Mobilizing Forces: Military Conscription as a Driver of Mobility in Argentina

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Abstract

A vast literature documents the presence of unexploited potential gains from migration within a country, in both developing and developed economies. One possible explanation of what prevents people from migrating is that a lack of experience living outside one's hometown earlier in life could make it more difficult to migrate later on. In this paper, I use the natural experiment of military conscription in Argentina, which randomly assigned not only military service, but also the location of service, to study the effect of this temporary displacement on long-run migration rates. I then use a rich source of administrative earnings and employment data to investigate the labor-market implications of conscription and, in particular, displacement. I find that conscription on the whole caused a small increase in the likelihood of appearing in the formal labor force, and a small increase in earnings particularly for those who were assigned to serve in the Navy. Assignment to military service outside of one's province of origin increased the likelihood of living outside the province of origin by 2.5 percent, and while the net effects of this displacement on earnings and employment are imprecisely estimated, the evidence suggests that there are modest long-term benefits of conscription in Argentina that are not fully attributable to displacement.

Acknowledgements

1 Introduction

A broad literature in economics documents the presence of unexploited potential returns to migration within both developing and developed countries. That is to say, workers have the potential to increase their earnings simply by moving to a different location within the same country, but often are not taking advantage of this opportunity. Indeed, a striking proportion of people live quite close to their place of birth. In Argentina, for example, as of the Census of 2001, about 80 percent of native-born citizens lived in their province of birth (constructed from IPUMS data, Minnesota Population Center 2019). In the US, the corresponding figure was about 69 percent as of 2009 (Molloy, Smith, and Wozniak 2011). While such figures do not necessarily imply a market failure or aggregate welfare loss, they strongly suggest that workers are not choosing their location of residence solely on the basis of where they would receive the highest returns to their work and skills. This could mean that households are more vulnerable to shocks to the local labor market than would be implied by a standard spatial equilibrium model in which workers are perfectly mobile.

One possible explanation for the high proportion of people living near their place of birth is that migration might be more difficult for people with no experience living outside of their original hometown. In such cases, people might find it particularly difficult to imagine finding a new place to live, a new job, new social connections, and the like. A corollary to this is that if these same people were to be induced to live outside of their hometown even temporarily, it might make it easier for them to migrate later on in life. In other words, moving once, or being temporarily displaced, might make it easier to relocate in the future. This, in turn, could make it easier to pursue new economic opportunities in a different part of the country, or to leave in the face of a local economic downturn, such as the departure of a major local employer. In keeping with this idea, Bryan, Chowdhury, and Mobarak (2014) offer groundbreaking experimental evidence that a one-time incentive to migrate temporarily for seasonal work encouraged workers to migrate temporarily not just once, but also again the following year without any further incentive. A question that this study could not speak to

is whether those same workers might, in the long run, be more likely to migrate permanently in pursuit of economic opportunity.

This paper is the first, to my knowledge, to specifically address the question of whether a temporary relocation can encourage higher migration rates for individuals in the long run, and to investigate what implications this might have for labor market outcomes. Specifically, I exploit random variation in both the assignment of military conscription in Argentina and, for a subset of conscripts, in the assignment of the location of service. Estimates from my preferred specifications indicate that while conscription in general did not have an appreciable effect on long-term migration, *displacement*, that is, being assigned to military service outside of one's home province, increased the likelihood of being observed in recent years living outside of that province by approximately 2.5 percent. The same specifications suggest that displacement was associated with a small and not statistically significant increase in earnings of about 0.3 percent, though this might mask a larger return accrued to those actually induced to migrate. Separately, I find that conscription on the whole caused a small increase in the likelihood of appearing in the formal sector, and a small increase in earnings particularly for those who were assigned to serve in the Navy.

Several features of the conscription system in Argentina are useful for the purposes of this paper:

First, conscription was assigned through a random lottery, which determined not only whether or not young men[1] were assigned to serve, but also the branch of the military in which they would serve – either the Army, Air Force, or Navy. This randomization permits a causal interpretation for estimated effects of conscription on various outcomes of interest.

Second, this assignment was based on objective and observable characteristics: the year of birth and the last 3 digits of an individual's National ID number, hereafter referred to as the DNI (*Documento Nacional de Identidad*). This meant that even if an individual were to defer service for a number of years, his assignment was fixed by the initial drawing

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[1]Only men were subject to conscription.

and would not change – thus, the assignment was not easily susceptible to manipulation or strategic behavior. Even the wealthy and well-connected generally found it difficult, though not impossible, to avoid military service if assigned. The fact that these criteria determined the conscription assignment also make it possible to observe the intended assignment using current administrative records, and thus to study the long-term effects of the program without directly observing whether an individual actually completed his assigned service.[2] See Section 2.1 for more details on the history and implementation of the conscription system.

Third, the fact that branch assignments were randomized also means that for the subset of the population living close to an Army base, but far from an Air Force or Navy base, there was random assignment to complete military service close to or far from home. Though I am not able to directly observe where (or if) an individual completed his service, typically, conscripts were sent to the nearest available base corresponding to their assigned branch. Henceforth, I will refer to the assignment of an individual to a branch of the military that did not have a corresponding base in his home province as a (temporary) displacement.[3]

A fourth feature that makes this an attractive research setting is the relative lack of confounding factors. Consider, for example, the conscription setting that has been studied most extensively, starting with Angrist (1990): the case of the Vietnam Draft in the US. Though there was nearly random assignment of draft status, deferrals for students allowed many wealthier children to avoid the draft, and among those who were drafted, many were sent to an unfamiliar place, fought in a violent and traumatic conflict, and upon returning home were at least supposed to have had access to various veterans' benefits, including college education and healthcare coverage. It would be challenging for any study to separate out these different factors in understanding the long-term effects of the draft assignment. In the Argentine case, with the exception of the brief Malvinas (Falklands) War with the UK, conscripts served in peacetime. As the service was structured to be a basic obligation of

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[2]Of course, this also means that I am restricted to Intent-to-Treat estimates, which might understate the effects of conscription on those who actually completed the service.

[3]Some people may have been required to serve far from home even if there had been a base nearby. This may cause me to underestimate the effects of displacement by classifying some leavers as stayers.

citizenship, no special benefits were established for ex-conscripts (again, with the exception of those who fought in the Malvinas). In the popular imagination, a colloquial nickname for the mandatory military service program, *colimba*, is reputed to derive from a description of daily life as a conscript: *correr, limpiar, barrer* – run, clean, sweep (Ablard 2017).[4] In other words, the 1–2 years spent as a conscript were widely felt to be tedious, a waste of time, and not necessarily an opportunity to learn new skills, but also generally less traumatic than being sent into combat. That said, I do not rule out the possibility that conscripts may have gained valuable skills or social connections, nor that they may have been exposed to some level of violence even in peacetime. Rather, I argue that in an analysis of conscription in Argentina, it is reasonable to attribute any effects uncovered directly to some aspect of the experience of conscription, rather than to external or secondary factors such as benefits or programs for veterans.

A final attractive feature of this setting is the availability of rich sources of administrative data. I was able to access several years of voter rolls, which should capture nearly the entire universe of adult citizens of Argentina (as long as they are still alive and in the country), along with over 20 years worth of monthly, employer-employee matched records of formal sector earnings collected by the Ministry of Labor. Because both data sources include the year of birth and DNI, I am able to link them to each other and to each individual's conscription assignment to build a detailed picture of the long-term economic effects of this natural experiment.

As noted above, this paper uses the random assignment of individuals to a military branch without a base nearby as a metric for temporary displacement. Of course, the differential effect of being assigned to one branch of the military versus another cannot necessarily be attributed solely to displacement, even if this is one difference between these assignments. Different branches might provide different training, experience, levels of exposure to violence, and access to social networks and employment opportunities. In order to address

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[4]The etymology is not entirely clear.

this concern, I take a difference-in-differences based approach. The simplest version of this approach is as follows: because all provinces but one housed at least one Army base, I take the effect of being assigned to the Air Force (Navy) compared to the Army for people from provinces *with* an Air Force (Navy) base, and then compare this difference to the analogous difference for people from provinces *without* an Air Force (Navy) base. For this latter group (i.e. provinces without an Air Force or Navy base), assignment to a branch for which there is no base in the province is a displacement, but in all other respects the difference between the experience of being assigned to the Air Force (Navy) compared to the Army would ideally be comparable.

Indeed, this is the identifying assumption underlying this analysis: that the difference between the experience of serving in the Air Force or Navy compared to serving in the Army (in terms of training, exposure to violence, etc.) is orthogonal to the presence of an Air Force/Navy base in a province. In other words, if an Air Force or Navy base were to be built in a province that previously did not have one, the assertion is that the effects of service in the Air Force/Navy *compared to the Army* would fall into line with the effects observed in other provinces that already had Air Force/Navy bases present.[5] This assumption would be violated if in provinces lacking Air Force/Navy bases, the distribution of skills pre-conscription or of opportunities available afterwards is such that workers in these provinces would have benefited differentially from the training received in the Air Force/Navy versus training received in the Army. If, however, this identifying assumption holds, then the difference-in-differences estimates as described can be fully attributable to the effect of displacement. It is important to note that this does *not* require the locations of bases to be as-if-randomly assigned, nor do provinces with and without Air Force/Navy bases need, in principle, to be similar on observable measures.[6] It also does not need to be the case that the effects of conscription are uniform across people from different provinces. As long as

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[5]This is equivalent to the assumption of parallel trends.

[6]This is true of any difference-in-differences analysis: treatment and control groups do not need to be statistically balanced at baseline as long as there are parallel trends.

heterogeneity in the effects of conscription itself can be effectively captured by controlling for differences in the effect of assignment to the Army, then it will be possible to isolate a pure displacement effect, separated from the effects of service in the Air Force or Navy.

This paper offers two main contributions to the literature on the economics of migration: it identifies a potential barrier to migration that has received little attention to this point, and studies the long-run effects of lowering this barrier. This follows in the long tradition in development economics grappling with the puzzle of large, persistent wage gaps within a country, with wages generally low in rural sectors and higher in urban areas, and the related question of why such wage gaps are not closed via workers migrating (see, for example: Lewis 1954; Harris and Todaro 1970; Young 2013; Bryan et al. 2014; Munshi and Rosenzweig 2016; Bazzi et al. 2016; Morten and Oliveira 2018; Lagakos et al. 2018; Bryan and Morten 2019; Lagakos et al. 2020. For a recent overview of this literature, see Lagakos 2020). In the canonical spatial equilibrium model of Rosen (1979) and Roback (1982), any differences across locations in quality of life due to real wages or amenities present an arbitrage opportunity for workers that can be exploited through migration (assumed to be costless); as such, in equilibrium, at least at the margins, workers will be indifferent between their chosen location and the next-best one, and thus will move away in the face of any local market downturn, causing rents to fall and real wages to rise in the negatively-shocked region, and essentially smoothing the shock across the entire economy. Starting from this framework, Topel (1986) was first (or among the first) to note that introducing costly migration to this model implies that less mobile workers will face larger negative earnings effects in the face of a negative shock, and many recent papers (for example: Yagan 2014; Bartik 2018; Zabek 2019) have documented that workers do not fully adjust to local downturns and thus do face negative labor market effects. These results highlight the importance of gaining a better understanding of the barriers to migration.

A number of studies have documented evidence of various barriers to migration in both developing and developed economies, from high rents due to zoning regulations (Hsieh and

Moretti 2019), to informal insurance networks (Munshi and Rosenzweig 2016), to basic road quality and transportation costs (Morten and Oliveira 2018). The idea that a lack of experience living outside of one's hometown might itself be a barrier to migrating has intuitive appeal: learning by doing is a feature of all kinds of human activities, it is easy to imagine that moving to a new place could be one such activity.[7] It is also backed by a number of pieces of suggestive evidence: Angrist and Chen (2011) find that Vietnam conscripts are more likely to live outside of state of birth than non-conscripts; Cadena and Kovak (2016), Schündeln (2013), and Basso and Peri (2020) all document how people who have immigrated internationally tend to migrate within their new home country at much higher rates than native-born citizens, thus helping to equilibrate local labor markets; Malamud and Wozniak (2012) provide quasi-experimental evidence that going to college (which at least for some people means temporarily relocating) causes people to have a higher propensity to migrate.[8] Most notably, Bryan, Chowdhury, and Mobarak (2014) show that experimentally inducing people to migrate seasonally from rural Bangladesh to the capital city makes them more likely to temporarily migrate again the following year.[9] But in general, even if we see that people who have experience moving also have a greater propensity to migrate again, it is difficult to establish a causal relationship; as with any study of migration, one has to contend with the fact that migrants are in most cases self-selecting, and thus that the association between moving experience and future mobility could be driven by some underlying preference or higher suitability for migration among the migrant group compared to the stayers. This paper is the first, to my knowledge, to directly investigate the causal relationship between

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[7]Another plausible mechanism is that strong social ties bind people to their hometown (cf. Zabek 2019), and that leaving even temporarily helps to proverbially "cut the cord" and make these ties less constraining.

[8]It should be noted, however, that in this last paper, the authors do not find evidence that the increased likelihood of migration is associated with the likelihood of going to college out of state. Nonetheless, even going to an in-state college often means establishing a temporary residence outside one's family home, so it could still be that this experience away from home is at least part of what encourages higher migration rates.

[9]Indeed, this study was in large part inspired by this finding. What the Bryan et al. (2014) experiment could not answer − at least not yet − is whether their "treated" group was in the long run also more likely to migrate *permanently*: to do so would require tracking down and following up with participants over several decades.

temporary displacement and subsequent increases in mobility.

