Making Big Decisions: The Impact of Moves on Marriage among U.S. Army Personnel [1]

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Abstract

We use exogenously determined, long-distance relocations of U.S. Army soldiers to investigate the impact of moving on marriage. In an event study analysis, we find that marriage rates increase sharply around the time of a move. Reduced form exposure analysis reveals that an additional move over a five year period increases the likelihood of marriage by 14 percent. Moves increase childbearing by a similar magnitude, suggesting that marriages induced by a move are formed with long-term intentions. We explore implications of our findings for theories of marriage related to search, bargaining, and decision-making costs.

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I. Introduction

Long-distance moves often occur alongside other life events. For example, young people are more likely to move between cities, but they are also at an age where they are investing in education, getting married, and making occupational commitments. Moves will then be correlated with important life decisions because they are also related to age. However, moves themselves may also have causal impacts on important life events. Moves provide a range of new experiences and opportunities, some anticipated and some unanticipated, and they also require careful planning and can be interpreted as a form of human capital investment (Becker 1993, Schultz 1961). These features of moves may lead to important causal impacts on outcomes like career choice and family formation.

Understanding whether long-distance moves causally affect other outcomes is important for two reasons. First, it can tell us more about how major life changes are related to one another. Identifying causal connections between these events can in turn inform our models of behavior. Second, long-distance relocation is increasingly discussed as a policy option for addressing poor prospects due to weak local labor demand or changes in local habitability from climate change. A more complete assessment of the effects of relocation would inform these policy responses.

Do long-distance moves causally affect other life outcomes? Our paper investigates this question by focusing on the effect of long-distance relocations on marriage using administrative personnel data on younger U.S. Army households. Our records contain monthly information on where a soldier is located, whether they are in training or not, their rank in the Army, and a range of other characteristics. The Army moves soldiers based on its staffing needs, over the preference of individuals, and we demonstrate that the frequency, timing and destination of relocations in our Army sample are random, conditional on a set of observable characteristics related to Army job, year of enlistment, and rank. Importantly, our approach eliminates any role for unobserved individual or household characteristics in determining moves, conditional on enlisting in the Army. It also removes any role for economic opportunity for household members as a factor in relocation. This strategy allows us to credibly identify the causal impact of moves on family formation for the population in our data.

We start with the empirical task of identifying whether moves causally affect marriage rates. We use two approaches. First, we conduct an event study analysis where we examine whether marriage probabilities change before, near, or subsequent to a move using a subset of our data that is not currently married. We find that marriage rates rise sharply just before and in the first two months after a move. We then estimate the reduced form relationship between exposure to moves and marriage outcomes, including other marriage-related events like divorce and the presence of children. We find that exposure to an additional move persistently increases the likelihood of marriage. An additional domestic move increases the likelihood of marrying by about 8 percentage points, or 14 percent relative to the mean, in our sample overall.

We then turn to theory to guide an investigation into the reasons for these relationships. We consider three major explanations for why moves might increase marriage formation. The first, search theory, posits that exposure to thicker or otherwise better marriage markets through relocation could increase the likelihood of marriage. However, this does not align with our finding that moves increase marriage formation very close to the time of relocation. Two other theories fit this pattern better. The first is a bargaining model of relationships and marriage, in which marriage might result from changing bargaining positions within a couple induced by a relocation order. The second is a model of costly decision-making, or procrastination, in which a couple could commit to marriage but doing so requires a costly decision process. Moves might induce such a process and thereby lower the cost of deciding to marry, in turn raising marriage rates in the affected population.

We develop versions of both models and show that the role of location differences in earnings opportunities and amenities is a distinguishing feature. We then return to our data to test whether differences in location characteristics can explain the patterns we find. The detailed location history in our data allows us to characterize location assignments on several dimensions, something which is not typically available in data sets of this size on the general population.[2] Our results show that moving has a substantial effect on marriage formation that is independent of location differences. This finding is consistent with a model in which moving lowers the cost of evaluating a potential marriage partner. The impact of moves is little affected by relocation frequency or characteristics, such as might support

[2] Some longitudinal data sets, like the PSID and NLSY, contain annual location information for respondents. The largest of these is the NLSY79, which begins with about 12,000 respondents. This shrinks considerably over successive waves. Our main sample has observations on over 180,000 individuals and includes exact dates of relocations.

a search or bargaining story. Taken together, these results support a role for decision costs in our findings. However, we find evidence that search and bargaining considerations still play a role, if not the predominant one. Furthermore, we find that post-marriage outcomes are similar for marriages formed close to a move and those formed well before or after a move. This suggests that move-induced marriages are similar in our quality measures to non-induced marriages.

Our paper makes two main contributions. The first is that our paper has implications for our understanding of how individuals make major life decisions, and the decision to marry in particular. Levitt (2016) notes that a challenge to understanding such decisions is the difficulty of generating analysis of real world decisions supported by credible identification. Models of marriage formation based on utility maximization of the partners, originating in Becker's foundational models and extending to bargaining models, have had great success explaining trends in marriage, divorce and fertility in recent decades (Lundberg and Pollak 2003; Isen and Stevenson 2010; Rotz 2016; Lundberg, Pollak and Stearns 2017; Pollak forthcoming). However, at the level of the individual decision-maker, factors other than bargaining power within a couple may influence the marriage choice. Our findings suggest that models of the marriage decision might fruitfully incorporate behavioral components such as procrastination, limited attention or cognitive (decision) costs, and adjustment costs (O'Donoghue and Rabin 2001; Alvarez et al. 2011; Rabin 2013 Masatlioglu et al. 2012) Our findings parallel those in Bailey, Beam and Wentz (2018) who find that marriages induced by a change in Vietnam era draft policy were in fact less likely to end in divorce than marriages formed in the absence of draft incentives.

A second contribution of this project is to advance our understanding of the full set of costs and benefits that families face in relocating. Our analysis is particularly relevant to settings in which policy provides strong incentives for relocation. Examples of these include relocation subsidies linked to unemployment insurance; post-disaster rebuilding and relocation efforts, particularly those that induce longer-distance moves; and public housing vouchers.[3] Research shows that moving to a new community can provide substantial opportunities for improved welfare.[4] Yet many households continue to

[3] Caliendo et al. (2015b) evaluate the German relocation assistance program for unemployed job seekers who take jobs in distant markets. Their IV estimates indicate that relocation assistance improves wage and employment outcomes for unemployed job seekers. Gregory (2014) evaluates the impact of rebuilding grants on the location choices of New Orleans homeowners following Hurricane Katrina.

[4] Chetty et al. (2014) highlights the importance of place in determining long-term welfare. Place can also impact neighborhood safety (Katz et al, 2001; Kling et al, 2005) and available school quality (Sanbonmatsu et al, 2006) as well as teen childbearing (Kearney and Levine 2012; Cutler and Glaeser 1997).

live in cities and neighborhoods where outcomes are likely to be poor. The reasons for this decision are not well understood and economists have long assumed that the psychic costs of migration may be substantial. However, there is little direct evidence to substantiate this assumption.[5] Our estimates provide a direct test of whether long-distance moves are disruptive to family formation. Our results run somewhat contrary to concerns about the disruptiveness of moves. Our findings suggest that, at least for young individuals, moves are not disruptive to nuclear family formation. More work is needed to understand the full social and family impacts of moves, and the age caveat is important, but our analysis suggests that relocation may not be uniformly detrimental to key relationships.

Our military sample has strengths and weaknesses for answering our questions of interest. Through its policies, the Army provides a unique opportunity to learn about the causal effects of migration. The families in our data are also of particular interest to policymakers. This is in part because they tend to come from populations that have experienced declining marriage rates in recent decades (Watson and McLanahan 2011) and in part because the active-duty military is still a large population that receives significant federal expenditures.[6] The modal soldier in our data is young, less educated (as they typically enlist prior to attending any college), and will serve in the Army for much of their 20s. Enlisted soldiers tend to come from families living in middle income neighborhoods, and minorities are overrepresented (Carter et al. 2017). The typical spouse in our data is not in the military (over 90%). To the extent that our results are generalizable to the population from which the Army draws its enlisted ranks, they can inform policies that focus on promoting economic opportunity and family stability. A weakness is that selection into the military is substantial under the all-volunteer force, and selection into the Army or marriage to an Army soldier may be related to willingness to relocate. Our design holds these characteristics constant across movers and non-movers, but the treatment effect of a move may differ from that in the general population, as with any treatment effect identified from a subset of the broader population. To address these concerns, we demonstrate robustness of our

[5] In a prominent paper, Kennan and Walker (2011) derive large implicit moving costs in a sample of white, male U.S. workers; and a recent paper exploiting relocation following a volcanic eruption in Iceland finds similarly large implicit moving costs (Nakamura et al 2016). Earlier evidence from the military finds that moves can cause disruptions for children which can place stress on families (Lyle 2006; Engel, Gallagher and Lyle 2010). Additional evidence from economics finds that migration leads to stress-related health conditions (Gibson et al, 2012), although it may help reduce other mental health issues (Stillman et al, 2009). Psychologists have documented a complex set of mental health outcomes associated with migration, with an emphasis in empirical work on transnational migrants (see Bhugra and Jones 2001 and Bhugra and Gupta 2011 for reviews). Beyond these studies, direct evidence on the disruptive potential of relocation is lacking despite indirect evidence of large non-pecuniary costs.

[6] There are over a million active duty service members with approximately three million dependents in their families. For comparison, full-time enrolled community college students totaled 2.4 million in 2006 (Jepsen et al. 2014).

main results to using a sub-sample that, for historical reasons, is likely less selected. Ultimately, we view our analysis as a credible first test of the impact of relocation on marriage rates. Given how little is known about the causal impact of moving on broader life outcomes, we hope sound identification in other populations will emerge to contribute to our knowledge on these questions.

II. Background on Moving in the Army

The Army has unique policies to support both moves and marriages. However, since support for marriage, and family more generally, do not differ between movers and non-movers in the Army, we address marriage and family support in Appendix A for interested readers. In this section, we focus on the Army's relocation supports. As is well-known, the Army frequently moves soldiers across military installations. Army soldiers typically make at least one permanent move every 3 to 5 years, excluding temporary location changes for short training periods and including international moves.[7] The Army's overriding motivation in making these reassignments is to meet staffing needs across its units as older soldiers leave the Army or are themselves reassigned. By moving soldiers around, the Army is able to maintain complete units with the necessary number of people in each rank and occupation. Army policy states: "[T]he primary considerations in reassigning a Soldier shall be the Soldier's current qualifications and ability to fill a valid requirement. Other factors such as availability, volunteer status, TOS [time on station, or current posting length], and other criteria shall be secondary."[8] In other words, the Army will place individuals in locations based on their rank and job, rather than their preferences. Soldiers, particularly at lower ranks, have minimal say in a move. At most, they can refuse one move during an enlistment contract. If they exercise that option, they may be barred from re-enlistment or they will not have say over their next move, which could be to a more undesirable location. For this reason, and likely others, soldiers at lower ranks rarely refuse a move.

The process of reallocating soldiers across units is also highly centralized, further reducing the scope for individual preferences to play a role in relocation.[9] A soldier's first assignment when leaving basic training is dictated by the demands in the month training is completed. Thus, assignment locations

[7] In addition to permanent moves, the Army also sends soldiers to training for up to 6 months. During a training, a soldier does not typically bring his family and the military does not pay for them to move their belongings.
[8] Department of Defense (DoD) Directive 1315.07. http://www.dtic.mil/whs/directives/corres/pdf/131507p.pdf
[9] An alternative military setting in which to study the impact of moves is to follow those stationed at locations that closed during the Defense Base Closure and Realignment Commission (BRAC). Post closings under BRAC, however, were known well in advance to the actual closing, thus making their assignments non-exogenous.

may vary dramatically between graduates of two different basic training classes, even within the same year. Initial assignments for the entire Army are made by a small staff of only a handful of people in the Army's Human Resources Command (HRC), limiting the likelihood that individual preferences are entering the assignment process. Computerized staffing models are continuously used to keep units filled based on staffing priorities, expected and actual retention rates, and unit priorities. These models, along with the ever changing needs of the Army, determine where a soldier could be sent on a follow-up assignment. [10] The information on potential candidates is maintained in the Army's centralized personnel data base and is not provided by sending units, nor do soldiers at the enlisted level observe the set of open positions. A soldier may learn his new location 4 to 18 months prior to a move, although according to personal communication with an officer who previously worked in enlisted assignments, notifications are typically made four to six months out. There are rare instances where assignments are changed or a soldier is informed in a shorter time period that she is moving; however, the Army tries to limit these instances because of concern for the soldier or her family.

As the largest branch of the U.S. armed forces, the Army assigns soldiers across a broad range of locations. During the period of our sample, the Army operated over 50 domestic posts to which soldiers could be permanently stationed. For example, some soldiers may be stationed in Washington, DC, while others are in Ft. Wainwright, Alaska. While similarities will exist between locations in terms of on-post services and housing options, locations vary on local amenities and economic opportunities. Previous research has used similar variation to study the effects of location and re-location on soldiers and their families: Lyle (2006) studies the effects of relocations on children's academic achievement and Lleras-Muney (2010) examines the effects of air quality on military dependents' health outcomes.

