Social Effects in Financial Decision-Making

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Abstract
Social influences could play a large role in an individual's financial decisions because of the consumer's inexperience with and the high costs of thinking through such decisions. We take advantage of a conditional randomization of individuals to social groups that vary in their levels of participation in a retirement savings program, two charitable giving programs, and purchase of life insurance to estimate how the groups affect individuals' participation in those programs. We find important effects for financial choices where the group's behaviors are observable and a likely topic of conversation, but no effects where the group's behaviors are unobservable or less likely to be discussed.

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I. Introduction

Choosing the optimal amount to save for retirement, to give to charities, or the amount of life insurance to purchase is a complicated problem for most individuals. Uncertainty about future earnings and social norms, how much others contribute to public goods, and the high complexity of financial instruments are only a few factors that make it difficult to solve these types of decision problems. With findings linking cognitive ability and experience to financial mistakes (Agarwal et al., 2009; Bertrand and Morse, 2011; Agarwal and Mazumder, 2013) as well as the prohibitively high costs of thinking through many financial decisions (Madrian & Shea, 2000), social groups could have large impacts on individual's financial choices.

Recent national policy efforts have recognized the potential importance of social effects in financial decisions. For example, the Consumer Financial Protection Bureau identifies leveraging peer networks as a best practice for workplace financial wellness programs (Consumer Financial Protection Bureau, 2014). In addition, the President's National Research Symposium on Financial Literacy and Education made it a top priority to understand the impact of social factors on financial attitudes and behaviors, specifically highlighting peer effects (Department of Treasury, 2008). The President's 2013 Advisory Council on Financial Capability encourages social group discussions as complements to workplace financial education (Department of the Treasury, 2013). Internationally, UN programs designed to provide financial assistance and World Bank reports assert the importance of social group effects in these domains (UN Capital Development Fund, 2015; World Bank, 2015).

There is some survey evidence that suggests social groups do impact individuals' financial decisions. Among workers with employer provided retirement funds, 25% discuss how to use the funds with peers (Employee Benefits Research Institute, 2008). 14% of federal employees participating in their workplace savings program state that peers are a top factor in their investment decisions (Thrift Savings Plan, 2013). A striking 78% of millennials state that they base their financial habits on their social group (American Institute of CPAs, 2013).

We use a conditional randomization of individuals to social groups to study whether these groups matter for young, low-income, moderately educated individuals' financial decisions including retirement savings, charitable giving, and the purchase of life insurance. The financial decisions of such low-income individuals have been the focus of persistent and renewed U.S. public policy efforts, from the establishment of the Consumer Financial Protection Bureau to widespread federal financial education efforts (GAO, 2012).

There are a number of econometric obstacles raised in Manski (1993) that must be overcome when estimating social effects. An individual's behavior may be related to that of her group because similar individuals sort into the groups or because common shocks affect both an individual's and her group's contemporaneous choices. In addition, it is difficult to quantify the extent to which the group affects the individual because the individual's behavior can feed back into the group's choices.

The most common approach to deal with these issues is to estimate the impact of a group's pre-determined characteristic on a person's outcome for a set of people who have been randomized to social groups. For example, Sacerdote (2001) and Lyle (2007) study how college roommates affect each other's academic performance. They regress an individual's performance on a measure of her own pre-determined academic ability (e.g. S.A.T. score) and her roommate's pre-determined academic ability. Because the measures of ability are determined before arriving at college, they are unrelated to the current shocks experienced in college and they are not affected by the roommate's current academic performance. The randomization of roommates solves any concerns about selection into social groups.

This approach requires the researcher to have pre-determined characteristics that are likely to exhibit social effects. In the above example, we might think that having a smart roommate affects a person's academic performance because her roommate could help her with homework or teach her better studying habits. We use a model to show that if a researcher does not have access to such a characteristic, a social group's past choice can serve as an index for all measured and unmeasured social group characteristics that affect an individual's current choice.

We combine our model with a conditional randomization of individuals to social groups to study social effects. Newly trained soldiers in the U.S. Army have no control over where they are transferred for their initial assignments. Because the Department of Defense assigns soldiers based on strategic needs, conditional on a small set of observables, soldiers are effectively randomized to units on military posts. There are a relatively large number of such units on any given military post (47 on average in our sample) and soldiers in these units work and live together, separate from other units. As such, members of a soldier's unit make up his social group.

As suggested by the model, we regress a soldier's financial decision twelve months after arrival at the new unit on the unit's mean financial decision from the month before the soldier arrived.[1] The conditional randomization of newly trained soldiers to units prevents any sorting into social groups; using the unit's past behavior as the treatment prevents contemporaneous shocks from biasing the estimated impacts and also clearly separates the impact of the unit on the soldier from any effects the soldier might have on the unit.

[1] One of our outcomes is measured in the January after the soldier arrives in the new unit because that is the first month in which a soldier's participation in the program is reflected.

We find that social groups matter for both of our charitable giving outcomes. A one standard deviation increase in the social group's Army Emergency Relief (AER) participation rate (18 percentage points) increases a soldier's participation rate by 10%; a one standard deviation increase in neighbors' Combined Federal Campaign (CFC) participation rate (23 percentage points) increases a soldier's participation rate by 8%. However, social groups do not have economically significant impacts on a soldier's retirement savings or life insurance purchase: a one standard deviation increase in group participation in the Thrift Savings Plan (TSP) increases retirement savings participation by 2% and reduces participation in the Servicemembers Group Life Insurance (SGLI) by 0.3%. Neither of these latter estimates is statistically distinguishable from zero.

We provide two potential explanations for the differences in findings across outcomes. First, in order for social groups to have an impact, a person must know what her group is doing. Regardless of the particular mechanism through which social effects occur—conformity (Bernheim, 1994), information transmission (Banerjee, 1992; Ellison and Fudenberg, 1993), etc.—if an individual does not observe or know her group's choices, then those choices seem unlikely to have any impacts on her. For both the Army Emergency Relief and Combined Federal Campaign programs, there are annual promotional campaigns that increase the salience of the programs and create an environment in which unit members have conversations about whether they have given to the programs. Even without explicit conversations, individuals' giving to these programs is relatively easy to observe—donations are routinely collected in a public, highly trafficked setting. Neither the retirement savings (TSP) nor the life insurance (SGLI) programs have similarly extensive promotional campaigns and soldiers sign up in private at their local military finance office.

Second, the institutional choice architectures for the SGLI and TSP may reduce the potential for social effects. Madrian and Shea (2001), Carroll et al. (2009) and Choi et al. (2003) demonstrate the power of default settings on financial behavior. These defaults can substitute for information that neighbors might provide and thereby reduce the importance of social groups in these domains. The SGLI has an explicit default and more than 80% of the newly trained soldiers choose this option. The TSP does not have an explicit default option, but there is enrollment assistance that might act as an implicit default option for many new employees. Thus, in the absence of workplace conversations about the TSP and SGLI, there is little scope for soldiers to learn about their unit's choices in these programs.

Our work is related to the literature on peer effects in financial decisions. Earlier work in this literature estimates positive, and often large, correlations between an individual's decision and her peers' decisions in stock market purchases and participation (Hong, Kubik, and Stein, 2004; Hong, Kubik, and Stein, 2005; Ivkovic and Weisbenner, 2007) and in charitable giving (Wu, Huang, and Kao, 2004). Some notable exceptions take advantage of natural experiments to estimate causal impacts of peers on retirement savings (Duflo and Saez, 2002) and charitable giving (Smith, Windmeijer, and Wright, 2013). More recently, field experiments have been used to isolate the mechanisms through which peer effects in financial decisions might operate. These experiments have shown that providing information to some individuals affects their peers' savings decisions (Duflo and Saez, 2003; Beshears, Choi, Laibson, Madrian, and Milkman, 2015), purchases of financial assets (Bursztyn, Ederer, Ferman, and Yuchtman, 2014), purchases of insurance (Cai, De Janvry, and Sadoulet, 2015), and charitable donations (Frey and Meier, 2004; Shang and Croson, 2009). Although these experiments are extremely informative about potential mechanisms, they do not directly estimate the naturally occurring, or organic, social effects at work in these settings. In particular, the experiments provide information that is costly to obtain; even though individuals do act on the information, they might not obtain it in their daily lives. Our estimates complement this line of literature by coming from a manipulation of social groups rather than information. To our knowledge, our results are the first estimates of social effects in financial decisions to come from a randomization of groups rather than a randomization of information. The results show the importance of this distinction as we only observe social effects for the outcomes for which there were informational campaigns and the groups' decisions were made publicly. In those outcomes that were private, we did not observe any economically or statistically significant social effects.

The rest of this article proceeds as follows. Section II provides background on the Army, its solders, and the four financial decisions we use as outcomes. Section III provides tests that support our assertion that soldiers are (conditionally) randomized to units. We use a model to show that the group's past behaviors can be interpreted as an index of all relevant group characteristics and to motivate our empirical specification in Section IV. Section V discusses our empirical strategy and regression specification. Section VI presents our main results. Section VII presents an analysis of role model effects and other extensions. Section VIII discusses the results and concludes.