This study also contributes to the literature that uses various types of random experiments or quasi-random relocation shocks (such as natural disasters) to study the economic effects of migration (examples include Bauer et al. 2013; Bryan et al. 2014; Chetty et al. 2016; Chyn 2018; Deryugina et al. 2018; Nakamura et al. 2019; Sarvimäki et al. 2019; Becker et al. 2020). The specific setting of this paper offers two key innovations to the existing body of research. First, in contrast to most migration-inducing shocks, peacetime conscription is temporary and does not generally entail a catastrophic, life-altering shock (e.g. a war, volcanic eruption, or hurricane) that makes it impossible to return home. This type of shock, while quite useful as a natural experiment, is often quite permanent and could potentially come with a large (usually negative) wealth shock and psychological distress, thus making it more difficult to attribute outcomes specifically to the relocation, and calling into question the implication that similar effects would be observed for the same people if they had simply decided to migrate on their own. Second, related to the previous point, because I observe both stayers and leavers *among* conscripts, along with an exempted control group, I can directly compare the long term outcomes of conscripts who (likely) stayed close to home and those who were sent far away. If the conscription experience is otherwise similar for stayers and leavers (though this is not a given), it allows me to isolate the effect of displacement from other aspects of the experience, thus potentially offering a higher degree of external validity in the measured effects of relocation.

Finally, this paper also contributes to the vast literature looking at the effects of military service on a wide range of outcomes. This includes numerous studies of the effects of military service on labor market outcomes (Berger and Hirsch 1983; Angrist 1990; Imbens and van der Klaauw 1995; Buonanno 2006; Paloyo 2010; Angrist and Chen 2011; Angrist et al. 2011; Galiani et al. 2011; Grenet et al. 2011; Bauer et al. 2012; Card and Cardoso 2012), along with studies looking at effects on educational attainment (Cipollone and Rosolia 2007; Keller, Wagener, and Poutvaara 2010); health (Bedard and Deschênes 2006; Angrist, Chen, and

Frandsen 2010; Autor, Duggan, and Lyle 2011); alcohol and tobacco use (Goldberg et al. 1991; Eisenberg and Rowe 2009); mortality (Hearst, Newman, and Hulley 1986; Conley and Heerwig 2012); and crime and violent behavior (Yager, Laufer, and Gallops 1984; Beckerman and Fontana 1989; Rohlfs 2010; Galiani, Rossi, and Schargrodsky 2011; Lindo and Stoecker 2014; Gibbons and Rossi 2020).

This study is also among the first to investigate the effects of conscription in a developing country, with the main other example of which I am aware being the study by Galiani, Rossi, and Schargrodsky (2011) of the effects of conscription in Argentina on crime, which serves as the starting point for this paper.[10] Given that conscription is currently more prevalent in developing countries than richer ones, might have different effects on people in the context of a developing country, and could even play a role in the process of economic development and structural transformation, there is good reason to investigate the labor market effects of conscription itself, even beyond the lens of displacement and migration.

2   Background

2.1   Conscription in Argentina[11]

A Brief History of Conscription in Argentina

Argentina's system of conscription, or *Servicio Militar Obligatorio* (Mandatory Military Service), was first enacted in 1901, and remained in place until 1995. When it was first introduced, conscription was presented more as a nation-building project than one of national defense or war-readiness. At that time, unlike in the European countries on which the conscription model was based, there was little concern about war with neighboring countries. There was, however, substantial concern among elites about domestic unrest. A very

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[10]More recently, Ertola Navajas et al. (2020) and Gibbons and Rossi (2020) have also studied long-term effects of conscription in Argentina.

[11]Much of the discussion contained herein draws from the descriptions in Galiani, Rossi, and Schargrodsky (2011) and Ablard (2017), supplemented by my own discussions with Rut Diamint and Jorge Battaglino at Universidad Torcuato di Tella, and with several officials in the Ministry of Defense of Argentina.

large fraction of the country's population was foreign-born or first-generation native-born, and elites saw these new arrivals as "unruly and unassimilated" (Ablard 2017), with little attachment to their new country or sense of Argentine national identity. With the influx of European immigrants also came new ideas and social movements seen as dangerous to the state; in particular, anarchism and syndicalism were popular among the large Italian immigrant community at the turn of the century.

Conscription offered a way to "Argentinize" the (male) populace by, at least in theory, training a generation of obedient, loyal soldiers with a strong sense of Argentine identity and pride. Conscripted forces could also be used to suppress domestic unrest (particularly labor and radical activists), help establish a stronger state presence in remote regions, and violently subdue and ultimately assimilate the indigenous population. Particularly with reforms introduced in 1911, military service was presented as an essential component of citizenship, intimately tied to the introduction of universal secret and obligatory male suffrage: the reform created a system to identify and register all available conscripts, and used that list to build up voter rolls. Identity documents issued in conjunction with this reform were then used to monitor and document compliance with both military service requirements and obligatory voting. By the 1950s, conscription was firmly established as a rite of passage for Argentine men.

For the most part, conscripts completed their military service in peacetime, with the important exception of the 1982 Malvinas (Falklands) War against the UK. However, this does not necessarily imply that military service was always peaceful. Along with the aforementioned role of subduing domestic unrest and indigenous peoples, the military was heavily involved in Argentine politics throughout the 20th century, with frequent coups (some aided by conscripted soldiers) overthrowing democratic governments and periods of rule by military regimes.[12] The last military dictatorship was particularly notorious for human rights

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[12]See Potash (1969, 1980, 1996) for a comprehensive account of the military's role in Argentine politics throughout the century. Notably, this three-volume account makes almost no mention of conscripted soldiers, focusing almost entirely on the actions and decisions of enlisted military officers.

abuses, including kidnappings, torture, forced disappearances, and extrajudicial executions. The extent to which conscripts were involved in these abuses is controversial. Though no conscripts have been formally implicated in any of the legal cases surrounding human rights violations, conscripts were involved in skirmishes with left-wing guerrilla forces, and it is possible that some were involved in other violent activities. With the return to democracy in 1983, the military's prestige and influence in public life greatly diminished, leading to a rapid reduction in the fraction of men called to perform military service. Public outcry surrounding the death in 1994 of a young conscript, Omar Carrasco, at the hands of military superiors, led to the suspension of mandatory military service and the transformation of the military to an all-volunteer force.[13]

The Conscription Process

During the era in which mandatory military service was in effect, the process followed a largely stable timeline:

The first step was issuing DNIs to young men (and ultimately women as well) in the year that they would turn 18. For people born in or after 1968, DNIs were issued at birth.[14] While it was required and expected that all citizens would receive their IDs at the appointed time, it is possible that some people received them on a delay. In theory this could allow individuals to try to manipulate their conscription assignment by delaying their DNI assignment, but there is no evidence, to my knowledge, of this happening, certainly not at any appreciable scale.

Next, on May 31 of the year before a given cohort would be called to serve, the *sorteo*, or draft lottery, took place in the headquarters of the National Lottery in Buenos Aires, and was broadcast live over the radio across the country. Prior to 1976, this took place in the

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[13]This case is cause for further caution about the level of violence to which conscripts were subjected during their service. Ablard (2017) notes that newspapers "frequently reported on the physical abuse of soldiers at the hands of their superior officers," which suggests that this case was not an outlier.

[14]People born before 1968 still had to wait until they were approaching age 18 to receive their documents, even as younger children were receiving them almost immediately at birth.

year that the cohort would turn 20 years old, so that they would begin their service at age 21. Due to a legal change passed in the late 1960s, later cohorts (those born in 1958 or later) were sorted in the year in which they would turn 18, and began service at age 19.[15] The lottery was conducted as follows: 1000 balls numbered from 1–1000 were placed in a lottery wheel to be drawn at random, with each ball representing a sort order for conscription. All men in the given cohort who shared the same last three digits of the DNI would then receive the same sort assignment. Thus, starting with 000 and going through to 999, one ball would be drawn for every possible 3-digit combination of DNI endings. The final results of this sort process would be published in newspapers across the country the following day; see Figure 1 for an example of how the sort results would be recorded.[16] A lower sort number meant one was more likely to be exempted from military service, but the exact cutoffs would be determined at a later date.

Figure 1: Example of *sorteo* result, 1976 cohort

DNI (last 3 digits)	Draft sort order
000	923
001	343
002	627
⋮	⋮
998	276
999	974

In the months after the *sorteo*, all men in the sorted cohort would receive a medical examination to determine whether or not they were physically fit for service. Importantly, this process was completed after the lottery, but before the draft cutoffs were announced, so people would not know with certainty at the time of this examination whether or not they

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[15]This meant that men born in 1956 and 1957 were automatically exempted from military service.

[16]The sort results given in this figure ultimately never went into effect, as mandatory military service was suspended in 1995, a few months after this lottery was completed.

would be expected to serve. In theory, this should mean that it was difficult to selectively avoid service based on their assignment. In practice, however, while it was impossible to know where the exact cutoffs would fall, it was generally feasible to guess with a reasonable degree of accuracy, especially if one's assigned number was very low or very high. As such, people assigned a high sort number might have more incentive to exert effort to manipulate the results of their medical examination, perhaps by bribing or otherwise persuading a doctor to declare them unfit for service. Indeed, Galiani, Rossi, and Schargrodsky (2011) show some evidence of such manipulation, in that in certain cohorts, the rates of actually completing service are somewhat lower among people with higher sort numbers compared to those with lower numbers who were also ultimately assigned to service.[17] However, they also find a dramatic jump in completing service around the cutoff points, suggesting that there was in fact a high degree of compliance with the assigned service.[18] Anecdotally, even fairly well-off and well-connected people found it difficult to get out of military service completely, and at least some considered the service a duty of citizenship and did not make an effort to avoid it. Still, if there was in fact some self-sorting out of the conscription assignment, while it does not affect the validity of the Intent-to-Treat estimates that I produce, it may somewhat complicate their interpretation, as these measures would encompass the net effect of both being assigned to military service and exerting greater effort to avoid conscription.[19]

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[17]It is worth noting here that among the cohorts for which they have this information, the replication data from Galiani et al. (2011) show that the fraction of people assigned to serve who ultimately completed military service was consistently lower than the fraction deemed medically fit to do so, suggesting that some individuals found ways to avoid military service other than obtaining a medical exemption. Specifically, I calculate from their data that 70.1 percent of men assigned to serve in the military from the 1958-1962 cohorts did so, while 8.6 percent of assignees received a medical exemption. This suggests that the remaining 21.3 percent likely found a different way to avoid military service (unless there is some substantial mismeasurement in one or more of these variables).

[18]All of this suggests that it might be a good setting for a Regression Discontinuity analysis, as it is reasonable to assume that the effort function for getting a medical or other exemption is continuous around the cutoff, given that the precise cutoff is not known in advance. Galiani et al. (2011) also provide evidence of a lack of manipulation in the neighborhood of these cutoffs. This could potentially be a useful extension to the analysis I present.

[19]The use of medical exams as a screening mechanism might also mean there was positive selection into military service, as poorer people may have been less healthy, and thus less likely to pass the medical exam (cf. Angrist and Krueger 1994). However, unlike the possible manipulation of medical results just discussed, there is no reason to expect this effect to vary by the assigned sort number, so it should not introduce any further bias or complication to ITT estimates.

Several months later, three cutoffs were announced determining conscription assignments. Sort numbers less than or equal to the first, lowest cutoff number would be exempt from any military service. Above this first cutoff and up to the second, conscripts were assigned to the Army, which took by far the largest share of conscripts. Above the second cutoff and up to the final cutoff, conscripts were assigned to the Air Force. Sort numbers above the highest cutoff were assigned to the Navy.[20] In most years, these cutoffs were assigned uniformly across the country. But for certain cohorts, there was geographic variation in the assignments. Men born in 1955 and 1965 faced different cutoffs depending on which of the 5 Army Corps they fell under, based on where they lived. The variation is more substantial among men born in and after 1966, as cutoffs were for these cohorts were determined separately for each of the 29 Military Districts. Legally, men assigned to serve in the military could automatically request a deferral for up to 2 years without an excuse, and could defer for up to 10 years in order to complete secondary or university education. A few other categories of people were also granted automatic exemptions, such as clergymen or sole providers for dependent parents or children. In addition, Congress would, from time to time, pass amnesty bills, allowing people who had avoided military service for a certain period of time without a formal exemption to be formally excused. Together, all of these factors may help explain why a nontrivial fraction of the male population was able to avoid military service despite having been assigned to it. Anecdotally, people who did comply with their service assignment tended not use the available deferrals, preferring to serve with other people the same age and to get military service over with as quickly as possible.

Finally, in the following year, often in March, conscripts would begin their term of military service. For the first 3 months, they would undergo basic training, and would be required to live full time in the barracks. After this, they would receive their permanent posting. Conscripts in the Army and Air Force were expected to serve for one year total, though

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[20] For certain cohorts or subsets of cohorts, one or more of the branches were excluded from the assignments, such that in effect there were only one or two cutoffs. In other cases, the entire cohort was assigned to military service, meaning that the first cutoff was effectively 0, as the sort assignments ranged from 1 to 1000.

occasionally they would be retained for a few additional months. Conscripts in the Navy were required to serve for 2 years. Once they had completed their required service, this was noted in their ID document. This certification of having completed (or been exempted from) mandatory service was – at least on paper – legally required in order to be employed in a formal sector job, and to get access to some social services.[21]

Life as a Conscript

In order to better understand the "treatment" to which conscripts in this natural experiment were subjected, it will be useful to offer a brief discussion of the actual experience of conscription.

