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ABSTRACT

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

We use exogenously determined, long-distance relocations of U.S. Army soldiers to investigate the impact of moving on marriage. We find that marriage rates increase sharply around the time of a move in an event study analysis. 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. These findings are consistent with a model where the marriage decision is costly and relocation lowers the costs to making this decision. Our results have implications for understanding how people make major life decisions such as marriage, as well as the cost of migration.

JEL Classification: J12, J61

Keywords: marriage, migration, relocation, decision-making costs

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I. Introduction

How do people make big decisions? The discrete nature of many important human activities, such as moving to a new city, emigrating to another country, choosing to get married, deciding to have children, or selecting an occupation, means that individuals must often engage in investment-like behavior. That is, they must decide whether to make a substantial change in their current activity, often at a large up-front cost and in a manner that may limit future activities.[2] When modeling these decisions, economists typically assume that individuals are forward-looking and continuously update their estimates of aggregated streams of future benefits and costs to a given activity. However, the complexity of these decisions may mean that, in practice, individuals only thoroughly update their estimates of them infrequently. Additionally, the assumption is often made, at least implicitly, that these decisions are made independently of other major decisions.[3] In reality, if the inputs into a given estimate are common with another decision, then re-evaluating one major decision may have implications for behavior on the other as well.

Take, as an example, an individual who receives a job offer in another state. As a result of this offer, she may update her views on the amenity value of her current local market. If she lives near the mountains, she may realize that all the hiking she thought she was going to do in her current location has proven less appealing than she first thought. This realization may make her value her location less. Independent of how she decides to resolve the job offer, she may then be more willing to accept a marriage offer from someone with roots in the Great Plains. As a result of thinking about her future, she may turn down the job offer but accept the marriage proposal in quick succession because incurring the cost to evaluate the job offer simultaneously lowered the cost of evaluating the marriage proposal. In essence, the option of moving has caused her to refine long-term goals (such as where she ultimately wants to settle, whether she wants to have children, long-term career goals, etc.) that serve as inputs into another decision – getting married.

[2] This formulation aligns closely with Becker's definition of human capital investments in the introduction to the first edition of *Human Capital* (3rd ed. 1993).

[3] Some examples: Kennan and Walker (2011) model individual migration decisions and relate these to expected job opportunities, but abstract from household formation decisions that may depend on migration choices; Brien et al (2006) model marriage formation but abstract from work, location, and fertility decisions.

In this paper, we study the impact of a large, exogenous shock to geographic location on the decision to engage in another long-term investment voluntarily: marriage. Specifically, we use data on U.S. Army households to examine the causal impact of long-distance moves on the likelihood of marriage. Importantly, our approach eliminates any role for unobserved individual or household characteristics in determining the timing and destination of moves. It also removes any role for economic opportunity for household members as a factor in relocation.[4] The Army moves soldiers based on its staffing needs, over the preference of individuals, and we demonstrate that the timing and destination of relocations in our Army sample is random, conditional on a set of observable characteristics related to Army job, year of enlistment, and rank. Individual traits such as risk-taking or openness to new experiences (conditional on enlisting in the Army) are also held constant across migrants and non-migrants through the Army's relocation policy.[5] This strategy allows us to credibly identify the causal impact of moves on family formation for the population in our data. The typical individual in our data is young, enlists in the U.S. Army prior to attending any college, and will serve in the Army for much of their 20s. Of those we observe to five years of service, fifty five percent will get married, and 90% of those will marry a civilian spouse.

We start with a simple model demonstrating how long-distance moves and marriage choices could be linked if the two share common, costly inputs in a model of decision-making. In this model, moves influence marriage rates by lowering the cost of making long-term decisions. This is true regardless of many other characteristics of the move, such as the size of the destination marriage market or the mover's ties to the original location. We next present an alternative search model of marriage to show how factors related to moves, such as social ties to a location and the size of the marriage market, could theoretically impact the decision to get married. The two are distinguishable in that a common

[4] The fact of self-selection in migration is so clear *a priori* that it is in itself a topic of interest to social scientists from across the disciplines. Demographic differences between migrants and the general population have been well-documented (see for example: Greenwood 1969; Greenwood, 1971; Greenwood, 1975). Selection into international migration on the basis of underlying skill has been of interest to those seeking to understand the impacts of immigration (Borjas, Bronars, and Trejo 1992; Kaestner and Malamud 2014; Cortes 2004). Differences between migrants and non-migrants on psychological measures – in particular, the willingness to take risk and locus of control – have been documented by psychologists and economists, and some research has sought to understand the direction of causality in these relationships. For example, Caliendo et al (2015a) model migration as a result of the wider job search strategies adopted by individuals with internal orientation to their locus of control (meaning that these individuals believe that they can strongly influence events in their own lives.)

[5] Another proposed method to measure moves is to study those stationed at locations that close 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.

costs model implies that moves per se should impact the marriage decision, even after controlling for various search-related factors.

We then turn to the empirical task of demonstrating how moves affect marriage rates. We present two analyses of the impact of relocation on marriage in our data. We start with an event study analysis where we find that marriage rates rise sharply shortly before and in the first two months after a move. We then estimate the reduced form relationship between exposure to moves and marriage outcomes, and assess the empirical support for mechanisms in the theoretical model. 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.[6] These include distance from home region; size of the potential marriageable population, or marriage market thickness; prevailing local economic conditions; and length of time a soldier was assigned there. This information helps us assess whether the quantity of moves are the primary driver of our results, or whether the nature of a location assignment is an important moderator.

We find that, overall, moving within the U.S. 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, and by 15 and 5 percent for men and women, respectively.[7] Moves have a non-linear impact on marriage rates, with a single move having the greatest impact while having three moves increases marriage rates by a cumulative 16 percentage points, or 28.5 percent. Our results are robust to studying samples of the population who have and have not made re-enlistment decisions when they move. They are also robust to using only soldiers who enlisted prior to 9/11, during a time when deployments were shorter, less common, and safer. This subsample arguably is less selected relative to the general population of young, middle-skilled men than those who chose to serve after 9/11.

We then investigate the reasons for these relationships. Our results on potential mechanisms suggest that moving lowers the cost of evaluating a potential marriage partner. The impact of domestic moves

[6] 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.
[7] Men make up about 85 percent of our sample and thus have a large impact on point estimates for the sample as a whole.

is little affected by relocation frequency or characteristics, such as might support a search story. We find that relocations lead to increased child-bearing and only a very modest increase in divorce, and we find no evidence that marriages formed around the time of a move are of lower quality, or "rushed," than other marriages.[8] Take together, these results support an evaluation costs story. However, we find evidence that search considerations play a role in our findings, if not the predominant one. Soldiers are more likely to get married if they are assigned to a region where they are from and less likely if the surrounding labor market provides better opportunities for potential spouses.

Our paper has implications for our understanding of how individuals make major life decisions. As Levitt (2016) notes, a major challenge to better understanding of such decisions is the difficulty of generating analysis of real world decisions supported by credible identification. Levitt (2016) approaches this by asking visitors to a website to list a decision they need help making, then providing them with advice the users know is randomly generated via a coin toss. In our setting, the timing and nature of a geographic location change is randomly determined for a large population. We then observe how a second major decision, marriage formation, is affected by the imposition of the first decision. Our findings strongly suggest the presence of common costly inputs into decision-making in large life choices. We are able to test alternative explanations but continue to find a major role for common inputs.

A second contribution of this project is to quantify how moving changes key family ties in an effort to advance our understanding of the full set of costs and benefits that families face in relocating. Research shows that moving to a new community can provide substantial opportunities for improved welfare through a range of mechanisms. Chetty et al. (2014) highlights the importance of place in determining long-term welfare. As noted above, 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).

Despite these benefits from relocation, many households continue to 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

[8] Specifically, additional moves do not differentially increase the probability of divorce among marriages that form close to a move date versus those that do not.

is little direct evidence to substantiate this assumption. 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. Family and social ties, in particular, have been hypothesized to play an important role in limiting relocation, but so far researchers have not been able to credibly identify causal impacts of relocation on these ties. Our estimates, therefore, will directly test whether moves are disruptive to family formation, using a large U.S.-based sample.

Although the Army provides a unique opportunity to learn about the causal effects of migration on families, its members and policies differ from the civilian context in important ways that should be considered when interpreting our results. Clearly, selection into the military is substantial under the all-volunteer force, although as mentioned above, about half our data comes from a period in which selection may have been less pronounced. The military also differs from civilian life in the programmatic support for both marriage and relocation. We have three responses to these valid concerns about the generalizability of our results. The first is that we credibly identify the impact of relocation on Army families, and these families are of independent interest as 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). The Department of Defense also devotes significant resources to supporting military families. As relocation is a major feature of military life, the question of how best to support military families around relocation is of first order interest to military policymakers. In addition, to fill enlisted positions the Army draws from a population that is of interest to a wider set of policymakers. Enlisted soldiers tend to come from families living in middle income neighborhoods, with less than 20 percent of the enlisted population coming from the top quintile neighborhoods in the U.S. (Carter et al. 2017). Enlisted soldiers typically hold only a high school degree or a GED, and minorities are overrepresented, especially

among women. Research has shown that less-educated and minority populations have experienced a decades-long decline in marriage prevalence and stability (Watson and McLanahan 2011). 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 both economic opportunity and family stability.