Army relocations differ in important ways from long-distance moves by civilians. The most significant of these is that the timing and destination of Army moves – especially at lower ranks -- are exclusively determined by Army human resources, and the consequences for refusing to relocate are severe. Army families also receive a level of support in moving that may be higher than for most civilians. Army relocations by definition guarantee employment in the destination, and soldiers face no real risk of termination if the new job is a poor fit. With a permanent relocation, the Army will pay to pack and

[10] Generally, soldiers who are stationed in the continental U.S. will not move within a year of a previous move, and soldiers who are stationed outside of the continental U.S. will not move within three years of a previous move.

ship all of the soldier's belongings along with the family's household items. The Army may also assist a family with finding new housing, and there is typically a supportive community in the new location that may assist families with adjusting to a new location.

However, these differences are not as great as they may at first seem. Civilians who make long distance moves predominantly say these are for job-related reasons, and in this way they are similar to Army moves (Molloy, Smith, and Wozniak 2014). Just like spouses of civilians, Army spouses who work need to find employment in the new location. Similar to other large employers, the Army provides some assistance in securing employment for relocating spouses, through the Military Family Act of 1985.[11] Schooling or childcare arrangements also need to be made for children in both situations, although for the military high quality options for these may be provided on post.

III. Data and Identification Strategy

We draw our sample from military personnel data for all non-civilian active duty Army employees who served at some point between 1991 and 2013. The data includes a number of demographic characteristics: race, gender, education, AFQT score, age, marital status, and number of dependents. We also have information on where a soldier is located, whether they are in training or not, their rank in the Army, and their pay.

A. The Five Years of Service Sample

We condition our sample to include only enlisted soldiers (non-officers) who stay in the Army through five years of service. The individuals in our sample therefore began their Army employment between 1991 and 2008. Some individuals in this sample will be in their first term of service, while some will have re-enlisted and thus chosen to stay in the Army. We further restrict our main sample to soldiers assigned to posts within the United States during their first five years. We exclude anyone who is stationed abroad at some point during that period (not including deployments overseas).[12] These sample restrictions balance a desire for generalizability against the need to have a sample that has sufficient

[11] This act established the Military Spouse Preference program, which gives military spouses preference over similarly qualified candidates for Department of Defense civilian jobs. The preference is only available around the time that the military member has a change of duty location (a move), and there needs to be a job opening available for this benefit to be advantageous.

[12] Deployment refers to relocation for Army combat activities. As such it differs from the relocations we study. Spouses do not accompany soldiers on deployments.

years of service over which to be subject to relocations. By restricting to moves within the U.S., we have a sample in which relocations are more similar to those taken by the general population.

Summary statistics for our full five years of service sample and, separately, for men are reported in Table 1 and discussed in more detail in Appendix B. The demographic measures reported in the table represent the characteristics of individuals at the end of their fifth year. Table 1 also reports a measure of exposure to moves, *total moves*. This is a simple count of the number of times that an individual moved between cities.[13] We exclude temporary training moves in our count, as it is uncommon for soldiers' families to accompany them on these moves. Our count therefore reflects the number of times a soldier received a new permanent posting, or location assignment. An individual in our sample moves, on average, 0.6 times during their 5 years. Most individuals (52%) in our data will have moved at least once with only 6% of the population moving more than once in five years.[14]

Table 1 also reports descriptive statistics for two important sub-samples. The first is soldiers enlisting prior to 9/11. Following 9/11, both the incidence and extent of active duty deployments increased. This could impact our analysis in two ways. First, deployments are typically taken into account when reassigning soldiers across posts, so a rise in the use of deployments could alter the prevalence and nature of relocation in the Army. Second, higher risk of active duty could affect selection into the Army. Deployments in the period of our data prior to 9/11, while common at 40%, were much shorter and less likely to involve combat as Operation Enduring Freedom (Afghanistan), and Operation Iraqi Freedom did not begin until 2001 and 2003 respectively.[15] The total months deployed is only around four months for those enlisting prior to 9/11. This pattern may have resulted in enlistees in the pre-9/11 period viewing the Army as more like a typical job.

We also show statistics for the subsample of soldiers who originally signed a six year contract when joining the Army. This is a small subsample, but it has two advantages for our analysis. First, by five

[13] We define a move to be a change in a soldier's posted location of more than 70.9 miles. A study by the Census Bureau defines an "extreme" commute to be one that is longer than 90 minutes. The average distance of one of these commutes is 70.9 miles. http://www.census.gov/newsroom/releases/pdf/poster_megacommuting_in_the_u.s.pdf

[14] 48% of the sample does not move, 46% moves once, 5.6% move twice, and less than 0.5% move more than twice. When including all international moves, 25% never move, 48% move once, 23% move twice, and 3.5% move three or more times.

[15] Prior to these operations, the last instance of significant combat operations for the Army was the Gulf War, which concluded in February 1991.

years of service, 78% were still on their original term of service (some chose to re-enlist early), so we observe this group for a long window in the Army but avoid the complication of re-enlistment. Second, later results show that moves are unrelated to demographic characteristics in this group, which is helpful for judging the validity of our identifying assumptions. The table shows that this group looks broadly similar to the full sample except they are slightly more educated and have higher AFQT scores.

B. Identification Strategy

To identify the causal impact of moves on marriage choice, we require that relocations be conditionally independent of unobservable factors that would affect the outcome variables. It would be difficult to defend this assumption using observational data on civilians. The Army process means that soldiers should be randomly reassigned, conditional on the information the Army uses to make its assignments.

We test this assumption using our *moves* variable. Specifically, we check whether factors that determine marriage also determine number of moves. We employ a basic regression-based balance test to assess this. We first regress *total moves* on characteristics of the individual that Army policy clearly states are related to assignment, specifically the individual's job, rank, sex, and the year of observation:

$$Moves_i = c + \theta_{jrys} + \mu_i \qquad (1)$$

θ_{jrys} is a vector of variables that the Army Human Resources Command uses to determine where to station individuals—specifically, military job (MOS), military rank, and year of enlistment (joint) fixed effects. We also interact that with sex of the individual *s*, as restrictions on jobs and assignments for women during this time could affect HRC's decisions. *c* is the intercept. We then regress *total moves* on these same base characteristics as well as observable demographic characteristics using the following regression:

$$Moves_i = c + \gamma' X_i + \theta_{jrys} + \mu_i \qquad (2)$$

which is the same regression as (1) except for the addition of X_i , a vector of demographic characteristics including civilian education, marital status, AFQT score, race, and age. We examine the results

of these regressions for two features. First, we examine whether the demographic characteristics explain a large portion of the outcome variable, as measured by the partial R-squared. If not, this suggests that other characteristics of the individual that we cannot see are also not explaining moves. The results for these regressions are in Table 2. The first two columns show the R-squared results for equations (1) and (2) for all those with 5 years of service. Columns (3) through (8) report the same tests for subsamples split by men, those entering prior to 9/11, and those who originally sign a 6 year contract with similar results. Adding the demographic characteristics increases the R-squared by less than 0.01, off a base of 0.15 to 0.29, when looking at total moves.

The second feature of the balance regressions that we examine are results of an F-Test of the joint significance of the demographic variables. P-values for these tests are included at the bottom of the table. The p-values for the first three groups (All, Men, and Pre 9/11) are all well below 0.05, indicating that the demographic variables in our data are jointly significantly related to the number of moves, despite their very modest explanatory power. Our understanding of the Army reassignment process leads us to conclude that relocations are random, conditional on the information observable to Army Human Resources Command (HRC). It is very likely that all such information is reflected in our data. The fact that the addition of a range of demographics, including AFQT score, contributes so little explanatory power in the balance regressions demonstrates that job, rank, and year are by far the dominant drivers of relocations, consistent with stated Army policy.[16] It is unsurprising that the Army HRC may use the demographic information they have available to fill slots, after conditioning on the primary job, rank and year factors. This would ensure racial, ethnic and gender balance in units. Our balance test analysis suggests such factors are secondary and contribute little to the Army relocation process. Nevertheless, we include these demographic measures in all of our regressions, maintaining the assumption that this is necessary to conditional random assignment.

However, in light of these results we perform several further checks. First, we relax the conditional random assignment assumption that unobservables are uncorrelated with observables. We adjust our coefficient estimates for the type of correlated unobservables assumed in Oster (2017) and present these results in Appendix C. Second, analysis using our Term 6 subsample (Columns 7 and 8) shows

[16] Our approach entails the same requirements as the "selection on observables" identification in, for example, Dillon and Smith (Forthcoming), but has two important advantages: we have detailed knowledge of the location assignment process, and we plausibly observe the complete set of relevant conditioning variables.

that demographic characteristics do not contribute significantly to total moves. The F-test condition of the balance test is satisfied for this group with a p-value of 0.07. This group is of further interest in our robustness analysis because by virtue of their initial contract length, few have made the decision on whether to re-enlist in the Army at the end of five years.

To understand the timing of moves in more detail, we carried out several additional checks. First, we repeated estimations of equations 1 and 2 with alternate characteristics of moves as the dependent variable to more fully assess whether relocation assignments are random. The alternative dependent variables measured: time to a next move and quality of destination based on marriage market density, natural amenities, outside employment opportunities, and rental price of housing. The results were very similar to those in Table 2. As was the case in the Table 2 estimates, some individual demographic characteristics were statistically significant, but their addition to the baseline specification added no explanatory power. We also added a measure of marital status at enlistment to the Table 2 specifications to check whether marriage affected the likelihood of a relocation assignment. Relocations are still conditionally randomly assigned after adding the marriage control. Results from these checks are available upon request.

As a second check, we examined histograms of time to a move to determine whether soldiers might anticipate a move around certain career points. We present a histogram of months to a move for all soldiers with three year initial contracts in Appendix C, Figure 1. Bunching at 17-19 months (halfway through the initial contract) and 36-39 months (after renewal of the initial contract) is apparent in the histogram, suggesting that these times when a relocation is relatively likely for soldiers entering with three year contracts. However, 80 percent of moves take place outside of these times, or "off spike." This suggests that soldiers face considerable uncertainty about when moves will happen. Histograms for soldiers entering on longer contracts are qualitatively similar, although the logical time spikes occur at different points.

IV. Main Impacts: Event Study and Exposure Analysis

We present two analyses of the impact of relocation on marriage: an event study analysis and an exposure analysis. The advantage of the event study analysis is that it allows us to observe how the timing of marriage relates to the timing of relocation. This question is naturally of interest and will provide

one piece of evidence on the mechanism governing any observed relationship. However, an event study design presents a few challenges. First, it is harder to define a sample of interest since many soldiers have no moves, and a few have repeated moves. Second, and relatedly, we are unable to demonstrate exogeneity using the usual balance tests given that we are only examining those who move. Finally, it does not allow us to control for a soldier's full relocation history as does our exposure analysis. The advantage of the exposure analysis, which we develop below, is that it overcomes the challenges of the event study but at the cost of abstracting from how the timing of marriage relates to relocation. We therefore view these two approaches as complementary. Each provides an important part of the picture of how marriage is affected by relocation, and of the mechanisms behind this. We present results from our event study analysis in this section before moving to the exposure analysis and its tests of our identification strategy in the next section.

A. Event Study: The Timing of Marriage Relative to Moves

To conduct the event study analysis, we limit our main five years of service sample to soldiers who meet three criteria: they were unmarried six months prior to their first move; they have no further moves in the next two years; and their first move occurs at least two years before the end of their fifth year. For this group, we can examine patterns in marriage rates before and after a single move. We begin by plotting marriage rates for this subsample in Figure 1 (the blue line). The dotted line represents an alternative sub-sample with meeting the same move and marriage criteria but balanced on age and Army tenure to match the full five years of service sample. We then drew a similar sample of non-movers (labeled "Age-Tenure Matched, No Move"). For this group, month zero refers to the month of Army service when the average individual in the age-tenure balanced sample first moved. "No move" individuals, however, had not moved up to that point and did not move for the next two years.

Figure 1 shows a pronounced increase in marriage rates around the time of a move in both mover samples (blue and dotted lines). Soldiers are typically informed of a relocation four to six months prior to the relocation, and our figure captures this period. In a version of the "parallel trends" test, the figure shows that marriage rates in months minus seven to minus one are comparable for movers and non-movers. This suggests that the groups are marrying at similar rates during the period in which movers are receiving information about new postings. Marriage rates then spike for the mover samples at the end of the period defined by information about a relocation (at the beginning) and the relocation itself (at the end). Different patterns in the post-move period for the two mover samples imply that

balancing on age and Army tenure is important. Marriage rates in the balanced subsample remain higher than in the non-mover group for two years after the "move" date.

To examine the change in marriage propensity around a move more closely, we regress the marital status dummy on a set of age dummies and a linear time trend, and plot the residuals by months relative to a move (again defined as month zero). This shows how marriage rates change around a move after accounting for a strong trend increase in marriage in our sample as well as age effects. Figure 2 shows the results. The figure shows a sharp increase in the share married one to three months after the domestic move. In the two months prior to the move, marriage rates level off from their trend. Consistent with fitting a linear trend through a series with a sharp level shift, the points early in the series are below trend but catching up, while the later points are above trend but decreasingly so. The patterns in Figures 1 and 2 are evidence that moves encourage the conversion of unmarried relationships into marriages, since the marriage surge arrives early in a soldier's new posting, making it unlikely that it formed out of a new, post-move relationship.[17]

One concern with our event study conclusions is that marriages around a move may simply be re-timed. In other words, these are marriages that may have happened regardless of a move, but it may be convenient for them to occur with a move. One source of this convenience might be military housing policy. Married soldiers live in separate housing from unmarried soldiers (or might have the option to live off of the Army post), so conditional on deciding to get married, it makes sense that soldiers would want to be legally married as of a move date in order to move directly into married personnel housing. The plateau in Figure 2 between minus two months and the move suggests some modest re-timing, but of the form that delays rather than accelerates marriage. Our exposure analysis speaks to this more directly, and to preview, we find evidence that the sharp increase in marriage rates at a move is unlikely to be due entirely to re-timing.