In the popular imagination, conscription was largely considered a wasted year (or two), especially for men from middle or higher class backgrounds. The tasks assigned to conscripts were generally menial, and the stipends paid were in most years extremely small.[22] Unlike the oft-studied former conscripts in the US, no special GI-Bill-type benefits were conferred to conscripts in Argentina over those who were exempted from conscription due to a low lottery number; conscription was structured as a duty for all male citizens, and thus not something that merited any special compensation. The one exception to this was for veterans of the Malvinas (Falklands) War against the UK: people who actually fought in this war are in theory entitled to a suite of benefits. Otherwise, any effects of conscription in Argentina on labor market outcomes are directly attributable to some aspect of the experience of military service, rather than to any health or educational benefits offered afterwards.

Based solely on the popular conception, it may seem reasonable to imagine that conscription really was little more than a temporary displacement and wasted 1–2 years of potential labor market experience. However, this is not a consensus view. Supporters of mandatory

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[21] It is not clear how strictly this requirement was enforced.

[22] I do not have data on the stipends issued from year to year, but my understanding is that while stipends were actually fairly generous for a stretch of years mid-century, enough that poorer conscripts could actually send some money home to their families, in time inflation and budget cuts dramatically reduced stipends to the point where they would cover little more than the purchase of cigarettes.

military service (including people more recently arguing for a return to the program) characterized it as a way to learn discipline and to mature; it was also billed as an on-ramp to the formal labor market and upward social mobility, and for some the first opportunity to access basic medical care, sanitation, and sometimes even basic education and literacy training (Ablard 2017).[23] It is also plausible that conscription offered an opportunity to form new social connections that ultimately may have been economically advantageous, at least for some, and it may also be that some conscripts learned practical skills that were transferable to the civilian labor market, though I do not have evidence specifically validating this. Finally, as was mentioned in the previous sub-section, completing one's conscription assignment offered a "passport" to the formal labor market in a literal sense: if one's identification documents did not indicate completion of required military service, he would (in theory) be ineligible for formal sector employment.

On the other hand, conscription may have also had direct negative impacts on recruits. Despite the characterization of Argentina's conscription program as a "peacetime" draft, there are many reports of conscripts being exposed to some level of violence, even beyond those who were actually sent to fight in the Malvinas War. Reportedly, conscripts routinely faced harassment or hazing at the hands of military officers and strict punishments for minor offenses. Occasionally, this harassment was even lethal, as in the case of the killing of conscript Omar Carrasco that ultimately led to the suspension of mandatory military service in Argentina. Conscripts were also at times involved with quelling domestic unrest, supporting coups, and engaging in skirmishes with guerrillas, militants, or even rival military factions. It is unclear if conscripts were involved in or witness to the various human rights abuses of the Dirty War and the National Reorganization Process under the military junta of the 1970s and '80s, but there is no direct evidence (to my knowledge) of involvement of conscripts, and in fact in rare cases conscripts were victims of such abuses, or were even "disappeared".

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[23]Several current and former military professionals independently mentioned to me in conversation that conscription for many rural young men offered their first encounter with a toothbrush.

All of this is to say that conscription on its own, at least for some recruits, was not merely a temporary displacement. However, displacement was part of the conscription experience for many recruits. Even those who were assigned to serve quite close to home were not allowed to leave the for their first 3 months of service. In fact, the earlier years of the program when the focus was more on nation-building and assimilation of immigrants' sons, active efforts were made to integrate people from different parts of the country, so even if their was a base near their home, they were likely to be assigned elsewhere. Over time, as budgets became more strictly constrained, conscripts started to be more frequently sent to the nearest available base.[24] Given that after the 3-month basic training period, conscripts were allowed to leave the base on weekends, there is good reason to think that it might have made a difference whether or not one was stationed reasonably close to home, as many conscripts would go home on weekends if it was feasible to do so.

The details of this displacement are central to the main research question of this paper, i.e. the effect of temporary displacement on long-term migration decisions. Given that I use the absence of a base corresponding to the assigned military branch in a given province as an indicator of "displacement," it is important to note that in a number of ways, conscripts whom I characterize as "stayers" (i.e. not displaced) may also have faced some level of displacement, which may cause me to underestimate the true displacement effect. As noted, conscripts may not have been assigned to the nearest available base. But even if the nearest available base was in the same province, and even if one was assigned to this base, Argentina's provinces are quite large geographically, so that base might be quite far from home. Conversely, there could be cases where there is no base for one's assigned branch in one's own province, but the nearest base is in a neighboring province and is quite close to the provincial border, such that this assignment does *not* actually require venturing particularly far from home.[25]

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[24]I do not have information about specific deployments of troops or how these decisions were made, nor do I have information on the timing of this shift towards keeping conscripts closer to home.

[25]All of this suggests that it could be useful to develop a more refined measure of displacement. It would certainly be useful to have some individual-level data on deployments, even for a subset of the population, but I was not able to access any such data.

2.2   Conscription as Development Policy

While many countries have, like Argentina, moved away from universal or random conscription in favor of volunteer-based forces, many of the countries that have retained conscription-based systems are developing or middle-income countries.[26] Yet very few studies of military conscription have looked at its effects in a developing country. The primary exception, to my knowledge, is the study of the effects of conscription on crime in Argentina by Galiani, Rossi, and Schargrodsky (2011), to which this paper is, in part, a follow-up.[27]

Since the effects of conscription in developing countries have received little attention in the literature, it is reasonable to ask whether these effects might be different from those observed in developed countries. In particular, one might reasonably wonder whether military conscription could help facilitate the process of structural transformation that historically has been crucial to countries' economic development, perhaps helping to incorporate people from rural, underdeveloped areas into formal labor markets, or making them more willing and able to move into more productive urban areas and/or out of less productive agricultural work.

Certainly, Argentine military propagandists and proponents of conscription embraced this line of thinking. Politicians and military officers offered panegyrics to the "habits of order and discipline" that conscripts would learn, the skills that men of the lower classes could bring back to teach their families, and the benefits of medical attention and basic education that conscripts might receive for the first time (Ablard 2017). Conscription was characterized as essentially "a social service directed to the physical and moral uplift of poor Argentine men," "a passport to formal sector employment," and a source of skills that would allow men to earn a living and support a family (Ablard 2017). Indeed, I do find some empirical evidence that conscription increased rates of formal labor force participation, and

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[26]Examples include Thailand, Iran, Brazil, Colombia, Egypt, Russia, and Turkey. See CIA (2019) for a complete list of military service obligations by country.

[27]As has been noted, Ertola Navajas et al. (2020) and Gibbons and Rossi (2020) also look at the effects of conscription in Argentina, but neither of these papers focuses on migration or labor market outcomes.

perhaps even earnings.

Yet basic neoclassical economic theory, as articulated at least as early as Oi (1967), would suggest that conscription is highly inefficient: if it really provided such a great benefit, people would presumably be willing to volunteer for the service. Setting wages and benefits sufficient to attract people to the military, if the labor market functions well, would be a more efficient way of attracting the people who would be most interested and capable to join the service, minimizes deadweight loss for people taken out of the labor market or higher education. Mandatory conscription, on the other hand, in this view functions essentially as a tax, requiring conscripts to sacrifice valuable labor market experience in exchange for generally below-market wages. This idea has encouraged the move to smaller, better paid, and highly professionalized fighting forces in many countries, including Argentina.

Of course, especially in a developing country, labor markets might not function very well, such that perhaps mandatory service could actually offer a long-term benefit to at least some people who would not otherwise volunteer for it.[28] Interestingly, even in relatively affluent countries, the evidence on the long-term labor market effects of conscription is not as clear-cut as the standard theory would suggest. While some studies find a negative effect of conscription on earnings (Angrist (1990) in the US for Vietnam era conscripts; Imbens and van der Klaauw (1995) in the Netherlands; Buonanno (2006) in the UK), others find no significant effect (Bauer et al. (2012) and Paloyo (2010) in West Germany; Angrist and Chen (2011) and Angrist, Chen, and Song (2011) in follow-up studies of Vietnam conscripts in the US;[29] Grenet et al. (2011) in the UK; Albrecht et al. (1999) in Sweden). Other studies have even found positive effects of conscription, at least for some segments of the population: Card and Cardoso (2012) find a significant 4–5% increase in earnings for conscripts with six

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[28]It's also worth noting that governments have budget constraints, so if leaders perceive a need to build a large military force, it might be impractical to offer a sufficiently high wage to attract the desired number of people, in which case making service mandatory, while still allocatively inefficient, might be difficult to avoid.

[29]This finding does not necessarily contradict the earlier one: they continue to find an initial decrease in earnings for veterans compared to non-veterans, but over time the effect of lost experience fades, and is ultimately canceled out by an increase in educational attainment. It is also worth noting that even Angrist (1990) does not find a negative effect of conscription on non-White veterans.

or fewer years of formal education in Portugal (and no significant effect on more educated groups); Berger and Hirsch (1983) document a slight earnings premium for Vietnam-era veterans in the US without a high school diploma, along with a slight penalty for more educated veterans, though these results may not fully account for the non-random selection bias in veteran status. While it is difficult to draw broadly generalizable conclusions from these varied results, there is at least a plausible case to be made that conscription in some settings might benefit at least a segment of the population, particularly less educated or lower-skilled workers.

There is also some direct evidence that military service might increase long-term geographic mobility. For example, Angrist and Chen (2011) find that being drafted increases the likelihood of living outside of one's birth state by about 3 percentage points. Relatedly, Pingle (2007) notes that much of the recently observed decline in the yearly rate of interstate migration within the US can be attributed to the decrease in the share of the population that is active-duty military, as active-duty military members tend to move much more often than civilians.[30] Malamud and Wozniak (2012) also find that the increased likelihood of going to college in order to *avoid* the Vietnam draft caused people to be more likely to migrate, but interestingly they find that this effect is not necessarily associated with going to an out-of-state college.

Despite the fact that primary focus of this paper is on the impact of displacement due to conscription on long-term mobility, a positive result to this effect does *not* imply that the most efficient way of improving labor market mobility in pursuit of an economic development strategy would be to draft everyone into the military. As discussed in this subsection, the effects of military conscription are highly context-specific and potentially quite heterogeneous, and it is reasonable to imagine that there could be more direct ways of promoting similar outcomes of mobility and/or formal labor market integration without mandatory military service. A compulsory program of any sort is also quite heavy-handed: given that the long-

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[30]Of course, being reassigned within the military is not the same as moving voluntarily.

term benefits may only accrue to certain segments of the population (e.g. workers with low levels of education), a voluntary program might be more sensible.

Finally, even if it is the case that conscription could help to facilitate the process of structural transformation and state-building in a developing country, a policymaker would need to weigh this potential benefit against legitimate concerns about increased violence and militarization among conscripts, including the potential risk to stable, democratic government that could also (arguably – see for example Acemoglu et al. 2019) be important for economic development. Indeed, recent evidence from Argentina supports the idea that former conscripts have a more militaristic worldview (Ertola Navajas et al. 2020), and the experience in Argentina itself with human rights abuses and economic mismanagement under military rule provide ample reason for caution.

3   Data Sources and Construction

This paper relies primarily on two sources of individual-level data: complete voter registration rolls from several recent elections, and monthly employer-employee matched earnings data from Argentina's Social Security system, the *Sistema Integrado Previsional Argentino* (SIPA).

3.1   Voter Rolls (2011-2017)

This dataset contains the name; DNI; sex; birth year; street address; and the province, department, and electoral circuit of current residence for all registered voters.[31] I primarily use the rolls corresponding to the 2017 election cycle for the main analysis, but I have available complete rolls corresponding to 2011, 2013, 2015, as well as partial rolls from 2003, for supplementary analysis. These records, in principal, are meant to capture the full universe of Argentine citizens living in the country at the time of each election, as voting

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[31]Birth years are excluded for some female voters, including all female residents of the City of Buenos Aires, and a small percentage of those living elsewhere.

is compulsory for all Argentine citizens and voter registration is automatic. Of course, in practice, the voter rolls may not be perfectly accurate. People may not update their address immediately upon moving, which could cause me to understate rates of internal migration. Individuals' sex, birth year, or even DNI might be recorded incorrectly, which could result in incorrect measurement of conscription status and could make it more difficult to match to employment records (discussed below). There may be some native citizens who are simply missing from the voter rolls, though this should be a very small fraction of the population. Even accurate records may not perfectly reflect the universe of people who were subject to conscription: citizens who have died or migrated out of the country *should* be missing from the rolls,[32] and some people who *are* on the rolls may be naturalized citizens who were never eligible for conscription.[33] Despite these concerns, this data source should suffice to capture a nearly complete universe of potential ex-conscripts and potential prime-age workers. This in turn allows me to observe whether an individual ever appears in the formal labor force during the period for which I have data, and to consider this as an outcome variable of interest.

In addition, I use the voter roll data to impute the following variables, which will be crucial for my analysis:

Province of origin, i.e. the province in which an individual resided at the time that their DNI was issued.[34] There is no (documented) function explicitly linking DNIs to the province in which they were originally issued, nor is this information included in the voter roll data. However, two key facts make it possible to use the voter roll data to generate a reasonable guess as to where many individuals were living when their DNIs were first issued. The

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[32]This could introduce survivorship bias.

[33]It is my understanding that a certain range of DNIs is reserved for non-native citizens, so I exclude from analysis DNI ranges that are sparsely populated and numbers higher than 50,000,000.

