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ESSAYS IN APPLIED ECONOMICS

Alumna: MARÍA GABRIELA ERTOLA NAVAJAS

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DEPARTAMENTO DE ECONOMÍA

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Introduction

The research in this thesis falls in the overlapping fields of applied economics, public economics, and development economics. Each of the chapters was written in order to stand alone, with its own introduction, literature review, and conclusions. This approach allows a judgement of each chapter as a separate publication.

Methodologically, the common aspect of the chapters is the use of impact evaluation techniques to identify causal relationships.

In Chapter 1, we estimate the causal impact of military conscription on long-term beliefs and personality traits. Even though military conscription is one of the most prevalent policies observed worldwide, its consequences on shaping men's personalities and beliefs remain unknown. To address potential endogeneity concerns we exploit the conscription lottery in Argentina. We combine administrative data from the conscription lottery with data from a survey we designed on beliefs and personality traits. We find that men who were conscripted are more likely to adopt a military mindset and that the effect is long lasting.

In Chapter 2, we estimate the short-term casual impact of a formal training program on labor outcomes. The program includes different programming languages and the use of new technologies. In general, the main challenge for identifying the causal impact of training programs is that individuals self-select into them. In order to address potential endogeneity concerns, we exploit the random assignment to the program. We find that students who participated in the program are less likely to have a formal job. This is an unexpected result. We explore potential underlying reasons to this result, and we find no evidence of individuals switching from formal to informal labor market nor evidence of individuals switching to university education. This chapter highlights the potential role

that governments have in encouraging formal labor opportunities through educational policies.

Finally, in Chapter 3 we study Argentine economic performance in the twentieth century, with a focus on the long-term impact of Peronism on economic growth. Using a synthetic control approach, we find a large negative effect of Peronism on GDP per capita. Our results suggest that Argentine GDP per capita by the end of the century was less than a half of the GDP per capita the country would have had in the absence of Peronism. In addition, we find no impact of Peronism on life expectancy and child mortality. Peronism, however, may have influenced other dimensions of development not covered in this study. Therefore, more research is needed in order to draw solid conclusions about the long-term impact of Peronism on development.

Chapter 1*

The Long-Term Effect of Military Conscription on Personality and Beliefs

I. Introduction

We provide empirical evidence on the causal impact of military conscription on subsequent personality and beliefs. To identify the causal relationship, we need a variable that affects being conscripted but does not affect personality traits and beliefs through any other mechanisms. For almost all of the 20th century, the draft lottery in Argentina randomly assigned young males to military conscription based on the last three numbers of their national ID. For reasons totally unrelated to their personality or beliefs, some men were eligible for military conscription, whereas others were not. This characteristic makes the draft lottery in Argentina an ideal natural experiment to identify the causal relationship.

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We use administrative data on draft eligibility and survey data on beliefs and personality traits for a sample of 1,133 Argentine males born between 1958 and 1976. Males in these cohorts were potentially eligible to serve (at age 18) in the period 1976 to 1994; thus, our survey allows us to address the long-term effects of military conscription. Our main finding is that men who were conscripted are more likely to adopt a military mindset and that the effect is long lasting. In particular, we find that men who were conscripted are more likely to justify violence to solve conflicts, to believe that military service should be mandatory, to support coups against civilian governments, to accept military interventions in foreign countries, and to support the right to bear arms. In addition, compared with men who were not conscripted, men who were conscripted are less tolerant, more disciplined, more politically conservative, more authoritarian, and more belligerent.

Military conscription is one of the most prevalent policies observed worldwide and typically affects young men. Given the many people who go through military conscription during their formative years, our results are useful for understanding how personality traits and beliefs are formed for a very salient part of the population.

Our paper pieces together multiple bodies of literature. Various authors have studied the impact of military conscription on a wide set of outcomes, including criminal behavior (Galiani, Rossi, and Schargrodsky 2011; Siminski, Ville, and Paull 2016; Albaek et al. 2017; Lyk-Jensen 2018) and labor market outcomes (Paloyo 2010; Grenet, Hart, and Roberts 2011; Bauer et al. 2012; Card and Cardoso 2012). In particular, Galiani, Rossi, and Schargrodsky (2011) exploit the same natural experiment and find that military conscription increases the likelihood of developing a criminal record during adulthood. Our results could capture some personal features that in more extreme forms

and for a smaller proportion of individuals may also be expressed as involvement in criminal activities.

There is also a strand of literature —starting with Angrist (1990)— that has exploited the Vietnam-era draft lottery to identify the causal impact of combat exposure on many outcomes, for example, future earnings (Angrist, 1990; Angrist and Chen 2007), alcohol consumption (Goldberg et al., 1991), cigarette consumption (Eisenberg and Rowe 2009), health (Angrist, Chen, and Frandsen 2010; Dobkin and Shabini, 2009; Autor, Duggan, and Lyle 2011), mortality (Conley and Heerwig 2012), and criminal behavior (Bouffard 2003; Rohlfs 2010). Some studies have correlated combat exposure with increased political participation (Blattman 2009), greater volunteerism (Nesbit and Reigbold 2011), and higher voter turnout (Teigen 2006). Grossman, Manekin, and Miodownik (2015) use the assignment of health rankings that determine combat eligibility in the Israel Defense Forces to study the causal effect of combat exposure on support for peaceful conflict resolution. They report that combat exposure hardens attitudes toward the rival and reduces support for negotiation. Combat exposure or serving in the military during wartime may be, however, a very different intervention compared with peacetime conscription.

Our findings are related to the specialized literature on the characteristics of the military and its culture. This literature focuses on the connection between military service and pro-military values and commonly compares individuals who are in (or planning to pursue) a military career against individuals who are not. In an early contribution, Goertzel and Hengst (1971) compare Army Reserve Officers' Training Corps cadets with undergraduate students. They find that Army cadets do not differ considerably from undergraduate students in the context of background variables but score higher on personality scales measuring authoritarianism, misanthropy, intolerance, aggressive

nationalism, political-economic conservatism, and belief in imperialism. More recently, Jackson et al. (2012) show a positive correlation between personality traits and the decision to enter the military. People who score lower in agreeableness and who are less open to new experiences during high school are more likely to enter the military after graduation.

