Peer Effects and Marriage Formation*

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Abstract

A large literature links marriage to later life outcomes for children and adults. As U.S. marriage rates have declined markedly in recent decades, particularly for those with less education, this connection raises concerns that declining marriage might exacerbate inequality in outcomes like children's achievement and adult longevity. In this paper, we provide causal evidence on one mechanism for marriage rate patterns: peer effects. We use exogenous assignment to peer groups for young adults in the U.S. Army to identify the impact of peer marriage on individual marriage decisions. A six percentage point increase in peer marriage rates - equivalent to the interquartile range of our identifying variation - increases the probability that an unmarried individual marries within 24 months by 0.7 percentage points (3.4 percent). These precisely estimated impacts are robust to a range of alternate specifications and show little evidence of heterogeneity across locations or by location characteristics. Our peer effect estimates remain positive for at least 36 months after assignment, providing evidence that peers do more than shift the timing of marriage. We note heterogeneity in the pattern of results by race in that the effect of peers is larger and more persistent for Black soldiers. We provide some evidence that the effect of peers is transmitted through role-modeling and social norms rather than network effects.

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1. Introduction

One of the most notable demographic shifts in the U.S. over the last half century has been the decline in marriage rates (Ruggles, 2015; Goldin, 2021). 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 (Figure 1). Marriage rates have steadily declined for this group in each of the last five decades. 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 (Carpenter et al., 2021; Robles and Kiecolt-Glaser, 2003; Kiecolt-Glaser and Wilson, 2017; Ginther and Zavodny, 2001; Antonovics and Town, 2004; Altonji et al., 2021; Calvo, 2022; Moynihan, 1965; Becker, 1993; Borella et al., 2018; 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 (Lundberg et al., 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). Among U.S. families in 2019, poverty rates varied substantially with family structure. Overall, the poverty rate was 8.5 percent, but married households had a poverty rate of 4.6 percent, while single-parent households had a poverty rate of 24.3 percent (female head) and 11.3 percent (male head) (Semega et al., 2020). The results are even more stark for families with children under age 6. The 2019 poverty rates for these sub-groups were: 6.3 percent (married), 45.7 percent (single-parent, female head), and 18.4 percent (single-parent, male head).

Identifying the reasons for declining marriage rates is critical to understanding its 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 et al., 2018; Autor et al., 2021; Shenhav, 2021), availability of contraceptives (Goldin and Katz, 2002), abortion access (Miller, Wherry, et al., forthcoming), changing norms and sexual values (Akerlof et al., 1996, Bertrand et al., 2015, Kearney and Wilson, 2018), changes in the availability of divorce (Gruber, 2004; Stevenson and Wolfers, 2007; Cunningham and Goodman-Bacon, 2020), 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 marriages 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 who entered the U.S. Army (hereafter, Army) from October 2001 through September 2019. We first show that assignment to peer groups for this sample of enlisted soldiers is as-good-as random once we condition on observable factors that affect the assignment decision. This conditionally random assignment

[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).

eliminates the potential for selection into peer groups, a major hurdle to credibly estimating peer effects in many contexts.

Since we observe the universe of enlisted soldiers on a monthly basis, we can measure peer group characteristics prior to an individual's assignment. We define the peer group to be the soldiers that are in a unit the month prior to a soldier's arrival. For each soldier that arrives at their first duty station, we then define the treatment to be the fraction of peers that are married in the month prior to their 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 a third and final common confound in peer effects estimation: common shocks to the peer group. We construct an instrument for peer marriage using peers' marriage rates at their previous assignment location. This allows us to control for potential unobservable environmental factors (e.g., upcoming deployment) which could affect both peer marriage rates as well as individual marriage rates. Since the assignment of enlisted soldiers to locations is conditionally random, this instrument is unrelated to the environment or institutional factors at the current location. We can then interpret our estimates as the causal effect of an individual's peer group on his or her subsequent marriage decisions.

Despite the specialized Army context, our empirical setting has several features that make it of broad interest. Ninety four percent of the sample on which we estimate peer effects has a high school degree or less, the age group that has experienced the sharpest decline in marriage rates in the U.S. population. 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. Also, Black and Hispanic men are well-represented in our sample, allowing us to precisely estimate separate impacts for these groups.

We find that a six percentage point increase in peers' marriage rates — an increase equivalent to the interquartile range in the identifying variation we exploint in share of peers married — increases the likelihood that an unmarried individual is married 24 months after arrival at a new location by 0.7 percentage points, or 3.4 percent

($p < 0.01$). 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 six percentage point increase in the fraction of an Black soldier's initial peer group who are married increases the likelihood of marriage at 36 months by 3.2 percentage points, or 7 percent ($p = 0.02$).

In addition to heterogeneity in the persistence and magnitude of our effects by race, we find suggestive evidence that peers have a greater influence over marriage decisions for women, although we have too few women in our sample to make strong claims in this area. We find little evidence that the effect of peers varies across locations or with location characteristics such as the size of the "marriageable" population, suggesting that peers influence marriage decisions in a variety of contexts.

To understand which peers matter most, we create more granular measures of an individual's peer group using the rank structure within the Army, identifying four distinct sub-groups of potential peers. Our results indicate that the effect of peers is driven by other junior enlisted soldiers and first-line supervisors. Individuals 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 on junior enlisted soldiers only, suggesting that non-linear complimentarities in marriage rates throughout the peer group may be important for our results.

Regarding mechanisms, our findings that close peers and first-line supervisors but not more senior supervisors influence marriage decisions provides suggestive evidence that our results are most consistent with peers affecting individual decisions by serving as role-models or establishing norms within the workplace environment, rather than a network effect. We do not find evidence that the results are driven by either increases in information about marriage benefits or access to more potential spouses, which would be more consistent with a network effect. Finally, we benchmark our estimates against previous findings on determinants of young adult marriage. Using back of the envelope calculations, peer effects can explain one-tenth to one-fourth of the effect of Army service on short-run marriage estimated in Greenberg et al. (2022).

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 the results; 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 own-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

[2]These difficulties were outlined in Manski (1993). Manski's model has subsequently been developed in Sacerdote (2001), Zimmerman (2003), Lyle (2007), Bramoullé et al. (2009), Goldsmith-Pinkham and Imbens (2013), Angrist (2014), and 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 specifically; 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 et al. (2014), Balbo and Barban (2014), Pink et al. (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 alcohol consumption and cell phone purchasing (Hinnosaar and Liu, 2022; Bailey et al., 2022)

Demonstration Program run by the U.S. Department of Housing and Urban Development (HUD) in the mid-1990s.[4] Chetty et al. (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 et al., 2001]), raising concerns that the findings from MTO may not generalize. However, other work has found evidence of neighborhood effects on marriage among 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 (Salt Lake, UT) and decreases by up to 0.5 percentage points per year (Nassau, NY) relative to growing up in the average U.S. county.[7] While these findings do not isolate the effect of peers from other place or neighborhood effects, they are consistent with peers exerting influence on own-marriage. 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 non-marital childbearing despite providing similar economic shocks. They hypothesize that the difference in responses is related to differing social norms over time.

An extensive structural literature has worked to develop and estimate marriage market matching functions, and several papers explore the role of peer behavior in

[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 et al., 2007; Ludwig et al., 2013).

[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.

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 (forthcoming) find that there is a negative relationship between cohort size and marriage rates for both men and women. They argue the relationship could be related to changes in match quality or the value of being single, both of which could be influenced by peer effects. Billari et al. (2007) provide a theoretical argument for the importance of peers in marriage decisions. Using data from the National Longitudinal Study of Adolescent to Adult Health (Add Health), Adamopoulou (2012) found that peers exert a positive influence on own marriage probability. In a similar vein, McDermott et al. (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, 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, approximately five hundred thousand service members are 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). During the period of our data (October 2001 - September 2019), more than 1.1 million individuals enlisted in the Army: between 54 and 71 thousand each year. The majority of individuals who joined 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 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 over a year. Due to the relatively short duration of boot camp and AIT, initial entry training constitutes a temporary assignment in the Army, and there are no provisions to relocate spouses to join soldiers on these assignments. Furthermore, soldiers have limited privileges during basic training and AIT, so there is little opportunity for social interactions, to include meeting potential spouses or interacting with the spouses of peers. In our data, 15 percent of enlistees are married at entry. While there is some marriage between entry and arrival at a soldier's first duty station, marriage during this period is relatively rare. Five percent of soldiers who were unmarried at entry are married upon arrival to their first location. Among the marriages that do occur, 82 percent of the marriages take place in the 60 days prior to arrival at a soldier's first assignment location. This indicates that these marriages are most likely converting pre-existing relationships into marriages after basic training rather than that soldiers are forming new relationships while at basic training.

After completing AIT, enlisted soldiers are assigned to one of many Army posts within and outside the continental United States. The Army assigns soldiers to locations based on a soldier's occupation and the requirements of Army units at each location at the time that a soldier completes initial entry training. The assignment of a soldier to an Army unit immediately following completion of AIT is formally the soldier's first permanent posting in the military. We focus our analysis on this initial assignment throughout the paper because soldiers have no ability to influence the assignment decision and the Army has yet to learn any information on which to condition the assignment other than the variables that we observe in our data.

[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.

Once a soldier arrives at a location, he or she is progressively assigned to a brigade (≈5,000 soldiers), battalion (≈700 soldiers), and company (≈100 soldiers). At each level, assignments are primarily made based on occupation, rank, and current staffing needs. The soldier has little ability to exert influence over the assignment process and thus who his or her eventual peer group will be. 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).

The most granular unit that we can identify in our data is the company, and we therefore construct all peer group measures at this level. 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. 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.[9]

4. Data

We use administrative military personnel data on enlisted Army service members obtained from the Office of Economic and Manpower Analysis at West Point, NY. The data is a monthly panel of all active-duty enlisted service members in the Army from October 2001 to September 2020. For each month in the sample window, we observe a snapshot of information for each active-duty Army enlisted member 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,

[9] 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 any Non-Commissioned Officer (NCO). An NCO is an enlisted soldier in the rank of Sergeant or above and has at least three years of service experience.

AFQT percentile score, education level, race and ethnicity, sex, origin state, length of initial enlistment contract, and year of birth.[10]

Vital to our empirical strategy is the conditional random assignment of individuals to peer groups. Within the Army, this assumption is most plausible for soldiers being assigned to their first unit after initial entry training. Starting with the full sample of soldiers who arrived at their first Army post from October 2002 to October 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. We restrict our analysis to soldiers with no previous Army assignment; 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 detailed in Section A.1.

4.1. Summary Statistics

Summary statistics for the full estimation sample are in Panels A and B of 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.

In our data, the vast majority of soldiers arrive at their first location four to nine months after the start of basic training. The 10th percentile, median, and 90th percentiles are 4 months, 5 months, and 8 months, respectively. 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 (69 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.[11]

In each month, we observe an individual's marital status and number of dependent children. Panel C of Table 1 provides summary statistics on marriage and fertility 24 months after arrival at the first duty station. Our main outcome variable, marriage,

[10] 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.

[11] The fraction of soldiers assigned to their home census region is higher than would be expected because more enlistees come from the South than any other census region, and many of the posts in our sample are also located in the South.

is coded as an indicator variable equal to one if an individual is married in a month and zero otherwise. Likewise, our measure of fertility is an indicator for having any dependent children in a month. Upon arrival to their first Army unit, 10 percent of our sample is married and seven percent have children. In column two we see that among soldiers who are unmarried when they arrive at their first location, 19 percent are married after two years.

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. Figure A1 shows what fraction of the sample is still in the original peer group by month in Panel A. At 24 months, 64 percent of individuals are still in their original peer group. Those individuals who are not in their original peer group have almost exclusively moved to another unit at the same location, although a small fraction do move to a new location. Panel B considers turnover from the perspective of the peer group. In this case, only 21.7 percent of the peers included in our main treatment variable are still in the peer group after 24 months. While this is not surprising given our context it is important in considering how our results will generalize to other settings.

4.2. Measuring the Fraction of Married Peers

To construct measures of each individual's peer group, we use the full monthly panel of Army enlisted members. The smallest administrative unit in the Army which can be identified is the company. In each month we calculate the fraction of soldiers in a 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 to that unit.

Information on peer marriage rates is shown in Table 1, Panel D. In our sample the average company has 112 enlisted soldiers and 47 percent of them are married.[12] 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 14.8 percentage points.

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

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 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. 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 30 percent of peers were married at their previous location. The standard deviation is 10 percentage points and the interquartile range is 14.3 percentage points. Figure A3 shows the variation in peer marriage residual of interacted fixed effects for sex, job, rank, month of arrival, location, and initial term of enlistment. The interquartile range in residual variation is six percentage points, which we will use to interpret the magnitude of our coefficients.

4.3. Other Data Sources

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 leverage SEER data because it allows us to construct population measures by single year of age groups, which is not generally possible using intercensal population estimates from the Census Bureau.

5. Empirical Strategy

5.1. A Structural Model of Social Effects

We adapt Manski's general model of social effects to our context by specifying 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:

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

As discussed above, the soldier's company constitutes the peer group in most of our analysis. $\bar{Y}_{g-i,t}$ is the average outcome for individuals in peer group 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 1 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, then $\omega_{g,t}$ 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 instrument for the average peer group outcome using outcomes that were 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 $\omega_{g,t}$.

5.2. Estimating Peer Effects

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:

$$Y_{ig,t+s} = \pi_0 + \pi_1 \bar{Y}_{g,t-1} + \pi_2 Z_{ig,t-1} + \theta_r + \zeta_{ig,t+s} \quad (2)$$

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. 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. We also explore the influence of peer marriage rates on fertility outcomes. $\bar{Y}_{g,t-1}$ is the fraction of individuals who were married in group g in the month before individual i arrived. $Z_{ig,t-1}$ is a vector of individual characteristics, defined in the period prior to arrival to group g. Our controls consist 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 $t + s$. θ_r is an interacted fixed effect 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 $\zeta_{ig,t+s}$. Our coefficient of interest is π_1.

5.3. Conditional Random Assignment to Peer Groups

Our identification strategy relies on the conditional random assignment of individuals to peer groups. This assumption implies that soldiers cannot select into peer groups and that soldiers are not assigned to peer groups based on any other factor than those we observe and on which we condition. In terms of Equation 1, conditional random assignment ensures that unobserved individual characteristics, $v_{i,t}$, are uncorrelated with any characteristics of the peer group. Without conditional random assignment to peer groups, estimates of peer effects will almost certainly be biased by selection.

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 no ability to influence the assignment process. Factors that do influence the assignment decision are a soldier's sex, occupation, rank, and the timing of the assignment.[13] 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 (forthcoming).

$$\bar{Y}_{g,t-1} = \delta_0 + \delta_1 Z_{ig,t-1} + \theta_r + \xi_{g,t-1} \tag{3}$$

Here $\bar{Y}_{g,t-1}$ is defined as the fraction of individuals in peer group g who where married in the month before individual i is assigned. $Z_{ig,t-1}$ and θ_r are defined as before and $\xi_{g,t-1}$ is the remaining error term.

If $\delta_1 \neq 0$ then we would reject our 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 3 on the sample of unmarried individuals are in Table 2. Column one includes only the interacted fixed effects. The assignment fixed effects explain a great deal of variation in the treatment ($R^2 = 0.77$). In columns two to four we progressively add controls. In each case we report the p-value of a joint test of the controls where the null hypothesis is that $\delta_1 = 0$. The coefficient estimates are generally not statistically significant, and in all cases we fail to reject that the coefficients are jointly equal to zero. Furthermore, adding controls does not increase the R^2, suggesting that individual controls do little to explain variation in treatment after we condition on the factors that affect the assignment decision. In column five we include all controls and again fail to reject that the coefficient estimates are jointly equal to zero ($p = 0.62$).

To further explore how individual characteristics vary with our main treatment variables we conduct a balance test across quartiles of residualized treatment in Table A1. While there are some statistically significant differences, there is no clear pattern

[13] 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.

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 0.04.[14] 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

We construct an instrument by measuring the marriage rate of the peers at the peers' previous location to eliminate the potential for common shocks to bias our estimates. With the additional assumption that the assignment of enlisted soldiers to Army posts is conditionally random, the measure of marriage that we use to construct the instrument is not influenced by the environment or institutions at the current location. We estimate Equation 2 by 2-Stage Least Squares (2SLS), where the first stage is given by:

$$\bar{Y}_{g,t-1} = \gamma_0 + \gamma_1 \bar{Y}_{g,prev\ location,t-1} + \gamma_2 Z_{ig,t-1} + \theta_r + \eta_{ig,t-1} \qquad (4)$$

Where $\bar{Y}_{g,prev\ location,t-1}$ is the average outcome for individuals assigned to group g in $t-1$, using marriage observed at their previous location of assignment and all other variables are defined as in Equation 2.