II. Background

Enlisted members of the active duty Army, commonly referred to as "soldiers," begin their service with approximately 10 weeks of basic training followed by 2-52 weeks of Advanced Individual Training (AIT) where they learn the specific skills related to their job, known as their primary military occupational specialty. These jobs vary from infantryman to helicopter crewman to supply clerk to intelligence analyst. Both basic training and AIT are only conducted at certain locations around the United States. Upon completion of their AIT, soldiers are relocated to join an operational unit of the Army in the United States or abroad (e.g., Korea or Germany). This includes routine service at their post, field training exercises, and deployments to serve in missions from peacekeeping to disaster relief to combat. In each case, soldiers work and live in close proximity with members of their unit for an extended period of time.

As a matter of policy, the Department of Defense and its military Services (in this case, the Army) assign military personnel based on organizational requirements.[2] The assignment process is not random, as military units differ in their requirements for certain occupational specialties (e.g., infantry soldiers vs. intelligence analysts) and certain levels of expertise (e.g., Privates vs. Sergeants). These requirements may also vary over time as organizational compositions change. However, for any given job, rank, month-year, and post, soldiers are effectively randomly assigned to units. So if we observe two infantry soldiers with the rank of Private First Class arriving to Fort Hood in October 2007, one may be assigned to a unit with high levels of charitable giving or retirement savings while another may not. The result is that soldiers receive their assignments from a series of third party decisions that we demonstrate are likely to be exogenous to the financial decisions of potential social groups.

Military units provide a convenient setting in which to study social effects given their standardized and segmented operations. On Army posts, a unit lives and works together, and does so apart from the other units. Most Army members' interactions occur with individuals in their own unit based on the co-location of their offices, motor pools and other facilities. They begin their day together, typically with physical training, they share the same daily work tasks

[2] See for example, Department of Defense Directive 1315.07 "Military Personnel Assignments" and U.S. Army Regulation 600-14 "Enlisted Assignments and Utilization management."

given the team oriented nature of most military work, and they spend their evening and weekend leisure times together based on a common unit training schedule. This is especially true for the junior enlisted soldiers that we analyze in our sample, as they are typically required to live in the unit barracks, most eat meals at the unit dining facility since their food is subsidized, and they socialize with their unit members based on their common work schedules and limited transportation options. The mean number of soldiers in the units in our sample is 134.

To analyze the social effects in financial-decision-making, we use Army administrative data covering enlisted service members serving on active duty in the U.S. Army from 2005-2013. To strengthen our case for the conditional random assignment of individuals to units, we restrict our sample to male soldiers assigned to traditional combat units (e.g., Infantry and Army Brigade Combat teams) immediately after they complete their basic job qualification training.[3] Since these new enlistees have no say in their post or unit of assignment, their social groups should be as good as randomly assigned. In addition, these restrictions mean we are analyzing social effects in more homogenous group settings. Taken together, the assignment process and selected sample enable us to focus on a group of individuals that approximate an experimental assignment of social groups with varying financial environments. Not surprisingly, this strategy has been used by economists to study the causal effects of military experiences on a variety of topics including pollution and children's health (Lleras-Muney 2010), payday lending (Carrell and Zinman 2014; Carter and Skimmyhorn 2015) and parental absences (Angrist and Johnson 2000).

[3] In Appendix A, we present results that use both male and female soldiers. These results are extremely similar to those presented in the main text. We omit females from the main analysis because there is some evidence that they were not perfectly randomly assigned to units and they constitute a very small fraction of soldiers in traditional combat units (7 %).

The administrative data contain detailed information on individuals as well as their units. We observe and use data on an individual's age, race, education level, Armed Forces Qualification Test (AFQT) score, marital status, military occupation, rank, post and unit. We combine these data elements with administrative outcome data to perform our analyses at the individual level. We evaluate social effects with respect to four different financial outcomes: two charitable giving decisions, a defined contribution retirement saving decision and a term life insurance decision. We provide summary statistics for the data in Table 1.

We have two outcomes related to charitable giving. The first measures individual donations to Army Emergency Relief (AER), a private non-profit organization dedicated to helping soldiers and their families with financial challenges, primarily through no-interest loans, grants, and scholarships.[4] The mean individual AER participation rate in our sample is 24% and the mean unit participation rate is 21%. The mean unit donation is $1.46 per individual per month. We observe all monthly AER contributions made via direct deposit from an individual's military pay. Although we do not observe their donations in cash or via the website, these latter methods of giving account for a very small fraction of dollars donated and a minority of donations.[5]

The second outcome measures individual donations to the Combined Federal Campaign (CFC). The CFC is the world's largest annual workplace campaign, managed by the Office of Personnel Management (OPM) for all federal government agencies (including the Army), and it enables millions of employees to donate to one or more of thousands of charities of their

[4] See www.aerhq.org for more information on this charity. We provide a copy of the donation form in Appendix 1.
[5] Using estimates provided by the AER Deputy Director for Finance and Treasurer (email to authors) for 2014, allotments constituted 74% of donations (by count) and 93% (by amount) of active duty soldiers' contributions.

choosing.[6] Individuals can donate via cash, check or payroll deduction.[7] The mean individual CFC participation rate in our sample is 36% and the mean unit participation is 41%. The mean unit donation is $3.79 per individual per month. We observe the payroll deduction donations for Army members each month.

Our third outcome measures individual contributions to the Thrift Savings Plan (TSP), the world's largest defined contribution retirement savings plan. The TSP is available to federal government employees (including military members) and managed by the Federal Retirement Thrift Investment Board.[8] TSP rules and eligibility vary some but for the active duty Army members in our sample, the TSP provides traditional and Roth saving accounts but no matching funds, since uniformed service members are also eligible for a defined benefit plan. The TSP offers six different index funds (e.g., government securities [G], common stock index [C], and lifecycle [L]) with low fees.[9] Enrollment in the TSP must be completed online and all contributions are made via payroll deduction. As a result, we observe a complete account of all service members' TSP contributions each month, though we only observe total contributions and not fund choices. The mean individual TSP participation rate in our sample is 24% and the mean unit participation rate is 19%. The mean unit contribution is $40.40 per individual per month. While several studies have evaluated social network effects on retirement savings (e.g., Duflo and Saez 2002, Madrian and Shea 2000, Beshears et al. 2015), none have done so in plans of this scale (membership or geographic distribution) or for federal agencies.

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[6] See http://www.opm.gov/combined-federal-campaign/ for more information on this program. We provide a copy of the donation form in Appendix B.
[7] As of 2014 the CFC only accepts donations via checks, payroll deduction, or online giving.
[8] See https://www.tsp.gov/index.shtml for more information on the TSP. We provide a copy of the enrollment form in Appendix 1.
[9] Fees were 0.029% in 2014. For a summary of the funds see: https://www.tsp.gov/PDF/formspubs/tsplf14.pdf.

Finally, we analyze the Servicemembers Group Life Insurance (SGLI), a relatively low-cost term life-insurance program for military members.[10] The basic premium rate is 7 cents for each $1000 of insurance. Eligible members (including the active duty members in our sample) are automatically enrolled in the maximum coverage amount ($400K) but can make changes to reduce or eliminate their coverage, provided the selected coverage is in an increment of $50K. These changes must be made in person at the post personnel office and so the switching costs are not trivial. We observe complete data on the actual payroll deductions for each individual each month and calculate the implied life insurance coverage level (e.g., $400K costs $29/month, $300K costs $21/month). The mean individual SGLI participation rate in our sample is 84% and the mean unit participation is 97%. The mean unit coverage level is $305,000 per individual.

Both the AER and the CFC have annual promotions while the TSP and SGLI programs do not. The AER conducts an annual donation campaign from March 1st through May 15th that is administered separately for each unit. Every year, a designated member of the unit provides standardized information about the AER, distributes donation forms, and collects individual donations (cash, check, or automatic withdrawal forms for payroll deduction). Giving is not required, but Army units often set a 100% contact goal and soldiers might feel especially inclined to donate given the charity's salience and potential impact on them or their colleagues. The CFC also conducts an annual campaign (from September 1st through December 15th) that Army units support in much the same way as they do the AER campaign.

Although there are standardized materials and methods used by all units to promote the AER and CFC, the individuals who are in charge of any given unit's campaign may be more or less persuasive in obtaining donations. We consider this to be a social effect rather than a potential omitted variable bias because it is an influence that an individual service member has

[10] See http://www.benefits.va.gov/insurance/sgli.asp for more information on SGLI.

on his social group. These campaigns make the AER and CFC common topics of conversation within a unit and so they might increase the potential for social effects within these programs.

Although the new soldiers are randomized to their units, there is still considerable variation in their peers' financial decisions. Figures 1a-1d present the distributions of the AER, CFC, TSP and SGLI participation rates across our sample units. For the AER and the CFC, soldiers can be randomized to units with anywhere from zero participation to nearly complete participation. Participation rates in the TSP program are more condensed, but still vary from no participation up to roughly 50% participation. There is little variation in SGLI participation rates. Figure 2 presents the distributions for the average dollar amounts for each program. For example, Figures 2a and 2b suggest that the average contributions to the AER and the CFC in most units are just a few dollars. Although there is slightly more variation in the amounts for the TSP and SGLI programs, these distributions suggest that the major differences a soldier will be exposed to come from differential participation rates. As such, we will use a unit's participation rate as our main treatment measure.

