Evaluating the Internal Labor Migration Effects of Compulsory Peacetime Conscription

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Abstract: Conscription typically involves migration to the duty base location. According to DaVanzo's (1981) theory of repeat migration, this military-motivated migration might foster post-military migration. This paper investigates this issue empirically using individual-level data drawn from the 1992, 1998, and 2001 editions of the French survey of labor market entrants '*Génération*.' The methods used to identify the causal effect of compulsory peacetime conscription on the post-military propensity to migrate exploit the abolition of conscription in France in October 1997 plus information on the timing of service among those who served. Conscription stimulated the post-military propensity to migrate for work of French male labor-market entrants with upper-class origins. Furthermore, there is evidence of a previously overlooked anticipatory effect of conscription: French males who were waiting to be called up for military service were approximately 25% less likely to migrate for work than comparable non-conscripts. Some implications of these findings and a consideration of their external validity are also provided.

Keywords: France, internal migration, conscription, policy evaluation.

Introduction

In his review of internal migration in developed countries, Greenwood (1997) wonders whether participation in the military might foster post-military geographical mobility (see also Shryock 1964). He argues, for example, that military personnel acquire migration experience and knowledge of alternative areas while in service, which might encourage a post-service propensity to migrate. Only a few studies have assessed this conjecture. Using U.S. census data, Cowper et al. (2000) and Bailey (2011) find higher rates of migration among veterans that persist over both the course of life and military staffing policies. Using longitudinal microdata, Lee (2012) and Doetsch (2012) contrast the geographical mobility of U.S. veterans and nonveterans in the aftermath of the American Civil War and World War I, respectively. They find that wartime military service has positive, or at least nonnegative, effects on post-war geographical mobility. Prior to this, Corvisier (1971) and Baker (1998) gathered quantitative evidence on how the French military promoted geographical mobility, in periods when the French army was composed mainly of professional soldiers or was frequently involved in military conflicts.

However, it appears that no one has studied the effect that compulsory peacetime conscription may exert on subsequent geographical mobility. Military service may involve migration to the duty base location, so conscripts may also acquire migration experience and knowledge of alternative areas of the country while in service. In the terminology of Eldridge (1965) and DaVanzo (1983), this 'primary migration' could then foster 'repeat migration.' Perhaps in the same vein, moving away from domicile for higher education is positively related to subsequent migration for work (Faggian, McCann, and Sheppard 2007).

Previous research has shown that the socio-economic effects of military service vary with the type of service (MacLean and Elder 2007). Indeed, a number of reasons suggest that migration patterns among veterans documented by previous research might not extrapolate to former peacetime conscripts. Prominent among these reasons is health: The incidence of wounds and diseases in the population of U.S. veterans, which is composed overwhelmingly of war veterans, is much higher than among former peacetime conscripts, and there is evidence that poor health strongly diminishes geographical mobility (Lee 2008). An alternative pathway by which military participation affects U.S. veterans' mobility is through difficulties in readjustment to civilian life after war in the form of joblessness or ostracism (Lee 2008), but these difficulties seem less likely to pose a problem for peacetime conscripts. Additionally, the migration experience and knowledge of alternative areas acquired by peacetime conscripts might differ from those gained by wartime conscripts and professional soldiers.[1]

Overall, therefore, the effect exerted by compulsory peacetime conscription on the subsequent propensity to migrate within a country is not easily predictable. However, it is important to measure it. Many countries around the world require young people to spend time in the armed forces, including Lithuania, Sweden, and Ukraine, which have recently reintroduced the draft, and Norway, which expanded conscription in 2015 to include female citizens (Bieri 2015). In France, one-month compulsory national service for people aged 16 is expected to be launched in 2022. The internal

[1] Lee (2012) highlights the importance of distance traveled while in service as a predictor of geographical mobility, so without the opportunity for traveling to distant places, military service during the American Civil War would indeed have decreased veterans' geographical mobility.

migration of young workers is a major mechanism for geographically redistributing labor resources in response to changing economic and demographic conditions, and thus helps to raise an economy's total output through a more efficient allocation of resources, to boost the speed at which workers and firms adjust to localized shocks, to reduce an area's equilibrium level of unemployment, and to speed up the process of income convergence across regions (Blanchard and Katz 1992; Saks and Wozniak 2011; Ozgen, Nijkamp, and Poot 2010).

In the next section, I argue for a causal effect of conscription on subsequent geographical mobility using DaVanzo's (1981) repeat migration theory. I then investigate this issue empirically using several treatment assessment methods applied to individual-level data drawn from the 1992, 1998, and 2001 editions of the French survey of labor market entrants '*Génération*' (Generation). I first provide background on French military service and review the data and the construction of the samples, then explain the measures of conscription status and internal migration. Previous studies contrasting migration patterns of veterans and nonveterans have not focused on migration for work due to lack of data. Generation enables me to do this.

The methods used in this paper to identify the causal effect of compulsory peacetime conscription on the post-military propensity to migrate exploit the abolition of conscription in France in October 1997. I find evidence that military service positively influenced subsequent decisions to migrate for work within France among individuals with upper-class origins. Moreover, making use of information on the timing of service among those who served, I find that conscription reduced the migration-for-work propensity of French males who were waiting to be called up. Some implications of these findings plus a consideration of their external validity are also provided.

This paper contributes to two main strands of work. First, it adds to previous work on the labor market effects of compulsory peacetime conscription. This literature emphasizes subsequent effects on employment, earnings, and occupation (see e.g. Imbens and van der Klaauw 1995; Maurin and Xenogiani 2007; Granier, Joseph, and Joutard 2011; Grenet, Hart, and Roberts 2011; Bauer et al. 2012; Card and Cardoso 2012; Hubers and Webbink 2015; Grönqvist and Lindqvist 2016; Torun and Tumen 2016; Albaek et al. 2017). A notable exception is Torun (2016), who finds that the expected interruption of civilian life created by military service reduces the labor force participation and employment of male teenagers who are waiting to be called up. Second, a body of research suggests that the extent of previous migration is closely correlated with subsequent migration behavior (see e.g. Eldridge 1965; Morrison 1971; Vanderkamp 1971; DaVanzo 1981, 1983; Newbold and Bell 2001; Faggian, McCann, and Sheppard 2007). Apparently, however, no one has previously investigated the impact of peacetime conscription on subsequent migration.