Our second response to questions about generalizability is that 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 stemming from climate change; and public housing vouchers.[9] To assess the full social costs of these policies, we need to understand their impacts on other life decisions, and on family ties and social networks more broadly. The analysis in this paper is a step in this direction. Third, and finally, we take a number of steps to assess whether our findings can be generalized to the civilian population. These include robustness analysis in which we restrict our sample to men who enlisted prior to 2001, a period in which we argue selection into the Army was less marked. In this way we hope to extend the earlier literature in which researchers have drawn on variation in the military to learn about the causal impacts of civilian policies in other contexts (see for examples: Lleras-Muney 2010; Carrell and Zinman 2014; Carter and Skimmyhorn, forthcoming).

The remainder of the paper proceeds as follows. In the next section, we explore the connections between relocation and family structure suggested by theory. Section III provides background on relocations in the Army and discusses our identification strategy. (For interested readers, Army policies that support marriage are discussed in Appendix A.) Section IV describes our data. Section V includes our event study analysis focused on marriage decisions around a move. Section VI describes the empirical approach to our exposure analysis, and our main results are presented in Section VII, along with a set of generalizability and robustness checks. We explore mechanisms suggested by theory in Section VIII. Section IX concludes.

[9] 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.

II. Theoretical Framework: The Role of Relocation in Marriage Decisions

As a starting point, it is possible that the fact of relocation per se – regardless of the types of markets it provides access to – might affect the likelihood of marriage. Such a situation might arise if there are complementarities in making major life decisions. For example, an exogenous relocation might spur a soldier to think through her desired career path in the Army and eventual transition to civilian employment, her desired place of later residence, or a number of other long-term outcomes. If such considerations are both costly and overlapping with considerations necessary for marriage, then relocation may lower the cost to evaluating a potential marriage. This effect 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) rather than through present-biased preferences (Rabin and O'Donoghue 2001).

To sketch a simple model of this channel, consider a population P of currently unmarried soldiers at risk to be exogenously assigned to relocate in the next period.[10] 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.[11] 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 any 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 could include discussions about the importance of ultimately settling close to family, the formulation of medium- and long-run career goals, and consideration of the importance

[10] For a more detailed model of marriage arising out of dating or cohabitation (and that also incorporates divorce), see Brien, Lillard and Stern (2006).

[11] 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.

of local amenities versus lower housing costs. These kinds of considerations could also lead to relationship-related conversations about child-rearing roles and other long-run aspirations for one's life.

As a result of a lower cost *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. Our approach only assumes that evaluation is costly, not that it deviates from other expected net benefit calculations. Thus, we have the following equation for the number of new marriages formed as a result of the exogenous relocation of *P* soldiers:

$$\delta p_R P \qquad (1)$$

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. Such a model would be consistent with finding a persistent level effect of relocation that is not substantially moderated by controlling for the nature of relocation. This result would also be consistent with a recent finding by Levitt (2016) that individuals can be incentivized to make welfare-enhancing major life decisions by a small exogenous coin-toss intervention, if decision-making by coin-toss is less costly than decision-making by other means.

However, an alternative source of a causal connection between relocation and marriage is the spousal search process. A general version of this process is not likely to imply an unambiguous impact of relocation on marriage, but it is useful to consider how relocation could influence behavior in a basic search model to help motivate later empirical tests of potential mechanisms. Consider a basic marriage market search model like that summarized in Keeley (1977), Montgomery and Trussell (1986), or Mortensen (1988).[12] In an infinite-horizon sequential search model where partners share benefits if a marriage forms, marriage benefits (or quality) are immediately observable, and offers of marriage cannot be held past a single period, the optimal strategy is to adopt a reservation spouse quality. A searching agent declines marriage with a potential spouse in the period in which they meet if the marriage quality that spouse could provide is below the reservation level. If it is at or above the reservation level, the marriage forms and search ends. Intuitively, the reservation quality equalizes the expected lifetime utility gain from forming a marriage with a partner at the reservation quality level with the net

[12] Lippman and McCall (1976) summarize the same simple model in the job search context.

benefit from rejecting such a partner and continuing to search. This yields a well-known result in the search literature: reservation utility is negatively related to the per period net cost of search.[13]

A search framework implies a number of channels through which relocation could alter the probability of marriage. These channels are likely to depend on the nature of the relocation, as well as the fact of relocation per se. Motivated by search considerations, we explore the impact of several features of a soldier's relocation history in our empirical analysis. Specifically, we explore the impacts of relocation histories that vary in access to length of posting, home location, thick marriage markets, and employment opportunities for potential spouses. Moreover, search is a time-consuming process. Hence, if search factors are relevant for the impact of relocation on marriage rates, we would expect to see marriages form some time after a move. If instead the fact of relocation per se is important (as in the costly relationship evaluation framework previously discussed), we would expect to see marriages form around the time of a move. We discuss the potential influence of each feature of relocation histories via a search framework in turn. A key conclusion from this exercise is that search factors imply that relocation history should matter for final marriage rates. In our empirical analysis, we assess search versus costly evaluation on both dimensions: (1) whether marriages occur near a move and (2) whether the nature of relocation histories matters for marriage rates.

Our data allow us to construct several measures of a soldier's relocation history. Our choice of these measures is informed by theory, although theory does not always imply an unambiguous impact of these factors. We first seek to capture the strength of a soldier's community ties. These ties could result from a longer posting spell, allowing ties to form, or from posting to the soldier's home location. If marriage and community are substitutes, we might expect that soldiers who are assigned to their home region or who experience a longer posting spell will marry at lower rates than those with the same number of moves but who are posted to places where they have fewer community ties.[14] Alternatively, if they are complements, postings with more community ties will still matter, but they will lead to higher marriage rates: it may be the case that soldiers have a higher likelihood of matching in markets where they have many social connections. Although access to community ties is unlikely to

[13] This discussion follows models in Keeley (1977), Montgomery and Trussel (1986), and Lippman and McCall (1976).
[14] Marriage and community could be substitutes if stronger community ties may provide a higher utility value of being single.

generate an unambiguous impact on marriage rates, search considerations suggest that this feature of a relocation history may affect marriage rates.

Postings to areas with thicker marriage markets should raise the likelihood of marriage, holding all else equal.[15] Additionally, non-marriage options for potential spouses, namely the labor market, might differ across postings and would be expected to influence the likelihood of marriage. Markets with better labor market options may mean that a given individual faces fewer potential partners who are interested in a match. On the other hand, markets characterized by better overall labor market outcomes may reflect a pool of higher quality potential spouses, with higher human capital. This factor could raise marriage probabilities for individuals assigned to such areas.

Finally, relocations may provide draws from additional marriage markets. If the probability of finding a match declines with time in a market, then relocations could increase the likelihood of marriage formation by allowing agents to sample from a new distribution in which they have a higher chance of matching. This channel might imply that additional moves increase marriage rates regardless of location history, but it would also imply that marriage propensity is increasing in the number of moves.

Later in the paper, we also explore differences in the impacts of international moves versus the domestic moves that are the focus of our main analysis. We treat international moves separately since they likely differ from domestic moves on many dimensions. For example, the pool of potential spouses in an international posting is likely much thinner and search more costly than in a domestic posting, given differences in language and culture. For related reasons, the value of not being married might decline in an international setting, as community ties and other social options are fewer for Americans stationed overseas.

III. Background on Moving in the Army and Identification Strategy

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 the appendix for interested readers. In this section, we focus

[15] We later define marriage market thickness empirically as the share of a geographic area's population in a soldier's age education, and race/ethnicity group, and of the opposite gender.

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. [16] 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."[17] 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 reenlistment or they will not have say over their next move, which could be in 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. A soldier's first assignment when leaving basic training is dictated by the demands in the month training is completed. Thus, assignment locations 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 any individual preferences are entering the assignment process. Computerized staffing models are continuously used to keep units filled based on distribution goals, 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. [18] 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 potential openings. A soldier may learn his new location 4 to 18 months prior to a move, although according to a personal communication with an officer who previously worked

[16] 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.
[17] Department of Defense (DoD) Directive 1315.07. http://www.dtic.mil/whs/directives/corres/pdf/131507p.pdf
[18] 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.

in enlisted assignments, these notifications are typically made four to six months out. There are always 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. 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 by job opportunities for family members and distance from extended family. This variation in move types allows us to measure both the effects of moving, as well as the effects of assignment to areas with higher unemployment rates and away from home and potentially family support. This identification strategy has been employed in previous research 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 at the effects of air quality on military dependents' health outcomes.

Army relocations differ in important ways from long-distance moves that a civilian might make. The most significant of these is that the timing and destination of Army moves are exclusively determined by Army leadership, 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 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). Army spouses who work need to find employment in the new location. The Army provides some assistance in securing employment for relocating spouses, similar to that provided by other large employers, through the Military Family Act of 1985. 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. Schooling or childcare arrangements also need to be made for children, although high quality options for these may be provided on post. Finally, some civilian moves occur because employers require a relocation, and the consequence for declining such a move may be job loss.

IV. Data: The Army Five-Years of Service Sample

We draw our sample from military personnel data for all non-civilian active duty Army employees who served at some point between 1991 until 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.