Another concern is that individuals may be making re-enlistment decisions simultaneously with their decision to get married. It might be the case that marriages form around re-enlistment plans but that the relationship between moves and marriages in Figure 1 is spurious. We check for this possibility in

[17] We repeated this exercise for the presence of children and for divorce rates (when instead conditioning on those who were married when they entered the Army). There is a small increase in having children around the date of a move, which is likely driven by marriages to a partner who already has children. There is essentially no change in divorce around a move. If we were concerned that the change in marriage rates around the move was driven by a routine updating of the data that month, we would then expect to see similar trends for children and divorce. Results are available upon request.

Figure 3A, which plots the raw (not residualized) probability of being married for the same sample of individuals in Figure 1 from one year before a re-enlistment to two years after (conditional on those who re-enlist).[18] As a related check, we also plot the raw probability of a first move around a re-enlistment, in Figure 3B, which addresses concerns that moves and re-enlistment might be jointly determined. From the graphs, we see fairly smooth changes in the relevant probabilities through the re-enlistment window, lessening our concern that the results in Figure 1 are a function of re-enlistment decisions, as opposed to move effects.

B. The Impact of Exposure to Moves on Marriage

We now turn to our reduced-form analysis of how exposure to moves over a five year period affects marriage propensity. Our goal is to examine causal effects of moves on marriage rates by a point in time, so our main specification is as follows:

$$Y_i = \alpha + \beta_1 Moves_i + \theta_{jrys} + \gamma' X_i + \varepsilon_i \qquad (3)$$

where Y_i is an indicator for whether individual *i* has married by the reference point in time, in our case, by five years of service. θ_{ijys} is the same job, rank, year of entry, and sex joint fixed effects in Equations (1) and (2). The vector X_i includes other background characteristics--specifically civilian education, AFQT score, race, age, and a control for months deployed. These are known to Army personnel when determining relocation, but as we discussed above, it is primarily the variables in θ that determine future job assignments.

Table 3 presents results from estimation of Equation 3. Each panel-column contains results from a separate regression. Panel A reports results for ever married in the course of a subject's five years in the sample, conditional on not being married when entering the Army. Panel B reports results where the outcome is marriage at 5 years of service for those that were and were not married when entering the Army.

Panel A shows that additional moves increase the likelihood that a subject who enters the Army un-married is married by the fifth year of Army service. An additional move raises the probability of

[18] This is a small subsample of about 600 soldiers.

marriage by 8 percentage points. This result represents an increase of almost 15 percent relative to the mean of 0.55. This effect is even larger, both in percentage and absolute terms when we restrict the sample to just men, as shown in the second column of Panel A. An additional move for men prior to their fifth year of service raises their likelihood of marriage by 8.5 percentage points, which is again an increase of 15 percent relative to the mean. Results are similar for those who enter prior to 9/11 and those who sign a six-term contract, with a move increasing marriage rates by 9.2 percentage points (17%) and 6.2 percentage points (12%), respectively, in these subsamples. The impacts for all samples are all significant at the 1 percent level.

In Panel B we see that the net impact (which includes the possibility of divorce and includes those who were married when they entered the Army) on marriage probability *at* five years – versus marriage *by* five years, which is our preferred measure – is still significantly positive and economically substantial. An additional move raises the probability of marriage at five years by 6 percentage points, equivalent to 10 percent at the mean. These persistent effects suggest the marriage rate increase at a move is unlikely to be explained by re-timing. We have also used an alternative sample of soldiers observed in the Army for eight years rather than five, and we find that exposure to additional moves increases the likelihood of marriage by a similar amount over this eight year period. It is even more unlikely that impacts over an eight year window simply reflect re-timing. The persistence of move-induced marriages implies that there exist at least some non-married relationships in which continued information gathering has no marginal benefit.

In Table 4, we show the impact of moves on age of marriage, as well as two other family structure outcomes: presence of children and divorce. Again, we estimate our model using our four main samples and report the results in separate columns. Panel A shows that an additional move decreases the age at which the subject enters marriage by about three months in the full sample and by a similar amount for men only, although the point estimate is not significant. The negative effect of a move on age of marriage is somewhat larger in the pre-9/11 sample and insignificantly different from zero in the Term 6 sample.

Panel B shows that additional moves also increase the likelihood that a subject has children by the fifth year of service. An additional move raises this likelihood by 4.7 percentage points for the full sample, which translates to an increase of 13 percent of the mean. Effects are similar across our four

subsamples. Panel C shows that moves also raise the likelihood that a subject has dissolved a marriage by the fifth year of service. Note that we only observe divorces that occur within the time of Army service, so a soldier must have been married at some point while also serving in the Army to be coded as divorced by the fifth year. The point impact of additional moves on divorce is small, and as observed in Table 3, the net impact of an additional move on marriage at the end of five years is substantial.

Throughout our analysis, we report robust standard errors. Our main estimating equations collapse our data to a cross-section of individuals, and the geographic reassignment that is our source of identification occurs on an individual basis. Unlike many other studies that use policy-induced identifying variation, there is no natural unit (below the individual) at which we might want to account for correlated errors. We prefer the robust standard errors for this reason. However, correlated errors remain a possibility, even if they do not align with our identifying variation. We have experimented with three levels of clustered standard errors to allow for this possibility: initial post location, initial post location by year, and the interaction of job, rank and year. Standard errors for our Table 3 results under these different clustering assumptions are reported in Appendix Tables C1 and C2. We report the full Table 3 analysis in Appendix Table C2, using standard errors clustered on the job-rank-year interactions. These adjustments have little impact on the standard errors, and our conclusions are unchanged under all of the alternative clustering assumptions.

We have now established several facts about the relationship between moves and marriage in our data. First, marriages induced by moves appear to form close to the move chronologically. Second, exposure to more moves over a five year period increases marriage rates at the end of that period by a substantial amount. This suggests that marriages formed near a move do not simply reflect re-timing. If the event study relationships above were driven by re-timing, we would expect that the impacts over longer periods to be small or zero. Finally, moves increase childbearing by about the same amount as they increase marriages. This suggests that moves enhance childbearing among soldiers for whom they also increase marriages, indicating that such marriages are formed with long-run intentions. We view the presence of children as a rough proxy for marriage quality. If marriages induced by moves are intended to be temporary in some way – perhaps they formed simply to preserve the option of continuing the relationship – we might not expect such marriages to lead to childbearing. We acknowledge that this measure is still certainly limited and will not reflect greater marriage quality in all instances.

In the next subsection, we examine the robustness of our main results. We then explore theoretical explanations for these facts.

C. Robustness Analysis

We provide evidence on the robustness of our estimates of the impact of additional moves marriage from Table 3 in two main ways. We first estimate Equation 3 on a variety of alternative time periods and samples to test the sensitivity of our results. We then allow for potential violations of our identifying assumptions in the form of unobservables correlated with our *moves* variable of interest.

Table 5 reports results from estimating our main equations of interest using alternative samples to gauge sensitivity. The panel structure combines outcomes from Tables 3 and 4, while the columns present results for alternative samples. In column 1, we restrict the sample to women. Column 2 shows those who enlisted after September 2001 and were thus more likely to deploy to active combat. Column 3 restricts to those who are at the end of their first-term, regardless of term length (be that 3, 4, 5, or 6 years). [19]

Column 1 shows that among women in our sample, additional moves have a smaller effect than they did for men, increasing the probability of marriage by 3 percentage points, or 5 percent of the mean, however the result is still statistically significant. Moving down the panels, results for women tend to be similar or smaller than for men, although impacts of an additional move are still significant for women and in the same direction as for men across all outcomes. Results in columns 2 and 3 show that impacts of an additional move are similar across outcomes in samples of soldiers who enlisted after 9/11 and when we limit observations to soldiers in their first term only. We conclude that the impacts we identified in our main (Table 3) analysis are not driven by our choice of preferred sample nor are they specific to the specialized subsamples in Table 3.

In Columns 4 and 5, which look at the timing of marriage relative to moves, we restrict our sample to soldiers who have had at least one move and who married while in the Army. Thus this exercise

[19] The first contract subsample is perhaps the most generalizable. However, rates of moves are lower in such a sample. After five years of service, 53% of soldiers will have moved at least once with 7% of soldiers moving more than once. When restricting to just the first contract, which can be for two or three years of service, 90% of the population did not have a permanent move. For the full sample, a move has no effect on re-enlistment (Equation 2), but for certain subsamples, a move does affect re-enlistment: for those not-married when entering, a move increases re-enlistment; for those married when entering, a move decreases re-enlistment.

excludes anyone who was married when entered or did not marry at all. We then separate this group into subsamples based on whether they were married inside or outside a 6 month window of a move. It is first interesting to note that only 32% of those that get married while in the Army get married during a six month window around a move, or three months before or after a move. Second, those who marry within that time window are about one year older (see the mean Age in Panel B) and are half as likely to get divorced – 5% versus 10% (Panel E). For those that get married within the 6 month time window of a move (Col. 3), an additional move increases divorce by 0.7 percentage points, a 14% effect, but it is not statistically significant. For those that get married outside of the 6 month time window (Col. 4), an additional move increases divorce by 17 percent and it is statistically significant. Columns 4 and 5 of Panel C again only include those who have at least one move and who marry while in the Army. For soldiers in both columns, an additional move reduces the likelihood of still being married at 5 years of service by 1 percentage point. This result suggests that additional moves are no more or less disruptive to someone who gets "hastily" married or not. By definition, we cannot analyze the impact of moves on marriage for these subgroups, since a soldier must marry while in the Army to be included in the timing of marriage subsamples. Based on the impacts of additional moves on age of marriage, presence of children, divorce, and marriage at five years, we conclude that marriages that form near a move are similar to those that are less coincident with a move.

We report a related check in Appendix Table C4. We allow higher-order relocations to impact marriage propensity differently than first moves. Soldiers experiencing second and third moves are more likely to be married at the end of five years than those who were only moved once, but the first move has the largest impact on marriage rates.

In Appendix C, Table C3, we report coefficients for total moves both with and without demographic controls. In all cases, the coefficients remain relatively stable across specifications. Adjusted coefficients following the method in Oster (2017), which take into account both the changes in coefficient size and changes in R-squared with the addition of other demographic characteristics, are also reported in the bottom row of each panel. Specifically, the adjusted coefficient is calculated as: $\beta^* = \tilde{\beta} - [\dot{\beta} - \tilde{\beta}]\left(\frac{1.3\tilde{R}-\tilde{R}}{\tilde{R}-\dot{R}}\right)$ where $\tilde{\beta}$ is the coefficient on *moves* in Equation 2 with additional covariates included and $\tilde{R}$ is the R^2 from that regression. $\dot{\beta}$ is the coefficient on *moves* in Equation 2 when no additional covariates are included and $\dot{R}$ is the R^2 from that regression. Hence, this adjustment produces a single

coefficient using the information in the two specifications estimated for each sample in a panel, and serves as a bound with the original coefficient. In every case for the full sample and just men, the coefficient is the same sign and of similar magnitude to our main effects with controls, which we take as evidence that the causal effects we report in Table 3 are accurate in their direction and magnitude.

V. Explanations from Theory

In the preceding empirical analysis, we established three facts about the relationship between moves and marriage in our sample of soldiers: marriage rates increase sharply at a move; additional moves over a soldier's first five years in the Army lead to higher marriage probabilities at the end of five years, implying a persistent effect that is distinct from re-timing; and additional moves increase the likelihood of having children by an amount similar to the marriage increase. Our robustness analysis also showed that outcomes are similar across marriages formed closer and farther from the time of a relocation. We now consider explanations from theory that could generate our findings.

To explain these facts, we first consider a bargaining model of marriage entry and continuation. Given the substantial change in marriage rates around the time of a move, move-induced marriages seem likely to form between individuals in committed but unmarried relationships. A challenge for our application is that existing bargaining models of marriage typically abstract from an option between the two extremes of marriage and an unmarried, single state characterized by active partner search, as the overview in Pollak (forthcoming) notes.[20] We adapt the model of bargaining within marriage in Lundberg and Pollak (2003) to our setting and extend it along the lines in Pollak (2016) to allow for a third state of committed non-marriage. [21] We also add amenities to the model to capture a role for location differences along this dimension, which we later test using our detailed location data. Lundberg and Pollak (hereafter LP) apply their model to a married couple's decision over whether to relocate together or to divorce when one spouse gets an advantageous job offer in another location.

[20] Brien, Lillard and Stern (2006) is an exception that models cohabitation explicitly. However, it is a Jovanovic-style model of learning about match quality, and as such, has little to say about bargaining in marriage. Lundberg, Pollak and Stearns explore trends in cohabitation (2016).

