1. Introduction

One of the major demographic shifts in the U.S. over the last half century has been the decline in marriage rates (Ruggles 2015; Goldin 2021; Kearney 2023). Adults without a bachelor's degree have seen marriage rates at ages 40 to 45 — the later years of fertility — decline by 25 to 30 percentage points since 1970 with steady declines in each of the last five decades (Figure 1). Those with bachelor's degree or higher have also experienced a decline in marriage rates by mid-life. However, after declining by about 10 percentage points since 1970, marriage rates for this group stabilized around 1990. The decline in marriage has deservedly garnered a large amount of attention among researchers, policymakers, and the broader public. Marriage is an important social institution, and it is linked to a range of benefits for individuals including improved health, higher personal earnings and greater household resources, as well as to important macroeconomic outcomes (Moynihan 1965; Becker 1993; Ginther and Zavodny 2001; Robles and Kiecolt-Glaser 2003; Antonovics and Town 2004; Kiecolt-Glaser and Wilson 2017; Borella, De Nardi, and Yang 2018; Altonji, Hynsjo, and Vidangos 2021; Carpenter et al. 2021; Calvo 2022; Massenkoff and Rose 2022).

Marriage has also been linked to important intergenerational effects. Being raised in a single parent household is correlated with less upward mobility. In communities with a greater proportion of single-parent households, even children of married parents have lower levels of upward mobility (Chetty et al. 2014). The impact of community marriage rates on subsequent economic mobility may work in part through providing peers and role models who encourage or discourage marriage. Chetty et al. (2014) find that family structure, as measured by the fraction of single-parent households in a commuting zone, is more strongly correlated with upward mobility than residential segregation, income inequality, primary school quality, and social capital (i.e., social networks and community involvement). Although not causally estimated, the weight of existing evidence suggests that family instability has a negative impact on children (McLanahan and Sawhill 2015; Lundberg, Pollak, and Stearns 2016).

Disparities in the extent of the marriage decline across education and income groups are also concerning (Watson and McLanahan 2011). Family structure has been linked to income inequality and poverty for individuals and their children (Thomas and Sawhill 2002; McLanahan and Percheski 2008; Kearney 2022). Poverty rates in the U.S. vary substantially with family structure. Among U.S. families in 2021, the poverty rate was 9.5 percent overall. Married-couple households had a poverty rate of 5.2 percent, while single-parent households had a poverty rate of 25.3 percent (female head) and 12.7 percent (male head). The disparities were even more stark for families with children under age 6. The 2021 poverty rates for these sub-groups were: 6.8 percent (married-couple), 44 percent (single-parent, female head), and 19.7 percent (single-parent, male head) (Creamer et al. 2022).

Identifying the reasons for declining marriage rates is critical to understanding the associated welfare impacts. A number of explanations have been proposed, including increased labor market opportunities and participation for women (Goldin 2006; Lundberg and Pollak 2007; Jensen 2012), a decrease in manufacturing employment (Gould 2021), a decline in the population of marriageable men (Wilson 1987; Craigie, Myers, and Darity 2018; Autor, Dorn, and Hanson 2021; Shenhav 2021), availability of contraceptives (Goldin and Katz 2002), abortion access (Miller, Wherry, and Foster 2023), changing norms and sexual values (Akerlof, Yellen, and Katz 1996; Bertrand, Kamenica, and Pan 2015; Kearney and Wilson 2018), changes in the availability of divorce (Gruber 2004; Stevenson and Wolfers 2007; Cunningham and Goodman-Bacon 2024), and government transfer payments.[1]

In this paper, our focus is on the importance of peers and role models in individual marriage decisions. Understanding the contribution of peers in marriage decisions is important because a key role for peers would mean marriage trends are self-reinforcing over time, which in turn affects the scope for policy. However, it is difficult to establish causality around peers and marriage formation because when individuals choose their peer group – by forming friendships or remaining in a neighborhood, for example – they may choose peers who are already like them in important ways. This endogenous relationship means that married individuals may have peers who are more likely to be married without either causally influencing the other.

We use a setting with exogenous peer group assignment to study the contribution of marriage among peers to individual marriage decisions. Our study population consists of all enlisted soldiers in the U.S. Army (hereafter, Army) who arrived at their first duty location from October 2001 through January 2018. We first show that assignment to peer groups for our sample of junior enlisted soldiers is as-good-as random once we condition on observable factors that affect the assignment decision. This conditionally random assignment eliminates the potential for selection into peer groups, a major hurdle to credibly estimating peer effects in many contexts.

Identifying an individual's reference group is a non-trivial task in most contexts (De Giorgi, Frederiksen, and Pistaferri 2020), requiring detailed data and institutional knowledge. In our study peer groups consist of all the individuals in an Army company during the month a soldier arrives at a particular location. A company is the main work group in the Army, normally consisting of just over 100 soldiers, and every location in our sample has many companies. We leverage the Army's rank structure to identify four distinct sub-groups of peers within each company, allowing us to analyze how the effect of peers varies with the proximity and nature of daily interactions.

[1] See Murray (1984, 1993), Moffitt (1992), and National Research Council (1998) for a discussion of the incentive effects of welfare, and in particular the Aid to Families with Dependent Children (AFDC) and Temporary Assistance to Needy Families (TANF) programs. Another strand of literature has focused on the incentive effects of health insurance: Yelowitz (1998), Decker (2000), Sohn (2015), Abramowitz (2016), and Barkowski and McLaughlin (2022).

Since we observe the universe of enlisted soldiers on a monthly basis, we can measure peer group characteristics prior to an individual's group assignment. For each soldier that arrives at their first duty station, we define the treatment to be the fraction of company-location peers that are married in the month prior to the soldier's arrival. As a result, peer group marriage rates are independent of any influence the assigned individual has on his or her peers after arrival. This addresses concerns about the reflection problem, a second major source of bias in peer effect estimation. We then estimate how marriage decisions are affected by assignment to peer groups with different pre-arrival marriage rates.

Our setting also allows us to address two final confounds in peer effects estimation: common shocks that affect the behavior of the peer group and the focal individual, as well as collinearity of peer outcomes and peer characteristics. We construct an instrument for our treatment (peer marriage rates) using peers' marriage status at their previous assignment location. This allows us to control for potential unobservable environmental factors (e.g. upcoming deployment, commander influence, and time-varying local effects) which could affect both peer marriage rates as well as individual marriage rates at a common location and time. Since the instrument is constructed using a soldier's marital status prior to arrival at the current location (in most cases 9-12 months before arrival) it is not influenced by the environment or institutional features of the current location upon arrival. The instrument also separates peer marriage rates (our peer outcome of interest) from a set of time-invariant peer characteristics (such as religion), allowing for separate identification of the impact of each on an individual's marriage outcome. Exploiting these features of our setting allows us to interpret our estimates as the causal effect of an individual's peer group on his or her subsequent marriage decisions.

Despite the Army context, our empirical setting has several features that make it of broad interest. First, ninety four percent of the sample on which we estimate peer effects has a high school degree or less – the group that has experienced the sharpest decline in marriage rates in the U.S. population. Second, we observe individuals at a point in time when many are making marriage decisions, so if there is a role for peers in shaping marriage decisions, we would expect to observe such patterns in our data. Our primary estimates come from the sample of individuals who are unmarried upon arrival at their first duty location. Among these individuals, one-in-five is married within 24 months of arrival. Finally, Black and Hispanic men are well-represented in our sample, allowing us to precisely estimate separate impacts for these groups.

We find that a 6.5 percentage point increase in peers' marriage rates — an increase equivalent to the interquartile range in the identifying variation we exploit in the share of married peers — increases the likelihood that an unmarried individual is married 24 months after arrival at a new location by 0.38 percentage points, or 1.9 percent ($p < 0.05$). Using the 95 percent confidence interval, we can rule out effects larger than 3.8 percent and smaller than 0.03 percent. The dynamics of the estimates indicate that the effect of peers takes time to develop but then accrues over time. We find evidence that the effect of peers is larger and more persistent for Black soldiers in our sample. For white soldiers, the effect of peers reaches a peak near 24 months after assignment and then decreases, but the effect of peers is still positive (although not statistically significant) 36 months after assignment. For Black soldiers, the effect of peers continues to grow over time: a 6.5 percentage point increase in the fraction of a Black soldier's initial peer group who are married increases the likelihood of marriage at 36 months by 1.6 percentage points, or 4.5 percent ($p < 0.01$). The persistence of peer effects out to 36 months, especially for Black soldiers, provides some evidence that our estimates are not solely the result of re-timed marriage. Aside from race, we find little evidence that the effect of peers varies with other individual characteristics or with location characteristics such as the size of the "marriageable" population, suggesting that peers influence marriage decisions in a variety of contexts.

With respect to more granular measures of peers, our results indicate that the effect of peers is driven by fellow junior enlisted soldiers, first-line supervisors, and second-line supervisors. Individuals in the peer group who are more senior, and therefore interact less frequently with junior enlisted soldiers, have no distinguishable effect on individual marriage decisions in our data. However, peer effects estimated using the broadest group definition produce larger overall impacts than those estimated using the more granular peer group measures, suggesting that non-linear complimentarities in marriage rates throughout the peer group may be important for our results.

We consider two types of mechanisms for the impact of peers on marriage behavior – conformist behavior and spillover effects. Conformist behavior refers to choosing one's own behavior to match that of peers. Spillover effects refer to a channel in which a more prevalent peer behavior makes it easier for an individual to engage in the same behavior. In our context, peer spillovers may work through married peers providing access to information on the benefits of marriage or to social connections to marriageable or marriage-inclined individuals. Our findings are most consistent with conformist behavior. We support this hypothesis by showing that (1) total peer effects have larger results than either close peers or supervisors, even though the latter are not part of a soldier's social network; (2) the size of the peer group does not significantly affect the impact of peers; and (3) our largest effects are for those racial and gender groups where we would expect smaller network sizes. All together, this set of results suggests that our results are not driven by either increases in information about marriage benefits or access to more potential spouses, which would be more consistent with a spillover effect.

Finally, we benchmark our estimates against previous findings on the determinants of young adult marriage. Using back of the envelope calculations, our peer effects can explain between five and ten percent of the effect of Army service on short-run marriage estimated in Greenberg et al. (2022). We argue that the effect of peers, while relatively small, is economically meaningful.

We contribute to the current literature in three ways. First, ours is the first paper to credibly identify the causal effect of peers on marriage, as we discuss in detail in the next section. Second, we leverage rich longitudinal data to identify changes in marriage decisions at the monthly level for at least two years following assignment to a peer group. This longitudinal analysis enables us to better understand how the effect of peers evolves over time. Finally, we identify important heterogeneity in the impact of peers. We combine these findings with further tests to assess potential mechanisms through which peers operate.

In Section 2 we summarize the extensive previous work pertinent to our study; Section 3 provides background information and context on Army assignment decisions and peer groups; Section 4 describes the data; Section 5 covers the empirical strategy; Section 6 presents our main results and examines their heterogeneity and robustness; Section 7 discusses mechanisms and the magnitude of our estimates; and Section 8 concludes.

2. Relevant Literature

Social interactions play an important role in individual decision-making across many domains of life. The idea that an individual's family, friends, co-workers, or more generally "peers" influence marriage and fertility has existed for some time (e.g., Hernes 1972) and has been explored in sociology (Sprecher and Felmlee 1992; Felmlee 2001). However, as in other contexts, it has been difficult to identify the causal effect of peers on marriage decisions due to selection into peer groups, simultaneity between individual and peer choices, and the effects of common environment or "common shocks."[2,3]

Several papers in the literature on neighborhood or place-based effects provide suggestive evidence that peer effects may be important in marriage decisions. The first set of evidence comes from the Moving to Opportunity (MTO) for Fair Housing Demonstration Program run by the U.S. Department of Housing and Urban Development (HUD) in the mid-1990s.[4] Chetty, Hendren, and

[2]These difficulties were outlined in Manski (1993). Manski's model has subsequently been developed in Sacerdote (2001), Zimmerman (2003), Lyle (2007), Bramoullé, Djebbari, and Fortin (2009), Goldsmith-Pinkham and Imbens (2013), and Angrist (2014), among others. Athey and Imbens (2017) provide a review of portions of this literature.

[3]A robust literature has studied the effect of peers on fertility decisions; the vast majority of studies find that peer fertility is positively related to own-fertility. Examples of such studies include: Bernardi (2003), Kuziemko (2006), Asphjell, Hensvik, and Nilsson (2014), Balbo and Barban (2014), Pink, Leopold, and Engelhardt (2014), Ciliberto et al. (2016), Fletcher and Yakusheva (2016), Mishra and Parasnis (2017), and Buyukkececi et al. (2020). Recent studies find evidence for peer effects in other common decisions among adults, including cell phone purchases and alcohol consumption (Bailey et al. 2022; Hinnosaar and Liu 2022).

[4]The MTO experiment took place from 1994-1998 in five cities and randomized families living in high poverty census tracts into one of three treatments: (1) a housing voucher conditional on moving to a census tract with a poverty rate below 10 percent, (2) a Section 8 housing voucher without any other conditions, or (3) a control group that retained access to public housing but did not receive a housing voucher. There is consistent evidence across studies that MTO induced treated families to move to neighborhoods with lower rates of poverty (Kling, Liebman, and Katz 2007; Ludwig et al. 2013).

Katz (2016) estimated the long-run effects of the MTO experiment on a range of outcomes, including marriage and fertility. The authors estimate that children in families who were offered housing vouchers were more likely to be married 14 to 18 years after treatment assignment, and the effect was almost twice as large for women, relative to men.[5]

The MTO population was disadvantaged along a number of dimensions (e.g., 91 percent were single-parent households [Katz, Kling, and Liebman 2001]), raising concerns that the findings from MTO may not generalize to other contexts. However, more recent work has also found evidence of neighborhood effects on marriage in other populations. Chetty and Hendren (2018a) find that childhood exposure to areas where more peers are predicted to be married increases own-marriage at age 26.[6] Chetty and Hendren (2018b) extends this work to consider differences in marriage probability by commuting zones (CZs) and counties. Among the 100 largest counties in the U.S., the authors find that the probability of marriage by age 26 increases by up to 0.4 percentage points per year in Salt Lake, UT and decreases by up to 0.5 percentage points per year in Nassau, NY relative to growing up in the average U.S. county.[7] In their study of the fracking boom, Kearney and Wilson (2018) show that the fracking boom in the 2000s and the coal boom and bust from the 1970s and 1980s had different impacts on marriage rates and childbearing within marriage despite providing similar economic shocks. They hypothesize that the difference in responses is related to differing social norms over time. While these findings do not isolate the effect of peers from other place or neighborhood effects, they are consistent with peers exerting influence on marriage decisions.

An extensive structural literature has worked to develop and estimate marriage market matching functions, and several papers explore the role of peer behavior in setting market-wide matching patterns (e.g., Brock and Durlauf 2001). Drewianka (2003) estimates a matching model and finds that marriage rates depend on the marriage rates of others in the same marriage market using county-level U.S. Census data. In a similar vein, recent work by Mourifié and Siow (2021) finds that peer effects are quantitatively important in explaining aggregate trends in U.S. marriage rates. Bronson and Mazzocco (2024) find that there is a negative relationship between cohort size and marriage rates for both men and women and that this relationship could be related to changes in match quality or the value of being single. They hypothesize that the value of being single could be influenced by peer effects. Billari et al. (2007) develop a model for the importance of peers, specifically social pressure, in marriage outcomes and use a simulation to show that their model can replicate patterns found in empirical data. Using data from the National Longitudinal Study of

[5]Marriage and fertility outcomes were measured from 2008 to 2012 for children over the age of 24 who were less than 13 at the time of assignment. Children in families who were offered housing vouchers conditional on moving to a low-poverty neighborhood were 1.9 p.p. (57 percent) more likely to be married. Children in families who were offered Section 8 housing vouchers were 2.8 p.p. (84 percent) more likely to be married.

[6]They find an average annual exposure effect of 2.5 percent, meaning that the outcomes for movers converge to the outcomes of incumbents at an average rate of 2.5 percent per year.

[7]These estimates are calculated at the 25th percentile of the income distribution.

Adolescent to Adult Health (Add Health), Adamopoulou (2012) found that peers exert a positive influence on own-marriage probability. In a similar vein, McDermott, Fowler, and Christakis (2013) found that divorce tends to spread through social networks. This literature suggests that peer effects could be important for marriage, but there is a lack of well-identified micro-level studies of marriage decisions to isolate the effect of peers on marriage.

Related to our paper, Greenberg et al. (2022) study the effect of Army service on a variety of outcomes, including marriage. Leveraging sharp cutoffs in enlistment eligibility based on Armed Forces Qualification Test (AFQT) scores, they find that Army enlistment increases the probability of marriage by an average of 7 percentage points 5 to 19 years after applying for service. They also find significant differences by race. Both Black and white enlistees marry at higher rates in the short-run relative to those who do not enlist, but Army service only has a significant effect on marriage in the long-run for Black service members. Five to 19 years after application, service in the Army increases the probability of marriage for Black applicants by an average of 15 percentage points.

3. Background on Peer Groups in the Army

In a given year during our sample period, approximately five hundred thousand service members were on active duty in the Army. Army service members fall into one of three categories: officers, warrant officers, or enlisted members. Enlisted members, the focus of our study, are by far the largest contingent: in 2019, for example, 81 percent of Army service members were enlisted (CNA 2019). From Fiscal Years 2001 to 2019, more than 1.1 million individuals enlisted in the Army: between 54 and 71 thousand each year. The majority of individuals who join the Army signed three to six year enlistment contracts.[8]

Prior to arriving at their first duty location, all enlistees must go through initial entry training, which generally takes place in two phases. In the first phase, enlistees complete Basic Combat Training (BCT), or "boot camp," for six to ten weeks. During boot camp, enlistees receive training on basic military skills and are introduced to the culture and values of the Army. The training received during boot camp is very general and is meant to establish a baseline level of knowledge for all enlisted soldiers, regardless of occupation. Following boot camp, enlistees complete Advanced Individual Training (AIT) where they develop skills tied directly to their military occupation. The duration of AIT varies by occupation, from a few weeks to just over a year.[9]

Due to the relatively short duration of initial entry training, the Army does not relocate spouses to join soldiers on these assignments. Furthermore, soldiers have limited privileges during their

[8]Only about two-thirds of individuals who start basic training complete their first term of enlistment. Soldiers attrit for a variety of reasons, including: failure to meet physical standards, inability to adjust to military life, or injury.

[9]For some jobs the training in BCT and AIT is combined into a single course, known as One Station Unit Training (OSUT). However, the BCT/AIT pipeline described is the most common.

initial training assignment, so there is little opportunity for social interactions, to include meeting potential spouses or interacting with the spouses of peers.[10]

The Army's Human Resources Command (HRC) is responsible for forecasting unit shortages by brigade (approx. 5,000 soldiers) and assigning soldiers to fill openings based on a soldier's occupation and projected date of arrival. After completing initial entry training, enlisted soldiers are assigned to an Army brigade at one of many Army posts within and outside the continental United States.[11] The assignment of a soldier to an Army unit immediately following completion of initial training is considered the soldier's first unit of assignment in the military. We focus our analysis on this initial assignment throughout the paper because soldiers have scarcely any ability to influence the assignment decision and the Army has yet to learn any information on which to condition the assignment decision other than the variables that we observe in our data.

Once a soldier arrives at a location, they initially report to their assigned brigade. In recent work, Bruhn et al. (2024) demonstrate that the assignment of soldiers to brigades within a duty location are as-good-as random conditional on occupation, location, contract length, and timing of arrival. After arriving to a brigade, soldiers are progressively assigned to a battalion (approx. 700 soldiers) and company (approx. 100 soldiers). At each level, assignments are primarily made based on occupation, rank, and current staffing needs, and soldiers have little ability to exert influence over the assignment process and thus their eventual peer group. As we outline in more detail below, we will use this assignment process to tease out the causal impacts of peer marriage rates on marriage decisions using a similar strategy to Lieber and Skimmyhorn (2018).

We use the Army company as our primary peer group definition as it is the smallest administrative unit in the Army which can be identified in the data. Army companies consist of approximately 40 to 200 soldiers, depending on the unit type. The company is the level at which daily work takes place, and also determines when soldiers spend time in the field or deployed. Upon arrival at a new post, junior soldiers who are unmarried are assigned a barracks room and provided with a meal card to eat at the local dining facility. The barracks are generally organized such that soldiers in the same company live in close proximity to each other. Thus, a junior soldier lives, eats, and works with other junior soldiers in the same company on a daily basis. Soldiers who are married or who are more senior may choose to live on or off-post, and are not required to eat meals in the dining facility.

[10]We observe some marriages in the data between entry and arrival at a soldier's first assignment (a 5 p.p. increase from 14.6 percent to 19.6 percent). Given the institutional environment already described and the fact that a large majority of these marriages (79 percent) occur in the 60 days leading up to arrival to the first unit of assignment, we conclude that these marriages most likely represent the conversion of pre-existing relationships into marriages rather than the formation of new relationships formed during initial training (Carter and Wozniak 2023).

[11]In addition to the factors described above, soldiers are assigned in accordance with guidance from Army senior leaders and Army Regulation 614-200, *Enlisted Assignments and Utilization Management*. For clarity, we describe the assignment process that was in place during the bulk of our sample period, but prior to 2005, the relevant unit for assigning soldiers was the Army division, rather than the brigade (Bruhn et al. 2024).