In two related papers, Stadelmann, Portmann, and Eichenberger (2015, 2018) study the link between serving in the military and the voting behavior of Swiss parliamentarians. They show that politicians who served in the military have a higher probability of accepting pro-military legislative proposals. Benmelech and Frydman (2015) analyze the relationship between military service of CEOs and managerial decisions, financial policies, and corporate outcomes. They find that firms run by military CEOs invest less, have lower expenditures on research and development, and pursue slightly lower leverage ratios than the nonmilitary peers. These findings suggest an association between military CEOs and more conservative investments and financial policies. An obvious drawback of these studies is that people self-select into military service. In this paper, we avoid selection problems by exploiting a well-documented random assignment. According to our review of the literature, our paper represents the first effort to identify the causal effect of military conscription on personality traits and beliefs.

Another strand of literature assesses the effects of military service on behavior and social awareness. In the context of the military in Norway, Dahl, Kotsadam, and Rooth (2018) study whether exposure of men to women in a traditionally male-dominated environment can change gender attitudes. They find that male soldiers adopt more egalitarian attitudes when randomly assigned to work with female soldiers. Carrell, Hoekstra, and West (2019) exploit data from the U.S. Air Force Academy where students

are randomly assigned to autonomous peer groups. They find that white Air Force cadets are more likely to choose Black roommates when their randomly assigned freshman roommate was also Black.

Finally, our paper also relates to a relatively new strand of literature that examines the impacts of events that occur during impressionable years. Giuliano and Spilimbergo (2014) find that macroeconomic conditions experienced during early adulthood affect life-long beliefs. Individuals who grow up during recessions tend to support more government redistribution, have less confidence in public institutions, and believe that success in life depends more on luck than on effort. This effect is higher when individuals are exposed to the shock between the ages of 18 and 25 years. Malmendier, Tate, and Yan (2011) find that CEOs' past experiences are related to how they make corporate financing decisions. They focus on CEOs who grew up in the Great Depression and served in the military, two events that occurred in their early adulthood, and find that those CEOs are averse to debt and lean excessively on internal finance. They also find, in contrast to Benmelech and Frydman (2015), that CEOs with military experience choose more aggressive or risk-taking capital structures with significantly higher market leverage ratios. More recently, Cantoni et al. (2017) exploit a major textbook reform in China between 2004 and 2010 to study the causal effect of school curricula on students' political attitudes. They find that students exposed to the new curriculum report that their mindset changed in the direction intended by the Chinese government.

II. Military conscription and the military culture

Military conscription is the mandatory enlistment in a country's armed forces. The origins of military conscription date back thousands of years to ancient Mesopotamia. Babylonian kingdoms employed a system of conscription called "ilkum," in which laborers owed military service to royal officials for the right to own land. The first

universal mass conscription of young men regardless of social class was in France during the French Revolution. After the French monarchy was overthrown in 1789, the French required a bigger army for its plans; thus, in 1793, the French government conscripted all unmarried and able-bodied men between the ages of 18 and 25 years.

Today, 35% of the world has military conscription.[1] Most commonly, men are conscripted at age 18 years for a period between 4 and 32 months.[2] During this period, young men are exposed to military training and to the military culture. In general, military training involves tasks intended to deconstruct their civilian status. Subsequently, having become receptive to new values, recruits are intensively exposed to the norms, authority relations, and disciplinary codes of the military organization, which are elucidated by senior members of the military (Soeters, Winslow, and Weibull 2006).

Military culture

The military has a specific form of institutional culture, where culture is defined as the values, norms, and assumptions that guide human action (Wilson 2008). Military culture has a language, a code of manners, norms of behavior, belief systems, a dress code, and rituals, and many of its tenets are defined by law (Meyer, Writer, and Brim 2016).

A specialized strand of the literature analyzes the codes and characteristics of military organizations. Lang (1965) points to various characteristics unique to the military. First, the uniform is worn inside and outside the organization. This characteristic

[1] Some countries have recently reintroduced military conscription (e.g., Sweden and Lithuania), and many countries that currently do not have military conscription are evaluating its reintroduction (e.g., France, Germany, and Italy). Retrieved from https://qz.com/1318379/france-joins-sweden-and-lithuania-in-bringing-back-mandatory-national-service/amp.

[2] Only a few countries also conscript women, e.g., China, North Korea, Israel, Eritrea, Taiwan, Malaysia, Libya, and Peru conscript both men and women.

relates to the degree to which the military organization controls various aspects and stages of personal life, much more than ordinary organizations. Second, hierarchy is heavily emphasized, which may lead to a certain authoritarian ideology. Third, a chain of command establishes a downward flow of directives, introducing discipline and control. Jobs in the military can be dangerous, and for this reason, military personnel are usually armed. If necessary, the military can use legitimized violence (Soeters, Winslow, and Weibull 2006), potentially making military members more prone to committing acts of violence.

The characteristics of military organizations relate to the individual characteristics of its members. Studies have described the military as being above average in authoritarianism, conservatism, aggressiveness, and traditionalism (Bachman, Sigelman, and Diamond 1987, Holsti 1998/99, Goertzel and Hengst 1971).

Soeters (1997) studies military culture among 30 countries and finds that despite occasional national differences, an international military culture also exists. In addition, Meyer, Writer, and Brim (2016) conclude that extended exposure to the military is unnecessary to absorb military culture and norms. These two factors are critical for the external validity of our findings because they suggest that our results from Argentina are likely to be valid in other countries and contexts, independent of the specific type of instruction and the period over which conscripts are exposed to military service.

