings effects discussed in Chapter 1, the PFMC appears to improve the financial situation of its students through a combination of knowledge and assistance. To assess whether this mechanism can explain the observed average effects (a $1,590 reduction), I conduct sensitivity analysis using a six parameter model of auto loans (purchase price, down payment percentage, loan term, interest rate, month of loan relative to joining the military and month in the military when the SCRA notification occurs) to compute

[10] Once notified of qualified debt by a soldier, a creditor is required to take several steps, including: reducing the individual's interest rate to 6% APR for all payments for the existing debt during the individual's service; forgive interest balances in excess of 6%; and the 6% APR must include all fees. Upon notification of qualified debt, the creditor typically calculates the account balance at the time of entry into service and reamortizes the payments at the new interest rate. Such procedures typically result in lower account balances and lower monthly payments for individuals. Based on author phone conversations and email discussions with executives at large financial institutions during spring 2012. For additional information on the required steps, see as a reference a DOJ manual on SCRA provisions at: http://www.justice.gov/usao/az/rights/Servicemembers_Civil_Relief_Act.pdf.

the principle owed at month 6 in year 1 (when I observe individuals on average), recomputed the balance if the loan was refinanced at 6% APR, and determine the potential balance reductions from SCRA protection.[11] I then vary the parameters to determine what conditions might generate the observed savings levels. Only variations in the automobile price and interest rate can generate these savings levels, but holding all other parameters constant, the average price would have to be $62,105, or the interest rate would have to be over 43%. This automobile balance is nearly seven times the actual average automobile balance for this group in the data ($8,992) and the interest rate is nearly six times the national average for commercial bank auto loans with 48 month terms (7.52%) based on Federal Reserve Data during the period under study.[12] These conditions are unreasonable as average parameters and as a result, the SCRA cannot fully explain the PFMC results for the automobile balances among this group in year 1.[13] In addition, the SCRA benefits assist all individuals with existing debt but the effects are visible only in the middle of the automobile balance distribution (balances greater than $25K). The SCRA mechanism might reasonably explain roughly 25% of the observed effect ($384/$1,590).

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[11] In the baseline specification I assume a $15,000 automobile loan with 10% down ($1,500), a 48 month term, taken 12 months prior to military entry with SCRA notification made at month 2 of military service (during AIT). These parameters generate an estimated average savings from SCRA protection at month 6 of $384.

[12] Federal Reserve data on average automobile loan rates by institution type can be found at the Board of Governors website. Accessed April 18, 2012 at: http://www.federalreserve.gov/releases/g19/hist/cc_hist_tc.html.

[13] There are two facts supporting the conclusion about the reasonability of these parameter estimates. First, since the estimated reduction of $1,590 is an average effect, it would require that all individuals with existing auto balances be eligible for and file for SCRA protection. In the most comparable research on behavioral assistance, Bettinger, Long and Oreopoulos (2009) find average effects of 30-40% in the context of financial aid. So it is unlikely that every student will take advantage of the SCRA even once informed about its potential benefits. If every student did not take advantage, then the required savings for those who did obtain SCRA protection would have to be even larger than $1,590, an unsupportable benefit level given the empirical distribution of auto loans in the sample.

The third mechanism (greater likelihood to sell) is possible given the emphasis on the total costs of car ownership but implies elastic demand for automobiles among this group (i.e., for young, single, non-parent individuals who have less need for a car early in their military service, as they typically live on base with access to food and other shopping). To test the feasibility of this explanation, in Table 2.8 I compare selected individual characteristics for the individuals in the subsamples in Panel B and Panel C. As the results show, the individuals in Panel C are less likely to have elastic demand for automobiles given their ages, marital statuses and number of dependents. In addition, if treatment induces selling cars then the change in the number of automobile trades during year 1 would be more negative for treated individuals. As column 6 of Table 2.7 reveals though, the course has no statistically significant effects on the change in the number of trades from year 0 to year 1 (p=0.956). Thus the third mechanism seems unlikely.

Table 2.8. Selected Summary Statistics by Auto Balances in Year 0

	(1)		(2)		(3)		(4)		
	Full Matched Sample		**No Previous Balance (Panel B)**		**Positive Previous Balance (Panel C)**		**Difference**		
	N=26,050		*N=21,747*		*N=4,303*		(3)-(2)		
Variable	**Mean**	**(Std Dev)**	**Mean**	**(Std Dev)**	**Mean**	**(Std Dev)**	**Mean**		**(Std Err)**
Age, years	22.65	(4.21)	22.39	(4.09)	23.97	(4.52)	**-1.58**	***	**(0.07)**
Married, %	24.46	(42.98)	22.07	(41.47)	36.53	(48.16)	**-14.47**	***	**(0.71)**
Number of dependents	1.04	(1.28)	0.98	(1.26)	1.32	(1.38)	**-0.33**	***	**(0.02)**

Source: Department of Defense, Census Bureau and Credit Bureau Data. Notes: Sample as defined in the text. Here the sample is restricted to those individuals with matched credit records in the year prior to entry into the Army (year 0). Column 2 reports the summary statistics for the subsample with no previous automobile trade balances and column 3 reports the summary statistics for the subsample with previous automobile trade balances. Column 4 reports the differences in the means by subsample, the standard errors for the differences and the significance levels for the t-tests of the equality of means. ***, **, * represent statistical significance for the difference in means at the 1%, 5% and 10% levels respectively.

Finally, the fourth mechanism would require treated individuals to lower their balances through trading in their current car for a less expensive one, by foregoing or delaying the purchase or lease of new and more expensive cars, or by upgrading their current automobiles for more affordable automobiles relative to the control group. This scenario seems the most likely given the inability of the other four explanations above to explain more than about 25% of the treatment effect. In addition, the demographic analysis (Table 2.8) suggests that this group has inelastic demand for automobiles and the subsample analysis (Table 2.7) suggests that the effects operate via the average balance (specifically a reduced probability of balances greater than $15K as shown in column 4) and not via the number of automobile trades. It thus appears that the immediate effects of the PFMC is to persuade individuals with existing balances to be more frugal in their car purchases and leases, through trading down, foregoing an upgrade or upgrading to a lesser degree than those in the control group.

2.6.G. Heterogeneous Treatment Effects

In this section I explore the heterogeneous treatment effects for the PFMC with individual characteristics on the cumulative balance. I test for heterogeneous effects along the same categories as in Chapter 1: gender, human capital, SES and marital status. In addition I test for differences by individuals' year 0 credit score quartiles. In Table 2.9, I present RD estimates for these heterogeneous treatment effects for individuals' cumulative balances. As the estimates reveal, only one characteristic, the fourth quartile of year 0 credit scores, has a statistically significant interaction with treatment. The interaction effect in column 5 suggests that treated individuals with high credit scores reduce their cumulative balances by $1,134 relative to treated individuals with credit scores in the lower two quartiles (the omitted category, which has credit scores equal to 0 based on the matched records in year 0). None of the other categories have statistically significant interaction effects with the PFMC.

Table 2.9. Heterogeneous Treatment Effects of PFMC on Cumulative Balance in Year 1

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Model:	Base	Female	Education	AFQT	Credit Score	SES	Married
PFMC Effects	-597 *	-636 *	-106	-607	-106	-613	-634 *
Std Err	(336)	(345)	(396)	(456)	(396)	(397)	(341)
Female	-190	-536	-192	-189	-187	-191	-191
Std Err	(139)	(460)	(139)	(140)	(139)	(139)	(140)
Female × PFMC		441					
Std Err		(578)					
Educ≥Some College	433 **	431 **	1,305 **	439 **	442 **	436	440 **
Std Err	(206)	(206)	(633)	(206)	(206)	(206)	(207)
SMC × PFMC			-560				
Std Err			(822)				
AFQT Q2	-259 ***	-260 ***	-258 ***	-81	-258 ***	-261	-260 ***
Std Err	(96)	(96)	(96)	(322)	(96)	(96)	(96)
AFQT Q2 × PFMC				-484			
Std Err				(463)			
AFQT Q3	-449 ***	-449 ***	-449 ***	-471	-449 ***	-453	-450 ***
Std Err	(100)	(100)	(100)	(426)	(100)	(100)	(100)
AFQT Q3 × PFMC				-131			
Std Err				(506)			
AFQT Q4	-860 ***	-860 ***	-859 ***	-1,634 ***	-862 ***	-863	-863 ***
Std Err	(111)	(111)	(111)	(423)	(111)	(111)	(111)
AFQT Q4 × PFMC				588			
Std Err				(565)			
Credit Score Q3	731 ***	730 ***	730 ***	731 ***	1,225 ***	729	730 ***
Std Err	(110)	(110)	(110)	(110)	(354)	(110)	(110)
Score Q3× PFMC					-338		
Std Err					(515)		
Score Q4	515 ***	515 ***	513 ***	518 ***	1,822 ***	515	512 ***
Std Err	(108)	(108)	(108)	(108)	(364)	(108)	(108)
Score Q4 × PFMC					-1,134 **		
Std Err					(469)		
SES Q3	-135	-131	-133	-126	-101	189	-163
Std Err	(213)	(213)	(213)	(213)	(211)	(542)	(212)
SES Q3× PFMC						-1,096	
Std Err						(1,361)	
SES Q4	-368 *	-367 *	-363 *	-364 *	-345 *	-68	-384 **
Std Err	(190)	(190)	(190)	(190)	(188)	(470)	(188)
SES Q4 × PFMC						-218	
Std Err						(874)	
Married	1,429 ***	1,428 ***	1,427 ***	1,429 ***	1,427 ***	1,429 ***	1,489 ***
Std Err	(144)	(143)	(143)	(144)	(144)	(144)	(498)
Married × PFMC							174
Std Err							(603)
Control Mean	6,117	6,117	6,117	6,117	6,117	6,117	6,117
Adj R^2	0.4514	0.4514	0.4515	0.4514	0.4517	0.4514	0.4516
N	33,931	33,931	33,931	33,931	33,931	33,931	33,931
Clusters	257	257	257	257	257	257	257

Source: Department of Defense, Census Bureau and Credit Bureau Data. Notes: Sample as defined in the text. Regression specifications as described in Table 2.5. with one exception: education levels are consolidated to those with< some college and those with ≥ some college (SMC). Heteroskedasticity robust standard errors, clustered at the AIT location-month level, are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

2.6.H. Discussion of Results

The PFMC appears to improve the financial behavior and financial outcomes for its students. Relative to individuals that did not complete the course, the PFMC has statistically significant positive effects for two of the five principle outcomes evaluated here and no statistically significant effects for the remaining outcomes. Specifically, the PFMC appears to reduce cumulative account balances by about 10%, reduce aggregate monthly payments by about 16%, while having statistically insignificant effects on the probability of being active in the credit market, individual credit scores and the adverse legal action index. For two of these outcomes (probability of active credit and credit score), the theoretical predictions were unclear and unfortunately, this analysis cannot shed any light on the empirical relationship between financial education and the outcome. For the adverse legal action index, it appears that the PFMC is, on average, unable to reduce the incidence of severely negative financial outcomes for students. The attenuating effects of leadership and role models and the availability of alternate financial education might both be particularly strong effects in this context as Army leaders will act to assist soldiers and minimize the incidence of these legal actions. Alternately, these outcomes might be rare and unlikely at this time horizon. Nonetheless, this result is still discouraging for the role of education in mitigating severely negative financial outcomes.

One encouraging aspect of these findings is the absence of any finding of an increase in individual credit financing to offset the increased retirement savings demonstrated in Chapter 1. If the PFMC did not induce individuals to change their consumption preferences or budgeting and spending procedures, the $17 average increase in retirement savings in year 1 ($33 in year

2) that the PFMC generates might be offset with increased credit financing. There is no evidence here of such an increase, implying that even if the credit outcome effects analyzed of the PFMC analyzed here were zero, the course would be on-balance welfare enhancing for individuals. To the extent that the course improves budgeting or increases preferences for saving and future consumption, these effects might continue to improve individual financial outcomes relative to those that did not attend the course.

Treatment Effect Mechanism

One of the primary goals of this research was to estimate the effects of financial education on financial behaviors where the effects of human capital could be separated from the effects of behavioral assistance. In Chapter 1 I demonstrated that the combined effects were large in the context of retirement savings in the Thrift Savings Plan. Unfortunately, as described above in Section 2.5, these effects may not be differentiable in this research. Given the role of the SCRA and the PFMC assistance in obtaining SCRA protections and benefits, the large and significant effects of the PFMC in reducing automobile trade balances should also be considered the combined effects of learning about the SCRA (perhaps 25% of the effect on automobile balances) and more general consumer awareness and motivation and human capital (the remaining 75% of the effect).

Overall, the estimates presented here suggest that the PFMC has a number of beneficial effects on overall financial behavior. However, the mechanism for these effects is not as precise as might be desired. Given the size, quasi-experimental nature, and rich data sources used in this analysis, separating and measuring these effects will be difficult for future research. More precise mechanism identification and estimation in future work will likely require similarly large experiments, detailed financial outcome data and either high frequency outcome data (in cases where the financial education develops both human capital and provides assistance to students) or a course that provides only human capital development and no assistance.

Threats to Identification

While the randomization tests in Chapter 1 established that the observable characteristics were unrelated to treatment and the sample used here is a random sample of the full sample, there remains the possibility that within this sample the observed effects are driven by differences in the characteristics of the control and treatment groups. To discount this possibility, in Table 2.10 (Panel B), I present the results of an additional randomization test for this sample. As the results, show, once again, the observable characteristics are unrelated to treatment. Of the 20 observable characteristics, none are statistically related to treatment at the $\alpha=0.05$ level and only one (AFQT Score) is statistically related to treatment at the $\alpha=0.10$ level, no more than what we would expect by chance. In addition, the AFQT finding, with a lower AFQT score among the treatment group, would bias against a finding of PFMC benefits since AFQT correlates positively with desirable financial behaviors. Table 2.10 also reveals that there is no selection in attrition between the control and treatment group (Panel A).

Interpretation of Estimates

The magnitude interpretations for the estimates in this chapter are the same as in Chapter 1. Delays in training, absences, peer or role model effects and simultaneous financial literacy training elsewhere in the Army will all attenuate the estimates in my estimates. As a result, these estimates should serve as lower bounds for the effects of the PFMC on financial behavior.