[21] We instead abstract from active partner search. Search considerations are most relevant for individuals outside committed partnerships, and these seem unlikely to explain the main patterns in our data (Keeley 1977, Montgomery and Trussell 1986, or Mortensen 1988). Partner search is time-consuming. Hence, if search factors are relevant for the impact of relocation on marriage rates, we would expect to see marriages form sometime after a move. Instead, we observe a discontinuous change in the rate of marriage formation close to the time of a move followed by a return to the same time trajectory of marriage formation present before a move.

In LP, the lack of a commitment device to adequately compensate a trailing spouse who agrees to relocate despite experiencing a decline in utility is a source of inefficiency. Because such spouses cannot be adequately compensated, some moves do not occur even though such a reallocation would raise the utility of both partners if it were possible. In our adaptation of the model, marriage might serve as a commitment device among unmarried couples that is itself a form of compensation.[22]

We also consider an alternative explanation inspired by insights from behavioral economics about procrastination and costly decision-making. If deciding to convert an existing relationship to marriage is costly, a relocation shock might affect the likelihood of marriage independent of bargaining considerations if it spurs costly thinking about the future. For example, an exogenous relocation might spur a soldier to think through her desired career path in the Army, her eventual transition to civilian employment, her desired place of later residence, or other long-term outcomes. If these considerations are both costly and complementary with considerations necessary for marriage, then relocation may lower the cost to evaluating a potential marriage. This could increase marriage rates if many individuals are in relationships that have not yet been evaluated for marriage. This mechanism operates through a spillover in evaluation costs. As such, it is related to a literature that explains delay in optimal actions through cognitive complexity (Rabin 2013; Greenleaf and Lehmann 1995). This mechanism is also similar in spirit to the rational inattention literature in price setting and consumer and investor behavior. For example, Alvarez et al (2011) develop a model of price updating by firms that face both adjustment (menu) costs and observation costs, i.e. it is costly to determine the optimal price at a given point. Adjusting a relationship to married status and determining whether it should be adjusted might be similarly costly.

To make our comparison of the two approaches more complete, we sketch models of each below before turning to the question of how we might distinguish these in the data.

A. Bargaining Model of Commitment, Marriage and Relocation

[22] This aligns with Matouschek and Rasul (2008), who distinguish between models that explain marriage formation when co-habitation is available. They find evidence, based on divorce law changes, that the decision to form a marriage rather than cohabitate is best explained by a model of marriage as a commitment device that makes dissolving the partnership more difficult.

We begin by considering a couple in a committed but unmarried partnership. As in LP, a couple consists of a leading partner and a trailing partner who share resources. Utility for a committed couple with relationship status *j=c* in location *k=0* is as follows:

$$(4) \quad \left(U^L_{j=c,k=0}, U^T_{j=c,k=0}\right) = (s_0(Y^L_0 + Y^T_0) + a^L_0 + C^L, (1 - s_0)(Y^L_0 + Y^T_0) + a^T_0 + C^T)$$

where Y^L_0 (Y^T_0) denotes earnings and $a^L_0(a^T_0)$ denotes the value of local amenities for the leading (trailing) partner in location *0*. C^L (C^T) is an individual-specific utility benefit to the leading (trailing) partner from being in the committed relationship.[23] More generally, $j \in \{s, c, m\}$ indicates the three possible relationship states of single, committed partnership, and married. If *j=c* or *j=m*, each member of the couple receives an additive utility bonus in addition to the utility they derive from location-specific income and amenities. s_0 is the share of total household income going to the leading partner. We adopt LP's assumption that this share is exogenously determined by relative earnings across the partners in their location *k*, and that it cannot be adjusted by the couple. Hence:

$$s_k = s(Y^L_k, Y^T_k), where$$

$$\partial s_k / \partial \frac{Y^L_k}{Y^T_k} > 0$$

We assume that an *(L,T)* pair forms a relationship determined by the highest level of commitment to which both agree. Hence couples may enter into commitment without marrying if two simple conditions hold within the partnership:

$$(5) \quad C^L > M^L \; and \; (1 - s_0)(Y^L_0 + Y^T_0) + C^T > Y^T_0$$

The L and T roles can be reversed without loss of generality, and the common amenity terms on both sides cancel. Here M^L is the utility benefit to the leading partner from marriage. These conditions mean that the leading partner prefers a committed, unmarried relationship to marriage, and the trailing partner finds this preferable to utility as a single person, which is equal to her individual income.

[23] In contrast to LP, we assume that none of the individual-specific relationship or amenities benefits contribute to the total household resources allocated by s_k. LP assume these benefits contribute to total resources but that they are exogenously allocated by s_k. This does not substantively affect the conclusions drawn from either model.

When the leading partner in such a couple receives orders to relocate to location $k=1$, he can change his relationship status, but he cannot choose not to relocate. The trailing partner can change both her relationship status and her location. It is possible she will agree to relocate while continuing the unmarried, committed partnership. This requires only:

$$(6) \quad (1 - s_1)(Y_1^L + Y_1^T) + a_1^T + C^T > Y_0^T + a_0^T.$$

But if s_1 is large—which may be related to Y_1^T being low as discussed above—then this condition may not hold. The leading partner may then offer marriage to compensate the trailing partner and convince her to relocate. This will be successful if the following holds:

$$(7) \quad (1 - s_1)(Y_1^L + Y_1^T) + a_1^T + M^T > Y_0^T + a_0^T.$$

As long as $s_1(Y_1^L + Y_1^T) + M^L > Y_1^L$, the leading partner will find it advantageous to offer marriage conditional on the trailing partner moving to the new location, even if in the original location he preferred unmarried commitment.

How should we interpret a change of relationship status from committed to married in this case? The bargaining in marriage (BIM) framework from which our model is drawn assumes that members of a couple anticipate bargaining outcomes in future periods when ranking prospective partners (Pollak forthcoming). In the context of such a model, it is possible to interpret this change of relationship status two ways. On the one hand, it may be a response by members of the couple to a truly unanticipated shock. This seems unlikely in our case but may apply in other contexts. A second interpretation is that the couple anticipated the possibility of relocation and the compensation of marriage that a joint relocation would require. The change of status then reflects an anticipated pattern of bargaining as the relationship unfolds. A related question is whether the leading partner's acceptance of M^L from the relationship instead of the preferred C^L constitutes a direct utility transfer to the trailing partner when they marry. This is one possible interpretation. However, it is also possible to interpret the shift in relative utility levels from the relationship status as an asset transfer in which the leading partner gives the trailing some claim (through marriage) to his future income in the event that the marriage dissolves. Marriage functions as insurance under this interpretation and protects the trailing spouse from the risk of lower income and non-marriage in the new location.

B. Common Decision Costs in Marriage and Relocation

To sketch a simple model where there are common costs between marriage and relocation, consider a population *P* of currently unmarried soldiers at risk to be exogenously assigned to relocate in the next period. Some share, p_R, of these soldiers are in relationships that have not yet been evaluated for long-run marriage quality while the rest, $(1- p_R)$, are not in a relationship that could lead to marriage.[24] Assume there is a cost, *C*, to evaluating a relationship for marriage quality. Suppose that p_R and *C* are an equilibrium such that no current relationships would be evaluated for marriage quality by the next period given cost *C*.

For convenience, assume that a share equal to 1 of the population *P* is assigned to relocate next period. This news lowers the cost of evaluating existing relationships for marriage because the relocation shock forces soldiers to think through a variety of long-term considerations, some of which overlap with the process of evaluating a relationship for marriage. Such evaluation costs might be difficult to quantify, but examples include discussions about the importance of ultimately settling close to family, the formulation of medium- and long-run career goals, consideration of the importance of local amenities versus lower housing costs, as well as conversations about child-rearing roles and other long-run aspirations for one's life.

As a result of the decline in *C*, some share δ of the $p_R P$ relationships will be evaluated and found to meet the criteria for marriage, perhaps through a standard expected lifetime utility maximization framework. Thus, we have the following equation for the number of new marriages formed as a result of the exogenous relocation of *P* soldiers:

$$\delta p_R P \qquad (8)$$

This means that $(1-\delta)p_R P$ remain unmarried, whether they continue in their relationship or not. The new marriages in Equation 1 would be the source of a causal increase in marriage rates as a result of relocation.

[24] If marriage quality can only be determined after sufficient "experience," then we could divide p_R into two groups: those for whom enough experience time had passed, and those for whom it had not. Express the former as some fraction α of p_R, αp_R. By the arguments that follow, moves will still increase marriage rates for population P in the short run following a relocation. Longer-run net impacts are ambiguous, due to the loss of experience for a portion of the population P, but can be determined empirically.

C. Distinguishing the Two Explanations

The theories differ on the role of amenities and earnings differences across locations in the marriage decision. In the bargaining model, earnings and amenities differences across locations determine whether marriage is required as compensation to continue a committed relationship. In the common decision costs model, differences across locations are irrelevant. One way to assess the contribution of these models to the marriage patterns in our data is to control for relative earnings and amenities differences across locations. If location characteristics explain rates of marriage formation across different kinds of moves, this would suggest that bargaining is an important factor in our findings. On the other hand, a small role for location characteristics suggests that moves "per se" are important. This, combined with our finding that marriage rates increase sharply around the time of a move, would be consistent with the common decision costs explanation.

VI. Evidence on Theoretical Mechanisms: The Role of Location and Move Characteristics

To explore the role of relative location differences in marriage rates, we add controls to capture a variety of differences across locations. Fortunately our data contain rich detail on where soldiers are located, which we can merge to additional information on local market characteristics. We also have information on a soldier's complete posting history, so we can control for a variety of ways in which one soldier's experience while enlisted might differ from another's.

We construct several measures of local characteristics at the MSA level and merge these to a soldier's location at each point in time. To control for employment opportunities for the non-military spouse, we construct employment-to-population ratios at the MSA-level from county-level data on total employment and population.[25] We also control for local housing costs and natural amenities. Housing costs are measured as median rental price for a two-bedroom unit, as this is likely the relevant part of the market for soldiers living off post. Natural amenities are measured at a fixed point in time using a scale from the USDA. [26] Finally, we construct a measure of relative marriage market quality. This is defined from the soldier's likely perspective, as the share of the population in an MSA of the opposite

[25] Total employment is from annual County Business Patterns (CBP) data, and annual county-population estimates are from SEER data. These employment to population rates are not gender-specific because CBP does not report employment separately by sex. The CPS is too small to accurately measure gender-specific employment at the MSA level.
[26] We construct the marriage market shares and median 2 bedroom rental cost at the MSA level using Census/ACS data and linearly interpolate between years. Natural amenities are measured following USDA (1999).

sex with the same level of education, race or ethnicity, and age range. Assignment to a location with better marriage opportunities might decrease a soldier's willingness to offer marriage to a current partner so we include it as a location characteristic that likely affects the soldier's bargaining power.

We also construct summary measures of a soldier's posting history to control for conditions that may encourage or discourage marriage. We define *longest spell* as the longest time someone spends in a single posting location. After controlling for number of moves, this variable controls for any impact of stability in location on marriage rates.[27] We also control for posting in a soldier's home region using a dummy equal to 1 if a soldier is ever posted there, since proximity to one's home community of family and friends may impact marriage propensity.[28] The expected sign is unclear, since family and friends may either substitute for marriage partners or improve marriage search. Lastly, we control for total distance moved over a soldier's first five years of service. Longer distance moves between postings impose a bigger migration shock on a couple. The impact of move distance on marriage formation is ambiguous in both models. Under the costly decision-making model, move distance may be irrelevant. However, it may also encourage a more serious evaluation of one's life preferences since it is a bigger shock. Under the bargaining model, new locations that are at greater distances may impose greater costs on trailing partners. On the one hand, this could decrease marriage formation if marriage is not a sufficient compensation for loss in the trailing partner's utility from a distant move. On the other, marriage formation may increase as leading partners offer marriage more often to make up for the utility loss their partner experiences. Neither model provides strong a prior for the effect of distance, but both suggest that it may play a role in the decision to marry at a move. We therefore control for this feature of relocation history.

An added benefit of these tests is that they also address concerns that the benefit to marrying in the Army might increase at the time of relocation. For example, it may be more efficient for a couple to marry and search jointly for new housing than to relocate as an unmarried pair and obtain separate housing for the non-Army partner. Similarly, it might be difficult for a non-Army partner to search

[27] Longest spell is not censored at five years, but includes the time an individual spends in his year-five location until the next reassignment. For example, if someone moved after 2 years of service, and then moved again at 7 years of service, the longest spell in a location would be 5 years.

[28] We use the census measures of divisions in the United States (New England, Middle Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain, and Pacific). We have repeated our analysis using both larger (Census region) and smaller (state) geographic units, and the results are not substantively different.

for work in a new location, so a move may induce this partner to exit the labor force and marry simultaneously (perhaps related to tax advantages for non-working spouses). The controls for local housing costs and employment opportunities help address concerns that marriage benefits change differentially at the time of a move because – if so – the propensity to marry should vary with these added controls.