III. Randomization Tests

We have argued that conditional on a full set of interactions between job, rank, post, and month-year, soldiers are randomly assigned to units. We test this in two ways. First, we check whether soldiers' observable characteristics are correlated with the treatments that they will be exposed to and second, we test whether soldiers' past behaviors are predicted by the treatments they will receive in the future. There are four separate treatment variables that we use: 1) the fraction of soldiers in the unit who give to the AER, 2) the fraction of soldiers in the unit who give to the CFC, 3) the fraction of soldiers in the unit who participate in the TSP, and 4) the fraction of soldiers in the unit who participate in the SGLI. These are measured for the unit that a new soldier will be transferred to upon completion of his initial training. We measure these treatments in the month before the soldier arrives at his new unit to preclude the possibility that the treatment is affected by the soldier himself.

Our balance tests regress the treatment a soldier is exposed to on that soldier's individual demographic characteristics. For each of our four outcomes, we estimate

$$\bar{Y}_{iut-1} = \beta_0 + X_{it}\beta_1 + \varphi_{jrlt} + \varepsilon_{iut} \tag{1}$$

where $\bar{Y}_{iut-1}$ is the mean participation for the unit *u* that soldier *i* is transferred to at time *t* (we measure participation rates in the month before the soldier arrives, denoted *t-1* here), X_{it} are the individual's demographic characteristics, φ_{jrlt} is a set of fixed effects for combinations of job, rank, post, and month-year, and ε_{iut} is the remaining error term. Standard errors are clustered by post. In the spirit of Altonji, Elder, and Taber (2005), we would be concerned about the validity of our conditional randomization if any of the demographic variables individually or jointly were strong predictors of the treatment.

The estimates are presented in Table 2. In the first two columns, the treatment is the fraction of the unit that gave to the AER in the month before the soldier arrived. In column (1), no demographic characteristics are included beyond the randomization controls. These controls account for 75% of the variation in treatment. As seen in column (2), including covariates for race, education, a quadratic in age, AFQT scores, and marital status does not increase our ability to predict treatment: the R-squared remains constant at 0.750 and an F-test for the joint significance of the added covariates does not reject the null of no effects.

The remaining columns conduct the same analysis for the other three treatments. In each case, the R-squared is unaffected by adding in soldiers' observables and the F-tests for joint significance of the observables are not rejected. These results provide support for the assertion that conditional on job, rank, post, and month-year, the soldiers in our sample are randomly assigned to units.

In addition to the balance tests, we run a placebo test that checks whether the treatment a soldier will receive in the future is correlated with his current behavior. In particular, for soldier *i* in unit *u* at time *t-1* (the month before the soldier transfers to the new unit) we estimate

$$y_{iut-1} = \beta_0 + \beta_1 \bar{Y}_{iut-1} + X_{it-1}\beta_2 + \varphi_{jrlt-1} + \varepsilon_{iut-1} \qquad (2)$$

where y_{iut-1} is the soldier's AER, CFC, TSP, or SGLI participation while in training (one month prior to arrival at the new unit), $\bar{Y}_{iut-1}$ is the mean of the participation for the unit that *i* will join at time *t* in the month before the soldier arrives *(t-1)*, X_{it-1} are the individual's demographic characteristics, φ_{jrlt-1} is a set of fixed effects for combinations of job, rank, post, and month-year, and ε_{iut-1} is an error term. Standard errors are clustered by post. For example, the regression tests whether soldiers who will be transferred to units with high AER participation rates are more likely to be giving to the AER even before they arrive at their new units. If so, then there would be evidence that the assignment of soldiers to units is not random after controlling for job, rank, post, and month-year.

The results are presented in Table 3. In column (1), the individual specific covariates, X_{it-1}, are omitted. The estimate suggests that there is not a strong relationship between the future unit's participation rate and whether the soldier was giving to the AER while still in training. The point estimate is quite small and is not statistically significant at conventional levels. In column (2), the soldier's demographic characteristics are included with little impact on the results. The remaining columns present the same placebo test for the three other outcomes. In each case, there is no measurable impact of future treatment on the soldier's behavior prior to transferring to the new unit. These tests provide further support for conditional random assignment in our sample.

IV. Model

As in Manski's (1993) seminal work, suppose that we can write an individual's choice as a function of her own characteristics, her social group's choices, her social group's characteristics, an unobservable shock that is common to all members of her social group, and other factors that affect her choice. The structural model for individual *i* in group *g* at date *t* is

$$y_{igt} = \alpha + \beta \bar{Y}_{gt} + z_{igt-1}\eta + \bar{Z}_{gt-1}\gamma + w_{gt} + \varepsilon_{igt} \tag{3}$$

where y_{igt} is the individual's choice, $\bar{Y}_{gt} = E_i[y_{igt}]$ is the average of her social group's choices, z_{igt-1} is a vector of length *k* of the individual's exogenous characteristics (determined in period *t-1*), $\bar{Z}_{gt-1} = E_i[z_{igt-1}]$ is a vector of length k of the averages of social group members' exogenous characteristics, w_{gt} is a group-specific, time-varying common shock, and ε_{igt} captures remaining influences on the individual's choice. The social effect β, the impact of the group's current choices, is distinct from γ, the influence of having a social group with certain characteristics. Manski (1993) terms the former endogenous social effects, the latter exogenous social effects.

There are at least three challenges to recovering the true parameters of equation (3). First, there is a simultaneity bias affecting β because not only does the group affect the individual, but the individual affects the group as well. This is the well-known reflection problem. Second, common shocks are likely to cause a standard omitted variables bias. Third, individuals often select which social group they join. If this selection is related to their characteristics and choices, then the estimated coefficients from equation (3) will be biased.

A commonly used approach to circumvent these issues is to integrate equation (3) over individuals (within a group),

$$\bar{Y}_{gt} = \frac{\alpha}{1-\beta} + \bar{Z}_{gt-1}\frac{\gamma+\eta}{1-\beta} + w_{gt}\frac{1}{1-\beta} \tag{4}$$

and substitute this back into equation (1) to yield the reduced form

$$y_{igt} = \left(\frac{\alpha}{1-\beta}\right) + z_{igt-1}\eta + \bar{Z}_{gt-1}\left(\frac{\gamma+\beta\eta}{1-\beta}\right) + w_{gt}\left(\frac{1}{1-\beta}\right) + \varepsilon_{igt}. \tag{5}$$

When combined with exogenous assignment of social groups, estimating the reduced form yields unbiased estimates of the combinations of endogenous and exogenous structural parameters. Without further restrictions, the individual structural parameters are not separately identified. Many papers that estimate social effects take this approach. For example, Sacerdote (2001), Zimmerman (2003), Lyle (2007), and Carrell, Sacerdote, and West (2013) regress a student's college GPA on a measure of her academic ability and a measure of her randomly assigned peers' academic abilities; Guryan, Kroft, and Notowidigdo (2009) regress professional golfer's scores on their own ability as well as the ability of their randomly assigned playing partners.

In these and many other cases, there is at least one observable variable in z_{igt-1} that influences the individual's choice, i.e. there is at least one variable such that $\eta \neq 0$. This provides a reason to think that the corresponding variable in $\bar{Z}_{gt-1}$ could also impact the individual's choice if social effects are important. When looking at academic achievement, a student's S.A.T. score is an important predictor of college G.P.A.; in the context of professional golf, indicators of past performance such as average driving distance, putts, or greens per round are tightly linked to current scoring. However, in some contexts, there will not be a set of observable z_{igt-1} or $\bar{Z}_{gt-1}$ that explain a large portion of the variance in behaviors. When estimating the reduced form (equation (5)) in these cases, it is not clear whether failure to reject the null of no effect is due to there being no true social effects or simply not having measures of the characteristics on which there are social effects.

We show how using a group's past choices can circumvent the problem of observing only a subset (or potentially none) of the group's characteristics that affect an individual's choice. The insight is that a group's behavior reflects all of the exogenous characteristics that impact their choices. First, note that a group's average characteristics, $\bar{Z}_{gt-1}$, are likely to be correlated from one period to the next. One likely reason for this is the selection of individual's into groups based on having similar characteristics. However, even if individuals are assigned to groups randomly, we could still have a positive correlation in group characteristics over time as long as group members join and leave continuously. In this case, some subset of the group will be the same across adjacent time periods and will mechanically create a non-zero correlation. Thus, we would expect $\hat{\mu}_1$ from the following regression to be nonzero and positive,

$$\bar{Z}_{gt-1}\left(\frac{\gamma+\beta\eta}{1-\beta}\right)= \mu_0+\mu_1\bar{Z}_{gt-2}\left(\frac{\gamma+\eta}{1-\beta}\right)+\vartheta_{gt-1} \tag{6}$$

Substituting the period *t-1* version of equation (4) that has been solved for $\bar{Z}_{gt-2}\left(\frac{\gamma+\eta}{1-\beta}\right)$ into equation (6) yields

$$\bar{Z}_{gt-1}\left(\frac{\gamma+\beta\eta}{1-\beta}\right)= \mu_0+\mu_1\left[-\frac{\alpha}{1-\beta}+\bar{Y}_{gt-1}-w_{gt-1}\frac{1}{1-\beta}\right]+\vartheta_{gt-1} \tag{7}$$

This shows how all of the social group's characteristics are related to the group's past choices. Because $\bar{Z}_{gt-1}\left(\frac{\gamma+\beta\eta}{1-\beta}\right)$ captures all of the social group's characteristics, positive impacts of some characteristics ($\gamma_j>0$) can be cancelled out by negative impacts of other characteristics ($\gamma_k<0$). However, to the extent that social groups are a bundle of characteristics, the total impact as presented in equation (7) is the relevant object for determining whether a social group's exogenous characteristics have a non-zero net effect on an individual's behavior.