Table 2.10. Randomization Tests for Credit Sample

Variable	Coeff	(Std Err)	p-value	Sig	Adj-R2	Obs	Clusters	Mean in Control
Panel A. Credit Matching Outcomes for Year 1 and Year 2 Samples								
Prob (Record Not Matched) Yr 1	0.73	(1.89)	0.697		0.0074	41,303	257	16.76
Prob (Record Not Matched) Yr 2	0.65	(2.08)	0.756		0.2646	41,303	257	12.42
Panel B. Individual Characteristics for Year 1 Sample								
Age	0.30	(0.270)	0.2641		0.0240	33,931	257	21.70
Experience	0.28	(0.243)	0.2540		0.0266	33,931	257	3.82
Female	5.04	(5.91)	0.3948		0.0112	33,931	257	11.92
Married	2.92	(2.29)	0.2027		0.0051	33,931	257	19.23
Number of Dependents	0.09	(0.061)	0.1194		0.0061	33,931	257	0.94
Education ≤ HS Graduate	0.10	(0.370)	0.7904		0.0337	33,931	257	0.02
Education = HS Graduate	-0.11	(1.694)	0.9487		0.0098	33,931	257	91.22
Education = Some College	0.43	(1.056)	0.6862		0.0024	33,931	257	6.37
Education ≥ College Graduate	-0.42	(0.931)	0.6548		0.0013	33,931	257	2.39
Minority	6.62	(6.04)	0.2741		0.0098	33,931	257	29.26
AFQT Score	-3.89	(2.24)	0.0838	*	0.0356	33,931	257	56.20
Summer Enlistment	-5.73	(5.15)	0.2668		0.6977	33,931	257	36.98
Enlistment Term Length	-0.02	(0.14)	0.8823		0.0398	33,931	257	3.85
AIT Length (Months)	-0.07	(0.30)	0.8059		0.1041	33,931	257	3.17
Monthly Basic Pay	8.84	(18.74)	0.6375		0.1678	33,931	257	1,605
Median HH Income in Home Zip	-1,560	(1065)	0.1442		0.6963	33,931	257	42,204
Months Deployed	0.17	(0.23)	0.4494		0.0337	33,931	257	1.06
Panel C. Lagged Credit Outcomes for Year 1 Sample								
Credit Score in Yr 0	-12.18	(14.59)	0.4046		0.0214	33,931	257	555.22
Predicted Score for Yr 0	-11.68	(14.44)	0.4193		0.0209	33,931	257	555.03
Missing Score for Yr 0	1.62	(2.65)	0.5417		0.0239	33,931	257	46.00
Prob(Credit Active) in Yr 0	-3.01	(2.19)	0.1700		0.0019	25,998	255	71.92
Agg. Monthly Payment in Yr 0	53.65	(895.81)	0.9523		0.0061	33,931	257	86.53
Adverse Legal Action Index in Yr 0	2.24	(3.63)	0.5376		0.0008	33,931	257	0.18
Cumulative Balance in Yr 0	1,623	(35,878)	0.964		0.0069	33,931	257	3,428
Auto Trades Balance in Yr 0	4,629	(19,386)	0.8115		0.0029	33,931	257	1,404
Credit Card Trades Balance in Yr 0	-6,855	(6,325)	0.2795		0.0035	33,931	257	353.55
Finance Trades Balance in Yr 0	1,361	(5,584)	0.8076		0.0005	33,931	257	209.50
Unpaide Balances in Yr 0	-2,314	(16,293)	0.8872		0.0087	33,931	257	1,276

Source: Department of Defense, Census Bureau and Credit Bureau Data. Notes: All data is for U.S. Army enlisted soldiers who enlisted between June 2006 and August 2009 and completed AIT at a given location within the 12 month period before and after program implementation, excluding the month just before, of and after program implementation. Each cell reports the RD estimate for the effect of the discontinuity at the month of program implementation on the covariate specified in the respective row. All regressions include the running variable (event month relative to program implementation), an indicator for positive values of the running variable, an interaction between the running variable and the discontinuity indicator, and fixed effects for the month that the individual began AIT. The monthly pay data is restricted to the individuals for whom this data was not missing (n=32,456 in full sample; n=14,629 in control); the median household income is restricted to the individuals for whom this data was not missing (n=13,583 in full sample; n=11,423 in control). The regressions in Panel B are all for the credit records that are matched in the individual's first year, beginning with AIT. Heteroskedasticity robust standard errors, clustered at the AIT location-month level, are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

2.7. ROBUSTNESS CHECKS

In this section I complete a series of robustness checks to validate the experimental validity, estimated effects and interpretations presented in Sections 2.1 through 2.6. Given the series of robustness checks in Chapter 1 that validated the quasi-experimental setup used in this analysis, I forego the analyses presenting regression estimates with and without covariates, balance of covariate event studies and predicted outcome event studies. The randomization test results in Section 2.6 validate the experimental design used in this analysis and subsume these omitted analyses. I focus instead on two robustness checks: comparing the estimated effects using the full matched sample and the subsample for individuals with active credit and functional form validation for my regression discontinuity estimates.

2.7.A. PFMC Effects by Matched Sample vs. Active Credit Subsample

In Section 2.4 I explained the rationale for imputing zeros for the balances and account trades for individuals with matched but inactive credit records. To demonstrate that the results are not driven by inclusion of these records, in Table 2.12 I present my estimates for the three relevant outcomes (omitting the probability of an active record and the credit score, since imputation was impossible for these outcomes) for the full matched sample (n=33,931) and the matched sample with active records (n=29,318).

As the estimates reveal, the findings hold for both samples. The findings are generally larger for the active sample but the control group means are also larger, suggesting roughly similar effect magnitudes (for example, 10% for the full sample cumulative balance effect vs. 12% for the active sample cumulative balance effect and 16% for the full sample aggregate monthly payment effect vs. 18% for the active sample payment effect). The statistical significances are also stronger in the active credit sample. Overall, these results demonstrate that the findings hold for the full matched sample and the active credit sample.

Table 2.11. Effects of PFMC on Financial Outcomes in Year 1, by Sample

	(1)	(2)		(3)	(4)		(5)
Outcome Variable	Probability (Active Credit)	Cumulative Balance		Adverse Legal Action Index	Aggregate Monthly Payment		Credit Score
Panel A: Full Sample (Matched Record)							
PFMC Effect	1.20	-585.43	*	0.002	-28.03	**	-5.47
Std Err	(1.99)	(334.66)		(0.039)	(11.79)		(3.76)
Control Mean	54.64	6,117		0.193	178.62		577.50
Adj R^2	0.1764	0.4516		0.5341	0.3795		0.4349
N	33,391	33,931		33,931	33,931		29,318
Clusters	257	257		257	257		256
Panel B: Active Credit Sample (Matched and Active Record)							
PFMC Effect	N/A	-807.23	**	0.007	-37.61	***	-5.47
Std Err		(381.79)		(0.046)	(13.06)		(3.76)
Control Mean		6,969		0.217	205.91		577.50
Adj R^2		0.4316		0.5290	0.3594		0.4349
N		29,318		29,318	29,318		29,318
Clusters		256		256	256		256

Source: Department of Defense, Census Bureau and Credit Bureau Data. Notes: Sample as defined in the text. Regression specifications as described in Table 2.5. Heteroskedasticity robust standard errors, clustered at the AIT location-month level, are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

2.7.B. Regression Discontinuity Functional Form

In Table 2.12 I present my regression estimates for the main outcome of interest, the cumulative balance in year 1, by functional form. Here the estimates vary by form and the average of all six estimates is -$595.

Table 2.12. RD Estimates of the Effects of PFMC on Cumulative Balance in Year 1, by Functional Form										
	Linear		Quadratic	Cubic	Local Linear					
					BW=4 Mo		BW=6 Mo	BW=8 Mo		
Form:	(1)		(2)	(3)	(4)		(5)	(6)		
Panel A: Cumulative Balance										
PFMC Effect	-585	*	-172	-145	2,888	**	-2,812	-2,743		
Std Err	(335)		(583)	(793)	(1,399)		(2,257)	(2,379)		
Control Mean	6,117		6,117	6,117	6,117		6,117	6,117		
Adj R^2	0.4516		0.4516	0.4516	0.5245		0.4961	0.4812		
N	33,931		33,931	33,931	3,897		10,921	18,438		
Clusters	257		257	257	53		105	157		

Source: Department of Defense, Census Bureau and Credit Bureau Data. Notes: Sample as defined in the text. Regression specifications as described in Table 2.5. Heteroskedasticity robust standard errors, clustered at the AIT location-month level, are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

2.8. EXTERNAL VALIDITY

In Section 1.8 I present a detailed discussion of the external validity of the PFMC. One additional consideration for the external validity of the course in the context of credit outcomes is the potential importance of the Servicemembers' Civil Relief Act (SCRA). Since the SCRA provides unique benefits to individuals joining the military, similar lessons and potential benefits might be unavailable in other financial education programs, especially the desirable protection of a maximum interest rate of 6% on existing debts. For this population, the integration of financial education efforts with consumer awareness information and practical assistance (how to file for SCRA protection) is undoubtedly appropriate and, based on the empirical estimates here, potentially valuable. To the extent that there are other consumer protection laws that apply to different student populations, perhaps one goal of these programs should be to educate students about their rights and assist them, to the extent possible, in obtaining the appropriate legal protections and available benefits. In this sense, financial education may be most successful when the awareness inherent in human capital development is combined with practical assistance to overcome behavioral tendencies to procrastinate or otherwise discount the future.

2.9. SUMMARY

This paper estimates the effects of financial education on economic behavior. I employ a natural experiment in the U.S. Army that implemented a mandatory 8 hour personal financial management course (PFMC) in a quasi-experimental manner. Previous research has shown that this course has large, pervasive and persistent effects on retirement savings but the mechanism for these effects is unclear. In this paper I use administrative data and individually matched credit data to estimate the effects of financial education on a variety of financial outcomes including credit scores, credit balances for several types of accounts, monthly payments and adverse legal actions. In some areas I find that the PFMC has positive effects, reducing cumulative account balances (especially for automobile and credit card accounts) and aggregate monthly payments. In other areas, including credit scores, the probability of being active in the credit market and the number of adverse legal actions, the PFMC has no statistically significant effects on financial behavior.

While one of the principle goals of this research was to determine if the beneficial effects of financial education operated via human capital development, behavioral assistance or a combination of these two mechanisms, the existing data does not provide conclusive evidence on this question. In the context of retirement savings and more general financial behavior, it appears that the PFMC's benefits are the result of a strategic combination of education and assistance. From an academic perspective this is disappointing in that it leaves open the unanswered question of the “teachability” of financial literacy. From a policy perspective, however, this research demonstrates in the largest experiment to date, the significant and lasting effects of carefully designed financial literacy education that is actionable in its content and methods.

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Appendix 2.1
Summary of Matching Process for Administrative and Credit Data

This appendix summarizes the results of the merge of military administrative and credit bureau data.[14] [15] Figure 2.3 depicts the results of the merge for Year 1 outcomes.[16]

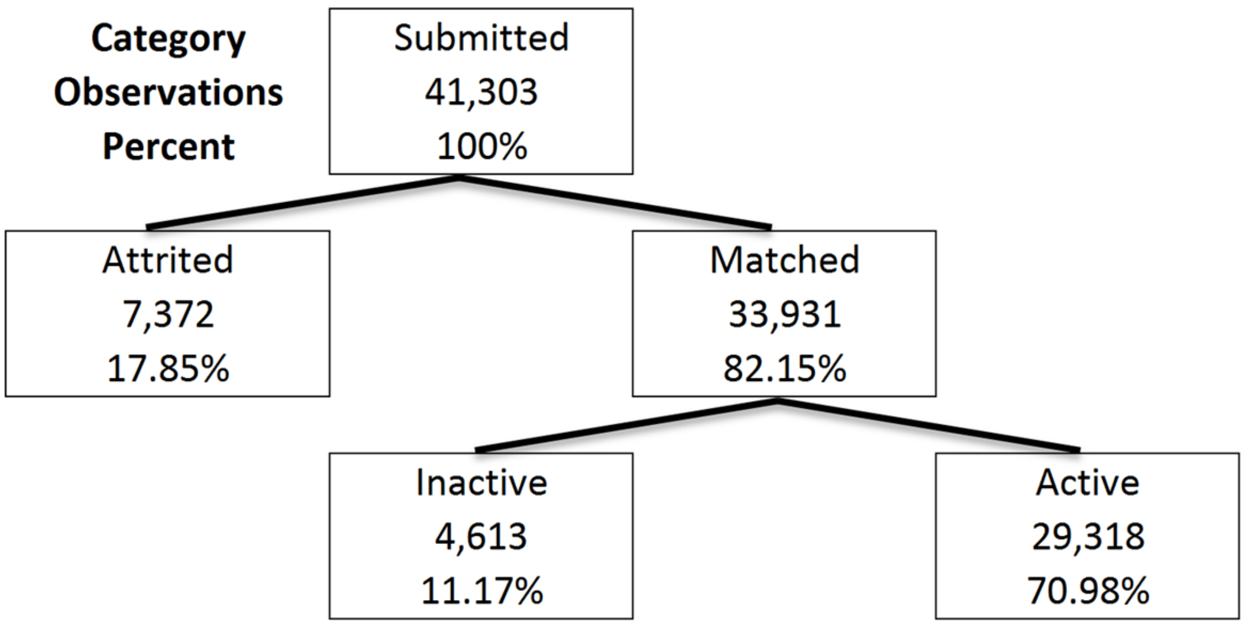

Figure 2.3. Sample Sizes and Match Statistics for Year 1 Credit Outcomes
Author compiled data based on the results of the individual-level data merge of Army administrative data and credit bureau data.

The merge results are largely successful: 82.15% of submitted observations were matched by the credit bureau in Year 1. Individuals in the "Attrited" category were not matched based on the credit bureau's search algorithm and the Army administrative data. Individuals in the

[14] For more information on the sample selection and merge process, see the first footnote in Appendix 1.1.