Unmarried junior soldiers interact with their married peers at work or in social situations outside of work. However, junior soldiers are prohibited from socializing with more senior soldiers outside of work.[12] We explore more granular peer definitions within a company based on the Army's rank structure in Section 6.2.

4. Data

We use administrative military personnel data covering all Army active-duty enlisted service members from the Total Army Personnel Database (TAPDB) obtained through the Office of Economic and Manpower Analysis at West Point, NY. From October 2001 to September 2020, we observe a monthly snapshot for each individual that includes information on a soldier's unit and location of assignment, number of dependent adults and children, marital status, rank, occupation, and whether the soldier is deployed. We also observe a rich set of soldier characteristics at entry, including: entry date, AFQT percentile score, education level, race and ethnicity, sex, origin state, length of initial enlistment contract, and year of birth.[13]

Vital to our empirical strategy is the conditional random assignment (CRA) of individuals to peer groups. Starting with the full sample of soldiers who arrived at their first Army post from October 2001 to January 2018, we impose a number of sample restrictions to ensure that we are able to isolate situations where the assignment of soldiers to peer groups is conditionally random. This entails three broad limitations. First, we restrict our analysis to soldiers assigned to their first unit after initial entry training. This is the point in a soldier's Army career when the CRA assumption is most plausible. We further restrict our analysis sample to soldiers who are then assigned to sufficiently large units without extremely specialized responsibilities and — in most of the analysis — for whom we observe a continuous record for 24 months after the initial assignment. The details on these restrictions are laid out in Section A.1.

To conduct some of the heterogeneity analysis, we use quarterly measures of employment and earnings from the Quarterly Workforce Indicators (QWI 2020). The QWI is extremely rich in that it provides employment and earnings estimates at the county level and by race or age and sex. We also use population data from the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) program (NCI 2021). We use SEER data because it allows us to construct

[12] Soldiers are allowed to live outside the barracks regardless of marital status once they achieve the rank of Staff Sergeant, which generally requires at least six years of service. Junior enlisted soldiers are not allowed to fraternize with Non-Commissioned Officers (NCOs). An NCO is an enlisted soldier in the rank of Sergeant or above, and would generally have at least three years of military experience.

[13] Throughout we will use the term race to refer to race and ethnicity. We define race as non-Hispanic ethnicity and of White, Black, Asian, Native American/Native Alaskan, Pacific Islander, or Other (two or more races) race. Hispanic is defined as Hispanic ethnicity of any race.

population measures by single year of age groups, which is not generally possible using intercensal population estimates from the Census Bureau.

4.1. Summary Statistics

Summary statistics for the full estimation sample are in Table 1. Columns two and three break out the sample by marital status on arrival; columns four and five break out the sample by gender. Our empirical analysis focuses on the sample of soldiers who were unmarried upon arrival to their first assignment (column 2).

In our data, the vast majority of soldiers arrive to their first assignment location four to nine months after the start of basic training. The modal soldier in our sample is an unmarried high school-educated white male serving a three-year enlistment contract who arrives to his first assignment approximately six months after joining the Army. The majority of our sample (64 percent) serves in a direct combat occupation and 61 percent deploy in their first two years at a location. Six percent of soldiers are stationed at a location in their home state and 35 percent in a location in their home census region.[14]

In Panel C of Table 1, we report information on soldiers' marriage and fertility before and after arrival. Here marriage is an indicator variable equal to one if an individual is married and zero otherwise. Likewise, fertility is an indicator equal to one if an individual has dependent children and zero otherwise. Upon arrival to their first Army unit, nine percent of our sample is married and eight percent have children. Among soldiers who are unmarried when they arrive at their first location, 20 percent are married after two years (column two).

Soldiers in the Army can expect to move every two to three years. As a result, there is significant turnover within the peer groups we study. Arriving soldiers spend approximately 30 months in their assigned Army company (Table 1, Panel D). In Figure A1, Panels A and B, we plot the share of the sample that is still in their original assigned company and location, respectively, over time. At 24 months, 69 percent of individuals are still in their original company. Those individuals who are not in their original company have almost exclusively moved to another unit at the same location: 97 percent are still at their original assignment location at 24 months. In Panel C, we plot the share of an individual's original peers who are still in the company over time. Among the peers we use to construct our treatment and instrument, 25 percent remain in the peer group 24 months after the focal individual's arrival. While this is not surprising given our context, it is important context for interpreting the magnitude of our results.

[14]The high share of soldiers assigned to their home census region is explained by the non-random distribution of Army posts across the U.S. combined with the fact that Army enlisted members come disproportionately from census regions with more Army posts. In our sample 65 percent of soldiers are from the South and West census regions, and the same census regions account for 86 percent of assignments.

4.2. Measuring the Fraction of Married Peers

To construct measures of each arriving soldier's peer group, we start with the full monthly panel of all Army enlisted service members, a larger and less restrictive sample than our estimation sample. In each month we calculate the fraction of soldiers in each company that are married. To avoid the well-known simultaneity bias between peer group and individual outcomes, we lag this measure by one month and map it back to each soldier in the sample. Thus the primary treatment is the fraction of soldiers who were married in a company in the month *before* a soldier arrived. Formally, this fraction can be expressed as:

$$\bar{Y}_{g,t-1} = \frac{\text{Number of Soldiers Married}_{g,t-1}}{\text{Number of Soldiers}_{g,t-1}} \tag{1}$$

Information on the resulting peer marriage rates is shown in Table 1, Panel D. In our sample the average company has 114 enlisted soldiers and 48 percent of them are married.[15] There is substantial variation in the fraction of peers who are married, as shown in Panel A of Figure 2 for the sample of unmarried soldiers. The fraction of peers who are married ranges from just over 10 percent to almost 85 percent; the standard deviation is 11 percentage points and the interquartile range is 15 percentage points. In Figure A2, we show that peer marriage rates do not vary systematically across the distribution of peer group size.

Our estimation strategy, described in detail in the next section, uses variation in peer marriage rates within sex, job, rank, month of arrival, location, and initial term of enlistment cells. Given the continuous nature of the treatment, we interpret our results utilizing only the residual treatment variation that exists in the sample to prevent errors from extrapolation. The distribution of our identifying variation is in Panel A of Figure A3. The interquartile range in residual variation is 6.5 percentage points; we will use a change in peer marriage rates of this size to interpret the magnitude of our coefficients throughout the paper. Notably, the interquartile range in residual variation does not vary significantly across sub-samples.

As is common in the peer effects literature, we construct an instrument for our main treatment to address the concern that the treatment is related to peer group-specific institutional or environmental factors that are also related to our outcomes of interest. The instrument is constructed as follows. First, for each enlisted member at location l, we measure their marital status at their previous duty location, l', six months before arriving at their current location. Measuring marital status at the previous location ensures that marriage decisions have not been influenced by the environment or institutional factors at the current location. Furthermore, since the assignments of enlisted soldiers

[15] The fraction of individuals who are married in the peer group (Panel D) is higher than among the individuals for whom we estimate peer effects (Panel C) because the peer group includes individuals who are older and who have been in the Army for longer.

depend primarily on the needs of the Army and a soldier's occupation and rank, the environment at previous locations is conditionally unrelated to the environment at the current location. We then calculate the fraction of individuals in a soldier's peer group who were married at their previous location in the month before a soldier arrives to the unit (see Equation 1). We illustrate this process in Figure A4.

Panel B of Figure 2 shows the variation in the instrument. Consistent with the fact that we construct the instrument from chronologically earlier observations of peer group members, the rates of peer marriage are overall lower but still range from less than 10 percent to over 60 percent married. On average 31 percent of peers were married at their previous location. The standard deviation is 10.1 percentage points and the interquartile range is 14 percentage points.

5. Empirical Strategy

5.1. A Structural Model of Social Effects

We begin with the canonical model of social effects set forth by Manski (1993). We specify the outcome for individual i assigned to peer group g in month t, $Y_{ig,t}$, as a linear function of observed and unobserved individual and peer group characteristics:

$$Y_{ig,t} = \beta_0 + \beta_{\bar{Y}}\bar{Y}_{g-i,t} + \beta_{\bar{Z}}\bar{Z}_{g-i,t-1} + \beta_Z Z_{ig,t-1} + \omega_{g,t} + \upsilon_{i,t} + \epsilon_{ig,t} \tag{2}$$

As discussed above, the soldier's company at the time of his assignment constitutes the peer group in most of our analysis. $Y_{ig,t}$ is the outcome of interest for individual i assigned to company g in month t. Individuals may only be assigned to one company in each month. $\bar{Y}_{g-i,t}$ is the average outcome for individuals in company g in month t, excluding individual i; $\bar{Z}_{g-i,t-1}$ is a vector of average exogenous characteristics of individuals assigned to group g, excluding individual i; $Z_{ig,t-1}$ is a vector of immutable or exogenous characteristics of individual i; $\omega_{g,t}$ accounts for any group-specific, time-varying factors that affect the outcome such as shared environment or institutional features (i.e., common shocks); $\upsilon_{i,t}$ captures any other unobserved idiosyncratic time-varying factors that are related to the outcome; and $\epsilon_{ig,t}$ is the remaining error term.

Examination of Equation 2 highlights the difficulty in estimating the causal effect of peers on individual outcomes. First, there is the reflection problem identified by Manski. The reflection problem is a simultaneity bias arising from the fact that the outcomes for individual i and the peer group are measured at the same time. In this case, $\beta_{\bar{Y}}$ may be positive due to individuals i's influence on peers, not the effect of peers on individual i. We address simultaneity bias by measuring peer group outcomes prior to the arrival of individual i. Second, individuals will generally select into their peer groups, so $\upsilon_{i,t}$ will be correlated with $\bar{Y}_{g-i,t}$ and the outcome of interest. Conditional random assignment breaks the relationship between $\upsilon_{i,t}$ and $\bar{Y}_{g-i,t}$, thus correcting this source of omitted variable bias. Third, individuals who are exposed to the same environment or experience the same institutional changes may respond in similar ways. If this is the case, $\omega_{g,t}$ will be correlated with the outcome for individual i ($Y_{ig,t}$) and the average peer group outcome ($\bar{Y}_{g-i,t}$). To address this problem, we construct an instrument for the average peer group outcome using peer marriage outcomes measured prior to each peers' arrival at the current location. Using previous peer outcomes, combined with the conditional random assignment of soldiers to Army posts, means that the instrument is unrelated to $\omega_{g,t}$.

Finally, there is the added challenge of separately identifying effects of peer outcomes $\left(\bar{Y}_{g-i,t}\right)$ from those of peer characteristics $\left(\bar{Z}_{g-i,t-1}\right)$ because in a single peer group one will be a linear combination of the other (Bramoullé, Djebbari, and Fortin 2009; De Giorgi, Pelizzari, and Redaelli 2010). Our instrumental variables strategy addresses this challenge as well, as we discuss in more detail below.

5.2. Estimating Effects of Peers on Own Marriage

Following Lieber and Skimmyhorn (2018), we manipulate the structural equation to create a reduced form model that can be estimated with our data. The full derivation of the estimating equations from the structural model is in Section A.2. The model we estimate is:

$$Y_{ig,t+s} = \pi_0 + \pi_1 \bar{Y}_{g,t-1} + \pi_2 Z_{ig,t-1} + \theta_r + \zeta_{ig,t+s} \tag{3}$$

Where $Y_{ig,t+s}$ is the outcome for individual i assigned to peer group g in month t, s months after assignment. Our main outcome of interest is an indicator for whether an individual is married 24 months after assignment. We choose to evaluate peer effects at 24 months for two reasons. First, a large share of soldiers will still be in their original peer group at this point (Figure A1), so the share of peers who were married upon arrival is still a valid measure of the peer group to which an individual is exposed. Second, as all soldiers in our data sign initial enlistment contracts of at least three years, retention in the data for 24 months does not require a decision about whether to stay in the Army beyond the initial contract, in general.[16] However, we also estimate the effect of peers at various time horizons, and in some cases we include individuals who leave the Army prior to 24 months, in which case the outcome is based on an individual's last observation in the data.

The treatment, $\bar{Y}_{g,t-1}$, is the fraction of individuals who were married in group g in the month before individual i arrived. Our coefficient of interest is π_1, which captures the impact of the share of a soldier's married peers on his later marriage outcome. In this specification π_1 reflects the combined impact of peer behavior $\left(\beta_{\bar{Y}}\right)$ and peer characteristics $\left(\beta_{\bar{Z}}\right)$.

[16]This is the case for all but a small share of occupations in which the duration of initial military training is longer than one year. In Section A.3, we estimate the effect of peer marriage on attrition and re-enlistment.

The vector $Z_{ig,t-1}$ includes exogenous characteristics of individual i, consisting of indicators for race, a quadratic in age, indicators for education level, a quadratic in AFQT percentile score, and a cubic in the number of months a soldier's unit has spent deployed from t to $t+s$. In all specifications we include an interacted fixed effect, θ_r, that captures factors that may affect the assignment decision (sex, occupation, rank, assignment location, month-year of arrival, and initial term of enlistment). We argue below that conditional on θ_r, individuals are randomly assigned to peer groups. The remaining error term is $\zeta_{ig,t+s}$.

This is a standard linear-in-means (LIM) model of peer influence. Recent research has explored ways to relax the restrictions of the LIM model (Boucher et al. 2024). However, our prior given the context is that the mean marriage rate is a group parameter of primary interest. The mean marriage rate likely reflects the social norm around marriage, making a LIM model appropriate for our setting.

5.3. Conditional Random Assignment to Peer Groups

Conditional random assignment of individuals to peer groups is a key element of our identification strategy. As noted above, the Army assigns soldiers from initial training to their first operational unit based on the staffing requirements of Army units, and soldiers have virtually no ability to influence the assignment process. Factors that do influence the assignment decision are a soldier's sex, occupation, rank, unit requirements by brigade and location, and the timing of the assignment.[17] We also condition on the duration of an individual's initial enlistment contract. Contract length may be related to an individual's long-run desire to stay in the Army or pursue college and therefore could reasonably be expected to be related to marriage and fertility decisions. Additionally, we compare individuals who have the same marital status upon arrival at their first assignment. Conditional on these factors, the characteristics of the peer group to which a soldier is assigned should be unrelated to any observable or unobservable individual characteristics, and, more specifically, to an individual's underlying propensity for marriage.

To test the validity of this assumption, we estimate models of the form below, following Carter and Wozniak (2023):

$$\bar{Y}_{g,t-1} = \delta_0 + \delta_1 C_{ig,t-1} + \theta_r + \xi_{g,t-1} \tag{4}$$

Here $\bar{Y}_{g,t-1}$ is the treatment, defined as the fraction of individuals in peer group g in the month before individual i is assigned who where married. The vector $C_{ig,t-1}$, which is a subset of $Z_{ig,t-1}$, includes

[17] Occupation is defined in the Army by Military Occupational Specialty (MOS), of which there are more than 300 in the data. Sex was considered in the assignment process for much of the sample period because women were not allowed to serve in certain types of Army units. Even after conditioning on occupation, sex could still be a determining factor in the assignment process.

an individual's age and AFQT percentile score as well as indicators for race and education level. The randomization fixed effects, θ_r, are defined as before and $\xi_{g,t-1}$ is the remaining error term.

If $\delta_1 \neq 0$ then individual characteristics are predictive of peer marriage rates, and we would reject the assumption of conditional random assignment of individuals to peer groups. If $\delta_1 = 0$, then it is evidence in support of the assumption of conditional random assignment. The results of estimating Equation 4 on the sample of unmarried individuals are in Table 2, columns one to four. Column one includes only the interacted fixed effects. The fixed effects explain approximately three-fourths of the variation in the treatment. In columns two to four we progressively add individual-level controls. In each column we report the p-value of a joint test of the controls where the null hypothesis is that $\delta_1 = 0$. None of the coefficient estimates are statistically significant at conventional levels, and in all cases we fail to reject that the coefficients are jointly equal to zero. Furthermore, the magnitude of the point estimates is small and adding controls does not increase the R^2, supporting the conclusion that individual demographic characteristics do not predict variation in the treatment after we condition on the factors that affect the assignment decision. In column four we include all controls and again fail to reject that the coefficient estimates are jointly equal to zero ($p = 0.51$).

Next, we conduct a balance test in Table A1 to further explore how individual characteristics vary with changes in the share of peers who are married upon arrival (i.e., the treatment). To isolate our identifying variation, we first residualize the treatment by regressing the treatment on our randomization controls (θ_r). We then break our unmarried sample into quartiles based on the residual treatment and check for covariate balance across quartiles. The first four columns show the sample means for each covariate within the quartile indicated. Subsequent columns test for mean differences in each covariate across quartiles of residual treatment. To assess the differences across treatment quartiles, we report both the normalized difference, $\hat{\Delta}$, and the t-statistic. While there are some statistically significant differences, there is no clear pattern of correlation between the treatment and any individual characteristics. The significant differences that do exist are small in magnitude: the largest normalized difference between quartiles of residual treatment is 0.02.[18] These two tests provide strong evidence that assignment to peer groups is as-good-as random once we condition on factors that could affect the assignment decision.

5.4. An Instrumental Variables Approach

As explained in Section 5.1, we use an instrumental variables approach to address two issues. The first is to address the possibility that time-varying factors that influence the behavior of an individual and their peers, so-called common shocks, have the potential to bias peer effect estimates. Examples

[18]While the t-statistic is informative as to whether detectable mean differences exist between two sub-groups of data, the normalized difference is informative about the magnitude (in standard deviation units) of the difference. Linear regression is likely to be sensitive to specification choice if the normalized difference in covariates exceeds 0.25 (Imbens and Wooldridge 2009; Imbens and Rubin 2015).

of common shocks in our context are deployments (which we examine in detail in Section 6.3), unit commanders who turnover every one to two years, and local labor market shocks that might affect local marriage incentives (e.g., plant closures). In each case, the shock has the potential to affect both the marriage decisions of incumbent peers and the focal soldier, which could lead to biased estimates of peer effects. Estimating Equation 3 instrumenting for $\bar{Y}_{g,t-1}$ parallels the specification in Lieber and Skimmyhorn (2018) and others and allows us to interpret π_1 as the causal impact of peers endogenous and exogenous characteristics on own marriage.

Our instrumental variable approach also allows us to solve a second identification problem common to the peer effects setting. Separate identification of the impacts of peer characteristics (exogenous peer effects) and peer outcomes (endogenous peer effects) requires an instrument for the latter. As demonstrated in De Giorgi, Pelizzari, and Redaelli (2010), this is possible if we observe non-overlapping sets of peer groups. These allow for construction of an instrument for peers' outcomes in our group of interest that is not collinear with peer characteristics in the group.[19] In the language of the literature, we observe peers-of-peers, but we do so by observing past outcomes of current peers. These are related to outcomes for current peers, but since they vary over time, they are not collinear with fixed characteristics of current peers.[20] Hence our IV approach allows us to instrument for average peer outcomes while also controlling for average peer characteristics. Recall that our marriage rate for current peers is calculated in the period before the focal individual arrives, so it is free of reflection bias. Estimating this version of Equation 3 with controls for exogenous peer characteristics parallels the specifications in De Giorgi, Pelizzari, and Redaelli (2010) and De Giorgi, Frederiksen, and Pistaferri (2020) and allows us to interpret π_1 as the causal effect of peer marriage rates alone on own marriage outcomes.

We construct an instrument using the marital status of peers at their previous locations (described in Section 4.2). For this instrument to be a valid, it must satisfy the following conditions: (1) previous peer marriage rates only affect individual marriage decisions through current peer marriage rates (exclusion restriction); (2) the relationship between previous and current peer marriage rates is monotonic; and (3) previous peer marriage rates are predictive of current peer marriage rates (relevance). If these assumptions hold and assignment to peer groups is conditionally random, the 2SLS estimates represent a positive-weighted average of Local Average Treatment Effects (LATEs)

[19]Details of this identification are given in Bramoullé, Djebbari, and Fortin (2009) as well as De Giorgi, Pelizzari, and Redaelli (2010). Applications can be found in De Giorgi, Pelizzari, and Redaelli (2010), Nicoletti, Salvanes, and Tominey (2018), and De Giorgi, Frederiksen, and Pistaferri (2020).

[20]One of the key aspects of the instrument in the peers-of-peers literature is having non-overlapping peers. In our setting, almost 25 percent of soldiers arrive in a company alone in the peer-group month. For these soldiers they would have a unique peer group. However, there are some soldiers that arrive in companies at the same time as other soldiers, thus they share peer groups. Further, our main specification excludes exogenous peer characteristics, but we provide estimates that include them along with other checks as part of our robustness analysis.

even allowing for heterogeneous treatment effects (Imbens and Angrist 1994; Frandsen, Lefgren, and Leslie 2023; Chyn, Frandsen, and Leslie 2024).

We argue that violations of the exclusion restriction are unlikely in our context, although violations could arise if the focal soldier interacted with individuals in the peer group at initial training. While hypothetically possible, it is unlikely in our sample. Even with conservative estimates, at most 28 percent of junior enlisted peers (a subset of all peers) could have possibly overlapped in initial training with the focal soldier, and of that 28 percent it is likely most would have been in different training units as they arrived at the first duty location prior to the focal soldier.[21] Additionally, only four percent of senior peers were serving in initial training units at the same time as the focal soldiers were in training, and most were likely in different training areas than the focal soldier. On top of this, the institutional details of Army initial training (described in Section 3), and the fact that soldiers have limited interactions with other trainees or trainers outside their squad, platoon, or company, make such an interaction unlikely. Even if a soldier did interact with a future peer during initial training, spouses and families are not present during training, so it is unlikely that marriage would have been a salient feature of the interaction. Regarding monotonicity, we could be concerned that monotonicity would be violated if a greater share of peers being married reduces the marriage pool for the focal soldier. As we will discuss later, the number of intra-Army marriages are limited, especially for men, which reduces this concern.