Conceptual Framework

Shields and Shields (1989) organize the theoretical literature on internal migration around four different approaches in which the potential migrant is viewed, in turn, as a supplier of labor, an investor in human capital, a consumer of locational amenities, and a producer of household commodities. Common to all four approaches, though, is the existence of uncertainty regarding living conditions and employment opportunities in the possible destinations, and of economic and non-economic costs associated with migration. Although potential migrants can engage in information-gathering to reduce uncertainty, this, like migration itself, is costly. As to migration costs, they stem largely from replacing or transferring elsewhere location-specific capital, i.e. 'concrete and intangible assets such as job seniority or close friendships whose value would be lost, costly to replace, or steadily diminished if one lived somewhere else.' (DaVanzo 1983)

Information costs and location-specific capital are central to DaVanzo's (1981) analytical framework for repeat migration. That framework suggests several reasons why migration from the domicile to the location of the duty base could foster post-military migration. First, conscripts may become more adept at collecting and processing migration-relevant information, in a learning-by-doing effect. Second, as also suggested by Lee (2008), conscripts may have the opportunity to collect, either firsthand or from peer conscripts, information about other places in the country at little or no cost. Third, as the amount of location-specific capital is likely to be positively correlated with the length of stay in a place, conscripts may have built up less location-specific capital in their place of origin. And fourth, conscripts may retain some location-specific capital in the place of deployment, so that they may be less reluctant to move there after service.

At the empirical level, a simple comparison of former conscripts' and non-conscripts' migration outcomes is unlikely to be informative about the causal effect of conscription on post-service geographical mobility, as certain types of men are more likely to serve than others (Angrist 1990; Imbens and van der Klaauw 1995). For one thing, these groups may differ in terms of unobserved characteristics likely to be correlated with their ability to acquire and process information and/or to replace and transfer location-specific capital. Conscripts, for example, are generally positively selected in terms of health and cognitive ability, and acquiring and processing information and/or replacing or transferring location-specific capital may be easier for healthier or cognitively abler individuals. Perhaps in this vein, Corvisier (1971) found that French soldiers serving in overseas territories came from more mobile families than soldiers serving in metropolitan France. Second, as argued by DaVanzo (1981), certain people might be intrinsically more migration-prone, and since military service may involve moving away from one's place of residence, the most migration-prone could be less reluctant to serve. For these reasons, mere comparisons of former conscripts' and non-conscripts' migration outcomes will probably show higher mobility for the former due to a positive selection bias effect.

Background, Data, and Sample

French National Service

All young French men were subject to so-called 'national service' (NS) at the age of 18, although they had the right to defer their service until the age of 22 with no specific justification (from 1970 onwards, women could also serve on a voluntary enrollment basis). Despite its obligatory nature, about a third of men born before 1975 avoided national service (Fize and Louis-Sidois 2018). The main reasons for exemption were medical conditions as assessed by military doctors and being the breadwinner of a family.

French national service was of two basic types. The standard type consisted of a ten-month period of military service (12 months up to 1992). The nonstandard type, which could last more than 10 months, could be served in the police, the fire service, as a technical assistant in an overseas territory, as an aid worker in a foreign country, or as a conscientious objector. In 1992, 94% of conscripts did the standard service (FMD 1993), as meeting the needs of the army took priority in the distribution of conscripts. Staying on in education was one way to avoid immediate national service and to increase the probability of accessing the nonstandard service (Maurin and Xenogiani 2007).

In times of peace, conscripts could serve ‘in any part of France or even Europe’ (NS Code, articles L. 70 and L. 94-3). From 1987 onwards, the French Ministry of Defense attempted to assign conscripts to military bases located in their region of residence, although a shift of human resources to the east was unavoidable because of the different centers of gravity of the population and of military units (FMD 1994). Ribouillault (1998, 58) states that about a third of conscripts had to travel far from their domiciles (to the east, or to ports or arsenals) to reach their duty bases. According to an official report, the average railway distance between the domicile and the duty base was 435 km (Chauveau 1990).

The difficulties encountered by the French army during the 1991 Gulf War convinced President Jacques Chirac of the necessity to rethink the French military. On 22 February 1996, he announced the intention to fully professionalize the armed forces by 2002. The transition to an all-volunteer force was made progressively. During the professionalization process, the 1997 reform of national service officially exempted men born after 1978 from service in October 1997. Men born before 1 January 1979 were still obliged to serve for ten months. But the fact that the reform introduced (or extended) deferments linked to work (education), and that the military administration stopped using conscripts in August 2001, led to a gradual decrease in the proportion conscripted for cohorts born after 1974 (Fize and Louis-Sidois 2018).

Data and Sample

The primary data source for this research is the Generation survey, run by the French Center for Research on Education, Training, and Employment (Céreq). Generation is a retrospective telephone survey of individuals who, in a certain year, left the education system for at least one year for the first time, conducted a few years after leaving education. Generation seeks to faithfully reconstruct month by month these individuals’ activity status so as to characterize their entry into the labor market. It also includes information on birth date, educational attainment level, place of residence (at the start of secondary education and at the end of studies), and workplace location for each job held during the survey period ('survey period' means the period between leaving education and being interviewed for Generation). Unfortunately, Generation did not ask for geographical information for the spell of national service.

I use the Generation surveys for 1992, 1998, and 2001 (Céreq 1997, 2001, 2004) obtained from the French Data Archives for Social Sciences (*Réseau Quetelet*). Individuals leaving education in 1992 (respectively 2001) were interviewed in 1997 (2004). Those leaving in 1998 were interviewed on four occasions: 2001, 2003, 2005, and 2008. I use the sample of 1998 leavers interviewed in 2001 because of its much larger size. For more information on Generation, see http://www.cereq.fr/index.php/themes/Acces-aux-donnees-Themes/Enquetes-d-insertion-Generation.

I use a variety of samples for the different analyses conducted. To be included in any of these samples, individuals must reach the age of 16–23 in the year in which they leave education and must meet the following conditions (Table A1 in the Appendix details their impact on the number of observations). First, demographic data must be non-missing. Second, the place of residence at the start of secondary education and at the end of studies must be in France (including overseas territories). Third, individuals must have at least one job during the survey period. Fourth, the location of the entry job (defined below) must be known and in France. Fifth, for individuals who worked before doing national service, job location(s) must be known and in France. And sixth, for the reasons given below, the place of residence on leaving education and the location of the entry job may not both be in overseas territories.

Maurin and Xenogiani (2007) use data from the French Labor Force Survey to show that military service was providing male students with incentives to pursue education after the age of 16, and that those incentives disappeared after the 1997 reform. I find evidence consistent with draft avoidance behavior (Card and Lemieux 2001; Bauer et al. 2014; Mouganie 2015; Torun and Tumen 2016) in a sample of individuals born between 1973 and 1982 constructed from the three editions of Generation. Table A2 shows the results of regressing three measures of educational attainment on complete sets of year-of-birth and gender dummies, plus an interaction of the male dummy with a dummy for being born after 1978. The three measures indicate a significant decline in relative educational attainment for men exempted from service.