[15] The data submission, security and matching process were handled by authorized DOD and credit bureau personnel. After initial coordination and price negotiation by the author, Army personnel completed and supervised the contract creation, legal review, contract signing, data security protocol establishment and data submission. Credit bureau personnel then matched the data and returned the matched sample without any personally identifying information. The process took approximately 5 months.

[16] The match rates vary for each potential outcome year since individuals may enter or leave the credit market. Similar statistics are available for the Year 2 and Year 3 outcomes from the author upon request.

“Inactive” category were matched but have no credit score or detailed data available due to infrequent transactions and activity. Individuals in the “Active” category were matched and have all items of the credit bureau data available for the appropriate period. Attrition is unrelated to treatment (p=0.6974), validating the use of matched individuals as representative of the full sample. Being active in the credit market is an outcome of potential interest and tested above in Section 1.3.

Appendix 2.2
Complete Regression Results for the PFMC Effects on Cumulative Balance in Year 1

Table 2.13. Complete Regression Results for PFMC Effects on Cumulative Balance in Year 1

Panel A: Parameter Estimates

Variable	Coefficient	(Std Err)	p-value	Significance
PFMC Effect (Discontinuity)	-585.43	(334.66)	0.081	*
Cohort Month	-152.56	(135.68)	0.262	
PFMC*Cohort Month	-0.26	(35.11)	0.994	
Age	-120.69	(243.28)	0.620	
Age2	2.25	(5.26)	0.669	
Experience	-83.46	(92.83)	0.369	
Experience2	1.20	(5.49)	0.828	
Female	-185.92	(138.49)	0.181	
Married	1,425.28	(143.10)	0.000	***
Number of Dependents	374.02	(45.65)	0.000	***
Education ≤ High School Graduate, %	-355.31	(382.94)	0.354	
Education = High School Graduate, %	481.02	(206.33)	0.021	**
Education = Some College, %	515.04	(482.20)	0.286	
Education ≥ College Graduate, %	80.73	(78.92)	0.307	
Minority, %	-19.44	(2.27)	0.000	***
AFQT Score, Percentile	171.79	(157.47)	0.276	
Enlistment Term Length, Years	179.29	(51.89)	0.001	***
Monthly Basic Pay, $	1.29	(0.15)	0.000	***
Socioeconomic Status	-0.0092	(0.0036)	0.011	**
Months Deployed	-96.17	(20.16)	0.000	***
Credit Score in Year 0	0.8980	(0.1715)	0.000	***
Cumulative Balance in Year 0	0.71	(0.01)	0.000	***
Missing Pay Indicator	789.13	(333.13)	0.019	**
Missing HH Income Indicator	-108.52	(258.68)	0.675	
Missing Year 0 Cumulative Balance Indicator	-307.58	(89.72)	0.001	***
AIT Length, Months	1,265.92	(2,343.42)	0.590	
Constant	579.17	(5,624.87)	0.918	

Panel B: Regression Statistics

Observations	33,931		Adjusted R^2	0.4516
Clusters	257		F-Statistic	372.79

Source: Department of Defense, Census Bureau and Credit Bureau Data. Notes: Sample as described in Table 2.3. The table reports the regression coefficients for all parameters except the fixed effects, specifically, time (month) fixed effects, location fixed effects, job category (branch) fixed effects and job specific fixed effects. The omitted category for education is those missing education data. Heteroskedasticity robust standard errors, clustered at the AIT location-month level, are reported in parentheses. ***, **, * represent statistical significance at the 1%, 5% and 10% levels respectively.

CHAPTER 3

ESTIMATING THE EFFECTS OF STRESS ON FINANCIAL DECISION-MAKING: EVIDENCE FROM U.S. ARMY DEPLOYMENTS AND THE SAVINGS DEPOSIT PROGRAM[1]

[1] An earlier version of this paper was presented at 2011 Western Economic Association International Conference Session on Effects of Deployment on Health, Families, and Earnings.

Estimating the Effects of Stress on Financial Decision-Making: Evidence from U.S. Army Deployments and the Savings Deposit Program

April 2012

ABSTRACT

Using a natural experiment, I estimate the effects of the stress of combat casualties in a U.S. Army unit on individuals' participation in the Savings Deposit Program (SDP), a no-risk 10% annual percentage rate (APR) savings account. I find a modest and statistically significant negative relationship between the stress of casualties and SDP participation on the order of 5%. While the natural experiment is imperfect, robustness checks in reduced samples suggest that this relationship holds for a variety of specifications and is not driven by non-random variation in individual characteristics. I find no differential effects of stress by gender, human capital levels, or length of deployment but I do find large negative and significant interaction effects between stress and being married. The confounding effects of overall activity levels and rural locations cannot be eliminated and as a result, the large scale field evidence presented here should be considered suggestive evidence of the adverse effects of stress on financial decision-making.

William Skimmyhorn

Harvard Kennedy School

79 JFK Street

Cambridge, MA 02138

bill_skimmyhorn@hksphd.harvard.edu

The President authorized a 10 per cent interest rate on Uniformed Services Deposits for all servicemen, including officers, who are stationed overseas.

Think of it! That's a return of 10 cents on every dollar for every year that you remain overseas. Few people, other than stock market speculators, have ever received such return on their money, and certainly no one has ever obtained it with such high degree of safety in their investment...

So, you save and your country benefits as well. Who could ask for a more pleasant prospect? Why not check into the Uniformed Services Deposits program today? Ask your finance officer about it.

–American Forces Network Bulletin
September 9, 1966[2]

3.1 HOW DOES STRESS AFFECT INDIVIDUAL SAVINGS DECISIONS?

This paper estimates the effects of stress on individuals' financial decisions. Using a natural experiment, I estimate the effects of the stress of combat casualties in a U.S. Army unit on individuals' participation in the Savings Deposit Program (SDP), a no-risk 10% annual percentage rate (APR) savings account. I find a modest and statistically significant negative relationship between the stress of casualties and SDP participation on the order of 5%. While the natural experiment is imperfect, robustness checks in reduced samples suggest that this relationship holds for a variety of specifications and is not driven by non-random variation in individual characteristics. I find no differential effects of stress by gender, human capital levels, or length of deployment but I do find large negative and significant interaction effects between stress and being married. The confounding effects of overall activity levels and rural locations cannot be eliminated and as a result, the large scale field evidence presented here should be considered suggestive evidence of the adverse effects of stress on financial decision-making.

For the purposes of this paper, a clinically precise definition of stress is not employed. Instead I will use a general formulation that defines stress as the reaction of an individual to an event (a stressor) to try to restore homeostasis. This definition draws on the seminal work of Hans Seyle (1936, 1975) and reflects the generally accepted definition used by the American

[2] This passage is a reproduction of an American Forces Network (AFN) information bulletin. Available at: http://www.25thida.com/TLN/tln1-29.htm. Accessed: 13 Dec 2010. The Uniformed Services Deposit Program is now called the Savings Deposit Program and is the financial instrument used in this research.

Institute for Stress, which defines stress as, "physical, mental, or emotional strain or tension."[3] When the distinction of a stressor (i.e., noise, crowding, or the casualty statistics) from stress (a physiological response) is required then I will use the more appropriate term. However, the terms will often be interchanged in this paper as they are elsewhere in the economic literature.

This research proceeds as follows: Section 3.2 reviews the existing literature and summarizes the key contributions of this work. Section 3.3 briefly reviews the existing theoretical models relating to stress and decision-making. Section 3.4 provides a brief summary of the Savings Deposit Program and its relevant provisions. Section 3.5 summarizes the data employed in the empirical analysis. Section 3.6 presents the empirical analysis and results. Section 3.7 conducts robustness checks. Section 3.8 summarizes the findings, presents policy recommendations and concludes.

3.2 A BRIEF SUMMARY OF THE LITERATURE ON STRESS AND DECISION-MAKING

Review of Existing Literature

This research contributes to the literature in economics, psychology and public policy. The topic (the effects of stress on decision-making) should be of interest to psychologists, economists and marketers; the policy instrument (a high yield government savings account) should be of interest to economists, government officials and citizens; and the effects on the population under study (deployed U.S. Army personnel) should be of interest to academics in a variety of fields, government officials, health care providers, military leaders and all citizens.

[3] The American Institute of Stress website discusses the definitional challenges of the term stress but provides this commonly accepted version. See: http://www.stress.org/Definition_of_stress.htm. Accessed: 14 Dec 2010.

This section reviews the relevant existing literature and identifies the contributions of the current research.

The first literature is the long-standing investigation by psychologists and health professionals into the effects of stress on individuals. This is a large literature with a number of important findings. What follows is a relatively brief review of the effects of stress on various performance measures. Laboratory results suggest adverse effects of stress on a variety of behaviors.[4] For a review of documented stressors and their effects, see Cohen (1980). Research on stress in more natural settings has also demonstrated a number of negative effects on cognitive and behavioral outcomes.[5]

One potential reason for a lack of more varied and detailed findings on the effects of stress is the nature of Human Subjects Research guidelines in the social sciences. If perceptions of control over a stressor reduce the effects of the stressor (Glass, Singer and Friedman (1969)

[4] A variety of stressors have been found to affect individuals including noise (Glass, Singer and Friedman (1969), Shaham, Singer and Schaeffer (1992)), stressful instruction sets (Shaham, Singer and Schaeffer (1992)), increased workload and time stress (Wickens et al. 1993), task inducements such as giving a speech (Preston et al. 2007), cold-pressors (Porcelli and Delgado (2009)), and a combination of multiple stressors (Wickens et al. (1993)). All have been shown to inhibit individual performance including reduced capacity to direct attention (as measured on the Stroop task), reduced performance on proof-reading tasks, and reduced tolerance for frustration. It is worth noting that the findings on these stressors are not universal. For example, Shanteau and Dino (1993) find no negative effects of stress (noise, heat, crowding and schedule interruptions) on verbal behavior or on most decision-making processes. They do however find negative effects on creativity measures. In spite of their findings, the results above serve as accepted evidence that stressors can affect individual behavior and decision-making.

[5] The adverse effects found in natural settings include; reduced reading ability among children (Cohen, Glass and Singer (1973)), reduced ability to concentrate among children (Cohen, Glass and Phillips (1979)), drug relapse (Niaura et al. (1988)), a relationship between perceived stress levels and smoking (Cohen & Lichtenstein (1990)), narcotic (alcohol, drugs or smoking) use and relapse (Hall, Havassy and Wasserman (1990), Shiffman and Waters (2004)), reduced performance on tasks with spatial demands (Wickens et al. (1993)), reduced self-control in constraining eating (Ward and Mann (2000)), absenteeism at work (Kim and Garman (2003)) and reduced performance by individuals and teams in medical settings (LeBlanc (2009)). While not all of these studies are experimental in nature, there exists significant evidence of the adverse effects of stress on individual behavior in a variety of task domains and situational environments.

and Cohen (1980)), then research protocols that require informed consent may generate non-findings of the effects of stressors on outcomes of interest.[6] These regulatory effects, or the perceptions of their existence, pose challenges for researchers in these fields and may reduce researchers' interests in pursuing stress-related research. While there are many researchers working on stress-related topics in spite of these restrictions, which seem appropriate in nearly all cases, there still may be implications for the findings in this literature. As long as informed consent requirements generate some perceptions of control over the stressors by research subjects, the current body of laboratory research ought to serve as a lower bound of the estimates of the effects of stress on the behaviors and measures in question, since without these perceptions of control individuals would likely experience more stress and exhibit reduced performance. These potential limitations of laboratory research also serve as a justification for using more natural experiments and naturalistic observational studies where human subjects requirements do not apply.

This research also contributes to the equally mature but evolving investigation by economists into individual decision-making processes and individual savings and consumption choices. This literature has at its roots the classical approaches to the study of individual consumption and savings decisions embodied in Friedman's Permanent Income Hypothesis (Friedman (1957)) and Modigliani's Life Cycle Hypothesis (Ando and Modigliani (1963)).[7] Both hypothesize that individuals (or households) choose consumption and savings levels to maximize their future utilities. The relevant portion of income for these decisions is an

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[6] For more detail on this possibility, see Gardner (1978).
[7] For a helpful review of these two theories see the announcement for Modigliani's 1985 Nobel Prize at: http://nobelprize.org/nobel_prizes/economics/laureates/1985/press.html. Accessed 17 Dec 2010.

individual's expectations about present and future permanent income. The models differ primarily in their time horizon; Friedman assumes an infinite horizon which includes planning for future decedents and Modigliani assumes a finite horizon confined to an individual's own life. Together these models serve as the rational and neoclassical economic benchmark against which more modern behavioral models of decision-making and choice have been judged. While this research will not settle this debate it will contribute to our understanding of the effects of stress on individual savings decisions, a phenomenon not recognized by the classical approach.

While the neo-classical approaches to decision-making and consumption-savings choices were predominant throughout the 1960s, 1970s and 1980s, modern economic approaches have questioned these approaches. Economists have increasingly modeled individual decision-making and choice as an interaction between different motivations and different processes in individuals' minds. Some of these processes are cognitive, rational and deliberative and others are emotional, impatient and motivational. These "two-mind" models are now common in the economics literature.[8] While each of the popular "two-mind" models has something to offer, the *Affective vs. Deliberative* model of Loewenstein and O'Donoghue (2005) will be used in this analysis because of its clear formulation of the underlying processes, its generation of well-specified testable hypotheses and its inclusion of stress as a factor in individual choice.

[8] Prominent examples in include those postulated as *Hot vs. Cool* (Metcalfe and Mischel (1999)), *Affective vs. Cognitive* (Shiv and Fedorikhin (1999)), *System 1 vs. System 2* (Frederick (2002)), *Automatic vs. Control* (Benhabib and Bisin (2004)), *Affective vs. Deliberative* (Loewenstein and O'Donoghue (2005)), and *Impulsive vs. Patient* (Fudenberg and Levine (2006)). They have much in common, including acknowledgement of a non-unitary decision making process for individuals, conceptualization of decision-making as a game played between an individual's present and future period selves, appreciation of the contributions of psychology to the improvement of economic theory and models, and commitment to empirical evaluation of these models in laboratory and natural settings.