III. Military conscription in Argentina

Military conscription in Argentina was mandatory between 1901 and 1994. The length of service was a minimum of 1 year in both the Army and the Air Force and up to 2 years in the case of the Navy. These services began with a 3-month instruction period where recruits learned military norms and military training. Following the initial training,

conscripts were allocated to a military unit to perform a specific duty, which was not necessarily involved in military tasks.[3]

From 1901 to 1976, males were conscripted at the age of 21 years; later, this aged was modified to age 18 years. The cohort born in 1955 was the last to serve at age 21 years, and the cohort born in 1958 was the first to serve at age 18 years.[4] The cohort born in 1976 was in the draft lottery but not drafted because military conscription was abolished in December 1994. Our analysis focuses on all cohorts that served at age 18, that is, on cohorts born between 1958 and 1975.

The eligibility of young males for military service was randomly determined by using the last three digits of their national identification numbers (IDs). Each year, a lottery assigned a number between one and 1,000 to each combination of the last three ID digits. The random assignment was conducted in a public session administered by the National Lottery. Results were broadcasted over the radio and published in major newspapers.

Following the lottery, individuals were called to have mental and physical examinations. Later, the government announced a cut-off number. Individuals whose ID number had been assigned a lottery number higher than the cut-off (and who had also passed the mental and physical examinations) were mandatorily called to military conscription.[5]

IV. Data and the survey

[3] For more details on military conscription in Argentina, see Rodriguez Molas (1983) and Galiani, Rossi, and Schargrodsky (2011).
[4] Because of this change, the cohorts born in 1956 and 1957 were not called to military conscription.
[5] Those individuals whose ID number was below the cut-off could be conscripted as volunteers, though the number of volunteers was not high (approximately 4%). Exemption was granted to clerics, seminarians, novitiates, and any individual with family members dependent upon him for support. Deferment to finish high school or attend college was granted up to a maximum of 10 years until the completion of studies. Deferment was also granted without a particular reason for a maximum of 2 years. In all cases, the lottery numbers and cut-offs used to decide eligibility were those of their specific cohort.

We measure personality traits and beliefs by using a web-based survey we conducted in November 2018.[6] We sent an email invitation to participate in the survey to an email list of approximately 19,000 Argentinian males born between 1958 and 1976. We received 1,133 completed surveys.

The call to answer the survey did not mention military conscription.[7] To encourage participation in the survey, participants were included in a raffle for smartphones. Participants entered the raffle with their last three ID digits. Asking for the last three ID digits to participate in raffles is a common practice in Argentina; thus, there is no reason to expect participants would associate the request of the last three ID digits with military conscription. One of the participants was awarded with a Samsung smartphone.

Survey questions

Our survey measures five personality traits and five specific beliefs related to military culture.

The personality traits are tolerance, discipline, authoritarianism, conservatism, and belligerence, and they are measured by using scales from the International Personality Item Pool (Goldberg 1999; Goldberg et al. 2006).[8] Each scale comprises a set of statements. The respondents indicate how much they agree or disagree with each item on a five-point scale where one is "Totally disagree" and five is "Totally agree." Following the literature, for each scale, we grouped the answers to obtain a single value.[9]

The specific beliefs are right to bear arms, justification of the use of violence to solve conflicts, justification of intervention of foreign countries, need for having

[6] The English version of the survey is presented in the appendix (Table A1).
[7] The English version of the recruitment email is presented in the appendix.
[8] International Personality Item Pool: A Scientific Collaboratory for the Development of Advanced Measures of Personality Traits and Other Individual Differences (http://ipip.ori.org/).
[9] Tolerance (Cloninger et al. 1994), discipline (Conn and Rieke 1994), authoritarianism (Simms et al. 2011), conservatism and belligerence (Tellegen 1995/2003).

mandatory military conscription, and justification for coups against democratically elected governments, and they are measured by using statements we wrote for this purpose. The respondents indicate how much they agree or disagree with each statement on a five-point scale where one is "Totally disagree" and five is "Totally agree." To analyze specific beliefs, we generate a dummy variable that takes the value of one if the person agrees or totally agrees with the statement, and zero otherwise.

From the survey, we also obtain self-reported information on the last three ID digits, year of birth, military conscription status, and pre-treatment characteristics (birth district, parents' education, parents' nationality, father's military conscription status).

Using the self-reported last three ID digits, year of birth, the lottery draft results, and the cut-off numbers by cohort, we define the dummy variable *Draft Eligible*, which takes the value of one for men whose last three ID digits obtained a lottery draft number above the cut-off, and zero otherwise.[10] We also construct the treatment variable *Conscription*, which takes the value of one for men who report being conscripted, and zero otherwise.

Interpretation of survey responses

The survey was conducted privately (online); thus, there is less reason to expect social stigma attached to particular responses or any changes in answers due to cues about what constitutes appropriate behavior (the so-called experimenter demand effect). In addition, for all outcomes and in each treatment assignment, we found responses in the full range, from one to five, and in every case, the modal response was provided by no more than 60% of men. These findings indicate that responses were not concentrated around a single "acceptable" response.

[10] We obtained draft lottery results and cutoff numbers from Galiani, Rossi, and Schargrodsky (2011).

The response rate to our survey, 6%, is lower than is typical in surveys using alternative methods (Shih and Fan 2008). A natural concern in this context is potential selection into the sample. If selection into the sample were nonrandom, our estimated treatment effects could be biased. For nonrandom selection into our sample to threaten the internal validity of our estimates, the selection would need to be differential by draft-eligibility status. We test for differential selection in the survey by assessing draft-eligibility status in three ways.