One possible implication of draft avoidance behavior is that certain age classes of male education leavers might be selected on migration proneness. Consider males leaving education in 1998. Those aged 16–19 (and therefore born after 1978) would have had no incentives to delay military service by continuing their education. By contrast, some of those aged 20–23 would have left education at an older age in order to delay their service. If one of the incentives for staying on in education is the risk of being deployed outside one's place of residence at a younger age, those aged 20–23 would, on average, be less migration-prone than the 16–19-year-old class. Some results of this research make sense in light of this reasoning.

Measures

I infer conscription status from the reported monthly activity status (only one status can be reported for any one month). Individuals who report a spell of national service during the survey period are classified as conscripts; otherwise they are classified as non-conscripts. The type of service cannot be distinguished generally in the data, but nonstandard service could also mean being deployed outside one's place of residence, so it may also have promoted subsequent internal migration. In the samples given in the last row of Table A1, 71% (23%) of the men surveyed in Generation 1992 (1998) are classified as conscripts. Among women, the proportion who volunteered was 0.5% (0.4%). There are no conscripts in Generation 2001 because the French military administration stopped using conscripts in August 2001.

A limitation of Generation is that individuals could have done their national service before leaving the education system. Such individuals are misclassified in the data as non-conscripts. Let S^* be the unobserved true indicator for having done the service, and let S be its observed error-ridden counterpart. Assume that $\text{prob}\left(S=1 \middle| S^*=0\right)=0$. Granier, Joseph, and Joutard (2011) report that $\text{prob}\left(S=0 \middle| S^*=1\right)=0.02$ in Generation 1992 and $\text{prob}\left(S=0 \middle| S^*=1\right)=0.125$ in Generation 1998. By the total probability theorem $\text{prob}\left(S^*=1\right)=\dfrac{\text{prob}\left(S=1\right)}{1-\text{prob}\left(S=0 \middle| S^*=1\right)}$, which can be calculated using the estimates of $\text{prob}\left(S=1\right)$ given above. Thus, expression (5) in Lewbel (2007) suggests that misclassification may attenuate the estimated effects of conscription by just about 5%.

Information on place of residence and work is provided at the level of commuting zones (*zones d'emploi*) constructed by the French Statistical Office (INSEE) using journey-to-work data from the 1990 Census.[2] This geography appears to be particularly appropriate for studying internal labor migration. First, operationalizing migration as a move between local labor markets matches the notion of migration used

[2] In Generation 1992, the geographic information is provided at the level of municipalities (*communes*). I allocate municipalities to commuting zones using a crosswalk created by INSEE: https://www.insee.fr/fr/information/2114596.

in most economic models of internal labor migration. Second, bounding migration by commuting patterns improves on the arbitrary nature of some alternative measures of migration (e.g. those based on cross-boundary movement: Yankow 2002; Tolbert, Blanchard, and Irwin 2009). And third, the exhaustive partition of metropolitan France into 348 commuting zones that remained unaltered until 2010 removes concerns about identifying migrations consistently in different editions of Generation.[3]

I call a migration between the commuting zone of residence at the end of studies and the zone where the entry job (defined below) is located an *entry move*. Looking at entry moves assesses the causal effect of conscription when it is expected to be clearest (i.e. right after completing the national service). More specifically, I analyze *onward entry moves*, defined as entry moves whose destination is other than the zone of residence at the start of secondary education. 93% of all entry moves observed in the whole sample are onward moves. The indicator for moving onward is M .

For non-conscripts, the entry job is the first job held during the survey period. For conscripts, though, there are two possibilities: The first is to equate the entry job to the first job held after completing the national service. The resulting migration indicator is M_1. In this case, the entry job coincides with the first job held during the survey period for those who did the service and then worked (47% of conscripts), but not for those who worked, did their service, then worked again (51%). (The remaining 2% who worked, did the service, and then did not work again are excluded from analyses of M_1.) This raises the issue that migration-for-work experience accumulated before doing national service might foster subsequent geographical mobility. I can calculate the

[3] Overseas territories were not zoned until 2007, so it cannot be ascertained whether individuals changed zone within overseas territories. This is the reason for the sixth selection criterion listed above.

number of zones where jobs were located, but this variable is correlated with S and, therefore, endogenous.

The second possibility, which yields migration indicator M_2, is to equate the entry job to the first job held during the survey period. The causal effect of conscription is thus implicitly evaluated depending on having served before working, as migration-for-work decisions by individuals who worked before national service cannot be influenced by the service itself. Focusing on individuals who served before working may introduce further biases into the analysis, but M_2 is useful in using the timing of service to assess the causal effect of conscription.

Descriptive statistics for the main variables are presented in Table A3 jointly with some definitions. Migration rates range from 0.28 to 0.35. These rates are somewhat higher than the 27% estimate developed by Margirier (2006) for individuals of any age drawn from Generation 1998. The higher migration rates are observed in the latter editions of Generation, which is consistent with the increasing mobility of French young adults documented by Baccaïni (2007). Educational attainment is measured on five levels. During the period of observation, education in France was influenced by a person's social origins and a very powerful determinant of the level at which he/she entered the workforce (Goux and Maurin 1997).

Identification Strategies

Difference-in-Differences (DID)

The DID strategy relies on the classification of young men into two groups according to the binary variable *Z*, whose values are set with reference to the 1997 reform of French national service and are therefore considered to be as good as randomly assigned. Differences in migration outcomes can be driven by aggregate factors unrelated to the reform, so women classified according to Z are included in the estimations as a control group.

The sample analyzed is drawn from the three editions of Generation. $Z = 1$ for individuals born in 1978 or earlier and $Z = 0$ for individuals born after 1978. In the case of males, $Z = 1$ indicates males who were eligible for service and $Z = 0$ males exempted from service (i.e. non-eligible). 54% of males with $Z = 1$ served (i.e. were treated), whereas less than 1% of males with $Z = 0$ did so. The coefficient on the interaction term between the male dummy and Z gives the causal effect of conscription eligibility. This effect divided by the difference in compliance rates between eligible and non-eligible men approximates the average causal effect of conscription on those who served (Angrist and Pischke 2009).