Unfortunately, while the models reviewed above have generated plausible and persuasive explanations of individual choice, they suffer from two significant shortcomings. First, their empirical record is incomplete. To be sure, nearly all of these works cite some empirical evidence that supports their conceptualization but few produce prospective hypotheses and empirical tests, instead relying on ad hoc and retrospective reviews of existing economic conditions. The three most widely cited pieces of evidence for these “two-mind” models are: Shiv and Fedorikhin (1999), who find that individuals under higher levels of cognitive load (memorizing seven digit numbers vs. two digit numbers) choose more affective food options (chocolate cake vs. fruit salad) when the presentation mode is real; Vohs and Heatherton (2000) who find that self-control is a limited resource that is depleted by a previous disciplined choice; and Ward and Mann (2000) who find that cognitive load undermines self-control among restrained eaters (dieters) but not among non-restrained eaters. But these findings provide no evidence on the relative contributions of each mental system and the conditions under which each system determines choice. Moreover, these empirical tests have not demonstrated that individual choice is an outcome of an intra-mind conflict.

Second, there is limited evidence of the effects of stress on economic decision-making and to the author’s knowledge, no existing evidence in the economic literature on the effects of stress on individual consumption-savings decisions. None of the evidence for the economic “two-mind” models discussed above includes evidence of the effects of stress on choice. But as the review of the psychology literature above demonstrates, there are some relevant findings for the effects of stress on elements of economic decision-making, including risk-preferences, learning and consumption choices.[9] The above findings clearly demonstrate that stress can affect individuals' economic decisions and outcomes, but none relate to the effects of stress on the consumption-savings decision. Since this decision is one of the most basic and most frequent economic choices that an individual makes and since it lies at the heart of many micro and macroeconomic models, further empirical work on this question is needed.

These first two literatures (psychology and economics), while separate for most of their history, are now converging in part due to the expanded interest among economists in behavioral models as a means of explaining individual choice and the advances of neuroscience and neuroeconomics in specifically identifying the physical locations and physiological processes involved in choice. While this convergence holds great promise for human understanding of individual choice and judgment, there remains a great deal of theoretical and empirical work to be done.

The final literature that this research serves to inform is the economic, health and policy literature devoted to analyzing the effects of stress and military service on military members. Since the 1940s the military and other offices of the U.S. government have been actively engaged in researching the behavioral effects of stress on its members in order to better understand these effects and find ways to reduce them. This line of research has focused

[9] First, stress produces a change in risk preferences (Porcelli and Delgado (2009)) by increasing risky behavior in the loss domain and reducing risky behavior in the gains domain, though other evidence finds no differences in risk preferences under stress (Shaham, Singer and Schaeffer (1992)). Second, stress correlates with risk preferences among London stock traders but does not predict profitability (Coates and Herbert (2008)). Third, stress slows learning on the Iowa Gambling Task (IGT) (Preston et al. (2007)). Fourth, stress generates differential gender performance on the IGT, with women performing better when stressed and men performing worse (van den Bos, Hartveld and Stoop (2009)), though other evidence found insignificant differential gender effects (Preston et al (2007)). Fifth, stress is correlated with increases in consumption (Lee, Moschis and Mathur (2001)).

primarily on the effects of stress on job performance with its principle concern being improving military capabilities and readiness. This research has consistently found negative effects of stress on job performance.[10] Since that time, the military has continued to research the effects of stress on its members. While the primary goal of military and government-sponsored research has been to improve military capabilities, a secondary concern of evaluating service members' health and welfare during and after their service has also emerged. As a prominent example, consider the economic assessment of the effects of military service during Vietnam on individuals' post-service earnings (Angrist (1990)).

Modern research on the non-military health and welfare effects of the stress of military service has dealt primarily with the causes, consequences and costs of Post-Traumatic Stress Disorder (PTSD). For a detailed discussion of PTSD, see Krueger (2008) and Harrison, Satterwhite and Ruday (2010). Since most recent research has focused on the effects of stress as it relates to military performance or on the extreme health effects of stress associated with PTSD, there remains a gap in our understanding of the effects of stress on the health and welfare of individuals in less than the most extreme cases.[11] This is a large topic area and will include the economic and social welfare of individuals as well as their health outcomes during and after deployments, during and after military service and the effects of military service on members' families and friends. To date there has been limited work in this area, though the

[10] Significant early findings were those by Shaffer (1947), who found that pilots reported reduced performance in combat as a result of stress; and Grinker and Spiegel (1947) who found that combat stress produces battle-fatigued soldiers and that psychological breakdowns are often associated with individual feelings of inadequacy. For a longer review of early findings of the effects of stress on military members, see Lazarus, Deese and Osler (1952). Academic research has often worked alongside this effort and produced findings related to stress and military performance and military service more generally (Harris, Ross and Hancock (2008)).

[11] For a notable exception to this gap, see Dineen, Pentzien and Mateczun (1994) who analyze the effects of predepolyment stress on the crew of a U.S. navy hospital ship. Even they note that "surprisingly" little has been written about precombat and deployment stress.

exogenous nature of many military decisions to its members presents many opportunities for studying individual outcomes with scientific approaches. This work contributes to this literature by evaluating the effects of the stress from one aspect of military service (deployments) on individual's financial decisions and outcomes. To the author's knowledge this study represents the first evaluation of the conditions of military deployments on the economic decisions and outcomes of service members.

Summary of Contributions

As the review highlights, this research locates itself at the nexus of several interesting and traditionally distinct literatures. First, this research informs the psychology literature on the effects of stress by estimating the effects of stress on individuals' personal financial decisions in a natural setting. Second, this research serves as one of the first and most significant empirical tests of economic "two-mind" models of decision making and, to the author's knowledge, the first empirical evaluation of the effects of stress on individuals' consumption-savings decisions. Finally, this research estimates the effects of the conditions of military deployments on individuals' financial choices with important economic outcomes.

3.3. THEORETICAL REVIEW & HYPOTHESES DEVELOPMENT

This section summarizes the relevant decision-making theory and presents the hypotheses that will be empirically tested. As mentioned above, while several theoretical models commonly referenced in the economics literature propose a "two-mind" model of choice, one of the clearest and most detailed is Loewenstein & O'Donoghue (2005). In this model, individual behavior is the outcome of an interaction between the distinct deliberative and affective systems. While both systems interact, the affective system has primacy for most decision-making tasks. In addition, the proximity of a stimulus plays a key role in determining the degree to which the affective system is activated. While the affective system enjoys primacy, it is not always dominant and the deliberative system can override the affective system through the exercise of willpower. This process is dependent on the mental and emotional state of the individual though, as both stress and cognitive load can undermine the deliberative system and reduce willpower. As a result, individuals make choices by balancing the desirability of an action and the associated costs of willpower. The critical predictions of the model for this research are that increased stress or higher cognitive loads will move behavior further from the deliberative optimum.

Implicit in this model is the ability to define the optimal behavior. In the case of Shiv and Fedorikhin (1999) the optimal (deliberative) choice is fruit salad, whereas the non-optimal (affective) choice is the chocolate cake. For the purposes of this research, participation in the SDP (a risk-free 10% APR) is assumed to be the optimal choice, based on its economic dominance of financial instruments comparable in risk, maturity and liquidity. Non-participation in the SDP is therefore the affective choice. Section IV will explain this dominance in more detail.

Recall that this analysis assumes that casualty statistics generate stress among deploying soldiers. The precise nature of this stressor (the casualty statistics), the stress generated (the individuals' responses) and their measurement are described in more detail in Section VI. For now, I assume an increasing monotonic relationship between knowledge of casualties and individual stress. With the optimal and non-optimal behaviors identified and the instrument of stress specified, I now formulate my primary hypothesis based on the Deliberative/Affective choice model summarized above.[12]

- **Hypothesis 1: Individuals with higher initial stress levels will exhibit lower SDP participation.[13]**

That is, individuals who experience higher casualty rates in their unit in their first month of a deployment will have lower probabilities of SDP participation.

3.4. THE SAVINGS DEPOSIT PROGRAM IN BRIEF

The financial instrument used in this research to examine individuals' decision making is the Savings Deposit Program (SDP). The SDP was established by President Johnson on August 14, 1966 through Executive Order 11298 with the stated goals of offering deployed service members a way to earn extra money while deployed to Southeast Asia and as a means of reducing the U.S. balance of payment deficit.[14] In conjunction with Section 1035 of Title 10, the program provides a 10% annual percentage rate (APR) of return on deposits of up to $10,000 for members armed forces deployed in eligible locations. President George W. Bush authorized SDP participation for members of Operation Enduring Freedom in Afghanistan in November of

[12] These hypotheses are also derived in Loewenstein and O'Donoghue (2005) in a more general setting. These predictions are not original to my work, but I aim to test their theory using a natural experiment.

[13] Initial levels of stress will be measured using the casualty statistics from the individual's month of arrival.

[14] See http://www.archives.gov/federal-register/codification/executive-order/11298.html for the actual text of the Executive Order.

2001 and Operation Iraqi Freedom in February of 2003.[15] These operations cover the vast majority of deployed soldiers today and represent a sizable population to study.

The returns on deposits are compounded quarterly, guaranteed by the Federal Government, and disbursed via the U.S. Treasury and the Defense Finance and Accounting Service (DFAS). The 10% APR can be earned on any deposits made once an individual has been deployed for at least 30 consecutive days and deposits can remain in the SDP account earning interest up to 90 days after an individual returns home. Returns on the SDP are taxable income. Deposits are relatively illiquid for the one year period, though individuals can withdraw their money prior to their return if they are facing economic hardship. Deposits can be made via check or payroll deduction.[16] The program is well established and appears to be well-known. Military members are advised of the SDP at their home stations, in pre-deployment training and readiness certifications, in transit to their country of deployment, in military periodicals, on military benefits-related websites and upon arrival in an eligible location.[17]

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[15] For more detailed information on the SDP, see a useful Fact Sheet at: http://www.usarpac.army.mil/SoldierFamilyWellBeing/Reintegration/USARPAC%20SAVINGS%20DEPOSIT%20PROGRAM%20FACT%20SHEET.htm or the Defense Finance and Accounting Service (DFAS) website at: http://www.dfas.mil/militarypay/woundedwarriorpay/savingsdepositprogramsdp.html

[16] The maximum monthly deposit for an individual participating by payroll deduction is equal to their unalloted pay and allowances. This will vary for a given individual based on their rank and special pays. For most enlisted soldiers, this provision would limit their monthly deposit to $1,400-5,000 and require 2-7 months to obtain the maximum deposit of $10K. For most officers this provision would limit their monthly deposit to $2,700-5,700 and require 2-4 months to obtain the maximum deposit. However, individuals do not need to achieve $10K to participate and the 10%APR accrues on all deposits made. See the current military pay scale at: http://www.dfas.mil/militarypay/militarypaytables.html . Accessed 20 Dec 2010.

[17] The most common periodicals that Army soldiers have access to while deployed are the *Stars & Stripes* and *The Army Times*. Both feature financial readiness sections and letters to the editor which routinely discuss the SDP, among other finance related topics.

The SDP is a unique instrument for at least two reasons. First, it is only available to selected members of the Armed Forces when they are deployed to designated combat zones. These restrictions aid the research program by ensuring treatment conditional on deployment. Second, this short-term (1-year) instrument enjoys a risk-free guaranteed 10% APR which is superior to any available financial product with comparable time horizon and liquidity. In Figure 3.1 I present a comparison of the rates of return offered by the SDP and comparable financial instruments during the period 2001-2010.

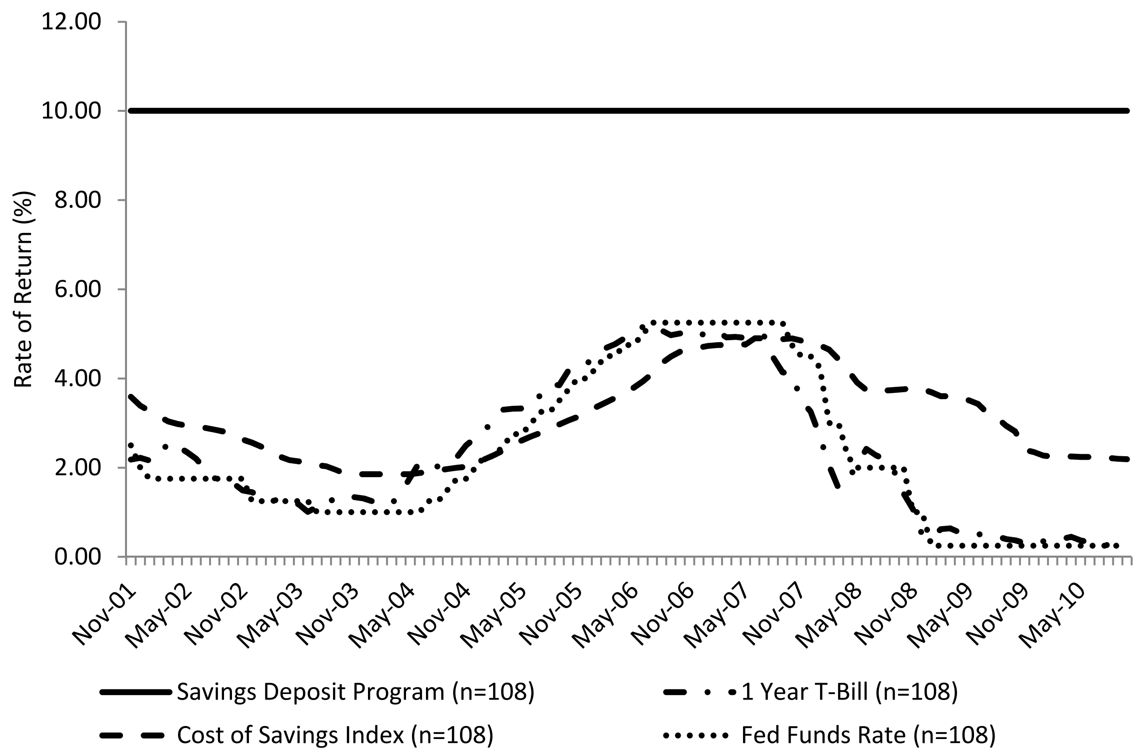

Figure 3.1. Comparison of Economic Rates of Return, 2001-2010

Economic rates of return for short-term risk-free financial products available from Nov. 2001 through Dec. 2010. Data depicted available from publicly available sites maintained by the Federal Reserve, Mortgage-x.com and the Department of Defense. The SDP rate of return is legally mandated at 10%. The cost of savings index is based on the interest rates Wells Fargo (later, Wachovia) pays to individuals on certificates of deposits. The federal funds rate is the rate at which depository institutions lend balances to each other overnight. n=432.