[34]Note that this is not necessarily the province of birth, but for most cohorts it represents the province of residence at the time when they received their conscription assignment, as DNIs were assigned shortly before this. However, due to a reform to the system for DNI assignments, cohorts born in and after 1968 were assigned a DNI immediately at birth. For these cohorts, the imputed province of origin will represent the province of birth, which could potentially mean that conscription variables are assigned incorrectly in the analysis due to the variation in cutoffs across provinces for cohorts born in and after 1965.

first fact is that DNIs were not assigned at random. Rather, in order to ensure that each number would uniquely identify an individual, the documents were produced in a centralized source in the capital and distributed from there to offices in each province. In most cases, the documents were printed in numerical order, and sent to provincial offices in packets containing a range of consecutive numbers.[35] Thus, in most cases, two individuals with numerically close DNIs were likely living in the same province at the time when they first received their documents, unless the numbers fall on opposite sides of a cutoff point between two ranges assigned to different provinces. The second key fact is the observation that motivated this paper: that a large majority of people live in their province of birth. This, together with the details of the process through which DNIs were assigned, suggests that in looking at any range of DNIs together with the current province of residence, we should expect to see a particular province disproportionately represented up to some cutoff, after which point a different province should be disproportionately represented. By identifying clusters of provinces of current residence in adjacent or near-adjacent DNIs, I derive a guess as to the province of origin for all individuals in a certain numerical range, and thus also classify individuals currently living in a different province as migrants. A description of the details of this procedure can be found in Appendix A.2.

Recently lived or currently living outside province of origin. This is the primary migration outcome that I use for analysis, essentially an indicator variable for migration out of the province of origin. It excludes individuals for whom the province of origin could not be imputed, and takes a value of 1 for those currently living (according to the 2017 voter roll) in a different province than the province of origin. Of course, some people may have moved out of their home province for some time and later moved back. In an effort to catch at least some of these cases and classify them as migrants,[36] I turn to an identifier variable included in the 2017 voter roll. This variable begins with a single letter that corresponds

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[35]I was able to confirm the details of this process by speaking to Dr. Diego Sombra at the National Registry of Persons (RENAPER), the agency that issues identification documents.

[36]My ideal outcome measure would be an indicator for "ever migrated", but this is infeasible as it would require me to be able to observe each individual's lifetime history of residences.

to a province's postal code, followed by a long series of numbers. I interpret this province postal code as a marker of a recent residence, corresponding to the location approximately 10 years prior.[37] This then allows me to classify people who have "recently" lived outside of their province of origin as migrants, even if their current residence is in the province of origin. The variable takes a value of 0 if both the "recent" and current residences are the same as the province of origin, or if the "recent" residence is missing but the current residence is the same as the province of origin.

3.2 SIPA Earnings Data (1995-2016)

This is dataset provides employer-employee matched monthly earnings for all formally employed workers in Argentina from 1995 to 2016. It also includes some basic demographic information about individuals such as sex and year of birth, along with details about the employer such as firm size, industry, and the age of the firm. Certain variables are available for a subset of years, including the type of employment contract, whether the employee is in a management position, and whether the firm is public sector. Notably, this excludes people working in the informal sector, as well as self-employed people. It also excludes people working "*in gray*," essentially employees hired as contract workers in order to skirt various labor regulations. The dataset also does not include individuals' occupations, education levels, or hours worked in each month, it also does not distinguish between regular pay and bonus pay.

From this dataset, I generate the following outcomes:

Participation in the formal labor force. Formal work is generally more stable and higher paying than informal or contract work, so the simple fact of whether (and how often) someone appears in the formal workforce is a useful economic indicator. Using the voter roll data as

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[37]I have not been able to verify my interpretation of this variable, however the evidence that it represents a recent residence is fairly strong: it is highly correlated with the current residence variable, and I have asked Argentine colleagues to provide some examples of people who had migrated to Buenos Aires from a different province, and recent migrants seemed to be coded with their former province, while people who had migrated many years prior had a code corresponding to their current residence. My best guess is that the province code corresponds to the place of residence at the time that these records were digitized in their current format. There are a few possible methods through which I might be able to test my interpretation of this variable more rigorously, but I have not been able to do this at the time of this writing.

the universe of potential workers, for each year of the SIPA data I select the men who would have been prime age workers (25–54 years old) at the end of that year, I then use the DNI to match these to the SIPA data.[38] Those who never appear in the formal employment data while prime age receive a value of 0, otherwise they are assigned a value of 1. Workers who are never prime age between 1995 and 2016 (i.e. those born in 1940 and earlier or 1992 and later) are excluded from all analysis of labor market outcomes.[39]

Average lifetime earnings. My primary measure of earnings is based on the average amount earned per prime-age working month (i.e. excluding months with little to no earnings reported). Calculating this measure presents a few challenges, including: identifying whether a person should be considered fully working in a given month; identifying whether pay that is substantially higher or lower than other months represents a data entry error (for example, a missing or extra 0), or if it should be included in the average; and averaging out earnings over several years when there is no consensus standard for price levels in Argentine data for much of the 2000s and 2010s.[40] For information about the time series of price levels that I constructed, see the following sub-section (3.3). I use those price levels to convert nominal wages to real wages, using January 2005 as a base. In an effort to make sure the average I construct is reasonably representative of a worker's typical monthly earnings, I identify and exclude months from the average where the values seem excessively high or low. Specifically, I deem income for a given month to be excessively high if it is the largest amount the worker earned from the given firm in the given year, and if the natural log of the ratio of that month's earnings divided by the worker's *second*-highest monthly earnings from the same firm that year is greater than 1.6 (roughly 5× higher).[41] I similarly deem all earnings below the 0.5th

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[38]For details on the process of matching across these datasets, including statistics on the successful match rate, see Appendix A.1.

[39]The last cohort to have any members conscripted into the military was born in 1975, so anyone born after this is in any case excluded from analysis due to collinearity of the birth year fixed effect and conscription status.

[40]The SIPA dataset unfortunately does not include hours worked, but in general I would not want to penalize the average earnings calculation by including months in which the worker put in substantially fewer hours than usual. I also would generally not want to include, for example, large retirement bonuses in the average (these are often seem to be orders of magnitude larger than the typical month's work).

[41]I make one exception to this, which is if the maximum earning amount that would otherwise be excluded

percentile of (prime-age male) worker earnings for the month in question to be excessively low to include in the worker's average. Note that this measure excludes months in which a worker does not appear in the formal sector, so average earnings of workers who are unemployed for long stretches or earn much of their income seasonally would be overstated. On the other hand, if a worker is employed part-time in the formal sector and supplements his income with informal work, I would underestimate his true monthly earnings. And of course, if a worker never appears in the formal labor force during the period covered by the SIPA data, or only appears outside of his "prime" working years, he will be excluded from analysis on this outcome entirely. For regressions on this outcome, I use the natural-log of this measure, so that the regression coefficients can be interpreted as a percent increase/decrease in earnings.

3.3   Additional Data

The following data are also necessary for my analysis:

Conscription assignments. These provide the correspondence between the last three digits of the DNI and the lottery sort order for each cohort from 1927 to 1976, as well as the cutoff points for exemption and for each branch. A small number of cases will be excluded from analysis because of internal inconsistencies in reported cutoffs, e.g. the highest number assigned to the Army exceeds the lowest number sorted to the Air Force. In cohorts born after 1965, some individuals from provinces that are divided into multiple military districts fall into a range of draft lottery numbers where their assigned branch would depend on the district; these ambiguous cases are also excluded from most analyses.[42]

Military base locations. Taken from the *Libro Blanco de la Defensa* (Ministerio de Defensa, Argentina 2015), I compiled a list of the provinces that currently have Army, Air Force, and Navy bases.[43] Any changes in the locations of bases either during the period

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is roughly at or below the 4th percentile of worker-firm earnings for the given month: in this case, I assume that the other months probably do not represent a full working month, and exclude them.

[42]For specifications that do not look at specific branch effects, some of these individuals might be included if their draft number is high enough that they were surely not exempt, even if it's unclear exactly which branch they would have served in.

[43]In some cases it was unclear whether a location on the map was actually a base where large numbers

when conscription was in place or afterwards are thus not reflected in the analysis that follows. This could well mean that the presence or absence of a base in a given province is recorded incorrectly, especially for earlier cohorts. It's also true that provinces are, in general, very large geographically, and in many cases very sparsely populated, such that having a base in your province would *not* necessarily imply that you completed your service close to home.

Price levels. There is broad acknowledgment that starting in 2007, the administration of President Nestor Kirchener exerted political pressure on the national statistics agency, INDEC, to manipulate its price level calculations in order to make inflation seem lower than it truly was (see Cavallo 2013; Cavallo, Cruces, and Perez-Truglia 2016; Cavallo and Rigobon 2016). This practice continued throughout the administration of his successor (and wife), Cristina Fernández de Kirchner, such that there are now no official national[44] price level statistics available between 2007 and mid-2016. Because inflation was very high over this period, to use nominal earnings to construct lifetime earnings measures would severely exaggerate earnings levels of younger people and those who appeared more often in the formal labor force in later years. As such, I combined price level data from several different sources (including Secretaría de Modernización 2018, which includes price level statistics compiled by provincial governments and the City of Buenos Aires for much of the period of data manipulation, as well as the replication data provided by Cavallo et al. 2016). Wherever possible (and reliable), I used INDEC-produced statistics for Greater Buenos Aires (as these are the most consistently available over time). For the period when these are unavailable and/or unreliable, I use data produced by a company called (confusingly) Buenos Aires City (and downloaded from the replication data for Cavallo et al. 2016), which was run by the former head of INDEC who was removed in favor of an appointee more amenable to data manipulation. Where this source is also unavailable, I use the index produced by the

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of people would be posted; in these ambiguous cases, I assumed they were in fact bases, which might mean that I classified some "leavers" incorrectly as "stayers".

[44]In practice, "national" price level statistics often in reality are based on Greater Buenos Aires price levels.

government of the City of Buenos Aires.[45]

4 Empirical Strategy

4.1 Effects of Conscription

To begin the analysis, I estimate the effects of the conscription on various outcomes of interest, using the basic specification:

$$Y_{icp} = \alpha + \beta\,Draft_{icp} + \delta_c \times \theta_p + \varepsilon_{icp}, \tag{1}$$

where Y_{icp} denotes the outcome of interest measured for individual i in birth-year cohort c from place-of-origin p, $Draft_{icp}$ is an indicator (or set of indicators) for conscription assignment status, δ_c is a full set of cohort fixed effects, and θ_p is a full set of province-of-origin[46] fixed effects. Note that conscription status can refer simply to whether an individual was assigned to any military service or not, or to a set of indicators for the specific branch of the military to which he was assigned. Because all of these are determined by random assignment, this specification yields a consistent estimate for the causal impact of one's conscription assignment on the given outcome. However, because I do not observe whether or not any individual actually completed his assigned service, all analyses will produce Intent-to-Treat estimates; my estimates may understate the true treatment effects. Birth-year cohort fixed effects are necessary for any specification as the underlying randomization is conducted at the cohort level: without these fixed effects, because earlier cohorts tended to have higher percentages assigned to military service, the coefficient on conscription would in part reflect age effects. Though the lottery assigning draft numbers was always conducted nationally at

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[45]Specific price levels used for each month, along with the source for each, are available from the author upon request.

[46]In principle, it could be helpful to define the place of origin at a sub-province level in order to, for example, examine heterogeneous effects on people from rural versus urban areas, or to derive a more precise measure of the distance to the nearest military base. It might be possible to do this as an extension to the imputation procedures described in Appendix Section A.2.

the cohort level, for some cohorts – mostly those born in 1965 or later – cutoffs for assignment to each military branch exhibited relatively high variability across different provinces, and in some cases even within a single province.[47] This makes it necessary, for the affected cohorts, to interact the cohort fixed effects with the military "zone," i.e. the geographic level at which the cutoffs varied. Again, failing to do so for these cohorts would result in a bias in the estimated effects of conscription toward the average outcomes of people from provinces with more conscripts. In order to maintain a consistent set of controls all cohorts, I interact *all* cohort fixed effects with province-of-origin fixed effects, even when cutoffs for a given cohort are uniform across the country.