Using the relationship between exogenous characteristics and past behavior in equation (7), an individual's choice can be written as a function of her social group's past choice and her own exogenous characteristics

$$y_{igt}= \pi_0+\pi_1\bar{Y}_{gt-1}+z_{igt-1}\pi_2+\left(\frac{1}{1-\beta}\right)\left(w_{gt}-\mu_1 w_{gt-1}\right)+\xi_{igt}. \tag{8}$$

The coefficients in equation (8) can be related back to the structural model's parameters: $\pi_0 = \left[\left(\frac{\alpha}{1-\beta}\right) + \mu_0 - \mu_1\left(\frac{\alpha}{1-\beta}\right)\right]$, $\pi_1 = \left(\left[\bar{Z}_{gt-2}(\gamma + \beta\eta)\right]'\left[\bar{Z}_{gt-2}(\gamma + \beta\eta)\right]\right)^{-1}\left[\bar{Z}_{gt-2}(\gamma + \beta\eta)\right]'\left[\bar{Z}_{gt-1}(\gamma + \eta)\right]$, $\pi_2 = \eta$, and $\xi_{igt} = \left[\vartheta_{gt-1} + \varepsilon_{igt}\right]$. This shows that π_1 is a combination of endogenous and exogenous social effects. As is usual in empirical studies based upon Manksi's (1993) linear-in-means framework, without additional restrictions, the structural parameters from the model are not individually identified: Equation (8) provides *k+2* coefficients, but there are *2k+2* parameters in the original structural model.

The advantage of using the social group's past behavior as a regressor is that it is able to serve as an index for all of the social group's observed and unobserved, pre-determined characteristics that affect the individual's outcome. In cases where an incomplete—or even empty—list of appropriate group characteristics are available, this approach provides a simple and parsimonious solution to the problem.

Although this approach has not been widely used in the peer effects literature, a related strategy has been used in the analyses of the Moving to Opportunity Experiments. For example, Kling, Liebman, and Katz (2007) regress an individual's outcomes on her neighborhood's poverty rate in the previous year and interpret the coefficient on the poverty rate as an index of all the neighborhood's characteristics that might be related to the individual's choices and outcomes.

As with the usual reduced form approach taken by past research, the group's past behavior and the individual's pre-determined characteristics are not biased by the omitted common shock from period *t*. However, it is clear from equation (8) that a simple OLS regression could produce biased estimates of π_1 because $\bar{Y}_{gt-1}$ is correlated with the previous period's common shock which is now in the error term. We discuss how we overcome this omitted variable bias in the next section.

V. Empirical Strategy

Our empirical strategy takes advantage of the conditional random assignment of soldiers to military units with varying financial environments. We limit our sample to soldiers who are just finishing their job qualification training and are transferred to a new unit for the first time. We adapt equation (8) to our empirical setting and estimate

$$y_{iut} = \pi_0 + \pi_1 \bar{Y}_{ut-1} + z_{iut-1}\pi_2 + \left(\frac{1}{1-\beta}\right)(w_{ut} - \mu_1 w_{ut-1}) + \varphi_{jrlt} + \xi_{iut}. \tag{9}$$

where y_{iut} is the outcome of interest twelve months after soldier i's arrival at the new unit u in month-year t, $\bar{Y}_{ut-1}$ is the mean of the outcome for the new unit in the month before the soldier's arrival, z_{iut-1} are the individual's demographic characteristics, φ_{jrlt} is a set of fixed effects for combinations of job, rank, post, and month-year, and ε_{iut} is the remaining error term. We cluster our standard errors at the post level.

Our primary interest is in the coefficient π_1 which tells us how an individual soldier's behavior is related to the past behavior of his neighbors. Intuitively, our specification compares outcomes for soldiers who are sent to the same military post in the same month and year, but are put in different units at that post. It is important to recognize that soldiers are not randomized to values of $\bar{Y}_{ut-1}$, they are randomized to particular units whose members differ on many different dimensions. As discussed in the model, we view $\bar{Y}_{ut-1}$ as a summary measure of the unit's characteristics that affect the soldier's choice ($\bar{Z}_{gt-2}$).

As we saw in Figures1a-1d, the means and variances of our treatment variables are quite different across the programs. We use a one standard deviation increase in the participation rates to interpret the size of our point estimates. Although this corresponds to different percentage point increases in the fraction participating in a program, it standardizes the variation in treatments that a soldier would face when being transferred to one unit instead of another.

Because the soldiers in our sample are randomly assigned to units, our estimates are not impacted by individuals sorting into social groups. We might worry that this same random assignment breaks down the correlation over time of a social group's characteristics. However, the structure of the military ensures that there will be a fairly strong correlation from one year to the next. When a soldier enlists, his contract typically lasts three to four years and soldiers rarely change units except when they are starting a new contract. Thus, a soldier's social group twelve months after he arrives at the unit will be comprised of roughly two-thirds of the soldiers who were in the unit when he arrived. Although we cannot estimate μ_1 directly, the structure of the military suggests that it will be strictly positive.

We circumvent the reflection problem by using treatments that could not have been affected by the soldier being treated—because our treatment variable is the unit's behavior in the month before the soldier arrived, the soldier's choices after arrival cannot affect the treatment he receives. This delineation between the treated group (the soldiers arriving at a new unit) and the treatment (behaviors of those already at the units) frees us from the reflection problem.

As emphasized in Lyle (2007) and Guryan, Kroft, and Notowidigdo (2009), common shocks can have meaningful impacts on estimated social effects. Our use of the unit's behavior in the month before the soldier arrives ameliorates concerns about common shocks in two ways. First, it is not possible for the individual soldier to be influenced directly by his new unit's common shock from the previous period: he was not physically in the unit when that shock materialized. As a result, w_{ut-1} will not bias our estimate of π_1 because w_{ut-1} will not have a direct impact on the soldier's choice twelve months after arriving at the new unit. Second, any shock at time *t* that affects the soldier's behavior will not have affected the unit's behavior in the past; i.e. w_{ut} will not be directly related to $\bar{Y}_{ut-1}$ and thus will not cause a bias in $\hat{\pi}_1$. However, if contemporaneous common shocks are correlated over time, then a common shock at time *t* could be correlated with both y_{iut} and $\bar{Y}_{ut-1}$. In that case, our estimate for π_1 would be biased. It is likely that shocks in adjacent periods are more highly correlated than shocks in periods farther apart (e.g. shocks might follow an AR(1) process). Thus, if common shocks are driving the results, the relationship between the treatment variable and a soldier's outcome three months after arrival should be larger than the relationship between the treatment variable and the soldier's outcome six or twelve months after arrival. We use this idea and explore the time pattern of our estimated effects to assess whether autocorrelation in the common shocks is biasing our results.

VI. Main Results

We present the main results in Table 4. For each of our four outcomes we provide two estimations of equation (9), one without covariates (odd numbered columns) and one with covariates (even numbered columns). Based on our point estimate (0.132) in column (1), a one standard deviation (18.4) increase in the unit's participation rate increases participation in the AER by 2.4 percentage points. Relative to the sample mean (23.8%), this represents a 10% increase in the probability of giving. When soldiers' demographic characteristics are included in the regression (column (2)), the results are nearly unchanged.

In columns (3) and (4), we present the same set of regressions for our other charitable giving outcome, participation in the CFC. As in the AER, we find that being sent to a unit with higher social group participation increases the probability that the soldier participates in the CFC. The point estimate implies that a one standard deviation (23.3) increase in the unit's participation rate increases a soldier's probability of giving to the CFC by 2.9 percentage points. Because 36.2% of soldiers participate in the CFC, this represents an 8% increase in the probability of giving. Again, we find that adding in a soldier's demographics does not affect the results.

Columns (5) and (6) present the results for the Thrift Savings Program. Unlike the charitable giving outcomes, we do not find strong evidence for an impact of the social group on the individual's savings decisions. If the point estimate were the true impact, it would imply that a one standard deviation (10.4) increase in the participation rate would increase participation in the savings program by 0.6 percentage points or 2% of the baseline savings rate. The results are therefore statistically and economically insignificant.

The results for our final outcome measure, life insurance purchase, are presented in columns (7) and (8). If true, the point estimate would imply that a one standard deviation increase in the participation rate would lead to a 0.3 percentage point or 0.3% reduction in life insurance purchases. As with savings, we do not find evidence of substantial social effects.

As discussed in the previous section, autocorrelation in a unit's unobservable, time-varying shocks could be biasing our results. First, it is worth noting that autocorrelation in the common shocks would suggest that we find positive social impacts for all of our financial outcomes. Finding positive social effects for only two of our outcomes immediately reduces concerns that autocorrelation in the common shocks is driving the results. To further explore the role of autocorrelation in the common shocks, we assess whether the relationship between the social group's participation and the soldier's behavior becomes weaker over time. For three of our four outcomes—AER, TSP, and SGLI—soldiers are able to begin participating at any point in the year (soldiers can only sign up for the CFC during its annual campaign).[11] Thus if the impacts of the social group on the individual become smaller in magnitude over time, that would suggest that our positive findings for the charitable giving outcomes are spurious.