The exclusion restriction is likely to be satisfied because the individuals for whom we estimate peer effects were stationed at basic training locations across the U.S. prior to assignment to their current location, so it is unlikely that their peers' decisions from previous locations have any direct effect on their current choice. The instrument is likely to be relevant due to correlation over time in marriage within individuals. Relevance is identified by the statistical significance and magnitude of γ_1 in the above equation. Finally, we assume the instrument is monotonic in that a higher fraction of peers who were married at their previous location always increases the fraction of peers who are married at the current location.

[14]While the t-statistic indicates 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).

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 2 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. Adding the controls in column two causes a relatively small decrease in the magnitude of the point estimate, consistent with our assumptions about conditional random assignment. To interpret the magnitude of our estimates, we also report the effect of moving up the interquartile range (IQR) in residual treatment (6.3 percentage points). The coefficient of 0.077 in column two implies that a six point increase in the peer married share increases the likelihood of marriage at 24 months by 0.5 percentage points ($p < .01$), or 2.5 percent. 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).

In columns three to six we turn to our IV strategy to address the problem of unobserved common shocks. 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.

Column four shows first-stage estimates of Equation 4. Our instrument is highly relevant with an F-stat of 451.8. The IV estimate in column six implies that moving up the IQR in residual treatment increases the likelihood of marriage at 24 months by 3.4 percent ($p < .01$).[15] Comparing the IV point estimate in column six with the OLS estimate in column two shows that the OLS estimate has a downward bias - the IV estimate is more than 30 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 ($p = 0.23$) suggests that we cannot reject the null hypothesis that the treatment is exogenous. However, given that the differences between the OLS and IV estimates suggest a role for common shocks, we estimate the remaining specifications using our IV strategy.[16]

[15] We also demonstrate that the main IV estimate in column six is robust to clustering at various other levels: the level of randomization, battalion, and post in Panel A of Table A2.

[16] 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

To better understand how the effect of peers evolves over time, we estimate Equation 2 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 6 of Table 3. Estimates are small and statistically insignificant for the first 9 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. Beginning in month 10, there is a positive effect that grows and becomes statistically significant after 16 months. The estimate at 24 months is the same as reported in Table 3. In Section A.3, we show that our results are robust to including individuals who leave the Army within 24 months of assignment. We find no evidence that the fraction of married peers is related to attrition from the Army over this period.

A natural question in our context is whether the effect of peers varies with combat deployments. A large proportion of our sample spends at least some time deployed in the first 24 months after assignment (61 percent). Deployments are of interest for two reasons. First, deployments serve as an example of a common shock that would bias our OLS estimates. We would thus expect that the difference between our OLS and IV estimates would be smaller for peer groups that did not experience a deployment. Second, 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 cannot be exposed to the non-military spouses or families of other soldiers while deployed, which might limit the information that can be transferred in this setting. Similarly, there is an incapacitation effect since soldiers cannot get married while deployed.

We estimate Equation 2 separately for peer groups that deployed and those that did not deploy within 24 months of a soldier's assignment in Table 4. Importantly, we define deployment at the unit, not the individual level. This excludes the possibility of selection into or out of deployment. There is suggestive evidence that the difference in OLS and IV estimates is smaller for units that did not deploy relative to units that did deploy (compare the differences in column one and two to those for column three and four). Comparing the point estimates in columns two and four, the effect of peers

admittedly little scope for peer effects as 97 percent of the sample is still married after 24 months. This result does suggest that marriages rates of peers is not leading to marriage dissolution (i.e., divorce).

is larger in instances where soldiers did not deploy (column 2), but the difference is not statistically significant (interaction term in column six). Even so, the estimate in column two may have more generalizability to other populations and is slightly larger than our main estimate in Table 3: a six percentage point increase in the fraction of peers who are married increases marriage at 24 months by 4.3 percent.

6.2. Other Peer Group Definitions

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.

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 of the rank Private (E-1 and E-2), Private First Class (E-3), and Specialist (E-4). This group includes 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, in fact, 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 have infrequent interactions with junior enlisted soldiers but provide oversight and supervision of junior enlisted soldiers and their supervisors.[17]

Figure 4 presents 2SLS estimates of π_1 in Equation 2 where the peer group is defined by the components of an Army company as described above. The point estimate for close peers is positive, about half the size of the overall peer effect, and

[17] 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.

marginally significant. The effect of first-line supervisors is of a similar magnitude and is statistically significant. However, second-line and senior supervisors do not have a detectable effect on marriage decisions. The point estimates are close to zero in magnitude and far from statistically significant. In Table A4 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 and first-line supervisors being the main driver of our results. Furthermore, defining the peer group as the entire Army company 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, 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.

6.3. Heterogeneity of Marriage Results

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 two-way combinations of race and gender.[18] This enables us to answer the question of whether peers matter more for marriage decisions among certain demographic groups, and all comparisons are within-group. The results are in Figure 5. While we cannot statistically distinguish between any of these coefficients, there are some notable patterns. First, within racial groups, the point estimate for women is larger in each case. The point estimates suggest a six percentage point increase in the fraction of peers who are married increases own-marriage at 24 months by 8.8, 4.8, and 8.9 percent for Black, white, and Hispanic women, respectively. Second, the point estimate for Black men is more than twice the size of the estimate for white men. Third, all of the point estimates are positive, suggesting that our results are not only applicable to white men who make up the majority of our sample. These results suggest that the effect of peers may be even larger in other demographic groups.

[18] There are substantial differences in racial composition by gender in our sample. For example, a little more than two-thirds of men in our sample are white, but only one-third of women are white. For this portion of the analysis, we limit the sample to white, Black, and Hispanic soldiers, which make up 95 percent of our sample.

Next, we look for any evidence of heterogeneity by age upon arrival. 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 16 percent are over 22. As with the analysis for race and sex above, we make all comparisons within age groups to determine whether there are differences in the importance of peers across age groups. The results are in Figure A5. The coefficients are not statistically different across age groups, but the point estimates suggest that soldiers who are less than 20 or older than 22 may be more influenced by peers.

Finally, given the divergence in marriage patterns by education level in the general U.S. population, we estimate the effect of peers for individuals with different levels of education at entry. Specifically, we are able to estimate the effect of peers for individuals with less than a high school education, high school graduates, and individuals with more than a high school education. The results in Figure A6 provide some evidence that peer effects are larger for individuals with less than or more than a high school education. However, since the vast majority of our sample consists of high school graduates, we interpret these results with caution.

6.3.2. Location Characteristics

In our sample of unmarried soldiers, we have 18 locations that receive at least 500 soldiers during the sample period. To look for heterogeneity by location, we estimate the effect of peers at each location separately and plot the point estimates in Figure A7. Among these 18 locations, 13 of the 18 point estimates are positive, and in no case can we rule out that the peer effect is equal to our main estimate from Table 3, column six.

Previous research has provided evidence for assortative mating, for example by education (Eika et al., 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 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.

The results are presented in Figure A8. First, we test for a differential effect of peers in locations with above median employment and earnings for individuals age 19-34. The point estimate in the case where the surrounding county has above median employment prospects is about half the size as 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 does not exclude our main estimate, so are we limited in what we can infer from this single point estimate. The estimate in instances where age 19-34 earnings are above the median is very close to our main estimate, suggesting no differential effect of peers. Next, we consider locations where a relatively higher fraction of the age 19-34 population is the same race as the focal individual. The point estimates are again imprecise, and they are not substantially different from the results by race in Figure 5. 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.

Finally, we look for evidence of any changes in our peer effect estimates over time by estimating Equation 2 separately for individuals who arrived in each year of our sample from 2002-2018. The results are in Figure A9. Thirteen of the 17 point estimates are positive, indicating that our results are not driven by a particular time period. Across all years, we cannot rule out that the point estimate is equal to our main estimate from Table 3, column six.

6.4. Additional Outcomes: Marriage Persistence and Fertility

To assess whether peers affect marriage decisions over a longer time horizon, in Figure 6, Panel A, we estimate Equation 2 by 2SLS by month on a balanced sample of individuals who remain in the Army for at least 36 months. The results indicate that the effect of peers peaks around 24 months but the point estimate remains positive out to 36 months. This could suggest that peers are influencing individuals to marry earlier, and there is a catch-up effect as individuals age. Another explanation could be that the importance of an individual's initial peer group decreases over time as individuals are re-assigned to new peers.

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.[19] In light of this finding, along with our finding that peers appear to be more important for Black soldiers (Figure 5), we estimate the effect of peers on marriage over 36 months separately for Black and white individuals in Panel B and C of Figure 6. The effect of an individual's initial peer group persists over time for Black individuals. A six 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 3.2 percentage points, or 7 percent ($p = 0.02$). In Panel C, the effect of peers for white individuals is very similar to the full sample estimates, peaking between 21 and 24 months and then declining over time, although the point estimates remain positive. The finding that the effect of peers grows over time for Black individuals provides some evidence that our results are not driven exclusively by re-timed marriage.

We now turn to studying the impacts of peer marriage rates on fertility. First, we estimate whether having more married peers increases the likelihood of having any children 24 months after assignment. [20] Table 5 shows estimates of Equation 2 where the dependent variable is an indicator for having children at 24 months. The IV estimate in column six shows a small positive effective of peers on fertility, although the result is statistically insignificant. A six percentage point increase in the fraction of married peers increases the likelihood of having a child 24 months after assignment by 1.8 percent ($p = 0.13$). We also estimate peer effects on fertility by month in Figure 7. The results again show no statistically significant impact on having children, but the effect of peers does grow over time.[21]

Given that our outcome is measured only 24 months after assignment to a peer group, it may be more reasonable to expect to see peer effects on fertility for the sample of individuals who were married upon arrival as the transition into parenthood likely requires less time and planning than for a single individual. In Table 6, we present estimates of Equation 2 for the married sample where the dependent variable is an indicator for having children 24 months after assignment. Here we find a marginally

[19] An important difference between our paper and Greenberg et al. (2022) is that all 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.

[20] We use the marriage rate of peers rather than the fertility rate of peers because they are highly correlated.

[21] We also estimate the effect of peers over a 36-month time horizon in Figure A10. Results using the fraction of peers with children in place of fraction of peers married are nearly identical; the two measures are highly correlated.

significant effect of peers on fertility. A six percentage point increase in the fraction of married peers increases the likelihood of having children by 24 months after assignment by 1.9 percent ($p < 0.1$). We do not view these results as precluding larger peer effects on fertility over longer time horizons than we can observe in our data.[22]

7. Investigating Mechanisms and Magnitudes

We find a positive effect of peer marriage rates on the likelihood of an individual getting married in the next two years. The effect is driven by a combination of various levels of peers within an Army company, though horizontal peers and immediate supervisors appear to be the most important. We also find suggestive evidence that Black individuals and women are more affected by their initial peer group assignment. The effect of initial peers gets smaller over time for white individuals, due either to a fade out effect or the aggregation of effects across multiple peer groups. However, for Black individuals the effect of initial peers persists and is still growing 36 months after assignment. We find limited evidence of heterogeneity by age, location, or location characteristics, suggesting that our positive peer effects apply broadly. In this section we consider the mechanism by which peers influence own-marriage decisions, and attempt to put the magnitude of our estimates in context.

7.1. Mechanisms

Why do peer marriage rates have the observed effects on own-marriage? We consider two channels: a ”pure” peer effect (i.e., imitation) or a network effect. We define a “pure” peer effect as choosing behavior to match that of others when that behavior is more common in the group. This could arise through learning from peers, from direct copying of a behavior, or through a desire to adhere to perceived norms. Alternatively, under a network effect mechanism, married peers may provide information on the benefits of marriage or access to social connections that increase the likelihood of marriage. Having more married peers in one’s group increases the likelihood of obtaining information or connections, and therefore increases the likelihood of marrying. Broadly speaking, in the pure peer effect channel, it is the peers themselves that influence an individual. In the network channel, individuals are connecting to something through peers.

[22] Figure A11 presents estimates of Equation 2 by month for the marriage and fertility outcomes on the sample of individuals who were married upon arrival.

Distinguishing between these two explanations is difficult empirically. However, we believe our setting allows us to explore each mechanism and to provide at least suggestive evidence on their relative importance. As noted earlier, we find that own-marriage behavior is responsive to marriage rates of horizontal peers and first-line supervisors, but impacts of marriage rates in the company overall are largest. This implies that non-linearities are present, with marriage rates among more senior supervisors contributing to the overall company effect (Figure 4). Since supervisors are prohibited from interacting with soldiers outside the workplace environment, 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 pure peer effects contribute to our results.

Next, we try to understand whether incumbent peers are the main driver of our results, or whether newly arriving individuals may also play a role. We view incumbent peers as most likely to set norms or to serve as role models for the group. As we have argued throughout the paper, conditional on our randomization controls, assignment to peer groups is as-good-as-random. Thus, the marital status of arriving peers is conditionally independent of the underlying marriage propensity of the soldiers already in the peer group. In addition to our primary treatment variable, we now add a measure of the fraction of arriving peers that are married, where we instrument for the fraction of arriving peers married upon arrival with the fraction of arriving peers who were married six months prior to arrival. We then estimate the effect of incumbent and arriving peers jointly and separately in Table A5. When we define the treatment as only arriving peers (column two), we find that arriving peers also have a positive and marginally significant effect on marriage within 24 months, but the effect is an order of magnitude smaller than the effect of incumbent peers (column one). This result is consistent with the estimates in column four, which show that the peer effect estimate is largest when we include both incumbent and arriving peers in the treatment measure. However, the estimates in column six where we include incumbent and arriving peers separately demonstrate that incumbent peers have the largest effect on marriage. Soldiers who arrive after the focal soldier have a relatively small and statistically insignificant effect on marriage decisions. We view this pattern of results as more in line with soldiers reacting to social norms present in their peer group near the time of arrival.

Under a network channel, we would expect peer effect to increase in the size of the peer group, since larger peer groups afford more opportunity to access larger networks or information. However, we do not find any statistically significant evidence that the effect of peers is influenced by the size of the peer group (Figure A12). Furthermore, if peers were purely transferring information, we would expect a larger effect of peers in cases where individuals have access to more potential partners to marry after receiving information. Examples include soldiers who are posted in or near their home state and closer to home social networks or soldiers posted to denser marriage markets. We explored heterogeneity in both dimensions in results discussed earlier and found no greater impacts of peers for such soldiers. In fact, for soldiers posted in their home state, the effect of peer marriage was insignificant.

In Figure A13 we explore this null finding further. We estimate Equation 2, where we estimate separate regressions for individuals assigned to their home state, a state adjacent to their home state, or their home census region. We define the groups to be mutually exclusive to see how our estimated peer effect varies with proximity to home state. We do not find evidence that soldiers who are living in their home state are more influenced by peers, and, in fact, the point estimate is negative for soldiers living close to home. However, the peer effect estimate for individuals assigned to a state adjacent to their home state is more than three times larger than our main estimate in Table 3, column six. The point estimate for soldiers assigned to their home region is of a similar magnitude to our main estimate, though statistically insignificant. A potential explanation for this pattern of results is that soldiers assigned to their home states are less likely to engage with their Army peers. Or, perhaps equivalently, these individuals may be part of other, competing peer groups simultaneously. Individuals assigned near their home state are relatively more likely to engage with their Army peers, and they are close enough to their home state to be able to convert pre-existing relationships into marriages. Individuals assigned in their home census region, but not in states adjacent to their home state, may find it harder to convert pre-existing relationships into marriages due to the larger physical distances between their location of assignment and their home. Consequently we would argue that Figure A13 provides evidence more in line with a pure peer effect rather than a network effect.

7.2. Magnitudes

To better understand the magnitude of our results, we benchmark our estimates against prior work. 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 increase marriage by 20.3 percentage points and 17.1 percentage points among Black and white applicants three years after applying, respectively.[23]

How much of the total increase in marriage for Army service members can be explained by peer effects? We do a back of the envelope calculation by first using data from the American Community Survey (ACS, 2022) to calculate the fraction of the age 18-35 population in a soldier's home state who are married at the time of a soldier's arrival to their first unit.[24] This is a rough measure 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. This is an estimate of the change in marriage behavior among peers encountered by a soldier who joins the Army. In our sample, the fraction of married peers is 16.9 percentage points higher in the Army relative to a soldier's home state. Our peer effect estimates imply that joining the Army would increase marriage near the three-year mark from application by 1.9 percentage points, or 9.5 percent.[25] The estimates are somewhat larger for Black service members (5.4 percentage points, 23.1 percent) relative to white service members (1.4 percentage points, 7.6 percent). This would suggest our peer effect estimates can explain a little more than one-quarter of the total effect of Army service on marriage for Black individuals and a little less than one-tenth for white individuals found in Greenberg et al. (2022).