The key identifying assumption of the DID strategy is that migration trends for men and women would have been the same in the absence of the reform. Figure 1 shows internal labor migration rates by birth year separately for men and women plus differences in these rates. Migration rates for men are generally slightly higher. Prior to 1979, the trends look fairly parallel. The differences after 1978 look roughly similar to the differences in previous years, so visually there appears to be little evidence that the abolition of conscription decreased migration among men. This conclusion, however, might be affected by compositional effects.[4]

Regression Discontinuity (RD)

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[4] The jump observed between 1974 and 1975 is due in part to the composition of the sample. Individuals born in 1974 are drawn from Generation 1992 and are therefore 18 years old. Individuals born in 1975 are drawn from both Generation 1992 (17 years old) and Generation 1998 (23 years old), so on average they are older and more educated than those born in 1974.

Grenet, Hart, and Roberts (2011) and Bauer et al. (2012) use the RD design of the military draft in Great Britain and Germany, respectively, to estimate the effect of military service on conscripts' subsequent labor market outcomes. In the same vein, the abolition of compulsory conscription in France represents a quasi-experiment to which male teenagers (but not males in their twenties) leaving the education system in 1998 were exposed. Figure 2 shows that the proportion of men who did national service by birth year (calculated from Generation 1998) dropped significantly at the threshold date, whereas the propensity to migrate (represented by M_1 and net of a linear trend in birth year) changed little. Visually, therefore, there appears to be little evidence that the abolition of conscription in France reduced migration among men. But, again, this conclusion might be affected by compositional effects.

To estimate the RD effect of conscription, let $(x_i - c)$ denote individual i's birth year centered at $c = 1979$. The following model is fitted on the sample of men drawn from Generation 1998:

$$M_{1i} = \alpha + \tau S_i + X_i'\beta + W_i'\delta + u_i, \tag{1}$$

where X_i is a vector of polynomial terms in $(x_i - c)$, W_i is a vector of individual-level controls, and u_i is an error term. The treatment group is males born before 1979 $(x_i < c)$ and therefore not exempted from service, and the control group is males born after 1978 $(x_i \geq c)$ and therefore exempted from service. Allocation to treatment and control groups is a deterministic function of the birth date, but doing national service is not, because not all men born before 1979 served. Hence, equation (1) is estimated by the instrumental variables (IV) method using $1[x_i < c]$ as an instrument for S_i, $1[\cdot]$ being the usual indicator function (Lee and Card 2008).

A major concern with this approach is that men close to the threshold date on each side may not be comparable in unobservables that affect M_1. Among men who left education in 1998, those in the treatment group are older and generally more educated, and some of them may have stayed on in education in order to delay their service, so they might be intrinsically less migration prone than men in the control group. In that case, potential migration outcomes and $1[x_i < c]$ would be negatively correlated, which would tend to bias both the reduced-form effect of $1[x_i < c]$ on M_{1i} and $\hat{\tau}$ in the negative direction.[5]

Timing of Service

Inspired by Lee's (2012) strategy for disentangling treatment and selection effects, I utilize the timing of service among those who served to tackle the selectivity issue. For conscripts who worked before doing their service, entry moves as represented by M_2 cannot be influenced by the service itself. However, these individuals were non-randomly selected into the army, so comparing their migration decisions to those of non-conscripts should provide information on the selection effect. Furthermore, under the assumption that the decision as to whether to do their service before or after working is uncorrelated with unobservables related to mobility, differences in the propensity to migrate between conscripts who served before working and conscripts who served after working may reveal the causal effect of conscription on post-service migration.

Results

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[5] This argument is different from that used by Granier, Joseph, and Joutard (2011) to discard the RD strategy. They argue that the discontinuity in the proportion of men who served was created by men who did their service at a time when it was easier to avoid it, which questions the validity of the proportion who served as an instrument for S.

I start this section by showing the results of OLS comparisons of migration outcomes by conscription status, then go on to report estimates derived from the estimation strategies presented above. Finally, I discuss some implications of the main findings and consider their external validity. Except when noted, regressions contain a complete set of dummies for commuting zone of residence at the end of studies plus the following personal determinants of onward migration (Schlottmann and Herzog 1981; DaVanzo 1983; Faggian, McCann, and Sheppard 2007; Newbold and Cicchino 2007): Age reached in the year of leaving education, dummies for educational attainment, and a dummy indicating a change of zone between the start of secondary education and the end of studies. The vector with these individual-level covariates is denoted by W.

OLS Estimates

A simple OLS regression of M_1 on S, W, and a complete set of year-of-birth dummies yields the results reported in Table 1. In the sample of men drawn from Generation 1992, conscripts are 3.0 (*S.E.* 0.9) percentage points (ppt) more likely to migrate at the entry job stage than non-conscripts. The estimated effect in the sample of men drawn from Generation 1998 is substantially larger: 6.1 (*S.E.* 1.0) ppt, which amounts to 17% of average migration in the 1998 sample. The estimate obtained in the sample drawn from the three editions of Generation suggests that conscripts are 4.7 (*S.E.* 0.7) ppt more mobile. The corresponding average marginal effects (AMEs) yielded by probit regressions are almost identical. These estimates do not account for selective recruitment by the army or for the possibility that migration-prone individuals could be less reluctant to serve. This latter reason may be behind the larger coefficient estimated in the 1998 sample, as after the announcement of the abolition of military service it became easier to avoid it, and those who then served had to be less reluctant to do so.

As to personal determinants of onward migration, the likelihood of migration increases by 0.6 (*S.E.* 0.3) ppt as men age. As expected, the higher the education level attained, the greater the propensity to migrate. In column (3), for example, men with a bachelor's degree are 32.7 (*S.E.* 1.7) ppt more likely to migrate than men without a CAP/BEP diploma, which represents a 96% increase in the average propensity to migrate in the whole sample of men (0.339). Having migrated during their studies increases the degree of mobility, especially in the oldest sample, where with all else being equal prior migrants are 7.8 (*S.E.* 1.5) ppt more likely to migrate.