As is standard, we will directly assess the relevance of the instrument using the first-stage estimate. For the reasons already mentioned, we estimate Equation 3 by Two-Stage Least Squares (2SLS), where the first stage is given by:

$$\bar{Y}_{g,t-1} = \gamma_0 + \gamma_1 \bar{Y}_{g,prev\ location,t-1} + \gamma_2 Z_{ig,t-1} + \theta_r + \eta_{ig,t-1} \tag{5}$$

Where $\bar{Y}_{g,prev\ location,t-1}$ is the share of individuals assigned to group g in $t-1$, who were married at their previous location of assignment and all other variables are defined as in Equation 3. Column four of Table 3 shows first-stage estimates of Equation 5. Our instrument is highly relevant with an F-statistic of 641.2 and follows the recommendations of Angrist and Kolesar (2024).

Having provided evidence that our instrument is valid, we next provide additional evidence that assignment to the instrument is conditionally random. We previously discussed conditional random assignment of the focal soldier to the peer group in Section 5.3. To further this argument, we now present evidence of conditional random assignment to the instrument as well in columns 5-8 of Table 2. The estimates in columns 5-8 are from a version of Equation 4 where the dependent variable is the instrument, rather than the treatment. The evidence is consistent with conditional random assignment; when we include all controls in column eight, we fail to reject that the coefficient

[21] We conservatively define someone as possibly overlapping with a focal soldier in initial training as someone who was in initial training within a year of when the focal soldier arrived at their first duty station.

estimates are jointly equal to zero ($p = 0.24$). In light of these results, our 2SLS estimates represent a weighted average of LATEs.

6. Results

6.1. Peer Effects on Marriage

We begin by estimating the effect of peers on individual marriage decisions in the 24 months after assignment to a peer group. Table 3 presents estimates of Equation 3 on the sample of individuals who were unmarried upon arrival. In columns one and two we report OLS estimates, with individual-specific controls added in column two beyond the randomization controls, θ_r, included in column one. We cluster standard errors at the peer group level, which is the level of treatment, to account for clusters of individuals receiving the same treatment (Abadie et al. 2023).[22] The coefficient of 0.063 in column one implies that a peer group where all peers are married (a treatment value of 1) would increase the likelihood an arriving soldier is married 24 months after arrival by 6.3 percentage points relative to a peer group with no married peers (a treatment value of 0). To better interpret the magnitude of our estimates, in the bottom of the table we report the effect of increasing the share of married peers by 6.5 percentage points, which is the interquartile range (IQR) in residual treatment (see Section 4.2). The coefficient of 0.063 in column one implies that a change of this magnitude increases the likelihood of marriage at 24 months by 0.41 percentage points ($p < .01$), or 2.08 percent. Adding controls in column two causes a relatively small decrease in the percentage effect, consistent with our assumptions about conditional random assignment. The coefficient of 0.054 in column two implies that a 6.5 p.p. increase in the share of married peers increases the likelihood of marriage at 24 months by 0.35 percentage points ($p < .01$), or 1.79 percent.

In columns three to six we instrument for current peer marriage rates as described in Section 5.4 to address the possibility of unobserved common shocks affecting our estimates. Column three shows that the reduced form effect of the fraction of peers married in their previous location on the likelihood of being married by 24 months is positive, statistically significant, and of a similar magnitude as our OLS estimates.

The 2SLS estimate in column six implies that a 6.5 p.p. increase in peer marriage increases the likelihood of marriage at 24 months by 1.9 percent ($p < .05$). Comparing the 2SLS point estimate in column six with the OLS estimate in column two shows that the OLS estimate has a downward bias — the 2SLS estimate is just over nine percent larger. We test for the endogeneity of treatment by conducting a Hausman test using the control function approach. The reported p-value in column six ($p = 0.843$) suggests that we cannot reject the null hypothesis that the treatment is exogenous.

[22]We demonstrate that our standard errors are minimally sensitive to other choices of clustering in Table A2.

However, given that the differences between the OLS and 2SLS estimates suggest a role for common shocks, we estimate the remaining specifications using our 2SLS strategy. This also allows us to estimate a specification controlling for exogenous peer characteristics.

We also estimate peer effects on marriage at 24 months for the sample of individuals who were married on arrival in Table A3. We find no evidence of peer effects for this sample, although there is admittedly little scope for peer effects as 97 percent of the sample is still married after 24 months. This result does suggest that marriage rates of peers is not leading to marriage dissolution (i.e., divorce).

We examine stability in our peer effect estimates by location and time period by estimating Equation 3 separately by location and year of arrival. To estimate heterogeneity by location, we limit the sample to locations that had at least 2,500 soldiers arrive over the entire sample period. In Panel A and B of Figure A5 we plot separate coefficient estimates for each location and year, respectively. We observe some heterogeneity along these dimensions, but the estimates are positive for nine of 12 posts and in nine of the 16 years, indicating that our Table 3 results are not driven by a particular location or time period.

To better understand how the effect of peers evolves over time, we estimate Equation 3 by 2SLS for each month following the month an individual is assigned to a peer group until 24 months after assignment. The results are presented in Figure 3 where the final estimate at 24 months is the same result from column six of Table 3. Estimates are small and statistically insignificant for the first five months after an individual is assigned to a peer group. This null result provides some additional assurance regarding our conditional random assignment assumption. If, instead, individuals were selecting into high-marriage peer groups with the intention of getting married, we would expect to see increased marriage rates emerge sooner. The estimates remain stable, small, and statistically insignificant through the first 12 months after arrival. Beginning in month 13, the positive effect of peers begins growing, becoming statistically significant after 17 months.[23]

6.2. Alternative Peer Group Definitions and the Role of Peer Group Characteristics

So far we have defined an individual's peer group as all other enlisted soldiers in the same Army company. However, the richness of our data coupled with the hierarchical rank structure of the Army allow us to consider alternative peer group definitions. The purpose of this analysis is to identify whether the marriage decisions of sub-groups of peers have a differential effect on own-marriage decisions, as well as whether there appear to be complementarities in the sub-group peer effects that combine into the overall effect we observe.

[23] In Section A.3, we show that our results are robust to including individuals who leave the Army before 24 months of assignment. We find no evidence that the fraction of married peers is related to attrition from the Army over this period.

The first sub-group we consider consists of individuals of a similar rank and experience level to the individuals for whom we estimate peer effects. Specifically, we define this peer group to consist of junior enlisted soldiers in the rank of Private (E-1 and E-2), Private First Class (E-3), and Specialist (E-4). These are the individuals with whom a soldier interacts on a daily basis, both inside and outside of work. We refer to this peer group as "Same Rank." Next, we consider the first-line supervisors for junior enlisted soldiers. First-line supervisors have three to five years of experience in the Army and work with junior enlisted soldiers daily. However, supervisors do not typically form social connections with junior enlisted soldiers outside of work, and, they are prohibited from maintaining a social relationship with a junior enlisted soldier. Second-line supervisors have six to twelve years of experience and interact with junior enlisted soldiers on a daily basis, but less frequently than first-line supervisors. Senior supervisors, who generally have more than twelve years of service, have infrequent interactions with junior enlisted soldiers but provide oversight and supervision of junior enlisted soldiers and their supervisors.[24]

Table 4 presents 2SLS estimates of π_1 in Equation 3 where the peer group is defined by the components of an Army company as described above. The point estimate in column two for close peers (Same Rank) is about a sixth the size of the peer effect for company marriage rates overall (column one), and is not statistically significant. The estimate of the effect of first-line supervisors in column three is almost twice as large as the Same Rank estimate, but is also not statistically significant. The estimate for second-line supervisors in column four is of a similar magnitude to those of the Same Rank (column two), while senior supervisors do not have a detectable effect on marriage decisions (column five). In Table 4 column six, we test for statistical differences in the effect of each defined peer group. While we cannot reject that the coefficients are equal at the five-percent level, the pattern of results is consistent with peers of the same rank, first-line, and second-line supervisors being the main driver of our results. Furthermore, defining the peer group as the entire company, as in our baseline specification, still results in the largest peer effect estimate. This suggests non-linearities in the influence exerted by peers across these sub-groups: individuals are influenced by individuals within their immediate social network as well as by supervisors with whom they interact at work but not socially, and the cumulative effect of higher marriage rates across these distinct peer groups may be larger than the sum of the contributions of each group alone.

Next, we confirm that the peer effects we estimate are, in fact, the result of the marriage decisions of peers, and not another peer characteristic that is correlated with marriage, as discussed in the peers-of-peers literature. For example, if having older peers encourages individuals to mature sooner, and if older peers are more likely to be married, then we might observe a positive effect of peer marriage rates on own-marriage even though peer marriage is not the driving factor. To

[24] In the data we define first-line supervisors to be Sergeants (grade E-5), second-line supervisors to be Staff Sergeants (grade E-6), and senior supervisors to be Sergeants First Class (grade E-7) or above.

consider this possibility, we estimate a variation of Equation 3 where we add in other peer group characteristics $\left(\bar{Z}_{g,t-1}\right)$.

The results of this exercise are reported in Table 5. In column one we report the baseline estimate from Table 3. In columns two to five we estimate regressions in which we include controls for peer group characteristics other than marriage rates: sex composition, racial composition, average age, average AFQT percentile score, and share of high school graduates. Across columns, the coefficient on the peer marriage rate remains relatively stable with the inclusion of these controls, although it is in some cases no longer statistically significant. In column six, we add controls for the share of peers that are Catholic, Protestant, Mormon, and of other religions as these characteristics could plausibly affect the marriage rates of peers and an individual's marriage choices if having religious peers is more important than having married peers. We find that the coefficient on our main estimate is of a similar magnitude to our baseline estimate in column one and marginally statistically significant. Finally, in column seven we include all the peer group controls jointly in a single regression. While no longer statistically significant, the coefficient on peer marriage rates indicates that a 6.5 p.p. increase in peer marriage rates increases the likelihood of marriage at 24 months by 1.37 percent, a slight decrease from the 1.93 percent reported in Table 3, column six. Re-scaling the remaining coefficients in column seven in the same manner, we find that peer marriage rates has the largest effect on own-marriage of all the included controls.[25] We conclude from this analysis that other factors that are correlated with peer marriage rates cannot explain our results, and in fact peer marriage rates have the largest effect on own-marriage given the existing in-sample variation.[26]

6.3. Additional Heterogeneity Analysis

6.3.1. Individual Characteristics

Motivated by differences in marriage rates by race in the general population, we next estimate the effect of peers on marriage for demographic groups defined by race and gender. There are substantial differences in racial composition by gender in our sample, so in our view it is important to consider potential gender heterogeneity alongside differences by race.[27] The results are in Table 6. As shown in column two, women's marriage probability is inversely related to peer marriage rates, with a large, negative coefficient of -0.21 on the interaction of female with treatment peer marriage rates as compared to a main effect of 0.07 for the treatment. Subsequent columns show that this result seems to be driven by Black and Hispanic women. In column four, we see that Black and

[25] To obtain these re-scaled coefficients, we first regress each variable on our randomization fixed effects, θ_r, and obtain the residuals. Next, we multiply the coefficient estimate by the interquartile range in residual variation and divide by the dependent variable mean.

[26] To provide information on how each of the peer group characteristics varies across the distribution of peer marriage rates, we report summary statistics for peer groups by quartile of treatment in Table A4.

[27] Approximately two-thirds of men in our sample are white, but only one-third of women are white.

Hispanic men are more positively affected by peer effects, even more so than white men. In column five, which restricts the sample to women, Black and Hispanic women with higher peer marriage rates have lower likelihood of marriage. However, we have only a small sample of women, and conditional on being married, women are substantially more likely to be married to another service member than men in our sample (see discussion in subsubsection 6.3.2 below). The latter fact reduces the potential generalizability of any results from our small sample of Army women. For these reasons, we conclude that our analysis does not allow us to speak definitively to the question of gender heterogeneity in the impact of peers on marriage. We therefore focus the remainder of our analysis on men in our sample, who predominantly marry civilian spouses.

Next, we look for evidence of heterogeneity by age and education. The average age upon arrival in our unmarried sample is 21 years old (Table 1), but over half of the sample is less than 21 upon arrival and about 14 percent are over 22. The majority of our sample has a high school degree. In Table A5 we interact the treatment with indicators for quartiles of age (column two) and education level (column three). In column two, there are no statistically significant differences across age quartiles, but there is some evidence that for individuals in the fourth quartile of age (older than ~21.5), the effect of peers is larger, although the difference is only marginally significant. The pattern of the point estimates in column two suggests that the higher the age the greater the impact of peer marriage rates on likelihood of marriage. Meanwhile, the estimates in column three show that those with less than a high school degree are more impacted by the marriage of peers than those with a high school degree. However, since the vast majority of our sample consists of high school graduates, we are hesitant to draw strong conclusions from this result.

Finally, we investigate whether peer effects differ between units that deployed and those that did not. In this case, the heterogeneity we consider affects companies of soldiers as a group, rather than individuals only.[28] This sheds further light on the potential for the common shock of deployment to affect our OLS results. Although our 2SLS approach addresses a range of common shocks, we can examine heterogeneity in our estimated peer effects with combat deployments directly. A large proportion of our sample spends at least some time deployed in the first 24 months after assignment (61 percent). If combat deployments are acting as a common shock, we would thus expect that the difference between our OLS and 2SLS estimates would be smaller for peer groups that did not experience a deployment. We report estimates separately for peer groups that deployed and those that did not deploy within 24 months of a soldier's assignment in Table 7. The level difference in OLS and 2SLS estimates is similar for units that did not deploy relative to units that did deploy

[28]Deployment refers to a period of service away from a soldier's assigned duty location. The deployments considered here are combat deployments during which a soldier receives hostile fire pay. We define deployment at the level of the unit, rather than the individual level, to exclude the possibility of selection into or out of deployment.

(comparing the differences in column one and two to those for column three and four), suggesting little role for deployment as a substantial common shock in our setting.

This exercise can also help us think about the role of location and proximity in our peer effects, something we elaborate on in the next subsection. It is unclear whether deployments would result in larger or smaller peer effects. On the one hand, soldiers spend more time together while deployed, so they may be more likely to be influenced by peers. On the other hand, soldiers' exposure to family and friend networks while deployed is reduced, which might decrease opportunities to find a spouse. In addition, there is an incapacitation effect since soldiers cannot get married while deployed. Comparing the point estimates in columns two and four, the effect of peers is larger in instances where soldiers did not deploy, but the difference is not statistically significant, as seen in column five where the coefficient on the interaction of fraction of peers married and deployment is not significant. Even so, the estimate in column two where soldiers did not deploy may be more generalizable to other populations and is larger than our main estimate in Table 3. For soldiers assigned to peer groups that do not deploy, a 6.5 percentage point increase in the fraction of peers who are married increases marriage at 24 months by 2.6 percent, although the result is not statistically significant in this smaller sample.

6.3.2. Supply of Potential Spouses

Previous research has provided evidence for assortative mating, for example by education (Eika, Mogstad, and Zafar 2019). We therefore might expect that the estimated peer effect would be different in situations when there are many potential spouses versus when there are not. This would be the case, for example, if individuals are constrained by the conditions of the local marriage market. To assess whether the supply of potential spouses is related to our peer effect estimates, we estimate the effect of peers on various sub-samples of the data where the supply of potential spouses is plausibly changing. Motivated by prior work that points to a decline in the population of marriageable men as an explanation for declining marriage rates, we also use data from the Quarterly Workforce Indicators (QWI) to identify soldiers assigned to locations where the labor market for potential spouses is strong or weak.

First, we consider whether an individual's peer group is a relevant marriage market. If this were the case, then our treatment, the share of peers who are married, captures not just the effect of peers, but also the supply of potential spouses. Notably, this would work against the positive peer effect we estimate, as more married peers would decrease the pool of available spouses, thereby *decreasing* the scope for new marriages to form.

We can identify soldiers that marry another Army service member from Oct. 2003 through the end of our sample period. Among married soldiers, 10.6 percent are married to another service member. However there is substantial heterogeneity by sex. Conditional on being married, almost half (45.7 percent) of women are married to another service member, while among married men only 6.7 percent are married to a service member. In light of the low rate of dual-marriage for men, the fact that approximately 95 percent of our sample is male, and noting that higher rates of peer marriage would tend to push our estimates downward through the mechanical effect of decreasing the supply of potential spouses, we argue that intra-group marriage, while not entirely irrelevant, is not a major factor in our context.[29]

Next, we assess how the effect of peers varies with the characteristics of the local marriage market by estimating Equation 3 on subsamples defined by location characteristics reflective of this market. The results are presented in Figure A6. First, we test for a differential effect of peers in locations with above median employment or earnings for individuals age 19-34. The point estimate in the case where the surrounding county has above median employment prospects is approximately 60 percent the size of our main estimate in Table 3, column six. This suggests that soldiers may have a more difficult time finding potential partners in these circumstances, but the estimate is insignificant and the confidence interval includes our main estimate, so we are limited in what we can infer from this single point estimate.[30] The estimate in instances where age 19-34 earnings are above the median is near zero, which is consistent with a limited supply of spouses constraining the effect of peers, but again the estimate is imprecise. Next, we consider locations where a relatively higher fraction of the age 19-34 population is the same race as the focal individual. While the results are imprecise, for Black soldiers the point estimate is near zero, while the result for Hispanic soldiers is not substantially different from the results by race in Table 6. Overall, we conclude that there is little evidence that local marriage market features strongly mediate the peer effects we identify.

Proximity to home may also impact our estimates of peer effects on marriage. For example, if a soldier has a set of potential marriage partners in his home state, then exposure to high rates of peer marriage while stationed near home may lead to larger increases in own-marriage rates. On the other hand, living near home peers may dilute the effect of Army peers. We test for heterogeneity by assignment location and report the results in Table A6. We categorize individuals into four mutually exclusive categories: those stationed in their home state, those stationed in a state adjacent to their home state, those stationed in their home census region who are not in the first two categories, and those not stationed in their home census region. We estimate Equation 3, where we interact the treatment with indicators for each of these categories. We do not find evidence that soldiers who are living close to home are differently influenced by peers. The coefficient on the interactions are

[29] If we limit the sample to peer groups with women and breakout the treatment by sex, we find no evidence that the share of female peers who are married has an effect on own-marriage for men at 24 months.

[30] The foregoing analysis of location characteristics may be limited due to a lack of variation in some characteristics in our sample. All the relative measures (e.g., median) were defined in sample because Army posts are generally located near cities. Compared to the full sample of U.S. counties, locations with an Army post generally have larger populations and higher employment.

all statistically insignificant, although the point estimates suggests stronger effects for individuals located in their home state. A potential explanation for this pattern of results is that individuals who live near home may be more likely to meet potential spouses from home and be influenced by their peers in the Army to get married.

6.4. Additional Outcome: Marriage Persistence

To assess whether peers affect marriage decisions over a longer time horizon, in Figure 4 we estimate Equation 3 by 2SLS by month on a balanced sample of individuals who remain in the Army for at least 36, 48, and 60 months, respectively. It is important to note that individuals who remain in the Army for 48 months are a subset of those who remain for 36, and so on. For the sample that stays in for at least 3 years, the results indicate that the effect of peers peaks around 24 months but the point estimate remains positive out to 36 months. For the samples that stay in for four or five years, the effect of peers peaks at nineteen months and drops below zero around 32 months (although not statistically significant).[31] These results could suggest fade out in the effect of earlier career peers. As can be seen in Figure A1, after 36 months the majority of soldiers are no longer in the original peer group (Panel A), and for those that are, approximately 90 percent of their original peers have moved. This turnover could drive fade out—as new peers arrive, the treatment becomes a poor measure of the actual peer group to which a soldier is exposed. Another explanation could be re-timing; that is, more married peers encourages individuals to marry earlier but there is a catch-up effect as individuals age so that the long-run effect of peers is close to zero. In Table A7, we report the OLS and 2SLS point estimates at two, three, four, and five year time horizons.

Greenberg et al. (2022) find that Army service increases marriage for both Black and white service members in the short-run. In the long-run, they find that the effects dissipate for white soldiers within five years of enlistment but persist for Black soldiers for at least 19 years.[32] In light of this finding, along with our finding that peers appear to be more important for Black and Hispanic male soldiers (Table 6), we examine the persistence of the effect of peers on marriage by race over 36 months in Figure 5. The effect of an individual's initial peer group persists over time for Black individuals. A 6.5 percentage point increase in the fraction of an individual's original peers who are married increases the likelihood of marriage at 36 months for Black soldiers by 1.6 percentage points, or 4.5 percent ($p < 0.01$). The effect of peers for white men is very similar to the full sample

[31] Importantly, the point estimates cannot be directly compared across panels as in each case the sample is restricted to individuals who remain in the Army for the entire period. To some extent differences across panels reflect heterogeneity by initial enlistment term length — individuals who sign longer initial enlistment contracts make up an increasing share of the sample moving from Panel A to Panel C.

[32] A key difference between our paper and Greenberg et al. (2022) is that all of our comparisons are between Army service members, whereas in Greenberg et al. (2022) the comparisons are between Army service members and individuals who were on the margin of serving in the Army.

estimates, peaking between 21 and 24 months and then declining over time, although the point estimates remain positive. For Hispanic men the effect is similar to white men in the 36-month sample, with the effect of peer marriage rates starting later, peaking between 18 to 20 months, and fading out around 28 months. The finding that the effect of peers grows over time for Black men provides some evidence that our results are not completely driven by re-timed marriage, at least for this group.