First, we check whether the proportion of draft-eligible men in our sample is similar to the population proportion. In the population, the average proportion of draft-eligible men for the cohorts 1958 to 1975 is 0.477. In our sample, the average proportion of draft-eligible men for these cohorts is 0.487. The difference between the two proportions is statistically indistinguishable from zero. In Table 1, we report population and sample proportions, by cohort. For 16 out of 18 cohorts, the difference between the population and sample proportions of draft-eligible men is statistically indistinguishable from zero.[11]

Second, we check whether the distribution of the last three ID digits in our sample is similar to the population (uniform) distribution. We first display the sample distribution of the last three ID digits, grouping the last three ID digits in bins of 100 consecutive numbers (10 bins of 100 numbers each). As shown in Figure 1, the sample distribution of the last three ID digits looks similar to a uniform distribution. Next, we run a chi-square test on the frequencies by using the original (ungrouped) data, and we

[11] In the appendix, we compare pre-treatment parents' nationality (Table A2), pre-treatment education (Figure A1), and pre-treatment district of origin (Table A3) between our sample and the population (cohorts 1958 to 1975). The differences between population and sample proportions in parents' nationality are statistically indistinguishable from zero. The distribution of education is different between our sample and the population (the population with low education is under-represented in our sample). Finally, for 12 out of 22 pre-treatment districts of origin, the differences between population and sample proportions are statistically indistinguishable from zero.

cannot reject the hypothesis that the sample distribution of the last three ID digits was drawn from a uniform distribution.

Third, although eligibility to be conscripted was randomly determined, we examine whether individuals' pre-treatment characteristics are balanced across the draft-eligible and the draft-exempted groups within our sample. Table 2 reports differences, by draft-eligibility status, in parents' education, parents' nationality, and whether his father was conscripted. These variables are all the pre-treatment characteristics available. For most of these pre-treatment characteristics, there are no statistically significant differences between the draft-eligible and the draft-exempted groups.[12]

Since (i) population and sample proportions of draft-eligible men are statistically indistinguishable, (ii) the sample distribution of the last three ID digits is statistically not different from the population (uniform) distribution, and (iii) pre-treatment characteristics are balanced within our sample; thus, we conclude the results reported in the next section are not subject to significant selection bias.

V. Econometric methods and results

We examine the causal effect of military conscription on beliefs and personality traits in a regression framework. Formally, we want to estimate the following equation:

$$Y_{ic} = \beta + \alpha\, Conscription_{ic} + \delta_c + \varepsilon_{ic} \qquad (1)$$

where Y_{ic} are the outcomes for individual i from birth cohort c, *Conscription* is a dummy variable that takes the value of one for those individuals who actually were conscripted, δ_c is a cohort fixed effect, and ε_{ic} is an error term. The coefficient of interest is α. We expect α to be negative in the equation for *Tolerance*, and positive for all other outcomes. In all estimates, we cluster standard errors at the ID-cohort level.

[12] Table A4 in the appendix reports differences, by draft-eligibility status, in birth district. Again, in most cases, there are no statistically significant differences between the draft-eligible and the draft-exempted groups.

Since military conscription is potentially endogenous in a model of beliefs and personality traits, we estimate equation (1) by two-stage least squares (2SLS), where we use *Draft Eligible* as an instrument for *Conscription*. The 2SLS estimator recovers the average treatment effect for draft-lottery compliers, that is, for the men conscripted because they were assigned a high lottery number but would not have been conscripted otherwise. Thus, 2SLS estimates do not need to generalize to the population of volunteers or to the population of young men who under no circumstances would have passed the pre-induction medical examination.

To draw general conclusions in a context of multiple outcomes, we construct an index of personality traits that aggregates the five personality trait measures, and an index of beliefs that aggregates the five belief measures. Each index is the equally weighted average of the z scores of its components (see Kling, Liebman, and Katz 2007). The z scores are levels standardized using the mean and standard deviation for the draft-exempted group. For the two indices, a higher z-score is associated with being closer to a military mindset. In addition to examining the effect of military conscription on broad indices, when we examine individual metrics, we address concerns of multiple hypotheses testing by presenting p values adjusted by using the false discovery rate procedure (Benjamini, Krieger, and Yekutieli 2006).

Columns (1) and (2) in Table 3 report first-stage estimates for the pooled sample of the 18 cohorts available, without and with controls. The point estimate of the coefficient on *Draft Eligible* in the pooled sample indicates that the probability of being conscripted is almost 40 percentage points higher for men in the draft-eligible group than for those in the draft-ineligible group. All first-stage effects are precisely estimated and significantly different from zero.

Columns (3) to (6) in Table 3 report the 2SLS estimates of equation (1), without and with controls.[13] There is a robust positive effect of military conscription on the indices of personality traits and beliefs. All coefficients in the 2SLS regressions are positive and statistically significant at the 1% level, indicating that being conscripted significantly moves beliefs and personality traits closer to a military mindset.[14]

To determine whether the effects are wide-ranging or concentrated in one or two outcomes, we estimate and report the effects on each separate metric, without and with controls, in Table 4. The effect on personality and beliefs of being conscripted appears quite general. For all ten metrics, the point estimates have the expected signs, and six of them are statistically significant.[15]

The size differences among personality traits are critical. For the mean effects without controls, we observe (Table 4) that tolerance is 5.4 percentage points lower (or 7.3% relative to the mean of the draft-ineligible group) for the conscripted men. Conservatism is 3.5 percentage points higher (5.5%) for the conscripted men, and the probability of having a violent personality increases by 4.4 percentage points (9.6%).

The differences in beliefs are even more notable. Being conscripted significantly increases the probability of accepting coups by 18.3 percentage points (280%), and the probability of supporting the right to bear arms by 14.6 percentage points (90%). These are nontrivial effects. Last, military conscription appears as a self-perpetuating institution: the probability of being in favor of military conscription is 17 percentage points higher (39%) for conscripted men.

[13] Table A5 in the appendix reports ordinary least squares (OLS) estimates of equation (1) for both the index of personality traits and the index of beliefs. Results without and with controls indicate that conscripted men have personality traits and beliefs more in line with those observed in military culture.
[14] Results are robust to excluding one cohort at a time (Figure A2 in the appendix).
[15] Table A6 in the appendix reports reduced-form estimates, without and with controls.

The 2SLS estimates are larger than the ordinary least squares (OLS) estimates. A plausible interpretation of this finding is that compliers are likely to be more prone to be influenced by military conscription than always-takers (i.e., volunteers) or never-takers (who find their way out if they are draft eligible).