3.5. SUMMARY OF DATA SOURCES

The data used in this analysis comes from a combination of several governmental and non-governmental sources.[18] The data set covers the period October 2001- December 2010. The administrative data on military members comes from U.S. Army and Department of Defense databases. The demographic data were provided by the Total Army Personnel Database. Individual pay data and SDP participation data were provided by the Defense Finance and Accounting Service. Military operational deployment data was provided by the Army's Office of Economic and Manpower Analysis. The data for the rates of return for alternative financial instruments were obtained from several sources. The Federal Reserve website provided data on the returns for 1-Year T-Bills and the intended Federal Funds rate.[19] Mortgage-x.com provided data on the rates of return for the Wells Fargo Cost of Savings Index.[20] In Figure 3.1 I present the rates of return for several comparable financial instruments from 2001-2010. As the graph shows, the SDP is a superior financial instrument to all comparable products throughout the time period of this analysis. It offers eligible individuals a high rate of return with no risk at a limited liquidity cost.

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[18] The data was obtained through the cooperation of the Office of Economic & Manpower Analysis (OEMA) at the United States Military Academy, West Point, NY. All personally identifying information has been removed from each observation so as to protect the anonymity of Army members.

[19] 1 Year T-Bill Data: http://www.federalreserve.gov/releases/H15/data/Monthly/H15_TB_Y1.txt Accessed: 13 DEC 2010. Fed Funds Target Rate Data: http://www.federalreserve.gov/monetarypolicy/openmarket.htm Accessed: 13 DEC 2010. To see the data in a more usable form, see: http://www.moneycafe.com/library/fedfundsrate.htm Accessed 13 DEC 2010. Note that since November 2009 the Federal Funds Target rate has been 0.00-0.25%. For this analysis I assumed that the rate was 0.25%.

[20] The Wells Fargo Cost of Savings Index (COSI) is based on the interest rates paid by Wells Fargo on CDs held by its depositors. It reflects the interest rate that WF is paying to individuals for CDs. Wells Fargo/Wachovia COSI Data is available at: http://www.mortgage-x.com/general/indexes/default.asp Accessed 26 OCT 2010.

Independent Variable of Interest (Stress) Data

The primary purpose of this research is to estimate the effects of stress on individual's financial decision-making. While no data exists that directly measures the physiological stress levels of deployed military members, reasonable instruments for this omission exist. Specifically, this analysis will use military casualty data as a measure of the stress that individuals arriving in a combat zone (Iraq or Afghanistan) face. I employ Department of Defense casualty data aggregated for each Army unit in each month of the sample period. The relationship between casualties and stress seems straightforward; higher levels of casualties in a country indicate a more dangerous situation for individuals serving there and will increase the stress placed upon arriving members.

3.6. ESTIMATING THE EFFECTS OF STRESS ON SAVINGS DECISIONS

Identification Strategy

Identification of an unbiased estimate of the effects of casualties on savings decisions requires random assignment of individuals to the control or treatment group. The natural experiment used here relies on random assignment of individuals to units once I condition on an individual's military occupation specialty (job category), their rank (experience) and the year. This identification requires an assumption of unconfoundedness, more commonly known as selection on observables. U.S. Army and Department of Defense Policy personnel and assignment policies support this assumption in that individuals are assigned to units (and hence locations) based on "the needs of the Army" and not individual characteristics or preferences

(Department of Defense (2005)). This assumption has been validated in the social science literature where the use of military assignment and movements is often used as an instrumental variable for evaluation of other parameters of interest, including the effects of parental absence on children's educational outcomes (Lyle (2006)), the effects of access to payday lending (Carrell & Zinman (2008)), differential treatment by race (Antecol & Cobb-Clark (2008)) and the health effects of pollution on children (Lleras-Muney (2012)). Once individuals are conditionally randomly assigned to a unit, they are assigned to the control or treatment group based on the exogenous variation in several important deployment characteristics including the deployment country (Iraq or Afghanistan), location within a country (base camps, forward operating bases, combat outposts, etc...), timing of arrival (periods of combat operations, election seasons, holidays, mission "surges," etc...), missions assigned once in a location (base defense, counterinsurgency, resupply convoy movements, garrison staff work, reconnaissance, etc...) and the level and type of enemy activity (large scale engagements, ambushes, sniper fire, improvised explosive devices, suicide bombings, etc...). In summary, there are a number of factors suggesting that the casualty rates of units are orthogonal to the observed characteristics of the individuals in the unit, conditional on their job, experience level and year. I preform randomization and robustness checks below to assess the viability of this assumption and the impact of randomization failures.

In addition to the selection on observables assumption detailed above, there are at least three other threats to the identification of stress as the primary factor affecting savings choice. A first challenge to the identification strategy proposed here is the inability to discriminate between the effects of stress and the effects individuals' levels of activity on individuals' savings decisions. That is, times of high casualties are likely to be times of high operational tempo (OPTEMPO) for military units. Reduced savings decisions could be the result of solders' experiencing increased stress associated with higher levels of casualties, or they could be the result of soldiers being too busy to enroll in the Savings Deposit Program. Assuming that casualty statistics are orthogonal to all relevant considerations may be inappropriate and disentangling these effects will be difficult. I have two strategies for dealing with this concern. First, while both activity levels and stress levels may contribute to lower participation, from a policy perspective, both constitute "stress" within a broad sense and suggest that the conditions of an individual's deployment may adversely affect their ability to participate in the program In this sense, whether the causal factor is stress, being too busy, or both, the adverse outcome on the individual is the same and policy design should account for this fact.[21] Second, to attempt to address this concern from a scientific standpoint I employ several methods. I will employ additional control variables to account for unit activity levels using country-month fixed effects, controls for unit function (percentage Combat Arms vs. Combat Support vs. Combat Service Support), controls for unit composition (indicator for Special Forces unit, indicator for member of a Brigade Combat Team, percentage Officer, percentage of high-experience members and percentage female), and instruments for the activity level a unit likely faces (holiday periods, election periods, troop levels and aggregate fuel consumption). While none of

[21] That is, if the purpose of the Savings Deposit Program is to assist soldiers by offering them high returns on their savings deposits, then reduced participation, be it from stress or from high levels of activity, are equally worrisome. The lesson to military leaders and program administrators is to remain sensitive to casualty levels as a measure of when participation levels are likely to decrease. The specific solutions might vary depending on the reason for non-participation (too busy or too stressed), but to the extent that the policy prescriptions are straightforward both reasons can be addressed through increased education, outreach and enrollment services during these periods. Even with estimates of reduced scientific interest, the results of this analysis are of practical interest.

these methods are perfect, they allow me to more confidently assign reduced participation to higher stress levels.

A second threat to my identification strategy involves the confounding effects of an individual's stress level (unit casualty) level with the individual's location and its availability of technologies to enroll in the SDP. If individuals who experience high casualty rates are located in rural areas on small bases and these bases lack internet connectivity and/or military finance offices then failure to enroll in the SDP may be the result of high levels of stress or lack or opportunities or both. Unfortunately the data employed here do not allow me to evaluate this confound explicitly as they do not contain detailed location information. If this confound is correct then the estimates presented here will be upward biased estimates of the effects of stress on SDP participation.

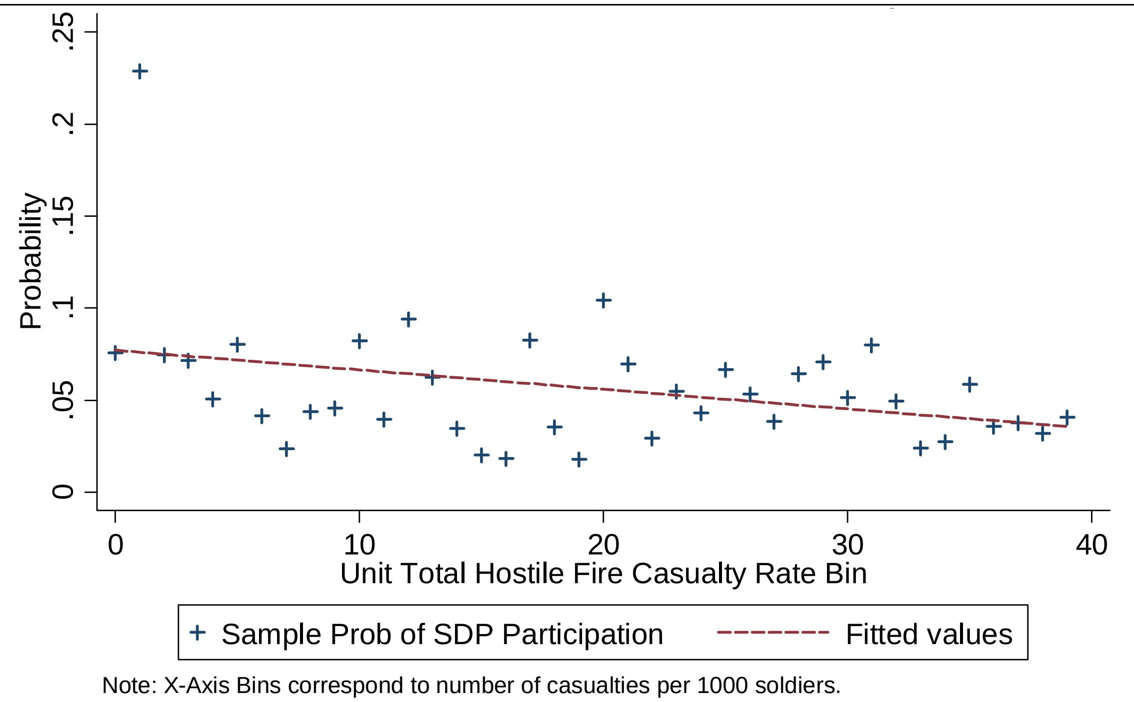
Despite the challenges described above, I believe that this research holds promise for demonstrating the effects of combat stress on individuals' financial decision-making. Modern warfare is extremely stressful for service members. Soldiers face challenging environmental and terrain conditions; they fight against traditional and irregular enemy formations; they perform traditional war-fighting, peace-keeping, nation-building, and counterinsurgency operations; they face disease and chemical, biological and radiological threats; they deploy for long periods of time; and they often deploy repeatedly. For these reasons and others, some have suggested that modern soldiers face greater combat stress than their counterparts in any previous conflict (Krueger (2008)). Since this is an empirical question, the results of this analysis can better inform our understanding of the consequences of modern stressors on the battlefield by demonstrating the existence and magnitude of any such effects, even if they represent average effects or if they are not solely attributable to the stress from casualty levels.

Empirical Results

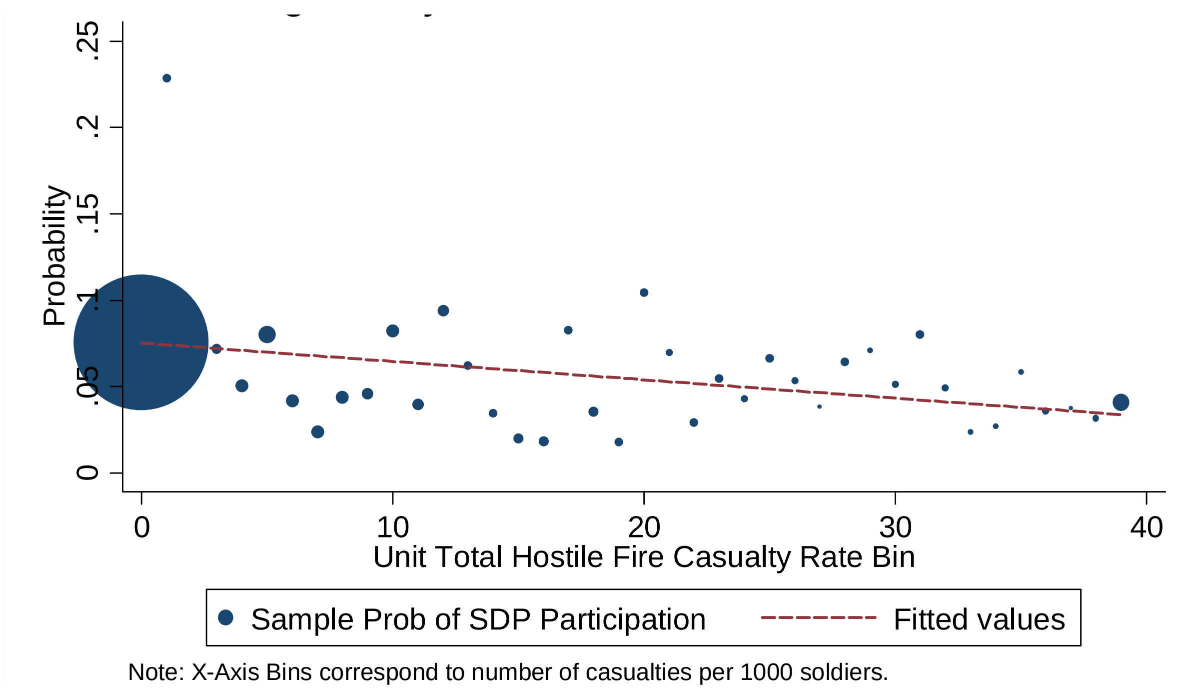
I begin the empirical analysis by reviewing the data in the sample. In Table 3.1 I present the summary statistics for the dependent and independent variables by casualty levels. The dependent variables of interest are the probability of participation, where participation is defined as any deposit into an SDP account; the total amount of money deposited in the SDP account; the deposit total conditional on participation and the time elapsed until participation, defined as the duration in days from the start of the deployment until the first deposit. These outcomes allow me to evaluate both extensive and intensive margin behaviors. On the extensive margin, the patterns in the probability of participation and unconditional deposit total suggest that deploying during times of positive casualties reduces individuals' participation in the SDP. On the intensive margin, the patterns in the conditional deposit level and the time to first deposit also suggest lower levels of participation among those deploying in units during months with casualties. However, there are many other differences between individuals who deploy in times of no casualties and those who deploy in times of positive casualties, as Table 3.1 makes clear. The results described above are not regression-adjusted and do not account for other potential differences across the control and treatment groups. Thus the results in Panel A of Table 3.1 are consistent with the hypothesis presented here and suggest additional analyses to causally link casualties and SDP participation.