A strand of literature has considered how government health care policies affect 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

[23]We use the three-year estimates as a basis for comparison because it most closely aligns with the timing of our estimates. After applying, applicants may wait for up to a year to ship to basic training and then complete initial entry training prior to arriving at their first unit. The soldiers for whom we estimate peer effects 24 months after arrival to their first unit would have applied 30-48 months before we measure our outcomes.

[24]We use the ACS 5-year estimates from 2009, 2014, and 2019 to calculate the fraction of age 18-35 individuals who are married in each state by year from 2005-2019.

[25]This estimate comes from multiplying our main coefficient estimate on the fraction of married peers by the estimated percentage point change in the fraction of married peers encountered by an individual that joins the Army. The estimate is slightly different from Table 3 because we restrict the sample to individuals arriving post-2004 for these calculations.

Care Act (ACA) in 2010, marriage increased for young adults in states that already had existing dependent coverage by 2-3 percentage points.[26] Our main estimate in Table 3, column six indicates that to generate a short-run increase in marriage on the order of 2 percentage points would require approximately a 19 percentage point increase in the fraction of peers who are married, a change similar to moving from the state with lowest rates of young adult marriage (Rhode Island) to the state with the highest rates of young adult marriage (Utah) in 2019. However, our estimates only take into account the immediate effect of peers, so it is possible that peers effects would be larger over longer time horizons.

8. Discussion

Our empirical setting provides unique features and high quality longitudinal data that allow us to answer new questions about marriage and peers. We estimate peer effects on a population of individuals that has experienced declining marriage rates over the last 50 years: individuals with no education beyond high school. Declining marriage is related to changes in family structure and stability during childhood, which has implications for the long-run outcomes of adults and children. We find consistent evidence that peers influence marriage within two years of an individual's arrival. While the closest set of peers appears to be the most important, first-line supervisors also exert an influence on marriage. The combination of peers at all levels is larger than the sum of each strictly defined peer group individually, suggesting that there is an interaction among overlapping peer groups that affects marriage decisions. We also find some evidence of heterogeneity by race and sex. For white individuals, the effect of peers decreases over time, suggesting either that peers influence the timing of marriage or that individuals are influenced by their current set of peers, which may change over time. However, the evidence is consistent with a persistent effect of peers on the marriage decisions of Black individuals. We also find some suggestive evidence that peers have more influence for women relative to men.

While we are limited in what we can say about the mechanisms that drive our effects, the evidence is most consistent with a direct effect of peers through role-modeling and norms. We do not find evidence that peers are providing greater access

[26] 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.

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 explain a substantial portion of the total effect of Army service on marriage.

The Army context allows use 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 in age, education, and other characteristics. The mean marriage rate in our sample of peers (which includes more senior military members) is 47 percent. There is, however, substantial variation suggesting that marriage is far from universal in the Army.

The effect we find for soldiers in this sample is not necessarily the net peer effect they will have while in the Army. We define a peer group at a point in time and for group of soldiers who they work with 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. 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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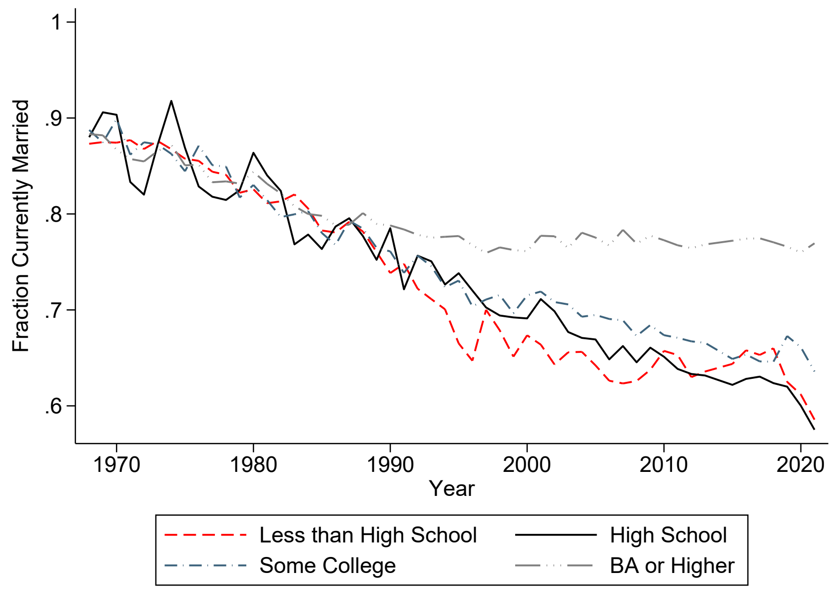
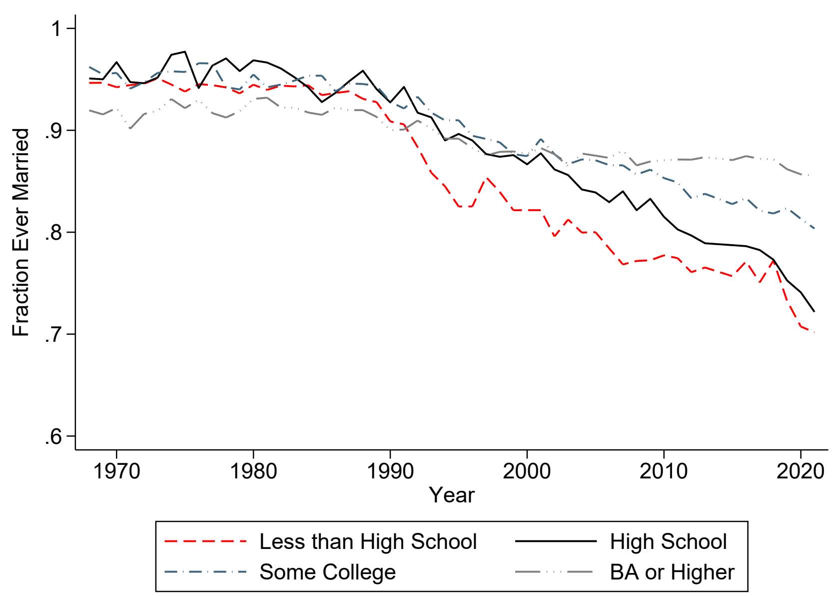
Figure 1. Changes in Marriage Formation, 1968-2021



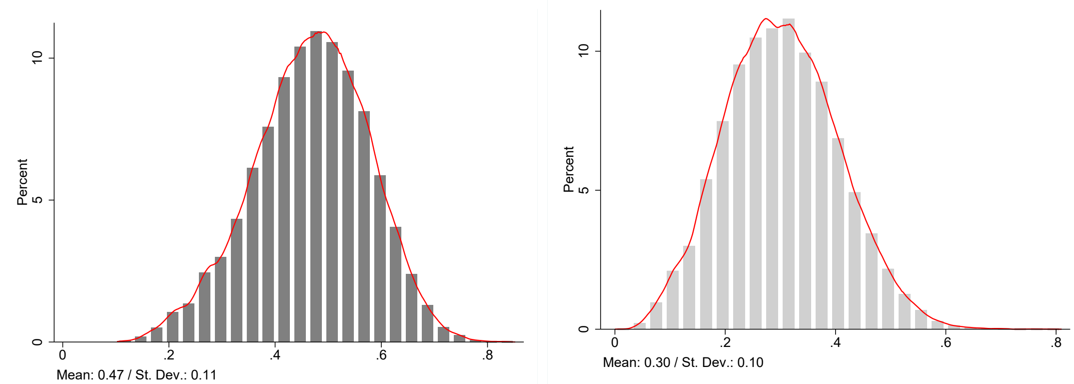
(a) Fraction of 40-45 Year Olds Currently Married



(b) Fraction of 40-45 Year Olds Ever Married

Notes. Figures are constructed using data from the Current Population Survey's Annual Social and Economic Supplement (ASEC, 2021) for years 1968-2021. In both panels, the sample is limited to individuals who are age 40 to 45 in each year. Prior to 1992, the ASEC captured education beyond high school as years of college. We consider anyone with four or more years of college to have achieved a BA to enable a comparison across time. Starting in 1992, education is coded by the degree achieved.

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

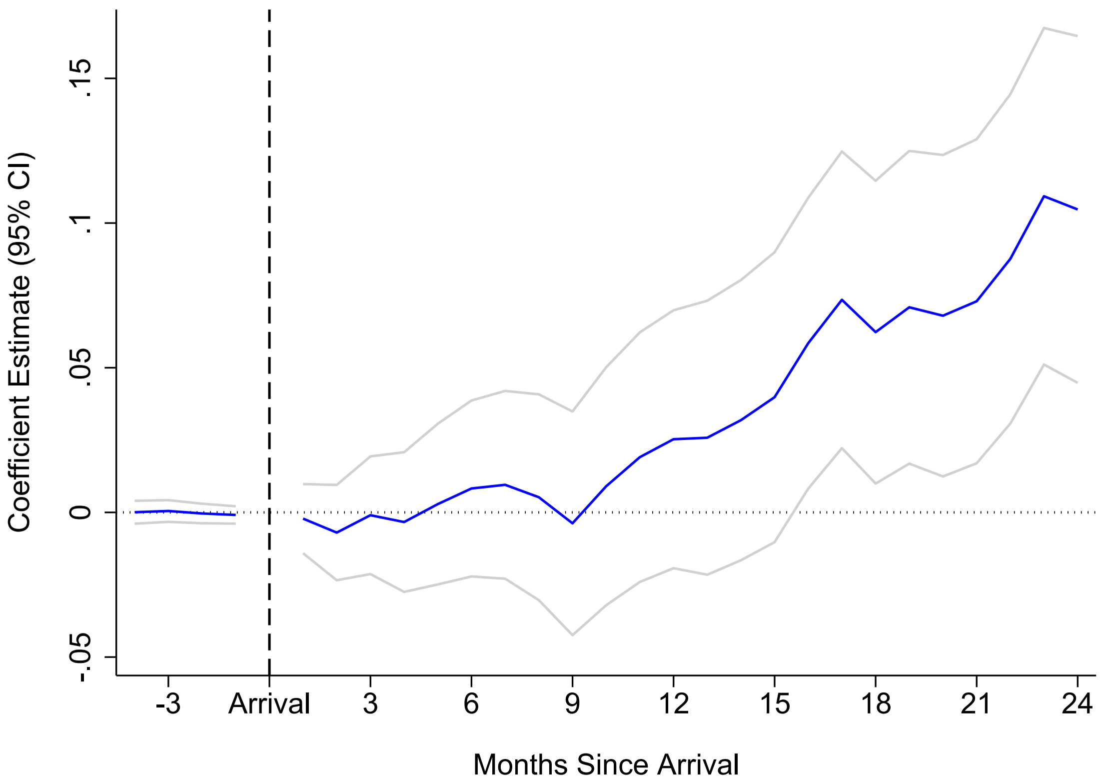


(a) Fraction of Peers Married (Treatment)

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

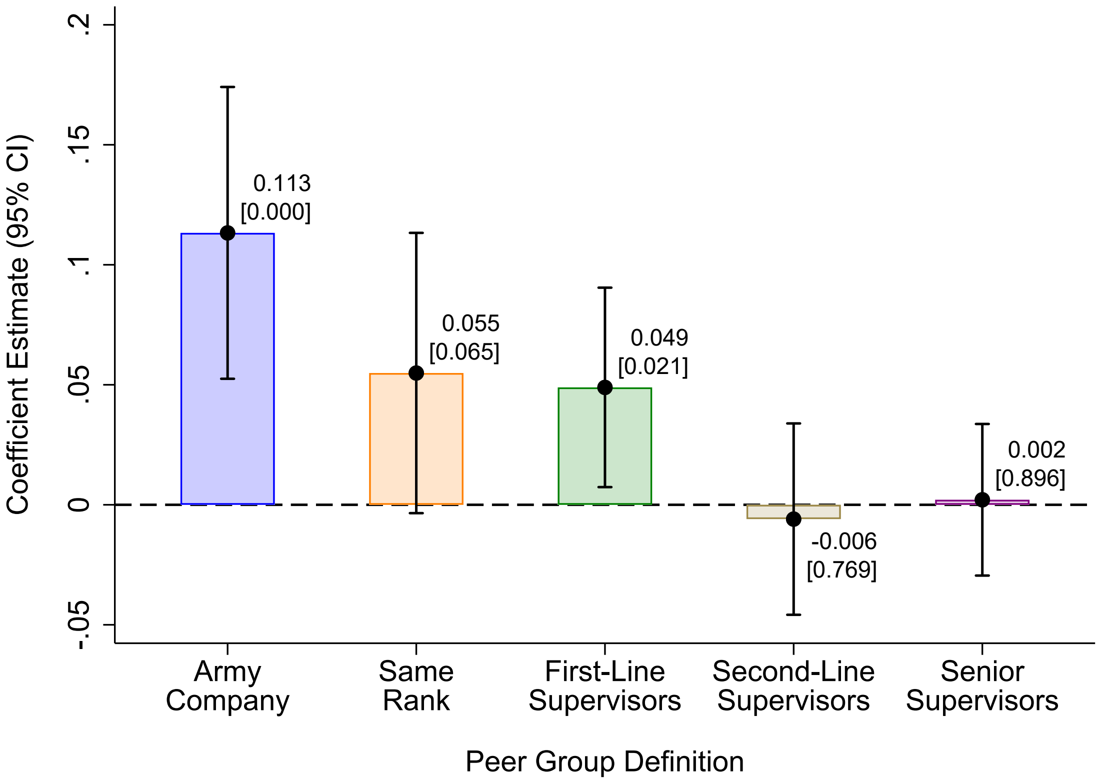
Notes. Panel A is a histogram of our primary treatment variable: the fraction of individuals in the peer group who were married in the month before an individual's arrival. Panel B is a histogram of the instrument. We construct the instrument by measuring marriage at an individual's previous location prior to assignment to the current location. Sample means and standard deviations are displayed below each figure. Each histogram has 100 equally sized bins. The red lines are kernel density estimates using an Epanechnikov kernel and Silverman's rule of thumb bandwidth.

Figure 3. Peer Effects on Marriage by Month Relative to Arrival (IV)



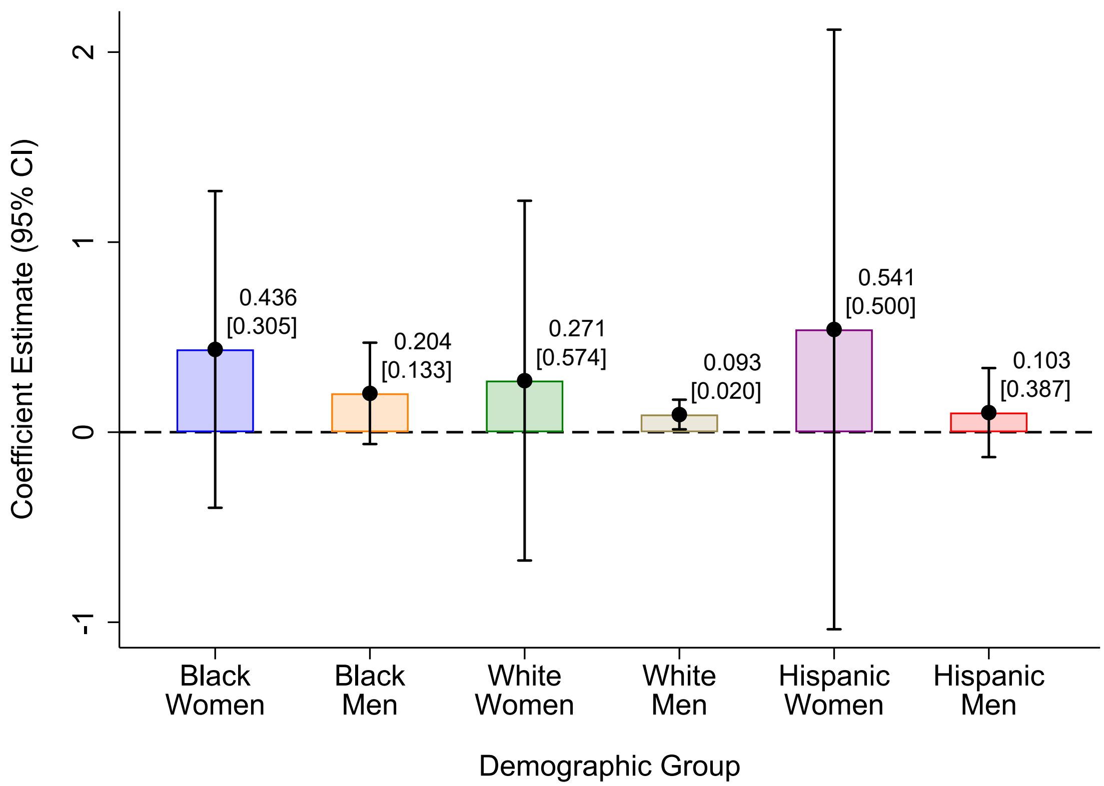
Notes. This figure displays 2SLS estimates of Equation 2 by month, beginning four months prior to an individual's assignment to a peer group. The dependent variable is an indicator for an individual being married in each month. In the month of arrival (month 0), all individuals are unmarried. The blue line reflects the coefficient estimates and 95 percent confidence intervals are in gray.