6.5. Additional Outcome: Fertility

In this section we look for evidence of peer effects on fertility. We view this analysis as exploratory, as it is difficult for us to detect peer effects on fertility given the relatively short time horizon of our analysis and the fact that a large majority of our sample is unmarried upon arrival. First, we estimate whether having more married peers increases the likelihood of having any additional children 24 months after assignment.[33] Table A8 shows estimates of Equation 3 where the dependent variable is an indicator for having children at 24 months. The 2SLS estimate in column six shows a small negative effect of peers on fertility, although the result is statistically insignificant: a 6.5 percentage point increase in the fraction of married peers lowers the likelihood of having a child within 24 months of assignment by 0.45 percent. We also estimate peer effects on fertility by month in Figure A7. The results again show no statistically significant impact of peers on having children.

Although the sample of soldiers who were married upon arrival is relatively small, there is more scope for peers to affect fertility decisions for this group as the transition into parenthood requires less time and planning than for an unmarried individual. In Table A9, we present estimates of Equation 3 for the married sample where the dependent variable is an indicator for having any additional children within 24 months of assignment. Here a six percentage point increase in the fraction of married peers increases the likelihood of having children within 24 months of assignment by 1.34 percent, but the estimate is not statistically significant. We do not view these results as precluding larger peer effects on fertility over longer time horizons than we can observe in our data.[34]

7. Investigating Mechanisms and Magnitudes

In this section we consider mechanisms that might explain our results and attempt to put the magnitude of our estimates in context. Before turning to that, it is helpful to recap our findings. Our results show a positive effect of peer marriage rates on the likelihood of an individual marrying in

[33] Results using the fraction of peers with children in place of fraction of peers married are nearly identical; the two measures are highly correlated.

[34] Figure A8 presents estimates of Equation 3 by month for the marriage and fertility outcomes on the sample of individuals who were married upon arrival.

the next two years. The effect is driven by marriage rates across the various levels of peers within an Army company. In terms of heterogeneity, we find evidence that Black and Hispanic men are more affected by their initial peer group assignment. Also, peer effects appear somewhat larger for soldiers stationed closer to home, though effects are present even for those stationed several states away. On other dimensions — location, year of arrival, age, education, sex, and labor and marriage market characteristics — we find limited evidence of heterogeneity, suggesting that peers positively influence marriage in a variety of contexts.

The long lag with which peer effects operate and the general persistence of the impact suggest to us that peers do not simply encourage marriages to form earlier (re-timing). Instead, higher peer marriage rates seem to encourage higher long-run marriage rates.[35] The effect of initial peers gets smaller over time for white and Hispanic men, due either to a fade out effect or the aggregation of effects across multiple peer groups. However, for Black men the effect of initial peers persists and is still growing 36 months after assignment.

7.1. Potential Mechanisms

Why do peer marriage rates have the observed effects on own-marriage? We consider two channels: conformist behavior and spillover effects. We define conformist behavior as choosing behavior to match that of others when that behavior is more common in the group. This could arise through copying a behavior (imitation or role-modeling) or through a desire to adhere to perceived norms (social pressure). Alternatively, for spillover effects, married peers provide access to some factor that raises marriage propensity. This could be information on the benefits of marriage (knowledge spillovers) or access to social connections to marriageable or marriage-inclined individuals. These channels align with the two prevalent approaches to modeling a role for peers in generating utility, as reviewed in Boucher et al. (2024). Under the conformist peer effect channel, it is the peers themselves who influence an individual. In the spillover channel, individuals are accessing something through peers, like information or matches, that raises marriage probabilities. Understanding the mechanism is relevant for understanding whether other policies or interventions might have similar impacts to peer marriage exposure.

Both channels are plausibly important to individuals generally and to soldiers in our setting. In the Army, there are unique benefits to marriage that are not typically found in the civilian sector. For example, lower ranking soldiers are required to live on-post in barracks unless they are married and higher ranking soldiers receive a greater housing allowance if they are married. Married soldiers also receive separation pay if they deploy or are geographically separated from their spouse for more

[35]Our view that a 9 to 12 month horizon is longer than re-timing would require is also informed by results in Carter and Wozniak (2023). They find that the timing of marriages is highly responsive to location assignment changes, suggesting that re-timing could occur quickly.

than 30 days. Therefore, the information gained from learning about these benefits may be especially influential in our setting as a part of the spillover effect. On the other hand, the military famously values conformity. This may extend implicitly to behaviors that are not directly part of military work. Both channels are plausibly important to civilians as well, with individuals weighing the gains of aligning their marriage choices with those of their social group, family, or local community as well as drawing information or marriageable connections from those communities.

Distinguishing between these explanations is difficult empirically. However, we believe our setting allows us to explore each mechanism and to provide suggestive evidence on their relative importance. We first consider the conformist peer effect channel. Some evidence in favor of this channel has already been reported. Namely, we find that marriage choices are responsive to the marriage rates of not only horizontal peers but also first-line and second-line supervisors. Moreover, impacts of marriage rates in the company overall are largest (Table 4). Since supervisors are prohibited from interacting with soldiers outside the workplace environment, it is unlikely that their social networks are relevant for newly enlisted soldiers. Instead, we posit that at least some of the peer effects we estimate are the result of workplace norms set by supervisors. We view this as suggesting that conformist peer effects contribute to our results.

Under a spillovers network channel, we would expect peer effects to increase in the size of the peer group, since larger peer groups afford more opportunity to access larger networks or information. We test this by interacting peer marriage rates with indicators for company size and report the results in column two of Table A6. The point estimates are consistent with the effect of peers increasing in the size of the peer group, but none of the differences are statistically significant at the five percent level. We take this as weak evidence that is consistent with a spillovers network channel.

Our main results differ by race, with white soldiers being less influenced by peer marriage rates than Black and Hispanic soldiers, as shown in Table 6. Black soldiers also continue to be influenced by their original peer group 36 months after arrival while for white soldiers the effect fades out (Figure 5). It has been well documented in the sociology literature that people tend to gravitate their networks towards people with whom they share similar characteristics, such as race or gender; a term referred to as homophily (for a review, see McPherson, Smith-Lovin, and Cook 2001). Marsden (1987) and Currarini, Jackson, and Pin (2009) further find that more populous racial groups have larger networks. Based upon this body of work, we hypothesize that Black and Hispanic soldiers have smaller social networks than white soldiers, both inside and outside of their assigned peer group. We conclude that the peer effects we find are unlikely to be due to a spillover network effect since the largest peer effects we find are from the racial groups (Black and Hispanic) that we would hypothesize to have smaller networks. To summarize, we find the weight of evidence to be more in favor of a conformist peer effect, rather than a spillover network effect. Our view is that the peer effects we find are explained primarily by soldiers imitating or modeling behavior they see among their peers or responding to existing social norms.

7.2. Magnitudes

To assess whether the peer effects we estimate are economically meaningful in the face of a 30 percentage point secular decline in marriage rates for individuals like those in our sample, we benchmark against estimates from other contexts. This is challenging since no direct comparisons are possible with previous literature. However, we compare our estimates to two relevant analyses on marriage propensity. Greenberg et al. (2022) estimate the effect of Army service on marriage for a sample of individuals who applied for Army service from 1990-2011, which has significant overlap with our sample. They find that Army service increases marriage by 18.5 percentage points three years after applying. By race, they find that Army service increases marriage by 20.3 percentage points and 17.1 percentage points among Black and white applicants three years after applying, respectively.[36]

How much of the total increase in marriage for Army service members can be explained by peer effects? To answer this question, we perform a back of the envelope calculation for the sample of male soldiers who are unmarried upon arrival to their first unit of assignment. First, we use data from the American Community Survey (ACS 2023) to calculate the fraction of the age 18-35 population in a soldier's home state who are married in the year a soldier arrives to their first unit.[37] This is an approximation of the prevalence of marriage among a soldier's counterfactual peers had they not joined the Army. For each soldier, we calculate the difference between the fraction of Army peers who are married and the fraction of peers from their home state who are married to arrive at an estimate of the change in marriage behavior among peers encountered by a soldier who joins the Army. On average, the fraction of married peers is 14.8 percentage points higher in the Army relative to home state peers in this sample. Our peer effect estimates therefore imply that joining the Army would increase marriage near the three-year mark from application (two years after arrival to

[36]In Greenberg et al. (2022), the authors estimate the effect of Army service on a range of outcomes using a regression discontinuity design where the running variable is an applicant's AFQT percentile score. Their estimation strategy leverages two distinct cutoffs: one at the 31st percentile and one at the 50th percentile. The estimates reported here come from the cutoff at the 31st percentile three years after first applying for Army service. We use the three year estimates as they most closely align with the timing of our estimates - two years after arrival to first duty station. In general, applicants spend several months awaiting the start of training and then on average six months in initial training in our sample. Accounting for time to process for entry and to conduct a move, individuals arrive at their first duty location approximately one year after applying, on average.

[37]We use the ACS 1-year estimates from 2001 to 2018 to estimate the fraction of age 18-35 individuals who are married in each state by year from 2001-2018.

first unit) by 1 percentage point, or 5.3 percent.[38] The estimates are somewhat larger for Black (1.9 p.p., 8.4 percent) and Hispanic (1.4 p.p., 6.7 percent) service members relative to white service members (0.8 p.p., 4.1 percent). We conclude that our peer effect estimates explain 5.4 percent of the total effect of Army service on marriage: 9.2 percent for Black individuals and 4.4 percent for white individuals.

Next, we benchmark our estimates against a strand of the literature that has explored the effect of government health care policies on marriage. Most recently, Barkowski and McLaughlin (2022) estimate the effect of the expansion of dependent coverage eligibility at the state and federal level. They find that state dependent coverage eligibility (which often prohibited marriage), decreased marriage among young adults (age 19-25) by 2.1 percentage points. They also find that when dependent coverage eligibility was expanded to all states under the Affordable Care Act (ACA) in 2010, marriage increased for young adults in states that already had existing dependent coverage by 2-3 percentage points.[39] For the sample described in the preceding paragraph, generating a short-run increase in marriage on the order of 2 p.p. would require approximately a 29.6 p.p. increase in the fraction of married peers. A change of this magnitude would be substantially larger than that experienced by an individual moving from the state with the lowest rate of young adult marriage (Rhode Island) to the state with the highest rate of of young adult marriage (Wyoming) in 2022 (18.7 p.p.).

Both of the foregoing exercises suggest that peer effects of the magnitude we estimate are relatively small, explaining less than one-tenth of the total effect of Army service on marriage and an order of magnitude smaller than the effect of the ACA on young adult marriage. However, it is important to note that our estimates only take into account the immediate effect of peers, so it is possible that peers have more or less influence over longer time horizons. For example, the effect of peers may have dynamic effects; a small change in peer marriage rates may "snowball" over time (see e.g., Dahl, Løken, and Mogstad 2014). Furthermore, we estimate peer effects in a setting with substantial turnover that most likely exceeds what is experienced by the general population. In a setting where peer groups are relatively stable over long periods, the effect of peers may be amplified. For these reasons, we contend that the effect of peers on marriage is economically meaningful.

[38] For this sample, our peer effect estimate is 0.068 ($p < 0.05$). We multiply this point estimate by the average change in peer marriage rates induced by joining the Army (14.8 p.p.) and divide by the mean marriage rate of arriving soldiers 24 months after arrival (19.1 percent in this sample).

[39] The dependent coverage provision of the ACA did not include any restrictions on marriage and expanded access to dependent coverage through age 26 for all states. The ACA thus removed a disincentive for marriage that was present in states that previously only expanded dependent coverage for individuals who were unmarried.

8. Concluding Discussion

Our empirical setting provides unique features and high quality longitudinal data that allow us to answer new questions about the influence of peers on marriage formation. We estimate peer effects using a sample of new enlistees into the U.S. Army. This group is exogenously assigned to peer groups within the Army but also has much in common with the general population that has experienced declining marriage rates over the last 50 years. We find a positive effect of peer marriage rates on the likelihood of an individual marrying in the next two years. The effect is driven by marriage rates across the various levels of peers within an Army company. We find evidence of heterogeneity by race (larger peer effects for Black and Hispanic soldiers). Our results are largely stable and consistent across many other dimensions that may be supposed to affect marriage, such as age, education, and labor and marriage market characteristics. For white and Hispanic men, the effect of peers approaches zero after 36 months, but the effect of peers continues to increase over the same time horizon for Black men. Due to the nature of our Army sample, we conclude that we can say little about gender-specific impacts of peers on marriage. Our results reflect impacts for men.

While we are limited in what we can say about the mechanisms that drive our effects, in our view the evidence is most consistent with a direct effect of peers through role-modeling and norms (conformist behavior). We do not find evidence that peers are providing greater access to social networks or transferring new information about the costs and benefits of marriage. The magnitude of our estimates suggests that peers play an important role in young adult marriage decisions in our sample. To better understand how peer effects contribute to long-run trends in marriage rates would require a dynamic model that we leave to future research. However, we are able to benchmark our estimates against prior work and find that peer effects from soldier's initial company assignments explain approximately five percent of the total effect of Army service on marriage.

The Army context allows us to credibly identify the effect of peers on marriage, but it also raises questions about the generalizability of our findings. To assess potential generalizability, it is worth considering aspects of Army life that may make peers more or less influential than in the general population. There are reasons to think peer effects may be either larger or smaller in our setting than for some civilian peer groups. First, we observe a group in which marriage formation is relatively common, so there is scope for the marriage decision to respond to peers if this is an important channel. In our primary sample of soldiers who are unmarried upon assignment to a peer group, 19 percent are married after 24 months. Second, selection into the Army might identify individuals who value fitting into a group. Both factors may mean that in our setting, peers would be more influential than in a general population sample.

On the other hand, the Army facilitates marriage by providing additional supports to married soldiers and their spouses. Also, individuals who enlist may tend to value structure and stability.

Both factors may raise marriage rates in the Army relative to a general population and thereby limit the scope for peers to influence individual decisions, leading peer effects in the Army context to be smaller than the broader population. Marriage rates in our sample are indeed higher than in a civilian population of similar age, education, and other characteristics. The mean marriage rate in our sample of peers (which includes more senior military members) is 48 percent. There is, however, substantial variation, suggesting that marriage is far from universal in the Army.

We define a peer group at a point in time and for groups of soldiers that work together directly. Soldiers will also have peers from initial training periods, other groups at their assigned location, and new peers they meet at new assignments. These peers may have positive or negative effects on marriage rates, so the effect we find for soldiers in this sample is not necessarily the net peer effect they will have while in the Army. This parallels civilian settings, where multiple peer groups of varying intensities likely overlap, such as old friends versus work colleagues. Given the variety of peer groups in daily life, it is likely that our setting identifies peer effects that are of a magnitude on par with at least some civilian settings.

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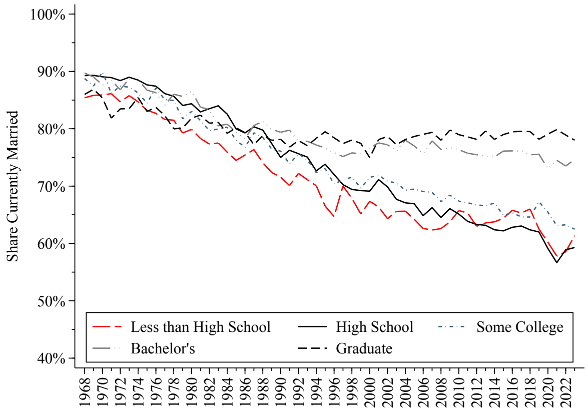
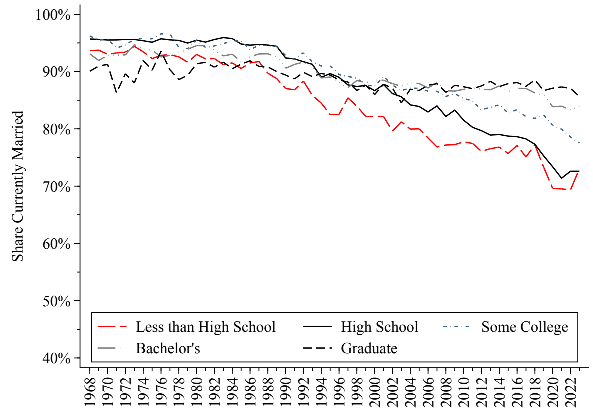
Figure 1. Share of Adults Aged 40-45 Who Are Married by Year, 1968-2023



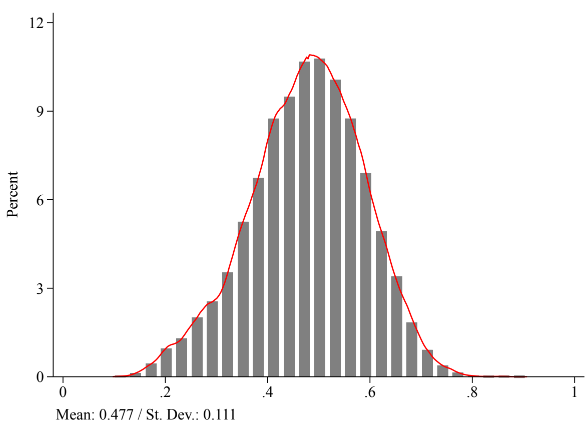
(a) Currently Married



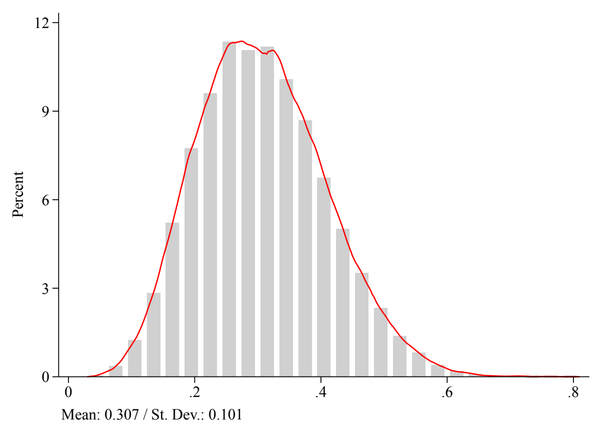
(b) Ever Married

Notes: Figures are constructed using data from the Current Population Survey's Annual Social and Economic Supplement (CPS ASEC) for years 1968-2023 (ASEC 2023). In both panels, the sample is limited to individuals who are aged 40 to 45 in each year. Prior to 1992, the ASEC captured education beyond high school as years of college. To enable a comparison over time, we count individuals with one to three years of college as "Some College"; four years of college as "Bachelor's"; and more than four years of college as "Graduate" for the years 1968-1991. Starting in 1992, education is coded by the degree achieved.

Figure 2. Treatment and Instrument Densities for the Sample of Unmarried Soldiers



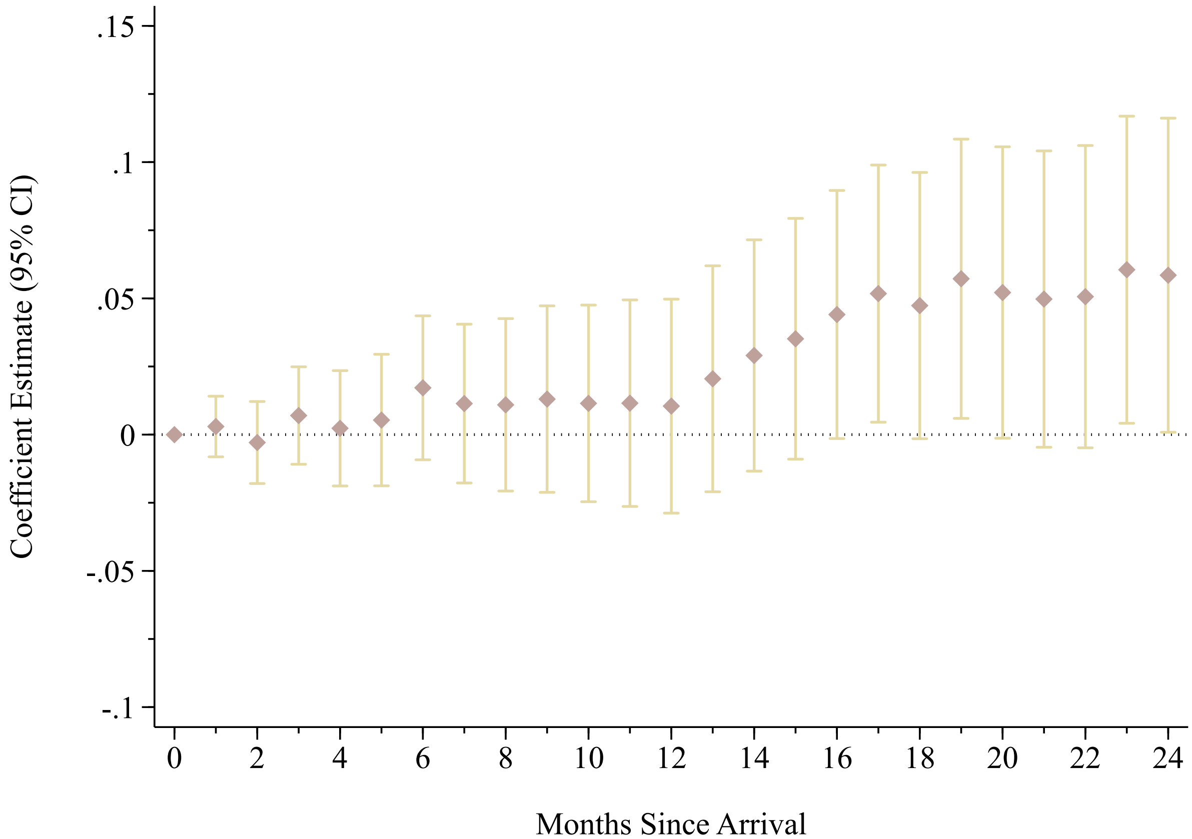
(a) Treatment: Fraction of Peers Currently Married



(b) Instrument: Fraction of Peers Married at *Their* Previous Location

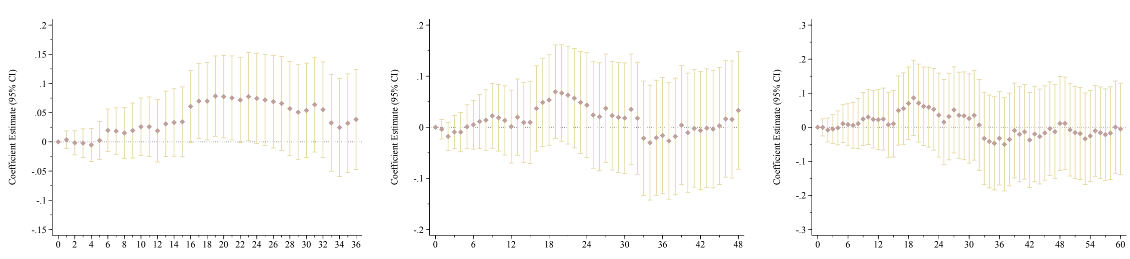
Notes: Panel A is a histogram of our primary treatment variable: the fraction of individuals in the peer group who were married in the month before the focal soldier's arrival. Panel B is a histogram of the instrument. We construct the instrument by measuring marriage at each peer's previous location prior to assignment to the current location. Sample means and standard deviations are displayed below each figure. The red lines are kernel density estimates using an Epanechnikov kernel and Silverman's rule of thumb bandwidth. The residualized treatment and instrument densities are in Figure A3.