We condition our sample to include only enlisted soldiers (non-officers) who stay in the Army through five years of service. Our main outcome – ever getting married during the first five years of service – further conditions the sample on those who are not married when they enter the Army. 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. The individuals in our sample therefore began their Army employment between 1991 and 2008. Importantly, we further restrict our 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. These sample restrictions balance a desire for generalizability against the need to have a sample that has sufficient 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. After we discuss results from our preferred five years of service sample, we explore the sensitivity of our analysis to a range of alternative samples including those who have not made re-enlistment decisions at the time they are in the Army for five years and those who enlisted prior to 2001 when the choice to enlist entailed lower risks of deployment.

Summary statistics, both overall and by gender, are reported in Panel A of Table 1. The demographics represent the characteristics of individuals at the end of their fifth year.

[Insert Table 1 Here]

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; 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.

To understand the impact of relocations on family structure outcomes we construct variables that we will use to measure both the frequency and nature of moves. Summary statistics on these measures are reported in Panel B of Table 1. The first measure, *total moves*, is a simple count of the number of times that an individual moved between cities.[19] Our count excludes temporary training moves, as it

[19] 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

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. We also condition our sample on moves that are within the United States and exclude anyone who is stationed abroad. 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.[20]

Other measures in Panel B characterize the moves we observe. We define *longest spell* as the longest time someone spends in a single location to measure the impact of stability in location. This allows us to distinguish between soldiers with the same number of moves but who experienced assignments of different lengths of time.[21] The average length for this variable is four years. We examine separation time away from extended family by measuring the time spent not in the soldier's home division of the United States.[22] Soldiers spend about half of their time away from their home division with an average of 3.1 of their first five years spent on assignment outside their home division.[23] We measure the local economic conditions prevailing in the areas of assignment using the average employment-to-population ratio that a soldier experiences in the course of his location assignments in the first five years. Although soldiers are employed by the Army, local economic conditions could impact family structure through other channels. Foremost among these: spouses or potential marriage partners likely have better labor market prospects in high-employment markets. The average employment-to-population ratio in the areas of assignment averages 48% over a soldier's first five years, which is similar to the yearly national average of 47%.[24] Finally, we create a measure of potential marriage market opportunities in an assignment area. We proxy for this using the average share of the adult population in a

[20] 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.

[21] 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.

[22] 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.

[23] There is some alignment between states contributing large numbers of enlisted soldiers and the presence of Army posts. For example, Texas, Kentucky, Georgia, Washington state, and North Carolina are the top five states in number of Army posts, and 20 percent of our sample are from one of these states. Texas alone contributes 10 percent of our sample, and has several posts.

[24] We use state and county employment totals are from published BEA series for 1969-2011. We combine the BEA employment estimates with state and county population estimates from SEER data available on the NBER website. We

metropolitan area that is of the opposite sex but the same educational and race/ethnicity background as our Army members. On average, about 2.4% of the population is of the opposite sex with the same race, ethnicity, and education background in a given year.[25]

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.[26]

Table 1 also reports descriptive statistics for sub-samples of soldiers enlisting prior to and post 9/11. We define the pre-9/11 enlisted sample as those who enlisted in September of 2001 or earlier. The post-9/11 sample enlisted in October 2001 and later. In later robustness analysis, we check whether our results are sensitive to using the pre- or post-9/11 subsamples. Following 9/11, both the incidence and extent of active duty deployments increased, as is clear from the table. 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 unlikely to involve combat as Operation Enduring Freedom (Afghanistan), and Operation Iraqi Freedom did not begin until 2001 and 2003 respectively.[27] The total months deployed is only around four months for those enlisting prior to 9/11, while it is close to 15.5 months for those enlisting after 9/11. This pattern may have resulted in enlistees in the pre-9/11 period viewing the Army as more like a typical job.

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then aggregate the county-level data to MSAs for soldiers posted to a metropolitan area and to non-MSA state averages for those posted outside an MSA.

[25] We have repeated our analysis using the log size of this population, rather than the share, and results are substantively similar.

[26] Changing administrative treatment of same-sex couples is unlikely to substantively affect marriage our sample. The repeal of the military's "don't ask don't tell" policy occurred in 2010, fairly late in our sample. This lifted the ban on open same-sex relationships but did not provide marriage benefits to same-sex couples. The military did not fully recognize same sex marriages until 2013, when the Supreme Court ruled the Defense of Marriage Act unconstitutional.

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

V. Event Study Analysis: The Timing of Marriage Relative to Moves

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 either have repeated moves or no 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 analyses, 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.

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. We then 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 (defined as month zero). This exercise 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.[28] Figure 1A shows the results for marriage around the time of the first move for those in the above sample who move domestically. The figure shows a sharp increase in the share married two to three months after the domestic move. In the one to two months prior to the move, marriage rates level off from their time trend. The month of the move shows further disruption, with marriage rates in the month of the move well below 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,

[28] We have done the same analysis with the raw, non-residualized data and results are similar, although it is harder to see the plateau and decline in marriage rates just before a move due to the underlying time trend.

while the later points are above trend but decreasingly so. Soldiers are typically informed of a relocation one to three months prior to the relocation. Taken together, these results then suggest that as soldiers receive news of a relocation, their trend rate of marriage slows temporarily until shortly after completion of the move, when marriage rates surge to levels well above trend. This pattern is the first piece of 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.[29]

In Figure 1B, we explore a possible role for conditions of the move by dividing our subsample into five groups by type of move: those moving within the U.S. from a worse to a better market in terms of labor market opportunities; those moving within the U.S. from a worse market to a better market in terms of marriage market prospects; those moving within the U.S. but in the opposite direction, from better to worse markets on those dimensions; and those moving from the U.S. to an international post in either Germany or Italy where spouses typically may accompany soldiers. The patterns for all five types of move are very similar to those in the first panel. This result implies that a large share of soldiers are marrying around the time of their first move, regardless of the marriage or labor market quality differential between domestic locations. There do appear to be modestly greater marriage rates for those who move to worse markets in terms of potential marriage market thickness. We will explore this result further in our exposure analysis.

[Insert Figure 1 Here]

One concern with our event study conclusions is that marriages around a move may simply be retimed. 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

[29] 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 mechanical updating of the data that month, we would then expect to see similar trends for children and divorce. Results are available upon request.

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. Our exposure analysis speaks to this 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 2 is spurious. We check for this possibility in Figure 2A, 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).[30] As a related check, we also plot the raw probability of a first move around a re-enlistment, in Figure 2B, 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.

[Insert Figure 2 Here]

VI. Estimating Equations and Tests for Random Reassignment of Soldiers

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

$$Y_i = \alpha + \beta_1 Moves_i + \theta_{jrys} + X_i\gamma + \varepsilon_i \qquad (2)$$

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_{jrys} is a vector of variables that the Army Human Resources Command uses to determine where to station individuals—specifically, 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

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

and assignments for women during this time could affect HRC's decisions. 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 discuss below, it is primarily the variables in θ that determine future job assignments.

In our main specification, the variable $Moves_i$ is the constructed *total moves* measure, which is simply the total number of permanent, long-distance location changes a soldier experienced during his five years in our sample (excluding temporary location changes for training).

If $Moves_i$ is conditionally independent of unobservable factors that would affect the outcome variables, then $\beta_1$can be interpreted as a causal impact. It would be difficult to defend this assumption using observational data on civilians. The Army, however, uses minimal information when reassigning soldiers to new locations, and soldiers have little to no input into the timing of a move or the location of their new post, as discussed in Section III. This process means that soldiers should be randomly reassigned, conditional on the information the Army uses to make its assignments.

To test for conditional random assignment of total moves we check whether factors that determine number of moves also determine our outcome variables. 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:

$$Moves_i = c + \theta_{jrys} + \mu_i \qquad (3)$$

where θ_{ijys} is the same job, rank, year, and sex of entry fixed effects in Equation (2) and *c* is the intercept. We then regress *total moves* on these same base characteristics as well as observable demographic characteristics. 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 the move. Specifically, we run the following additional regression:

$$Moves_i = c + X_i\gamma + \theta_{jrys} + \mu_i \qquad (4)$$

which is the same regression as (3) except for the addition of X_i , a vector of demographic characteristics including civilian education, marital status, AFQT score, race, and age. The results for these regressions are in Table 2. The first two columns show the R-squared results for equations (3) and (4) for all those with 5 years of service. Adding the demographic characteristics increases the R-squared by less than 0.01, off a base of 0.15 to 0.27, 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 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. Columns (3) through (6) report the same tests for subsamples split by gender with similar results. 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.[31] 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.

[Insert Table 2 Here]

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

[31] 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.

coefficient estimates for the type of correlated unobservables assumed in Oster (forthcoming) and present these results in Appendix C. Second, we identify a subgroup of our data in which demographic characteristics do not contribute significantly to total moves: soldiers who initially enlisted for six year contracts. The F-test condition of the balance test is satisfied for this group (see Appendix C Table C2). This group is of further interest in our robustness analysis because by virtue of their initial contract length, they have not made the decision on whether to re-enlist in the Army. We report estimates from our main analysis restricted to this group as part of our robustness checks in Table 5 Column 1.

VII. Results: The Impact of Exposure to Moves on Marriage

In this section, we present our main results from estimating the impact of additional moves on marriage using Equation 2. We follow this with a summary of further analysis we performed to assess the robustness of the main results, before turning to a discussion of mechanisms in the following section.