DID Estimates

Tables 2 and 3 present the main results yielded by the DID strategy. Table 2 shows coefficients from regressions containing all individuals, while Table 3 shows results obtained by using subsamples of the data. In column (1) of Table 2, M_1 is regressed on $Male*Z$, W, and complete sets of year-of-birth and gender dummies. The size of the coefficient estimate on $Male*Z$ is 0.015 (*S.E.* 0.008) and it is statistically different from zero at around the 5% significance level. This suggests that men born before 1979 are 1.5 ppt more likely to migrate at the entry job stage than comparable men born later. This effect divided by the difference in compliance rates between the two groups (0.53) approximates the causal effect of conscription on men who served: 2.8 ppt, which amounts to 8% of the average propensity to migrate at the entry job stage. The size of the effect changes little when a linear trend interacted with the male dummy is added to the specification (column 2), or when probit regressions are run and the AME of $Male*Z$ is calculated following Ai and Norton (2003).[6]

[6] I also checked whether the probability of working abroad (and, thus, of being excluded from sample) differs between eligible and non-eligible men. *Ceteris paribus*, eligible

Migration propensities tend to rise with education, and since military service induced individuals to remain in education, it might have fostered migration for reasons unrelated to repeat migration. To assess the size of the total effect (direct and indirect) of conscription, I re-estimate the models dropping the personal determinants. Results are presented in columns (3) and (4) of Table 2. The equality of the coefficient on $Male*Z$ in the models including and excluding personal determinants is rejected (p-values < 0.05),[7] but the total effect is larger under common trends only.

The ability to migrate provided by military service might be greater for groups on whom the military service could have a greater impact. Hence, I re-estimate the models with observations classified according to the father's last socio-occupational category[8] and to the type of municipality (rural/urban) at the end of studies. Results are presented in Table 3. The data reveals no significant shift in the relative propensity to migrate of men with lower-class origins, but men from upper-class families born before 1979 are about 4 ppt more likely to migrate than comparable men born later. This effect divided by the difference in compliance rates between the two groups of upper-class men (.53) yields a causal effect of conscription on male conscripts from upper-class families of about 7.5 ppt, or 20% of the average propensity to migrate of upper-class men (0.379). As to the rural/urban split, equality of conscription eligibility effects

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men are more likely to work abroad, but the difference is small, especially after accounting for a linear trend for men.

[7] Cross-model tests were conducted with the help of Stata command *suest*.

[8] This characteristic is missing for 9% of observations. As in Goux and Maurin (1997), children of farmers, routine non-manual employees, and manual skilled and non-skilled workers form the lower class; children of employers, managerial employees, and technicians, supervisors, and other lower-grade professionals form the upper class.

cannot be rejected whether or not common trends are assumed (p-values > 0.53). Since both urban and rural residents show similar differences in compliance rates between eligible and non-eligible men, the causal effect of conscription on rural/urban men who serve seems to be about the same.

RD Estimates

Table 4 presents the main results for the RD approach. The personal determinants now exclude age as its effect is captured by $(x_i - c)$. In column (1), X_i consists of $(x_i - c)$; column (2) adds $(x_i - c)^2$; columns (3) and (4) allow for extra flexibility by adding the interaction between $1[x_i < c]$ and X_i to the controls used in columns (1) and (2), respectively. The first entry in each column gives the reduced-form effect of $1[x_i < c]$ on M_{1i}, the second entry is the first-stage effect of $1[x_i < c]$ on S_i, and the third entry gives $\hat{\tau}$. Following Kolesár and Rothe (2018), the standard error for $\hat{\tau}$ is the usual heteroskedasticity-robust IV standard error.

In column (1), men born before 1979 migrate *less*, on average, than men born later. The difference of 2.9 (*S.E.* 1.3) ppt attains statistical significance at 5%. Men born before 1979 are also 13.7 (*S.E.* 1.1) ppt more likely to be conscripts. The estimate for τ suggests that conscripts are 20.9 (*S.E.* 9.9) ppt *less* likely to move than non-conscripts. The negative sign of the reduced-form coefficient and of $\hat{\tau}$ seems to result from the caveat pointed out when discussing the RD strategy. Results remain essentially unchanged when a quadratic term in $(x_i - c)$ or the interaction between $1[x_i < c]$ and $(x_i - c)$ is added to the specification. In column (4), the estimated first-stage effect of $1[x_i < c]$ on S_i is almost zero and the resulting $\hat{\tau}$ is much more imprecisely measured.

I re-estimate model (1) using the bivariate probit method (see e.g. Wooldridge 2010) allowing S_i to be endogenous and including $1[x_i < c]$ in the probit for S_i. (Due to computational difficulties, dummies for commuting zones are replaced by dummies for province of residence at the end of studies.) Table 5 presents the main estimated AMEs plus the estimated correlation for the error terms in the probits for M_{1i} and S_i (denoted by $\hat{\rho}$). The correlation between unobservables that affect M_{1i} and S_i seems small. The estimated AME of $1[x_i < c]$ on S_i is now larger, whereas the estimated AME of S_i on M_{1i}, which is positive in some specifications, is still measured very imprecisely. Overall, the results from this more sophisticated RD analysis do not improve on the visual evidence presented in Figure 2.

Timing-of-Service Estimates

Table 6 shows the main results of regressing M_2 on S, an indicator for having done national service before working (denoted by T), W, and a complete set of year-of-birth dummies. The coefficient estimated on S gives the counterfactual difference in migration rates between conscripts and non-conscripts if all conscripts had worked before doing national service, so that migration could not be influenced by the service itself. This effect plus the coefficient on T gives the total mobility advantage of a conscript over a non-conscript at the entry job stage.

In the sample of men drawn from Generation 1992, the estimated coefficient on S appears to be negative and statistically different from zero (column 1), suggesting that conscripts who worked before doing their service are 5.2 (*S.E.* 1.0) ppt *less* likely to migrate than non-conscripts. A similar result is obtained in comparable samples drawn from Generation 1998 (column 2) and from the three editions of Generation (column 3). This result is also robust to estimation by probit.

This unexpectedly negative coefficient on S is probably the consequence of a previously overlooked anticipatory effect of compulsory conscription. Torun (2016) finds that the expected interruption in civilian life created by military service reduces labor force participation and employment among male teenagers who are waiting to be called up (see also Oi 1967). Additionally, I find evidence that migration for work is less prevalent among young male labor market entrants who are waiting to be called up. The standard human capital model provides a simple explanation for this (see e.g. Borjas 2015). 76% of those who did national service and were employed before that had to change employers on their return, so potential conscripts have a shorter period over which they can collect the returns on migration, which decreases net gains from migration and, hence, lowers the probability of migration.

The existence of an anticipatory effect of conscription precludes estimating its causal effect on post-service migration from the estimated coefficients reported in each column of Table 6. Let ε^s $(\varepsilon^s > 0)$, ε^a $(\varepsilon^a < 0)$, and ε^c $(\varepsilon^c > 0)$ denote the selection, anticipatory, and causal effects associated with conscription, respectively. The coefficient on S gives $\varepsilon^s + \varepsilon^a$. The anticipatory effect is absent in the case of those who did their service before working, so the sum of the coefficients on S and T is $\varepsilon^s + \varepsilon^c$, so the coefficient on T gives $\varepsilon^c - \varepsilon^a$.