Figure 4. Defining Peer Groups Based on Proximity of Daily Interactions



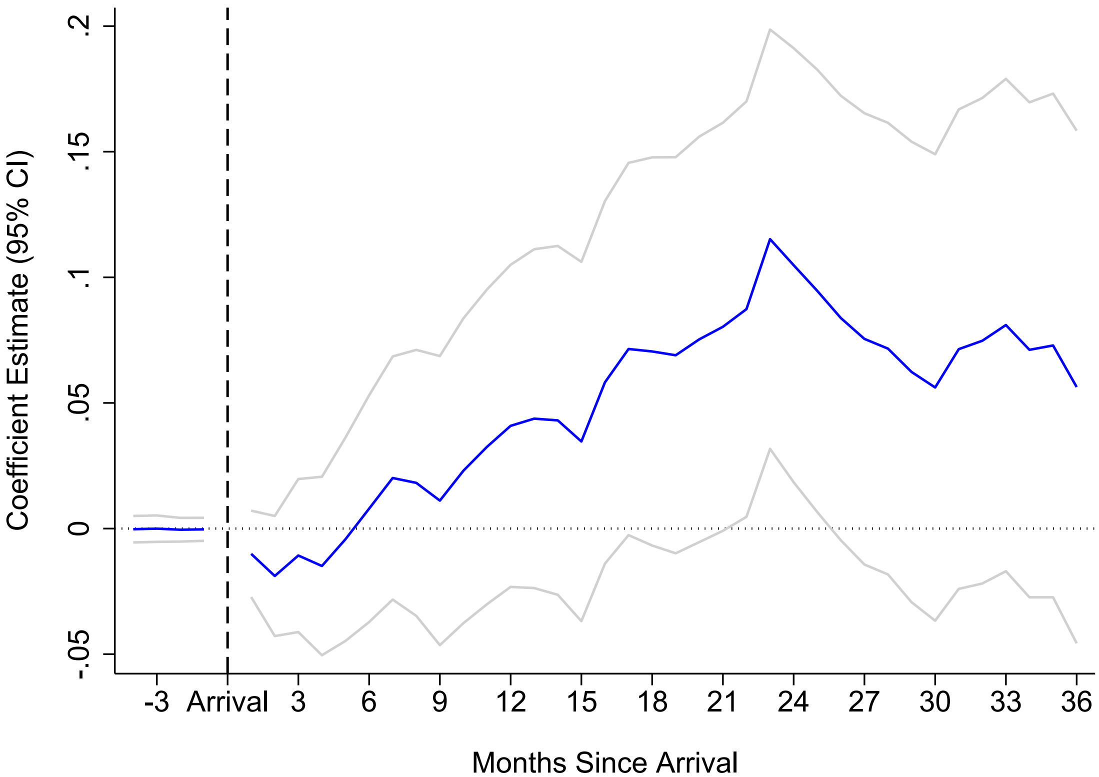
Notes. This figure displays coefficient estimates of Equation 2 with 95 percent confidence intervals for the sample of individuals who were unmarried on arrival to their first assignment. Next to each coefficient we report the point estimate and the associated p-value in brackets. Each coefficient is from a separate regression. The dependent variable is an indicator for individual i being married 24 months after assignment to the peer group. The labels on the horizontal axis indicate how the peer group is defined as described in the text. Company includes all individuals in the same Army company - our primary definition of a peer group throughout the paper. p-values are reported in brackets beneath each coefficient. The coefficient on the Army company peer group is slightly larger than that reported in Table 3, column six, because the sample on which we estimate these effects is restricted to individuals who have each of the peer groups defined. Some individuals do not have the more granular treatments defined; this occurs when individuals are in units with no peers of a given rank.

Figure 5. The Effect of Peers Across Demographic Groups

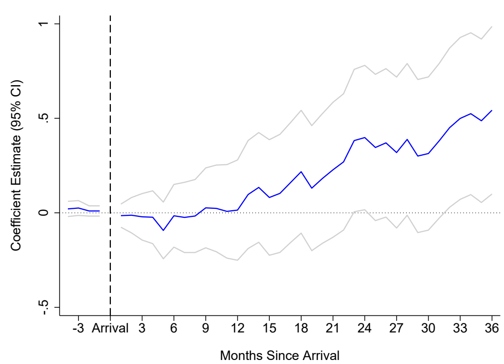


Notes. This figure shows coefficient estimates of Equation 2 with 95 percent confidence intervals for the sample of individuals who were unmarried on arrival to their first assignment. Next to each coefficient we report the point estimate and the associated p-value in brackets. Each coefficient is from a separate regression estimated on a sub-sample of the data defined by the labels on the horizontal axis. The dependent variable is an indicator for individual i being married 24 months after assignment to the peer group. In addition to being matched on θ_r, all comparisons in this figure are within race.

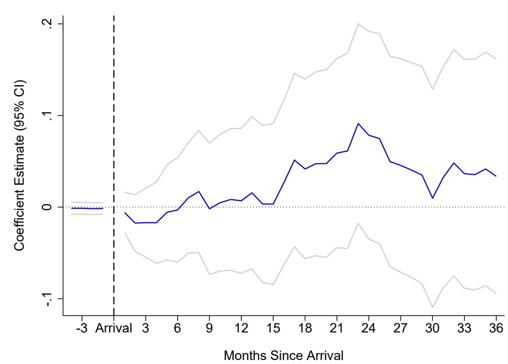
Figure 6. Peer Effects on Marriage by Month Since Arrival - 36 Month Window (IV)



(a) Full Sample



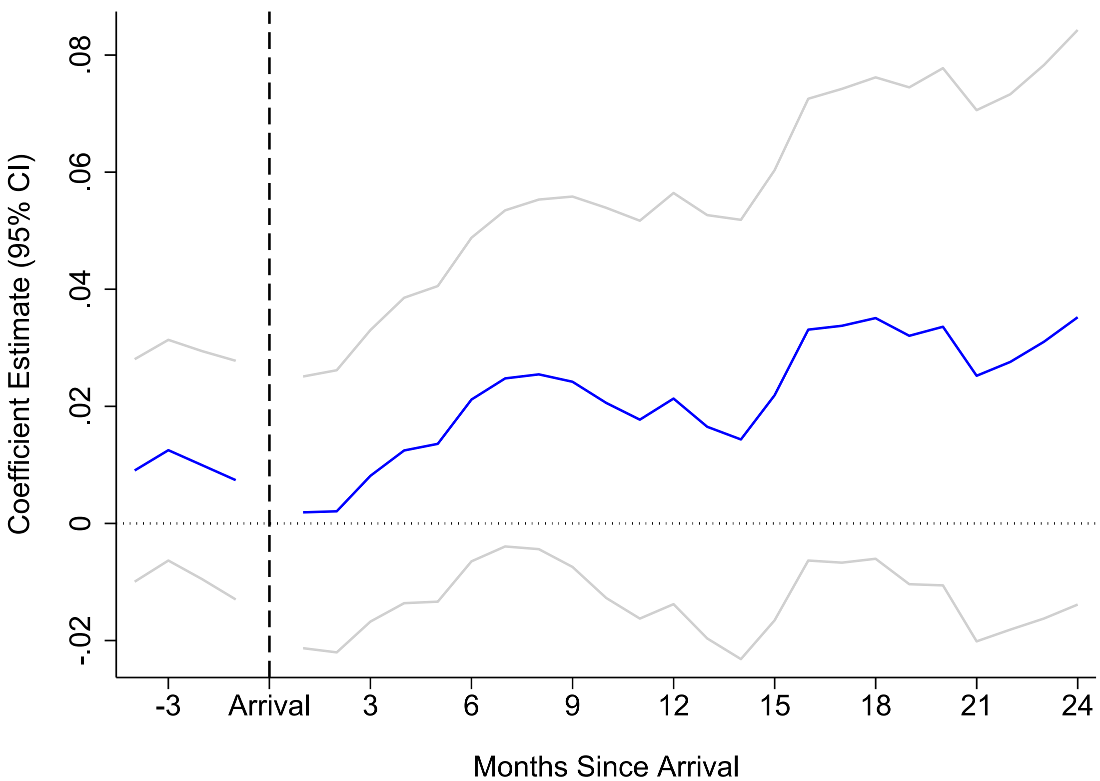
(b) Black



(c) White

Notes. This figures displays 2SLS estimates of Equation 2 by month, beginning four months prior to an individual's assignment to a peer group. The dependent variable is an indicator for an individual being married in each month. In the month of arrival (month 0), all individuals are unmarried. The blue line reflects the coefficient estimates and 95 percent confidence intervals are in gray. We restrict the sample to individuals who are still in the Army 36 months after assignment to their first operational unit so that we estimate long-term estimates on a balanced sample.

Figure 7. Peer Effects on Fertility by Month Relative to Arrival (IV)



Notes. This figure displays 2SLS estimates of Equation 2 by month, beginning four months prior to an individual's assignment to a peer group. The dependent variable is an indicator for an individual having children in each month. In the month of arrival (month 0), all individuals are unmarried. The blue line reflects the coefficient estimates and 95 percent confidence intervals are in gray.

Table 1. Summary Statistics

	Full Sample (1)	Unmarried (2)	Married (3)	Men (4)	Women (5)
Panel A. Soldier Characteristics					
Female	0.04	0.04	0.03	0.00	1.00
White	0.66	0.66	0.62	0.67	0.33
Black	0.14	0.14	0.14	0.13	0.40
Hispanic	0.15	0.15	0.18	0.15	0.19
Age	21.12	20.81	23.89	21.12	21.18
	(2.95)	(2.60)	(4.16)	(2.93)	(3.26)
AFQT Percentile	56.72	56.89	55.17	57.05	48.44
	(18.67)	(18.69)	(18.38)	(18.66)	(16.82)
High School Graduate	0.82	0.84	0.71	0.82	0.87
Panel B. Other Service Information					
Direct Combat Occ.	0.69	0.70	0.59	0.71	0.07
3-year Initial Contract	0.69	0.68	0.72	0.69	0.53
4-year Initial Contract	0.25	0.26	0.23	0.25	0.45
5 or 6-year Initial Contract	0.06	0.06	0.05	0.06	0.02
Months in Training	5.40	5.39	5.52	5.37	6.16
	(1.79)	(1.79)	(1.83)	(1.80)	(1.48)
Deployed within 24 Months of Arrival	0.61	0.61	0.66	0.62	0.47
Months Deployed	5.82	5.75	6.36	5.87	4.54
	(5.36)	(5.36)	(5.31)	(5.35)	(5.40)
Completed First Term	0.92	0.92	0.92	0.92	0.87
Re-enlisted	0.46	0.45	0.59	0.46	0.56
Assigned in Home State	0.06	0.06	0.07	0.06	0.08
Assigned in Home Region	0.35	0.35	0.37	0.35	0.41
Panel C. Marriage and Fertility					
Married on Arrival	0.10	0.00	1.00	0.10	0.09
Married, $t+24$	0.27	0.19	0.97	0.27	0.38
Married to Another Service Member, $t+24$	0.02	0.02	0.02	0.01	0.16
Has Children on Arrival	0.07	0.02	0.52	0.07	0.08
Has Children, $t+24$	0.18	0.12	0.72	0.18	0.24
Panel D. Treatment Variables					
Fraction of Peer Group Married, t-1	0.47	0.47	0.50	0.47	0.55
	(0.11)	(0.11)	(0.10)	(0.11)	(0.09)
Fraction of Peer Group Married	0.30	0.30	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	112.33	112.35	112.18	112.53	107.31
	(36.98)	(36.89)	(37.75)	(36.83)	(40.24)
Months Assigned to Peer Group	27.03	27.06	26.78	27.20	22.85
	(14.92)	(14.87)	(15.39)	(14.92)	(14.28)
Fraction of Peers Remaining	0.22	0.22	0.23	0.22	0.21
in Peer Group, $t+24$	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)
Observations	145,253	130,679	14,574	139,676	5,577

Notes. This table reports the mean and standard deviation for select variables for the estimation sample as described in Section A.1. Columns two and three divide the sample by marital status upon arrival to an individual's first Army assignment. 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.

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

	FE Only (1)	Race/Age (2)	Education (3)	All (4)
Black		0.000		0.000
		(0.001)		(0.001)
Hispanic		0.000		0.000
		(0.001)		(0.001)
Age		-0.000		-0.000
		(0.001)		(0.001)
Age^2		0.000		0.000
		(0.000)		(0.000)
High School Graduate			-0.001	-0.001
			(0.001)	(0.001)
Some College			-0.000	-0.001
			(0.001)	(0.001)
BA or Higher			0.004	0.004
			(0.005)	(0.005)
AFQT Percentile			-0.000	-0.000
			(0.000)	(0.000)
AFQT $Percentile^2$			0.000	0.000
			(0.000)	(0.000)
Observations	130,679	130,679	130,679	130,679
R-squared	0.774	0.774	0.774	0.774
Individual Controls	No	Yes	Yes	Yes
F-Statistic (Joint Test of Controls)		0.883	0.881	0.841
P-Value of F-Statistic		0.530	0.492	0.616

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 the fraction of individuals in a peer group who were married in the month before an individual arrived. 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. Race indicators for Native American, Pacific Islander, and Other Race are included with other race indicators but the estimates are omitted from the table for brevity. 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 (1)	OLS (2)	Reduced Form (3)	First Stage (4)	IV (5)	IV (6)
Fraction of Peers Married, $t-1$	0.082	0.077			0.109	0.105
	(0.021)***	(0.021)***			(0.031)***	(0.031)***
Fraction of Peers Married at Previous Location, $t-1$ (IV)			0.075	0.720		
			(0.022)***	(0.008)***		
Observations	130,679	130,679	130,679	130,679	130,679	130,679
Clusters	2,445	2,445	2,445	2,445	2,445	2,445
R-squared	0.251	0.258	0.258	0.876		
Individual Controls	-	X	X	X	-	X
DV Mean	0.195	0.195	0.195		0.195	0.195
Effect of Moving up IQR (%)	2.652	2.500	2.192		3.508	3.380
Endog. Test p-value					0.241	0.226
F-Stat				451.8		
F-Stat p-value				$< .01$		

Notes. This table presents estimates of Equation 2 for the sample of individuals who were unmarried on arrival to their first assignment in columns 1-3, 5, 6. Column 4 shows estimates from the first-stage regression, Equation 4. In columns 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 column 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, job, rank, month of arrival, location, 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 columns five and six 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 0.063 and the instrument (row 2) is 0.057. For brevity, this statistic is labeled "Effect of Moving up IQR."

Table 4. Peer Effects by Peer Group Deployment Status

	Did Not Deploy		Deployed		Interacted Specification	
	OLS (1)	IV (2)	OLS (3)	IV (4)	OLS (5)	IV (6)
Fraction of Peers Married, $t-1$	0.112	0.139	0.070	0.108	0.112	0.139
	(0.036)***	(0.053)***	(0.028)**	(0.040)***	(0.036)***	(0.053)***
Fraction of Peers Married, $t-1$ x Deployed					-0.042	-0.031
					(0.045)	(0.066)
Observations	48,419	48,419	75,943	75,943	124,362	124,362
Clusters	1,838	1,838	2,024	2,024	2,408	2,408
DV Mean	0.201	0.201	0.188	0.188	0.193	0.193
Effect of Moving up IQR (%)	3.458	4.297	2.319	3.557		

Notes. This table presents 2SLS estimates of Equation 2 for the sample of individuals who were unmarried on arrival to their first assignment. In columns one and two, the sample is restricted to individuals who were assigned to units that did not deploy within 24 months of an individual's assignment. In columns three and four, 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 5 due to splitting the sample by deployment status. All the comparisons in this table 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, job, rank, month of arrival, location, and initial term of enlistment, θ_r, and individual controls are included in all regressions. In columns 3-6, a cubic for the months a soldier's unit was deployed is also included to account for differences in deployment duration. 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 sample in this table is 0.062. For brevity, this statistic is labeled "Effect of Moving up IQR."