A. Estimation Results from Main Specification

Table 3 presents results from estimation of Equation 2. Each panel-column contains results from a separate regression. Panels A-E report results for three marriage outcomes and two other family structure outcomes. These are: ever married in the course of a subject's five years in the sample, conditional on not being married when entering the Army (Panel A); age of marriage, conditional on marrying while in the Army (Panel B); marriage at 5 years of service for those that were and were not married when entering the Army (Panel C); presence of children as dependents (Panel D); and divorce, conditional on prior marriage while in the Army (Panel E).

[Insert Table 3 Here.]

Panel A shows that additional moves increase the likelihood that a subject is married by the fifth year of Army service. An additional move raises the probability of marriage by 8 percentage points. This result represents an increase of almost 15 percent relative to the mean of 0.55. This effect is largest for men, both in percentage and absolute terms, as shown in the second and third columns 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. The results for the men drive the positive impact of moves on marriage in the overall sample. Among women in our

sample, additional moves have a smaller effect, increasing the probability of marriage by 3 percentage points, or 5 percent of the mean. The impacts for men, women, and the combined sample are all significant at the 1 percent level. The positive effect of more moves on marriage rates is also economically significant, particularly for men.

Panel B reveals that moves encourage marriage formation by other measures as well. An additional move decreases the age at which the subject enters marriage by about three months overall, with larger point estimates for women than for men. Finally, in Panel C we see that the net impact (which includes the possibility of divorce) 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.[32]

The remaining panels of the table show the impact of additional moves on two other family structure outcomes: presence of children and divorce. 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. But if moves increase childbearing among soldiers for whom they also increase marriages, this suggests that such marriages are formed with long-run intentions. We acknowledge that this measure is still certainly limited and will not reflect greater marriage quality in all instances. The divorce is of interest as a parallel outcome to marriage, but interpretation of any impacts is less straightforward. Finding that moves increase divorce may suggest that our evaluation costs mechanism applies similarly to exiting marriage as to entering, but it may alternatively indicated that moves are significant stressors on a marriage.

Panel D 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 population overall. The effect is slightly larger for men, and statistically significantly so, and translates to an increase of 13 percent of the mean for men (same as the overall population) and about 7 percent for

[32] In Panel C, we use a broader sample of all enlisted soldiers whom we observe to five years of service. The results are robust, however, to conditioning on being unmarried at the time of entry.

women. We conclude from this that marriages induced by moves tend to form under the intention of lasting for the long-run.

Panel E shows that, in addition to their positive effects on marriage and fertility, 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. These results are larger for women than for men; however, because divorce is much less frequent for men, the impact relative to the mean is about 6 percent for men and closer to 5 percent for women.

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 Table B1. We also report the full Table 3 analysis in Appendix Table B2, 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 view the results of our main exposure analysis as further evidence that the relationship between moves and marriage does not simply reflect re-timing. Exposure to more moves over a five year period significantly increases marriage rates at the end of that period. 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.[33]

[33] 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 unlikely that impacts over an eight year window simply reflect re-timing. Results from this sample are available upon request.

B. Robustness Analysis and Non-linear Impacts

We provide evidence on the robustness of our estimates of the impact of additional moves marriage from Table 3 in three main ways. First, we examine the sensitivity of our estimates to the inclusion (or exclusion) of demographic controls. Our preferred specification in Table 3 includes these controls, but omitting them allows us to further test our assumption of conditional random assignment. We then estimate Equation 2 on a variety of alternative time periods and samples, including the initial six year contract subsample, to test the sensitivity of our results. Finally, we explore whether moves have a non-linear relationship with our outcomes and examine the effects when including international moves in order to provide a more complete picture of how relocation impacts our family structure outcomes.

In Appendix C, Table C1, 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 (forthcoming), 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.

We next report the first of several subsample analyses. Table 4 reports estimates when we split our results by the time period a soldier entered the Army. As noted previously, the nature, frequency, and length of deployments changed following 9/11, which may have altered both selection into the Army and the pattern of regular reassignments. Given these differences, we examine the sensitivity of our results to the use of samples entering before and after 9/11. Overall, we conclude that the impact of additional moves on our outcomes of interest are similar in the two subsamples by time period. Our

results for marriage hold for both period subsamples. An additional move increases the likelihood of marriage by 9.2 percentage points in the pre-9/11 period and by 6.7 points in the post-9/11 period. Men are more likely to get married when they experience an additional move relative to women. A move increases marriage for men by 10 percentage points (20% from the mean) in the pre-9/11 period and 7.4 percentage points (12.8% from the mean) in the post period. For women, however, a move results in a 3.5 percentage point increases (6%) in the pre-9/11 period and by 2.2 percentage points (3.7%) in the post period. The net impact of an additional move on the likelihood of marriage at five years of service (Panel C) is still positive and economically large in both the pre- and post-periods, at 7.6 percentage points or 13 percent in the pre-period and 6.0 percentage points, or an increase of 9 percent from the mean in the post period.

The prior results for divorce rates (panel E), seem to be driven by the pre-period for men. Men are 14% more likely to get divorced with an additional move during their first 5 years if they enlisted prior to 9/11, but the result is no longer statistically significant and the magnitude falls in the post period. For women, however, an additional move does not influence divorce for those entering prior to 9/11, but a move increases divorce rates by 1.8 pp (7 percent) for those entering post 9/11.

[Insert Table 4 Here.]

Table 5 reports results from estimating our main equations of interest using alternative samples to gauge sensitivity. The panel structure is the same as previous tables, while the columns present results for alternative samples. In columns 1 and 2, our main specifications are estimated on samples defined using alternative contract lengths, rather than five years of observed service. Results from the sample of soldiers whose first term was six years are reported in Col. 1, and results from those observed at the end of their first term (be that 3, 4, 5, or 6 years) are reported in Col. 2. These subsamples are of interest for several reasons. Because our main sample contains a large number of individuals who have already chosen to re-enlist, it is reasonable to ask whether estimates from our preferred five-year sample might differ across soldiers who originally selected different contract lengths, or across those who re-enlist or do not re-enlist.[34] Soldiers may also negotiate the location of their next move at the time

[34] 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

of re-enlistment. If this negotiation is occurring based on marriage or family decisions, our original results could be biased. By looking at the period of the initial contract only, we reduce this risk. Additionally, as shown in Appendix C, Table C2, the subsample of soldiers enlisting for initial six-year contracts passes both elements of the balance tests in Table 2.

The results in Columns 1 and 2 show that an additional move significantly increases the likelihood of ever marrying regardless of initial term length. The size of the effect is somewhat smaller but still economically meaningful (6.2 percentage point increase) when we restrict to those with an initial term of six years, and it is larger, 10.2 percentage points on a mean of 26%, for a sample that observes soldiers at the end of their first contract, regardless of length. Results in both columns confirm that the results hold even for those who have not yet re-enlisted.

[Insert Table 5 here.]

In Columns 3 and 4, 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.[35] 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. Panel C is a little more informative on this topic. Columns 3 and 4 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

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.

[35] Thus this exercise excludes anyone who was married when entered or did not marry at all.

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 have so far found that additional moves lead to higher marriage rates. Table 6 next looks at the effects of moves while 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 each move has a diminishing effect.

[Insert Table 6 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.

VIII. Further Evidence on Mechanisms: The Role of Move Characteristics in Moderating the Impacts of Moves

In addition to number of total moves, a soldier's relocation history can differ in a number of ways; for example, how her career is divided into spells in different locations and whether any assignments were in a more familiar part of the country. As we discussed theoretically in Section II, characteristics of posting locations and timing could influence marriage rates differentially. To explore the role that the nature of relocation plays in family structure outcomes, we re-estimate Equation 2 adding in additional

summary measures of a subject's location history. This specification allows us to answer the question: Does the nature of a relocation history – e.g. more time in a home region – matter for family structure outcomes, controlling for number of moves? Specifically, we estimate the following equation:

$$Y_{it} = \alpha + \beta_1 Move_{it} + \beta_2 condition_{it} + \beta_3 condition_{it} * Move_{it} + \theta_{jrys} + X_{it}\gamma + \mu_{it} \qquad (5)$$

Here, *condition* is one of five summary measures of a soldier's relocation history, or moderators: an indicator if the solider is ever stationed in their home region; the longest spell length someone is in a single location; a measure of the average employment to population ratio that an individual faces; a measure of total distance traversed between location assignments; and a measure of the marriage market prospects in a location. For each of these variables (except the home region dummy), we have normalized them to have a mean of zero and a standard deviation of 1, and t is again fixed at five in our reported estimates. We interact each with the number of moves an individual faces because we are interested in both the main effects of these conditions and in their interaction with total moves. The main effect estimates allow us to answer questions about how the types of locations a soldier is exposed to affect marriage, while the interactions indicate whether certain types of relocation histories moderate or enhance the average effect of an additional move. We report the results for marriage in Table 7. We discuss their ultimate implications, in light of the theory, at the end of this section, after all results have been presented.

Focusing on Table 7, the first column repeats our previous estimation of Equation 2 and reports the effect of an additional move on the likelihood that a soldier gets married by five years of service. Column 2 adds the indicator for ever stationed in home region and its interaction with total moves to Equation 2. The results show that a soldier who is stationed in his home region at some point is 1.0 percentage points more likely to marry, but the effect of a home assignment decreases the positive effect of moves on marriage (by 2.3 percentage points or 4.2%). As shown in Column 3, having a longer spell length in a single location enhances the move effect by 1.4 percentage points but spell length has a statistically zero main effect on marriage. The impact of moves per se (the coefficient on total moves) is only modestly affected by adding controls for these types of location history, implying that the main impact of moves on marriage propensity is robust across assignments that allow for more or fewer ties to the local community.