Table 5 presents the results for our outcomes at three different lengths of time: three months after the soldier arrived to the unit, six months after the soldier arrived, and twelve months after the soldier arrived. For the AER, we see that the impact of the social group actually appears to increase with the time the soldier has spent in the unit. This is not consistent with the hypothesis of autocorrelated shocks. Instead, it is plausible that the impact of the social group grows as one spends time with them—it takes time to get to know people well enough to learn about and be influenced by their personal financial decisions like charitable giving. We do not find strong evidence of autocorrelated shocks in either of our other outcomes (TSP or SGLI). Taken together, these results suggest that our main estimates were not driven by unit level, time-varying shocks.

Although our analysis has focused on the extensive margin of whether or not a soldier participates in the AER, CFC, TSP, or SGLI, we have also explored whether the intensive margin was affected. In particular, we estimated whether a unit's participation in a program affects the *amount* a soldier gives to that program (where non-participants are coded as zeros). These results are presented in Table 6**.** Although we saw significant impacts on a soldier's participation, we do not find statistically significant or consistent evidence that the amount given

[11] The single sign-up period (one per year) prevents this analysis from having a meaningful interpretation for the CFC outcome. The impact of the social group at three months and six months is almost necessarily zero for soldiers who arrived at their new units from January through June because they will not have had a chance to sign up for the CFC by the three and six month marks. As such, comparing the results at these different time frames is not likely to tell us much about autocorrelation in the unobservable common shocks with respect to the CFC.

is affected. A one standard deviation increase in a unit's AER participation rate (18 percentage points) is estimated to reduce the soldier's AER giving by 11% ($0.17). This suggests that if the point estimate were correct, soldiers induced to participate by their units' AER participation are likely to give lower donations. However, for the CFC program we do not observe a similar pattern: a one standard deviation increase in the unit's CFC participation rate (23 percentage points) is estimated to increase a soldier's CFC giving by 2% ($0.09). For the savings and life insurance programs, we again find no evidence of a social effect.

VII. Extensions

It is unlikely that a soldier interacts equally with everyone in his unit. Our sample of junior enlisted men are more likely to interact with other junior enlisted men rather than with the commissioned officers. There are a number of reasons for this including that junior soldiers and officers live apart from one another (the latter often living off the base); they eat separately (officers typically do not dine in the military cafeterias and typically sit together if they do); and they do not socialize with one another when off duty (fraternization policies restrict such interactions). Finally, their work interactions are less frequent and conducted with the unit's mission requirements in mind. Normally, this would suggest that the officers' program participation should have little impact on the junior enlisted men, but in our particular case, there could be social effects in the form of role model effects (rather than peer effects). If officers convey their own charitable giving or savings decisions to junior enlisted soldiers in briefings, personal interactions, or unit communications, this could provide important information to the junior enlisted soldiers on what behaviors are expected of them or what behaviors they seek to demonstrate. Thus empirically, it is not clear whether officers' participation rates will affect junior enlisted soldiers' decisions.

To evaluate this question, we augment our specification to have separate treatment variables for officers. For the unit the soldier will be transferred to, we include a variable for the fraction of junior enlisted (grades E1-E4) men who participated in the program and a variable for the fraction of officers who participated in the program. Again these participation measures are for the month before the soldier arrived. We present results from these augmented regression specifications in Table 7**.** In this table, the estimates in each column come from a separate regression. For example, the top two entries in column (1) report the estimated impacts of the unit's junior enlisted and officer participation rates in the AER on the soldier's AER participation (0.117 and -0.005 respectively). Our estimated impacts on charitable giving are coming primarily through the behavior of other junior enlisted peers. We do not find evidence for role model effects found in other military settings (e.g., for academic major choices of West Point cadets as in Lyle 2007 or junior officers' military performance as in Lyle and Smith 2014). This could be due to the more private or personal nature of many financial decisions, a difference in the mentoring between the settings, or simply that a more senior person's optimal savings, charitable giving, and life insurance decisions are not the same as those for a junior enlisted soldier. Although a full exploration of this difference is beyond the scope of this paper, it is worthy of further attention.

As mentioned previously, we interpret our treatment variables as indexes of everything about a unit that is related to its participation in the outcomes. However, when a soldier arrives at a new unit, he is exposed to the social group's decisions for the AER, CFC, TSP and SGLI simultaneously. We can make some progress towards isolating the impact of each of the unit participation rates by including all of the treatment variables in our main regression specification. For example, the AER and CFC charities could be complements with each other. In that case, units with high AER participation rates might also have high CFC participation rates. When we estimated the impact of a unit's AER participation rate on a soldier's probability of giving to the AER, our previous estimates will have attributed the impacts of a unit's AER and CFC participation rates only to the AER participation rate.

Table 8 presents the results when all of the treatment variables are included for each of our four outcomes. In column (1), the estimated coefficient on the unit's AER participation rate is 0.124; as a point of comparison, the estimated coefficient on the unit's AER participation in our original specification is extremely similar, 0.133. Although the unit's CFC participation is (marginally) related to a soldier's AER giving, the impact of the unit's AER giving is not materially affected by the inclusion of their CFC giving. We find similar results for the other three outcomes. Our estimated impact of units' CFC giving on a soldier's CFC giving went from 0.126 to 0.112; for the TSP it went from .054 to .042; and for the SGLI, it went from -0.017 to -0.016. Overall, this provides additional support for interpreting our main results as causal effects.

VIII. Discussion

Using exogenous variation in social groups' financial environments driven by military assignments, we find important social effects with respect to two charitable giving decisions but little or no evidence of these effects with respect to retirement savings and life insurance decisions. We briefly explore potential explanations for these differences and discuss potential policy applications.

The results above raise the question as to why we observe differential effects for different financial choices. We offer one potentially unifying explanation but note that our evidence is only suggestive. Beshears et al. (2015) note that information about peers' decisions might move individual decisions towards the group average by revealing private information about individual payoffs or by highlighting a social norm. Interestingly, both they and Bhargava and Manoli (2015) find an oppositional reaction in which providing information on peers' decisions reduced the desired behaviors. Duflo and Saez (2002) suggest that peer effects in the workplace operate through discussion and information sharing as opposed to simple imitation. This explanation resonates with us but requires more understanding of the relative levels of conversation and information sharing for each of our outcomes.

The AER and CFC conduct annual campaigns within the military (discussed above) and these campaigns provide a natural opportunity for increased discussions as well as information provision about what social group members have done previously, how the unit members may have behaved in the past, and so on. The result is that for these financial decisions the charities and the military have provided a substantial amount of information and also generated many opportunities for workplace conversations. To the extent that these conversations or information sessions are public or generate subsequent small group discussions, they may reveal the private information and norms highlighted by Beshears et al. (2015). However, because the campaigns are conducted without explicit information on group savings rates and individuals may not feel judged by the nature of the group discussions, these organic social effects may not generate the oppositional effects found previously. This may explain our strong findings in these areas.

Conversely, while the charities and their associated campaigns may generate meaningful discussions and the exchange of information, the TSP and SGLI programs are generally not thought to do so. For SGLI, individual decisions are often made in private since soldiers typically validate or adjust their coverage levels at arrival to a new unit and in preparation for a deployment. The health related nature of these decisions may place them in a more private sphere that soldiers are less likely to talk about. In addition, the initial default levels (to maximum coverage) and the choice architecture that requires individuals to opt-out of coverage may also reduce the potential for organic social effects to take hold. Given related findings on the stickiness of defaults (Carroll et al., 2009) and the high participation rates (84% of soldiers in our sample have SGLI coverage), soldiers may not learn much about SGLI and its appropriateness. The result is that individuals are neither prepared to have workplace discussions on life insurance nor are there many opportunities for them to do so. As Duflo and Saez (2002) note, this may explain Madrian and Shea's (2000) results in which automatic enrollment for some employees in employer-sponsored savings plans did not increase voluntary enrollment by others. As a result, social effects within financial decisions may be challenging to detect in the face of strong choice architectures.

The combination of unique institutional features may explain our TSP findings along similar lines. Recall that soldiers are not defaulted into the TSP and there are no significant incentives for participation such as matching. In terms of institutional features, military members are eligible for a defined benefit pension and this may reduce their motivation to learn about the TSP and its benefits. In addition, the Army provides a significant amount of financial education to its new soldiers (especially since 2008) and this education is often paired with deliberate enrollment assistance, which Skimmyhorn (2016) shows dramatically increases participation. This greater participation may operate similar to a default ("sign here") as much as an opportunity to build financial human capital. The result is that soldiers may not be especially well-informed about the TSP and may be uncomfortable having conversations with their social group about the benefits of participation. We note that TSP decisions are also made privately, with soldiers visiting their local military finance office or logging on through a computer, thereby reducing the opportunities for workplace discussions.

Taken together, the unifying explanation for our differential findings by financial choice suggests that organic social effects may operate when there are natural opportunities for social group members to discuss programs and share their experiences and information. These interactions will vary depending on the institutional features of the financial decision and the workers' comfort and confidence in talking about their choices.