Table 5. Peer Effects on Fertility

	OLS (1)	OLS (2)	Reduced Form (3)	First Stage (4)	IV (5)	IV (6)
Fraction of Peers Married, $t-1$	0.037	0.032			0.044	0.035
	(0.017)**	(0.017)*			(0.026)*	(0.025)
Fraction of Peers Married at Previous Location, $t-1$ (IV)			0.025	0.720		
			(0.018)	(0.008)***		
Observations	130,679	130,679	130,679	130,679	130,679	130,679
Clusters	2,445	2,445	2,445	2,445	2,445	2,445
R-squared	0.244	0.267	0.267	0.876		
Individual Controls	-	X	X	X	-	X
DV Mean	0.125	0.125	0.125		0.125	0.125
Effect of Moving up IQR (%)	1.881	1.599	1.154		2.199	1.780
Endog. Test p-value					0.733	0.844
F-Stat				451.8		
F-Stat p-value				$< .01$		

Notes. This table presents estimates of Equation 2 for the sample of individuals who were unmarried on arrival to their first assignment in columns 1, 2, 3, 5, 6. Column 4 shows estimates from the first-stage regression, Equation 4. In columns 1-3, 5, and 6, the dependent variable is an indicator for individual i having children 24 months after assignment to the peer group. In column 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, job, rank, month of arrival, location, 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 columns five and six 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 0.063 and the instrument (row 2) is 0.057. For brevity, this statistic is labeled "Effect of Moving up IQR."

Table 6. Peer Effects on Fertility - Married Sample

	OLS (1)	OLS (2)	Reduced Form (3)	First Stage (4)	IV (5)	IV (6)
Fraction of Peers Married, $t-1$	0.115	0.114			0.232	0.232
	(0.075)	(0.075)			(0.117)**	(0.116)**
Fraction of Peers Married at Previous Location, $t-1$ (IV)			0.154	0.665		
			(0.077)**	(0.011)***		
Observations	14,574	14,574	14,574	14,574	14,574	14,574
Clusters	1,931	1,931	1,931	1,931	1,931	1,931
R-squared	0.383	0.403	0.403	0.884		
Individual Controls	-	X	X	X	-	X
DV Mean	0.721	0.721	0.721		0.721	0.721
Effect of Moving up IQR (%)	0.928	0.945	1.170		1.881	1.856
Endog. Test p-value					0.185	0.180
F-Stat				196		
F-Stat p-value				$< .01$		

Notes. This table presents estimates of Equation 2 for the sample of individuals who were married on arrival to their first assignment in columns 1, 2, 3, 5, 6. Column 4 shows estimates from the first-stage regression, Equation 4. In columns 1-3, 5, and 6, the dependent variable is an indicator for individual i having children 24 months after assignment to the peer group. In column 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, job, rank, month of arrival, location, 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 columns five and six 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 0.058 and the instrument (row 2) is 0.055. For brevity, this statistic is labeled "Effect of Moving up IQR"

A. Appendix

A.1. Sample Construction

We construct the estimation sample as follows. First, we restrict the sample to soldiers who have no previous Army experience and have never been assigned to an operational Army unit. Specifically, we identify junior soldiers in the grade of E-4 or below who are serving an initial enlistment contract of two to six years. We only include soldiers who we observe in each month from entry until exit or four years after entry, whichever is first. 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. Second, we place limitations on the types of units to which a soldier may be assigned. We require the unit to be an operational Army unit based in the U.S. and not associated with recruiting or training of any kind.[27] We also require each unit to have 40 to 200 individuals assigned to ensure we can identify a soldier's company.[28] These restrictions ensure that we focus our empirical analysis on sample of soldiers for whom we can identify the relevant peer group and who had no ability to influence the peer group to which they were assigned. In Table A6, we provide summary statistics for the sample of all enlistees in column one and the sample of individuals who meet the above criteria in column two.[29]

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 October 2018. Our empirical strategy uses variation in the fraction of peers who are married within cells defined by the interaction of sex, occupation, rank, assignment location, month-year of arrival, and initial term of enlistment.[30] We drop any singleton observations, as well as any cells for which there is no variation in treatment, as these

[27] 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.

[28] 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 very different peer dynamics from the rest of the sample.

[29] While we do lose a substantial portion of the sample due to the sample restrictions, the arrival sample in column two is largely similar to the sample of all enlistees. Based on our knowledge of how the Army assigns soldiers to their initial unit, we view the restrictions above as necessary to isolate plausibly random variation in peer group assignment.

[30] 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.

observations do not contribute to identification (Miller, Shenhav, et al., forthcoming). In columns three through six of Table A6 we provide summary statistics for each group of individuals that is not included in the full estimation sample.[31] The sample on which we estimate peer effects consists of $145,253$ enlisted soldiers (column seven).[32]

While our estimation sample is similar to the full sample of enlisted soldiers, there are some notable differences. In particular, there are relatively few women in the estimation sample (only four percent compared to 17 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 four). Second, since only 17 percent of enlistees are women, it is less likely that a woman arrives in the same month at the same location as another woman of the same occupation, rank, initial term of enlistment, and marital status (column five). Finally, for the majority of the sample period, women were not able to serve in many Army units, so it was more likely that women would be assigned to the same unit, conditional on arriving at the same location in the same month (column six). Another notable difference is that our sample draws heavily from soldiers in direct combat occupations (68 percent compared to 39 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.[33]

A.2. Deriving Estimating Equations

This derivation largely follows from Lieber and Skimmyhorn (2018). Our structural model of social effects is given by Equation 1. Taking the expectation of Equation 1

[31] 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.

[32] The difference in the fraction of soldiers who serve in a direct combat occupation between Table A6 column seven and Table 1 column one is due to differences in when occupation is measured. In Table A6, occupation is measured at entry. In Table 1, occupation is measured upon arrival to a soldier's first location. A small number of soldiers re-classify into other occupations during basic training.

[33] We use the term direct combat occupation to refer 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.

over individuals in group g:[34]

$$\bar{Y}_{g,t} = \frac{\beta_0}{1-\beta_{\bar{Y}}} + \bar{Z}_{g,t-1}\left(\frac{\beta_{\bar{Z}} + \beta_Z}{1-\beta_{\bar{Y}}}\right) + \left(\frac{1}{1-\beta_{\bar{Y}}}\right)[\omega_{g,t} + \bar{\upsilon}_{g,t}] \tag{A1}$$

Plugging this expression for $\bar{Y}_{g,t}$ back into Equation 1 yields a reduced form equation:

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

The only structural parameter that is identified in Equation A2 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 $\bar{Z}_{g-i,t-1}$ 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, and no more than a third of the company leaves in any year. We capture this serial correlation in the following theoretical regression equation:

$$\bar{Z}_{g,t-1}\left(\frac{\beta_{\bar{Y}}\beta_Z + \beta_{\bar{Z}}}{1-\beta_{\bar{Y}}}\right) = \mu_0 + \mu_1\bar{Z}_{g,t-2}\left(\frac{\beta_{\bar{Z}} + \beta_Z}{1-\beta_{\bar{Y}}}\right) + \xi_{g,t-1} \tag{A3}$$

Now solving the $t-1$ version of Equation A1 for $\bar{Z}_{g,t-2}\left(\frac{\beta_{\bar{Z}}+\beta_Z}{1-\beta_{\bar{Y}}}\right)$:

$$\bar{Z}_{g,t-2}\left(\frac{\beta_{\bar{Z}} + \beta_Z}{1-\beta_{\bar{Y}}}\right) = \left[\bar{Y}_{g,t-1} - \frac{\beta_0}{1-\beta_{\bar{Y}}} - \left(\frac{1}{1-\beta_{\bar{Y}}}\right)[\omega_{g,t-1} - \bar{\upsilon}_{g,t-1}]\right] \tag{A4}$$

And plugging this expression into Equation A3:

[34] 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 ($\bar{Y}_{g-i,t} = \bar{Y}_{g,t}$).

$$\bar{Z}_{g-i,t-1}\left(\frac{\beta_{\bar{Y}}\beta_Z + \beta_{\bar{Z}}}{1-\beta_{\bar{Y}}}\right) = \mu_0 + \mu_1\left[\bar{Y}_{g,t-1} - \frac{\beta_0}{1-\beta_{\bar{Y}}} - \left(\frac{1}{1-\beta_{\bar{Y}}}\right)[\omega_{g,t-1} - \bar{\upsilon}_{g,t-1}]\right] + \xi_{g,t-1} \quad \text{(A5)}$$

Finally, we substitute the right-hand side of Equation A5 into the reduced form equation (Equation A2):

$$Y_{ig,t} = \frac{\beta_0}{1-\beta_{\bar{Y}}} + \mu_0 + \mu_1\left[\bar{Y}_{g,t-1} - \frac{\beta_0}{1-\beta_{\bar{Y}}} - \left(\frac{1}{1-\beta_{\bar{Y}}}\right)[\omega_{g,t-1} - \bar{\upsilon}_{g,t-1}]\right] + \xi_{g,t-1} + \beta_Z Z_{ig,t-1} + \left(\frac{1}{1-\beta_{\bar{Y}}}\right)\left[\omega_{g,t} + \beta_{\bar{Y}}\bar{\upsilon}_{g,t}\right] + \upsilon_{i,t} + \epsilon_{ig,t} \quad \text{(A6)}$$

Which reduces to:

$$Y_{ig,t} = \pi_0 + \pi_1\bar{Y}_{g,t-1} + \pi_2 Z_{ig,t-1} + \zeta_{ig,t} \quad \text{(A7)}$$

Where $\pi_0 = \frac{\beta_0}{1-\beta_{\bar{Y}}}(1-\mu_1) + \mu_0$, $\pi_1 = \mu_1$, $\pi_2 = \beta_Z$, and $\zeta_{ig,t} = (\frac{1}{1-\beta_{\bar{Y}}})(\omega_{g,t} - \mu_1\omega_{g,t-1}) + (\frac{1}{1-\beta_{\bar{Y}}})(\beta_{\bar{Y}}\bar{\upsilon}_{g,t} - \mu_1\bar{\upsilon}_{g,t-1}) + \upsilon_{i,t} + \xi_{g,t-1} + \epsilon_{ig,t}$. 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 A7 will lead to downward biased estimates of π_1.

A.3. Addressing Selective Attrition

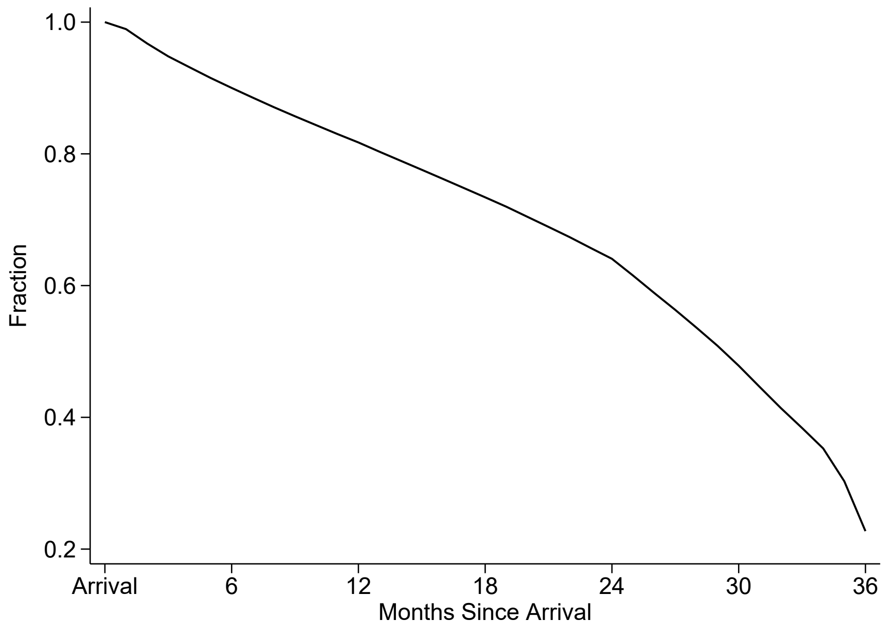
In our main specifications, we limited our sample to individuals who remain in the Army for at least 24 months after assignment. If our treatment has a direct affect on the probability that soldiers remain in the Army, it could be that the peer effect we estimate is affected by selective attrition out of sample and is not capturing the effect of peers' marriage decisions.

To examine this issue, we first estimate Equation 2 by 2SLS where the outcome is an indicator for being in the Army each month. The estimate in column one of Table A7 indicates that there is no statistical or economically meaningful relationship between the treatment and attrition.[35] 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. Columns two and three of Table A7 show that there is a positive relationship between our treatment and the probability that a soldier re-enlists, but the treatment is not associated with differences in first term completion.

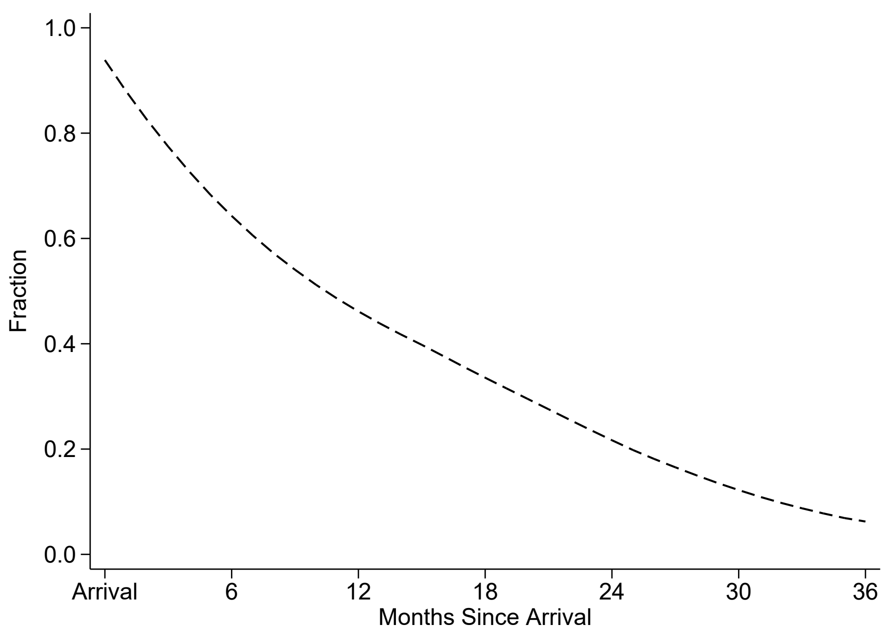
We next show that including in our sample individuals who leave the Army within two years of entry does not substantially change any of our main results. To include individuals who leave the Army before 24 months, we estimate Equation 2 by 2SLS on the unmarried sample where the dependent variable is an indicator for being married at 24 months or at an individual's last observation, whichever is earlier. The results in Table A8 are qualitatively the same as Table 3. Since marriage is observed earlier for individuals who attrit, the estimated peer effect is slightly smaller than in the main sample.

[35] We plot these estimates by month in Figure A14.

Figure A1. Duration of Exposure to Peers



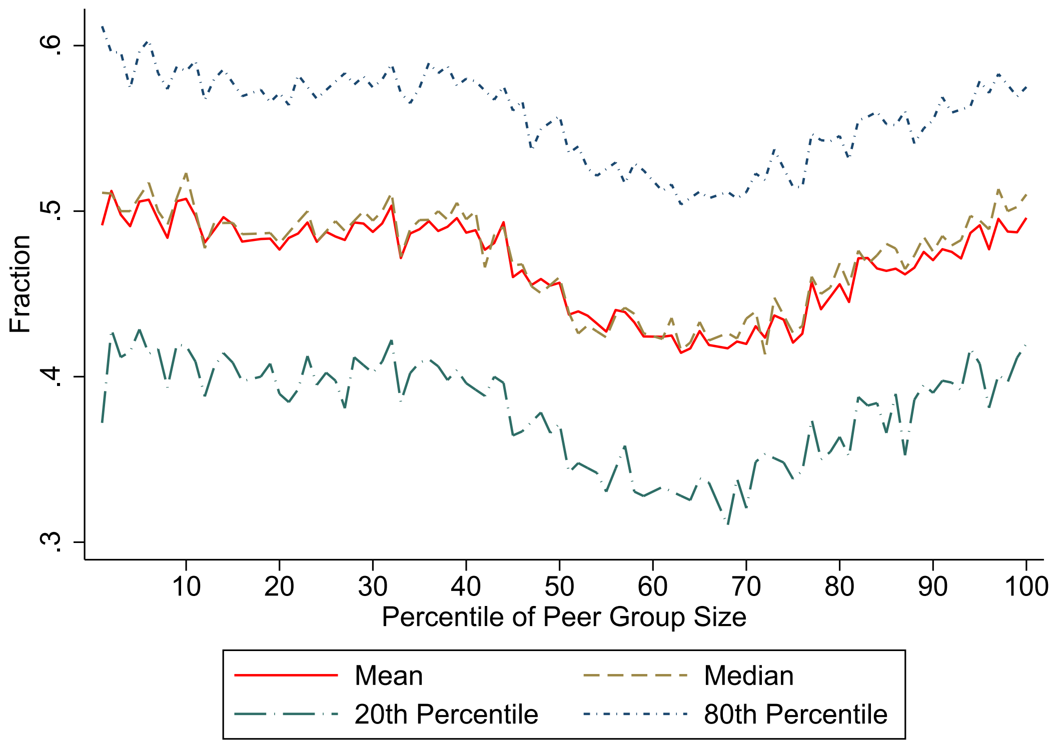
(a) Fraction of Individuals still in the Peer Group



(b) Fraction of Original Peers still in the Peer Group

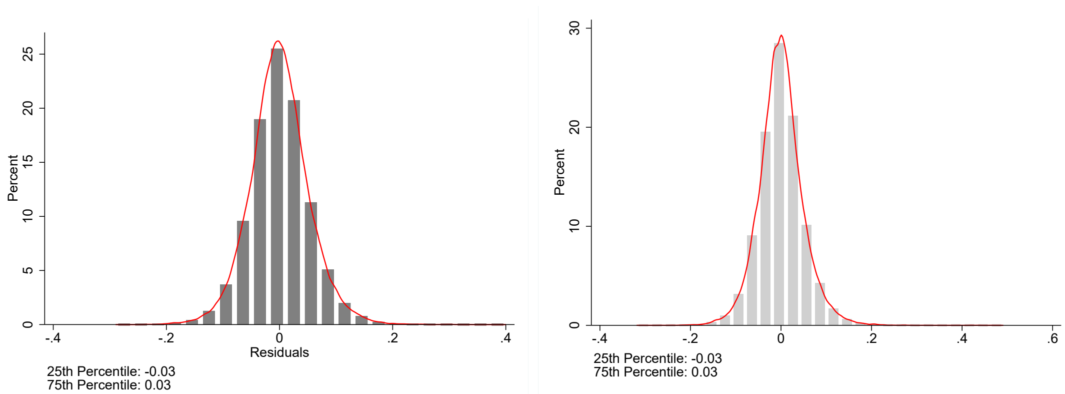
Notes. Panel A plots the fraction of individuals who are still assigned to their original peer group (Army company), by month. Panel B plots the fraction of an individual's original peers (i.e. those individuals who are included in the treatment measure) who are still in the peer group, by month.