4.2   Displacement Effect: Simple Example

While the above specification is econometrically well-identified, it does not allow me to distinguish the *displacement* effect from any other possible effects of conscription. In order to demonstrate how I attempt to isolate the displacement effect from other effects of conscription, it will be useful to consider a slightly simplified example. To begin, consider the ideal experiment we would want to run. Suppose that we had a system in which conscription status was randomly assigned, and conscripts were also randomly assigned to complete their service close to or far away from home. Then we could run the very simple regression:

$$Y_i = \tilde{\alpha} + \tilde{\beta} \, Draft_i + \tilde{\gamma} \, Draft_i \times Far_i + \tilde{\varepsilon}_i$$

and the coefficient $\tilde{\gamma}$ would give an estimate of the effect of displacement (being sent far from home), net of the more general effect of conscription, on any outcome of interest. Of course, this idealized set up is not exactly what took place in reality. Even if there may have been an element of randomness in the assignment of locations for conscripts in Argentina, I do not observe the actual locations in which individuals completed their service. There is,

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[47]In cases where province-cohorts are divided across different military "zones" (either the 5 Army Corps or the 29 military districts), I exclude from analysis anyone whose draft number makes his conscription status ambiguous.

however, a fairly close analogue that I *do* observe. Consider the case of an individual living in hypothetical Province A, which contains an Army base but no Air Force base. [48] If we restrict our attention to just this province, and exclude (for now) people who were assigned to the Navy, we can run the following regression:

$$Y_{ic} = \tilde{\alpha}^A + \tilde{\beta}^A AnyDraft_{ic} + \tilde{\gamma}^A AirForce_{ic} + \tilde{\delta}_c^A + \tilde{\varepsilon}_{ic}^A, \tag{2}$$

where *AnyDraft* is an indicator for assignment to any military branch – in this case, either the Army or the Air Force, and $\tilde{\delta}_c^A$ is a complete set of cohort fixed effects. Because the branch assignments were designated through a random lottery drawing (within each cohort c), this specification allows us to estimate a fully-identified causal impact on outcome Y for people from Province A of being assigned to the Air Force as compared the Army, represented by the coefficient $\tilde{\gamma}^A$. Importantly, for reasons of cost and ease of coordination, conscripts were generally stationed as close as possible to their home. Thus, for conscripts from Province A, the assignment to serve in the Army or the Air Force determined whether they would have to complete their service far from home, or have a reasonably high chance of staying nearby.[49] If the experience of serving in the Air Force was identical to that of serving in the Army, then the only difference between these assignments would be attributable to the temporary displacement. In this case, we would have a close approximation of our idealized experiment. Of course, one might reasonably be concerned that the experience of being assigned to the Air Force would *not* have been identical to that of the Army. Fortunately, I do not need to make such a strong assumption, as I can run the same regression (2) from above for individuals from hypothetical Province B, which has both an Air Force and an

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[48]In all of the discussion that follows in this subsection, "Navy" could substitute for "Air Force" without any substantive change in the interpretation.

[49]It is worth emphasizing that it need not be the case that everyone who served in the Army was assigned to the base closest to their home. Even if many people are not sent to the closest Army base, if there is no Air Force base in the province, we can be sure that serving in the Air Force required relocating, compared with some unknown but likely fairly high probability of staying local when serving in the Army. This may bias estimates of displacement effects toward zero, meaning that they would represent a lower bound (in absolute value) for the true effects.

Army base. This will produce an estimate of the differential effect of being assigned to the Air Force versus the Army when there is no displacement. These two regressions can be pooled into a single Difference-in-Differences specification as follows:

$$y_{it} = \tilde{\alpha}^B + \tilde{\alpha}^D ProvA_p + \tilde{\beta}^B Draft_{ipc} + \tilde{\beta}^D Draft_{ipc} \times ProvA_p$$
$$+ \tilde{\gamma}^B AirForce_{ipc} + \tilde{\gamma}^D AirForce_{ipc} \times ProvA_p + \tilde{\delta}^A_c + \tilde{\delta}_c \times ProvA_p + \tilde{\varepsilon}_{ipc}$$

where $\tilde{\gamma}^D$ estimates the difference between the $Air\,Force - Army$ effect for Province A (which lacks an Air Force base) as compared to Province B (which has one). If the only difference for people in Province A versus Province B of the experience of serving in the Air Force (as compared to the Army) is that people from Province A have to leave the province while people from Province B do not, then $\tilde{\gamma}^D$ can be characterized as an estimate of the effect of temporary displacement.

4.3   Displacement: Main Specification

My preferred empirical specification differs only slightly from the simplified version described above; the primary difference is that I consider both Air Force and Navy service together. While nearly every province has an Army base, only some have Air Force or Navy bases. Because Air Force and Navy bases appear in different but partially overlapping sets of provinces, a few additional interaction terms are needed in order to properly calculate the displacement effect. To be precise, I estimate the following regression equation:[50]

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[50]Because the only province that has a Navy base but no Air Force base is the very small and somewhat idiosyncratic province of Tierra del Fuego, it is actually excluded from specifications that separate the $AirForce \times NoBase$ and $Navy \times NoBase$ effects, so all interaction terms with $NoBaseAirOnly$ are dropped.

$$Y_{iczp} = \beta_1 AnyDraft_{iczp} + \beta_2^A AirForce_{iczp} + \beta_2^N Navy_{iczp}$$

$$+ \beta_3^A AnyDraft_{iczp} \times NoBaseAirOnly_p + \beta_3^N AnyDraft_{iczp} \times NoBaseNavyOnly_p$$

$$+ \beta_3^E AnyDraft_{iczp} \times NoBaseEither_p$$

$$+ \beta_4^A AirForce_{iczp} \times NoBaseNavyOnly_p + \beta_4^N Navy_{iczp} \times NoBaseAirOnly_p$$

$$+ \beta_5^A AirForce_{iczp} \times NoAirBase_p + \beta_5^A Navy_{iczp} \times NoNavyBase_p$$

$$+ \delta_c \times \theta_p + \varepsilon_{iczp},$$

or, somewhat more succinctly:

$$Y_{iczp} = \beta_1 AnyDraft_{iczp} + \beta_2 AirNav_{iczp}$$

$$+ \beta_3 AnyDraft_{iczp} \times NoBase_p^C + \beta_4 AirNav_{iczp} \times NoBase_p^C \qquad (3)$$

$$+ \beta_5 AirNav_{iczp} \times NoBase_p + \alpha_c \times \theta_p + \varepsilon_{iczp}$$

In equation 3, $AnyDraft_{iczp}$ represents and indicator for assignment to *any* of the three military branches; $AirNav_{iczp}$ represents two separate indicator variables, $AirForce_{iczp}$ and $Navy_{iczp}$;[51] δ_c represents a complete set of cohort fixed effects; and θ_p represents a complete set of province-of-origin fixed effects. The term $NoBase_p^C$ serves as an indicator that there is no base corresponding to a *different* (or "complementary") branch than the one with which the term is interacted. For example, for a person assigned to the Air Force while his native province has no Navy base, the term $AirNav_{iczp} \times NoBase_p^C$ would correspond to $AirForce_{iczp} \times NoNavyBase_p$, and would take a value of 1 regardless of whether or not the province has an Air Force base. The term $AnyDraft_{iczp} \times NoBase_p^C$ represents

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[51]It could alternatively represent a composite variable for Air Force *or* Navy if we wanted to lump these effects together.

two regression terms simultaneously: $AnyDraft_{iczp} \times NoAirBase_p$ and $AnyDraft_{iczp} \times$ $NoNavyBase_p$. While we are not necessarily interested in the coefficients on these $NoBase_p^C$ interaction terms themselves, they need to be included in order to correctly pool together the $AirForce_{iczp} \times NoAirBase_p$ and $Navy \times NoNavyBase_p$ Diff-in-Diff specifications into a single regression that properly accounts for heterogeneity in effects across provinces.

The main coefficient of interest is β_5, which gives an estimate of the effect of being assigned to a branch of the military with no base in one's native province, i.e. assignment to the Air Force from a province with no Air Force base, and/or assignment to the Navy from a province with no Navy base. The identifying assumption required for a causal interpretation of the β_5 coefficient is akin to the standard difference-in-differences assumption of parallel trends. Specifically, we need to assume that the effects of the experience of serving in the Navy or Air Force – that is, of all aspects of that experience *other than* the distance from home, that are distinct from the experience of serving in the Army – are orthogonal to one's proximity to the nearest Navy or Air Force Base. Note that this does *not* require that the locations of Navy or Air Force bases be "as-if" randomly assigned, nor that provinces with a Navy or Air Force base be similar on observable characteristics to those without one. It also does not require that the effects of conscription be uniform across different provinces. What it does require is that if there is heterogeneity in the effects of conscription between provinces with and without Air Force or Navy bases, that this heterogeneity can be captured by controlling for differential effects of serving in the Army. Put differently, the assumption is that if we were to build an Air Force (Navy) base in a province that previously did not have one, the difference in the effect of serving in the Air Force (Navy) versus the Army for people in that province would not systematically differ from the observed effect on people in provinces that had already had an Air Force (Navy) base.

It is not obvious that this identifying assumption is a reasonable one, but it is at least to some extent testable. One possible test would be perform separate regressions for each province and/or each cohort of various outcomes on $AnyDraft_{iczp}$, $AirForce_{iczp}$,

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and $Navy_{iczp}$, such that the coefficients on Air Force and Navy reflect the difference between serving in these branches versus the Army. If those differences are reasonably stable within a province over time, and especially if they are reasonably similar to estimates for other provinces that also have (or lack) an Air Force or Navy base, this would suggest that it is sensible to think that the Air Force/Navy branch-specific effects (separate from any displacement effects) are comparable across provinces regardless of the presence or absence of the corresponding base. Another potential test would be to interact various province-level statistics (e.g. province HDI or GDP per capita, out- or in-migration rates, average education levels, etc.) with the regressors from the main specification. If this does not change coefficient estimates substantially, and these additional terms do not have too much explanatory power, it suggests that we may not need to be overly concerned about confounding province characteristics.

Finally, perhaps the best possible way of ensuring that displacement effects are not being driven by province-level characteristics would be to identify cases where bases were closed down or new bases were opened. Such a scenario would allow us to observe how the Air Force/Navy vs. Army effect changes within a province as people from that province assigned to the Air Force/Navy go from being "leavers" in one cohort to "stayers" in the next (or vice versa). Essentially, this would be a Triple-Difference analysis, comparing the Air Force/Navy vs. Army effect (first difference) between provinces with and without an Air Force/Navy base (second difference) before versus after a base is built or decommissioned (third difference). Unfortunately, at the time of writing, I do not have the necessary information on base openings or closures to perform this analysis.

One final concern to note about my analysis is that there are several potential sources of attenuation bias, particularly when it comes to the effect of displacement, but also for the more straightforward conscription and military branch effects. First, because I do not observe compliance with conscription assignments, all estimates are of "Intent-to-Treat" effects rather than (generally preferable) LATE estimates. Second, there is reason to be concerned about

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incorrect imputation of treatment assignment variables: if there is any typo in the DNI or year of birth, conscription treatment variables become completely incorrect, as we will assign the wrong draft lottery number, and potentially also the wrong applicable cutoffs. Third, because of the way I impute province of origin (see Section 3.1), it is very likely that some percentage of these values is incorrect. This, in turn, might lead me to characterize an individual as being in a "No Base" province for his assigned branch when in fact his province does contain a base for that branch, or vice versa: I could be identifying some subset of "stayers" and "leavers" incorrectly. Errors in the imputed province could also bias estimates of simple conscription effects for cohorts in which the assignment cutoffs vary depending on the location by causing me to misstate the conscription assignment. All of these could attenuate my estimates of both conscription and displacement effects. A fourth set of potential sources of measurement error applies specifically to the measurement of the displacement effect, but not conscription branch effects: the metric I use for displacement is imprecise and prone to a number of potential errors. The locations of the bases themselves may have changed over time, and may have even been recorded incorrectly. I classify conscripts as "leavers" or "stayers" based on the absence or presence of a base corresponding to their branch assignment in their home province, but on top of the possibility that my province imputations are flawed, provinces themselves are quite large in terms of area, and some bases may be farther from population centers than others, so even if the base to which an individual was assigned was in his home province, he may have had to travel quite far to get there, and thus may have experienced a very similar displacement to his peers coming from outside of the province. Conversely, a person living quite close to the border of another province may be classified as a "leaver" while having in fact served quite close to home. Finally, we might be concerned that even people deployed quite close to home spent most of their time on and around the base rather than at home, so even though this would likely be an easier adjustment for people in such a scenario compared to others who were sent hundred of miles away, this experience, which I essentially use as a my experimental control group, may still have had some features

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of a temporary displacement.

5 Results

Table 1 shows some basic summary statistics for the men included in the data I have available. There are a few points worth highlighting from this table. First, note that the percentage of people assigned to serve in the military is generally decreasing over time. This highlights the importance of controlling for birth cohort fixed effects, otherwise effects of older age would be incorrectly attributed to being assigned to complete military service. Second, notice that the percentage of the cohort for whom the conscription assignment is ambiguous jumps for cohorts born in and after 1965: this is because these cohorts were assigned to different branches by based on the military district in which they resided. Some of the more populous provinces are broken up into multiple military districts, resulting in cases where because I only observe the province of origin and not the military district, I cannot ascertain the applicable branch assignment. For a similar reason, my estimate of the percentage of the cohort assigned to military service (Column 4) begins to diverge dramatically from the estimate in the data from Galiani et al. (2011) (Column 7): because they do not observe the place of origin for these cohorts, they can only ascertain conscription assignments for those whose draft numbers were so high or so low that their assignment status would hold regardless of where they lived.[52] Finally, it is worth comparing the numbers of people in each cohort in my data (Column 1) to the number given by Galiani et al. (2011) (Column 6). For earlier cohorts, my data tend to undershoot the cohort counts, which is likely largely attributable to mortality, as my data come from 2017 voter rolls rather than contemporaneous counts. This could mean that I exclude some people from my analysis who appear in the earlier years of earnings data, due to my inability to match those people to the recent voter rolls. For younger cohorts, the counts seem to align reasonably well, with my counts actually exceeding

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[52]This could also bias estimates of conscription effects substantially for these cohorts if I did not include province (or military district) fixed effects.