[Insert Table 7 about here.]

Column 4 adds a control for the average labor market quality experienced by a soldier across her assignments. This variable is measured as the average employment-to-population ratios in the MSAs, weighted by the time spent in each location that a soldier was assigned. Local labor market conditions may affect marriage formation if they provide stronger outside (non-marriage) options for the substantial majority of spouses who are non-military. Consistent with this theoretical impact, average employment to population ratio experienced is negatively related to marriage in our sample. As mentioned, this variable is standardized to mean zero and standard deviation one, so the estimate in Table 7 implies that assignment to labor markets that are on average one standard deviation stronger, as measured by employment ratios, reduces the likelihood of marriage by 1.46 percentage points. However, experienced local economic conditions have no effect on the overall impact of additional moves on marriage formation. Additional moves are marriage enhancing, even if soldiers stationed in better labor markets are less likely to marry overall. In unreported results, we condition the sample just on women. Average employment ratios only reduce marriage likelihood by 0.5 percentage points and the result is not statistically significant. This finding suggests that women's marriage rates are not affected by the local labor market conditions in their posting location.

In column 5 we show results adding a control for and interaction with the total distance someone travels for their moves. The effect of the total number of moves is still positive and statistically significant. The results imply that moving a total distance of one standard deviation more than the average will increase the likelihood of marriage by 3.5 percentage points (2.5 percentage points for women) but that for each additional move, this impact is diminished.

Lastly, in column 6 we measure the effects of a "thicker" marriage market, defined as the share of the population in an MSA of the opposite sex with the same level of education, race or ethnicity, and age range. We control for the MSA's average employment to population in this regression, since larger MSAs may have both larger shares of their populations in marriageable ages and have stronger outside employment opportunities. We find a negative effect overall from being in a location with better marriage prospects of 1.49 percentage points, even after controlling for the local employment rate. However, this negative effect is reduced with an additional move.

In column 7 we examine whether total moves continue to have the effects we saw in our baseline analysis in Table 4 after controlling for the full set of location history summary measures and interactions. The coefficient on total moves remains relatively constant and statistically significant. Even when controlling for the types of places people spend time, more moves increase the likelihood of marriage by 5.6 percentage points, or ten percent of the mean.

Now that we have a picture of how location characteristics help determine the impact of moves on marriage, we can discuss what the results in Table 7 imply for the theoretical mechanisms behind marriage that we outlined in Section II. A 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 positive effect of relocation on marriage is not primarily explained by access to better marriage markets, as might be the case if search frictions were a major determinant of marriage propensity.

The positive, but diminishing, impacts of successive moves in Table 6 provide further suggestive evidence of a role for decision costs. Additional moves provide new opportunities to evaluate the state of an existing relationship, successively increasing the size of δ in Equation 1, and ultimately the total number of married individuals out of those in an unmarried relationship.

Finally, our results suggest that search factors play some role. We find that search factors generally operate in the expected direction on marriage propensity, although there are some puzzling findings. Posting to one's home region (defined as Census divisions) and having a long-term posting increase the likelihood of marriage very modestly, and posting to markets with better labor market options decrease this likelihood. These findings are what a standard search model would predict if search is lower cost in an area one knows well and if marriages are less likely when outside options are better. However, posting to a thicker marriage market actually decreases the likelihood of marriage, even after controlling for the stronger labor market opportunities that accompany the younger populations that push up the share of a population in the prime age range for marriage. This outcome may be the result of marriage seekers setting higher quality thresholds in thicker markets.

The interactions of relocation characteristics with numbers of moves shows that additional moves moderate the main effect of these characteristics. This finding is true for three of our five characteristics measures (posting in home region, total move distance, and marriage market thickness). For the other two measures (long posting spell and average labor market quality) the interaction with moves is insignificant. This result is consistent with the idea that moves disrupt the impact of other marriage determinants. However, even after including a full set of move characteristics and their interactions with numbers of moves, the impact of an additional move per se is still statistically and economically meaningful. We therefore conclude that domestic moves powerfully enhance marriage probabilities. In our view, the model most consistent with this pattern is one in which the marriage decision itself is costly, and the fact of a relocation lowers the costs to making this decision.

IX. Conclusion

We use conditionally exogenous relocations of U.S. Army soldiers to examine the impact of long-distance moves on family structure. 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. 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.

These results are surprising if moving is viewed as either a disruption to the progression of life events or as an investment that competes with other investments of time and money (like marriage) for an individual's resources. On the other hand, there are theoretical reasons to think moving may be complimentary to family investments. We first considered a model in which relocation likely requires investment in thinking about long-term plans that may simultaneously lower the cost of considering

other types of long-term commitments, like marriage. We then sketched a basic search model and used it to inform a number of tests for moderating influences on the relationship between moves and marriage. We find that the factors suggested by a traditional search model typically affect the likelihood of marriage in the expected direction, but the size of such effects is modest. Instead, domestic relocations per se have statistically and economically large effects on the likelihood of marriage in our sample, even after controlling for a number of factors that might be expected to influence marriage under the search model. This suggests that the decision to marry may be affected by other events requiring long-term planning. This in turn implies that a disruptive event, like a relocation, may actually strengthen family ties rather than strain them.

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Figure 1A: Residualized Marriage Rates Surrounding a Move
(Domestic Moves only)

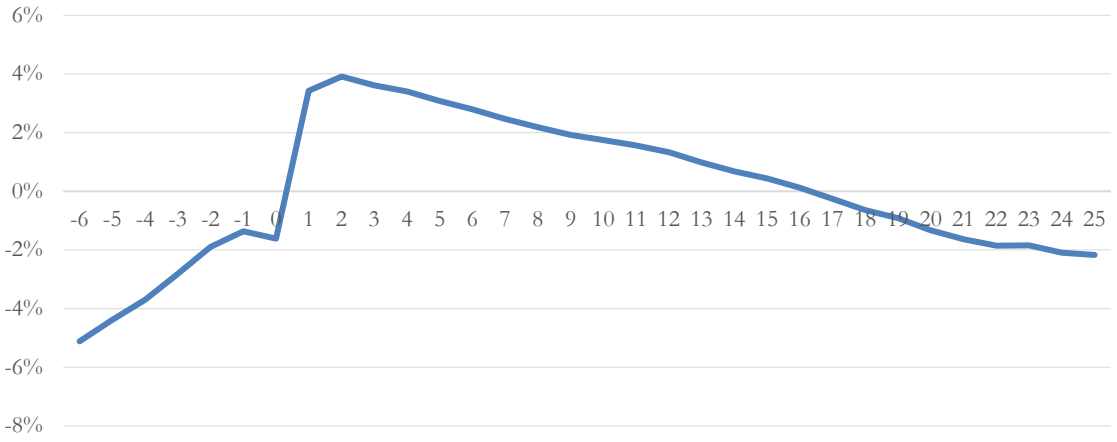

Figure 1B. Residualized Marriage Rates Surrounding a Move

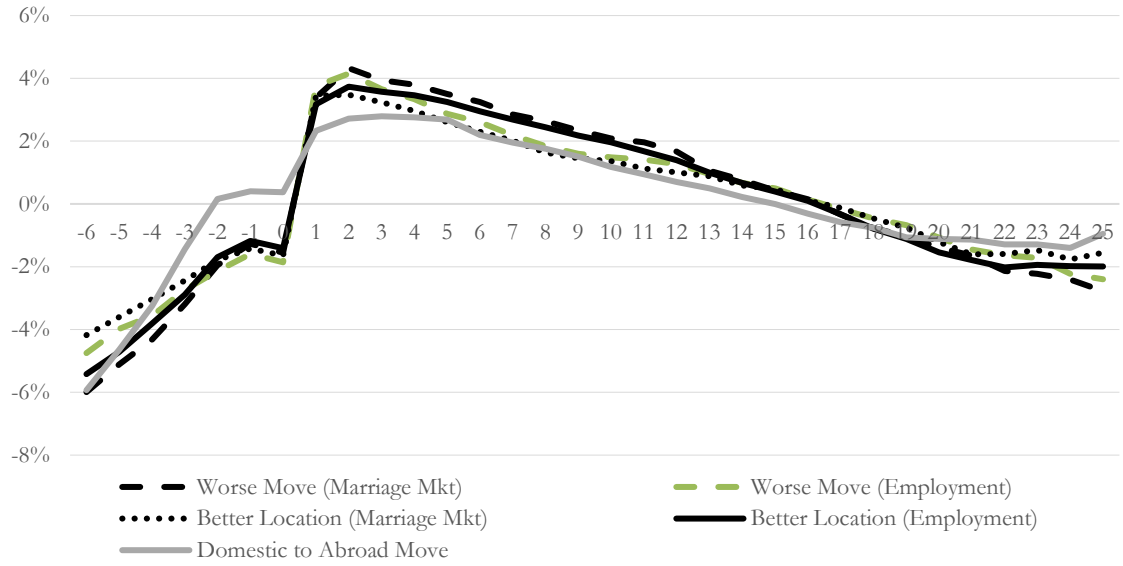

Figure 1 includes individuals who enlist in the Army for at least 5 years, are not married 6 months before their first move, and who have no further moves and their move occurs at least two years before the end of their fifth year. It plots the residuals from a regression of marriage on age dummies and a time trend. The x-axis is balanced around the first move (0 is the month of the move). Figure 1A just includes moves that are from one domestic post to another. Figure 1B splits individuals into five groups by type of move: those moving within the U.S. from a worse to a better market in terms of labor market opportunities; those moving within the U.S. from a worse market to a better market in terms of marriage market prospects; those moving within the U.S. but in the opposite direction, from better to worse markets on those dimensions; and those moving from the U.S. to an international post (either Italy or Germany).