We first incorporate location characteristics and posting history into our exposure analysis. We add the controls described above to the regression specification (3), which produced our main impact estimates. We report the results in Table 6. The first column repeats our main analysis from Table 3 Panel A using the subset of our sample for which we can match our full range of location characteristics. The point estimate of the impact of an additional move on marriage propensity at five years is little changed – rising from 0.79 to 0.84. Column 2 adds location characteristics averaged over all of a soldier's postings and weighted by time in a posting location to the Column 1 specification, while Column 3 adds our measures of posting history. Column 4 includes all additional controls together. Looking at the impact of an additional move across these four specifications, we observe that the impact is modestly smaller after additional controls are included. However, the final column shows that – even after accounting for this range of move conditions – the impact of an additional move on marriage at five years is still an increase of 7 percentage points. This suggests that relocation history and posting location characteristics explain little of the overall impact of additional moves on marriage rates after five years in the Army.

We next extend our event study analysis from Figure 1 to examine patterns across different "types" of moves. We divide our subsample into five groups by type of move: those moving within the U.S. between better and worse marriage markets by one standard deviation in terms of our measure of marriage market prospects (Figure 4A); those moving within the U.S. between better and worse markets by at least one standard deviation in terms of labor market opportunities (Figure 4B); those who are moving to a new city with at least one standard deviation higher or lower cost of housing (Figure 4C); those who are moving to a new city with better or worse amenities (Figure 4D); and those moving from the U.S. to an international post in either Germany or Italy, where spouses typically may accompany soldiers (Figure 4E). The patterns for types of move are very similar to those in Figure 1. This result implies that a large share of soldiers are marrying around the time of their first move, regardless of the amenity differential between domestic locations.

As a final check, we formalize the impact of location characteristics in our event study using a regression restricted to the subsamples in Figure 1. This estimating sample consists of all individuals who were unmarried at enlistment in the Army and who moved at most one time. We construct an annual panel from our monthly data and regress a time-varying indicator for marriage status $\left(\theta_{i,t}^{M}\right)$ on an indicator for moving between years t-1 and t as follows:[29]

$$(9) \quad \theta_{i,t}^{M} = \alpha_0 + \alpha_1 move_{i,t} + move_{i,t}\delta' Z_{i,j,k} + \gamma' X_i + \varepsilon_{i,t}$$

We interact the move indicator with a vector *Z,* which captures differences between the origin and destination locations. *Z* includes variables indicating the move is to a location with a better marriage market; to a better labor market; to a location with more natural amenities; and to a more expensive rental housing market, all measured as standard deviations difference between the origin and destination location's values on these dimensions. *Z* also includes indicators for move distance. *X* is a vector of the same individual demographic characteristics used as controls in all previous specifications.

The results are reported in Table 7. The first column shows that a move in the past year increases marriage rates in this subsample by 10 percentage points. This is similar to impacts of an additional move over five years in the exposure analysis conducted using our full sample. The second column shows that interacting differences between the origin and destination locations has little effect on the impact of a move in the last year. The same is true after adding non-linear controls for distance of the move. Column 4 adds both the distance and relative local characteristics controls to Equation 9, and Column 5 adds (separate) origin and destination fixed effects. Across all specifications, the impact of a move in the past year is little changed, ranging only from 8.6 to 11 percentage points. However, some features of moves do appear to matter, in that they are statistically and economically significant. Longer-distance relocations in particular are significantly more likely to result in marriage.[30]

Now that we have a picture of how location characteristics help determine the impact of moves on marriage, we can discuss what the results imply for the theoretical mechanisms behind marriage. A

[29] We count all moves during year the same, regardless of whether the move was at the beginning, the middle, or the end of the year.
[30] Few domestic moves exceed 2,000 miles in total distance.

robust finding is that controls for location characteristics and relocation history have little impact on the positive effects of relocation per se on marriage probabilities. This finding suggests that the fact of relocation – regardless of origin and destination – is important for marriage timing. We argue this is consistent with an important role for common decision costs, by the logic in the previous section.

VII. Conclusion

We use conditionally exogenous relocations of U.S. Army soldiers to examine the impact of long-distance moves on the propensity to marry. We first present event study analysis showing the timing of marriages around moves. We find that marriage rates rise sharply shortly before and in the first two months after a move. We then use the conditional random assignment of relocations to present reduced-form analysis showing that additional moves encourage marriage, raising the likelihood of marriage and of having children present as dependents, both persistently over a five year period.

These effects are economically significant as well. In our preferred sample, the likelihood of marrying prior to five years of Army service rises by 8 percentage points with an additional domestic move, representing an increase of 14 percent from the mean marriage rate. This result is driven by the impact of the first move. Subsequent domestic moves have positive, but smaller, impacts on marriage likelihood. Additional moves also lower the age of marriage by a statistically significant 3.7 tenths of a year, or about 4.5 months, and raise the likelihood of having children present by almost 13 percent off the mean. These results are robust to a variety of specification changes to using a range of alternative samples in estimation.

To explain these results, we turned to theory. We first ruled out explanations that seemed at odds with our facts. Persistently higher marriage rates among those exposed to more moves suggest that our results are not driven by re-timing of marriages that would happen otherwise. The fact that marriage rates increase around the same time as a move suggests that enhanced marriage search is also unlikely to explain our results. A bargaining model of marriage is more consistent with our facts. Moves induced by relocation assignments may raise the benefit to marrying a partner, leading marriages to coincide with moves as in our analysis. However, moves may also encourage investments in long-term planning, which may be a complement to the marriage decision. We therefore consider an alternative explanation for our results in which long-term planning lowers the cost of evaluating a relationship

for marriage. This leads to an increase in marriage rates at the time of the move if these relationships would otherwise postpone the decision.[31] Our robustness analysis also showed that outcomes are similar across marriages formed more and less close to the time of a relocation.

We test the bargaining and common decision costs models against one another using rich data on location characteristics and a soldier's posting history. We find that factors that should contribute to variation in the impact of a relocation under the bargaining model have little effect on our estimates. We conclude that this is more consistent with the alternative model of common decision costs in long-term planning around a move contributing to the marriage decision.

What does this tell us about the decision to marry? Our analysis suggests that behavioral factors, such as procrastination, may play a role in the decision to marry. Consistent with this, we find that marriages induced by a "nudge" appear just as persistent as marriages that form without one. Our results parallel those in Bailey, Beam and Wentz (2018) in this way. However, it is important to keep two caveats in mind. First, evidence that nudges encourage marriage formation may not tell us much about trends in marriage rates over time. The workhorse bargaining and search models of marriage formation may be better suited to that task. Second, policy changes to encourage marriage may have different impacts in different populations. Importantly, Rotz (2016) finds that changes in the legal age of marriage that prevented early teen marriages led to rising marriage stability. But partners affected by the laws in the Rotz study were almost ten years younger than our affected military families. Policies that affect marriage among twentysomethings (which is the typical age of first marriage in the U.S. for both men and women) may have very different effects from those that affect teen marriage.[32]

Finally, what might our findings tell us about the impacts of relocation in more general settings? Civilians report job opportunities as by far the most common reason for relocation. Army relocations may be more common that civilian moves, but as we have shown, there is considerable uncertainty around the arrival of a distant offer. In this way, Army moves likely similar to the arrival of civilian opportunities that require moving, particularly in a young, less educated, U.S. population. Our findings may therefore also relate to the benefits and costs of policies to encourage job-related migration.

[31] This channel will also lead to persistently higher marriage rates if some non-marriage relationships dissolve if the couple remains unmarried but not if they marry.
[32] Bailey, Beam and Wentz (2018) make a similar point.

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Table 1: Summary Statistics the Five Years of Service Sample of Enlisted Army Members

	All	Men	Pre 9/11	Term 6 Sample
Panel A: Demographics				
Female	0.13		0.15	0.14
Age	26.03	26.01	25.70	26.00
	(3.76)	(3.72)	(3.43)	(3.86)
Black	0.20	0.17	0.24	0.15
Hispanic	0.12	0.12	0.10	0.10
Other Race	0.06	0.06	0.06	0.06
AFQT Score	59.65	60.10	59.75	67.98
	(19.08)	(19.18)	(18.47)	(16.81)
GED	0.10	0.11	0.06	0.07
High School Dropout	0.01	0.01	0.01	0.01
High School Graduate	0.76	0.76	0.81	0.76
Some College / Associates	0.09	0.09	0.08	0.12
College Plus	0.03	0.03	0.03	0.04
Ever Deployed	0.69	0.72	0.40	0.67
Months Deployed	10.30	10.78	3.83	9.86
	(8.81)	(8.81)	(5.76)	(8.54)
In First Contract, Not Yet Renewed	0.19	0.19	0.19	0.78
Stay in through 10 Years of Service	0.44	0.46	0.45	0.33
Currently married to another Military Member	0.07	0.04	0.07	0.07
Ever Married to another Military Member	0.10	0.06	0.12	0.11
Total Moves	0.58	0.58	0.67	0.42
	(0.62)	(0.62)	(0.63)	(0.57)
Panel B: Outcomes				
Ever Married	0.65	0.64	0.62	0.61
	(0.48)	(0.48)	(0.49)	(0.49)
Has Dependent Children	0.46	0.46	0.41	0.41
	(0.50)	(0.50)	(0.49)	(0.49)
Divorced | Married	0.09	0.07	0.07	0.09
	(0.28)	(0.25)	(0.25)	(0.29)
N	182,694	158,592	80,774	22,813

Note: Department of Defense Data. Includes active duty enlisted soldiers that stay in the Army for at least 5 years of service who are never stationed abroad. Pre 9/11 includes those who enter prior to October 2001. Term 6 Sample inclues those whose original contract was for six years. Standard deviations are in parentheses below the means for continuous variables.

Table 2: Tests of Conditional Random Assignment
Dependent Variable: Total Number of Moves

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All		Men		Pre 9/11		Term 6 Sample	
GED		0.047***		0.046***		0.077***		-0.004
		(0.005)		(0.005)		(0.009)		(0.016)
High School Dropout		0.040**		0.044***		0.053**		0.044
		(0.016)		(0.017)		(0.026)		(0.051)
Some College		0.013**		0.010		0.006		-0.012
		(0.005)		(0.006)		(0.009)		(0.015)
College Plus		-0.106***		-0.113***		-0.069***		0.022
		(0.011)		(0.011)		(0.016)		(0.030)
AFQSC		-0.002***		-0.002***		-0.002***		-0.001**
		(0.000)		(0.000)		(0.000)		(0.000)
Black		-0.005		-0.015***		-0.009		0.001
		(0.004)		(0.005)		(0.006)		(0.013)
Hispanic		-0.012**		-0.015***		-0.017**		0.004
		(0.005)		(0.005)		(0.008)		(0.014)
Other Race		-0.001		0.003		0.000		0.021
		(0.006)		(0.007)		(0.009)		(0.018)
Age		0.021***		0.022***		0.033***		0.025**
		(0.004)		(0.004)		(0.008)		(0.011)
Age Squared		-0.000***		-0.000***		-0.001***		-0.000**
		(0.000)		(0.000)		(0.000)		(0.000)
Observations	182,694	182,694	158,592	158,592	80,774	80,774	22,813	22,813
R-squared	0.17	0.18	0.16	0.16	0.22	0.22	0.29	0.29
Mean	0.58	0.58	0.58	0.58	0.67	0.67	0.42	0.42
F-Test p-value		0.00		0.00		0.00		0.07

Notes: This table reports results from regressions of the total number of moves someone has during their first five years on individual characteristics. The odd columns include full interactions of job, rank, year, and sex. The even columns include additional individual controls as designated in the left hand column. P-values on the F-Tests of the joint significance of the individual controls added in the even columns are included in the last row. Columns 5 and 6 include a subsample of individuals who enlisted prior to September 2001. Columns 7 and 8 include a subsample that signed 6 year term contracts when first enlisting in the Army. ***, **, and * denotes significance at the 1%, 5%, and 10% level respectively with robust standard errors reported in parentheses below the coefficient.

Table 3: The Effect of Moves on Marriage Propensity

	(1)	(2)	(3)	(4)
	All	**Male**	**Pre-9/11**	**Term 6 Sample**
Panel A: Ever Married | Not Married when Enter				
Total Number of Moves	0.079***	0.086***	0.092***	0.062***
	(0.002)	(0.002)	(0.004)	(0.008)
Observations	144,254	125,395	64,311	18,200
R-squared	0.129	0.116	0.162	0.261
Mean of Marriage Rates	0.55	0.55	0.52	0.51
Average Number of Moves	0.57	0.56	0.66	0.41
Panel B: Married at 5 Years				
Total Number of Moves	0.064***	0.071***	0.076***	0.048***
	(0.002)	(0.002)	(0.003)	(0.007)
Observations	182,694	158,592	80,774	22,813
R-squared	0.141	0.134	0.172	0.260
Mean of Marriage	0.61	0.62	0.59	0.57
Average Number of Moves	0.58	0.58	0.67	0.42

Notes: This table reports coefficients on the number of moves someone experiences during their first five years in the Army from a linear probability model. The data includes those who enlist between 1991 and 2008. The dependent variable is denoted in the title of each panel. Panel A includes everyone that stays in the Army for 5 years who was not married when they entered, Panel B includes the full five years of service sample. Each regression includes controls for education, AFQT score, race, deployment months, age, age squared, as well as full interactions of job, rank, year, and sex. ***, **, and * denotes significance at the 1%, 5%, and 10% level respectively with robust standard errors reported in parentheses below the coefficient.