Table 3.1. Summary Statistics by Unit Casualty Level						
	(1)		(2)		(3)	
	Full Sample		**Zero Casualties**		**Positive Casualties**	
	N=288,772		*N=260,113*		*N=28,659*	
Variable	**Mean**	**(Std Dev)**	**Mean**	**(Std Dev)**	**Mean**	**(Std Dev)**
Panel A. Outcomes						
Probability (Participation), %	7.36	(26.1)	7.54	(26.4)	5.78	(23.3)
Deposit Total, $	597	(2,304)	613	(2,333)	456	(2,022)
Deposit Total | Participation, $	8,108	(3,347)	8,128	(3,333)	7,879	(3,498)
Time to First Deposit | Participation, days	66	(59)	66	(60)	65	(55)
Panel B. Individual Characteristics						
Age, years	27.8	(7.0)	28	(7.1)	26.1	6.1
Female, %	7.9	(26.9)	8.4	(27.8)	2.8	(16.6)
Married, %	47.2	(49.9)	46.4	(49.9)	54.1	(49.8)
Number of dependents	1.4	(1.5)	1.4	(1.5)	1.2	(1.5)
Minority, %	32.5	(46.8)	33.1	(47.0)	27.1	(44.4)
Less than HS education, %	1.6	(12.5)	1.6	(12.6)	1.4	(11.8)
High school graduate, %	69.2	(46.6)	68.5	(46.5)	76.0	(42.7)
Some college, %	8.1	(27.2)	8.2	(27.5)	6.8	(25.1)
College graduate, %	12.0	(32.5)	12.2	(32.7)	10.6	(30.8)
Greater than college education, %	1.0	(9.9)	1.0	(10.1)	0.6	(7.4)
AFQT Score, percentile	60.2	(20.3)	60.1	(20.3)	61.3	(20.4)
Monthly pay, hundreds of $	41.59	(18.99)	41.98	(19.24)	38.02	(16.13)
Officer, %	14.5	(35.2)	15.0	(35.6)	10.2	(30.3)
Warrant Officer, %	5.2	(22.3)	5.6	(23.0)	2.2	(14.7)
Military experience, years	5.8	(6.4)	5.9	(6.4)	4.5	(5.5)
Deployment length, months	8.8	(4.3)	8.8	(4.3)	8.8	(4.2)
Number of previous deployments	0.9	(1.2)	0.9	(1.2)	0.9	(1.2)
Previous SDP participant, %	4.5	(20.6)	4.6	(20.9)	3.2	(17.6)
Panel C. Treatment Variables						
Hostile fire casualty rate, %	0.154	(0.597)	0.000	(0.000)	0.157	(1.17)
Hostile fire death rate, %	0.013	(0.096)	0.000	(0.000)	0.013	(0.278)

Source: Department of Defense Data. Notes: All data is for U.S. Army soldiers. The probability of participation is the fraction of individuals with any SDP deposit total greater than zero. Time to first deposit is the number of days between the deployment start and the first deposit transaction. The married variable represents formal and common law marriages for anyone who has ever been married. The less than or equal to high school graduate variable includes high school dropouts and GED holders. The some college variable includes those with an Associate's Degree. Monthly compensation represents the base pay during the deployment divided by 100. Armed Forces Qualification Test (AFQT) statistics are calculated for enlisted individuals only (n=231,743). Military experience is the amount of time in years that the individual had served at the date of deployment. Previous SDP participant is an indicator variable set equal to one if the individual had ever participated in the SDP prior to the current deployment observation. The hostile fire casualty rate is the sum of deaths and injuries from hostile fire in a unit divided by the total number deployed in the unit and the numbers here represent averages of the unit casualty rates.



Panel A. Unweighted Scatterplot



Panel B. Weighted Scatterplot with Bins Weighted by the Number of Individuals

Figure 3.2. Scatterplots of SDP Participation vs. Unit Total Hostile Fire Casualty Rate

To further evaluate the relationship between stress and SDP participation I completed a variety of scatterplots. In Figure 3.2 I provide a non-parametric look at the bivariate relationship between the casualty rate in a given month and the SDP participation rate, in an unweighted and weighted format. The graphs and the associated best fit lines reveal a negative relationship between the casualty rate and the probability of SDP participation. These graphs further support the hypothesized relationship between stress and savings.

Based on the summary statistic observations and the graphical analysis discussed above I also completed an initial statistical analysis of the outcome variable means for below and above the median casualty levels in the full sample. Note that for both countries and for the full sample the median and modal casualty rate is zero. In fact, of the 288,772 observations in the sample, only 28,659 (11.0%) have positive unit casualty rates. Thus despite the apparently large sample, there are comparatively few observations that provide for identification of the effects of casualties on SDP participation. Nonetheless, in Table 3.2 I present t-tests of the differences in the outcome means for the zero and positive casualty subsamples.

Table 3.2. Differences in Outcomes by Unit Casualty Level

	(1) **Zero Casualty Mean** *N=260,113*	(2) **Positive Casualty Mean** *N=28,659*	(3) **Difference** (1) - (2)	
Variable	**Mean**	**Mean**	**Mean**	**p-value**
Probability (Participation), %	7.54	5.78	1.76	0.00
Deposit Total, $	613	456	157	0.00
Deposit Total | Participation, $	8,128	7,879	249	0.00
Time to First Deposit | Participation, days	66	65	1	0.47

Source: Department of Defense Data. Notes: All data is for U.S. Army soldiers. The probability of participation is the fraction of individuals with any SDP deposit total greater than zero. Time to first deposit is the number of days between the deployment start and the first deposit transaction. Column 3 tests whether the differences in the mean values by casualty level are different from zero using a t-test. The mean difference and p-value for the t-tests is listed. These results are not regression adjusted.

On the external margin, the differences in the probability of participation and the deposit total are significant at the α=0.01 level. On the intensive margin, the difference in the SDP deposit total conditional on participation is significant at the α=0.01 level while the difference in the time to first deposit is insignificant at the α=0.10 level. While these differences do not control for other differences in the control and treatment groups, they do justify further analysis via multivariate regression.

Multivariate Regression Analysis

With a better understanding of the distributions of the stressor and explanatory variable data I now turn to an introduction of the regression specifications for my hypothesis testing. An individual's participation in the SDP is determined by a number of factors: their level of stress, their individual experiences and capabilities, the opportunity cost of participation, and the conditions of their deployment. These can be broadly categorized as individual characteristics, time fixed effects and unit characteristics. Equation (1) formalizes this relationship:

$$Y_{ijkt} = \alpha + \beta \cdot STRESS_{jkt} + \gamma \cdot X_{ijt} + \delta_t + \tau_{jkt} + \varepsilon_{ijkt} \qquad (1)$$

In this model Y_{ijkt} is a measurement of individual i's SDP participation in country $j$in unit k in time period t. $STRESS_{jkt}$ is a measurement of stress experienced by the individual and will be estimated by the hostile-fire casualty rate in unit k in country j during time period t. As a result, β is the coefficient of primary interest and I hypothesize that $\beta < 0$. That is, as an individual's unit casualty rate increases, the individual is less likely to participate in the SDP. X_{ijt} is a vector of individual characteristics including a quadratic in age, education level, gender, minority status, military income, AFQT score, an individual's military occupation specialty (job), rank category (officer, warrant officer or enlisted), a quadratic in military experience, a quadratic in the length of the deployment, the country of deployment, the number of previous deployments and an indicator for previous SDP participation. δ_t is a vector of time fixed effects that includes indicators for each country-month combination and a seasonal control for the winter period.[22] τ_{jkt} is a vector of unit characteristics including the percentages of the unit by job types (combat arms, combat support and combat service support), by officer status, by female and by high levels of experience (>5 years). ε_{ijkt} is the error term assumed to be orthogonal to all other variables. The critical assumption for identification is selection on observables, specifically:

$$Y_{ijkt} \perp STRESS_{jkt} \mid X_{ijt}, \delta_t, \tau_{jkt} \tag{2}$$

That is, conditional on an individual's job, experience and year, the treatment of stress via casualties is randomly assigned. I apply this general model to the specific hypotheses from Section III to generate the empirical tests for this research.

- **Hypothesis 1: Individuals with higher stress levels will exhibit lower SDP participation.[23]**

I conduct my empirical of this hypothesis using Equation (1) and multivariate regression analysis. I use a linear probability model for simplicity of interpretation and the sample size justifies this selection. Table 3.3 displays the regression results.

[22] The most notable potential time trend in casualty data is the proposition of a "fighting season." This is particularly relevant in Afghanistan, where heavy winters and extreme terrain can make fighting untenable in winter months. I use an indicator variable for winter months (December, January and February) to control for this phenomenon.

[23] Initial levels of stress will be measured using the casualty statistics from the individual's month of arrival in their deployment country.

Table 3.3. OLS Estimates of the Effect of Stress on Probability of SDP Participation									
	(1)			(2)			(3)		
Variable	**Coeff**		**(Std Err)**	**Coeff**		**(Std Err)**	**Coeff**		**(Std Err)**
Mean outcome in control	7.54			7.54			7.54		
Own unit casualty rate	-30.52	**	(12.15)	-24.90	**	(12.62)	-23.29	*	(12.61)
Age	0.20	***	(0.07)	0.20	***	(0.07)	0.20	***	(0.07)
Age^2	-0.003	***	(0.001)	-0.003	***	(0.001)	-0.003	***	(0.001)
Female	5.66	***	(0.29)	5.63	***	(0.29)	5.16	***	(0.28)
Married	0.52	***	(0.14)	0.49	***	(0.14)	0.51	***	(0.14)
Number of dependents	-0.47	***	(0.05)	-0.45	***	(0.05)	-0.45	***	(0.05)
Minority	1.42	***	(0.12)	1.44	***	(0.12)	1.41	***	(0.12)
Education ≤ HS	-0.13		(0.31)	-0.13		(0.31)	-0.14		(0.31)
Some college	0.69	***	(0.23)	0.65	**	(0.23)	0.66	***	(0.23)
College graduate	2.58	***	(0.31)	2.54	***	(0.31)	2.55	***	(0.31)
Education ≥ College	3.07	***	(1.00)	2.99	***	(1.00)	2.88	***	(0.99)
AFQT	0.03	***	(0.00)	0.03	***	(0.00)	0.03	***	(0.00)
Monthly compensation (hundreds $)	0.08	***	(0.01)	0.07	***	(0.01)	0.07	***	(0.01)
Officer	3.50	***	(0.87)	3.90	***	(0.86)	3.69	***	(0.86)
Warrant Officer	1.91	**	(0.91)	2.24	**	(0.91)	2.25	**	(0.91)
$Experience^2$	0.003		(0.004)	0.003		(0.004)	0.003		(0.004)
Deployment length	1.14	***	(0.08)	1.14	***	(0.07)	1.10	***	(0.07)
Deployment $length^2$	-0.01	*	(0.00)	-0.01	**	(0.00)	-0.01	**	(0.00)
Number of previous deployments	-1.51	***	(0.06)	-1.50	***	(0.06)	-1.50	***	(0.06)
Previous SDP participant	41.68	***	(0.74)	41.49	***	(0.72)	41.43	***	(0.72)
Individual Characteristics	Yes			Yes			Yes		
Time Fixed Effects	No			Yes			Yes		
Unit Characteristics	No			No			Yes		
Number of Observations	288,772			288,772			288,772		
Number of Clusters	44,453			44,453			44,453		
Adjusted R^2	0.2215			0.2249			0.2262		

Source: Department of Defense Data. Notes: The probability of participation is the fraction of individuals with any SDP deposit total greater than zero. The hostile fire casualty rate is the sum of deaths and injuries from hostile fire in a unit divided by the total number deployed in the unit and the numbers here represent averages of the unit casualty rates. The married variable represents formal and common law marriages for anyone who has ever been married. The less than or equal to high school graduate variable includes high school dropouts and GED holders. The some college variable includes those with an Associate's Degree. Monthly compensation represents the base pay during the deployment divided by 100. Military experience is the amount of time in years that the individual had served at the date of deployment and the linear experience term is absorbed in the assignment fixed effects (jobxexperiencexyear). Previous SDP participant is an indicator variable set equal to one if the individual had ever participated in the SDP prior to the current deployment observation. For observations missing education data, AFQT scores or experience data, zero values are assigned and indicators reflecting the missing data are used. Robust standard errors are clustered at the unit-month level. ***, ** and * reflect statistical significance at the 1%, 5% and 10% levels.

In the baseline specification (column 1) I include the treatment variable (Own unit casualty rate) and all individual characteristics, including those required for my assumption of unconfoundedness (fixed effects for job×experience×year). In column 2 I add additional time fixed effects and in column 3 I add unit characteristics. In all specifications the relationship between stress (casualties) and financial decision-making (SDP participation) is negative, stable and statistically significant. In the most complete specification (column 3) the coefficient of interest is -23.29 and it is statistically significant at the 10% level (p=0.065). This implies that a change in the own unit casualty rate from 0 to 1 would reduce SDP participation by 23.29% on a mean participation rate of 7.54%. While this is a very large effect, such a change is unlikely and exaggerates the likely effects of casualties in most cases. I discuss the appropriate magnitude for treatment effect estimates in greater detail below. Nonetheless, initial regression results suggest they hypothesized negative relationship between stress and savings holds.