Overall, our results indicate that military conscription has long-lasting effects on beliefs and personality traits. Conscripted men are less tolerant, more disciplined, more politically conservative, more authoritarian, and more belligerent. In addition, conscripted men are more likely to justify violence to solve conflicts, believe that military service should be mandatory, support coups against civilian governments, accept military interventions in foreign countries, and support the right to bear arms. The effect of military conscription on specific beliefs is stronger than its effects on personality traits. Nevertheless, the effect on personality traits is substantive and statistically significant.

Although our study relies on well-documented randomization, we conduct a placebo experiment to further test the exogeneity of our instrument. To achieve this objective, we exploit that the cohort of 1976 faced the lottery but was not ultimately drafted.[16] We create a fake cut-off number for this cohort by using the cut-off number for the 1975 cohort. Next, we compare outcomes for those with high and low numbers and find no differences between the two groups: the coefficient of the fake dummy for being draft eligible is statistically not significant for all outcomes (Table A7 in the appendix), and most of the coefficients are small and have the opposite sign. This placebo exercise also addresses the potential concern that the outcome of the lottery could have a direct effect on personality traits and beliefs through mechanisms other than military conscription.

[16] The lottery for the cohort born in 1976 was on May 27, 1994, but military conscription was abolished in December 1994.

Finally, we explore differential effects of military conscription for (i) men conscripted in the Navy (and thus completed 2 years of service, rather than 1 year for the Army and the Air Force), (ii) men conscripted during the Malvinas War (cohorts 1962 and 1963), and (iii) men conscripted during military dictatorships (7 cohorts served during military dictatorships and 11 cohorts during democracies). In all cases, the estimated differential effects are statistically not significant (Table A8 in the appendix).

VI. Conclusions and discussion

Military conscription is one of the most prevalent policies worldwide. Until now, however, no clear evidence has been provided on the causal effect of military conscription on an individual's mindset. Our paper provides novel evidence on the role military conscription has on shaping men's beliefs and personality traits.

Our empirical strategy combines administrative data on the military conscription lottery in Argentina with data from a survey on beliefs and personality traits. We find strong evidence that conscripted men are more likely to have mindsets in line with those observed in military culture. The magnitudes of the estimated effects are both statistically significant and quite large.

Our paper contributes to current policy discussions on the costs and benefits of reintroducing military conscription. These discussions are critical because some countries (mainly European countries, e.g., Sweden and Lithuania) have recently reintroduced military conscription, and many other countries (e.g., Italy, Romania, France, and Germany) are currently discussing returning to some type of military conscription.[17] Our paper contributes to these policy discussions by providing empirical evidence that military conscription, in addition to producing men who can serve in the

[17] Retrieved from https://www.washingtonpost.com/world/2018/10/19/military-draft-is-making-comeback-europe/?noredirect=on&utm_term=.a522c4488da0.

military, has the incidental effect of producing men who adopt a military mindset. Of course, Argentina's experiences with military rule in the recent past might affect the extent to which our results can be generalized to different settings.

To conclude, our paper highlights the important role of military conscription in shaping values and beliefs. Our natural experiment, however, does not identify the mechanisms through which military conscription affects personality traits and beliefs.

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Table 1. Draft-eligibility status

Cohort	Sample size	Population proportion	Sample proportion	Difference
1958	43	0.825	0.870	-0.045
1959	70	0.680	0.681	-0.001
1960	57	0.659	0.606	0.053
1961	80	0.650	0.624	0.027
1962	64	0.680	0.735	-0.055
1963	48	0.650	0.623	0.027
1964	37	0.600	0.676	-0.076
1965	58	0.607	0.705	-0.098
1966	44	0.373	0.451	-0.078
1967	41	0.333	0.186	0.147**
1968	79	0.413	0.381	0.032
1969	68	0.446	0.526	-0.080
1970	57	0.502	0.532	-0.030
1971	81	0.281	0.264	0.017
1972	68	0.164	0.268	-0.104*
1973	57	0.240	0.203	0.037
1974	55	0.256	0.210	0.046
1975	49	0.257	0.340	-0.083
Total	1,133	0.477	0.487	-0.010

Notes: *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 2. Pre-treatment characteristics, by draft-eligibility assignment

	Draft-eligible mean	Non draft-eligible mean	Difference
Father's country of birth	0.920	0.910	0.010
	(0.271)	(0.286)	(0.017)
Mother's country of birth	0.906	0.921	-0.015
	(0.292)	(0.270)	(0.017)
His father did military conscription	0.623	0.632	-0.009
	(0.485)	(0.483)	(0.029)
Father's maximum level of education			
No instruction	0.015	0.010	0.004
	(0.120)	(0.101)	(0.007)
Incomplete primary school	0.129	0.120	0.009
	(0.335)	(0.326)	(0.020)
Complete primary school	0.250	0.225	0.025
	(0.433)	(0.418)	(0.025)
Incomplete secondary school	0.114	0.114	0.000
	(0.318)	(0.318)	(0.019)
Complete secondary school	0.158	0.181	-0.023
	(0.365)	(0.385)	(0.022)
Incomplete high education	0.024	0.033	-0.009
	(0.152)	(0.178)	(0.010)
Complete high education	0.063	0.038	0.026*
	(0.244)	(0.191)	(0.013)
Incomplete university	0.073	0.083	-0.010
	(0.259)	(0.276)	(0.016)
Complete university	0.165	0.186	-0.021
	(0.371)	(0.389)	(0.023)
Mother's maximum level of education			
No instruction	0.011	0.016	-0.005
	(0.104)	(0.124)	(0.007)
Incomplete primary school	0.116	0.103	0.013
	(0.320)	(0.305)	(0.019)
Complete primary school	0.310	0.248	0.062**
	(0.463)	(0.432)	(0.027)
Incomplete secondary school	0.101	0.115	-0.014
	(0.302)	(0.320)	(0.019)
Complete secondary school	0.212	0.246	-0.034
	(0.409)	(0.431)	(0.025)
Incomplete high education	0.024	0.010	0.013*
	(0.152)	(0.101)	(0.008)
Complete high education	0.138	0.153	-0.015
	(0.345)	(0.360)	(0.021)
Incomplete university	0.034	0.036	-0.002
	(0.182)	(0.187)	(0.011)
Complete university	0.053	0.069	-0.016
	(0.223)	(0.253)	(0.014)