Table 4: The Effect of Moves on Other Marriage and Family Outcomes

	(1)	(2)	(3)	(4)
	All	**Male**	**Pre-9/11**	**Term 6 Sample**
Panel A: Age Married | Not Married when Enter and Ever Married = 1				
Average Number of Moves	-0.039**	-0.026	-0.063**	0.092
	(0.019)	(0.020)	(0.027)	(0.074)
Observations	79,944	68,845	33,538	9,196
R-squared	0.271	0.244	0.353	0.451
Mean Age	23.13	23.21	22.88	23.14
Average Number of Moves	0.63	0.63	0.74	0.46
Panel B: Ever Have Dependent Child during 5 Years				
Total Number of Moves	0.047***	0.050***	0.069***	0.031***
	(0.002)	(0.002)	(0.003)	(0.007)
Observations	157,447	136,954	70,068	19,905
R-squared	0.141	0.129	0.170	0.265
Average Likelihood of Children	0.37	0.37	0.32	0.32
Average Number of Moves	0.57	0.57	0.66	0.41
Panel C: Dissolve Marriage | Ever Married during 5 Years				
Total Number of Moves	0.005***	0.004***	0.007***	0.009*
	(0.001)	(0.001)	(0.002)	(0.005)
Observations	118,072	101,810	49,698	13,776
R-squared	0.176	0.102	0.228	0.349
Mean of Marriage Dissolution	0.09	0.07	0.07	0.09
Average Number of Moves	0.63	0.63	0.73	0.46

Notes: This table reports coefficients on the number of moves someone experiences during their first five years in the Army from a linear probability model. The data includes those who enlist between 1991 and 2008. The dependent variable is denoted in the title of each panel. Panel A includes those at the 5 year mark who get married while in the Army, Panel B includes the full five years of service sample, and Panel C includes those who are ever married during those 5 years. Each regression includes controls for education, AFQT score, race, deployment months, age, age squared, as well as full interactions of job, rank, year, and sex. The regressions for Panel B and Panel C also include controls for whether someone was married to another Army member. ***, **, and * denotes significance at the 1%, 5%, and 10% level respectively with robust standard errors reported in parentheses below the coefficient.

Table 5: The Effect of Moves on Other Marriage and Family Outcomes: Alternative Samples

	Women	Post 9/11	End of First-Term	Married Less than 6 Months	Married Greater than 6 months
	(1)	(2)	(3)	(4)	(5)
Panel A: Ever Married | Not Married when Enter					
Total Number of Moves	0.030***	0.068***	0.102***		
	(0.007)	(0.003)	(0.003)		
Constant	0.267	-0.738***	-1.694***		
	(0.174)	(0.080)	(0.041)		
Observations	18,859	79,943	229,888		
R-squared	0.240	0.103	0.146		
Mean of Marriage Rates	0.59	0.58	0.26		
Average Number of Moves	0.61	0.50	0.11		
Panel B: Age Married | Not Married when Enter and Ever Married = 1					
Total Number of Moves	-0.138**	-0.022	0.231***	-0.416***	-0.249***
	(0.060)	(0.026)	(0.032)	(0.067)	(0.060)
Constant	21.998***	22.438***	21.601***	23.786***	22.483***
	(0.175)	(0.077)	(0.056)	(0.153)	(0.114)
				0	0
Observations	11,099	46,406	59,046	14,370	30,874
R-squared	0.413	0.229	0.344	0.436	0.363
Mean Age	22.64	23.31	22.55	23.73	22.81
Average Number of Moves	0.64	0.55	0.18	1.20	1.07
Panel C: Married at 5 Years					
Total Number of Moves	0.014**	0.054***	0.061***	-0.010**	-0.010**
	(0.006)	(0.003)	(0.003)	(0.004)	(0.005)
Constant	-0.426***	-1.033***	-2.366***	1.036***	0.935***
	(0.134)	(0.051)	(0.072)	(0.089)	(0.071)
				0	0
Observations	24,102	101,920	279,298	14,370	30,879
R-squared	0.203	0.120	0.192	0.352	0.287
Mean of Marriage	0.57	0.63	0.37	0.97	0.94
Average Number of Moves	0.62	0.51	0.11	1.20	1.07
Panel D: Ever Have Dependent Child during 5 Years					
Total Number of Moves	0.024***	0.027***	0.038***	0.044***	0.037***
	(0.007)	(0.003)	(0.003)	(0.013)	(0.010)
Observations	20,493	87,379	279,298	14,213	30,156
R-squared	0.251	0.115	0.229	0.341	0.269
Average Likelihood of Children	0.34	0.41	0.24	0.50	0.58
Average Number of Moves	0.61	0.50	0.11	1.20	1.07
Panel E: Dissolve Marriage | Ever Married during 5 Years					
Total Number of Moves	0.011*	0.004*	0.013***	0.007	0.017***
	(0.007)	(0.002)	(0.002)	(0.005)	(0.006)
				0	0
Observations	16,262	68,374	107,957	14,370	30,876
R-squared	0.257	0.151	0.201	0.413	0.316
Mean of Marriage Dissolution	0.22	0.10	0.05	0.05	0.10
Average Number of Moves	0.64	0.55	0.15	1.20	1.07

Notes: This table reports coefficients on the number of moves someone experiences during their first five years in the Army from a linear probability model. The dependent variable is denoted in the title of each panel. The title of each column denotes the conditioning sample: Col. 1 includes women, Col. 2 includes those who entered the Army after September 2011, Col. 3 includes individuals at the end of their first term – regardless of length, Col. 4 includes who get married within a 6 month window on either side of a move, and Col. 5 includes those who get married while in the Army but outside the 6 month window. Panel A includes everyone that stays in the Army for 5 years who was not married when they entered, Panel B includes those at the 5 year mark who get married while in the Army, Panel C includes the full five years of service sample, Panel D includes the full five years of service sample, and Panel E includes those who are ever married during those 5 years. Each regression includes controls for education, AFQT score, race, deployment months, age, age squared, as well as full interactions of job, rank, year, and sex. The regressions for Panel D and Panel E also include controls for whether someone was married to another Army member. ***, **, and * denotes significance at the 1%, 5%, and 10% level respectively with robust standard errors reported in parentheses below the coefficient.

Table 6: Effect of Moves on Marriage Propensity, Adding Controls for Location Characteristics

	(1)		(2)	(4)
	Full Sample	**Location Characteristics**	**Posting History**	**All Characteristics**
Moves	0.084***	0.080***	0.065***	0.070***
	(0.003)	(0.003)	(0.006)	(0.006)
Average Employment to Population		-0.010***		-0.010***
		(0.002)		(0.002)
Average Marriage Market		0.003		0.003
		(0.004)		(0.004)
Average 2br Rental Cost		-0.033***		-0.034***
		(0.003)		(0.003)
Average Ammenities		0.023***		0.023***
		(0.002)		(0.002)
Ever in Home Division			-0.002	-0.000
			(0.004)	(0.004)
Longest Spell			0.007***	0.008***
			(0.002)	(0.002)
Total Distance of Moves			0.016***	0.004
			(0.006)	(0.006)
Total Distance Squared			-0.020***	-0.019***
			(0.005)	(0.005)
Observations	97,973	97,973	97,973	97,973
R^2	0.150	0.153	0.150	0.154
Mean of Outcome	0.53	0.53	0.53	0.53

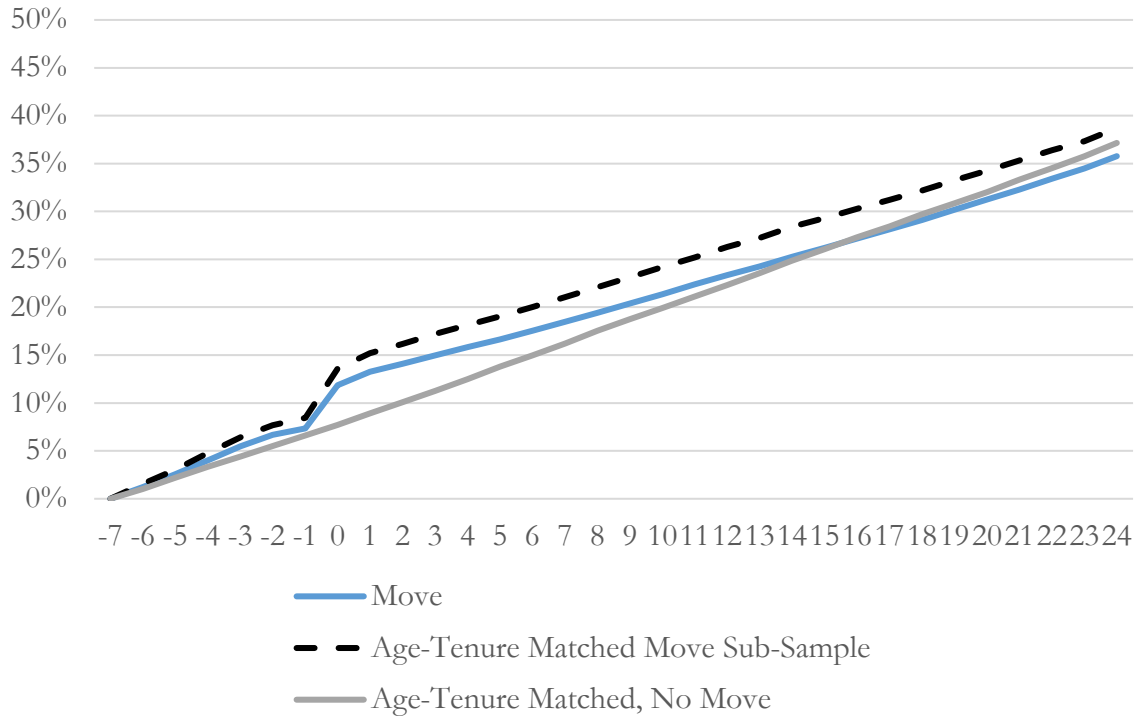
Notes: This table reports coefficients of marriage on the number of moves someone experiences during their first five years in the Army from a linear probability model. The sample restricts to individuals who were not married when they entered. Each subsequent column adds additional characteristics on the type of move people experience. Each regression includes controls for education, AFQT score, race, deployment months, age, age squared, as well as full interactions of job, rank, year, and sex. ***, **, and * denotes significance at the 1%, 5%, and 10% level respectively with robust standard errors reported in parentheses below the coefficient.

Table 7: Probability of Married Status in Year t1, t2, t3, t4, and t5 | Not Married when Enter

	(1)	(2)	(3)	(4)	(5)
Move in a Year	0.107***	0.109***	0.091***	0.092***	0.086***
	(0.002)	(0.002)	(0.011)	(0.011)	(0.010)
Move x Change in Marriage Mkt. Location (Standard Deviation)		0.002		0.002	0.005
		(0.003)		(0.003)	(0.003)
Move x Change in Employment Mkt. Location (Standard Deviation)		0.006***		0.006***	0.002
		(0.002)		(0.002)	(0.002)
Move x Change in Ammenities (Standard Deviation)		0.003*		0.002	-0.002
		(0.002)		(0.002)	(0.002)
Move x Change in Rental Prices (Standard Deviation)		-0.010***		-0.010***	-0.004*
		(0.002)		(0.002)	(0.002)
Moved Less than 300 Miles			-0.006	-0.005	-0.005
			(0.009)	(0.009)	(0.009)
Moved between 300-1,000 Miles			0.021***	0.022***	0.018***
			(0.006)	(0.006)	(0.006)
Moved between 1,000-2,000 Miles			0.031***	0.030***	0.024***
			(0.006)	(0.006)	(0.006)
Moved More than 2,000 Miles			0.003	0.003	-0.005
			(0.032)	(0.032)	(0.032)
Location Fixed Effects					X
Observations	567,770	567,770	567,770	567,770	567,770
R-squared	0.193	0.193	0.193	0.193	0.202
Number of People	113,554	113,554	113,554	113,554	113,554

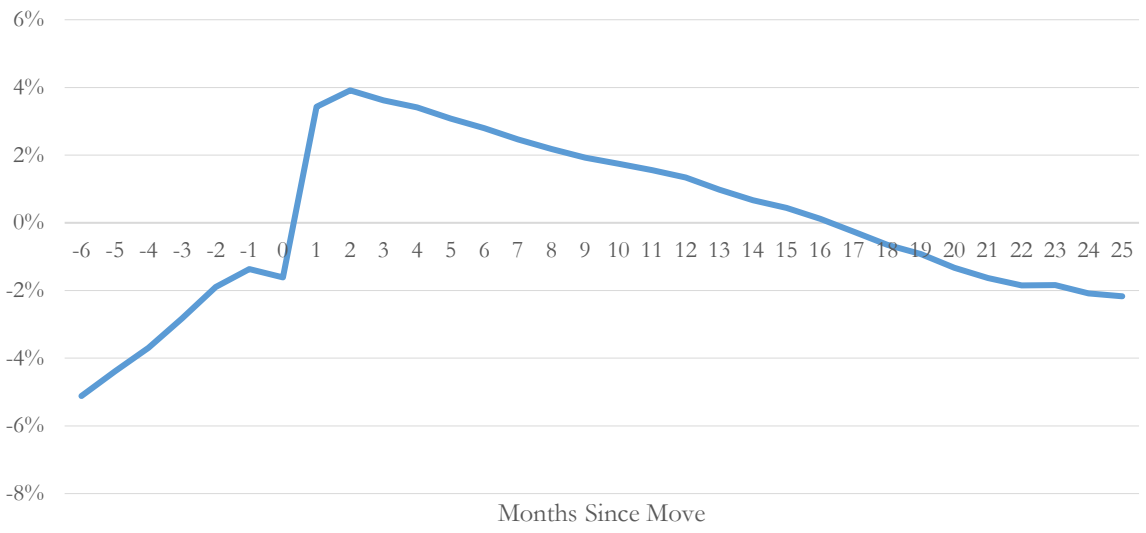
Notes: This table includes results from a regression of being married in a year of service on whether someone moved in that year, for each of the first five years in service. The data includes those who enlist between 1991 and 2008. Each regression includes controls for year of service, education, AFQT score, race, deployment months, age, age squared, as well as full interactions of job, rank, and year. Column 5 includes controls for someone's initial and final location, as well as an indicator if they stayed in the same location.***, **, and * denotes significance at the 1%, 5%, and 10% level respectively with robust standard errors reported in parentheses below the coefficient.