While this explanation is plausible, we cannot rule out other explanations for our results. One possibility is that social effects are less likely to overcome preferences (generally assumed to be fixed, and likely somewhat stable over the time horizons we consider here) than they are to overcome information deficits. In our setting this might mean that the CFC and AER results demonstrate that social effects can overcome information shortages that change an individual's willingness to donate, but that they are less likely to change more enduring risk preferences (in the case of SGLI) or time preferences (in the case of the TSP). Given existing findings on the potential for peers to affect individual risk preferences (Ahern, Duchin, and Shumway, 2014) and entrepreneurial decisions (Lerner and Malmendier, 2013) this possibility seems less likely. One other possibility is that retirement savings and life insurance decisions are viewed by individuals as more private or personal in nature and therefore less amenable to conversations. So while we have grouped all of our choices in a broad financial framework, individual retirement savings and life insurance decisions may be viewed as more sensitive and private than charitable donations, especially given the campaigns for the latter.

The external validity of our estimates warrants some attention. On the one hand, there is reason to believe that our sample is similar to other young populations with respect to three of our financial decisions. For the two outcomes where we estimate significant social effects (charitable giving), our sample looks very similar to young individuals (18-24) nationwide. Andreoni (2015) estimates that about 33% of this group donates to charity. Our CFC (which include churches, the most common source for low income family donations) estimates are similar with 36% of soldiers participating. In addition, our retirement savings (i.e., TSP) estimates are also similar to the civilian population with 24% of sample members participating compared to 23% of civilians nationwide.[12] Our sample differs in important ways with respect to life insurance decisions with our sample members participating at much higher rates (84%) than their civilian peers (33%), perhaps a result of the Department of Defense life insurance program attempting to overcome the adverse selection problem.[13]

On the other hand, military life differs in many important ways from civilian life. Selection into the military, the prevalence of teamwork in most jobs and daily work, and the proximity of work and leisure lives (as described in Section II) all suggest that social effects may be more likely in the military setting. If so, our estimates might serve as upper bounds for the role of social groups in influencing individual financial decisions. The estimates might generalize most usefully to other military services, public sector organizations, and other settings that include workplace campaigns, substantial teamwork and/or proximate living arrangements.

The policy implications for our findings vary by domain. For charitable organizations and employers interested in increasing donations, workplace campaigns and other organizational

[12] Author calculations using the 2009 National Financial Capability Studies. We compare 18-24 year old enlisted military respondents to similarly aged civilian respondents. Data available: http://www.usfinancialcapability.org/downloads.php

[13] See footnote 12 for a description of the method.

policies designed to increase peer interactions and discussions may create positive externalities. One important element may be the generation of workplace conversations that enables endogenously selected peers to discuss their choices. We cannot definitively say whether the lack of social effects for the savings and life insurance programs is due to few opportunities for discussion given the unique military choice architecture or due to less of a role of social groups in these decisions. They might also suggest that when individuals do not invest their own time and effort in a financial choice, then there may be reduced potential for positive externalities or social multiplier effects in programs that promote these activities such as workplace financial education or choice architecture reforms. Finally, our results suggest that fostering broader communication about the information received in the multitude of financial education efforts could itself be an important component to these policies.

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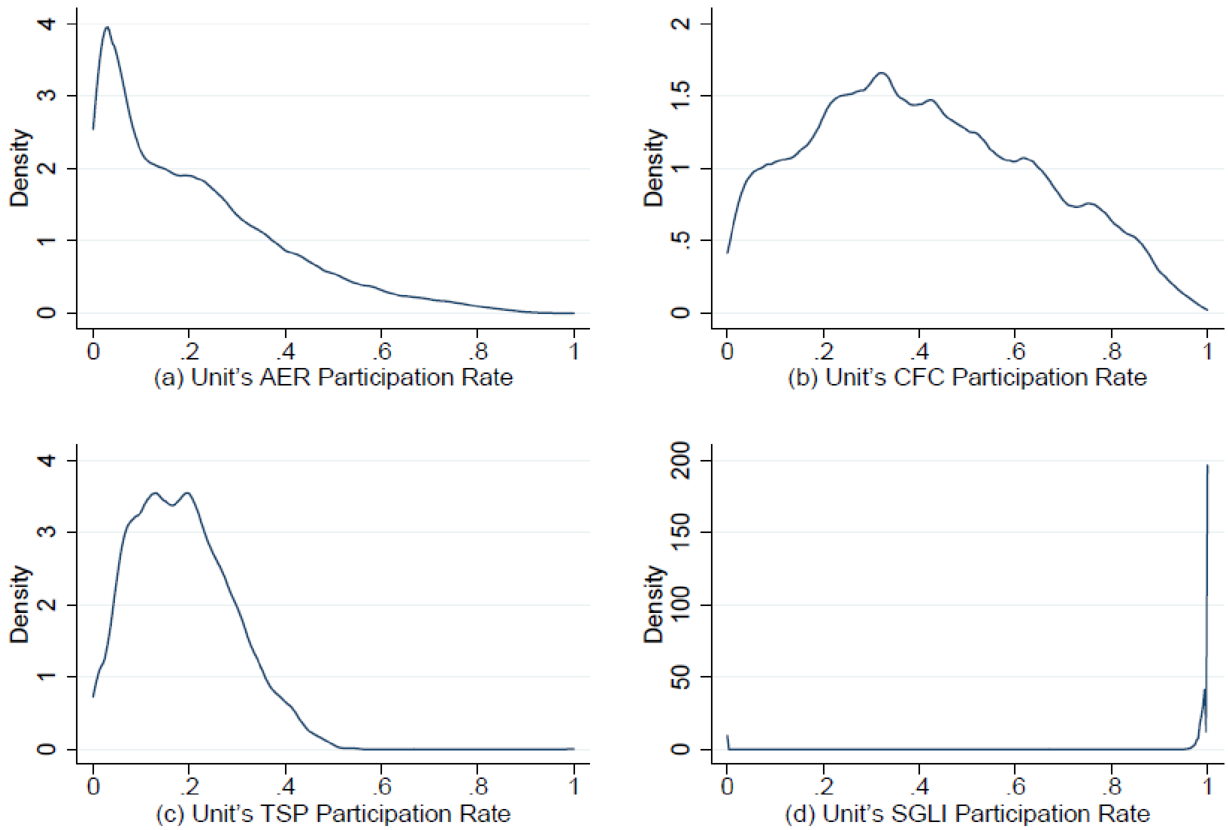

Figure 1: Pre-Arrival Unit Participation Rate Distributions

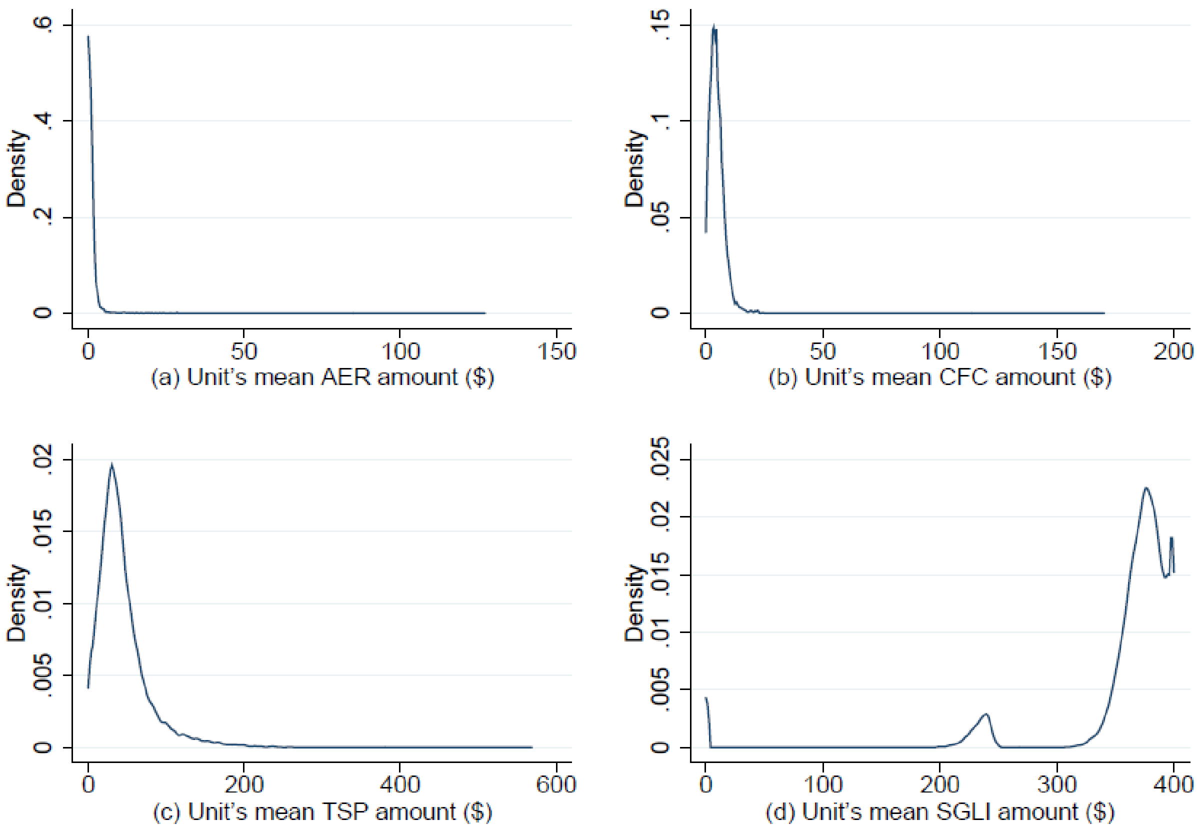

Figure 2: Pre-Arrival Unit Mean Dollar Amounts Distributions

Table 1: Summary Statistics

	Mean	Standard deviation
Panel A: Soldiers' Outcomes		
AER	0.238	0.426
CFC	0.362	0.481
TSP	0.235	0.424
SGLI	0.839	0.368
Panel B: Unit Participation Rates in Programs (Treatment)		
AER	0.210	0.184
CFC	0.411	0.233
TSP	0.187	0.104
SGLI	0.971	0.148
Panel C: Soldiers' Demographics (Covariates)		
White	0.683	0.465
High school degree	0.860	0.347
College degree or more	0.048	0.214
Age	23.150	4.662
AFQT score	58.287	19.237
Married	0.289	0.453