But if, as the evidence presented above suggests, the average size of the causal effect on subsequent migration is 0.028, then the result of subtracting this quantity from the estimated coefficient on T would approximate the absolute value of the negative anticipatory labor migration response to conscription. Thus, in the sample drawn from all three editions of Generation (column 3 of Table 6), the size of the anticipatory effect would be 9.5 ppt, which amounts to over a quarter of the average propensity to migrate at the entry job stage.

This size, however, might be somewhat inflated. Because of the deferments linked to working introduced by the 1997 reform, men who left education in 1998 and worked before doing their national service might be less migration prone than comparable men who served before working. Suppose that the selection effect is $\underline{\varepsilon}^s$ for the former group and $\overline{\varepsilon}^s$ for the latter, with $\overline{\varepsilon}^s > \underline{\varepsilon}^s > 0$. Then, in the sample drawn from Generation 1998 the coefficient on T would give $\varepsilon^c - \varepsilon^a + \left(\overline{\varepsilon}^s - \underline{\varepsilon}^s\right)$, and the sum of the coefficients on S and T would be $\overline{\varepsilon}^s + \varepsilon^c$. This sum would be larger than the coefficient on S in column (2) of Table 1, which is about $\frac{\underline{\varepsilon}^s + \overline{\varepsilon}^s}{2} + \varepsilon^c$. Indeed, the 3.3 ppt difference between the sum of the estimated coefficients on S and T reported in column (2) of Table 6 and the corresponding estimated coefficient on S listed in Table 1 is statistically significant (p-value 0.00). The same calculation on the sample drawn from Generation 1992 yields a statistically insignificant difference of 1.0 ppt (p-value 0.15). In the sample from all three editions of Generation, the 1.4 ppt difference is statistically significant (p-value 0.00).

Table 7 presents the results of splitting the data according to the father's last socio-occupational category and whether the municipality is rural or urban. If, as the evidence presented above suggests, the causal effect on subsequent migration for men from lower-class (upper-class) families is zero (0.075), then the anticipatory response will be larger for the former: 11.3 vs. 6.9 ppt. Formally, the hypothesis of equality of anticipatory effects by class location is rejected (p-value 0.01). As to the rural/urban split, if the causal effect on subsequent migration is assumed to be the same for both groups, then the size of the anticipatory effect will be larger for individuals from urban municipalities: The null of equality of coefficients on T by type of municipality is rejected (p-value 0.01).

Discussion

Overall, the evidence presented here suggests that compulsory peacetime conscription has an effect on subsequent internal labor migration which stems from the behavior of individuals from upper-class families, for whom military service significantly increases the post-service propensity to migrate for work. This finding has implications in light of DaVanzo's repeat migration hypothesis. It suggests, for example, that the improvement in collecting and processing migration-relevant information provided by military service, plus the acquisition of knowledge of and the formation of friendships at the place of destination are significant for individuals from upper-class families, but not for individuals with lower-class origins. However, without information on the experiences of conscripts while in service it is not possible to ascertain the specific pathways by which conscription produces these heterogeneous responses by class location.

According to the evidence that compulsory military service reduces the propensity to migrate for work among young men who are waiting to be called up, conscription seems to be acting as a barrier to migration (Klaassen and Drewe 1973), reducing the mobility of labor market entrants who would otherwise have moved to find more productive opportunities. This barrier appears to be stronger for individuals from lower-class families and from urban municipalities, who are the least mobile groups of the population.

There remains the issue of the external validity of these findings. During the period considered, French conscripts had to serve for 10 months and attempts were made to assign them to military bases in their region of residence. A longer period of service may stimulate the formation of location-specific capital at the place of destination, while a greater distance between the domicile and the duty base may reduce the number of trips home and, thus, the amount of location-specific capital built up at the place of residence. Other considerations are whether conscripts change duty bases during military service and how far they travel while in service. For all these reasons, the post-service impact of compulsory conscription on internal labor migration in France may not be perfectly externally valid. The negative anticipatory effect on labor migration would presumably not work in countries where the law requires employers to continue any pre-existing contracts after military service.

Conclusion

I have used the abolition of compulsory conscription in France in 1997 and information on the timing of service among those who served to test whether compulsory peacetime conscription fosters a propensity to migrate at the entry job stage. My most reliable evidence suggests that conscription stimulates the post-service propensity to migrate of male labor market entrants with upper-class origins. Furthermore, conscription reduces the propensity to migrate for work among young men waiting to be called up by approximately a quarter. However, these results may not be perfectly externally valid. Longer periods of service at a duty base further away might intensify the association between conscription and subsequent migration, whereas the negative anticipatory effect on labor migration may not work in countries where the law requires employers to continue any pre-existing contracts after the service.

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Declaration of Interest

The author declares that he has no conflict of interest.

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Appendix

Table A1. Sample selection criteria.

	Generation 1992	Generation 1998	Generation 2001
No. of individuals reaching the age of 16–23 in the year in which they leave education	22,721	41,389	8,781
After deletion for missing demographic data	22,426	40,950	8,708
After deletion for not living in France at the start of secondary education or at the end of studies	22,330	40,650	8,708
After deletion for not having at least one job during the survey period	21,716	38,577	8,449
After deletion for not having the entry job in a known place of France[a]	21,190	32,813	7,159
After deletion for working before doing national service and not having job location(s) in known places of France	21,061	31,893	7,159
After deletion for having the place of residence on leaving education and the location of the entry job in overseas territories.	21,059 (11,577)	31,893 (17,308)	7,159 (3,401)

Notes: No. of males in parentheses. [a]: Most cases have the entry job located abroad.

Table A2. The effect of the 1997 reform on three educational outcomes.

	Age in the year leaving education	High school graduation[a]	Years of completed education
Male*(1973–78)	Ref.	Ref.	Ref.
Male*(1979–82)	-.27 (.03)	-.050 (.009)	-.13 (.04)
Year-of-birth and gender dummies	Yes	Yes	Yes
R-squared	.57	.36	.39
Observations	46,651	46,651	46,651

Notes: OLS estimates. Sample drawn from Generation 1992, 1998, and 2001, containing individuals born in 1973–82 reaching the age of 16–23 in the year in which they leave education. Heteroskedasticity-robust standard errors in parentheses. [a]: The individual passed the baccalauréat, which is the general secondary-school leaving diploma.

Table A3. Descriptive statistics.