Figure 1. Share Married by Months to First Move



Note: Figure 1 shows marriage rates for three groups of people. The Blue line includes individuals who enlist in the Army for at least 5 years, are not married 6 months before their first move, and who have no further moves and their move occurs at least two years before the end of their fifth year. The dotted line represents an alternative sub-sample meeting the same move and marriage criteria but matched on age and Army tenure to match the full sample from the Blue line sample. For the first two groups, the x-axis represents months since move. For the third group, month zero refers to the month of Army service when the average individual in the age-tenure balanced sample first moved. "No move" individuals, however, had not moved up to that point and did not move for the next two years. All moves are domestic moves.

Figure 2. Share Married by Months to First Move, Residualized



Note: Figure 2 plots the residuals from a regression of marriage on age dummies and a time trend using the "Move Sample" from Figure 1. The x-axis is balanced around the first move (0 is the month of the move).

Figure 3A: Share Married by Months to Re-enlistment

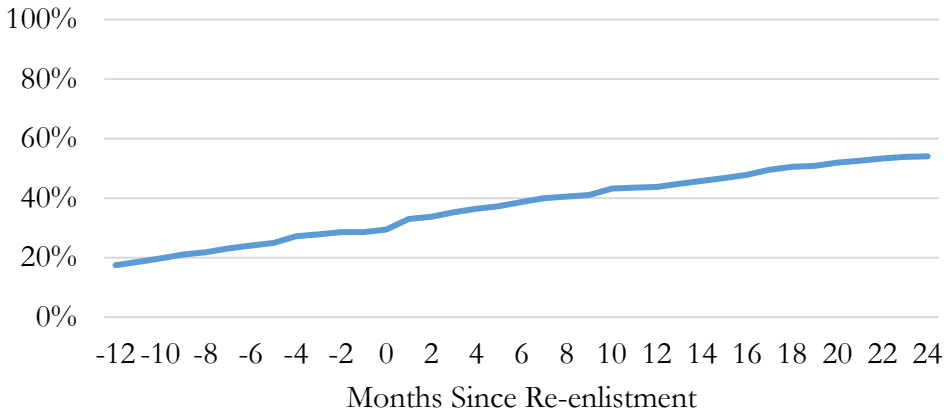
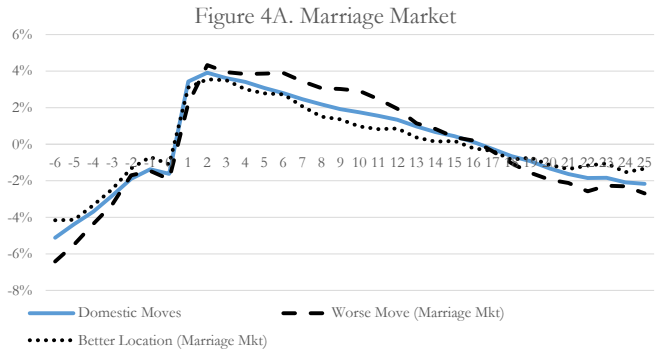
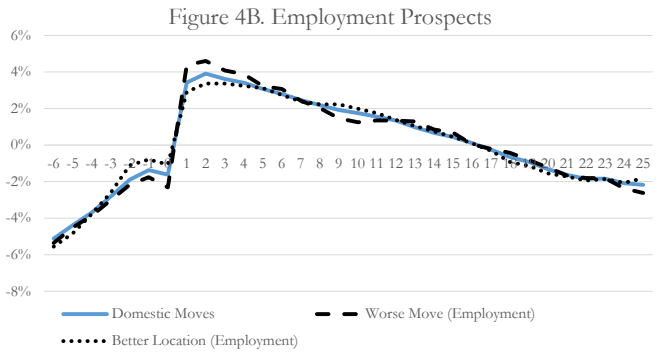


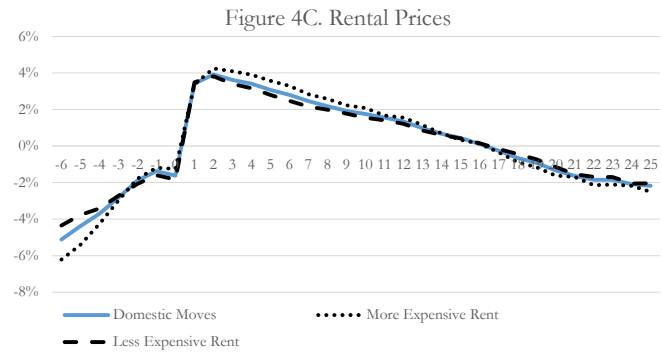
Figure 3B: Share Experiencing First Move by Months to Re-enlistment

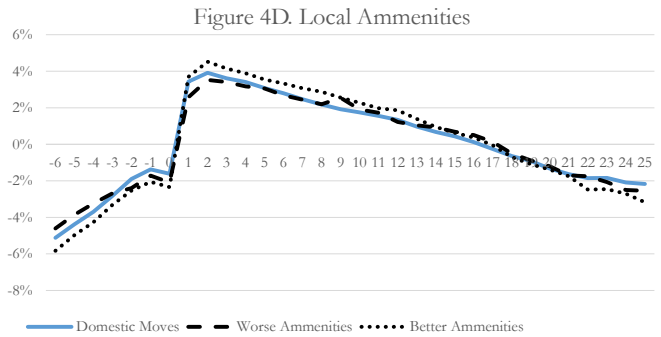
Note: Figures 3a and 3b includes the same sample of people in Figure 1 but further restricts to those who re-enlist during the five year period, and are never stationed abroad during the 5 years of service that we study.

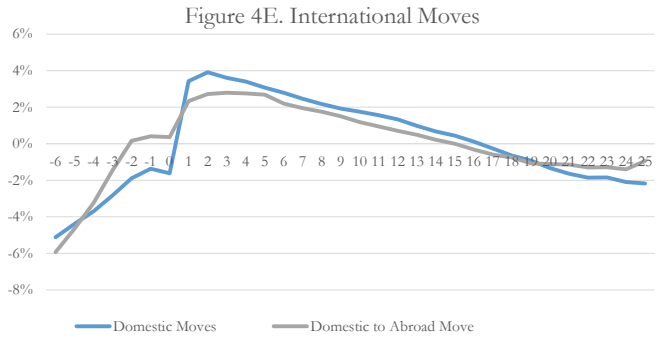
Figure 4. Residualized Marriage Rates by Location of Moves











Note: Figure 4 maps marriage rates surrounding a move. Figure 4A splits individuals by those moving within the U.S. from a worse market to a better market or better to worse marriage market by one standard deviation in terms of marriage market prospects; Figure 4B does the same but for employment to population ratios; Figure 4C looks at moving to more or less expensive housing areas; Figure 4D does the same but by ammenities; and Figure 4E focuses on those moving from the U.S. to an international post (either Italy or Germany).

Appendix A. Additional Background on Marriage Benefits in the Army and the Five Years of Service Sample

Marriage Benefits in the Army

MacDermid et al. (forthcoming) document that the U.S. military views families as a key partner in defense readiness under the all-volunteer force. In particular, they note that Department of Defense directives "… specify an extensive list of required programs and services aimed at supporting families, including deployment support, relocation assistance, child care at subsidized rates, education, care for family members with special needs, programs to improve spouses' access to jobs and careers, counseling, and financial planning assistance." Despite this assistance, MacDermid et al. document that, since 1980, marriage rates in the military had generally converged towards those of civilians, perhaps in part, as they note, because the stress, long hours, and unpredictability of military life may counter the generally supportive environment for marriage within military policy.[1]

The Army supports marriages in a number of ways that differ from the civilian population. First, enlisted soldiers of lower ranks are typically required to live in on-post housing (barracks). If the individual gets married, however, she is allowed to move off post with her spouse.[2] Second, those living off-post receive tax-free housing pay (BAH), in addition to their regular pay, and married soldiers receive a larger BAH than those without dependents.[3] BAH is set by duty location, and an individual without dependents will receive at least 75% of what a soldier with a family will receive.[4]

Family members also receive free health care through TRICARE (the military health care system). As soldiers are often separated from their family for deployments and trainings, during these time periods a soldier is compensated with a $250 monthly family separation allowance.[5] When the Army moves a solider to a new post, the Army will pay for the whole family and their belongings travel to the new

[1] Segal and Segal (2004) describe the military as a "greedy" organization that "requires great commitment, time, and energy." Specifically, they discuss the risk of injury or death, separation from family, relocations, residence in foreign locations, long and unpredictable hours, the need to conform, and the masculine nature of the institution. (p. 32-33)
[2] In some rare situations, on-post housing is overcapacity and soldiers are allowed to move off post.
[3] Housing pay does not increase with the number of dependents.
[4] For a description of how BAH is calculated, see http://www.defensetravel.dod.mil/Docs/perdiem/BAH-Primer.pdf
[5] Military Pay Charts over Time: http://www.dfas.mil/militarymembers/payentitlements/military-pay-charts.html

destination, either by car or plane.[6] [7] [8] Because of these added benefits from marriage, it is possible that some marriages form fraudulently. These added benefits, however, would not differentially effect movers versus non-movers. The only added benefit from being married during a move is a greater weight allowance on a move, an incentive that is likely not great enough to impact marriages.

As servicemembers are compensated more while married there is an incentive to get and stay married.[9] If a couple is married for ten years while serving, the spouse may be eligible for half of the servicemembers' retirement pension. Eligibility for this benefit depends on the state where the individual applies for divorce, although they have a choice over where they apply (home state, previous state where they lived, etc.).

Patterns of marriage and divorce among military members have been examined by previous researchers. However, a strong consensus about these patterns has yet to emerge. This is likely due in part to the fact that participation in the institutions of marriage and the military has changed dramatically across cohorts, so even the basic patterns from descriptive analysis are sensitive to the time period under study. Consistent with this, MacDermid (forthcoming) shows that marriage rates among enlisted military members were considerably below those of the civilian population prior to the 1990s, then rose to levels above those of the civilian population, before converging back to levels closer to, but still above, those of civilians by 2002. Papers focusing on the late 1990s and 2000s find that marriage rates in the military, particularly in samples restricted to males, consistently exceed those of civilians.[10] The differences in marriage rates between civilians and the military have been attributed to differences in selection, younger ages of first marriage among military members, and substantial benefits for marriage in the military. Researchers have also examined divorce propensity across military and civilian populations. Using longitudinal data from the NLSY79, Lundquist (2007) finds that marriages within a military employment spell are more likely to divorce as compared to a matched sample of civilians. However, other researchers find that divorce rates are similar for military members and civilians (Karney, Loughran and Pollard 2012). Similarly, there is disagreement in the literature as

[6] An E4 soldier with a dependent gets an extra 1,000 pounds to transport, an additional $830 in Dislocation Allowance Pay.
[7] Dislocation Allowance (DLA) by rank and year can be found here:
http://www.defensetravel.dod.mil/site/otherratesDLA.cfm
[8] Weight Allowances: http://www.belvoir.army.mil/jppsoma/files/Outbound/WeightAllowance.pdf
[9] Article 134 of the Uniform Code of Military Justice (UCMJ) states that adultery is a punishable offense, although enlisted soldiers are not often prosecuted.
[10] See Karney, Loughran and Pollard (2012) for a summary.

to whether deployments increase the likelihood of divorce (Karney and Crown 2007; Negrusa, Negrusa and Hosek 2014).[11]

Previous papers have also examined the relationship between moves in the military and spousal employment, which could relate to family structure decisions. While causality has not been established, it has been well documented that being a military spouse is associated with higher levels of unemployment and lower wages (see, for example, Castaneda and Harrell (2008), Lim et al (2007), Wardynski (2000), and Harrell et al. (2004)). Inability to find a job as a result of moves could lead to fewer spouses wanting to marry military members or, once married, increases in familial stress as spouses struggle to obtain or maintain a job. Castaneda and Harrell (2008) report the most common reasons for working are related to paying expenses and personal fulfillment, but also boredom. Employment availability in an assigned location may play a significant role in a spouse's happiness.[12]

[11] Some of these differences are likely due to differences in data sources, with some researchers relying entirely on publicly available data such as the CPS and NLSY for both military and civilian samples, and others having access to military personnel data for the military population statistics. Neither is obviously better, as the advantage of higher quality administrative data in military personnel records may be offset by the challenge of trying to construct comparable statistics from public data on civilians.