The data are for male soldiers in traditional combat units who were transferred to their first unit between 2003 and 2012. AER is Army Emergency Relief; CFC is Combined Federal Campaign charities; TSP is Thrift Savings program; SGLI is Servicemembers Group Life Insurance. Panel A presents means and standard deviations of outcomes for soldiers in our sample twelve months after arrival at the new unit. Panel B presents participation rates for the units the soldiers were transferred to in the month prior to the soldier's arrival. Panel C presents soldiers' demographic information.

Table 2: Balance Tests

	(1) AER	(2) AER	(3) CFC	(4) CFC	(5) TSP	(6) TSP	(7) SGLI	(8) SGLI
White		0.00113		0.000593		0.00105		4.29e-05
		(0.00184)		(0.00161)		(0.000743)		(9.40e-05)
High school degree		0.000958		-0.00337		0.000506		0.000113
		(0.00197)		(0.00360)		(0.000530)		(0.000212)
College degree		1.32e-05		0.00389		0.00206		-0.000331
		(0.00368)		(0.00733)		(0.00154)		(0.000466)
Age		6.60e-05		-0.000721		-0.000891		-4.91e-05
		(0.00139)		(0.00197)		(0.000548)		(0.000125)
Age-squared		-4.58e-06		9.04e-06		1.57e-05		1.07e-06
		(2.50e-05)		(3.32e-05)		(1.03e-05)		(2.55e-06)
AFQT score		-8.13e-05*		-2.53e-05		4.29e-06		5.78e-07
		(4.72e-05)		(5.44e-05)		(1.61e-05)		(2.54e-06)
Married		0.00171		0.00143		-0.000198		-3.12e-05
		(0.00101)		(0.00210)		(0.000768)		(0.000103)
Observations	81,666	81,666	81,927	81,927	81,666	81,666	81,666	81,666
R-squared	0.750	0.750	0.753	0.753	0.913	0.913	0.998	0.998
Job x rank x post x month-year FE	yes	yes	yes	yes	yes	yes	yes	yes
Demographics	no	yes	no	yes	no	yes	no	yes
p-value of F-stat	-	0.199	-	0.196	-	0.392	-	0.929
Sample mean	0.210	0.210	0.411	0.411	0.187	0.187	0.971	0.971

Dependent variable is participation rate of unit the soldier will be transferred to (program given in column heading). Indicators for interactions between job, rank, post, and month-year included in all specifications. Soldiers' demographics included in even numbered columns. P-value of F-statistic for joint significance of demographics reported. Standard errors clustered by post.
*** p<0.01, ** p<0.05, * p<0.1

Table 3: Placebo Tests, Impact of Future Unit's Participation Rate on Soldiers' Behavior in Month Preceding Move

	(1) AER	(2) AER	(3) CFC	(4) CFC	(5) TSP	(6) TSP	(7) TSP	(8) TSP
Unit participation rate	-0.023	-0.023	0.004	0.004	0.020	0.017	-0.013	-0.013
	(0.018)	(0.017)	(0.008)	(0.008)	(0.078)	(0.079)	(0.026)	(0.025)
Observations	80,296	80,296	80,557	80,557	80,296	80,296	80,296	80,296
Adjusted R-squared	0.400	0.401	0.361	0.362	0.255	0.258	0.419	0.420
Job x rank x post x month-year FE	yes	yes	yes	yes	yes	yes	yes	yes
Demographics	no	yes	no	yes	no	yes	no	yes
Unit participation rate std. dev.	0.184	0.184	0.232	0.232	0.104	0.104	0.0770	0.0770
Sample mean	0.103	0.103	0.113	0.113	0.179	0.179	0.988	0.988

Dependent variable is whether soldier participated in program (specified in column heading) in the month before arriving at new unit. Unit participation rate is the new unit's average participation in the specified program in the month before the soldier arrives. Indicators for interactions between job, rank, post, and month-year included in all specifications. Soldiers' demographics included in even numbered columns. Standard errors clustered by post. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Impact of Unit Participation Rates on Soldiers' Behaviors Twelve Months After Transfer

	(1) AER	(2) AER	(3) CFC	(4) CFC	(5) TSP	(6) TSP	(7) SGLI	(8) SGLI
Unit participation rate	0.132**	0.133**	0.130***	0.130***	0.054	0.051	-0.017	-0.018
	(0.059)	(0.059)	(0.051)	(0.050)	(0.085)	(0.085)	(0.026)	(0.026)
Observations	81,666	81,666	81,927	81,927	81,666	81,666	81,666	81,666
Adjusted R-squared	0.134	0.135	0.200	0.201	0.188	0.192	0.959	0.959
Job x rank x post x month-year FE	yes	yes	yes	yes	yes	yes	yes	yes
Demographics	no	yes	no	yes	no	yes	no	yes
Unit participation rate std. dev.	0.184	0.184	0.233	0.233	0.104	0.104	0.148	0.148
Sample mean	0.238	0.238	0.362	0.362	0.235	0.235	0.839	0.839

Dependent variable is whether soldier participated in program (specified in column heading) twelve months after arriving at new unit, except for columns (3) and (4). In those, dependent variable is indicator for participation in the CFC in the January following the soldier's first CFC campaign after arriving at the unit. Unit participation rate is the new unit's average participation in the specified program in the month before the soldier arrives, except (3) and (4) which use the unit's participation in the January preceding the soldier's arrival. Indicators for interactions between job, rank, post, and month-year included in all specifications. Soldiers' demographics included in even numbered columns. Standard errors clustered by post. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Impacts of Neighbors for Different Lengths of Time After Arrival

	(1)	(2)	(3)
	3 months	6 months	12 months
		AER	
Unit participation rate	0.004	0.052**	0.133**
	(0.017)	(0.022)	(0.059)
		TSP	
Unit participation rate	0.009	0.027	0.051
	(0.089)	(0.082)	(0.085)
		SGLI	
Unit participation rate	-0.021	-0.025	-0.018
	(0.034)	(0.034)	(0.026)

Each point estimate is from a separate regression. The dependent variable is whether soldier participated in program at the specified number of months after arriving at new unit. Unit participation rate is the new unit's average participation in the specified program in the month before the soldier arrives. Indicators for interactions between job, rank, post, and month-year as well as soldiers' demographics are included in all specifications. Standard errors clustered by post. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Impact of Unit Participation Rates on Soldiers' Behaviors Twelve Months After Transfer, Dollar Amounts

	(1) AER	(2) AER	(3) CFC	(4) CFC	(5) TSP	(6) TSP	(7) SGLI	(8) SGLI
Unit participation rate	-0.909	-0.895	0.395	0.404	27.785	26.102	-71.713	-72.267
	(0.864)	(0.868)	(0.636)	(0.628)	(20.226)	(20.151)	(42.929)	(43.819)
Observations	81,666	81,666	81,927	81,927	81,666	81,666	81,666	81,666
Adjusted R-squared	0.373	0.374	0.241	0.244	0.179	0.185	0.723	0.725
Job x rank x post x month-year FE	yes	yes	yes	yes	yes	yes	yes	yes
Demographics	no	yes	no	yes	no	yes	no	yes
Unit participation rate std. dev.	0.184	0.184	0.233	0.233	0.104	0.104	0.148	0.148
Sample mean	1.462	1.462	3.787	3.787	40.40	40.40	304.9	304.9

Dependent variable is the monthly dollar amount of soldier's participation in the program (specified in column heading) twelve months after arriving at new unit, except for columns (3) and (4). In those, dependent variable is the monthly dollar amount given to the CFC in the January following the soldier's first CFC campaign after arriving at the unit. Unit participation rate is the new unit's average participation in the specified program in the month before the soldier arrives, except (3) and (4) which use the unit's participation in the January preceding the soldier's arrival. Indicators for interactions between job, rank, post, and month-year included in all specifications. Soldiers' demographics included in even numbered columns. Standard errors clustered by post. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Impacts of Officers and Junior Enlisted Members on Soldiers' Behaviors