	Generation 1992		Generation 1998		Generation 2001	
	Men	All	Men	All	Men	All
Migrated at the entry job stage (M_1)	0.317	0.306	0.354	0.348	0.338	0.339
Migrated at the entry job stage (M_2)	0.282	0.287	0.342	0.341	0.338	0.339
Conscript $(S=1)$	0.712	0.394	0.228	0.126	0	0
National service before working[a]	0.432	0.432	0.542	0.540		
Age (in years)	20.0 (1.7)	20.0 (1.7)	20.1 (1.8)	20.3 (1.8)	20.4 (1.8)	20.8 (1.7)
Born before 1979	1	1	0.591	0.652	0.176	0.214
Bachelor diploma[b]	0.058	0.075	0.065	0.094	0.119	0.133
Short-cycle tertiary education[c]	0.202	0.210	0.259	0.316	0.176	0.276
Upper secondary education (*Baccalauréat*)	0.213	0.224	0.179	0.179	0.361	0.299
Secondary education (CAP/BEP)[d]	0.431	0.401	0.381	0.312	0.293	0.258
Less than a CAP/BEP diploma	0.095	0.090	0.116	0.098	0.050	0.034
Moved during studies[e]	0.113	0.131	0.109	0.140	0.176	0.214
Individuals	11,577	21,059	17,308	31,893	3,401	7,159

Notes: Standard deviations in parentheses. [a]: Proportion among conscripts. [b]: Degrees awarded after more than two years of post-baccalauréat study. [c]: Degrees awarded after two years of post-baccalauréat study (DEUG, BTS, DUT). [d]: Vocational diplomas. [e]: Indicator for moving between the start of secondary education and the end of studies.

Tables

Table 1. OLS estimates of the effect of compulsory peacetime conscription on the propensity to migrate at the entry job stage.

	Dependent variable: M_1		
	Generation 1992	Generation 1998	Generation 1992, 1998, 2001.
	(1)	(2)	(3)
Conscript $(S=1)$	.030*** (.009)	.061*** (.010)	.047*** (.007)
Age			.006** (.003)
Bachelor diploma	.391*** (.031)	.282*** (.023)	.327*** (.017)
Short-cycle tertiary education	.175*** (.022)	.104*** (.017)	.143*** (.013)
Upper secondary ed. (Baccalauréat)	.082*** (.020)	.010 (.016)	.043*** (.011)
Secondary education (CAP/BEP)	.012 (.016)	-.023* (.013)	-.002 (.009)
Less than a CAP/BEP diploma	Ref.	Ref.	Ref.
Moved during studies	.078*** (.015)	.026** (.012)	.037*** (.009)
Intercept	-.179*** (.048)	.019 (.027)	-.238*** (.074)
Year-of-birth and commuting zone dummies	Yes	Yes	Yes
R-squared	.13	.12	.11
Observations	11,440	17,145	31,986

Notes: Samples containing men reaching the age of 16–23 in the year in which they leave education. In columns (1) and (2), age is perfectly collinear with year-of-birth dummies. Heteroskedasticity-robust standard errors in parentheses. *: Significant at 10%. **: Significant at 5%. ***: Significant at 1%.

Table 2. Difference-in-differences estimates of the effect of compulsory peacetime conscription on the propensity to migrate at the entry job stage.

	Dependent variable: M_1			
	Including personal determinants		Excluding personal determinants	
	(1)	(2)	(3)	(4)
Male $*$ Z	.015* (.008)	.016 (.012)	.018** (.008)	.010 (.012)
Year-of-birth, gender, and commuting zone dummies	Yes	Yes	Yes	Yes
Male*Birth year	No	Yes	No	Yes
Observations	59,801			

Notes: Sample drawn from Generation 1992, 1998, and 2001, containing individuals reaching the age of 16–23 in the year in which they leave education. $Z=1$ for individuals born before 1979 and $Z=0$ otherwise. Heteroskedasticity-robust standard errors in parentheses. *: Significant at 10%. **: Significant at 5%.

Table 3. Difference-in-differences estimates of the effect of compulsory peacetime conscription on the propensity to migrate at the entry job stage, by class location and rural/urban type of municipality.

	Dependent variable: M_1			
	Lower class	Upper class	Rural	Urban
Excluding Male*Birth year	(1)	(2)	(3)	(4)
Male $*Z$	.004 (.010)	.039** (.016)	.015 (.020)	.016* (.009)
Including Male*Birth year	(5)	(6)	(7)	(8)
Male $*Z$	-.003 (.016)	.043** (.022)	.033 (.031)	.012 (.013)
Personal determinants plus year-of-birth, gender, and commuting zone dummies	Yes	Yes	Yes	Yes
Observations	35,018	19,397	10,910	48,891

Notes: Samples drawn from Generation 1992, 1998, and 2001, containing individuals reaching the age of 16–23 in the year in which they leave education. $Z=1$ for individuals born before 1979 and $Z=0$ otherwise. Heteroskedasticity-robust standard errors in parentheses. *: Significant at 10%. **: Significant at 5%.

Table 4. Reduced-form, first-stage, and IV estimates of the effect of compulsory peacetime conscription on the propensity to migrate at the entry job stage.

	(1)	(2)	(3)	(4)
Reduced form (dependent variable: M_1)				
Born before 1979 $(x_i < c)$	-.029** (.013)	-.034** (.014)	-.027** (.014)	-.008 (.026)
First stage (dependent variable: S)				
Born before 1979 $(x_i < c)$	.137*** (.011)	.166*** (.009)	.128*** (.011)	.038 (.024)
IV (dependent variable: M_1)				
Conscript $(S = 1)$	-.209** (.099)	-.202** (.084)	-.213** (.107)	-.208 (.703)
Polynomial in $(x_i - c)$	Linear	Quadratic	Linear	Quadratic
$1[x_i < c]$*polynomial in $(x_i - c)$	No	No	Yes	Yes
Personal determinants plus commuting zone dummies	Yes	Yes	Yes	Yes
Observations	17,145			

Notes: Sample drawn from Generation 1998 containing men born in 1975–82. Heteroskedasticity-robust standard errors in parentheses. Personal determinants do not include age. $1[\cdot]$ is the usual indicator function. **: Significant at 5%. ***: Significant at 1%.

Table 5. Bivariate probit estimates of the effect of compulsory peacetime conscription on the propensity to migrate at the entry job stage. Average marginal effects.