[12] There is also a literature on the impact of combat deployments on military families. Angrist and Johnson (2000) use military survey data and find that deployments of a male soldier decrease wives' employment rates but that deployments of female soldiers are associated with no change in husband's employment. Deployments of female soldiers are, however, associated with higher rates of divorce. A recent study by RAND finds that marital stress increases during deployments, but marital satisfaction is similar when compared to eligible soldiers who did not deploy (Meadows et al, 2016).

Appendix B. Detailed Discussion of Sample Summary Statistics

The military has traditionally been male dominated, and consistent with that women make up only 13% of our sample. The Army also has a long history of disproportionately high service from African-Americans. Nearly 20% of our overall sample is black, but these rates differ markedly between men and women with a larger share of women (38%) being African American. The Armed Forced Qualification Test (AFQT) is given to all soldiers entering the military. It measures cognitive ability, helps screen individuals into the Army, and helps determine their military occupation within the Army. In our sample, the average AFQT was 60 (the cutoff for entering the Army is a score of 30, and the highest is 100). Women in our sample score slightly lower than men on average. This difference represents about a 3.4 point higher mean for men (56.70 versus 60.10). Although the difference is statistically different at the 1% level, it is economically small when compared to a standard deviation on AFQT score of nearly 20 points for both women and men.

Men and women in our sample are similar on a number of other characteristics. The average soldier is 26 years old, and 12-13% are Hispanic. Because we limit our sample to enlisted individuals, 76 percent are high school graduates, and roughly another 10 percent have some post-secondary education but no BA. The shares with other levels of educational attainment are small. About 20 percent of our sample is still serving a first term without having renewed; the remaining 80 percent have re-enlisted. Ultimately, roughly one-quarter to one-third stay for at least ten years. In our sample, which is censored to include people who are still in the Army, the average soldier is observed in the Army for 8.4 years.

By the time women and men reach five years of Army service, they face notably different family structures, as shown in Panel C. Women in our sample have fewer dependents than men (1.10 versus 1.45) and are much more likely to be married to another service member. Women and men in our sample marry at similar rates, but women are less likely to have children, conditional on being married in our observation period, and are more likely to divorce in that period.

Appendix C. Robustness Analysis and Alternative Specifications

Table C1: Alternative Clustering Methods for Main Table 3 Impacts

	Ever Married	Married at 5 Years
Domestic Moves	0.079	0.064
Robust Standard Errors	(0.002)	(0.002)
Cluster on First Location	<0.012>	<0.010>
Cluster on First Location x Year	[0.004]	[0.004]
Cluster on Job x Rank x Year	{0.003}	{0.002}
Observations	144,254	182,694
R-squared	0.129	0.141
Mean	0.55	0.61
Indep Mean	0.57	0.58

Notes: This table reports results from Table 3 for the full sample of enlisted soldiers who stay through a 5 year term length. Below the coefficient, Robust standard errors are included in (); standard errors clustered on first location of assignment are included in <>; standard errors clustered on first location interacted with first year of assignment are in []; and standard errors clustered on job x rank x year interactions are included in { }.

Table C2: Table 3 Analysis with Clustering on Job x Rank x Year

	(1)	(2)	(3)	(4)
	All	**Male**	**Pre-9/11**	**Term 6 Sample**
Panel A: Ever Married | Not Married when Enter				
Total Number of Moves	0.079***	0.086***	0.092***	0.062***
	(0.003)	(0.003)	(0.004)	(0.009)
Observations	144,254	125,395	64,311	18,200
R-squared	0.129	0.116	0.162	0.261
Mean of Marriage Rates	0.55	0.55	0.52	0.51
Average Number of Moves	0.57	0.56	0.66	0.41
Panel B: Marriage at 5 Years				
Total Number of Moves	0.064***	0.071***	0.076***	0.049***
	(0.002)	(0.002)	(0.003)	(0.007)
Observations	182,694	158,592	80,774	22,813
R-squared	0.141	0.134	0.172	0.260
Mean of Marriage	0.61	0.62	0.59	0.57
Average Number of Moves	0.58	0.58	0.67	0.42

Notes: This table reports coefficients on the number of moves someone experiences during their first five years in the Army from a linear probability model. The data includes those who enlist between 1991 and 2008. The dependent variable is denoted in the title of each panel. Panel A includes everyone that stays in the Army for 5 years who was not married when they entered, Panel B includes the full five years of service sample. Each regression includes controls for education, AFQT score, race, deployment months, age, age squared, as well as full interactions of job, rank, year, and sex. ***, **, and * denotes significance at the 1%, 5%, and 10% level respectively with standard errors clustered by the interaction of job x rank x year reported in parentheses below the coefficient.

Table C3: The Effect of Number of Moves on Marriage with and without Demographic Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All		**Male**		**Pre-9/11**		**Term 6 Sample**	
Panel A: Ever Married | Not Married when Enter								
Total Number of Moves	0.087***	0.079***	0.094***	0.086***	0.098***	0.092***	0.071***	0.062***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)	(0.008)	(0.008)
Observations	144,254	144,254	125,395	125,395	64,311	64,311	18,200	18,200
R-squared	0.118	0.129	0.103	0.116	0.153	0.162	0.246	0.261
Mean of Marriage Rates	0.55	0.55	0.55	0.55	0.52	0.52	0.51	0.51
Average Number of Moves	0.57	0.57	0.56	0.56	0.66	0.66	0.41	0.41
Oster Adjust Coefficients	0.051		0.065		0.060		0.015	
Panel B: Married at 5 Years								
Total Number of Moves	0.074***	0.064***	0.081***	0.071***	0.084***	0.076***	0.060***	0.049***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.007)	(0.007)
Observations	182,694	182,694	158,592	158,592	80,774	80,774	22,813	22,813
R-squared	0.107	0.141	0.094	0.134	0.134	0.172	0.218	0.260
Mean of Marriage	0.61	0.61	0.62	0.62	0.59	0.59	0.57	0.57
Average Number of Moves	0.58	0.58	0.58	0.58	0.67	0.67	0.42	0.42
Oster Adjust Coefficients	0.052		0.061		0.065		0.029	

Notes: This table reports coefficients on the number of moves someone experiences during their first five years in the Army from a linear probability model. The data includes those who enlist between 1991 and 2008. The dependent variable is denoted in the title of each panel. Panel A includes everyone that stays in the Army for 5 years who was not married when they entered, Panel B includes the full five years of service sample. The even columns include full interactions of job, rank, year, and sex. The odd columns includes controls for education, AFQT score, race, deployment months, age, age squared, as well as full interactions of job, rank, year, and sex. ***, **, and * denotes significance at the 1%, 5%, and 10% level respectively with robust standard errors reported in parentheses below the coefficient.

Appendix C.4. Non-Linearities of Additional Moves and International Moves

The table below (Appendix Table C4) looks at the effects of moves taking into account the fact that additional moves may have a non-linear impact on marriage. Column 1 conditions on those who have at most one move while Column 2 conditions on having at most three moves and includes dummies for having 1, 2, or 3 moves compared to having no moves at all. In both columns, a single move increases the likelihood of marriage by 9.3 percentage points. Having two moves increases marriage rates by 13.3 percentage points and having three moves increases it by 15.7 percentage points, suggesting that additional moves increase the likelihood of marriage, but at a diminishing rate.

[Insert Table C4 about here.]

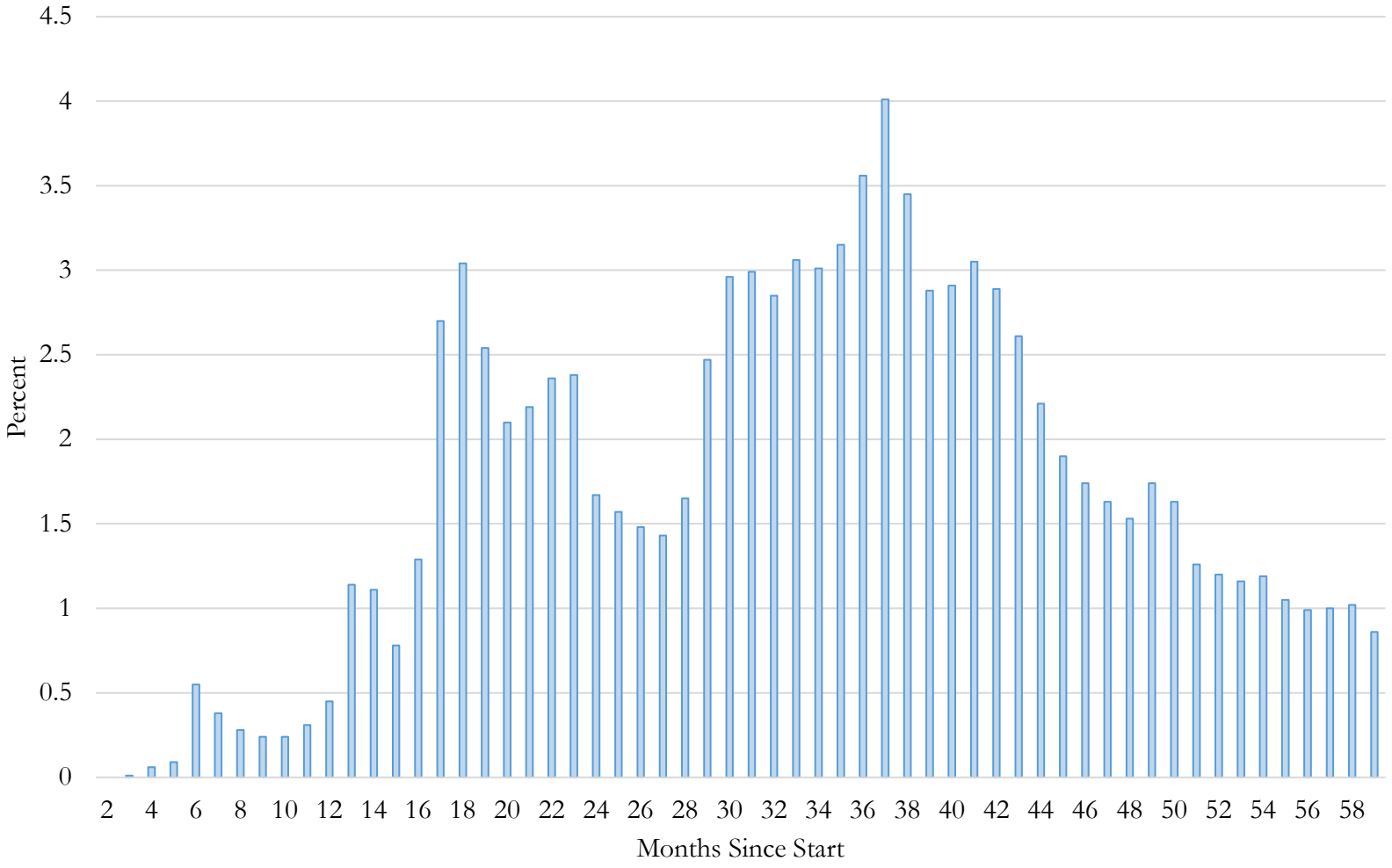
In columns 3 and 4, we expand our sample to include soldiers who are relocated abroad to explore further differences in the impact of moves by distance and frequency. In this sample, additional moves can be either domestic to the US (as was the case in all previous estimates) or international (defined as Germany or Italy, where spouses are typically allowed to accompany a soldier). As shown, even when including international moves, there is a positive effect on marriage rates. Column 5 splits moves into domestic and international moves. International moves have a smaller, but still positive and statistically significant, coefficient.

Table C4: Non-linear & International Impacts of Moves

	(1)	(2)	(3)	(4)	(5)
		Domestic		**Including Abroad Moves**	
	All	**Max 1 Move**	**Max 3 Moves**	**All**	**Max 1 Move**
Moves	0.079***			0.0103***	
	(0.002)			(0.0013)	
One Move		0.093***	0.093***		0.0495***
		(0.003)	(0.003)		(0.0025)
Two Moves			0.133***		
			(0.006)		
Three Moves			0.157***		
			(0.023)		
Total Moves, Including International					
Observations	144,254	134,996	144,192	281,163	206,451
R^2	0.129	0.131	0.130	0.0879	0.1051
Mean of Outcome	0.55	0.55	0.55	0.53	0.53

Notes: This table reports coefficients on the number of moves someone experiences during their first five years in the Army from a linear probability model. The sample restricts to individuals who were not married when they entered. Column 1 and 4 include all moves. Columns 2 and 5 restrict to the sample that has at most one move during the five years and One Move is an indicator variable equal to one if they have one move. Column 3 restricts to a max of three moves with each variable representing an indicator variable. Each regression includes controls for education, AFQT score, race, deployment months, age, age squared, as well as full interactions of job, rank, year, and sex. ***, **, and * denotes significance at the 1%, 5%, and 10% level respectively with robust standard errors reported in parentheses below the coefficient.

Appendix C Figure 1. Distribution of First Moves by Month from Start of Contract



Note: Appendix C Figure 1 reports a histogram of the time soldier have from when they start until their first move during their first five years. The sample only includes soldiers who initially sign up for three year term lengths.