Figure A2. Relationship Between Peer Group Size and Fraction 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

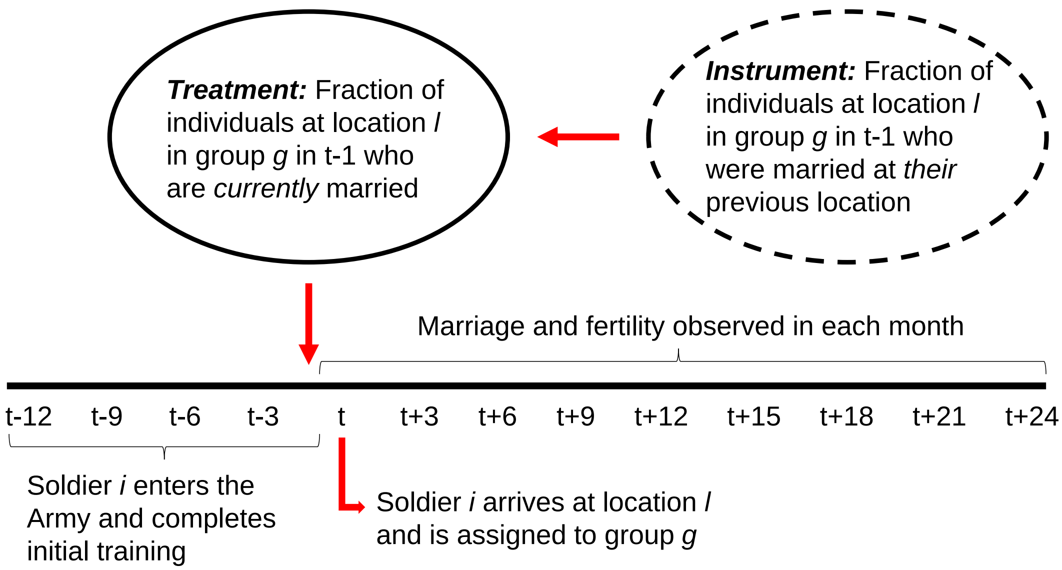


(a) Fraction of Peers Married (Treatment)

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

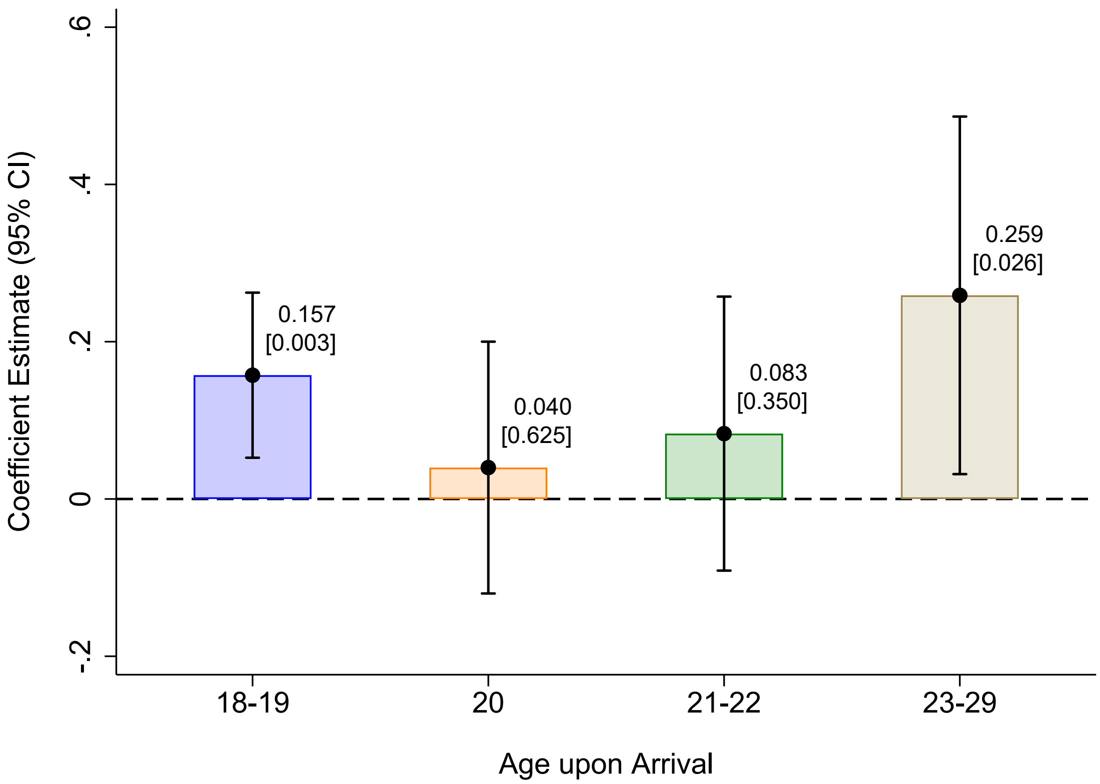
Notes. This figure is similar to Figure 2, but in this case we plot the distributions of the residuals of each variable (the treatment and the instrument) after regressing it on interacted fixed effects for sex, job, rank, month of arrival, location, and initial term of enlistment. Panel A is a histogram of the residuals for our primary treatment variable: the fraction of individuals in the peer group who were married in the month before an individual's arrival. Panel B is a histogram of the residuals for the instrument. We construct the instrument by measuring marriage at an individual's previous location prior to assignment to the current location. Below each figure we report the 25th and 75th percentiles. Each histogram has 100 equally sized bins. The red lines are kernel density estimates using an Epanechnikov kernel and Silverman's rule of thumb bandwidth.

Figure A4. Treatment and Instrument Definitions.



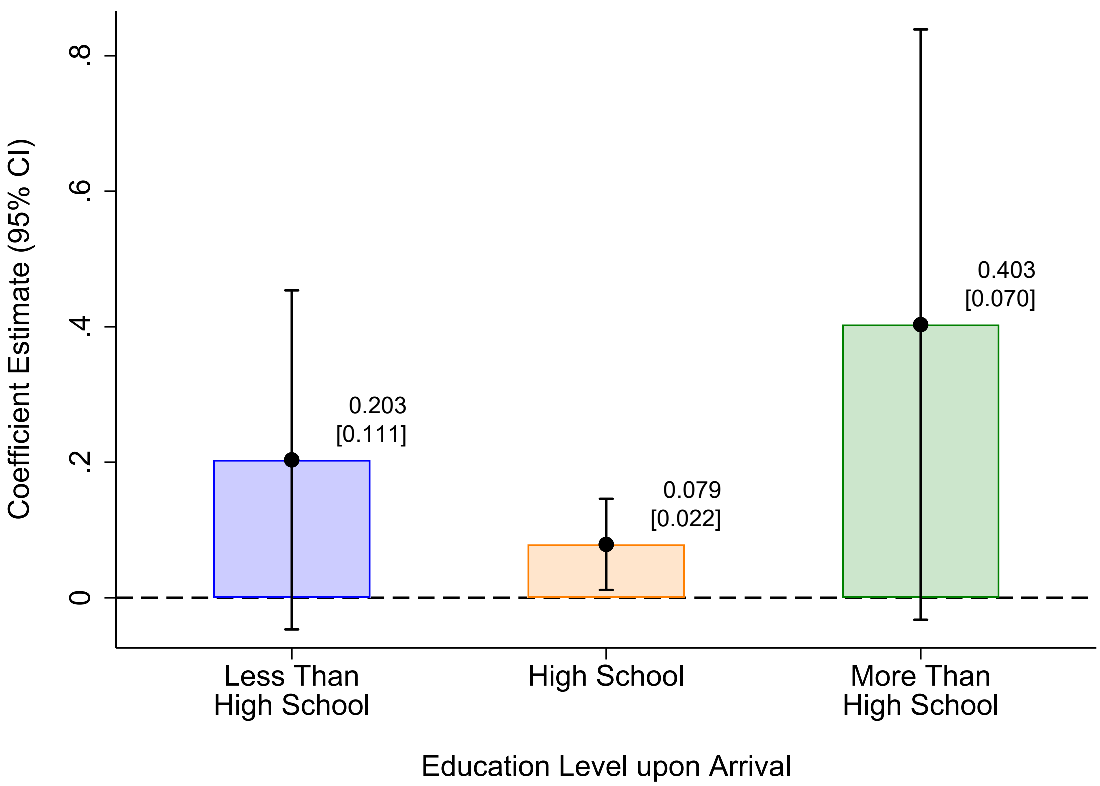
Notes. This figure depicts how the treatment and instrument were constructed relative to the timing of an individual's assignment. We condition the sample such that we only compare outcomes between soldiers who had the same marital status upon arrival to their first unit.

Figure A5. Heterogeneity by Age



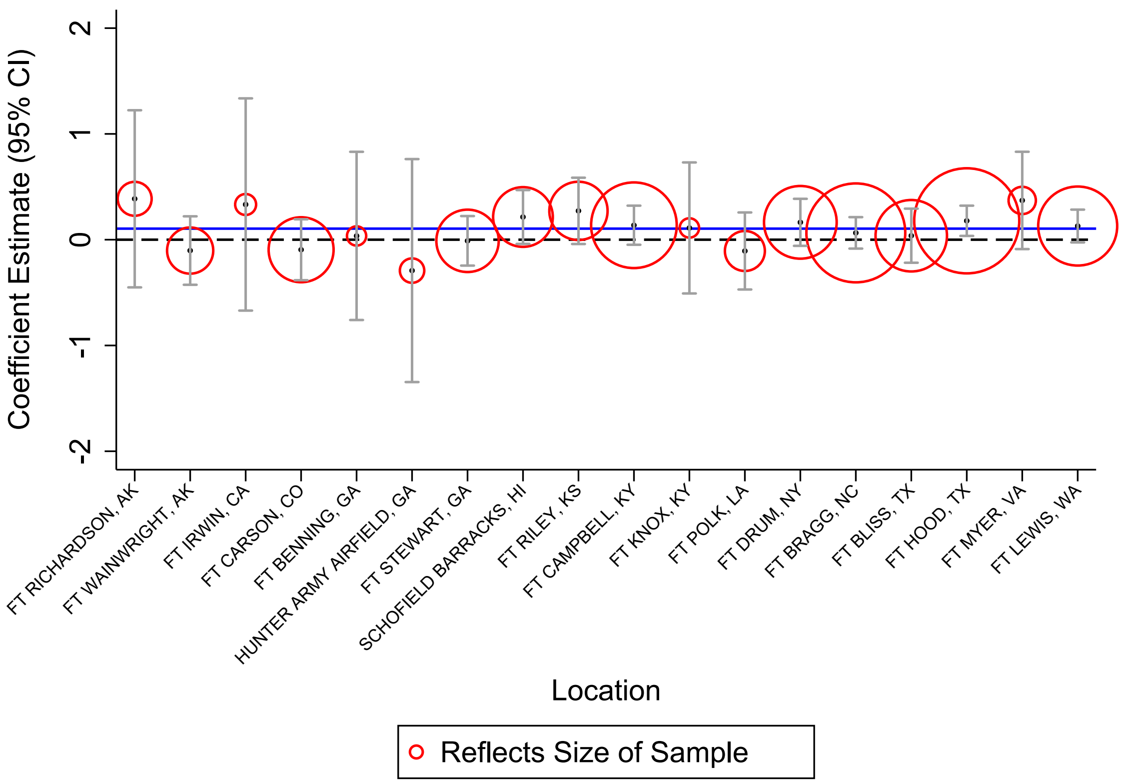
Notes. This figure shows coefficient estimates of Equation 2 with 95 percent confidence intervals for the sample of individuals who were unmarried on arrival to their first assignment. Next to each coefficient we report the point estimate and the associated p-value in brackets. Each coefficient is from a separate regression estimated on a sub-sample of the data defined by the labels on the horizontal axis. The dependent variable is an indicator for individual i being married 24 months after assignment to the peer group. In addition to being matched on θ_r, all comparisons in this figure are within age groups.

Figure A6. Heterogeneity by Education



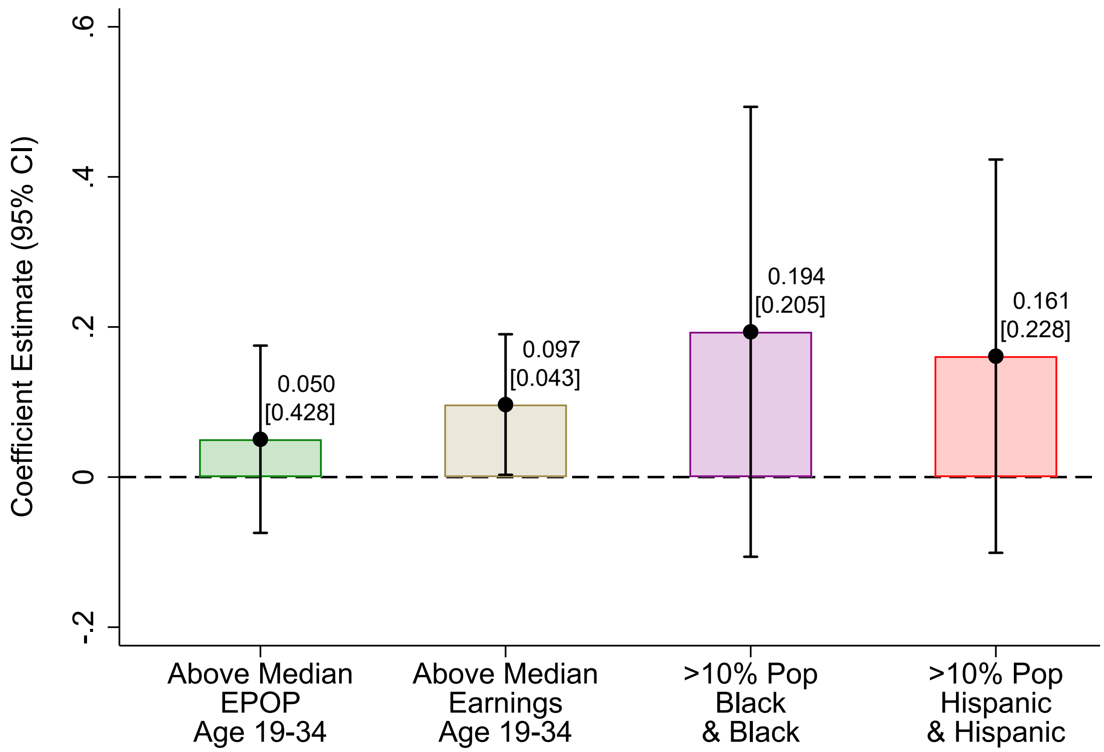
Notes. This figure shows coefficient estimates of Equation 2 with 95 percent confidence intervals for the sample of individuals who were unmarried on arrival to their first assignment. Next to each coefficient we report the point estimate and the associated p-value in brackets. Each coefficient is from a separate regression estimated on a sub-sample of the data defined by the labels on the horizontal axis. The dependent variable is an indicator for individual i being married 24 months after assignment to the peer group. In addition to being matched on θ_r, all comparisons in this figure are within education groups.