Figure 3. Peer Effects on Marriage - Unmarried Sample (2SLS)



Notes: This figure displays 2SLS coefficient estimates and 95-percent confidence intervals from Equation 3 by month relative to arrival (month zero) for the sample of soldiers who were unmarried upon arrival. The dependent variable in each month is an indicator for an individual being married.

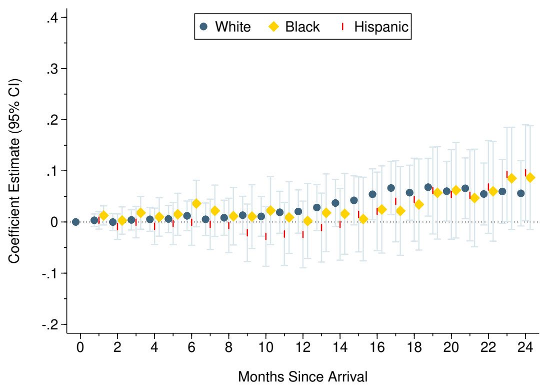
Figure 4. Longer Run Peer Effects on Marriage - Unmarried Sample (2SLS)



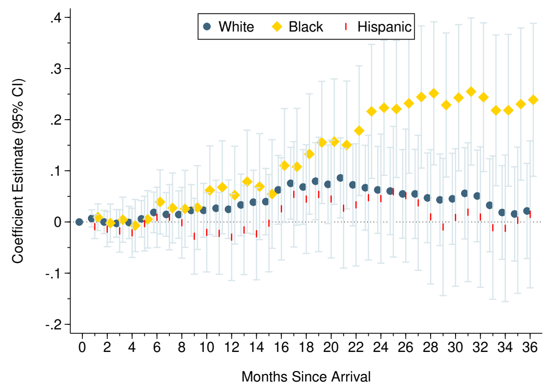
(a) Three Years (b) Four Years (c) Five Years

Notes: This figure displays 2SLS coefficient estimates and 95-percent confidence intervals from Equation 3 by month relative to arrival (month zero) for the sample of soldiers who were unmarried upon arrival. The dependent variable is an indicator for an individual being married. In each panel, the sample consists of soldiers who are in the Army until the end of the sample window. As such, each event study is estimated on a balanced sample, but point estimates are not directly comparable across panels. The number of individuals included in each Panel is 85,159, 55,407, and 38,824 in Panels A, B, and C, respectively.

Figure 5. Peer Effects on Marriage By Race - Unmarried Sample (2SLS)



(a) Two Years



(b) Three Years

Notes: This figure displays 2SLS coefficient estimates and 95-percent confidence intervals from Equation 3 by month relative to arrival (month zero) for the sample of soldiers who were unmarried upon arrival. The dependent variable is an indicator for an individual being married. In both panels, the sample is limited to *male* soldiers. Panel B also restricts the sample to individuals who are in the Army for at least 36 months after arrival. The regression specifications include interactions for Black, Hispanic, Asian, Native American, and Pacific Islander. Only Black and Hispanic are shown as the sample size for other races is small. In both panels, we display the "combined" coefficients. For White soldiers, this is just the coefficient on our primary treatment variable. For Black and Hispanic soldiers, this is the linear combination of the coefficient on our primary treatment variable and the coefficient on the relevant interaction term. In each panel, the sample consists of soldiers who are in the Army until the end of the sample window.

Table 1. Summary Statistics

	Full Sample (1)	Unmarried (2)	Married (3)	Men (4)	Women (5)
Panel A. Soldier Characteristics					
Female	0.05	0.05	0.04	0.00	1.00
White	0.65	0.65	0.62	0.66	0.34
Black	0.15	0.15	0.14	0.14	0.40
Hispanic	0.15	0.15	0.19	0.15	0.18
Age	21.01	20.74	23.61	21.02	20.85
	(2.73)	(2.45)	(3.74)	(2.73)	(2.84)
AFQT Percentile	55.56	55.70	54.22	55.97	47.78
	(18.38)	(18.40)	(18.19)	(18.41)	(16.05)
Less Than High School	0.11	0.10	0.18	0.11	0.04
High School Graduate	0.83	0.85	0.72	0.83	0.89
Some College/Associate's	0.05	0.04	0.08	0.05	0.05
Bachelor's or Higher	0.01	0.01	0.02	0.01	0.01
Panel B. Other Service Information					
Direct Combat Occ.	0.64	0.65	0.57	0.67	0.05
3-year Initial Contract	0.68	0.67	0.72	0.69	0.51
4-year Initial Contract	0.24	0.24	0.21	0.23	0.41
5-year Initial Contract	0.06	0.07	0.04	0.06	0.08
6-year Initial Contract	0.02	0.02	0.02	0.02	0.01
Months in Training	6.26	6.25	6.32	6.22	6.95
	(1.61)	(1.61)	(1.60)	(1.61)	(1.39)
Deployed within 24 Months of Arrival	0.61	0.61	0.63	0.62	0.46
Months Deployed (Cond. on Deploying)	9.36	9.35	9.38	9.36	9.23
	(3.54)	(3.54)	(3.46)	(3.53)	(3.66)
Completed First Term	0.93	0.93	0.93	0.93	0.88
Re-enlisted	0.41	0.39	0.53	0.40	0.46
Assigned in Home State	0.06	0.06	0.07	0.06	0.07
Assigned in Home Region	0.35	0.35	0.37	0.35	0.42
Panel C. Marriage and Fertility					
Married on Arrival	0.09	0.00	1.00	0.10	0.07
Married, $t+24$	0.27	0.20	0.97	0.27	0.38
Has Children on Arrival	0.08	0.03	0.53	0.08	0.07
Has Children, $t+24$	0.19	0.13	0.72	0.18	0.23
Panel D. Treatment Variables					
Fraction of Peer Group Married, $t-1$	0.48	0.48	0.50	0.48	0.55
	(0.11)	(0.11)	(0.11)	(0.11)	(0.09)
Fraction of Peer Group Married	0.31	0.31	0.32	0.30	0.39
at Previous Location, $t-1$ (IV)	(0.10)	(0.10)	(0.10)	(0.10)	(0.09)
Peer Group Size	113.62	113.63	113.57	113.58	114.36
	(36.91)	(36.88)	(37.29)	(36.66)	(41.43)
Months Assigned to Peer Group	29.63	29.65	29.50	29.81	26.33
	(14.88)	(14.82)	(15.43)	(14.88)	(14.54)
Observations	155,571	140,914	14,657	147,797	7,774

Notes: This table reports the mean and standard deviation for select variables for our main estimation sample. Columns two and three divide the sample by marital status upon arrival to an individual's first Army assignment. Our analysis focuses on the sample in column two - those individuals who are unmarried upon arrival to their first assignment location. Columns four and five divide the sample by gender. Standard deviations are only reported for continuous variables. Time is measured in months, where t is the month an individual arrives at a location. In Panel B, direct combat occupation refers to military occupations that the Army classifies as "combat arms." These occupations consist of infantry, engineers, field artillery, air defense artillery, aviation, special forces, and armor. In Panel D, the treatment and instrument are measured in the month prior to a soldier's arrival ($t-1$).

Table 2. Regression-Based Tests for Conditional Random Assignment to Peer Groups

		Treatment				Instrument		
	FE Only (1)	Race/Age (2)	Education (3)	All (4)	FE Only (5)	Race/Age (6)	Education (7)	All (8)
Black		0.0001		0.0003		0.0007		0.0009
		(0.0006)		(0.0006)		(0.0006)		(0.0006)
Hispanic		-0.0003		-0.0002		-0.0006		-0.0005
		(0.0005)		(0.0006)		(0.0005)		(0.0005)
Asian		0.0016		0.0016		0.0003		0.0003
		(0.0013)		(0.0013)		(0.0012)		(0.0012)
Nat. Amer.		0.0002		0.0003		0.0023		0.0024
		(0.0018)		(0.0018)		(0.0018)		(0.0018)
Pac. Isl.		0.0004		0.0005		-0.0002		0.0000
		(0.0014)		(0.0014)		(0.0013)		(0.0013)
Age		0.0000		0.0001		0.0001		0.0001
		(0.0001)		(0.0001)		(0.0001)		(0.0001)*
High School			0.0009	0.0009			-0.0002	-0.0002
			(0.0007)	(0.0007)			(0.0006)	(0.0006)
SMC/ASC			-0.0011	-0.0012			-0.0010	-0.0012
			(0.0012)	(0.0012)			(0.0011)	(0.0011)
BA+			0.0000	-0.0003			-0.0079	-0.0084
			(0.0062)	(0.0062)			(0.0053)	(0.0053)
AFQT Perc.			0.0000	0.0000			0.0000	0.0000
			(0.0000)	(0.0000)			(0.0000)	(0.0000)
Observations	140,914	140,914	140,914	140,914	140,914	140,914	140,914	140,914
R-squared	0.761	0.761	0.761	0.761	0.741	0.741	0.741	0.741
Individual Controls	-	✓	✓	✓	-	✓	✓	✓
F-Statistic (Joint Test of Controls)		0.347	1.665	0.923		1.343	0.985	1.274
P-Value of F-Statistic		0.912	0.155	0.511		0.234	0.414	0.238

Notes: This table presents estimates of Equation 4 for the sample of individuals who were unmarried on arrival to their first assignment. In columns 1-4 the dependent variable is the fraction of individuals in a peer group in the month before an individual arrived who were married (Treatment). In columns 5-8 the dependent variable is the fraction of individuals in a peer group in the month before an individual arrived who were married at their previous assignment location (Instrument). Interacted fixed effects for sex, job, rank, month of arrival, location, and initial term of enlistment, θ_r, are included in all regressions. The column headings indicate the types of controls included. Heteroskedasticity robust standard errors are reported in parentheses beneath each coefficient (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). The last two rows report the F-statistic and associated p-value from a hypothesis test where the null hypothesis is that the coefficients on all controls (excluding the fixed effects) are jointly equal to zero.

Table 3. Peer Effects on Marriage

	OLS		Reduced Form	First Stage	2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Fraction of Peers Married, $t-1$	0.063	0.054			0.069	0.059
	(0.020)***	(0.020)***			(0.029)**	(0.029)**
Fraction of Peers Married at Previous Location, $t-1$ (IV)			0.043	0.737		
			(0.022)**	(0.007)***		
Observations	140,914	140,914	140,914	140,914	140,914	140,914
Clusters	3,047	3,047	3,047	3,047	3,047	3,047
R-squared	0.257	0.263	0.263	0.878	-	-
Individual Controls	-	✓	✓	✓	-	✓
Dep. Var. Mean	0.198	0.198	0.198	0.477	0.198	0.198
Effect of Moving up IQR (%)	2.076	1.790	1.421		2.284	1.927
F-Stat				641.2		
F-Stat p-value				$< .01$		
Endog. Test p-value					0.764	0.843

Notes: This table presents estimates of Equation 3 for the sample of individuals who were unmarried on arrival to their first assignment in col.'s 1-3, 5, and 6. Col. 4 shows estimates from the first-stage regression, Equation 5. In col.'s 1-3, 5, and 6, the dependent variable is an indicator for individual i being married 24 months after assignment to the peer group. In col. 4 the dependent variable is the fraction of individuals in a company who were married in the month before soldier i arrived. Interacted fixed effects for sex, occupation, rank, assignment location, month-year of arrival, and initial term of enlistment, θ_r, are included in all regressions. Standard errors, clustered at the company level, are reported beneath each coefficient (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). In col.'s 5 and 6, we report the p-value from a Hausman endogeneity test where the null hypothesis is that the treatment is exogenous. To ensure that we use in-sample variation to interpret our results, we report a re-scaled coefficient. This is calculated by multiplying each coefficient by the interquartile range (IQR) in residual treatment variation and dividing by the dependent variable mean. For brevity, this statistic is labeled "Effect of Moving up IQR (%)." The IQR for the treatment (row 1) is 0.065 and the instrument (row 2) is 0.058.

Table 4. The Effect of Peers by Frequency of Daily Interactions

	Company (Baseline) (1)	Same Rank Only (2)	First-Line Supervisors Only (3)	Second-Line Supervisors Only (4)	Senior Supervisors Only (5)	Joint (6)
Peer Group Definition:						
Army Company	0.059					
	(0.029)**					
Same Rank		0.010				0.007
		(0.029)				(0.029)
First-Line Supervisors			0.018			0.017
			(0.021)			(0.022)
Second-Line Supervisors				0.009		0.007
				(0.021)		(0.021)
Senior Supervisors					0.001	0.002
					(0.017)	(0.018)
Observations	140,914	140,914	140,914	140,914	140,914	140,914
Clusters	3,047	3,047	3,047	3,047	3,047	3,047
Dep. Var. Mean	0.198	0.198	0.198	0.198	0.198	0.198
Hypothesis Tests (p-value):						
Same Rank = First-Line Supervisor						0.807
Same Rank = Second-Line Supervisor						0.999
Same Rank = Senior Supervisor						0.867
First-Line Supervisor = Second-Line Supervisor						0.762
First-Line Supervisor = Senior Supervisor						0.575

Notes: This table presents estimates of Equation 3 for the sample of individuals who were unmarried on arrival to their first assignment. The dependent variable is an indicator for individual i being married 24 months after assignment to the peer group. The column headings indicate the individuals who are counted in the focal individual's peer group. Interacted fixed effects for sex, occupation, rank, assignment location, month-year of arrival, and initial term of enlistment, θ_r, and individual controls are included in all regressions. Standard errors, clustered at the company level, are reported beneath each coefficient (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). In col. 6, we report p-values from Wald tests on whether the indicated coefficients are equal.

Table 5. Controlling for Other Peer Group Characteristics

	Baseline	Sex Composition	Racial Composition	Avg. Age	AFQT & Education	Religious Composition	All Characteristics
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fraction of Peers Married, $t-1$	0.059	0.044	0.053	0.049	0.042	0.056	0.042
	(0.029)**	(0.030)	(0.031)*	(0.055)	(0.032)	(0.030)*	(0.055)
Share Female		0.045					0.040
		(0.022)**					(0.026)
Share White			-0.015				0.000
			(0.020)				(0.025)
Avg. Age				0.001			-0.001
				(0.002)			(0.003)
Avg. AFQT					0.000		0.000
					(0.000)		(0.000)
Share HSG					-0.047		-0.044
					(0.028)*		(0.028)
Share Catholic						-0.012	-0.017
						(0.040)	(0.041)
Share Protestant						0.027	0.018
						(0.031)	(0.032)
Share Mormon						0.070	0.067
						(0.126)	(0.126)
Share Other Rel.						0.025	0.023
						(0.046)	(0.046)
Observations	140,914	140,914	140,914	140,914	140,914	140,914	140,914
Clusters	3,047	3,047	3,047	3,047	3,047	3,047	3,047

Notes: This table presents 2SLS estimates of variations of Equation 3 for the sample of individuals who were unmarried on arrival to their first assignment. In col.'s 2-7, we add controls for other peer group characteristics. The dependent variable is an indicator for individual i being married 24 months after assignment to the peer group. Interacted fixed effects (θ_r) and individual controls are included in all regressions. Standard errors are reported beneath each coefficient (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). The dependent variable mean is 0.198.

Table 6. Heterogeneity by Sex and Race

	Baseline Estimate	By Sex	By Race	By Race	
				Men	Women
	(1)	(2)	(3)	(4)	(5)
Treatment	0.059	0.070	0.055	0.056	0.082
	(0.029)**	(0.030)**	(0.032)*	(0.033)*	(0.205)
Treatment x Female		-0.211			
		(0.124)*			
Treatment x Black			-0.050	0.031	-0.302
			(0.048)	(0.050)	(0.248)
Treatment x Hispanic			0.047	0.040	-0.184
			(0.043)	(0.044)	(0.303)
Female (Combined)		-0.141			
		(0.120)			
Black (Combined)			0.005	0.087	-0.220
			(0.049)	(0.052)*	(0.187)
Hispanic (Combined)			0.103	0.096	-0.102
			(0.047)**	(0.048)**	(0.243)
Observations	140,914	140,914	140,914	133,683	7,231
Clusters	3,047	3,047	3,047	3,019	1,622

Notes: This table presents 2SLS estimates of Equation 3. The treatment is the fraction of peers who were married in the month prior to arrival. The dependent variable is an indicator for being married 24 months after assignment to the peer group. In col.'s 2-5, we estimate interacted specifications by interacting the primary treatment and instrument with indicators for sex and race. For these interacted specifications, we present the total effect by demographic group by combining the main effect with the interaction terms. Col.'s 1-3 include the full sample of soldiers who were unmarried upon arrival to their first assignment. Col. 4 is restricted to men and col. 5 is restricted to women. Interacted fixed effects for sex, occupation, rank, assignment location, month-year of arrival, and initial term of enlistment, θ_r, and individual controls are included in all regressions. Standard errors, clustered at the company level, are reported beneath each coefficient (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Table 7. Peer Effects by Peer Group Deployment Status

	Did Not Deploy		Deployed		Interacted Specifications	
	OLS	2SLS	OLS	2SLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.093	0.084	0.033	0.043	0.073	0.071
	(0.037)**	(0.052)	(0.026)	(0.038)	(0.040)*	(0.040)*
Treatment x Deployed					-0.024	
					(0.045)	
Treatment x Deployed for < 9 Months						-0.043
						(0.047)
Treatment x Deployed for ≥ 9 Months						-0.007
						(0.046)
Observations	49,723	49,723	83,424	83,424	140,914	140,914
Clusters	2,161	2,161	2,453	2,453	3,047	3,047
Dep. Var. Mean	0.212	0.212	0.187	0.187	0.198	0.198

Notes: This table presents 2SLS estimates of Equation 3 for the sample of individuals who were unmarried on arrival to their first assignment. The treatment is the fraction of peers who were married in the month prior to arrival. In col.'s 1 and 2, the sample is restricted to individuals who were assigned to units that did not deploy within 24 months of an individual's assignment. In col.'s 3 and 4, the sample is restricted to individuals who were assigned to units that deployed within 24 months of an individual's assignment. The total sample size is smaller than in Table A8 due to splitting the sample by deployment status. All the comparisons in col.'s 1-4 are between soldiers who shared the same deployment status (did or did not deploy) in addition to all the other characteristics captured in θ_r. The dependent variable is an indicator for individual i being married 24 months after assignment to the peer group. Interacted fixed effects for sex, occupation, rank, assignment location, month-year of arrival, and initial term of enlistment, θ_r, and individual controls are included in all regressions. Col.'s 5 and 6 are interacted specifications that look for heterogeneity in the effect of peers based on peer deployment. In col. 5, we add an interaction between our primary treatment and an indicator for peer group deployment within 24 months of arrival. Col. 6 breaks out deployments into those that were less than or at least nine months long. In these columns we also interact all of our controls with an indicator for deploying within 24 months of arrival.

A. Appendix

Appendix material is for online publication only.

A.1. Sample Construction

To construct our main estimation sample, we first impose a set of restrictions to ensure that we identify legitimate soldier arrivals to new locations. We start with the sample of soldiers who have no previous Army experience and who arrive to their first operational Army unit from October 2001 through January 2018.[40] We require soldiers to have observations from initial entry training and to arrive at their first operational unit within 18 months of joining the Army. We drop any arrivals for soldiers who are not 17 to 35 years old at entry or who are missing information on sex, race, education, AFQT percentile score, or occupation as these variables are critical to the analysis. Summary statistics for this sample, reported from the time a soldier arrives to their first unit, are in column one of Table A10. In total there are just over 751 thousand arrivals that meet the criteria above during the sample period.