	(1) AER	(2) CFC	(3) TSP	(4) SGLI
Junior enlisted participation rate	0.117***	0.132**	0.019	-0.03
	(0.042)	(0.051)	(0.050)	(0.028)
Officer participation rate	-0.005	-0.007	-0.01	-0.004
	(0.034)	(0.029)	(0.014)	(0.007)
MOS x grade x post x month-year FE	yes	yes	yes	yes
Demographics	yes	yes	yes	yes
Sample mean	0.238	0.362	0.235	0.84

Dependent variable is whether soldier participated in program (specified in column heading) twelve months after arriving at new unit, except for column (2) in which the dependent variable is an indicator for participation in the CFC in the January following the soldier's first CFC campaign after arriving at the unit. Separate variables for junior enlisted program participation and officer participation included. These measures are the new unit's average participation in the specified program in the month before the soldier arrives, except for column (2) which uses participation in the January preceding the soldier's arrival. Indicators for interactions between job, rank, post, and month-year as well as soldiers' demographics are included in all specifications. Standard errors clustered by post. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Impact of All Treatments Together on Soldiers' Decisions

	(1) AER	(2) CFC	(3) TSP	(4) SGLI
AER participation rate	0.124**	0.124**	0.009	0.003
	(0.053)	(0.049)	(0.025)	(0.003)
CFC participation rate	0.080*	0.112***	-0.008	-0.004
	(0.043)	(0.040)	(0.012)	(0.004)
TSP participation rate	-0.118	-0.177*	0.038	0.009
	(0.116)	(0.102)	(0.088)	(0.009)
SGLI participation rate	-0.101	-0.212	0.215	-0.017
	(0.145)	(0.409)	(0.171)	(0.024)
Observations	79,829	79,829	79,829	79,829
Adjusted R-squared	0.137	0.204	0.190	0.961
MOS x grade x post x month-year FE	yes	yes	yes	yes
Demographics	yes	yes	yes	yes

Dependent variable is whether soldier participated in program (specified in column heading) twelve months after arriving at new unit, except for columns (2). In (2), dependent variable is indicator for participation in the CFC in the January following the soldier's first CFC campaign after arriving at the unit. Each participation rate is the new unit's average participation in the specified program in the month before the soldier arrives, except for CFC which uses the unit's participation in the January preceding the soldier's arrival. Indicators for interactions between job, rank, post, and month-year included in all specifications. Standard errors clustered by post. *** p<0.01, ** p<0.05, * p<0.1

Appendix A: Including Female Soldiers in Analysis

In this appendix, we relax our sample inclusion criteria. Specifically, we include female soldiers in our sample and rerun the balance tests, placebo tests, and primary analyses. Women comprise a small fraction of the junior enlisted in traditional combat troops—only 7% of the sample is female.

The balance tests for the sample with both genders are presented in Appendix Table A1. The first columns examine the AER participation rate at the units the soldiers will be transferred to. Column (2) shows that women are significantly less likely to go to a unit with high AER participation rates. Columns (4) and (6) show that women are systematically less likely to be transferred to units with high CFC participation rates or to units with low savings rates. Taken together, these results suggest that the conditional randomization of soldiers to units might not be entirely independent of a soldier's gender, though the differences in the treatments are small in magnitude.

Although there might be some question about the validity of the conditional randomization across genders, the placebo tests presented in Appendix Table A2 ameliorate these concerns. For none of our four treatment outcomes is the future treatment associated with the soldier's past behavior. In addition, the point estimates tend to be very small in magnitude. This suggests that even if the conditional randomization is imperfect across genders, future treatments are not correlated with fixed, soldier-specific variables that drive her participation decisions. This in turn suggests that any bias due to imperfect conditional randomization will be small.

Lastly, we present the primary results from the main text for the sample of both male and female soldiers in Appendix Table A3. The estimated effects are all very similar to those found for the male only sample in Table 4. This is not surprising for two reasons. First, as argued above, any bias that results from imperfect randomization is likely to be quite small. Second, only 7% of the sample is female. As such, the estimated impact for that subgroup (whether due to heterogeneous treatment effects, bias, or other reasons) would have to be extremely large to materially affect the estimated impacts for the full sample of males and females.

Appendix Table A1: Balance Tests for Sample that Includes Male and Female Soldiers

	(1) AER	(2) AER	(3) CFC	(4) CFC	(5) TSP	(6) TSP	(7) SGLI	(8) SGLI
Female		-0.0178**		-0.0232**		0.0107***		-1.37e-07
		(0.00792)		(0.00964)		(0.00235)		(0.000376)
White		0.00114		7.99e-05		0.000982		5.88e-05
		(0.00184)		(0.00172)		(0.000665)		(0.000108)
High school degree		0.000780		-0.00293		0.000638		0.000116
		(0.00200)		(0.00336)		(0.000622)		(0.000212)
College degree		-0.000376		0.00321		0.00222		-0.000489
		(0.00295)		(0.00605)		(0.00171)		(0.000506)
Age		-6.18e-05		-0.000306		-0.000830*		-5.68e-05
		(0.00141)		(0.00209)		(0.000460)		(0.000112)
Age-squared		-1.07e-06		2.00e-06		1.43e-05*		1.32e-06
		(2.61e-05)		(3.55e-05)		(8.25e-06)		(2.43e-06)
AFQT score		-7.89e-05		-2.93e-05		3.64e-06		1.14e-06
		(4.68e-05)		(5.82e-05)		(1.75e-05)		(2.50e-06)
Married		0.00124		0.000501		-0.000108		-9.89e-05
		(0.000955)		(0.00209)		(0.000812)		(0.000140)
Observations	87,753	87,753	87,992	87,992	87,753	87,753	87,753	87,753
R-squared	0.751	0.751	0.756	0.756	0.911	0.911	0.998	0.998
MOS x grade x post x month-year FE	yes	yes	yes	yes	yes	yes	yes	yes
Demographics	no	yes	no	yes	no	yes	no	yes
F-stat	0	0.016	0	0.013	0	0.002	0	0.845
Sample mean	0.206	0.206	0.403	0.403	0.187	0.187	0.971	0.971

Dependent variable is participation rate of unit the soldier will be transferred to (program given in column heading). Indicators for interactions between job, rank, post, and month-year included in all specifications. Soldiers' demographics included in even numbered columns. P-value of F-statistic for joint significance of demographics reported. Standard errors clustered by post.
*** p<0.01, ** p<0.05, * p<0.1

Appendix Table A2: Placebo Tests for Sample with Male and Female Soldiers

VARIABLES	(1) AER	(2) AER	(3) CFC	(4) CFC	(5) TSP	(6) TSP	(7) TSP	(8) TSP
Unit participation rate	-0.025	-0.024	0.003	0.004	0.023	0.021	-0.008	-0.008
	(0.016)	(0.016)	(0.008)	(0.008)	(0.075)	(0.075)	(0.022)	(0.021)
Observations	86,287	86,287	86,526	86,526	86,287	86,287	86,287	86,287
Adjusted R-squared	0.396	0.397	0.355	0.357	0.257	0.260	0.416	0.416
MOS x grade x post x month-year FE	yes	yes	yes	yes	yes	yes	yes	yes
Demographics	no	yes	no	yes	no	yes	no	yes
Unit participation rate std. dev.	0.181	0.181	0.231	0.231	0.103	0.103	0.0765	0.0765
Sample mean	0.105	0.105	0.116	0.116	0.176	0.176	0.988	0.988

Dependent variable is whether soldier participated in program (specified in column heading) in the month before arriving at new unit. Unit participation rate is the new unit's average participation in the specified program in the month before the soldier arrives. Indicators for interactions between job, rank, post, and month-year included in all specifications. Soldiers' demographics included in even numbered columns. Standard errors clustered by post. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Appendix Table A3: Main Results for Sample with Male and Female Soldiers

VARIABLES	(1) AER	(2) AER	(3) CFC	(4) CFC	(5) TSP	(6) TSP	(7) SGLI	(8) SGLI
Unit participation rate	0.131**	0.133**	0.125**	0.127**	0.065	0.059	-0.023	-0.023
	(0.057)	(0.057)	(0.052)	(0.052)	(0.082)	(0.080)	(0.026)	(0.027)
Observations	87,753	87,753	87,992	87,992	87,753	87,753	87,753	87,753
Adjusted R-squared	0.133	0.134	0.195	0.197	0.188	0.192	0.960	0.960
MOS x grade x post x month-year FE	yes	Yes	yes	yes	yes	yes	yes	yes
Demographics	no	Yes	no	yes	no	yes	no	yes
Sample mean	0.234	0.234	0.358	0.358	0.232	0.232	0.839	0.839
Unit participation rate s.d.	0.182	0.182	0.231	0.231	0.103	0.103	0.147	0.147

Dependent variable is whether soldier participated in program (specified in column heading) twelve months after arriving at new unit, except for columns (3) and (4). In those, dependent variable is indicator for participation in the CFC in the January following the soldier's first CFC campaign after arriving at the unit. Unit participation rate is the new unit's average participation in the specified program in the month before the soldier arrives, except (3) and (4) which use the unit's participation in the January preceding the soldier's arrival. Indicators for interactions between job, rank, post, and month-year included in all specifications. Soldiers' demographics included in even numbered columns. Standard errors clustered by post. *** $p<0.01$, ** $p<0.05$, * $p<0.1$