In Table 3.4 I present results for all four SDP outcomes of interest using the same specification from column 3 of Table 3.3. In this table I only display the coefficient of interest (own unit casualty rate). These results confirm that the effects of casualties on SDP participation decisions appear to operate only on the extensive margin.[24] With respect to this margin, column 1 reveals that a change in the unit casualty rate from 0 to 1 reduces an individual's probability of participation by 23.29% relative to a control group mean of 7.54% (p = 0.065) and column 2 reveals that this change in the casualty rate reduces an individual's average SDP deposits by $1,983 relative to a control group mean of $613 (p = 0.060). On the intensive margin, column 3 reveals that a change in the unit casualty rate from 0 to 1 reduces

[24] For the complete regression results for these outcomes, see Appendix 3.2 (Table 3.4.A).

an average SDP deposits by $10,395 relative to a control group mean of $8,128 but the difference is statistically insignificant (p = 0.176) and column 4 reveals that this change in the casualty rate delays the time until a first SDP deposit by 95 days relative to a control group mean of 66 days but the difference is statistically insignificant (p = 0.568).

Table 3.4. OLS Estimates of the Effect of Stress on SDP Outcomes

	(1)		(2)		(3)		(4)	
Variable	**Coeff**	**(Std Err)**	**Coeff**	**(Std Err)**	**Coeff**	**(Std Err)**	**Coeff**	**(Std Err)**
Outcome	Probability of Participation		SDP Deposit Total		SDP Deposit Total | Participation		Time to first Deposit | Participation	
Mean in control	7.54		613		8,128		66	
Own unit casualty rate	-23.29 *	(12.61)	-1983.08 *	(1054.36)	-10395.32	(7686.51)	-94.93	(166.06)
Number of Observations	288,772		288,772		21,261		21,261	
Number of Clusters	44,453		44,453		44,453		44,453	
Adjusted R^2	0.2262		0.2331		0.2170		0.1633	

Source: Department of Defense Data. Notes: Variables are defined as in Tables 3.1-3.3. All regression specifications include individual characteristics, unit characteristics and time fixed effects. Robust standard errors are clustered at the unit-month level. ***, ** and * reflect statistical significance at the 1%, 5% and 10% levels.

Interpreting the Magnitude of the Estimated Effects

Thus far the estimate magnitudes reported for the effects of casualties on SDP participation have been for a change in the unit casualty rate from 0% to 100%, an unlikely change and one that makes the effect sizes reported unrealistic. A more common measure in the social sciences literature is a standardized treatment variable, but that measure is also inappropriate in the current context given the highly unbalanced sample (n=260,113 observations have casualty values equal to 0 and n=28,659 observations have non-zero casualty levels). While there is no correct measure, I offer two potential interpretations of the magnitude of the effect of casualties on SDP participation based on the underlying natural experimental framework. First, if individuals are randomly assigned to units conditional on some controls, then the most reasonable interpretation for comparison is to compare the estimated differences in the probability of participation for a typical unit in the no casualty control group and a typical unit in the positive casualty treatment group. The mean casualty rate in the control group is 0% and the mean casualty rate in the treatment group is 1.57%. Using these means, the estimated difference in the probability of participation in the SDP for a typical individual in a no casualty unit compared to a typical individual in a unit that experiences casualties is 1.57% x -23.29% = -0.366 percentage points on a control group mean of 7.54%. The magnitude of this effect method is approximately 4.85%, a modest effect.

The second method for estimating the effect size uses the movement from the 25th percentile in the casualty distribution to the 75th percentile in the casualty distribution among those units experiencing casualties. This difference is 2.272-0.617 =1.66. Using this level of casualty I estimate the magnitude of the effect as 1.66% x -23.29% = -0.386 percentage points on a control group mean of 7.54%. The magnitude of this effect method is approximately 5.11%, a slightly larger but still modest effect. Nonetheless, to the extent that there are pervasive effects of combat casualties in a single month on a decision that can occur any time within a deployment lasting up to 19 months even controlling for a host of other environmental (country and season), job (occupation, experience level, type of unit) and personal (family characteristics, age, education) sources of daily stress, these results remain notable.

Having established suggestive evidence that there is a negative relationship between casualties and SDP participation I now turn to the principle threats to my identification strategy.

3.7 ROBUSTNESS CHECKS

The primary threats to my identification strategy are two-fold: failures of my assumption of randomization conditional on observables and confounding effects of other deployment characteristics.

Randomization Tests

Identification of an unbiased estimate of the effects of casualties on savings decisions requires random assignment of individuals to the control or treatment group. The natural experiment used here relies on random assignment of individuals to units once I condition on an individual's military occupation specialty (job category), their experience (rank) and the year. An ideal random experiment would generate balance among all individual characteristics between those individuals deploying in units with no casualties (the control group) and those individuals deploying in units with casualties (the treatment groups), conditional on these observable factors. To test this assumption I complete a randomization test in two steps. In Table 3.5 I present the randomization test results. In column 1 I validate the important individual characteristics that determine SDP participation outcomes by regressing SDP participation on the individual characteristics used in column 3 of Table 3.3. These characteristics predict SDP participation and validate my control variables. In column 2 I regress my treatment variable on these same characteristics, controlling for the characteristics supporting the selection on observables assumption and additional time fixed effects and unit level controls. The results of this test reveal five individual characteristics that are statistically related to my treatment variable, implying that this natural experiment is imperfect in its implementation. The individual characteristics that fail the randomization test and are related to treatment at a statistically significant level are: female (p=0.000), previous SDP participant (p=0.000), monthly compensation (p=0.064), deployment length (p=0.004 for the linear term and p=0.001 for the quadratic) and college graduate (p=0.070).

Table 3.5. Randomization Test Between Casualty Rates and Individual Characteristics

Variable	(1) Coeff (Std Err)	(2) Coeff (Std Err)
Outcome	Probability of Participation	Own unit casualty rate
Mean in control	7.54	0.00
Age	0.203*** (0.068)	0.0001 (0.0001)
Age^2	-0.003*** (0.001)	-0.0000 (0.0000)
Female	5.17*** (0.277)	-0.019***(0.004)
Ever married	0.508*** (0.139)	-0.002 (0.004)
Number of dependents	-0.448*** (0.049)	-0.0017 (0.0012)
Minority	1.41*** (0.118)	-0.003 (0.003)
Less than high school education	-0.135 (0.309)	-0.007 (0.008)
Some college	-0.656*** (0.228)	0.006 (0.006)
College graduate	2.54*** (0.313)	0.011* (0.006)
Greater than college education	2.88*** (0.992)	0.011 (0.014)
Monthly compensation (hundred $)	0.069*** (0.007)	-0.0005*(0.0003)
AFQT score	0.029*** (0.003)	-0.0000 (0.0001)
Officer	3.70*** (0.860)	-0.025 (0.022)
Warrant Officer	2.25*** (0.907)	-0.033 (0.021)
$Experience^2$	0.003 (0.004)	-0.0000 (0.0001)
Deployment length	1.09*** (0.069)	-0.013***(0.002)
Deployment $length^2$	-0.001** (0.004)	-0.0009***(0.0003)
Number of previous deployments	-1.50*** (0.059)	0.001 (0.004)
Previous SDP participant	41.4*** (0.722)	-0.020***(0.007)
Number of Observations	288,772	288,772
Number of Clusters	44,453	44,453
Adjusted R^2	0.2262	0.1215

Source: Department of Defense Data. Notes: Variables are defined as in Tables 3.1-3.3. All regression specifications include individual characteristics, unit characteristics and time fixed effects. Robust standard errors are clustered at the unit-month level. ***, ** and * reflect statistical significance at the 1%, 5% and 10% levels.

While these randomization test failures reveal non-random assignment to the treatment group based on some individual characteristics, they do not reveal the resulting bias in my estimates. To determine these effects I conduct a series of robustness checks on reduced samples using the SDP participation decision outcome to evaluate the effects of stress on the portions of the sample that meet the assumption of random assignment. In Table 3.6 I present the results of these robustness checks. The results from these robustness checks support the conclusion that there is a negative relationship between stress and SDP participation. Column 5 in Table 3.4 provides the most stringent check by reducing the sample to those with none of the characteristics that were statistically related to treatment. In this case, the reduced sample (n=36,164) omits females, individuals with previous deployments, college graduates, individuals with monthly compensation above the sample median ($3,759), and individuals with deployment length above the median (9 months). In this specification an increase in the unit casualty rate from 0 to 1 reduces the probability of SDP participation on average by 40.89 percentage points relative to the reduced sample control group mean of 1.24% that is significant (p=0.000). The relative magnitude of this effect is over ten times larger than in the full sample. Overall, the robustness checks in Table 3.6 reveal that the randomization failures and imperfection of the natural experiment used in this study actually generate a downward bias in the magnitude of the estimated coefficients and increase the variation in the observed outcomes, reducing the level of statistical significance of the estimates of stress on individual savings decisions. In this case the revised estimates are much larger for the reduced samples than for the full sample and the reduced sample mean SDP participation rates in the control group are much smaller. This provides suggestive evidence that the randomization failures inherent in this natural experiment do not threaten the validity of the identification of a negative effect of stress on savings and the primary estimates should be considered as a lower bound on the effects of stress on SDP participation.

Table 3.6. OLS Estimates of the Effect of Stress on Probability of SDP Participation in Reduced Samples (Robustness Checks)

	(1)		(2)		(3)		(4)	
Variable	**Coeff**	**(Std Err)**	**Coeff**	**(Std Err)**	**Coeff**	**(Std Err)**	**Coeff**	**(Std Err)**
Mean in control	7.54		5.83		4.50		1.24	
Own unit casualty rate	-23.29 *	(12.61)	-54.12 ***	(19.09)	-62.68 ***	(19.47)	-40.89 ***	(10.79)
Reduced Sample Specifications								
Omits females	No		Yes		Yes		Yes	
Omits previous deployers	No		Yes		Yes		Yes	
Omits college graduates	No		Yes		Yes		Yes	
Mo. compensation ≤ median	No		No		Yes		Yes	
Deployment length ≤ median	No		No		No		Yes	
Number of Observations	288,772		113,482		82,245		36,164	
Number of Clusters	44,453		22,674		15,755		10,385	
Adjusted R^2	0.2262		0.1379		0.0600		0.1050	

Source: Department of Defense Data.
Notes: Variables are defined as in Tables 3.1-3.3. All regression specifications include individual characteristics, unit characteristics and time fixed effects. Robust standard errors are clustered at the unit-month level. ***, ** and * reflect statistical significance at the 1%, 5% and 10% levels.

Confounding Effects of Activity Levels and Remote Locations

The second threat to identification involves important confounds associated with the natural experiment wherein casualty rates may be correlated with other conditions that individuals face that also reduce SDP participation. These threats could bias my results even if the assumption of random assignment holds. The first potential confound is that the activity levels of individuals' units may correlate with the units' casualty rates. If individuals in units with casualties are also in units that are especially busy, then the failure to enroll in the SDP might reflect stress (from casualties) or a lack of time (from the busy schedule) or a combination of the two. While this confound cannot be completely dismissed, robustness checks suggest that there is a negative effect of casualties on SDP participation even controlling for the activity level experienced by individuals. First, the regression specifications used above incorporate fixed effects for each country in each month, capturing country wide trends and activity levels for the entire U.S. Army force in each country. Second, I complete additional robustness checks incorporating several other controls for activity levels including election timing in each country, Islamic holidays in each year, a quadratic in the aggregate troop level in each country and a quadratic in aggregate U.S. fuel consumption (available only for Afghanistan beginning in Jan. 2002). In Table 3.7 I present the results of these robustness checks.

These robustness checks reveal that the estimates of stress on savings are remarkably stable. This is not surprising as the original specification included fixed effects at the country×month level. The estimate for the effect of stress on SDP participation remains statistically significant for all specifications except the final specification in column 4. The reduced sample size for this specification (fuel consumption data was only available from the U.S. Central Command for Afghanistan) increases the standard error of the estimate and the estimate is statistically insignificant at conventional level ($p=0.150$). However, the coefficient remains stable and actually increases slightly (as does the mean SDP participation rate in the control group for this reduced sample). Taken together, these results suggest that while activity levels cannot be eliminated as a source of reduced SDP participation, there appears to be a separate and distinguishable negative effect generated by casualties.

Table 3.7. OLS Estimates of the Effect of Stress on Probability of SDP Participation with Additional Controls for Activity Levels (Robustness Checks)

	(1)		(2)		(3)		(4)	
Variable	**Coeff**	**(Std Err)**	**Coeff**	**(Std Err)**	**Coeff**	**(Std Err)**	**Coeff**	**(Std Err)**
Mean in control	7.54		7.54		7.54		7.74	
Own unit casualty rate	-23.29 *	(12.61)	-23.29 *	(12.61)	-23.29 *	-(12.61)	-27.85	(19.33)
Additional Controls for Activity Level								
Time fixed effects	Yes		Yes		Yes		Yes	
Holidays, elections, surges	No		Yes		Yes		Yes	
Quadratic in U.S. troop level	No		No		Yes		Yes	
Quadratic in U.S. fuel consumption	No		No		No		Yes	
Number of Observations	288,772		288,772		288,772		159,127	
Number of Clusters	44,453		44,453		44,453		21,606	
Adjusted R^2	0.2262		0.2262		0.2262		0.2252	

Source: Department of Defense Data.
Notes: Variables are defined as in Tables 3.1-3.3. All regression specifications include individual characteristics, unit characteristics and time fixed effects. Robust standard errors are clustered at the unit-month level. ***, ** and * reflect statistical significance at the 1%, 5% and 10% levels.

Third, the stress from casualties does not appear to affect the average time until the first deposit for individuals as demonstrated in Tables 3.2 and 3.3. Instead, the effects from the stress of casualties appear to operate on the extensive margin, implying that individuals would have to be busy for their entire deployment, an unlikely scenario. However, even with these controls and evidence, I cannot rule out the possibility that the unobserved unit activity levels could correlate with casualties and bias my findings upwards. While this confound remains important, the distinction between how busy an individual is and their stress level is somewhat arbitrary. In many contexts being busy is synonymous with experiencing stress. This critique might be better noted as a shortcoming of the definition of stress used in this study (casualties) than the general conclusion of the adverse effects of exogenous deployment conditions on individual financial decisions.