Next, we impose a set of sample restrictions to focus our empirical analysis on the sample of soldiers for whom we can identify the relevant peer group and conditional random assignment is most likely to hold. We require soldiers to arrive to an operational Army unit based in the U.S. and not associated with recruiting or training of any kind.[41] We require each unit to have 40 to 200 individuals assigned to ensure we can identify a soldier's company.[42] We also exclude any soldiers whose unit moves to a new location within 36 months of the soldier's arrival. This could occur when the Army re-organizes existing units by moving some units to new locations. We do not include soldiers in an occupation that becomes obsolete within 36 months of arrival or who have assignment considerations or physical limitations on file. In each of these cases, it becomes possible, although still not likely, that the soldier can influence their eventual assignment. Finally, we limit the sample to soldiers in the grade of E-4 or below who signed an initial enlistment contract for three to six years. After imposing these sample restrictions, we are left with approximately 340 thousand arrivals over the sample period, shown in column two of Table A10. While we lose a substantial portion of the sample due to the sample restrictions, the sample in column two is largely similar to the sample of all arrivals in column one. Based on our knowledge of how the Army assigns soldiers

[40]We cut off the sample in January 2018 to ensure that all of our estimates are based upon data from before the onset of the COVID-19 pandemic in February 2020.

[41]We also do not count soldiers who are initially assigned to a rear-detachment. A rear-detachment is a small portion of a unit that stays behind during an operational or combat deployment.

[42]Some of the unit identifiers in the data represent units that are much larger than companies (battalions or brigades). In this case, we cannot identify a soldier's peer group. Units that have fewer than 40 individuals are too small to be Army companies and consist of specialized units that have different peer dynamics from the rest of the sample.

to their first unit, we view the restrictions above as necessary to isolate plausibly random variation in peer group assignment.

For the majority of the analysis, we also restrict the sample to individuals for whom we can observe outcomes for at least 24 months after assignment. We thus estimate peer effects on a balanced sample of soldiers who arrived to their first assignment before January 2018.[43] Our empirical strategy uses variation in the fraction of peers who are married within cells defined by the interaction of pre-arrival marital status, sex, occupation, rank, assignment location, month-year of arrival, and initial term of enlistment.[44] We drop any singleton observations, as well as any cells for which there is no variation in treatment, as these observations do not contribute to identification (Miller, Shenhav, and Grosz 2023). We present summary statistics for each of these groups in columns four and five of Table A10.[45] Columns six and seven of Table A10 present summary statistics for our unmarried and married estimation samples, respectively.

Given the relatively small size of the married sample, and the persistence of marriage over time (especially over the time horizons we examine), we focus on the sample of soldiers who are unmarried upon arrival to their first Army assignment in column six of Table A10. While this sample is similar to the sample of all arrivals in column one, there are some notable differences. In particular, there are relatively few women in the estimation sample (only five percent compared to 14 percent in column one). There are a number of factors that lead women to be under-represented in the estimation sample. First, women are more likely to leave the Army during their first term (column three). Second, since only 14 percent of arrivals are women, a greater proportion of women are dropped as singletons because they do not arrive with another woman in the same occupation, rank, assignment location, month-year of arrival, and initial term of enlistment cell (column four). Another notable difference is that our sample draws heavily from soldiers in direct combat occupations (65 percent compared to 41 percent in column one). This is because soldiers in these occupations are more likely to be assigned to units where our assumption of conditional random assignment will hold.

[43] In column three of Table A10, we present summary statistics for the sample of soldiers who attrit within 24 months of arrival. When we estimate peer effects at longer time horizons (36, 48, or 60 months), we drop any soldiers who we do not observe over the entirety of the relevant time period.

[44] Since marriage persists over time, we only compare outcomes for individuals who had the same marital status upon arrival. This ensures we are not comparing marriage outcomes for individuals who had different marital statuses upon arrival.

[45] Singletons are all individuals who do not have another soldier in the same cell defined by pre-arrival marital status, sex, occupation, rank, assignment location, month-year of arrival, and initial term of enlistment. Individuals with no variation in treatment are individuals who have another soldier in the same cell, but have no within-cell variation in the fraction of peers who are married. The vast majority of these instances are cases where soldiers are assigned to the same company.

A.2. Deriving Estimating Equations

This derivation largely follows from Lieber and Skimmyhorn (2018). A structural model of social effects is given by Equation 2, reproduced below.

$$Y_{ig,t} = \beta_0 + \beta_{\bar{Y}}\bar{Y}_{g-i,t} + \beta_{\bar{Z}}\bar{Z}_{g-i,t-1} + \beta_Z Z_{ig,t-1} + \omega_{g,t} + \upsilon_{i,t} + \epsilon_{ig,t} \tag{A1}$$

Where $Y_{ig,t}$ is the outcome of interest for individual i assigned to company g in month t; $\bar{Y}_{g-i,t}$ is the average outcome for individuals in company g in month t, excluding individual i; $\bar{Z}_{g-i,t-1}$ is a vector of average exogenous characteristics of individuals assigned to group g, excluding individual i; $Z_{ig,t-1}$ is a vector of immutable or exogenous characteristics of individual i; $\omega_{g,t}$ accounts for any group-specific, time-varying factors that affect the outcome such as shared environment or institutional features (i.e., common shocks); $\upsilon_{i,t}$ captures any other unobserved idiosyncratic time-varying factors that are related to the outcome; and $\epsilon_{ig,t}$ is the remaining error term.

Taking the expectation of Equation A1 over individuals in group g, yields:[46]

$$\bar{Y}_{g,t} = \frac{\beta_0}{1-\beta_{\bar{Y}}} + \bar{Z}_{g,t-1}\left(\frac{\beta_{\bar{Z}} + \beta_Z}{1-\beta_{\bar{Y}}}\right) + \left(\frac{1}{1-\beta_{\bar{Y}}}\right)[\omega_{g,t} + \bar{\upsilon}_{g,t}] \tag{A2}$$

Plugging the expression for $\bar{Y}_{g,t}$ back into Equation 2 yields a reduced form equation:

$$Y_{ig,t} = \frac{\beta_0}{1-\beta_{\bar{Y}}} + \left(\frac{\beta_{\bar{Y}}\beta_Z + \beta_{\bar{Z}}}{1-\beta_{\bar{Y}}}\right)\bar{Z}_{g,t-1} + \beta_Z Z_{ig,t-1} + \left(\frac{1}{1-\beta_{\bar{Y}}}\right)\left[\omega_{g,t} + \beta_{\bar{Y}}\bar{\upsilon}_{g,t}\right] + \upsilon_{i,t} + \epsilon_{ig,t} \tag{A3}$$

The only structural parameter that is identified in Equation A3 is β_Z, the effect of an individual's own exogenous characteristics on the outcome. The reduced form parameters are composite parameters that incorporate endogenous ($\beta_{\bar{Y}}$) and exogenous ($\beta_{\bar{Z}}$) social effects and the effect of individual characteristics (β_Z). As pointed out by Lieber and Skimmyhorn (2018), failure to distinguish the coefficient on $\bar{Z}_{g,t-1}$ from zero could indicate that the controls used in estimation do little to explain variation in the outcome of interest, rather than the lack of a social effect. To address this possibility, we employ their method to relate all exogenous group characteristics to the group's past choice.

First, note that there will be serial correlation in $\bar{Z}_{g,t}$ due to the fact that many of the individuals in a group remain the same across periods. In our context, individuals stay in a company for approximately three years with about a third of the company rotating out each year. We capture this serial correlation in the following theoretical regression equation:

[46]We assume that the number of individuals in peer group g tends to infinity so that individual i's contribution to the average group outcome is negligible ($\bar{Y}_{g-i,t} = \bar{Y}_{g,t}$).

$$\bar{Z}_{g,t-1}\left(\frac{\beta_{\bar{Y}}\beta_Z + \beta_{\bar{Z}}}{1-\beta_{\bar{Y}}}\right) = \mu_0 + \mu_1 \bar{Z}_{g,t-2}\left(\frac{\beta_{\bar{Z}} + \beta_Z}{1-\beta_{\bar{Y}}}\right) + \xi_{g,t-1} \tag{A4}$$

Now solving the $t-1$ version of Equation A2 for $\bar{Z}_{g,t-2}\left(\frac{\beta_{\bar{Z}}+\beta_Z}{1-\beta_{\bar{Y}}}\right)$:

$$\bar{Z}_{g,t-2}\left(\frac{\beta_{\bar{Z}} + \beta_Z}{1-\beta_{\bar{Y}}}\right) = \bar{Y}_{g,t-1} - \frac{\beta_0}{1-\beta_{\bar{Y}}} - \left(\frac{1}{1-\beta_{\bar{Y}}}\right)(\omega_{g,t-1} - \bar{\upsilon}_{g,t-1}) \tag{A5}$$

And plugging this expression into Equation A4:

$$\bar{Z}_{g-i,t-1}\left(\frac{\beta_{\bar{Y}}\beta_Z + \beta_{\bar{Z}}}{1-\beta_{\bar{Y}}}\right) = \mu_0 + \mu_1\left[\bar{Y}_{g,t-1} - \frac{\beta_0}{1-\beta_{\bar{Y}}} - \left(\frac{1}{1-\beta_{\bar{Y}}}\right)[\omega_{g,t-1} - \bar{\upsilon}_{g,t-1}]\right] + \xi_{g,t-1} \tag{A6}$$

Finally, we substitute the right-hand side of Equation A6 into the reduced form equation (Equation A3):

$$\begin{aligned} Y_{ig,t} = {} & \frac{\beta_0}{1-\beta_{\bar{Y}}} + \mu_0 \\ & + \mu_1\left[\bar{Y}_{g,t-1} - \frac{\beta_0}{1-\beta_{\bar{Y}}} - \left(\frac{1}{1-\beta_{\bar{Y}}}\right)(\omega_{g,t-1} - \bar{\upsilon}_{g,t-1})\right] + \xi_{g,t-1} + \beta_Z Z_{ig,t-1} \\ & + \left(\frac{1}{1-\beta_{\bar{Y}}}\right)\left[\omega_{g,t} + \beta_{\bar{Y}}\bar{\upsilon}_{g,t}\right] + \upsilon_{i,t} + \epsilon_{ig,t} \end{aligned} \tag{A7}$$

Which reduces to:

$$Y_{ig,t} = \pi_0 + \pi_1 \bar{Y}_{g,t-1} + \pi_2 Z_{ig,t-1} + \zeta_{ig,t} \tag{A8}$$

Where $\pi_0 = \frac{\beta_0}{1-\beta_{\bar{Y}}}(1-\mu_1) + \mu_0$, $\pi_1 = \mu_1$, $\pi_2 = \beta_Z$, and $\zeta_{ig,t} = \left(\frac{1}{1-\beta_{\bar{Y}}}\right)(\omega_{g,t} - \mu_1\omega_{g,t-1}) + \left(\frac{1}{1-\beta_{\bar{Y}}}\right)(\beta_{\bar{Y}}\bar{\upsilon}_{g,t} - \mu_1\bar{\upsilon}_{g,t-1}) + \upsilon_{i,t} + \xi_{g,t-1} + \epsilon_{ig,t}$. This expression now describes the outcome for soldier i with respect to past peer group choices, exogenous individual characteristics, and a remaining error term.

In general, estimates of π_1 will be biased due to the presence of the previous period's common shock, $\omega_{g,t-1}$, in the error term. If μ_1 is positive, which is reasonable given that many individuals in the peer group remain the same from month to month, then the direction of the bias depends on the signs of $Cov(\bar{Y}_{g,t-1}, \omega_{g,t-1})$ and $Cov(Y_{ig,t}, -\omega_{g,t-1})$. The sign of the first term is likely positive and the second term negative, suggesting that estimation of Equation A8 will lead to downward biased estimates of π_1.

A.3. Addressing Selective Attrition

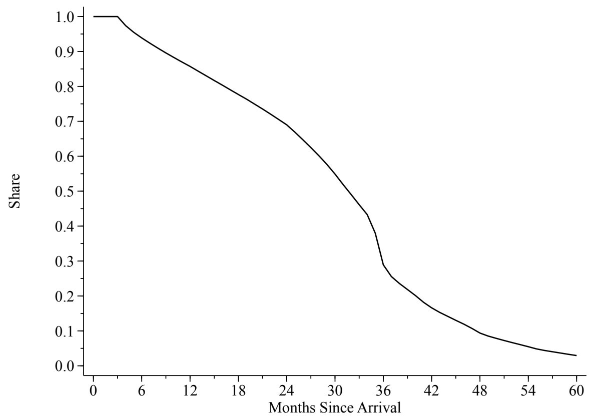
In our main specifications, we limited our sample to individuals who remain in the Army for at least 24 months after assignment. If our treatment has a direct effect on the probability that soldiers remain in the Army, it could be that the peer effect we estimate is affected by selective attrition out of sample and is not capturing the effect of peers' marriage decisions. To limit the scope for attrition to bias our estimates, we measure outcomes at 24 months, a time in which a large majority of soldiers are still in their initial enlistment contract. As such, remaining in the main estimation sample does not require a positive decision to stay in the Army beyond a soldier's initial obligation.

To examine attrition further, we first estimate Equation 3 by 2SLS on a sample that includes individuals who attrit within 24 months of arriving to their first Army assignment. The estimate in column one of Table A11 indicates that there is no statistical or economically meaningful relationship between the treatment and attrition.[47] We also estimate the effect of the treatment on two other downstream outcomes: the probability of completing the first term of enlistment and re-enlisting. Here we find some evidence that more married peers increases first term completion (column two) and re-enlistment (column three). The effect on first term completion is small and marginally significant, while the effect of a 6.5 p.p. increase in the share of married peers on re-enlistment is 1.7 percent and statistically significant.

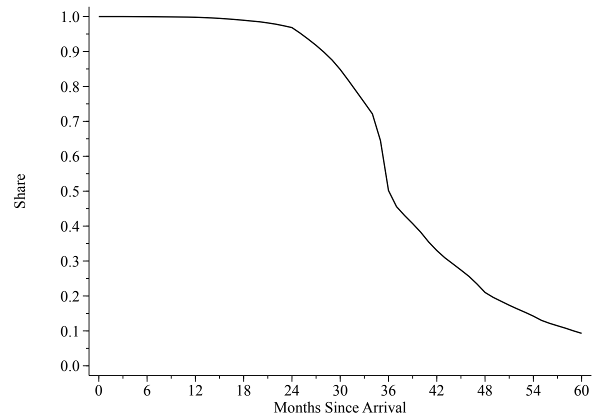
We next show that including individuals who leave the Army within two years of arrival in our sample does not substantially change any of our main results. To include individuals who leave the Army before 24 months, we estimate Equation 3 by 2SLS on the unmarried sample where the dependent variable is an indicator for being married at 24 months or at an individual's last observation, whichever is earlier. The results in Table A12 are qualitatively similar to our main results in Table 3.

[47]The magnitude of the estimate suggests that a 6.5 p.p. increase in the share of married peers increases the likelihood of remaining in the sample at 24 months by 0.17 p.p. and is not statistically significant. We plot these estimates by month in Figure A9.

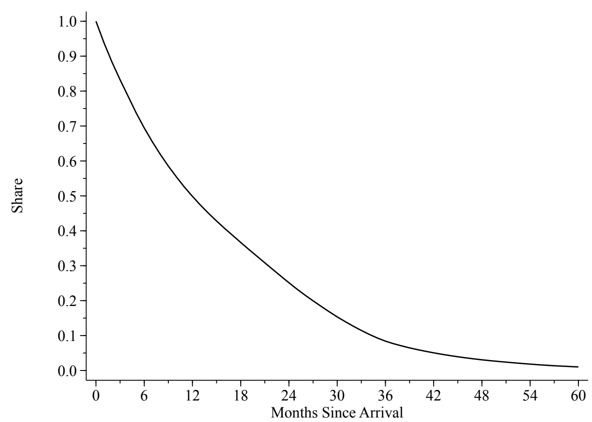
Figure A1. Duration of Exposure to Peers



(a) Share of Individuals In Original Company



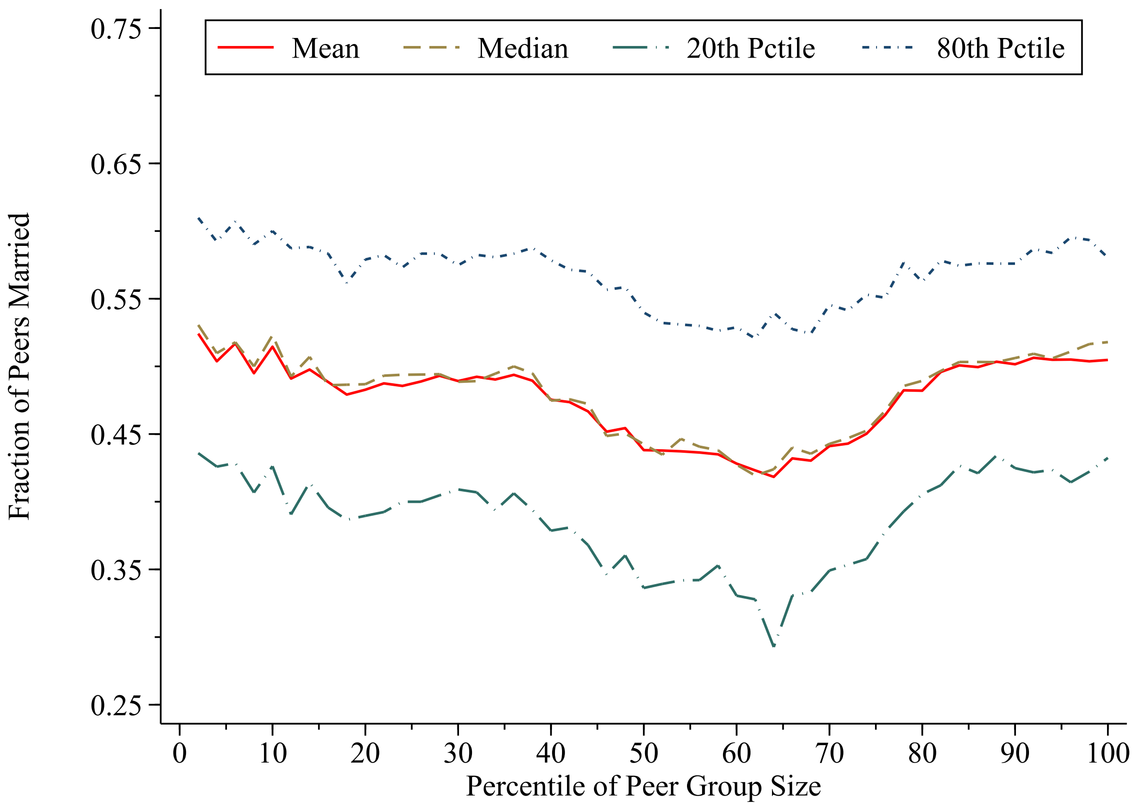
(b) Share of Individuals At Original Location



(c) Share of Original Peers In Company

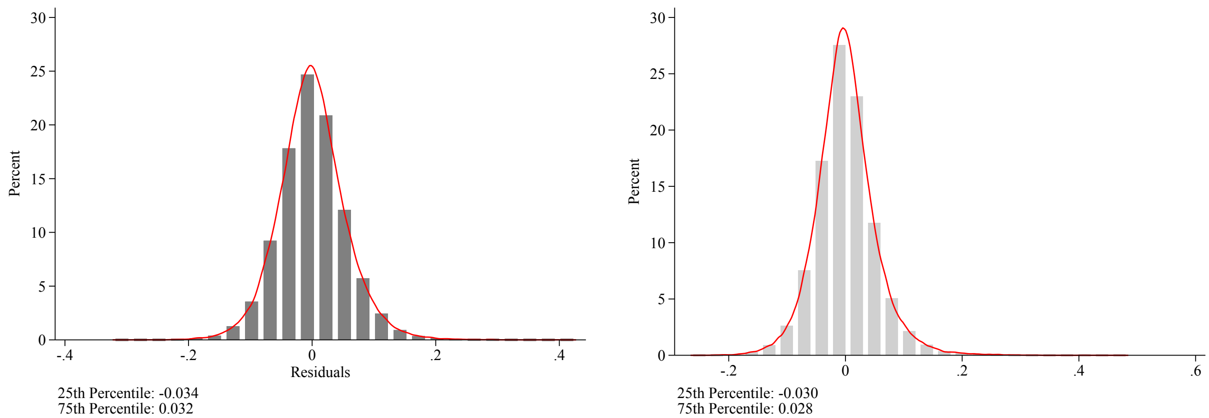
Notes: Panel A plots the share of individuals who are still assigned to their original Army company (peer group). Panel B plots the fraction of individuals who are still assigned to their original duty location. Panel C plots the share of an individual's original peers (i.e., those individuals who are included in the treatment) who are still in the original company. Time is measured in months relative to the focal individual's arrival.

Figure A2. Relationship Between Peer Group Size and Fraction of Peers Married



Notes: For each percentile of peer group size, we calculate four moments of the distribution of our primary treatment variable: the fraction of the peer group that is married. This figure plots those four moments over the the distribution of peer group size.