those in the Galiani et al. (2011) data in some cases. This could perhaps mean that I am incorrectly including some people who were naturalized relatively recently and were thus not subject to the conscription lottery.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cohort	# Men (1000s)	Age Served	Age rec'd DNI	% Assigned to Serve	% Assgn. Unclear	#Men in GRS data	% Assigned to Serve	% Actually Served
1941	87	21	18	100	2.1	161	100	50.9
1942	93	"	"	96.0	2.1	165	95.9	50.0
1943	101	"	"	88.9	4.8	170	88.8	50.4
1944	110	"	"	77.1	5.4	175	77.1	48.0
1945	119	"	"	73.7	0	180	73.8	44.6
1946	123	"	"	79.1	0	185	79.1	43.5
1947	131	"	"	72.1	0	190	72.1	40.6
1948	141	"	"	70.7	0	196	70.6	39.1
1949	148	"	"	78.6	0	201	78.6	39.8
1950	158	"	"	76.0	0	202	78.6	42.9
1951	165	"	"	87.1	0	204	87.1	36.0
1952	169	"	"	87.5	0	205	87.6	41.1
1953	175	"	"	85.6	0	206	85.6	43.7
1954	180	"	"	93.2	0	208	93.1	46.4
1955	184	"	"	98.2	0.2	209	97.6	46.1
1956	193	N/A	"	0	0	211	0	–
1957	197	N/A	"	0	0	212	0	–
1958	197	19	"	82.8	0	217	82.6	45.0
1959	201	"	"	68.1	0	219	68.1	46.3
1960	205	"	"	66.1	0	220	66.0	45.6
1961	210	"	"	65.0	0	221	65.1	45.9
1962	218	"	"	68.1	0	225	68.1	46.1
1963	218	"	"	65.1	0	228	65.1	43.5
1964	221	"	"	60.0	0	231	60.1	39.2
1965	223	"	"	59.8	1.5	234	57.1	38.2
1966	221	"	"	31.3	15.2	238	10.5	20.4
1967	226	"	"	23.9	17.7	241	2.5	15.6
1968	227	"	0	34.8	15.3	245	11.7	16.7
1969	239	"	"	39.4	17.2	248	20.1	17.0
1970	250	"	"	44.7	17.0	252	24.8	18.6
1971	264	"	"	19.3	26.3	255	6.1	11.6
1972	265	"	"	9.3	13.2	259	0.9	6.0
1973	269	"	"	16.6	25.3	265	4.1	7.6
1974	281	"	"	18.0	19.7	271	8.1	7.4
1975	297	"	"	19.4	20.4	277	0.1	8.3
1976	303	N/A	"	0	0	283	0	–

This table shows statistics for cohorts born between 1941 and 1976. Earlier cohorts are excluded as workers from these cohorts would not be considered "prime-age" by 1995, the earliest year for which I have earnings data. Column (1) lists the total number of men (in 1000s) in the cohort who appear in the 2017 voter rolls. Compare this to the number from the Galiani et al. (2011) replication data given in Column (6). Column (2) indicates the age at which the cohort served in the military, Column (3) gives the age at which the cohort received their DNI paperwork (0 indicates "at birth"). Column (4) gives the percentage of men from Column (1) who were asigned to serve in the military, and (5) gives the percentage for whom the military branch assignment was ambiguous. Column (7) gives the percentage assigned to service in the military according to the Galiani et al. (2011) replication data. Column (8) gives the percentage that actually served in the military, also from the Galiani et al. (2011) replication data.

5.1 Conscription Effects

To begin the main analysis, I investigate the overall effects of conscription and of being drafted into each of the three branches of the military: the Army, Air Force, and Navy. While this has not been the main focus of this paper, it provides a set of well-identified results to serve as a benchmark for the rest of the analysis to follow. Table 2 shows the overall effects of being conscripted into the military, as well as the separate effects of being drafted into each branch, on long-term migration, formal labor force participation, and earnings.[53] In all cases, these effects represent the difference compared to men who were not assigned to serve in the military, after controlling for province-cohort fixed effects.[54]

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[53]See Section 3 for definitions of each of these variables.

[54]Note that I use heteroskedasticity-robust standard errors but I do not cluster standard errors at the province-cohort level. This is because *within* each province-cohort, the actual randomization is done on the individual level. More precisely, it is done based on the last 3 digits of the DNI, but there is no reason to think that people sharing the same DNI last 3 digits would be more likely to share any other pre-treatment traits compared to any other randomly selected individual of the same province-cohort, and thus no compelling reason to cluster standard errors at this level.

Table 2: Effects of Conscription

	Live outside province of origin		Appear in formal labor force		ln(Avg. earnings)	
	(1)	(2)	(3)	(4)	(5)	(6)
Draft (any)	.056		.128***		.0015*	
	(.041)		(.044)		(.0009)	
Army		.014		.092*		.0014
		(.044)		(.048)		(.0010)
Air Force		.081		.120		.0012
		(.083)		(.093)		(.0019)
Navy		.287***		.202***		.0032**
		(.068)		(.076)		(.0016)
Prov×BirthYr FEs	Y	Y	Y	Y	Y	Y
Observations	5,387,016	5,383,226	5,387,016	5,383,226	3,721,971	3,674,018
Mean(Y)	21.82	21.87	69.49	68.65	7.06	7.06

Robust standard errors in parentheses. $^*p < .1$, $^{**}p < .05$, $^{***}p < .01$. Province-cohorts uniformly exempted or assigned to the same branch are excluded. Variation in the number of observations from Columns (1) to (2) (and similarly is, in (3) vs. (4) and (5) vs. (6)) is due to exclusion in Column (1) of province-cohorts uniformly drafted that are included in Column (2) because they were drafted into different branches, and due to inclusion of people in (1) who were known to have been assigned to military service, but for whom the assigned branch is ambiguous such that they are excluded from (2).

The first thing to note about these results is that the effect sizes are generally quite small, even those that are highly statistically significant. For example, the estimated effect of being assigned to the Navy on living outside one's province of origin carries a p-value < .001, while the magnitude of about 0.3 percentage points indicates a likelihood of migration about 1.3 percent higher than the population mean of 21.9 percent. It bears repeating that there are at least two main reasons why the effect sizes I find here are likely to be underestimates: (1) all estimates are Intent-to-Treat, and thus will be biased towards zero to the extent that people were able to avoid completing military service (or joined the service voluntarily despite not being assigned to it); (2) there are various potential sources of measurement error in the assignment variables, the province-of-origin variables, the outcome measures, and even potentially the ID and birth year variables (as discussed in Section 3), which may attenuate my estimates.

Conscription overall does not have a significant effect on migration, but does significantly increase the likelihood of appearing in the formal labor force by 0.13 percentage points (about 0.2 percent higher than the population mean), and causes a not-quite-significant increase in average prime-age earnings by about 0.15 percent (.0015 log-points). Together, this suggests that it is unlikely that conscription is solely a temporary displacement, even if many felt the experience to be a waste of time. That is to say, there would appear to be some effects of conscription that are driven by some mechanism outside of migration alone: perhaps some benefits of training, social connections, or even basic education or health services offered to conscripts – though I cannot distinguish between these mechanisms. This stands somewhat in contrast to the finding by Galiani, Rossi, and Schargrodsky (2011) that conscription has negative labor market consequences, though they similarly find very small and often not significant effects, and the differences could be attributable to the different timing at which the outcomes were measured, or to the somewhat indirect way they constructed variables of formal labor force participation, unemployment, and earnings based on occupational categories. That the experience of conscription in Argentina had effects beyond simple displacement is consistent with the findings in that paper that conscription is associated with an increase in crime rates, and also with more recent work (Ertola Navajas et al. 2020; Gibbons and Rossi 2020) suggesting that it affected long-term personality traits, beliefs, and even rates of intimate partner abuse and violence.

When broken down to assigned branches, it appears that most of the effects are being driven by service in the Navy. Given that the Navy is the branch with the fewest number of bases, thus requiring relocation for the greatest fraction of draftees, this is consistent with (though not necessarily evidence for) the hypothesis that the migration effects, and at least some of the labor-market effects, might be driven by the initial displacement. However, it is worth reiterating that the Navy required a longer period of service of two years (instead of one), and so might have had a deeper effect due to having more time to impart particular skills, more time to forge social connections, and even perhaps due to the displacement itself

lasting longer. For consistency and comparability across outcomes, I have only included in the sample men who would have been of prime working age (defined as 25–54) during at least one year of the time period for which I have wage data, i.e. from 1995–2016. This means that cohorts born before 1941 are excluded from the analysis.[55] My findings on migration, the only outcome that I can measure for earlier cohorts, remain qualitatively quite similar when these cohorts are included.

5.2   Displacement Effects

Next, I turn to the primary question of interest for this paper, in which I attempt to isolate the long-term effect of being sent far from home. Table 3 shows the results from my preferred specifications on the same three outcomes considered in Section 5.1.

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[55]No one born after 1975 was eligible for conscription, so these cohorts are necessarily excluded from the analysis.

Table 3: Effects of temporary displacement

	Live outside province of origin		Appear in formal labor force		ln(Avg. earnings)	
	(1)	(2)	(3)	(4)	(5)	(6)
Draft (any)	.080	.082	.127*	.129*	.0010	.0010
	(.066)	(.066)	(.075)	(.075)	(.0016)	(.0016)
Draft×NoBaseNavyOnly	−.056	−.062	−.062	−.070	−.0006	−.0008
	(.100)	(.100)	(.114)	(.114)	(.0024)	(.0024)
Draft×NoBaseAirOnly	−2.57	–	.136	–	−.0676*	–
	(2.05)		(1.55)		(.0362)	
Draft×NoBaseEither	−.149	−.146	−.018	−.012	.0024	.0025
	(.114)	(.114)	(.117)	(.117)	(.0024)	(.0024)
Air Force	−.101	−.040	−.040	.039	−.0035	−.0018
	(.107)	(.119)	(.122)	(.138)	(.0026)	(.0031)
Navy	.011	−.024	.176*	.130	.0004	−.0005
	(.087)	(.092)	(.101)	(.107)	(.0022)	(.0023)
Air×NoBaseNavyOnly	.161	.105	.261	.188	.0076	.0061
	(.169)	(.176)	(.198)	(.207)	(.0042)	(.0044)
Navy×NoBaseAirOnly	−.298	–	2.82	–	.0011	–
	(3.32)		(2.98)		(.0649)	
Air/Navy×NoBase	**.485*****		**−.131**		**.0027**	
	(.116)		(.131)		(.0027)	
Air×NoBaseEither		**.307**		**−.374**		**−.0022**
		(.214)		(.231)		(.0048)
Navy×NoNavyBase		**.552*****		**−.043**		**.0044**
		(.130)		(.148)		(.0031)
Prov×BirthYr FEs	Y	Y	Y	Y	Y	Y
Observations	5,328,605	5,326,367	5,328,605	5,326,367	3,633,357	3,631,483
Mean(Y)	21.83	21.83	68.58	68.58	7.06	7.06

Robust standard errors in parentheses. $*p < .1$, $**p < .05$, $***p < .01$. Province-cohorts uniformly exempted or assigned to the same branch are excluded. Columns (2), (4), and (6) exclude the only province that has a Navy base but no Air Force base, which is the extremely small and rather idiosyncratic province of Tierra del Fuego. Results are nearly identical for the coefficients of interest when this province is included.

Consistent with my hypothesis, the conscription effects on migration appear to be concentrated among those who are assigned to a branch with no base in their home province. Specifically, none of the military branches have a significant effect on migration for people living in provinces that contain a base corresponding to that branch. In contrast, being assigned to serve in the Air Force or Navy when there is no corresponding base in the province of origin increases the long-term likelihood of living outside of that province by almost 0.5 percentage points, a 2.2 percent increase over the population average. This result is highly

statistically significant (p < .001). Looking at the Navy and Air Force effects separately (in Column 2), we see that the above effect is largely being driven by the enrollment in the Navy. Being assigned to the Air Force when there is no Air Force base in one's home province does not appear to be significantly different from being assigned to the Air Force when there is a base in the province. However, I do find a positive estimate and in terms of magnitude, the effect is a bit more than half the size of the (highly significant) effect of being assigned to the Navy in a province with no Navy base, and is an order of magnitude larger than the (slightly *negative*) baseline Air Force effect for men from provinces that did have an Air Force base.

All of this suggests that being temporarily relocated via assignment to a branch that necessitated completing one's military service outside of his home province is associated with a significantly higher likelihood of living outside that home province later in life. In other words, a temporary relocation as a young adult does seem to help lower the barriers to leaving one's native area later in life. This naturally lends itself to the question of whether the temporary displacement has discernible implications for one's long-term labor market outcomes and economic wellbeing. Columns (3)–(6) of Table 3 show the net effects of temporary displacement on formal labor force participation and log average formal monthly earnings. It is important to note that these are the *overall* effects of displacement, rather than the effects for those actually induced to eventually migrate, because a large fraction of temporarily displaced conscripts *do not* end up migrating out of their home province. As such, the fact that we do not see significant gains in formal labor force participation or earnings as a result of the displacement does not necessarily indicate that there are no unexploited returns to migration. Rather, because the fraction of people induced to migrate is quite small in absolute terms, it could well be that the returns to these workers in terms of employment and wages are being masked by the null, or even potentially negative, effects on people who returned to their native province after conscription and remained there (or avoided serving altogether).

It is also worth noting that we continue to see certain effects of conscription on labor

market outcomes in these results that appear to be unrelated to the presence or absence of a nearby base. For example, we see that the draft overall has a small positive effect on formal labor force participation, and that being sent to the Navy has an additional positive effect on top of this. We also saw in Table 2 that at least the Navy has a small positive earnings effect. Because we see little evidence of a migration effect for conscripts with a base nearby, it is unlikely that these facts could be explained by the displacement effect of living on a base, even if that base was close to home. Rather, this suggests that there may have been some level of labor-market value in the conscription experience, perhaps due to some aspect of the training, development of "non-cognitive" skills like discipline or grit, or valuable social connections.