Notes: *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 3. Main results

	(1)	(2)	(3)	(4)	(5)	(6)
	Conscription		Index of personality traits		Index of beliefs	
Draft Eligible	0.393***	0.394***				
	(0.027)	(0.027)				
Conscription			0.342***	0.328***	0.382***	0.393***
			(0.103)	(0.102)	(0.112)	(0.111)
F test	219.447***	220.793***	219.447***	220.793***	219.447***	220.793***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Method	OLS	OLS	2SLS	2SLS	2SLS	2SLS
Controls	No	Yes	No	Yes	No	Yes
Observations	1,133	1,133	1,133	1,133	1,133	1,133

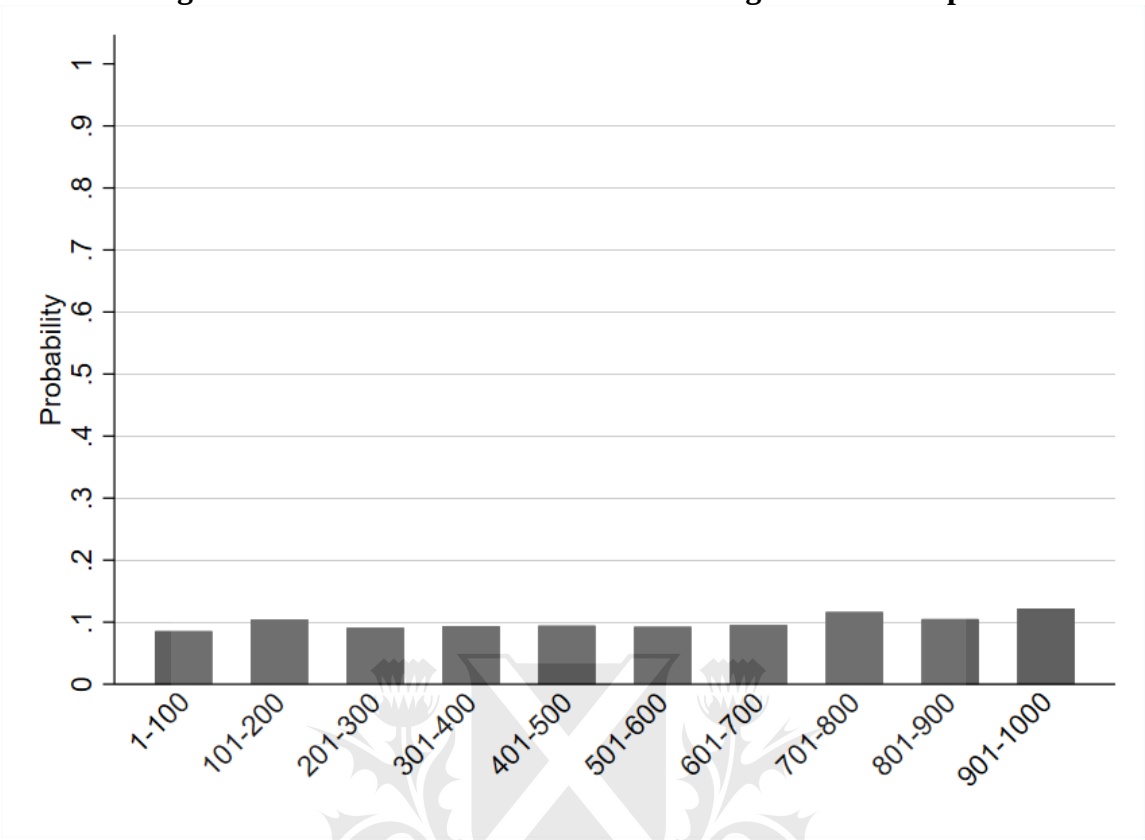
Notes: Standard errors clustered at the ID-cohort level are in parentheses. All models include cohort dummies. The set of controls includes district of origin dummies and all variables listed in Table 2. In the 2SLS models, Conscription is instrumented using Draft Eligible. The F test is the F test of excluded instruments (p values are in parentheses). *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 4. Impact of military conscription on personality traits and beliefs, by outcome

	(1) Tolerance	(2) Discipline	(3) Conservatism	(4) Authoritarianism	(5) Violence or Belligerence	(6) In favor of right to bear arms	(7) Justify violence to solve conflicts	(8) Accept countries' interventions	(9) In favor of military conscription	(10) Accept coups
Conscription	-0.054***	0.006	0.035**	0.028	0.044**	0.146**	0.088	0.089	0.170**	0.183***
	(0.015)	(0.017)	(0.015)	(0.026)	(0.018)	(0.067)	(0.068)	(0.071)	(0.081)	(0.046)
P value	0.000	0.739	0.019	0.287	0.014	0.030	0.196	0.212	0.037	0.000
FDR-p-value	0.002	0.362	0.041	0.147	0.039	0.050	0.119	0.119	0.050	0.001
% change	-7.34	0.80	5.46	5.40	9.57	90.24	45.25	36.93	39.35	279.80
Controls	No	No	No	No	No	No	No	No	No	No
Observations	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133
Conscription	-0.053***	0.004	0.035**	0.024	0.042**	0.149**	0.072	0.101	0.192**	0.187***
	(0.015)	(0.017)	(0.015)	(0.026)	(0.018)	(0.068)	(0.069)	(0.071)	(0.081)	(0.045)
P value	0.000	0.828	0.019	0.361	0.017	0.029	0.296	0.158	0.017	0.000
FDR-p-value	0.002	0.331	0.031	0.192	0.031	0.039	0.175	0.100	0.031	0.001
% change	-7.20	0.53	5.46	4.63	9.14	92.09	37.02	41.91	44.44	285.91
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133