Figure A3. Residual Treatment and Instrument Densities for the Sample of Unmarried Soldiers

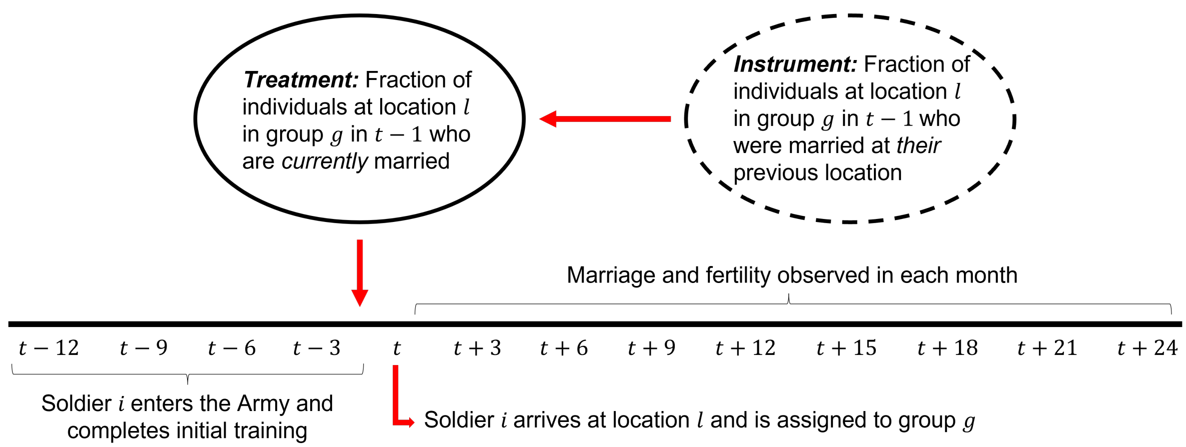


(a) Treatment: Fraction of Peers Currently Married

(b) Instrument: Fraction of Peers Married at *Their* Previous Location

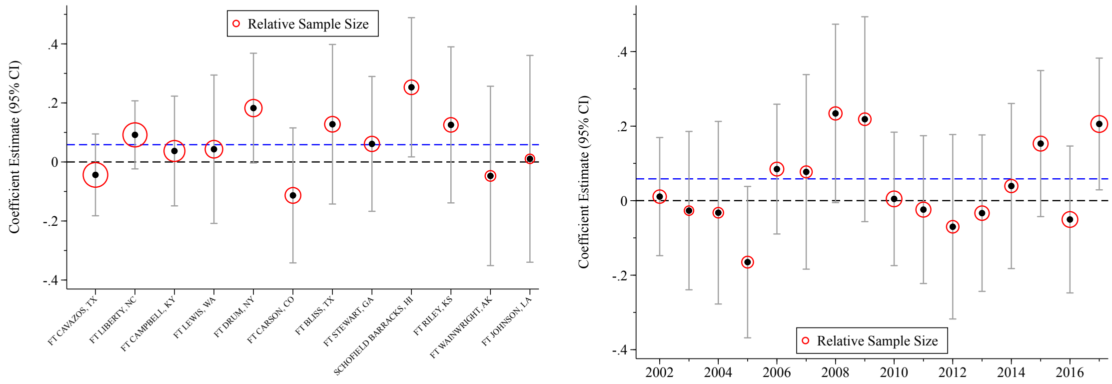
Notes: This figure plots the histograms of the residual treatment (Panel A) and instrument (Panel B). We obtained the residuals by regressing each variable on our interacted fixed effects for sex, job, rank, month-year of arrival, location, and initial term of enlistment. Sample means and standard deviations are displayed below each figure. The red lines are kernel density estimates using an Epanechnikov kernel and Silverman's rule of thumb bandwidth. The raw treatment and instrument densities are in Figure 2.

Figure A4. Treatment and Instrument Definitions.



Notes: This figure depicts how the treatment and instrument were constructed relative to the timing of an individual's assignment. We condition the sample such that we only compare outcomes between soldiers who had the same marital status upon arrival to their first unit.

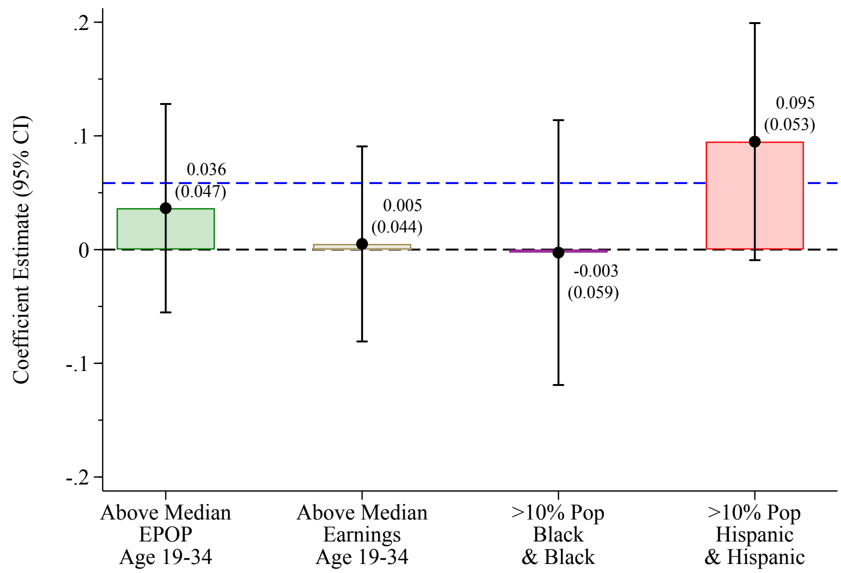
Figure A5. Heterogeneity by Location and Across Time



(a) By Location (b) By Year

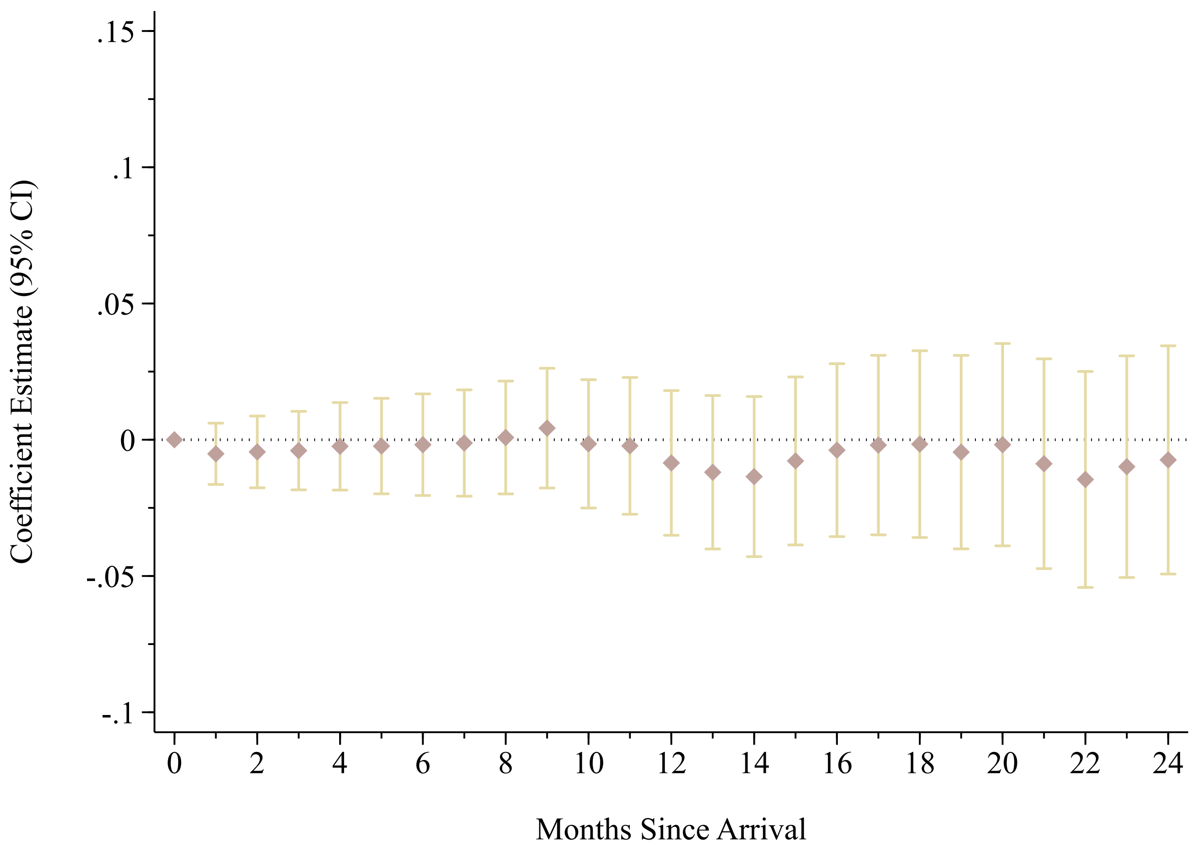
Notes: This figure shows coefficient estimates of Equation 3 with 95 percent confidence intervals for the sample of individuals who were unmarried on arrival to their first assignment. In Panel A each coefficient is from a separate regression estimated only on the location indicated on the horizontal axis. In Panel B, each coefficient is from a separate regression estimated only on the sample of individuals who arrived to their first duty station in the indicated year. In both panels, the red circles indicate the relative number of observations in the full sample that are used to estimate each coefficient. The dashed blue line is the main estimate from Table 3, col. 6.

Figure A6. Heterogeneity by County Location Characteristics



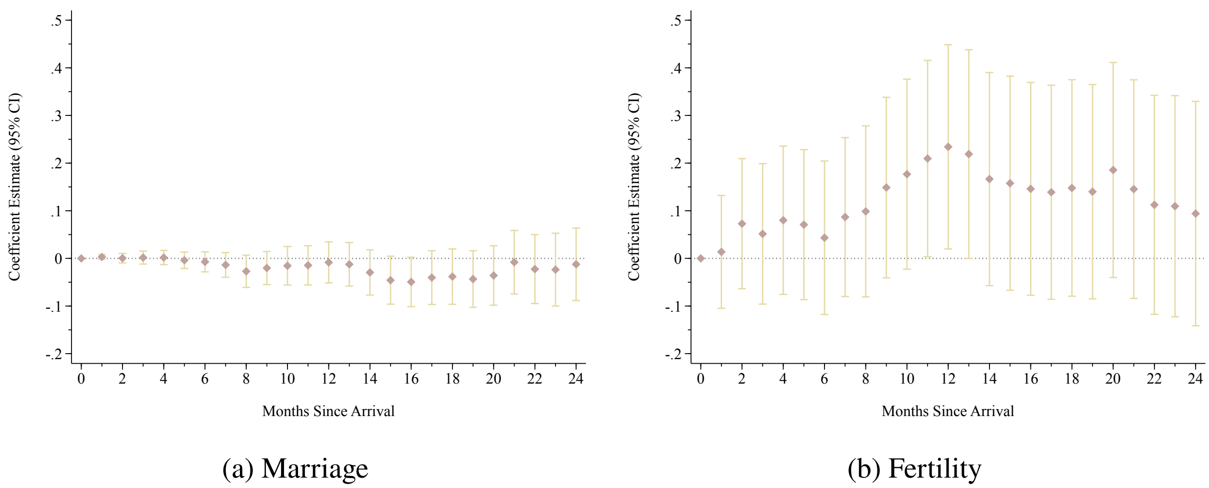
Notes: Each estimate comes from a separate regression. Our first two characteristics from left are based on the labor market prospects for individuals age 19-34 in the county of assignment. In each case we limit the sample to locations that meet the indicated criteria, where the median is defined in our sample (not relative to all U.S. counties) and report the coefficient estimates of Equation 3. The two estimates on the right limit the sample to locations where greater than 10 percent of the population is Black or Hispanic, respectively. The reported estimate is the combined coefficient of the main effect plus the interaction term of Black and Hispanic soldiers, respectively. Next to each coefficient we report the point estimate and the associated standard error in parentheses. The dependent variable is an indicator for individual i being married 24 months after assignment to the peer group. The dashed blue line is the main estimate from Table 3, col. 6.

Figure A7. Peer Effects on Fertility - Unmarried Sample (2SLS)



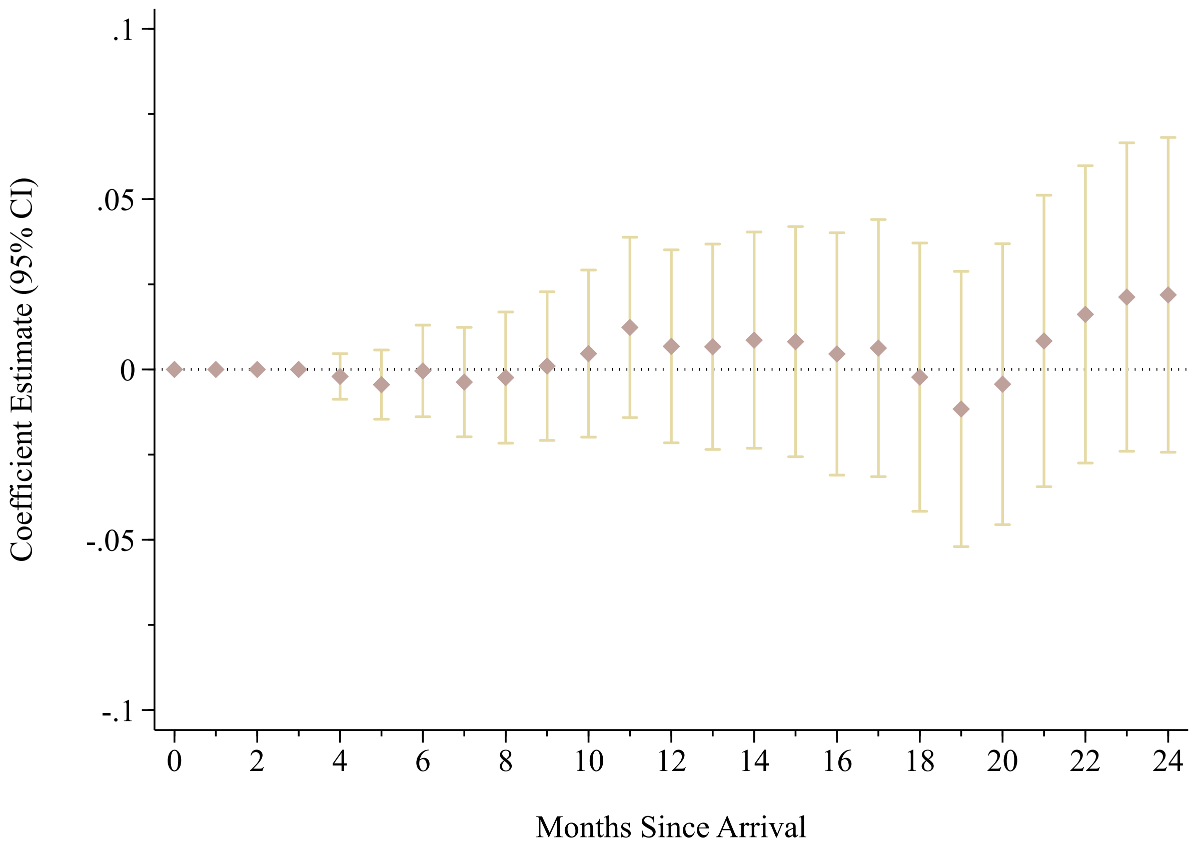
Notes: This figure displays 2SLS coefficient estimates and 95-percent confidence intervals from Equation 3 by month relative to arrival (month zero) for the sample of soldiers who were unmarried upon arrival. The dependent variable in each month is an indicator for an individual having had a child since arrival.

Figure A8. Peer Effects on Marriage and Fertility — Married Sample (2SLS)



Notes: This figure displays 2SLS coefficient estimates and 95-percent confidence intervals from Equation 3 by month relative to arrival (month zero) for the sample of soldiers who were married upon arrival. In Panel A, the dependent variable is an indicator for an individual being married. In Panel B, the dependent variable is an indicator for an individual having had a child since arrival.

Figure A9. Peer Effects on Remaining in the Army by Month Since Arrival (2SLS)



Notes: This figure displays 2SLS coefficient estimates and 95-percent confidence intervals from Equation 3 by month relative to arrival (month zero). The sample differs from our primary samples in that we do not require individuals to remain in the Army for 24 months after arrival to their first assignment. The dependent variable is an indicator for being in the sample in each month.

Table A1. Covariate Balance Across Quartiles of Residualized Treatment.

	$\bar{X}_{q1}$ (1)	$\bar{X}_{q2}$ (2)	$\bar{X}_{q3}$ (3)	$\bar{X}_{q4}$ (4)	$\hat{\Delta}_{q4,q3}$ (5)	$t_{q4=q3}$ (6)	$\hat{\Delta}_{q3,q2}$ (7)	$t_{q3=q2}$ (8)	$\hat{\Delta}_{q2,q1}$ (9)	$t_{q2=q1}$ (10)	$\hat{\Delta}_{q4,q1}$ (11)	$t_{q4=q1}$ (12)
White	0.650	0.652	0.648	0.646	-0.004	-0.570	-0.010	-1.310	0.000	0.640	-0.010	-1.240
Black	0.147	0.149	0.149	0.151	0.005	0.710	0.000	-0.050	0.010	0.870	0.010	1.530
Hispanic	0.149	0.146	0.150	0.148	-0.006	-0.830	0.010	1.450	-0.010	-1.180	0.000	-0.560
Asian	0.024	0.022	0.022	0.025	0.018	2.450**	0.000	0.150	-0.010	-1.690*	0.010	0.910
Nat. Amer.	0.010	0.010	0.011	0.010	-0.005	-0.670	0.010	0.780	0.010	0.690	0.010	0.800
Pac. Isl.	0.021	0.021	0.021	0.021	0.001	0.110	0.000	0.190	0.000	-0.080	0.000	0.210
Age	20.742	20.730	20.737	20.739	0.001	0.120	0.000	0.360	0.000	-0.640	0.000	-0.150
< High School	0.107	0.101	0.104	0.102	-0.004	-0.490	0.010	1.090	-0.020	-2.410**	-0.010	-1.810*
High School	0.841	0.847	0.847	0.847	-0.002	-0.200	0.000	0.010	0.020	2.410**	0.020	2.220**
SMC/ASC	0.044	0.043	0.040	0.042	0.009	1.140	-0.010	-1.970**	0.000	-0.410	-0.010	-1.230
BA+	0.009	0.009	0.009	0.009	0.000	-0.040	0.000	0.610	0.000	-0.610	0.000	-0.040
AFQT Perc.	55.712	55.733	55.630	55.705	0.004	0.540	-0.010	-0.750	0.000	0.160	0.000	-0.050
Training Time	6.259	6.247	6.267	6.245	-0.014	-1.850*	0.010	1.660*	-0.010	-0.950	-0.010	-1.150
Home State	0.057	0.056	0.060	0.057	-0.012	-1.620	0.020	2.530**	-0.010	-0.880	0.000	0.030
Home Region	0.349	0.344	0.350	0.347	-0.006	-0.800	0.010	1.670*	-0.010	-1.430	0.000	-0.550
Fr. Peers Married	0.409	0.456	0.495	0.548								

Notes: This table displays the mean differences for select covariates across the distribution of residual treatment for the unmarried sample. To isolate the identifying variation, we first regress the treatment on our randomization controls (θ_r) and obtain the residuals. We then calculate quartiles using the residual treatment. The first four columns show the sample means for each covariate within the quartile indicated. Subsequent columns then test for mean differences in each covariate across quartiles of residual treatment. To assess the differences across treatment quartiles, we report the normalized difference, $\hat{\Delta}$ and the t-statistic. The normalized difference for any sub-groups of the data h and j is defined as: $\hat{\Delta}_{hj} = \frac{\bar{X}_h - \bar{X}_j}{\sqrt{(s_h^2 + s_j^2)/2}}$, where s_k^2 and N_k are the sample variance for covariate X within sub-group k and the number of observations in sub-group k, respectively. The t-statistic is calculated as: $t = \frac{\bar{X}_h - \bar{X}_j}{\sqrt{(s_h^2/N_h + s_j^2/N_j)}}$. We indicate statistical significance with the reported t-statistics: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. $N_{total} = 140,914$, $N_{q1} = 35,229$, $N_{q2} = 35,228$, $N_{q3} = 35,229$, $N_{q4} = 35,228$.

Table A2. Robustness of Main Results to Different Levels of Clustering.