The second potential confound to my identification is the possibility that unit casualties may correlate with unit location. If individuals assigned to remote locations experience both high casualties and lower levels of access to SDP enrollment technologies (either in a military finance office or via the internet), then my findings might be upward biased. This is a serious concern and one that the current study does not afford much insight into. Unfortunately the Department of Defense casualty data and personnel data used here does not include information on an individual's assignment location within a country or the features of their assignment location (e.g., access to a finance office or the internet). While casualties in Iraq and Afghanistan occur in both rural and urban settings, and at remote and accessible locations, there is little available information on the composition of casualties based on these divisions. This means that while the confound is potentially valid, there is no information from which to form a prior on the likelihood that casualty rates among U.S. Army units are higher in areas where opportunities to enroll in the SDP are limited. In addition, there is one finding from this study that suggests that the remoteness of an individual’s location is unlikely to be driving the results. While there are a number of remote bases, often called Combat Outposts (COPs), used by the U.S. Army in both Iraq and Afghanistan, individuals assigned to these locations typically rotate in and out of these locations from larger bases. If individuals at remote locations desire to enroll in the SDP but are unable do the lack of access of their primary location, then we would expect that they might enroll in the SDP when they move to larger bases, either temporarily or permanently. Such decisions would be visible in delayed enrollment times for those in units with high casualties. Yet the results here suggest that there are no effects of casualties on the intensive margin, including the time until the first SDP deposit (See Table 3.4, p=0.568). This provides some weak evidence that the results are not driven by the confounding effects of access to SDP enrollment opportunities.

The adverse effects of stress on financial decision-making estimated in this study appear to be statistically significant and robust to a number of confounds. However, these robustness checks are not decisive and I cannot rule out the possibility that there are unobserved characteristics of individuals' deployment conditions that are correlated with both their unit's casualty level and their SDP participation levels. The evidence presented here is suggestive but not conclusive.

Heterogeneous Treatment Effects

I also explore the heterogeneous treatment effects between casualties and several categories of individual characteristics: sex, marital status, human capital (college graduate and Armed Forces Qualification Test (AFQT) score), and the deployment length. In Table 3.8 I present the results of these tests. I find no statistically significant differential gender effects of stress on SDP participation (p = 0.881), no differential effects by human capital levels as measured by AFQT score (p = 0.835) or among college graduates (p = 0.943), and no differential effects by deployment length (p = 0.564 for the linear term and p=0.866 for the quadratic term). I do find a statistically significant negative interaction between being married and the stress generated by casualties (p = 0.0027) that implies that a married individual exposed to casualties has a ((29.70x1.57)/7.54) = 6.18 percent lower probability of SDP participation than a non-married individual exposed to casualties.

Table 3.8. OLS Estimates of Heterogeneous Treatment Effects			
	(1)	(2)	(3)
Variable	**Coeff (Std Err)**	**Coeff (Std Err)**	**Coeff (Std Err)**
Mean in control	7.54	7.54	7.54
Own unit casualty rate	-23.29* (12.61)	79.37 (60.95)	-6.74 (13.28)
Female	5.16*** (0.28)	5.15*** (0.28)	5.16*** (0.28)
Cas rate × Female		9.88 (66.2)	
Married	0.51*** (0.14)	0.56*** (0.14)	0.56*** (0.14)
Cas rate × Married		-30.12** (13.60)	-29.70** (13.39)
College graduate	2.55*** (0.31)	2.53*** (0.32)	2.54*** (0.31)
Cas rate × College graduate		2.60 (36.50)	
AFQT	0.03*** (0.003)	0.03*** (0.003)	0.03*** (0.003)
Cas rate × AFQT		-0.09 (0.41)	
Deployment length	1.10*** (0.07)	1.10*** (0.07)	1.10*** (0.07)
Cas rate × Deployment Length		-8.14 (14.12)	
Deployment length^2	-0.01*** (0.004)	-0.01*** (0.004)	-0.01** (0.004)
Cas rate × Deployment length^2		-0.14 (0.81)	
Number of Observations	288,772	288,772	288,772
Number of Clusters	44,453	44,453	44,453
Adjusted R^2	0.2262	0.2263	0.2262

Source: Department of Defense Data. Notes: Variables are defined as in Tables 3.1-3.3. All regression specifications include individual characteristics, unit characteristics and time fixed effects. Robust standard errors are clustered at the unit-month level. ***, ** and * reflect statistical significance at the 1%, 5% and 10% levels. Column 1 reports the coefficients from the main specification from the baseline model from Table 3. Column 2 reports the coefficients, standard errors and statistical significance for all of the individual characteristics and interaction variables between the casualty rate and these variables. A joint F test of the significance of the individually insignificant interaction terms supports removing these terms from the model (F=0.01, p=0.913). Column 3 reports the revised model that includes all original individual characteristics, unit characteristics and time fixed effects and the statistically significant interaction terms.

In the final specification (column 3) the main effect estimate of casualties on SDP participation is -6.74 percentage points and the interaction estimate between casualties and being married is -29.70 percentage points and these estimates are jointly significant (F = 5.94, p =0.015). This suggests that the reduced SDP participation generated by exposure to casualties in this sample is largely driven by the effect of casualties on married individuals. I note that this result is counter to my prior expectation. I expected that while married individuals might have negative main effect estimates due to competing financial needs, the prevalent use of powers of attorney by married soldiers would allow their spouses to enroll them in the SDP. Nonetheless, it appears that married individuals may acutely experience the effects of casualties in their units.

Overall, the results of this study provide suggestive evidence that there is a modest negative relationship between casualties and financial decision-making. This relationship appears to operate exclusively on the extensive margin, implying that stress inhibits individuals from enrolling in the SDP. While the natural experimental variation employed here is imperfect, the negative relationship holds and is strengthened when the sample is confined to those individuals for whom random assignment is valid. This relationship is robust to concerns over the confounding effects of unit activity levels, implying that while being busy might also reduce the probability of SDP participation, there is a separate and statistically distinguishable negative effect generated by casualties in an individual's own unit. In addition, while there is no evidence that casualty rates correlate with low levels of access to SDP enrollment, this is an important potential confound that the present study cannot rule out. Finally, the stress generated by exposure to casualties appears to affect married individuals more than their non-married counterparts.

3.8. SUMMARY

This study provides suggestive evidence that stress may reduce savings and adversely affect financial decision-making. Since typical economic models of decision-making ignore such factors they may benefit from incorporation of such psychological influences and research into financial decisions may be subject to traditional omitted variables biases. In this study exposure to casualties in a U.S. Army soldier's unit during the month of deployment reduces their probability of participating in the Savings Deposit Program approximately 5%.

While the sample considered here is not representative of the U.S. population it is representative of the Armed Forces and should inform defense policy planners of the role of deployment conditions on service members. Moreover, to the extent that the members of this sample are self-selected members of an All-Volunteer Force (AVF), their own decisions, preparations, training and attitudes suggest that stress may influence decision-making even among motivated and resilient groups. More broadly this suggests that vocational stressors play a role in individual's financial choices, especially those choices related to their occupation (e.g., their 401(k), medical benefits, insurance decisions, etc...) and while the population may be unique, the general sign of the results seems generalizable.

The nature of the stressor in this research, combat casualties, is also unique and somewhat atypical for society as a whole. To be clear, military service, especially in armed conflict is uniquely stressful and the treatment effects estimated here do not provide easily translatable measures for other jobs or stress treatments. Many U.S. military members undergo extreme stress and trauma as part of their service and there is a substantial literature on the effects of extreme stress and the consequences of PTSD. But the military doesn't enjoy a monopoly on extremely stressful events. In fact, as Reid (1990) notes, up to 8% of the U.S. population experiences a traumatic event in a given year, when the definition of trauma includes robbery, sexual assault, serious motor vehicle accidents, bereavement and natural disasters. This proportion increases to 10% if trauma includes family violence and divorces with significant financial impacts. Thus traumatic events, while not commonplace in U.S. society, are not rare events either. As a result, the general result of negative effects of stress on savings and deliberative financial decision-making should apply to individuals outside the military as well, with implications for stress in other jobs and the role of other life stressors (job loss, divorce, etc...) on financial decision-making.

While this research design provides early estimates for the effects of stress on savings decisions, the psychological mechanism(s) through which stress may operate are not identified here. The research at hand provides reduced form estimates and cannot establish whether the stress is changing risk or time preferences, undermining willpower, degrading cognitive capabilities or otherwise influencing individual decisions. Even so, the large scale field evidence provided here should motivate additional research into the particular effects of stress on financial decisions.

Finally, this study has some straightforward policy implications. First the research suggests that policy design should be sensitive to the level of stress that an individual is facing. Choice architectures and information provision should reflect the potential levels of stress that individuals making choices will face, including choices as varied as life insurance payouts and retirement decisions. Second, in the context of this particular policy, to the extent that the SDP was established as a benefit to military members performing combat service, increasing participation in the SDP is a natural objective. Reform of the SDP enrollment rules, legal or administrative, to provide for pre-deployment enrollment would likely boost SDP participation since service members could make their decisions without the daily stresses of combat inhibiting their decision-making. Since pre-deployment training and preparation for all services already includes a host of decisions and validations with respect to medical (checkups), administrative (emergency data notification), legal (wills and powers of attorney), financial services (payroll deductions) and health care (health and life insurance), adding SDP enrollment seems straightforward and advisable. If legal requirements mandate enrollment after combat deployment, individuals could sign delayed enrollment contracts during pre-deployment processing. Current choice architecture for U.S. Army soldiers does not appear to optimally promote SDP enrollment.

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Appendix 3.1

Summary of Data and Dataset Procedures

This project combines data from several sources. Military administrative data, operational data and casualty data was provided by the Office of Economic & Manpower Analysis (OEMA) at the United States Military Academy, West Point, New York. Given the sensitive nature of the data I have signed a non-disclosure agreement with OEMA that restricts my right to publish this work without their prior review and approval. Savings Deposit Program (SDP) participation data was provided by the Defense Finance and Accounting Service (DFAS) to OEMA to merge with the military administrative data. Personally identifying information was removed from all data before I received it.

The sample for this study is U.S. Army soldiers who deployed to Afghanistan or Iraq from October 2001 through Dec 2010 for a period of 1-19 months. The population is further restricted to individuals serving in deployed units of 10 members of larger and those individuals not missing data on their age, number of dependents, gender, race and military experience. For individual missing data on their experience, education level and Armed Forces Qualification Test (AFQT) score, I assigned the individuals a value of zero and created indicator variables to record those missing data for each variable. These indicators were included in all regressions. I also omitted all individuals for whom I could not confirm a deployment location of Iraq or Afghanistan, based on individual assignment data and/or unit data. This includes omitting those missing country data and those assigned to Kuwait. To prevent the unnecessary influence of outliers, I winsorized total casualty rates, hostile fire death rates, SDP balances, monthly compensation and military experience variables at the 99th percentiles. I winsorized hostile fire injury data at the 95th percentile. For individuals with incomplete deployments as of December 2010, I imputed the SDP deposit total based on the median rate of return and current deposits.

Appendix 3.2

Additional Regression Results

Table 3.4.A. Full OLS Estimates of the Effect of Stress on SDP Outcomes

	(1)			(2)			(3)			(4)		
Variable	**Coeff**		**(Std Err)**	**Coeff**		**(Std Err)**	**Coeff**		**(Std Err)**	**Coeff**		**(Std Err)**
Outcome	Probability of Participation			SDP Deposit Total			SDP Deposit Total | Participation			Time to first Deposit | Participation		
Mean in control	7.54			613			8,128			66		
Own unit casualty rate	-23.29	*	(12.61)	-1983.08	*	(1054.36)	-10395		(7686)	-94.93		(166.06)
Age	0.20	***	(0.07)	14.18	**	(6.22)	3.06		(37.79)	-0.94		(0.93)
Age^2	0.00	***	(0.00)	-0.27	***	(0.11)	-0.16		(0.56)	0.01		(0.02)
Female	5.16	***	(0.28)	414.21	***	(24.42)	-45.46		(101.03)	2.06		(1.77)
Married	0.51	***	(0.14)	74.14	***	(12.34)	407.94	***	(88.84)	-0.85		(1.67)
Number of dependents	-0.45	***	(0.05)	-65.54	***	(4.34)	-390.17	***	(35.49)	1.51	**	(0.70)
Minority	1.41	***	(0.12)	95.72	***	(10.25)	-66.92		(81.79)	2.28		(1.46)
Education ≤ HS	-0.14		(0.31)	-8.04		(26.57)	-275.08		(420.89)	-5.20		(6.05)
Some college	0.66	***	(0.23)	41.43	**	(19.55)	-32.77		(175.12)	-6.46	**	(2.75)
College graduate	2.55	***	(0.31)	250.36	***	(28.21)	232.53		(162.23)	-4.91		(3.07)
Education ≥ College	2.88	***	(0.99)	270.74	***	(93.35)	428.81		(352.18)	-15.09	***	(5.57)
AFQT	0.03	***	(0.00)	2.76	***	(0.25)	13.85	***	(2.69)	0.02		(0.05)
Monthly compensation (hundreds $)	0.07	***	(0.01)	8.71	***	(0.70)	30.31	***	(3.97)	-0.13	*	(0.07)
Officer	3.69	***	(0.86)	368.42	***	(76.75)	747.67		(514.41)	-3.46		(8.20)
Warrant Officer	2.25	**	(0.91)	243.23	***	(79.15)	758.32		(618.43)	-1.28		(9.95)
$Experience^2$	0.003		(0.004)	0.130		(0.428)	1.320		(1.761)	0.096		(0.091)
Deployment length	1.10	***	(0.07)	74.93	***	(6.26)	738.25	***	(75.46)	-7.65	***	(2.03)
Deployment $length^2$	-0.01	**	(0.00)	0.61		(0.40)	-20.23	***	(3.50)	0.50	***	(0.09)
Previous deployments	-1.50	***	(0.06)	-122.20	***	(5.23)	-45.20		(55.64)	7.61	***	(1.35)
Previous SDP participant	41.43	***	(0.72)	3467.88	***	(65.27)	349.75	***	(93.14)	-20.58	***	(2.03)
Number of Observations	288,772			288,772			21,261			21,261		
Number of Clusters	44,453			44,453			44,453			44,453		
Adjusted R^2	0.2262			0.2331			0.2170			0.1633		

Source: Department of Defense Data. Notes: Variables are defined as in Tables 1-3. All regression specifications include individual characteristics, unit characteristics and time fixed effects. Robust standard errors are clustered at the unit-month level. ***, ** and * reflect statistical significance at the 1%, 5% and 10% levels.