Notes: Standard errors clustered at the ID-cohort level are in parentheses. FDR-p-values are False Discovery Rates adjusted p values, following the procedure in Benjamini, Krieger, and Yekutieli (2006). All models are estimated using 2SLS. Conscription is instrumented using Draft Eligible. All models include cohort dummies. The set of controls includes district of origin dummies and all variables listed in Table 2. Percentage change is calculated relative to the mean of the outcome in the draft-ineligible group. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Figure 1. Distribution of the last three ID digits in our sample



Appendix

Invitation to answer the survey

We invite you to participate in an investigation of personality traits. This is a strictly academic project directed by a team of researchers from Universidad de San Andrés. Answering this survey should take you approximately 10 minutes. Your answers are completely anonymous. After completing the questionnaire, you will be given a code with which you will be participating in a raffle for smartphones (Samsung Galaxy J7 Neo). At the end of the survey, we will give you the details to participate in the raffle.

Table A1. Questions Survey

Personal Information	Questions
	Month of birth
	Year of birth
	District of origin
	Father's country of birth
	Father's maximum level of education
	Mother's country of birth
	Mother's maximum level of education
	Did your father do military conscription?
	Did you do military conscription?
Beliefs/Personality traits	Questions
Beliefs	Having a weapon should be a right
	The use of violence is justified to resolve certain conflicts
	Intervention from one country to another is justified under certain circumstances
	Military service should be mandatory
	A coup is acceptable when a government is incompetent
Authoritarianism	I boss people around
	I like having authority over others
	I insist that others do things my way
	I make demands on others
	I have a strong need for power
	I am known as a controlling person
Conservatism	I tend to vote for conservative political candidates
	I believe in one true religion
	I believe that we should be tough on crime
	I tend to vote for liberal political candidates
	I believe in the importance of art
	I don't consider myself religious
	I believe that there is no absolute right and wrong
	I believe that criminals should receive help rather than punishment
Discipline	I believe laws should be strictly enforced
	I use swear words
	I try to follow the rules
	I oppose authority
	I respect authority
	I know how to get around the rules
	I like to stand during the national anthem
	I resist authority
	I break rules
Tolerance	I accept people as they are
	I am a bad loser

	I respect others
	I get irritated easily
	I sympathize with the homeless
	I lay down the law to others
	I believe there are many sides to most issues
	I treat people as inferiors
	I believe that others have good intentions
	I am quick to judge others
	I can accept a lot from others
	I am annoyed by others' mistakes
Violence/Belligerence	I get back at others
	I try to forgive and forget
	I hold a grudge
	I rarely get irritated
	I do things out of revenge
	I cheat to get ahead
	I have a sharp tongue
	I would never take things that aren't mine
	I seldom get mad
	I rarely complain

Table A2. Population and sample proportions of parents' nationality

		Population	Sample	Difference	P value
Father's nationality	(Argentina = 1)	90.850	91.370	-0.520	0.524
Mother's nationality	(Argentina = 1)	91.390	91.460	-0.070	0.936

Table A3. Population and sample proportions of pre-treatment district of origin

	Population	Sample	Difference	P value
Buenos Aires	0.477	0.531	-0.054	0.003
Catamarca	0.008	0.009	-0.001	0.766
Chaco	0.026	0.017	0.009	0.033
Chubut	0.011	0.009	0.002	0.658
Cordoba	0.085	0.067	0.018	0.011
Corrientes	0.024	0.018	0.007	0.173
Entre Rios	0.031	0.025	0.006	0.114
Formosa	0.012	0.008	0.005	0.125
Jujuy	0.016	0.018	-0.002	0.840
La Pampa	0.008	0.008	0.000	0.766
La Rioja	0.007	0.008	-0.001	0.981
Mendoza	0.043	0.030	0.013	0.032
Misiones	0.024	0.010	0.014	0.000
Neuquen	0.012	0.022	-0.010	0.021
Rio Negro	0.016	0.008	0.007	0.010
Salta	0.027	0.036	-0.010	0.098
San Juan	0.016	0.009	0.007	0.031
San Luis	0.009	0.007	0.002	0.226
Santa Cruz	0.007	0.008	-0.001	0.511
Santa Fe	0.086	0.082	0.004	0.631
Santiago del Estero	0.021	0.016	0.005	0.169
Tucuman	0.035	0.052	-0.017	0.014

Notes: Buenos Aires includes the military districts of Bahia Blanca, Buenos Aires, Junin, La Plata, San Martin, and Tandil. Cordoba includes the military districts of Rio Cuarto and Cordoba. Santa Fe includes the military districts of Rosario and Santa Fe. Santa Cruz includes Tierra del Fuego. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table A4. District of origin, by draft-eligibility assignment