Figure A7. Heterogeneity by Location



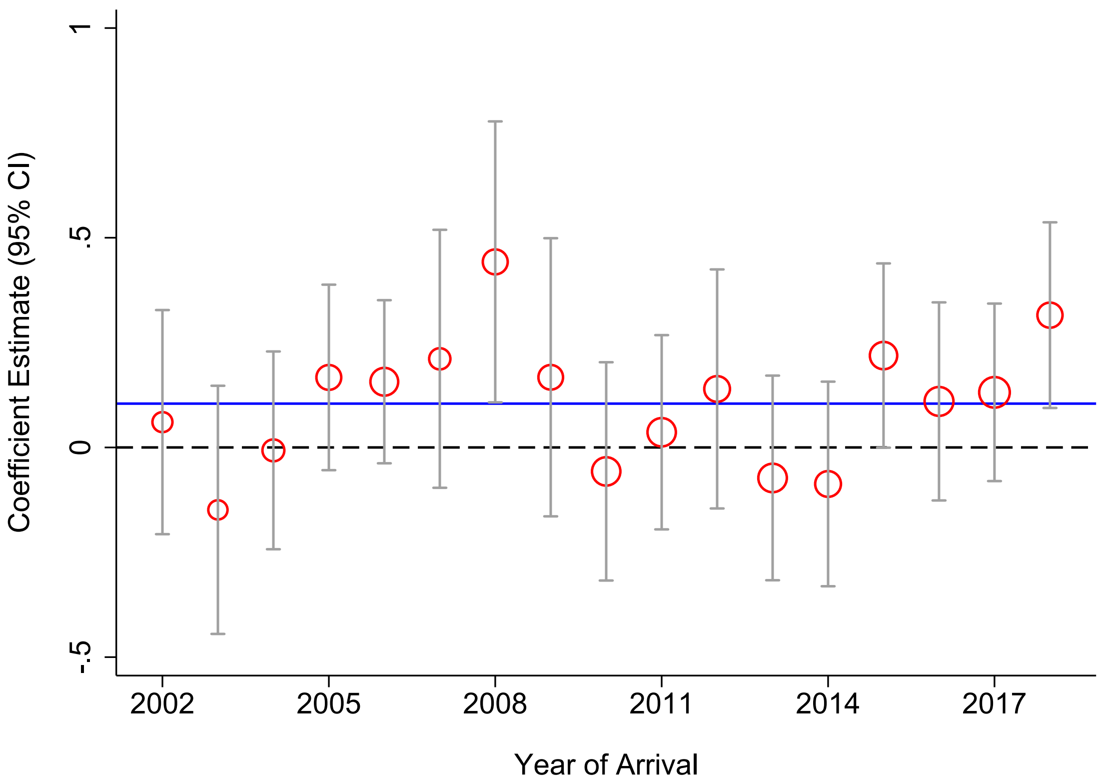
Notes. This figure shows coefficient estimates of Equation 2 with 95 percent confidence intervals for the sample of individuals who were unmarried on arrival to their first assignment. Each coefficient is from a separate regression estimated only on the location indicated on the horizontal axis. The red circles indicate the relative number of observations in the full sample at each location. The blue line is the main estimate from Table 3.

Figure A8. Heterogeneity by County Location Characteristics



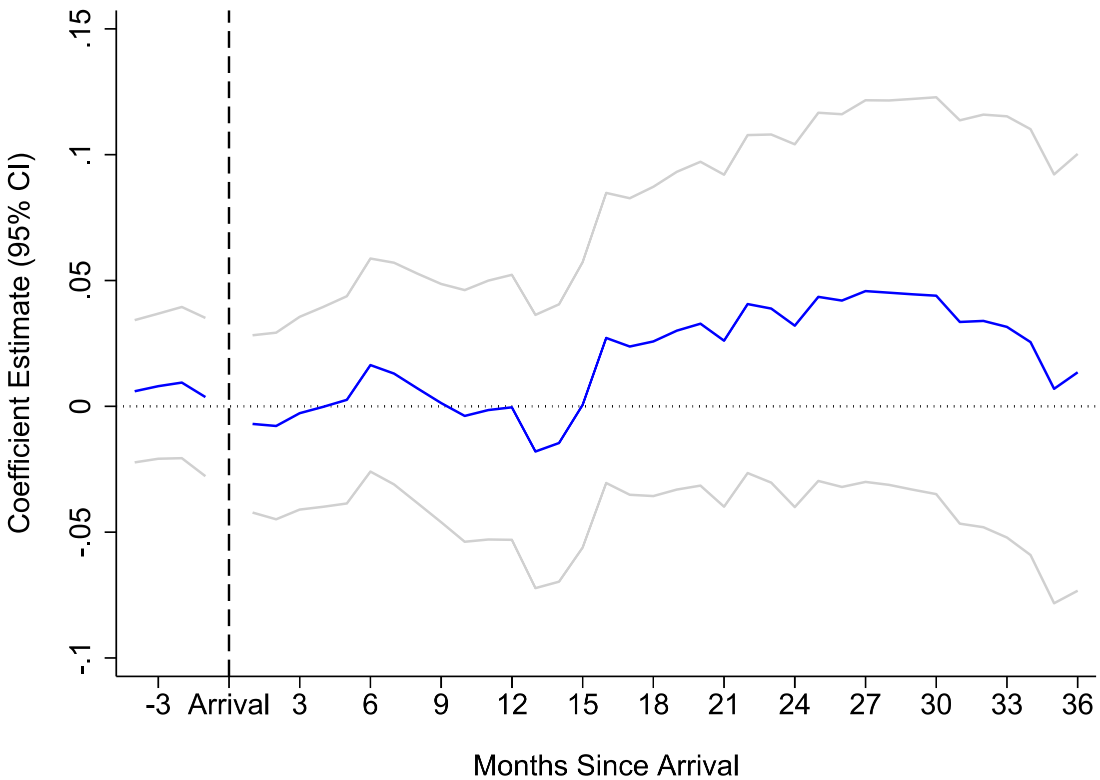
Notes. This figure shows coefficient estimates of Equation 2 with 95 percent confidence intervals for the sample of individuals who were unmarried on arrival to their first assignment. Next to each coefficient we report the point estimate and the associated p-value in brackets. Each coefficient is from a separate regression. The dependent variable is an indicator for individual i being married 24 months after assignment to the peer group. Our first two characteristics from left are based on the labor market prospects for individuals age 19-34 in the county of assignment. We define counties within our sample to be above or below median in each quarter. Next, we isolate instances where Black or Hispanic soldiers are assigned to locations where at least 10 percent of the county population is Black or Hispanic, respectively.

Figure A9. Heterogeneity by Year



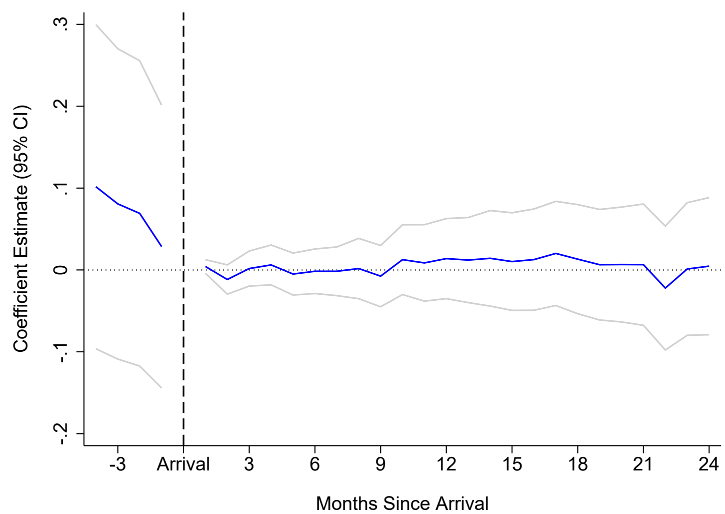
Notes. This figure shows coefficient estimates of Equation 2 with 95 percent confidence intervals for the sample of individuals who were unmarried on arrival to their first assignment. Each coefficient is from a separate regression estimated only on the sample of individuals who arrived to their first duty station in the indicated year. The red circles indicate the relative number of observations in the full sample from each year. The blue line is the main estimate from Table 3.

Figure A10. Peer Effects on Fertility by Month Since Arrival - 36 Month Window (IV)

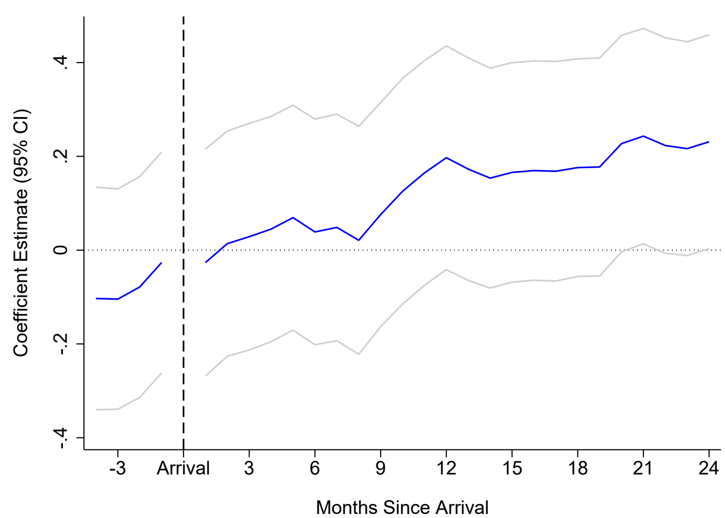


Notes. This figures displays 2SLS estimates of Equation 2 by month, beginning four months prior to an individual's assignment to a peer group. The dependent variable is an indicator for an individual having children in each month. In the month of arrival (month 0), all individuals are unmarried. The blue line reflects the coefficient estimates and 95 percent confidence intervals are in gray. We restrict the sample to individuals who are still in the Army 36 months after assignment to their first operational unit so that we estimate long-term estimates on a balanced sample.

Figure A11. Peer Effects by Month Relative to Arrival (IV) for the Married Sample



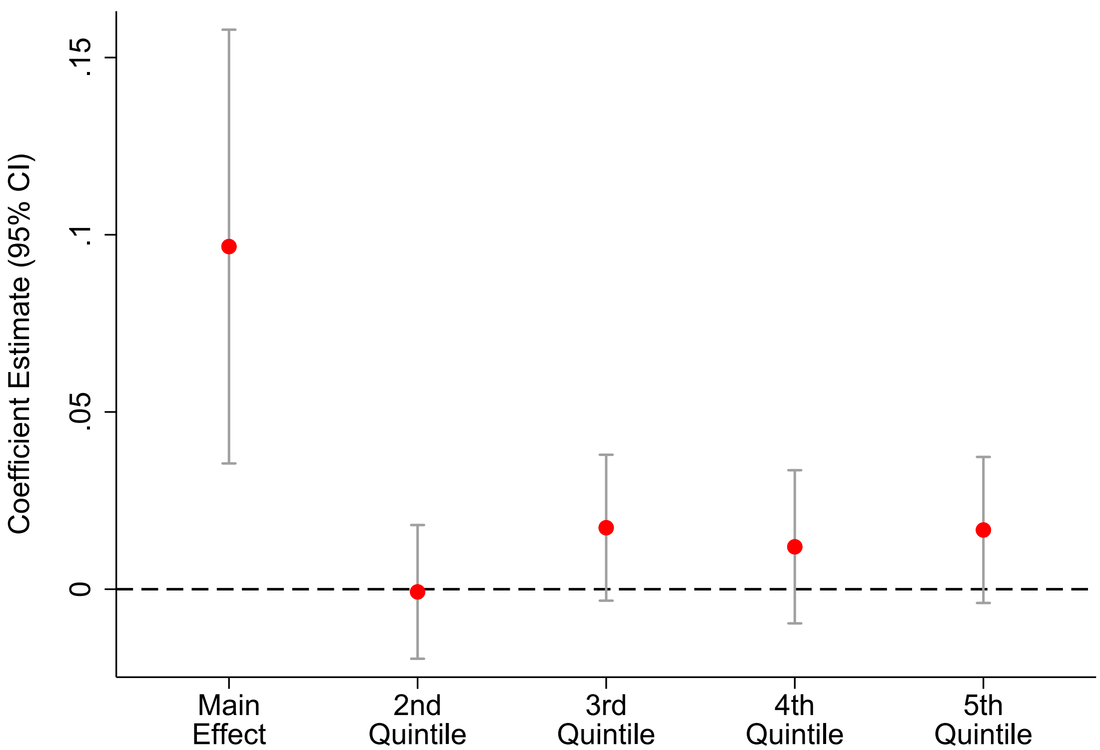
(a) Outcome: Marriage



(b) Outcome: Fertility

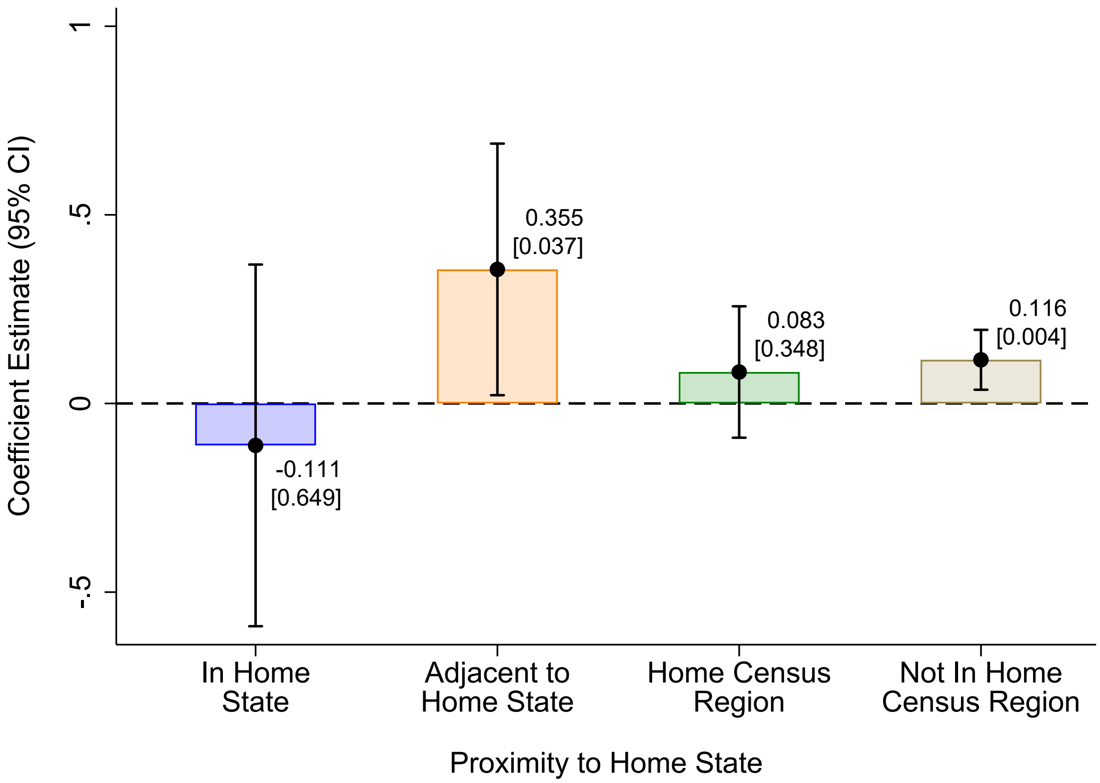
Notes. This figure displays 2SLS estimates of Equation 2 by month, beginning four months prior to an individual's assignment to a peer group. In Panel A, the dependent variable is an indicator for an individual being married in each month. In Panel B, the dependent variable is an indicator for an individual having any children in each month. In the month of arrival (month 0), all individuals are married. The blue line reflects the coefficient estimates and 95 percent confidence intervals are in gray.