Figure 2A: Probability Married Surrounding a Re-Enlistment

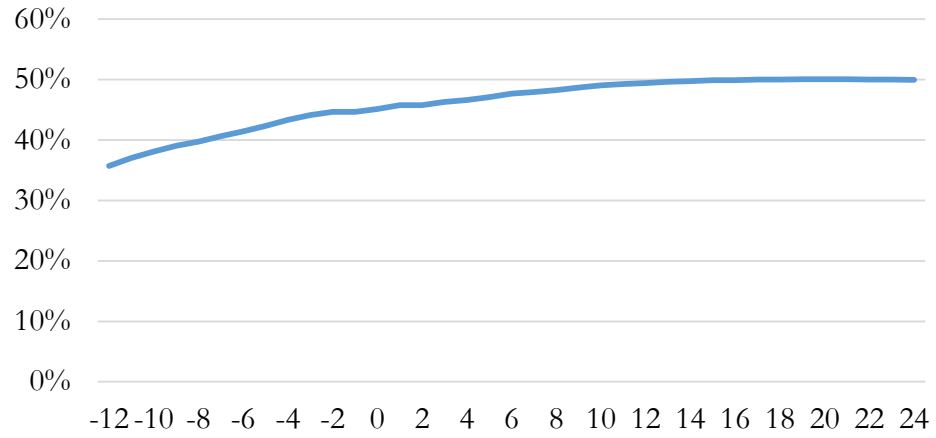

Figure 2B: Probability of first move around a Re-Enlistment

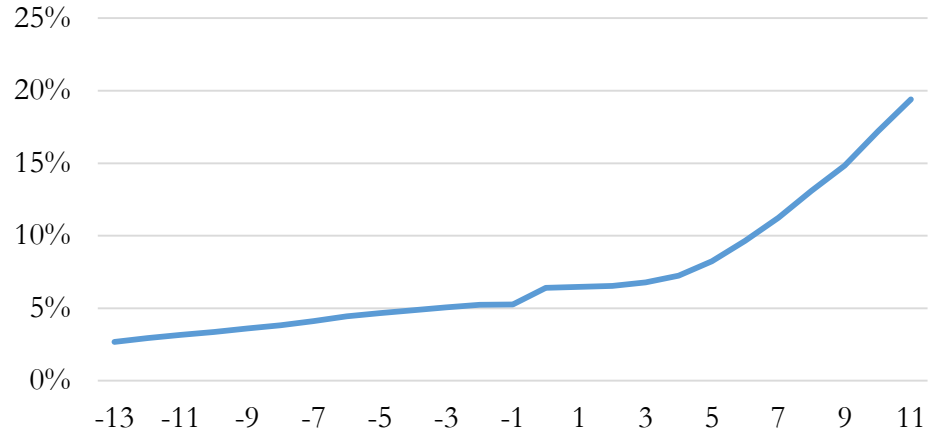

Figure 2 includes the same sample of people in Figure 1 but further restricts to those who re-enlist during the five year period, and are never stationed abroad during the 5 years of service that we study. Unlike Figure 1 which present residual results, Figure 2 shows raw probabilities.

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

	All	Men	Women	Pre 9/11	Post 9/11
Panel A: Demographics					
Fraction Female	13.19%			14.75%	11.96%
Age	26.03	26.01	26.15	25.70	26.30
	(3.76)	(3.72)	(4.07)	(3.43)	(3.99)
Fraction Black	19.66%	16.84%	38.22%	24.22%	16.05%
Hispanic	11.96%	11.75%	13.35%	10.35%	13.24%
Other Race	5.94%	5.61%	8.17%	6.42%	5.57%
AFQT Score	59.65	60.10	56.70	59.75	59.58
	(19.08)	(19.18)	(18.11)	(18.47)	(19.54)
GED	10.42%	11.27%	4.83%	6.32%	13.68%
High School Dropout	0.75%	0.79%	0.46%	0.69%	0.79%
High School Graduate	76.09%	76.27%	74.90%	81.27%	71.99%
Some College / Associates	9.39%	8.60%	14.65%	8.44%	10.15%
College Plus	3.34%	3.07%	5.17%	3.28%	3.39%
Ever Deployed	69.27%	71.67%	53.49%	40.17%	92.34%
Months Deployed	10.30	10.78	7.14	3.83	15.43
	(8.81)	(8.81)	(8.16)	(5.76)	(7.31)
Still in First-Term	19.3%	19.1%	20.7%	19.4%	19.2%
Stay in through 10 Years of Service	44.3%	45.7%	36.1%	44.8%	43.0%
Currently married to another Military Member	6.7%	3.7%	26.2%	7.4%	6.1%
Ever Married to another Military Member	10.5%	6.3%	37.7%	11.9%	9.4%
Panel B: Moves					
Total Moves	0.58	0.58	0.62	0.67	0.51
	(0.62)	(0.62)	(0.62)	(0.63)	(0.59)
Longest Spell	4.60	4.62	4.42	4.68	4.52
	(1.73)	(1.75)	(1.60)	(1.94)	(1.53)
Total Distance	690.53	685.69	722.35	850.29	563.92
	(1013.47)	(1012.65)	(1018.27)	(1133.46)	(886.96)
Time not in Home Region	3.13	3.15	2.99	3.13	3.13
	(1.50)	(1.49)	(1.53)	(1.51)	(1.49)
Average Employment / Population	0.48	0.48	0.48	0.48	0.48
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Marriage Market	0.024	0.024	0.021	0.025	0.023
	(0.016)	(0.016)	(0.017)	(0.016)	(0.015)
Panel C: Outcomes					
Number of Dependents	1.40	1.45	1.10	1.23	1.54
	(1.38)	(1.39)	(1.31)	(1.34)	(1.41)
Ever Married	64.80%	64.34%	67.80%	61.90%	67.09%
Kids	45.57%	45.80%	44.08%	40.88%	49.29%
Divorced | Married	8.71%	6.62%	21.79%	6.62%	10.22%

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 and Post 9/11 includes those who enter October 2001 – October 2007. 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)
	All		Men		Women	
GED		0.0477***		0.0462***		0.0649***
		(0.0049)		(0.0050)		(0.0212)
High School Dropout		0.0401**		0.0442***		-0.0099
		(0.0164)		(0.0169)		(0.0629)
Some College		0.0132**		0.0094		0.0331**
		(0.0055)		(0.0060)		(0.0138)
College Plus		-0.1062***		-0.1131***		-0.0660**
		(0.0106)		(0.0114)		(0.0290)
AFQSC		-0.0020***		-0.0022***		-0.0009***
		(0.0001)		(0.0001)		(0.0003)
Black		-0.0054		-0.0147***		0.0415***
		(0.0042)		(0.0046)		(0.0110)
Hispanic		-0.0119**		-0.0148***		0.0151
		(0.0047)		(0.0050)		(0.0143)
Other Race		-0.0008		0.0026		-0.0053
		(0.0063)		(0.0068)		(0.0170)
Age		0.0212***		0.0217***		0.0149
		(0.0037)		(0.0040)		(0.0107)
Age Squared		-0.0003***		-0.0003***		-0.0003
		(0.0001)		(0.0001)		(0.0002)
Constant	0.5815***	0.3901***	0.5758***	0.3849***	0.6190***	0.4433***
	(0.0014)	(0.0546)	(0.0015)	(0.0583)	(0.0038)	(0.1573)
Observations	182,694	182,694	158,592	158,592	24,102	24,102
R-squared	0.1723	0.1764	0.1567	0.1614	0.2692	0.2713
Mean	0.58	0.58	0.58	0.58	0.62	0.62
F-Test p-value		0.00		0.00		0.00
Sample Entering Before 9/11						
R-Squared	0.2185	0.2212	0.2003	0.2037	0.3216	0.3231
F-Test p-value		0.000		0.000		0.03907
Sample Entering After 9/11						
R-Squared	0.1082	0.1142	0.0950	0.1014	0.1992	0.2039
F-Test p-value		0.000		0.000		0.000

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. We also include R-Squared and F-Test p-values for the subsamples of individuals entering before October 2001 or after in the bottom of the table. ***, **, 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 Number of Moves on Family Outcomes