Finally, it is worth noting that while never significant in the above results, for certain outcomes the effect of being assigned to the Air Force when there no Navy base nearby is comparable or even larger than the Air Force effect when there is no Air Force base nearby. This is of concern because it may indicate a violation of the identifying assumption discussed in the previous section: that the Air Force − Army effect in provinces with both an Air Force and Army base can serve as a reasonable control for the non-displacement aspects of the Air Force assignment in provinces that do not have an Air Force base. If there are appreciable differences in the value of being assigned to the Air Force that are potentially correlated with the presence of an Air Force base, then we might not be able to ascribe a causal interpretation to the Air Force × No Base coefficient, measuring specifically the displacement effect on a given outcome.

Of course, the ability to ascribe a causal interpretation to the estimates of the displacement effects depends on the validity of the underlying identifying assumptions. Because I cannot directly prove that these assumptions hold (and indeed, there is at least some potentially contradictory evidence), I present an alternate set of specifications in Tables 4 and 5, in which I focus on provinces that do have an Army base, but lack either an Air Force base, a Navy base, or both. To understand the advantage of this approach, consider a

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province that does have an Army base, but no Navy base. For men from this province, the random assignment to the Army versus the Navy is, in effect, an assignment to perform military service either (likely) fairly close to home, or relatively far away with (near) certainty, respectively.[56] Because this assignment is fully random, there is little question as to the validity of the causal interpretation of estimates from these specifications. The concern with this set of specifications, however, is that the difference between serving in the Navy (or Air Force) versus the Army may not simply be the distance traveled to reach the base. Different branches may offer different training, skills, and a different network of peers and officers, and importantly the standard service term for the Navy was two years as opposed to one year for the Air Force and Army. Further, several provinces are quite large geographically, so even reporting to the closest base in such a province might mean traveling quite far from home; conversely, for some provinces, the closest Air Force or Navy base might have not have been inside of the province, but might have still been just across provincial boundaries and thus quite close to home for at least of subset of residents.

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[56]It is certain in the sense that if men assigned to the Navy do indeed serve in the Navy, there is no way to do so in their home province. This does not account for people getting out of the service or switching to a different branch; it could also be that some of these people are assigned to a base that is still fairly close to home despite being in a different province.

Table 4: Migration response: Army (base nearby) vs. Air Force/Navy (no base nearby)

	Live outside province of origin			
	No Air Base (excl. Navy) (1)	No Navy Base (excl. Air Force) (2)	Neither Navy nor Air Base (3)	Missing Air and /or Navy Base (4)
Draft (any)	-.101	-.010	-.095	-.015
	(.095)	(.060)	(.095)	(.060)
Air Force (no base)	.298*		.302*	.249
	(.179)		(.179)	(.177)
Navy (no base)		.528***	.755***	.530***
		(.092)	(.148)	(.092)
Prov×BirthYr FEs	Y	Y	Y	Y
Observations	1,286,061	2,945,748	1,389,109	3,015,221
Mean(Y)	26.73	22.42	26.76	22.52

Robust standard errors in parentheses.$^*p < .1$, $^{**}p < .05$, $^{***}p < .01$
Province-cohorts uniformly exempted or assigned to the same branch are excluded.

With all this said, Table 4 shows results for long-term migration that are fairly consistent with the estimates presented in the preferred specifications, and consistent with the idea that being assigned to live outside of one's home province could make it substantially more likely that a person moves again to a new province later in life. It is worth noting that assignment to the Air Force does not seem to have quite as strong an effect as assignment to the Navy, which might suggest that living outside of one's home province for a longer time period causes a larger increase in the propensity to migrate, though I cannot rule out the possibility that the difference is due to other aspects of the experience of being assigned to either of these branches.[57] However, it is notable that the coefficients on both the Air Force and Navy are very similar to those estimated in Table 3, meaning that using the effects of serving in the Air Force/Navy (versus the Army) when there *is* a base nearby as a control does not change

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[57]It could also be that I have incorrectly identified the locations of Air Force bases, though it seems less likely that I have incorrectly recorded a province as missing a base, and more likely that in some provinces I incorrectly identified non-base Air Force properties that did not actually house troops as bases. Of course, given that I used records giving current base locations as my data source and that the prestige, budget share, and most importantly number of personnel dedicated to the military have fallen precipitously since the return to democracy in 1983, it is quite possible that several bases have been decommissioned that would in fact have held conscripts, and as such that men I've categorized as "displaced" were in fact able to serve quite close to home.

the estimated effects substantially.[58] This, in turn, suggests that it might be reasonable to attribute the migration effects of conscription largely to displacement, bolstering the claim of a causal relationship.

Table 5: Labor market effects: Army (base nearby) vs. Air Force/Navy (no base nearby)

	No Air Base (excl. Navy)	No Navy Base (excl. Air Force)	Neither Navy nor Air Base	Missing Air and /or Navy Base
	Appear in Formal Labor Force			
	(1)	(2)	(3)	(4)
Draft (any)	.094	.089	.082	.094
	(.092)	(.063)	(.091)	(.063)
Air Force (no base)	-.281		-.296	-.321*
	(.186)		(.186)	(.184)
Navy (no base)		.087	.343**	.091
		(.103)	(.151)	(.103)
Prov×BirthYr FEs	Y	Y	Y	Y
Observations	1,286,061	2,945,748	1,389,109	3,015,221
Mean(Y)	71.39	69.40	70.97	69.28
	ln(Average Earnings)			
	(1)	(2)	(3)	(4)
Draft (any)	.0037**	.0017	.0038**	.0017
	(.0018)	(.0013)	(.0018)	(.0013)
Air Force (no base)	-.0042		-.0043	-.0034
	(.0037)		(.0037)	(.0036)
Navy (no base)		.0039*	.0019	.0039*
		(.0021)	(.0030)	(.0021)
Prov×BirthYr FEs	Y	Y	Y	Y
Observations	913,262	2,031,954	980,636	2,076,254

Robust standard errors in parentheses.$^*p < .1$, $^{**}p < .05$, $^{***}p < .01$
Province-cohorts uniformly exempted or assigned to the same branch are excluded.

Table 5 shows analogous specifications for formal labor force participation and log-earnings. Unlike the corresponding migration results, no obvious pattern jumps out in these results. Depending on the specification, we see some positive earnings effects from assignment to the Army or Navy, and possible positive effects on formal labor force participation

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[58]With the exception of Column (3), which shows a larger effect for assignment to the Navy.

from assignment to the Navy, but these effects are not consistent across specifications, and do not seem to align in any clear way with displacement. Indeed, all the coefficients on the effect of assignment to the Air Force are *negative* (though generally not significant) for both formal labor force participation and earnings. While this does seem to imply that the effects of conscription were not *restricted* to effects from displacement – that is to say that the labor market effects of conscription seem to derive at least in part from some mechanism other than displacement – it does *not* mean that increased migration rates failed to translate to labor market gains. Because many workers were *not* induced to migrate by the initial displacement, all we can say is that the average net effect of the displacement was insufficient to improve overall earnings or employment, but this does not preclude the possibility that there were in fact positive effects on those who were induced to migrate, these might just be undetectable due to null (or even possibly negative) effects on those whose migration decisions were unaffected.

6   Conclusion

When we look at the high proportions of people who live relatively close to their place of birth, it is easy to imagine that this might be simply because people simply prefer the familiarity of their native place, where they are most likely to have family ties, a broader social network, and a large amount of useful local knowledge. However, this paper, while not contradicting this hypothesis, provides evidence that there is more to it than this. Specifically, I find evidence that temporarily displacing someone from their native area makes them about 2–3% more likely to move away again in the long run.[59] If it were the case that people had full information about the potential benefits of migrating elsewhere, and simply chose not to do so because these benefits were outweighed by the disutility of living outside of their native place, we would not expect those preferences – and the resulting decisions about migration – to be changed by spending a short period living outside of that place. Yet I find that random

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[59] The true effect is likely larger, for various reasons discussed throughout the paper.

conscription assignment in Argentina to a branch of the military that did not have a base nearby was associated with an increase in the long-term likelihood of living outside one's native province, and that this effect did not apply to people assigned to the same branch of the military but for whom there was a corresponding base nearby.

However, in contrast to the hypothesis that this conscription program constituted a pure waste of time, or that any long-term labor market benefits would be confined to those induced by displacement to migrate at higher rates, I find evidence of modest labor market benefits to conscripts that do not seem to be restricted to those who served far from home. Specifically, I find a 0.1 percentage-point increase in formal labor force participation rates that appears to apply to draftees across the board (though this effect is not significant when restricting attention to provinces with bases missing), a small additional increase of about 0.2 percentage points for being assigned to the Navy, and a small increase in earnings of about 0.3–0.5 percent from assignment to the Navy irrespective of place of origin.[60] None of these effects is restricted to conscripts who were sent far from home, suggesting that they are probably attributable to some other aspect of military service.[61] Moreover, I do not see any significant difference in the effects on these outcomes among the "displaced" conscripts when compared with their non-displaced counterparts.

This is not to say that there are no unexploited returns to migration in this setting. It's important to keep in mind that the increase in migration rates among displaced conscripts is fairly small, such that even a substantial earnings and/or employment benefit accrued to those induced to migrate may be disguised by the null (or even negative) effects on the people who returned home after military service and stayed there. Still, it seems clear that the labor market benefits of conscription in Argentina, though they may be small, are not solely attributable to displacement. Further research would be required to try to identify the

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[60]These effects are also likely somewhat understated because I do not observe whether individuals actually completed their assigned military service.

[61]It's unlikely that these effects are attributable to the minor displacement of living close to home but in barracks, as I do not find any migration effects among the conscripts that I classify as more likely to have served close to home.

specific mechanism for these effects, and to understand whether there are any conclusions that we might be able to generalize to other developing countries. We cannot say from this study whether these effects could be generated through a similar training program, perhaps unconnected to the military, and perhaps even voluntary: there is not enough information to know whether the "mandatory" and "military" aspects of mandatory military service were necessary to achieve the observed labor market effects, and whether this might be different in a developing country compared to a developed one. What we *can* say is that given the small magnitude of observed effects, it's quite probable that the costs of this program, in terms of lost civilian labor market experience, the fiscal cost of administering the program, and the less tangible costs in terms of crime, violence, and attitudes toward politics and democracy, could easily have outweighed the modest benefits. There are likely many more cost-effective and humane way of achieving comparable or better outcomes.

A number of migration-related questions remain open for further research. The most pressing question is whether there was in fact an economic benefit to those who were induced to migrate in the long-term by the initial temporary displacement: did these people earn more, were they better able to adjust via migration to local labor-market shocks, were they made more flexible in any other way in terms of being able to switch between jobs or industries more easily? Is there an alternative intervention that would be *more* effective in terms of encouraging future migration, or less authoritarian than compulsory relocation? All of these questions remain unanswered. Also unanswered is *who were* the people induced to migrate – did they tend to be higher or lower skilled workers, is it really the case that this program helped to incorporate people from poorer, remote areas into more productive sectors? Lastly, given the potentially high costs and the unclear benefits of inducing higher migration rates, it is reasonable to ask whether it would be better to implement place-based policies that target workers in areas facing a localized downturn, rather than trying to encourage these workers to migrate in search of opportunities elsewhere.

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Appendix

A Data Appendix

A.1 Merging records across data sources

While in principle the DNI is meant to represent a unique individual identifier, which would in turn allow a perfect correspondence between the SIPA data and the voter rolls, there are several complicating factors. I briefly discuss these complications, and how they affect my analyses, here.

In the SIPA data, workers are identified by a number known as a "CUIL" (*Código Único de Identificación Laboral,* or Unique Labor Identification Code) rather than the DNI. CUILs are constructed by adding two digits before the DNI (typically 20 for men and 27 for women, 23 and 24 are also used in some cases), along with one check digit at the end. A small number of the CUILs included in the SIPA data did not follow this pattern, and so could not be converted to a DNI. Of the 21,222,006 unique CUILs included in the SIPA records from 1995 to 2016, 3,347 (0.02%) were in an invalid format and thus had to be dropped.

In addition, I found that in the Voter Roll data, there were relatively few DNIs listed with values greater than 50,000,000 (to be precise, 9,797 records out of 33,094,829 (0.03%) fall in this range. Because DNIs were quite sparsely assigned in this range, mostly assigned to people born in the 1990s or early 2000s, and because non-citizens are generally assigned DNIs somewhere in this range (though I was not exactly sure where that cutoff is generally supposed to fall), I eliminated all CUILs from the SIPA data corresponding to DNIs greater than 50,000,000[62]. This brought the total number of individuals from the SIPA data to try to match to the Voter Rolls down to 19,794,288, or around 93.3% of the original set of potentially valid DNIs.

If DNIs were truly unique, there would presumably be a one-to-one DNI-to-CUIL cor-

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[62]I also eliminated DNIs of 0

respondence. However, DNIs assigned early on were intended to be unique *within sex* only. All DNIs that are 7 digits or fewer (below 10,000,000) are potentially duplicated across sex. In general, this corresponds to people born in 1951 or earlier – people born in 1952 or later were generally assigned an 8-digit DNI, which was intended to be unique regardless of sex. This means that in order to properly match such records, both the DNI and sex have to be recorded accurately. In addition, there are a small number of cases in which multiple people of the same sex were assigned the same 7-digit-or-lower DNI, and a small number of cases in which two people were assigned the same 8-digit DNI.