Figure A12. Heterogeneity by Peer Group Size



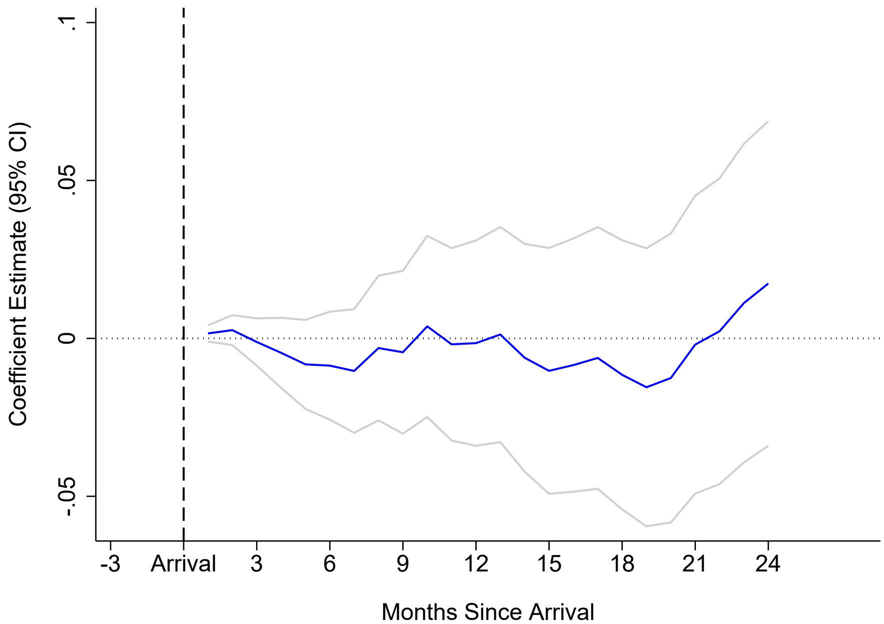
Notes. This figure shows coefficient estimates and 95 percent confidence intervals from a version of Equation 2 estimated on the sample of individuals who were unmarried on arrival to their first assignment. All coefficients come from a single regression. We create indicators for each quintile of peer group size and interact these indicators with the main treatment (fraction of peers married). The first coefficient from left is the main effect for individuals assigned to peer groups in the first quintile of peer group size. All other coefficients are estimates of differences between the indicated quintile and the first quintile.

Figure A13. Heterogeneity by Proximity to Home State



Notes. This figure displays coefficient estimates of Equation 2 with 95 percent confidence intervals for the sample of individuals who were unmarried on arrival to their first assignment. Next to each coefficient we report the point estimate and the p-value. Each coefficient is from a separate regression. The dependent variable is an indicator for individual i being married 24 months after assignment to the peer group. The labels on the horizontal axis indicate the proximity between an individual's home state and their state of assignment. The proximity measures are mutually exclusive. In the regressions that produce these estimates, all soldiers are matched on proximity, so these estimates provide information about whether peers exert more or less influence when individuals are stationed close to home. The sample size is increasing as we broaden the proximity measure. The breakdown for the unmarried sample ($N = 130,078$) is 7,399 (5.7 percent) home state, 10,908 (8.4 percent) adjacent to home state, 29,644 (22.8 percent) in home census region, and 82,127 (63.1 percent) outside of home census region.

Figure A14. Peer Effects on Remaining in the Army by Month Since Arrival (IV)



Notes. This figure displays 2SLS estimates of Equation 2 by month, beginning with the month an individual is assigned to a peer group. The dependent variable is an indicator for being in the sample in each month. The blue line reflects the coefficient estimates and 95 percent confidence intervals are in gray.

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)	$t_{q4=q3}$ (5)	$\hat{\Delta}_{q4,q3}$ (6)	$t_{q3=q2}$ (7)	$\hat{\Delta}_{q3,q2}$ (8)	$t_{q2=q1}$ (9)	$\hat{\Delta}_{q2,q1}$ (10)	$t_{q4=q1}$ (11)	$\hat{\Delta}_{q4,q1}$ (12)
White	0.660	0.662	0.658	0.661	0.761	0.006	-1.142	-0.009	0.603	0.005	0.223	0.002
Black	0.139	0.141	0.142	0.137	-1.654	-0.005	0.239	0.002	0.812	0.006	-0.603	-0.013
Hispanic	0.147	0.144	0.147	0.145	-0.492	-0.004	0.901	-0.006	-0.799	-0.003	-0.389	0.007
Age	20.782	20.828	20.839	20.805	-1.617	0.009	0.527	0.018	**2.283**	-0.013	1.187	0.004
High School Graduate	0.839	0.837	0.837	0.836	-0.273	0.000	-0.034	-0.008	-0.754	-0.006	-1.060	-0.002
Associate's/Some College	0.043	0.045	0.044	0.044	-0.048	0.003	-0.725	0.009	1.200	0.000	0.427	-0.006
BA or Higher	0.010	0.011	0.012	0.010	**-2.414**	0.003	0.399	0.000	**2.018**	0.016	0.002	-0.019
AFQT Percentile	56.696	57.066	57.088	56.715	**-2.552**	0.001	0.153	-0.020	**2.529**	0.020	0.129	0.001
Training Time	5.413	5.397	5.405	5.342	**-4.497**	-0.040	0.537	-0.009	-1.124	-0.035	**-5.086**	0.004

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 a t-statistic and the normalized difference, $\hat{\Delta}$. The t-statistic for any sub-groups of the data h and j is calculated as: $t = \frac{\bar{X}_h - \bar{X}_j}{\sqrt{(s_h^2/N_h + s_j^2/N_j)}}$, 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. Similarly the normalized difference is defined as: $\hat{\Delta}_{hj} = \frac{\bar{X}_h - \bar{X}_j}{\sqrt{(s_h^2 + s_j^2)/2}}$. Any t-statistic with an absolute value greater than or equal to 1.96 is in bold, indicating that differences are statistically significant at the 5 percent level. $N_{total} = 130,679$, $N_{q1} = 32,670$, $N_{q2} = 32,670$, $N_{q3} = 32,675$, $N_{q4} = 32,664$.

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

	Robust (1)	θ_r (2)	Company (3)	Location (4)
Panel A. Marriage				
Fraction of Peers Married, $t-1$	0.105	0.105	0.105	0.105
	(0.031)***	(0.031)***	(0.031)***	(0.027)***
DV Mean	0.195	0.195	0.195	0.195
Panel B. Fertility				
Fraction of Peers Married, $t-1$	0.035	0.035	0.035	0.035
	(0.025)	(0.025)	(0.025)	(0.025)
DV Mean	0.125	0.125	0.125	0.125
Observations	130,679	130,679	130,679	130,679
Clusters		28,826	2,445	28

Notes. This table presents 2SLS estimates of equation 2 for the sample of individuals who were unmarried on arrival to their first assignment where the level of clustering is varied. The column headings indicate the level of clustering. In Panel A the dependent variable is an indicator for individual i being married 24 months after assignment to the peer group. In Panel B the dependent variable is an indicator for individual i having children 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 (1)	OLS (2)	Reduced Form (3)	First Stage (4)	IV (5)	IV (6)
Fraction of Peers Married, $t-1$	-0.016	-0.015			0.003	0.004
	(0.026)	(0.026)			(0.042)	(0.043)
Fraction of Peers Married at Previous Location, $t-1$ (IV)			0.003	0.665		
			(0.028)	(0.011)***		
Observations	14,574	14,574	14,574	14,574	14,574	14,574
Clusters	1,931	1,931	1,931	1,931	1,931	1,931
R-squared	0.407	0.409	0.409	0.884		
Individual Controls	-	X	X	X	-	X
DV Mean	0.973	0.973	0.973		0.973	0.973
Effect of Moving up IQR (%)	-0.0940	-0.0900	0.0160		0.0200	0.0260
Endog. Test p-value					0.552	0.548
F-Stat				196		
F-Stat p-value				$< .01$		

Notes. This table presents estimates of Equation 2 for the sample of individuals who were married on arrival to their first assignment in columns 1, 2, 3, 5, 6. Column 4 shows estimates from the first-stage regression, Equation 4. In columns 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 column 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, job, rank, month of arrival, location, 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 columns five and six 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 0.058 and the instrument (row 2) is 0.055. For brevity, this statistic is labeled "Effect of Moving up IQR."

Table A4. Peer Effects on Marriage Based on Proximity of Daily Interactions

	Baseline	Same Rank	First-Line Supervisors	Second-Line Supervisors	Senior Supervisors	Joint
	(1)	(2)	(3)	(4)	(5)	(6)
Peer Group Defined as:						
Army Company	0.113					
	(0.031)***					
Same Rank		0.055				0.046
		(0.030)*				(0.030)
First-Line Supervisors			0.049			0.045
			(0.021)**			(0.022)**
Second-Line Supervisors				-0.006		-0.014
				(0.020)		(0.020)
Senior Supervisors					0.002	0.003
					(0.016)	(0.016)
Observations	129,005	129,005	129,005	129,005	129,005	129,005
Clusters	2,434	2,434	2,434	2,434	2,434	2,434
Hypothesis Tests:						
Same Rank = First-Line Supervisor						0.962
Same Rank = Second-Line Supervisor						0.097
Same Rank = Senior Supervisor						0.200
First-Line Supervisor = Second-Line Supervisor						0.0610
First-Line Supervisor = Senior Supervisor						0.132

Notes. This table presents estimates of Equation 2 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 (DV Mean=0.195). The column headings indicate the individuals who are counted in the focal individual's peer group. Interacted fixed effects for sex, job, rank, month of arrival, location, 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 column six, we report p-values from Wald tests on whether the indicated coefficients are equal. The number of observations in this table is slightly less than in Table 3 because some individuals do not have the more granular treatments defined. This occurs when individuals are in units with no peers of a given rank.

Table A5. The Cumulative Effect of Peers

	(1)	(2)	(3)	(4)	(5)	(6)
Fraction of Incumbent Peers Married, $t-1$	0.101				0.099	0.092
	(0.034)***				(0.034)***	(0.035)***
Fraction of Arriving Peers Married					0.005	
$t+1$ to $t+12$					(0.013)	
Fraction of Arriving Peers Married		0.036				0.027
$t+1$ to $t+24$		(0.019)*				(0.019)
Fraction of All Peers Married			0.102			
$t-1$ to $t+12$			(0.035)***			
Fraction of All Peers Married				0.120		
$t-1$ to $t+24$				(0.035)***		
Observations	110,498	110,498	110,498	110,498	110,498	110,498
Clusters	2,409	2,409	2,409	2,409	2,409	2,409
DV Mean	0.194	0.194	0.194	0.194	0.194	0.194

Notes. This table presents estimates of Equation 2 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 in month t. All regressions include the individual soldier and randomization controls as described in the text. The sample differs slightly from Table 5 because we must be able to measure arriving soldiers at the unit level for at least 24 months after a soldier's arrival. In column one the treatment is defined as before. In column two, the treatment is the fraction of peers who arrive after the focal soldier who are married upon arrival. While we always measure incumbent peers based on the month prior to a focal soldier's arrival, we consider two distinct groups of arriving peers: (1) those arriving within one year of the focal soldier; and (2) those arriving within 24 months of the focal soldier. In columns three and four the treatment combines the marital status of incumbent and arriving peers over the indicated time horizon. In columns five and six, the peer measures are dis-aggregated to estimate the effect of incumbent and arriving peers simultaneously.

Table A6. Summary Statistics for Select Sub-Samples

	All Enlistees (1)	Arrival Sample (2)	On/After Oct. 2018 (3)	Attrition (4)	Singletons (5)	No Variation (6)	Estimation Sample (7)
Female	0.17	0.11	0.12	0.16	0.18	0.08	0.04
White	0.62	0.61	0.55	0.62	0.57	0.66	0.66
Black	0.19	0.18	0.20	0.21	0.21	0.16	0.14
Hispanic	0.14	0.15	0.18	0.12	0.15	0.14	0.15
Age	21.38	21.21	21.05	20.98	22.06	21.01	20.71
	(3.64)	(3.45)	(3.41)	(3.24)	(4.02)	(3.23)	(2.94)
AFQT Percentile	59.28	57.86	57.43	55.61	60.32	59.25	56.69
	(19.31)	(18.91)	(18.86)	(17.98)	(19.25)	(19.02)	(18.73)
High School Graduate	0.76	0.79	0.83	0.77	0.75	0.81	0.82
Some College	0.06	0.06	0.04	0.06	0.09	0.06	0.05
BA or Higher	0.06	0.04	0.04	0.03	0.08	0.02	0.01
Direct Combat Occ.	0.39	0.53	0.53	0.53	0.34	0.53	0.68
3-Year Initial Contract	0.47	0.57	0.61	0.58	0.41	0.51	0.69
4-Year Initial Contract	0.32	0.29	0.26	0.27	0.36	0.29	0.25
5 or 6-Year Initial Contract	0.19	0.12	0.11	0.11	0.21	0.20	0.06
Married at Entry	0.15	0.14	0.14	0.14	0.24	0.12	0.08
Had Children at Entry	0.09	0.09	0.07	0.09	0.14	0.07	0.05
Completed First Term	0.64	0.78	0.78	0.03	0.88	0.90	0.92
Re-Enlisted	0.36	0.42	0.44	0.01	0.52	0.50	0.46
Observations	1,134,827	359,050	44,662	44,532	108,762	15,841	145,253

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 entered the Army to enable a comparison across each sub-sample. Column one includes all individuals who entered the Army from FY 2002-2019. Column two, the arrival sample, consists of individuals who met basic criteria to be included in the sample, described in detail in Section A.1. Columns three through seven divide the population in column two into five distinct groups. First, column three is made up of individuals who arrived at their first assignment on or after October 2018. We are unable estimate the effect of peers for this group because we cannot observe outcomes for at least 24 months. Column four consists of individuals who leave the Army within 24 months of arriving at their first operational Army unit. For the majority of the analysis we exclude this group from the sample, but we do show that including them does not change our main result. Column five consists of individuals who are unmatched in that they did not arrive with another soldier of the same marital status, sex, occupation, rank, assignment location, month-year of arrival, and initial term of enlistment. Column six consists of individuals who have a match, but have no within-match variation in treatment. This occurs when two matched soldiers are assigned to the same peer group, for example. Column seven is the estimation sample on which we estimate the effect of peers. The sample includes individuals who were unmarried and married upon arrival, although the majority of our analysis focuses on the sample of individuals who were unmarried upon arrival.

Table A7. Peer Effects on Other Outcomes

	In Sample $t + 24$ (1)	Completed First Term (2)	Re-Enlisted (3)
Fraction of Peers Married, $t - 1$	0.017	-0.006	0.069
	(0.026)	(0.021)	(0.040)*
Observations	157,546	122,918	122,918
Clusters	2,472	2,418	2,418
DV Mean	0.860	0.919	0.450
Effect of Moving up IQR (%)	0.128	-0.0380	0.970

Notes. This table presents 2SLS estimates of Equation 2 for the sample of individuals who were unmarried on arrival to their first assignment. The column headings indicate the dependent variable. In column one, the sample is the same as in Table A8, where individuals who exit the Army within 24 months are included. The sample in column two is the same, but the dependent variable is missing for individuals who have not served in the Army long enough to complete their first term. In column three, the sample is the same as Table 3, but the dependent variable is again missing for individuals who have not served in the Army long enough to complete their first term. Interacted fixed effects for sex, job, rank, month of arrival, location, and initial term of enlistment, θ_r, and individual controls are included in all regressions. 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 0.063. For brevity, this statistic is labeled "Effect of Moving up IQR." Column one does not include controls for cumulative months deployed because this control is undefined for individuals who attrit.

Table A8. Peer Effects on Marriage - Including Individuals Who Attrit

	OLS (1)	OLS (2)	Reduced Form (3)	First Stage (4)	IV (5)	IV (6)
Fraction of Peers Married, $t-1$	0.073	0.073			0.088	0.087
	(0.018)***	(0.018)***			(0.027)***	(0.026)***
Fraction of Peers Married at Previous Location (IV)			0.063	0.718		
			(0.019)***	(0.008)***		
Observations	157,546	157,546	157,546	157,546	157,546	157,546
Clusters	2,472	2,472	2,472	2,472	2,472	2,472
R-squared	0.243	0.248	0.248	0.875		
Individual Controls	-	X	X	X	-	X
DV Mean	0.184	0.184	0.184		0.184	0.184
Effect of Moving up IQR (%)	2.497	2.496	1.935		3.029	2.985
Endog. Test p-value					0.439	0.474
F-Stat				603.2		
F-Stat p-value				$< .01$		

Notes. This table presents estimates of Equation 2 for the sample of individuals who were unmarried on arrival to their first assignment in columns 1, 2, 3, 5, 6. In this case, the sample includes individuals who arrived to their first assignment before October 2018 but subsequently left the Army within 24 months. Column 4 shows estimates from the first-stage regression, Equation 4. In columns 1-3, 5, and 6, the dependent variable is an indicator for individual i being married 24 months after assignment to the peer group or at the last observation, whichever is earlier. In column 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, job, rank, month of arrival, location, and initial term of enlistment, θ_r, are included in all regressions. Standard errors, clustered at the company level, are reported beneath each coefficient (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). 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 0.063 and the instrument (row 2) is 0.057. For brevity, this statistic is labeled "Effect of Moving up IQR." Controls for cumulative months deployed are not included in these regressions since this control is undefined for individuals who attrit.