	All	Male	Female
	(1)	(3)	(5)
Panel A: Ever Married | Not Married when Enter			
Total Number of Moves	0.079***	0.085***	0.030***
	(0.002)	(0.002)	(0.007)
Observations	144,254	125,395	18,859
R-squared	0.129	0.116	0.240
Mean of Marriage Rates	0.55	0.55	0.59
Average Number of Moves	0.57	0.56	0.61
Panel B: Age Married | Not Married when Enter and Ever Married = 1			
Total Number of Moves	-0.037**	-0.025	-0.130**
	(0.019)	(0.020)	(0.060)
Observations	79,944	68,845	11,099
R-squared	0.271	0.244	0.413
Mean Age	23.13	23.21	22.64
Average Number of Moves	0.63	0.63	0.64
Panel C: Marriage at 5 Years			
Total Number of Moves	0.064***	0.071***	0.014**
	(0.002)	(0.002)	(0.006)
Observations	182,694	158,592	24,102
R-squared	0.141	0.134	0.203
Mean of Marriage	0.61	0.62	0.57
Average Number of Moves	0.58	0.58	0.62
Panel D: Ever Have Dependent Child during 5 Years			
Total Number of Moves	0.047***	0.050***	0.024***
	(0.002)	(0.002)	(0.007)
Observations	157,447	136,954	20,493
R-squared	0.141	0.129	0.251
Average Likelihood of Children	0.37	0.37	0.34
Average Number of Moves	0.57	0.57	0.61
Panel E: Dissolve Marriage | Ever Married during 5 Years			
Total Number of Moves	0.005***	0.004***	0.011*
	(0.001)	(0.001)	(0.007)
Observations	118,072	101,810	16,262
R-squared	0.176	0.102	0.257
Mean of Marriage Dissolution	0.09	0.07	0.22
Average Number of Moves	0.63	0.63	0.64

Notes: This table reports linear probability regression results on the number of moves someone experiences during their first five years in the Army. 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 those at the 5 year mark who get married while in the Army, Panel C includesthe 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 4: The Effect of Number of Moves on Family Outcomes by Period Entering Army

	Enter Pre 9/11			Panel B: Enter Post 9/11		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Male	Female	All	Male	Female
Panel A: Ever Married | Not Married when Enter						
Total Number of Moves	0.092***	0.100***	0.035***	0.067***	0.074***	0.022**
	(0.004)	(0.004)	(0.010)	(0.003)	(0.003)	(0.010)
Observations	66,648	54,855	9,456	77,606	70,540	9,403
R-squared	0.159	0.144	0.280	0.098	0.091	0.215
Mean of Marriage Rates	0.52	0.51	0.57	0.58	0.58	0.60
Average Number of Moves	0.65	0.66	0.69	0.50	0.49	0.54
Panel B: Age Married | Not Married when Enter and Ever Married = 1						
Total Number of Moves	-0.063**	-0.042	-0.188**	-0.019	-0.014	-0.083
	(0.027)	(0.029)	(0.082)	(0.026)	(0.027)	(0.087)
Observations	34,731	28,102	5,436	45,213	40,743	5,663
R-squared	0.347	0.324	0.477	0.224	0.205	0.372
Mean Age	22.9	22.93	22.60	23.30	23.40	22.67
Average Number of Moves	0.7	0.74	0.73	0.55	0.55	0.56
Panel C: Marriage at 5 Years						
Total Number of Moves	0.076***	0.085***	0.016*	0.054***	0.060***	0.011
	(0.003)	(0.003)	(0.009)	(0.003)	(0.003)	(0.009)
Observations	83,787	68,860	11,914	98,907	89,732	12,188
R-squared	0.169	0.164	0.238	0.116	0.111	0.183
Mean of Marriage	0.59	0.59	0.58	0.63	0.64	0.56
Average Number of Moves	0.66	0.67	0.69	0.51	0.51	0.55
Panel D: Ever Have Dependent Child during 5 Years						
Total Number of Moves	0.069***	0.073***	0.041***	0.027***	0.030***	0.008
	(0.003)	(0.003)	(0.009)	(0.003)	(0.003)	(0.009)
Observations	70,068	59,811	10,257	87,379	77,143	10,236
R-squared	0.170	0.155	0.263	0.115	0.103	0.243
Average Likelihood of Children	0.32	0.32	0.31	0.41	0.41	0.37
Average Number of Moves	0.66	0.66	0.69	0.50	0.50	0.54
Panel E: Dissolve Marriage | Ever Married during 5 Years						
Total Number of Moves	0.007***	0.007***	0.002	0.004*	0.002	0.018*
	(0.002)	(0.002)	(0.009)	(0.002)	(0.002)	(0.010)
Observations	51,566	41,879	7,819	66,506	59,931	8,443
R-squared	0.223	0.159	0.299	0.147	0.075	0.220
Mean of Marriage Dissolution	0.07	0.05	0.16	0.10	0.08	0.27
Average Number of Moves	0.72	0.73	0.72	0.56	0.55	0.57

Notes: This table reports linear probability regression results on the number of moves someone experiences during their first five years in the Army. Columns 1-3 include those who enter prior to October 2001 and Columns 4-6 include those who enter October 2001 – October 2007. 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 those at the 5 year mark who get married while in the Army, Panel C includesthe 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 job, rank, year, and gender structural controls. 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 5: The Effect of Number of Moves on Family Outcomes: Alternative Samples

	Term 6	End of First-Term	Married Less than 6 Months	Married Greater than 6 months
	(1)	(2)	(3)	(4)
Panel A: Ever Married | Not Married when Enter				
Total Number of Moves	0.062***	0.102***		
	(0.008)	(0.003)		
Constant	-1.285***	-1.694***		
	(0.217)	(0.041)		
Observations	18,200	229,888		
R-squared	0.261	0.146		
Mean of Marriage Rates	0.51	0.26		
Average Number of Moves	0.41	0.11		
Panel B: Age Married | Not Married when Enter and Ever Married = 1				
Total Number of Moves	0.093	0.231***	-0.416***	-0.249***
	(0.074)	(0.032)	(0.067)	(0.060)
Constant	21.744***	21.601***	23.786***	22.483***
	(0.229)	(0.056)	(0.153)	(0.114)
Observations	9,196	59,046	14,370	30,874
R-squared	0.451	0.344	0.436	0.363
Mean Age	23.14	22.55	23.73	22.81
Average Number of Moves	0.46	0.18	1.20	1.07
Panel C: Marriage at 5 Years				
Total Number of Moves	0.048***	0.061***	-0.010**	-0.010**
	(0.007)	(0.003)	(0.004)	(0.005)
Constant	-1.633***	-2.366***	1.036***	0.935***
	(0.138)	(0.072)	(0.089)	(0.071)
Observations	22,813	279,298	14,370	30,879
R-squared	0.260	0.192	0.352	0.287
Mean of Marriage	0.57	0.37	0.97	0.94
Average Number of Moves	0.42	0.11	1.20	1.07
Panel D: Ever Have Dependent Child during 5 Years				
Total Number of Moves	0.031***	0.038***	0.044***	0.037***
	(0.007)	(0.003)	(0.013)	(0.010)
Observations	19,905	279,298	14,213	30,156
R-squared	0.265	0.229	0.341	0.269
Average Likelihood of Children	0.32	0.24	0.50	0.58
Average Number of Moves	0.41	0.11	1.20	1.07
Panel E: Dissolve Marriage | Ever Married during 5 Years				
Total Number of Moves	0.009	0.013***	0.007	0.017***
	(0.005)	(0.002)	(0.005)	(0.006)
Observations	13,776	107,957	14,370	30,876
R-squared	0.349	0.201	0.413	0.316
Mean of Marriage Dissolution	0.09	0.05	0.05	0.10
Average Number of Moves	0.46	0.15	1.20	1.07

Notes: This table reports linear probability regression results on the number of moves someone experiences during their first five years in the Army. The dependent variable is denoted in the title of each panel. The title of each column denotes the conditioning sample: Col. 1 includes individuals have 6 year term lengths, Col. 2 includes individuals at the end of their first term – regardless of length, Col. 3 includes who get married within a 6 month window on either side of a move, and Col. 4 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 includesthe 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: The Roles of Non-linearities and International Moves on the Impacts of Moves on Marriage

	(1)	(2)	(3)	(4)	(5)
	Max 1 Move	**Move Dummies, Max 3 Moves**	**Including Abroad Moves**	**Including Abroad Moves, Max 1 move**	**Domestic and Abroad Moves, Max 1 Move**
Domestic Move					0.059***
					(0.003)
One Move	0.093***	0.093***			
	(0.003)	(0.003)			
Two Moves		0.133***			
		(0.006)			
Three Moves		0.157***			
		(0.023)			
Total Moves (Include Abroad)			0.0103***	0.0495***	
			(0.0013)	(0.0025)	
International Moves					0.017***
					(0.004)
One Abroad Move					
Two Abroad Moves					
Three Abroad Moves					
Observations	134,996	144,192	281,163	206,451	206,451
R-squared	0.131	0.130	0.0879	0.1051	0.106
Mean of Outcome	0.55	0.55	0.53	0.53	0.53

Notes: This table reports linear probability regression results of marriage on the number of moves someone experiences during their first five years in the Army. The sample construction is the same as Col. 4 of Table 5 where it restricts to individuals who were not married when they entered. Cols. 1, 4 and 5 restrict to a maximum of one move, and Cols. 3-5 include international moves to Germany or Italy. 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: How Controlling for Relocation Chacteristics Affects the Impact of Moves on Marriage

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Full Sample	**Time at Home**	**Length of Stay**	**Employment to Population**	**Distance of Moves**	**Marriage Market**	**All Additional Move Controls**
Domestic Moves	0.079***	0.088***	0.086***	0.0795***	0.0484***	0.0768***	0.056***
	(0.002)	(0.003)	(0.003)	(0.0025)	(0.0037)	(0.0026)	(0.005)
Ever in Home Division		0.010**					0.005
		(0.004)					(0.005)
Ever Home x Moves		-0.023***					-0.017***
		(0.005)					(0.005)
Longest Spell			0.001				0.007***
			(0.002)				(0.002)
Longer x Moves			0.014***				0.004
			(0.003)				(0.003)
Average Employment to Pop				-0.0146***		-0.0156***	-0.016***
				(0.0018)		(0.0015)	(0.002)
Emp x Moves				0.0009			-0.001
				(0.0023)			(0.002)
Total Distance of Moves					0.0355***		0.043***
					(0.0034)		(0.004)
Distance x Moves					-0.0161***		-0.019***
					(0.0021)		(0.002)
Average Marriage Market						-0.0149***	-0.012***
						(0.0031)	(0.003)
Average Marriage Market x Moves						0.0086***	0.010***
						(0.0025)	(0.003)
Observations	144,254	144,254	138,134	128,814	144,254	128,792	124,891
R^2	0.129	0.129	0.133	0.1318	0.1299	0.1320	0.135
Mean of Outcome	0.55	0.55	0.55	0.54	0.55	0.54	0.54

Notes: This table reports linear probability regression results of marriage on the number of moves someone experiences during their first five years in the Army. The sample construction is the same as Col. 4 of Table 5 where it restricts to individuals who were not married when they entered. Each 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.