	(1)	(2)	(3)	(4)
Dependent variable: S				
Born before 1979 $(x_i < c)$	.231*** (.008)	.221*** (.013)	.253*** (.008)	.234*** (.007)
Dependent variable: M_1				
Conscript $(S = 1)$	-.028 (.066)	-.014 (.072)	.042 (.094)	.162* (.090)
Polynomial in $(x_i - c)$	Linear	Quadratic	Linear	Quadratic
$1[x_i < c]$*polynomial in $(x_i - c)$	No	No	Yes	Yes
Personal determinants plus province dummies	Yes	Yes	Yes	Yes
$\hat{\rho}$	.15 (.12)	.13 (.13)	.04 (.16)	-.16 (.15)
Observations	17,145			

Notes: Sample drawn from Generation 1998 containing men born in 1975–82. Heteroskedasticity-robust standard errors in parentheses. Personal determinants do not include age. $1[\cdot]$ is the usual indicator function. *: Significant at 10%. ***: Significant at 1%.

Table 6. Timing of service and propensity to migrate at the entry job stage.

	Dependent variable: M_2		
	Generation 1992	Generation 1998	Generation 1992, 1998, 2001.
	(1)	(2)	(3)
Conscript $(S=1)$	-.052*** (.010)	-.088*** (.012)	-.062*** (.007)
National service before working $(T=1)$	.092*** (.010)	.182*** (.015)	.123*** (.008)
Personal determinants plus year-of-birth and commuting zone dummies	Yes	Yes	Yes
Observations	11,577	17,308	32,286

Notes: OLS estimates. Samples containing men reaching the age of 16–23 in the year in which they leave education. Heteroskedasticity-robust standard errors in parentheses. ***: Significant at 1%.

Table 7. Timing of service and propensity to migrate at the entry job stage, by class location and rural/urban type of municipality.

	Dependent variable: M_2			
	Lower class	Upper class	Rural	Urban
	(1)	(2)	(3)	(4)
Conscript $(S=1)$	-.058*** (.010)	-.069*** (.013)	-.029 (.018)	-.071*** (.008)
National service before working $(T=1)$	.113*** (.011)	.144*** (.015)	.079*** (.021)	.134*** (.009)
Personal determinants plus year-of-birth and commuting zone dummies	Yes	Yes	Yes	Yes
Observations	18,933	10,555	6,249	26,037

Notes: OLS estimates. Samples drawn from Generation 1992, 1998, and 2001, containing men reaching the age of 16–23 in the year in which they leave education. Heteroskedasticity-robust standard errors in parentheses. ***: Significant at 1%.

Figures

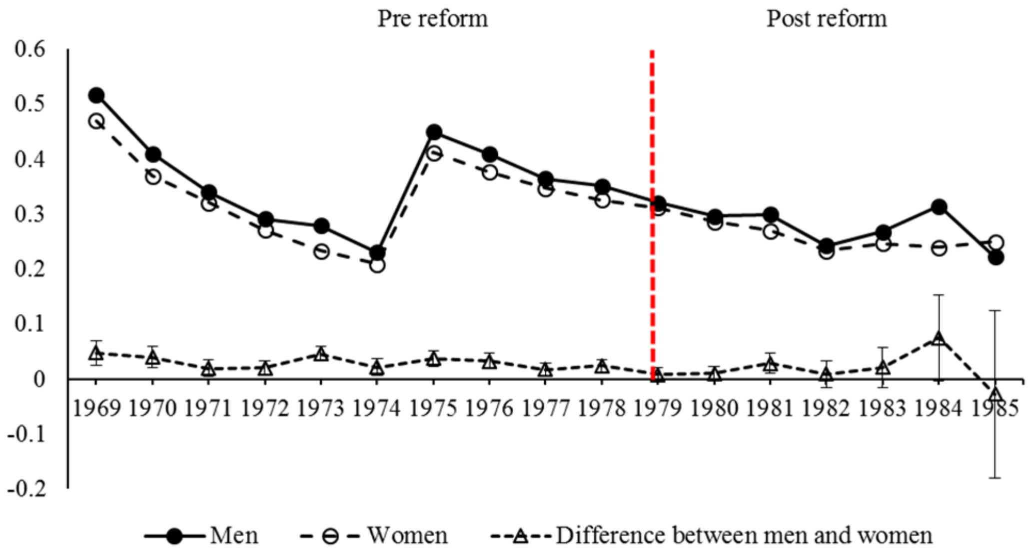

Figure 1. Proportion migrating at the entry job stage and difference in migration proportions between men and women, by birth year.

Notes: Sample drawn from Generation 1992, 1998, and 2001, containing individuals reaching the age of 16–23 in the year in which they leave education. Circles represent the ratio of migrants as measured by M_1 to entry job acceptances. Triangles represent differences between men and women in the proportion migrating. Error bars show 95% confidence intervals robust to heteroskedasticity.

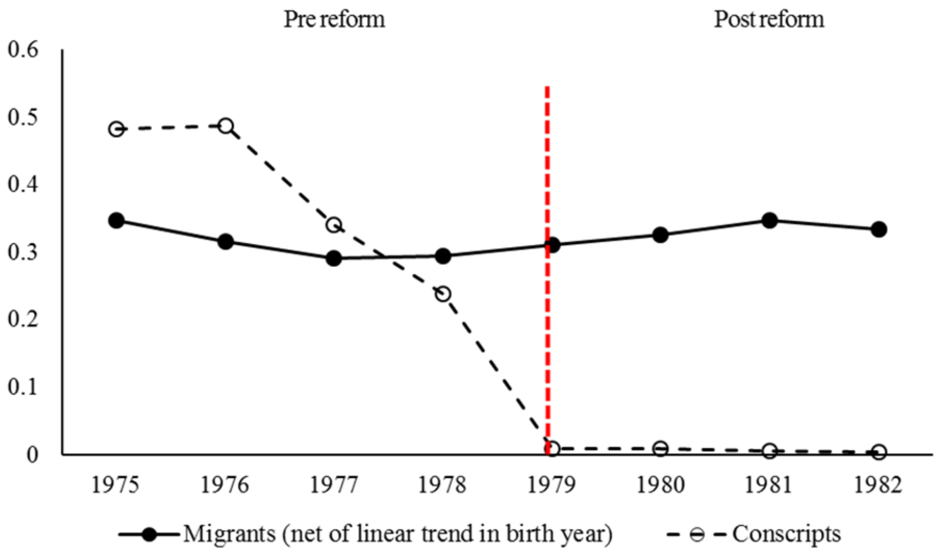

Figure 2. Proportion of men migrating at the entry job stage and proportion doing national service, by birth year.

Notes: Sample drawn from Generation 1998 containing men born in 1975–82. Solid circles represent the ratio of migrants as measured by M_1 to entry job acceptances net of a linear trend in birth year. Hollow circles represent the proportion of conscripts in the corresponding birth cohort.