To match the remaining records from the SIPA data to the Voter Rolls, I divided both into two segments, one with 8-digit DNIs, and one with 7-or-fewer-digit DNIs. Starting with the 8-digit DNIs: we begin with 17,402,257 unique CUILs in this range. From this, I drop cases where two different CUILs correspond to the same DNI, and also cases with duplicated DNIs in this range in the Voter Roll data. This leaves us with 16,802,039 unique 8-digit DNIs in the SIPA data, about 96.6% of the valid DNIs in this range. Of these, 616,658 (3.67%) do not match up to any record in the Voter Rolls. This failure to match could be attributed to several potential explanations: (1) people may have died and been removed from the Voter Rolls, (2) the DNI might be recorded incorrectly in either data source, or (3) the person may be a non-citizen or otherwise ineligible to vote. For the sake of consistency across the analysis, I excluded all cases that I could not match to a Voter record.

Next, I consider DNIs below 10,000,000. These need to matched on both sex and DNI in order to be valid[63]. We begin with 2,392,031 observations in this range in the SIPA data. Of these, 52,501 (about 2.2%) are removed either because of the sex of the worker is not recorded in the SIPA data, or because of inconsistencies in the sex as recorded across different variables, workplaces, and years. Of these, 652,347 (27.9%) fail to match to a record of the corresponding sex in the voter rolls, whereas 1,687,183 records do match. This relatively

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[63]It might not be necessary in all cases to match on sex, as some ranges of DNIs were only assigned to men or women, not both. However, identifying such ranges would have been quite costly and unlikely to generate a large quantity of additional matches.

high rate of attrition is likely due to many of these people having died and been removed from the Voter Rolls.

All told, this leaves us with 17,872,564[64] records (or about 90.3% of the 19,794,288 valid DNIs between 0 and 50,000,000) from the SIPA data matched with a reasonable degree of confidence to the Voter Rolls. In order to avoid misclassifying records that appear in the Voter Rolls but are dropped from the SIPA data due to duplication or other data inconsistencies as never appearing in the formal sector (which is one of the main outcome variables I look at), I drop 439,986 individuals (around 1.3% of the 33,094,829 records in the valid DNI range) from the Voter Roll data.

A.2 Imputation of province of origin

The imputation procedure I develop for province of origin relies on two observations: (1) that many people do not move from their province of origin, and (2) that DNIs were issued through centralized distribution of pre-filled forms of consecutive numbers. Thus, if we sort all individuals from the Voter Rolls by their DNI, we would expect to see high levels of correlation between adjacent observations in the *current* province of residence. We would also expect to see reasonably well-defined breaks, in which we go from a large cluster of individuals mostly residing in one province, to another cluster of individuals mostly residing in another province. Further, if we see, for example, three people in a row living in Province A, followed by one person in Province B, then another cluster of people in Province A, we can reasonably infer that the one person we identified in Province B is a migrant originally hailing from Province A. Based on this intuition, I implement the following procedure[65] (I also include an illustrative example from the actual Voter Roll data that is reasonably representative of the full dataset):

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[64]A much smaller number are included in the final analysis, as for this paper I study only men, only those in cohorts in which some individuals were conscripted (and some not), generally only those who were of prime working age at some point between 1995 and 2016, and generally excluding those for whom I could not impute a province of origin.

[65]This description is slightly simplified for the sake of clarity. The full STATA do-file that implements the imputation procedure is available from the author upon request.

Figure A1: Starting Data (Example)

	province	DNI	birthyr
249	Santa Fe	XXXX2951	1956
250	Buenos Aires	XXXX2952	1956
251	Santa Cruz	XXXX2953	1958
252	Santa Fe	XXXX2954	1958
253	Santa Fe	XXXX2957	1956
254	Santa Fe	XXXX2962	1956
255	Córdoba	XXXX2963	1958
256	Santa Fe	XXXX2966	1952
257	Buenos Aires	XXXX2969	1956
258	Santa Fe	XXXX2972	1958
259	Santa Fe	XXXX2973	1958
260	Santa Fe	XXXX2976	1958
261	Tierra del Fuego	XXXX2980	1958
262	Santa Fe	XXXX2982	1956
263	Santa Fe	XXXX2984	1958
264	Santa Fe	XXXX2985	1959
265	Buenos Aires	XXXX2986	1958
266	Santa Fe	XXXX2988	1958
267	Buenos Aires	XXXX2991	1956
268	Santa Fe	XXXX2993	1956
269	Santa Fe	XXXX2995	1957
270	Córdoba	XXXX2997	1956
271	Santa Fe	XXXX2998	1958
272	Tucumán	XXXX3001	1956
273	Tucumán	XXXX3002	1958
274	Tucumán	XXXX3005	1958
275	Santiago del Estero	XXXX3006	1956
276	Tucumán	XXXX3010	1956

Step 1: Starting with the data sorted by DNI (see Figure A1), define "groups" of consecutive observations *currently* living in same place, born around the same time. Specifically, I consider an individual to be the start of a new "group" if he lives in a different province from the previous individual, or if his birth year is more than 2 years before or after the previous person's. The resulting groupings for the example data are shown in Figure A2.

Figure A2: Initial Grouping of Example Data

	province	DNI	birthyr	grp_tag	num_grp
249	Santa Fe	XXXX2951	1956	0	2
250	Buenos Aires	XXXX2952	1956	1	1
251	Santa Cruz	XXXX2953	1958	1	1
252	Santa Fe	XXXX2954	1958	1	3
253	Santa Fe	XXXX2957	1956	0	3
254	Santa Fe	XXXX2962	1956	0	3
255	Córdoba	XXXX2963	1958	1	1
256	Santa Fe	XXXX2966	1952	1	1
257	Buenos Aires	XXXX2969	1956	1	1
258	Santa Fe	XXXX2972	1958	1	3
259	Santa Fe	XXXX2973	1958	0	3
260	Santa Fe	XXXX2976	1958	0	3
261	Tierra del Fuego	XXXX2980	1958	1	1
262	Santa Fe	XXXX2982	1956	1	3
263	Santa Fe	XXXX2984	1958	0	3
264	Santa Fe	XXXX2985	1959	0	3
265	Buenos Aires	XXXX2986	1958	1	1
266	Santa Fe	XXXX2988	1958	1	1
267	Buenos Aires	XXXX2991	1956	1	1
268	Santa Fe	XXXX2993	1956	1	2
269	Santa Fe	XXXX2995	1957	0	2
270	Córdoba	XXXX2997	1956	1	1
271	Santa Fe	XXXX2998	1958	1	1
272	Tucumán	XXXX3001	1956	1	3
273	Tucumán	XXXX3002	1958	0	3
274	Tucumán	XXXX3005	1958	0	3
275	Santiago del Estero	XXXX3006	1956	1	1
276	Tucumán	XXXX3010	1956	1	1

Step 2: Exclude singleton groups.[66] Re-sort included observations by DNI; all observations will be adjacent to at least one other from the same province, many will now have several more adjacent observations from the same province.

Step 3: Re-group observations by the same criteria as Step 1. Resulting groups for the example data after this step are shown in Figure A3.

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[66]Here, I introduce a slight modification for the Province of Buenos Aires, which is the largest province in the country by a wide margin; includes the urban and suburban areas outside of Buenos Aires city proper, but also extends far beyond this; receives the largest number of internal migrants (though it does *not* have the highest fraction born outside the province); and has relatively low out-migration rates for people born there. Because it is not uncommon that two adjacent observations living in this very large province are both migrants, I exclude groups smaller than 3 current Buenos Aires residents as part of this same step.

Figure A3: Example Data: Re-grouped

	province	DNI	birthyr	grp_tag	num_grp	grp_tag_new	num_grp_new
180	Santa Fe	XXXX2951	1956	0	2	0	68
181	Santa Fe	XXXX2954	1958	1	3	0	68
182	Santa Fe	XXXX2957	1956	0	3	0	68
183	Santa Fe	XXXX2962	1956	0	3	0	68
184	Santa Fe	XXXX2972	1958	1	3	0	68
185	Santa Fe	XXXX2973	1958	0	3	0	68
186	Santa Fe	XXXX2976	1958	0	3	0	68
187	Santa Fe	XXXX2982	1956	1	3	0	68
188	Santa Fe	XXXX2984	1958	0	3	0	68
189	Santa Fe	XXXX2985	1959	0	3	0	68
190	Santa Fe	XXXX2993	1956	1	2	0	68
191	Santa Fe	XXXX2995	1957	0	2	0	68
192	Tucumán	XXXX3001	1956	1	3	1	3
193	Tucumán	XXXX3002	1958	0	3	0	3
194	Tucumán	XXXX3005	1958	0	3	0	3

Step 4: Analogous to excluding singleton groups in Step 2, now exclude groups of two. Then redo grouping as in Step 3. Results for the example data after this step are shown in Figure A4.

Figure A4: Example Data: Re-grouped (again)

	province	DNI	birthyr	grp_tag	num_grp	grp_tag_new	num_grp_new
180	Santa Fe	XXXX2951	1956	0	2	0	68
181	Santa Fe	XXXX2954	1958	1	3	0	68
182	Santa Fe	XXXX2957	1956	0	3	0	68
183	Santa Fe	XXXX2962	1956	0	3	0	68
184	Santa Fe	XXXX2972	1958	1	3	0	68
185	Santa Fe	XXXX2973	1958	0	3	0	68
186	Santa Fe	XXXX2976	1958	0	3	0	68
187	Santa Fe	XXXX2982	1956	1	3	0	68
188	Santa Fe	XXXX2984	1958	0	3	0	68
189	Santa Fe	XXXX2985	1959	0	3	0	68
190	Santa Fe	XXXX2993	1956	1	2	0	68
191	Santa Fe	XXXX2995	1957	0	2	0	68
192	Tucumán	XXXX3001	1956	1	3	1	6
193	Tucumán	XXXX3002	1958	0	3	0	6
194	Tucumán	XXXX3005	1958	0	3	0	6

Step 5: Keep only groups of 5 or more. Define the province, the min and max DNI, and the min and max birth year for all remaining groups.

Step 6: Reintroduce excluded observations and sort by DNI. Those falling within year and DNI range of a defined group are "absorbed" into the surrounding group. Absorbed observations from a different province are labeled migrants. Non-absorbed observations are

not assigned to any province of origin. The final groupings for the example data are shown in Figure A5.[67]

Figure A5: Final Example Data with Province of Origin Imputations

	province	DNI	birthyr	grp_tag_new	num_grp_new	grp_prov	excl_final	absorbed
249	Santa Fe	XXXX2951	1956	0	68	Santa Fe	0	0
250	Buenos Aires	XXXX2952	1956	.	.	Santa Fe	0	1
251	Santa Cruz	XXXX2953	1958	.	.	Santa Fe	0	1
252	Santa Fe	XXXX2954	1958	0	68	Santa Fe	0	0
253	Santa Fe	XXXX2957	1956	0	68	Santa Fe	0	0
254	Santa Fe	XXXX2962	1956	0	68	Santa Fe	0	0
255	Córdoba	XXXX2963	1958	.	.	Santa Fe	0	1
256	Santa Fe	XXXX2966	1952	.	.	.	1	.
257	Buenos Aires	XXXX2969	1956	.	.	Santa Fe	0	1
258	Santa Fe	XXXX2972	1958	0	68	Santa Fe	0	0
259	Santa Fe	XXXX2973	1958	0	68	Santa Fe	0	0
260	Santa Fe	XXXX2976	1958	0	68	Santa Fe	0	0
261	Tierra del Fuego	XXXX2980	1958	.	.	Santa Fe	0	1
262	Santa Fe	XXXX2982	1956	0	68	Santa Fe	0	0
263	Santa Fe	XXXX2984	1958	0	68	Santa Fe	0	0
264	Santa Fe	XXXX2985	1959	0	68	Santa Fe	0	0
265	Buenos Aires	XXXX2986	1958	.	.	Santa Fe	0	1
266	Santa Fe	XXXX2988	1958	.	.	Santa Fe	0	1
267	Buenos Aires	XXXX2991	1956	.	.	Santa Fe	0	1
268	Santa Fe	XXXX2993	1956	0	68	Santa Fe	0	0
269	Santa Fe	XXXX2995	1957	0	68	Santa Fe	0	0
270	Córdoba	XXXX2997	1956	.	.	.	1	.
271	Santa Fe	XXXX2998	1958	.	.	.	1	.
272	Tucumán	XXXX3001	1956	1	6	Tucumán	0	0
273	Tucumán	XXXX3002	1958	0	6	Tucumán	0	0
274	Tucumán	XXXX3005	1958	0	6	Tucumán	0	0
275	Santiago del Estero	XXXX3006	1956	.	.	Tucumán	0	1
276	Tucumán	XXXX3010	1956	.	.	Tucumán	0	1

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[67]The keen observer might note that there appear to be two excluded observations right after the defined end of the "Santa Fe" group that are likely candidates for inclusion in that group, and that the switch-over from the "Santa Fe" to "Tucumán" groups happens around a X999 − X001 boundary, which in the context of the larger dataset appears to be fairly common (specifically, I see many groups ending in 50 or 00 (and starting with 01 or 51), and to a lesser extent ending in 25 or 75 (and starting with 26 or 76). Clearly, it could be useful to take such observations into account in future research using these data or other similar data. It is also worth noting that a potential shortcoming with the procedure described here is that it might do a better job identifying province of origin for places from which few people leave, and might generate misleading imputations for bordering provinces with high levels of cross-migration, or for provinces that in general receive large numbers of migrants.