Appendix A. 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] These added benefits, however, would not differentially effect movers versus non-movers. 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 destination, either by car or plane, and will also compensate the soldier and his/her family for potential

[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

expenses from moving (called Dislocation Allowances).[6] [7] [8] Because of these added benefits from marriage, it is possible that some marriages form fraudulently. The added benefits from being married during a move are: a greater weight allowance for moving items, as well as higher Dislocation Allowances, incentives that are 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

[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.

and civilians (Karney, Loughran and Pollard 2012). Similarly, there is disagreement in the literature as 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. Analysis with alternative clustering levels.

Table B1: Clustering with Multiple Methods

	Ever Married	Age Married	Married at 5 Years	Ever Kids	Ever Dissolve
Domestic Moves	0.079	-0.037	0.064	0.047	0.005
Robust Standard Errors	(0.002)	(0.019)	(0.002)	(0.002)	(0.001)
Cluster on First Location	<0.012>	<0.023>	<0.010>	<0.006>	<0.002>
Cluster on First Location x Year	[0.004]	[0.021]	[0.004]	[0.003]	[0.002]
Cluster on Job x Rank x Year	{0.003}	{0.020}	{0.002}	{0.002}	{0.002}
Observations	144,254	79,944	182,694	157,447	118,072
R-squared	0.129	0.271	0.141	0.141	0.176
Mean	0.55	23.13	0.61	0.37	0.09
Indep Mean	0.57	0.63	0.58	0.57	0.63

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 B2: The Effect of Number of Moves on Family Outcomes, Clustering by Job x Rank x Year

	All	Male	Female
Panel A: Ever Married | Not Married when Enter			
Total Number of Moves	0.079***	0.085***	0.030***
Robust Standard Errors	(0.002)	(0.002)	(0.007)
Clustered Standard Errors	(0.003)	(0.003)	(0.008)
Observations	144,254	125,395	18,859
R-squared	0.129	0.116	0.240
Number of Clusters	12,165	8,319	3,846
Panel B: Age Married | Not Married when Enter and Ever Married = 1			
Total Number of Moves	-0.037*	-0.025	-0.130**
Robust Standard Errors	(0.019)	(0.020)	(0.060)
Clustered Standard Errors	(0.020)	(0.021)	(0.062)
Observations	79,944	68,845	11,099
R-squared	0.271	0.244	0.413
Number of Clusters	10,165	7,121	3,044
Panel C: Marriage at 5 Years			
Total Number of Moves	0.064***	0.071***	0.014**
Robust Standard Errors	(0.002)	(0.002)	(0.006)
Clustered Standard Errors	(0.002)	(0.002)	(0.007)
Observations	182,694	158,592	24,102
R-squared	0.141	0.134	0.203
Number of Clusters	13,335	9,082	4,253
Panel D: Ever Have Dependent Child during 5 Years			
Total Number of Moves	0.047***	0.050***	0.024***
Robust Standard Errors	(0.002)	(0.002)	(0.007)
Clustered Standard Errors	(0.002)	(0.002)	(0.007)
Observations	157,447	136,954	20,493
R-squared	0.141	0.129	0.251
Number of Clusters	12,608	8,615	3,993
Panel E: Dissolve Marriage | Ever Married during 5 Years			
Total Number of Moves	0.005***	0.004***	0.011
Robust Standard Errors	(0.001)	(0.001)	(0.007)
Clustered Standard Errors	(0.002)	(0.001)	(0.007)
Observations	118,072	101,810	16,262
R-squared	0.176	0.102	0.257
Number of Clusters	12,033	8,361	3,672

Notes: This table reports linear probability regression results on the number of moves someone experiences during their first five years in the Army. 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 those at the 5 year mark who get married while in the Army, Panel C includesthe 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 standard errors clustered at the job x rank x year level below the coefficient.

Appendix C: Additional tests for a role for unobservables in Army relocations.

Table C1: The Effect of Number of Moves on Family Outcomes

	All		Male		Female	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Ever Married | Not Married when Enter						
Total Number of Moves	0.087***	0.079***	0.094***	0.085***	0.038***	0.030***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.007)	(0.007)
Observations	144,254	144,254	125,395	125,395	18,859	18,859
R-squared	0.118	0.129	0.103	0.116	0.221	0.240
Mean of Marriage Rates	0.55	0.55	0.55	0.55	0.59	0.59
Average Number of Moves	0.57	0.57	0.56	0.56	0.61	0.61
Oster Adjust Coefficients	0.051		0.061		-0.0003	
Panel B: Age Married | Not Married when Enter and Ever Married = 1						
Total Number of Moves	-0.069***	-0.037**	-0.058***	-0.025	-0.150**	-0.130**
	(0.019)	(0.019)	(0.020)	(0.020)	(0.062)	(0.060)
Observations	79,944	79,944	68,845	68,845	11,099	11,099
R-squared	0.237	0.271	0.210	0.244	0.372	0.413
Mean Age	23.13	23.13	23.21	23.21	22.64	22.64
Average Number of Moves	0.63	0.63	0.63	0.63	0.64	0.64
Oster Adjust Coefficients	0.042		0.045		-0.070	
Panel C: Marriage at 5 Years						
Total Number of Moves	0.074***	0.064***	0.081***	0.071***	0.022***	0.014**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.006)	(0.006)
Observations	182,694	182,694	158,592	158,592	24,102	24,102
R-squared	0.107	0.141	0.094	0.134	0.183	0.203
Mean of Marriage	0.61	0.61	0.62	0.62	0.57	0.57
Average Number of Moves	0.58	0.58	0.58	0.58	0.62	0.62
Oster Adjust Coefficients	0.052		0.061		-0.010	
Panel D: Ever Have Dependent Child during 5 Years						
Total Number of Moves	0.059***	0.047***	0.060***	0.050***	0.046***	0.024***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.007)	(0.007)
Observations	157,447	157,447	136,954	136,954	20,493	20,493
R-squared	0.117	0.141	0.102	0.129	0.213	0.251
Average Likelihood of Children	0.37	0.37	0.37	0.37	0.34	0.34
Average Number of Moves	0.57	0.57	0.57	0.57	0.61	0.61
Oster Adjust Coefficients	0.026		0.036		-0.020	
Panel E: Dissolve Marriage | Ever Married during 5 Years						
Total Number of Moves	0.004***	0.005***	0.003**	0.004***	0.007	0.011*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.007)	(0.007)
Observations	118,072	118,072	101,810	101,810	16,262	16,262
R-squared	0.168	0.176	0.094	0.102	0.237	0.257
Mean of Marriage Dissolution	0.09	0.09	0.07	0.07	0.22	0.22
Average Number of Moves	0.63	0.63	0.63	0.63	0.64	0.64
Oster Adjust Coefficients	0.012		0.008		0.026	

Notes: This table reports linear probability regression results on the number of moves someone experiences during their first five years in the Army. 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 those at the 5 year mark who get married while in the Army, Panel C includesthe 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. 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. 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 C2: Tests of Conditional Random Assignment

	(1)	(2)
	Sample: Enlist for 6 Year Term	
	Dependent Variable: Total Number of Moves	
GED		-0.0031
		(0.0164)
High School Dropout		0.0439
		(0.0508)
Some College		-0.0126
		(0.0150)
College Plus		0.0218
		(0.0302)
AFQSC		-0.0007**
		(0.0003)
Black		0.0009
		(0.0130)
Hispanic		0.0041
		(0.0143)
Other Race		0.0204
		(0.0184)
Age		0.0252**
		(0.0109)
Age Squared		-0.0004**
		(0.0002)
Observations	22,813	22,813
R-squared	0.2886	0.2894
Mean	0.42	0.42
F-Test p-value		0.064

Notes: This table reports results from regressions of the total number of moves someone has during their first five years on individual characteristics. Column 1 includes full interactions of job, rank, year, and sex. Column 2 includes 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. ***, **, and * denotes significance at the 1%, 5%, and 10% level respectively with robust standard errors reported in parentheses below the coefficient.