	Robust (1)	θ_r (2)	Company (3)	Location By Arrival Month (4)	Location (5)
Fraction of Peers Married, $t-1$	0.059	0.059	0.059	0.059	0.059
	(0.028)**	(0.029)**	(0.029)**	(0.029)**	(0.029)*
Dep. Var. Mean	0.198	0.198	0.198	0.198	0.198

Notes: This table presents 2SLS estimates of Equation 3 for the sample of individuals who were unmarried on arrival to their first assignment where the method of calculating standard errors is varied. In col. 1, we report heteroskedasticity-robust standard errors. In all other col.'s standard errors are clustered at the level indicated by the col. heading. The dependent variable is an indicator for individual i being married 24 months after assignment to the peer group. Interacted fixed effects (θ_r) and individual controls are included in all regressions. Standard errors are reported beneath each coefficient (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Table A3. Peer Effects on Marriage — Married Sample

	OLS		Reduced Form	First Stage	2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Fraction of Peers Married, $t-1$	-0.015	-0.020			-0.009	-0.012
	(0.025)	(0.026)			(0.038)	(0.039)
Fraction of Peers Married at Previous Location, $t-1$ (IV)			-0.009	0.691		
			(0.027)	(0.011)***		
Observations	14,657	14,657	14,657	14,657	14,657	14,657
Clusters	2,280	2,280	2,280	2,280	2,280	2,280
R-squared	0.382	0.383	0.383	0.889	-	-
Individual Controls	-	✓	✓	✓	-	✓
Dep. Var. Mean	0.975	0.975	0.975	0.500	0.975	0.975
Effect of Moving up IQR (%)	-0.095	-0.121	-0.053		-0.053	-0.077
F-Stat				256.5		
F-Stat p-value				$< .01$		
Endog. Test p-value					0.799	0.795

Notes: This table presents estimates of Equation 3 for the sample of individuals who were married on arrival to their first assignment in col.'s 1-3, 5, and 6. Col. 4 shows estimates from the first-stage regression, Equation 5. In col.'s 1-3, 5, and 6, the dependent variable is an indicator for individual i being married 24 months after assignment to the peer group. In col. 4 the dependent variable is the fraction of individuals in a company who were married in the month before soldier i arrived. Interacted fixed effects for sex, occupation, rank, assignment location, month-year of arrival, and initial term of enlistment, θ_r, are included in all regressions. Standard errors, clustered at the company level, are reported beneath each coefficient (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). In col.'s 5 and 6, we report the p-value from a Hausman endogeneity test where the null hypothesis is the the treatment is exogenous. To ensure that we use in-sample variation to interpret our results, we report a re-scaled coefficient. This is calculated by multiplying each coefficient by the interquartile range (IQR) in residual treatment variation and dividing by the dependent variable mean. For brevity, this statistic is labeled "Effect of Moving up IQR (%)." The IQR for the treatment (row 1) is 0.060 and the instrument (row 2) is 0.058.

Table A4. Peer Group Differences Across the Distribution of Treatment

	Treatment Quartile				Raw Diff.	Cond. Diff.
	1st	2nd	3rd	4th	(4)-(1)	(4)-(1)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Demographic Characteristics						
Share Female	0.01	0.04	0.08	0.13	0.11	0.04
	(0.05)	(0.09)	(0.11)	(0.12)	(0.00)***	(0.00)***
Share White	0.72	0.66	0.61	0.57	-0.15	-0.06
	(0.10)	(0.12)	(0.13)	(0.13)	(0.00)***	(0.00)***
Share Black	0.11	0.15	0.19	0.24	0.13	0.06
	(0.07)	(0.10)	(0.11)	(0.12)	(0.00)***	(0.00)***
Share Hispanic	0.13	0.14	0.14	0.14	0.01	-0.00
	(0.04)	(0.05)	(0.05)	(0.05)	(0.00)***	(0.00)*
Age	24.80	25.60	26.43	27.86	3.07	2.24
	(0.76)	(0.90)	(1.10)	(1.55)	(0.01)***	(0.02)***
AFQT Percentile	58.83	56.83	56.07	55.27	-3.56	0.00
	(4.87)	(4.85)	(5.01)	(5.35)	(0.07)***	(0.06)
High School	0.78	0.78	0.76	0.73	-0.05	-0.05
	(0.06)	(0.07)	(0.07)	(0.07)	(0.00)***	(0.00)***
Assoc./Some College	0.07	0.08	0.10	0.13	0.06	0.04
	(0.03)	(0.04)	(0.04)	(0.06)	(0.00)***	(0.00)***
BA or Higher	0.03	0.03	0.03	0.04	0.01	0.01
	(0.02)	(0.02)	(0.03)	(0.03)	(0.00)***	(0.00)***
Home State	0.06	0.08	0.09	0.09	0.03	0.01
	(0.05)	(0.07)	(0.07)	(0.07)	(0.00)***	(0.00)***
Panel B: Unit Characteristics						
Peer Group Size	114.05	114.24	115.39	110.82	-3.23	0.96
	(31.07)	(35.59)	(39.31)	(40.77)	(0.44)***	(0.62)
Combat Arms	0.84	0.72	0.56	0.37	-0.47	-0.16
	(0.30)	(0.39)	(0.42)	(0.40)	(0.01)***	(0.00)***
Junior Enlisted	0.68	0.66	0.63	0.58	-0.10	-0.11
	(0.07)	(0.07)	(0.07)	(0.10)	(0.00)***	(0.00)***
First-Line Supervisors	0.28	0.30	0.32	0.34	0.06	0.07
	(0.07)	(0.06)	(0.06)	(0.07)	(0.00)***	(0.00)***
Second-Line Supervisors	0.04	0.04	0.06	0.08	0.04	0.05
	(0.02)	(0.03)	(0.04)	(0.07)	(0.00)***	(0.00)***
Share First Term	0.63	0.56	0.51	0.43	-0.20	-0.15
	(0.08)	(0.08)	(0.09)	(0.10)	(0.00)***	(0.00)***
Months of Service	42.18	48.98	55.00	65.49	23.32	15.16
	(7.09)	(7.68)	(9.01)	(13.77)	(0.14)***	(0.17)***
Panel C: Religious Characteristics						
Athiest/None	0.23	0.22	0.22	0.21	-0.02	-0.01
	(0.05)	(0.05)	(0.05)	(0.05)	(0.00)***	(0.00)***
Catholic	0.19	0.18	0.18	0.17	-0.02	0.00
	(0.05)	(0.04)	(0.04)	(0.04)	(0.00)***	(0.00)
Mormon	0.01	0.01	0.01	0.01	-0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)***	(0.00)
Protestant	0.46	0.48	0.50	0.51	0.05	0.01
	(0.06)	(0.07)	(0.07)	(0.07)	(0.00)***	(0.00)***
Other	0.10	0.10	0.09	0.09	-0.02	-0.01
	(0.04)	(0.04)	(0.04)	(0.04)	(0.00)***	(0.00)***
Fr. Peers Married	0.33	0.45	0.52	0.62	-	-

Notes: This table reports the mean and standard deviation for select variables across the distribution of the treatment (i.e., the fraction of married peers) in col.'s 1-4 (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). Col. 5 is the raw difference between col. 4 and col. 1., estimated from a regression of each variable on indicators for treatment quartiles. Col. 6 is again the difference between col. 4 and col. 1, but conditions on our randomization fixed effects. Standard errors are clustered at the level of randomization in col.'s 5 and 6.

Table A5. Heterogeneity by Age and Education

	Baseline (1)	By Age (2)	By Education (3)
Treatment	0.059	0.046	0.053
	(0.029)**	(0.030)	(0.030)*
Treatment x 2nd Quartile of Age		-0.010	
		(0.008)	
Treatment x 3rd Quartile of Age		0.020	
		(0.013)	
Treatment x 4th Quartile of Age		0.038	
		(0.021)*	
Treatment x Less Than HSG			0.054
			(0.053)
Treatment x More Than HSG			-0.005
			(0.066)
Less Than HSG (combined)			0.107
			(0.056)*
More Than HSG (combined)			0.048
			(0.071)
Observations	140,914	140,914	140,914
Clusters	3,047	3,047	3,047

Notes: This table presents 2SLS estimates of Equation 3. The treatment is the fraction of peers who were married in the month prior to arrival. The dependent variable is an indicator for being married 24 months after assignment to the peer group. In col.'s 2-3, we estimate interacted specifications by interacting the primary treatment and instrument with indicators for quartiles of age and indicators for education, respectively. In col. 3, we present the total effect by education group by combining the main effect with the interaction terms. All columns include the full sample of soldiers who were unmarried upon arrival to their first assignment. Interacted fixed effects for sex, occupation, rank, assignment location, month-year of arrival, and initial term of enlistment, θ_r, and individual controls are included in all regressions. Standard errors, clustered at the company level, are reported beneath each coefficient (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). The 25th, 50th, and 75th percentiles of age are 19.083, 19.917, and 21.583, respectively.

Table A6. Heterogeneity by Peer Group Size and Proximity to Home State

	Baseline	By Peer Group Size	By Proximity to Home State
	(1)	(2)	(3)
Treatment	0.059	0.021	0.047
	(0.029)**	(0.043)	(0.031)
Treatment x 2nd Quartile of Size		0.021	
		(0.050)	
Treatment x 3rd Quartile of Size		0.085	
		(0.048)*	
Treatment x 4th Quartile of Size		0.051	
		(0.052)	
Treatment x Home Census Region			0.030
			(0.035)
Treatment x Adj. to Home State			0.000
			(0.049)
Treatment x Home State			0.069
			(0.064)
Observations	140,914	140,914	140,914
Clusters	3,047	3,047	3,047

Notes: This table presents 2SLS estimates of Equation 3. The treatment is the fraction of peers who were married in the month prior to arrival. The dependent variable is an indicator for being married 24 months after assignment to the peer group. In col.'s 2-3, we estimate interacted specifications by interacting the primary treatment and instrument with indicators for quartiles of peer group size and indicators for proximity to home state, respectively. In col. 3, we categorize individuals into four mutually exclusive categories: those stationed in their home state, those stationed in a state adjacent to their home state, those stationed in their home census region who are not in the first two categories, and those not stationed in their home census region. Interacted fixed effects for sex, occupation, rank, assignment location, month-year of arrival, and initial term of enlistment, θ_r, and individual controls are included in all regressions. Standard errors, clustered at the company level, are reported beneath each coefficient (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). The 25th, 50th, and 75th percentiles of peer group size are 83, 114.5, and 138, respectively.

Table A7. Longer Run Peer Effects on Marriage - Unmarried Sample

	Two Years		Three Years		Four Years		Five Years	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fraction of Peers Married, $t-1$	0.054	0.059	0.064	0.041	0.068	0.042	0.066	0.003
	(0.020)***	(0.029)**	(0.031)**	(0.044)	(0.043)	(0.059)	(0.049)	(0.069)
Observations	140,914	140,914	85,159	85,159	55,407	55,407	38,824	38,824
Clusters	3,047	3,047	2,943	2,943	2,783	2,783	2,673	2,673
R-squared	0.263	-	0.308	-	0.322	-	0.341	-
Dep. Var. Mean	0.198	0.198	0.356	0.356	0.496	0.496	0.597	0.597
Effect of Moving up IQR (%)	1.790	1.927	1.111	0.719	0.818	0.496	0.645	0.030

Notes: This table presents estimates of Equation 3 for the sample of individuals who were unmarried on arrival to their first assignment. The column headings indicate the time horizon for the dependent variable as well as the estimation method (OLS or 2SLS). The dependent variable is an indicator for individual i being married at the end of the time horizon indicated. Interacted fixed effects for sex, occupation, rank, assignment location, month-year of arrival, and initial term of enlistment, θ_r, and individual-level controls are included in all regressions. Standard errors, clustered at the company level, are reported beneath each coefficient (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$).

Table A8. Peer Effects on Fertility

	OLS		Reduced Form	First Stage	2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Fraction of Peers Married, $t-1$	0.017	0.007			0.007	-0.007
	(0.015)	(0.015)			(0.021)	(0.021)
Fraction of Peers Married			-0.005	0.737		
at Previous Location, $t-1$ (IV)			(0.016)	(0.007)***		
Observations	140,914	140,914	140,914	140,914	140,914	140,914
Clusters	3,047	3,047	3,047	3,047	3,047	3,047
R-squared	0.242	0.253	0.253	0.878	-	-
Individual Controls	-	✓	✓	✓	-	✓
Dep. Var. Mean	0.107	0.107	0.107	0.477	0.107	0.107
Effect of Moving up IQR (%)	1.037	0.454	-0.332		0.428	-0.450
F-Stat				641.2		
F-Stat p-value				$< .01$		
Endog. Test p-value					0.514	0.330

Notes: This table presents estimates of Equation 3 for the sample of individuals who were unmarried on arrival to their first assignment in col.'s 1-3, 5, and 6. Col. 4 shows estimates from the first-stage regression, Equation 5. In col.'s 1-3, 5, and 6, the dependent variable is an indicator for individual i having had any children within 24 months of assignment to the peer group. In col. 4 the dependent variable is the fraction of individuals in a company who were married in the month before soldier i arrived. Interacted fixed effects for sex, occupation, rank, assignment location, month-year of arrival, and initial term of enlistment, θ_r, are included in all regressions. Standard errors, clustered at the company level, are reported beneath each coefficient (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). In col.'s 5 and 6, we report the p-value from a Hausman endogeneity test where the null hypothesis is the the treatment is exogenous. To ensure that we use in-sample variation to interpret our results, we report a re-scaled coefficient. This is calculated by multiplying each coefficient by the interquartile range (IQR) in residual treatment variation and dividing by the dependent variable mean. For brevity, this statistic is labeled "Effect of Moving up IQR (%)." The IQR for the treatment (row 1) is 0.065 and the instrument (row 2) is 0.058.

Table A9. Peer Effects on Fertility — Married Sample

	OLS		Reduced Form	First Stage	2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Fraction of Peers Married, $t-1$	0.049	0.051			0.103	0.094
	(0.081)	(0.081)			(0.119)	(0.120)
Fraction of Peers Married at Previous Location, $t-1$ (IV)			0.065	0.691		
			(0.083)	(0.011)***		
Observations	14,657	14,657	14,657	14,657	14,657	14,657
Clusters	2,280	2,280	2,280	2,280	2,280	2,280
R-squared	0.370	0.381	0.381	0.889	-	-
Individual Controls	-	✓	✓	✓	-	✓
Dep. Var. Mean	0.422	0.422	0.422	0.500	0.422	0.422
Effect of Moving up IQR (%)	0.706	0.731	0.927		1.475	1.341
F-Stat				256.5		
F-Stat p-value				$< .01$		
Endog. Test p-value					0.534	0.626

Notes: This table presents estimates of Equation 3 for the sample of individuals who were married on arrival to their first assignment in col.'s 1-3, 5, and 6. Col. 4 shows estimates from the first-stage regression, Equation 5. In col.'s 1-3, 5, and 6, the dependent variable is an indicator for individual i having had any children within 24 months of assignment to the peer group. In col. 4 the dependent variable is the fraction of individuals in a company who were married in the month before soldier i arrived. Interacted fixed effects for sex, occupation, rank, assignment location, month-year of arrival, and initial term of enlistment, θ_r, are included in all regressions. Standard errors, clustered at the company level, are reported beneath each coefficient (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). In col.'s 5 and 6, we report the p-value from a Hausman endogeneity test where the null hypothesis is the the treatment is exogenous. To ensure that we use in-sample variation to interpret our results, we report a re-scaled coefficient. This is calculated by multiplying each coefficient by the interquartile range (IQR) in residual treatment variation and dividing by the dependent variable mean. For brevity, this statistic is labeled "Effect of Moving up IQR (%)." The IQR for the treatment (row 1) is 0.060 and the instrument (row 2) is 0.058.

Table A10. Summary Statistics for Select Sub-Samples

	Valid Arrivals (1)	Sample Restrictions (2)	Attrition ($t+24$) (3)	Singletons (4)	No Variation (5)	Unmarried Sample (6)	Married Sample (7)
Panel A. Soldier Characteristics							
Female	0.14	0.12	0.18	0.20	0.10	0.05	0.04
White	0.61	0.61	0.62	0.57	0.64	0.65	0.62
Black	0.19	0.18	0.22	0.22	0.17	0.15	0.14
Hispanic	0.14	0.15	0.12	0.15	0.14	0.15	0.19
Age	21.64	21.51	21.25	22.34	21.27	20.74	23.61
	(3.27)	(3.18)	(2.99)	(3.67)	(2.92)	(2.45)	(3.74)
AFQT Percentile	58.05	56.34	54.06	58.51	55.77	55.70	54.22
	(18.80)	(18.54)	(17.41)	(19.01)	(18.50)	(18.40)	(18.19)
Less Than High School	0.11	0.11	0.15	0.08	0.11	0.10	0.18
High School	0.79	0.80	0.77	0.76	0.83	0.85	0.72
Some College/Associate's	0.06	0.06	0.05	0.08	0.05	0.04	0.08
Bachelor's or Higher	0.04	0.03	0.02	0.07	0.01	0.01	0.02
Married	0.19	0.18	0.17	0.30	0.14	0.00	1.00
Has Children	0.13	0.12	0.13	0.19	0.10	0.03	0.53
Panel B. Service Information							
Direct Combat Occ.	0.41	0.50	0.49	0.33	0.41	0.65	0.57
3-Year Initial Contract	0.49	0.57	0.59	0.42	0.49	0.67	0.72
4-Year Initial Contract	0.33	0.29	0.28	0.37	0.29	0.24	0.21
5-Year Initial Contract	0.09	0.08	0.08	0.10	0.14	0.07	0.04
6-Year Initial Contract	0.08	0.06	0.05	0.10	0.08	0.02	0.02
Months of Service	7.25	6.71	6.65	7.33	6.77	6.25	6.32
	(2.30)	(1.97)	(1.96)	(2.25)	(1.88)	(1.61)	(1.60)
Completes First Term	0.79	0.78	0.04	0.89	0.90	0.93	0.93
Re-enlists	0.38	0.36	0.01	0.45	0.43	0.39	0.53
Observations	751,204	340,235	50,365	114,108	19,246	140,914	14,657

Notes: This table reports the mean and standard deviation for select variables for various sub-samples of the data. Standard deviations are only reported for continuous variables. All variables are measured from the month an individual arrived to their first operational Army unit. Col. 1 includes all individuals who arrived to their first operational unit from Oct. 2001 through Jan. 2018, who arrived within three to 18 months of entry, and who were not missing information critical to the analysis (job, date of birth, sex, race, education, or AFQT percentile score). Col. 2 includes those individuals who meet our additional sample restrictions described in detail in Section A.1. Col.'s 3-7 divide the population in col. 2 into five distinct groups. Col. 3 is made up of individuals who leave the Army within 24 months of arriving at their first operational Army unit. Col. 4 consists of individuals who are unmatched in that they did not arrive to their first assignment location in the same month-year as another soldier of the same marital status, sex, occupation, rank, and initial term of enlistment. Col. 5 consists of individuals who are matched, but who have no within-match variation in treatment. This occurs when two matched soldiers are assigned to the same peer group, for example. Col.'s 6 and 7 are the estimation sample on which we estimate the effect of peers, broken out by marital status on arrival.

Table A11. Peer Effects on Other Outcomes

	In Sample $t+24$ (1)	Completed First Term (2)	Re-Enlisted (3)
Fraction of Peers Married, $t-1$	0.022	0.028	0.100
	(0.024)	(0.017)*	(0.034)***
Observations	170,041	140,914	140,914
Clusters	3,099	3,047	3,047
Dep. Var. Mean	0.860	0.927	0.393
Effect of Moving up IQR (%)	0.167	0.196	1.663

Notes: This table presents 2SLS estimates of Equation 3 for the sample of individuals who were unmarried on arrival to their first assignment. The column headings indicate the dependent variable. In col. 1, the sample includes individuals who exit the Army within 24 months. In col.'s 2 and 3 the sample is the main estimation sample used in Table 3. Interacted fixed effects for sex, occupation, rank, assignment location, month-year of arrival, and initial term of enlistment, θ_r, and individual controls are included in all regressions, but we do not include controls for deployment in col. 1 because deployment is undefined for individuals who attrit. Standard errors clustered at the peer group level are reported beneath each coefficient (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). To ensure that we use in-sample variation to interpret our results, we report a re-scaled coefficient. This is calculated by multiplying each coefficient by the interquartile range (IQR) in residual treatment variation and dividing by the dependent variable mean. The IQR for the treatment is 0.065. For brevity, this statistic is labeled "Effect of Moving up IQR."

Table A12. Peer Effects on Marriage - Including Individuals Who Attrit

	OLS		Reduced Form	First Stage	2SLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Fraction of Peers Married, $t-1$	0.050	0.052			0.066	0.068
	$(0.018)^{***}$	$(0.018)^{***}$			$(0.026)^{***}$	$(0.025)^{***}$
Fraction of Peers Married			0.050	0.739		
at Previous Location, $t-1$ (IV)			$(0.019)^{***}$	$(0.007)^{***}$		
Observations	170,041	170,041	170,041	170,041	170,041	170,041
Clusters	3,099	3,099	3,099	3,099	3,099	3,099
R-squared	0.248	0.254	0.254	0.876	-	-
Individual Controls	-	✓	✓	✓	-	✓
Dep. Var. Mean	0.187	0.187	0.187	0.479	0.187	0.187
Effect of Moving up IQR (%)	1.755	1.827	1.748		2.312	2.366
F-Stat				870.9		
F-Stat p-value				$< .01$		
Endog. Test p-value					0.390	0.403

Notes: This table replicates Table 3 on a sample that includes individuals who leave the Army within 24 months of arrival to their first duty location. Estimates of Equation 3 for the sample of individuals who were unmarried on arrival to their first assignment are presented in col.'s 1-3, 5, and 6. Col. 4 shows estimates from the first-stage regression, Equation 5. In col.'s 1-3, 5, and 6, the dependent variable is an indicator for individual i being married 24 months after assignment to the peer group. In col. 4 the dependent variable is the fraction of individuals in a company who were married in the month before soldier i arrived. Interacted fixed effects for sex, occupation, rank, assignment location, month-year of arrival, and initial term of enlistment, θ_r, are included in all regressions. Standard errors, clustered at the company level, are reported beneath each coefficient (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). In col.'s 5 and 6, we report the p-value from a Hausman endogeneity test where the null hypothesis is the the treatment is exogenous. To ensure that we use in-sample variation to interpret our results, we report a re-scaled coefficient. This is calculated by multiplying each coefficient by the interquartile range (IQR) in residual treatment variation and dividing by the dependent variable mean. The IQR for the treatment (row 1) is 0.065 and the instrument (row 2) is 0.058. For brevity, this statistic is labeled "Effect of Moving up IQR (%)." We do not include controls for deployment in these specifications because deployment is undefined for individuals who attrit.