	Draft-eligible mean	Non draft-eligible mean	Difference
Buenos Aires	0.567	0.497	0.070**
	(0.496)	(0.500)	(0.030)
Catamarca	0.011	0.007	0.004
	(0.104)	(0.083)	(0.006)
Chaco	0.015	0.021	-0.006
	(0.120)	(0.142)	(0.008)
Chubut	0.011	0.009	0.002
	(0.104)	(0.092)	(0.006)
Cordoba	0.058	0.074	-0.016
	(0.234)	(0.262)	(0.015)
Corrientes	0.018	0.019	-0.001
	(0.133)	(0.136)	(0.008)
Entre Rios	0.018	0.029	-0.011
	(0.133)	(0.169)	(0.009)
Formosa	0.011	0.005	0.006
	(0.104)	(0.072)	(0.005)
Jujuy	0.016	0.017	-0.001
	(0.127)	(0.130)	(0.008)
La Pampa	0.013	0.005	0.008
	(0.112)	(0.072)	(0.006)
La Rioja	0.007	0.003	0.004
	(0.104)	(0.059)	(0.005)
Mendoza	0.040	0.024	0.016
	(0.196)	(0.153)	(0.010)
Misiones	0.011	0.010	0.001
	(0.104)	(0.101)	(0.006)
Neuquen	0.020	0.024	-0.004
	(0.140)	(0.153)	(0.009)
Rio Negro	0.011	0.007	0.004
	(0.104)	(0.083)	(0.006)
Salta	0.027	0.045	-0.018
	(0.163)	(0.207)	(0.011)
San Juan	0.009	0.010	-0.001
	(0.095)	(0.101)	(0.006)
San Luis	0.005	0.007	-0.001
	(0.074)	(0.083)	(0.005)
Santa Cruz	0.011	0.007	0.004
	(0.104)	(0.083)	(0.006)
Santa Fe	0.076	0.088	-0.012
	(0.265)	(0.283)	(0.016)
Santiago del Estero	0.007	0.024	-0.017**
	(0.085)	(0.153)	(0.007)
Tucuman	0.034	0.067	-0.033***
	(0.182)	(0.250)	(0.013)

Notes: Buenos Aires includes the military districts of Bahia Blanca, Buenos Aires, Junin, La Plata, San Martin, and Tandil. Cordoba includes the military districts of Rio Cuarto and Cordoba. Santa Fe includes the military districts of Rosario and Santa Fe. Santa Cruz includes Tierra del Fuego. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table A5. OLS estimates: impact of military conscription on personality traits and beliefs

	(1)	(2)	(3)	(4)
	Index of personality traits		Index of beliefs	
Conscription	0.234***	0.216***	0.326***	0.290***
	(0.051)	(0.050)	(0.056)	(0.055)
Controls	No	Yes	No	Yes
Observations	1,133	1,133	1,133	1,133
Mean of output	0.067	0.067	0.086	0.086
SD of output	0.628	0.628	0.695	0.695

Notes: Standard errors clustered at the ID-cohort level are in parentheses. All models include cohorts' fixed effects. The set of controls includes the district of origin dummies and all variables listed in Table 2. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table A6. Reduced form

	(1) Tolerance	(2) Discipline	(3) Conservatism	(4) Authoritarianism	(5) Violence or Belligerence	(6) In favor of right to bear arms	(7) Justify violence to solve conflicts	(8) Accept countries' interventions	(9) In favor of military conscription	(10) Accept coups
Draft Eligible	-0.021***	0.002	0.014**	0.011	0.017**	0.058**	0.035	0.035	0.067**	0.072***
	(0.006)	(0.007)	(0.006)	(0.010)	(0.007)	(0.027)	(0.027)	(0.028)	(0.033)	(0.019)
P value	0.000	0.742	0.023	0.295	0.017	0.033	0.204	0.218	0.042	0.000
FDR-p-value	0.002	0.375	0.048	0.151	0.047	0.057	0.123	0.123	0.060	0.002
Controls	No	No	No	No	No	No	No	No	No	No
Observations	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133
Draft Eligible	-0.021***	0.001	0.014**	0.009	0.017**	0.059**	0.028	0.040	0.076**	0.074***
	(0.006)	(0.007)	(0.006)	(0.010)	(0.007)	(0.028)	(0.028)	(0.029)	(0.033)	(0.018)
P value	0.000	0.833	0.024	0.376	0.023	0.034	0.312	0.171	0.022	0.000
FDR-p-value	0.002	0.333	0.04	0.201	0.04	0.049	0.185	0.109	0.04	0.001
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133	1,133

Notes: Standard errors clustered at the ID-cohort level are in parentheses. FDR-p-values are False Discovery Rates adjusted p values, following the procedure in Benjamin, Krieger, and Yekutieli (2006). All models include cohort dummies. The set of controls includes the district of origin dummies and all variables listed in Table 2. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table A7. Placebo regression: cohort that faced the lottery but eventually was not drafted

	(1) Tolerance	(2) Discipline	(3) Conservatism	(4) Authoritarianism	(5) Violence or Belligerence	(6) In favor of right to bear arms	(7) Justify violence to solve conflicts	(8) Accept countries' interventions	(9) In favor of mandatory conscription	(10) Accept coups
Draft Eligible	0.002	-0.028	-0.031	-0.011	-0.027	0.025	-0.128	0.016	-0.026	0.019
	(0.023)	(0.030)	(0.027)	(0.039)	(0.027)	(0.106)	(0.107)	(0.138)	(0.150)	(0.062)
Constant	0.729***	0.749***	0.623***	0.508***	0.477***	0.133**	0.233***	0.300***	0.500***	0.033
	(0.014)	(0.019)	(0.013)	(0.022)	(0.018)	(0.063)	(0.079)	(0.085)	(0.093)	(0.033)
Method	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Controls	No	No	No	No	No	No	No	No	No	No
Observations	49	49	49	49	49	49	49	49	49	49

Notes: Standard errors clustered at the ID-cohort level are in parentheses. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table A8. Further results: Navy, Malvinas War, and dictatorship

	(1)	(2)	(3)	(4)
	Index of personality traits		Index of beliefs	
Navy				
Draft Eligible	0.125***	0.116**	0.127***	0.131***
	(0.046)	(0.046)	(0.049)	(0.049)
Draft Eligible*Navy	0.039	0.053	0.095	0.094
	(0.067)	(0.067)	(0.087)	(0.085)
Controls	No	Yes	No	Yes
Observations	1,133	1,133	1,133	1,133
Malvinas War				
Draft Eligible	0.146***	0.143***	0.153***	0.164***
	(0.044)	(0.045)	(0.049)	(0.049)
Draft	-0.110	-0.117	-0.027	-0.079
Eligible*Malvinas	(0.138)	(0.133)	(0.128)	(0.130)
Controls	No	Yes	No	Yes
Observations	1,133	1,133	1,133	1,133
Dictatorship				
Draft Eligible	0.147***	0.149***	0.130**	0.155**
	(0.056)	(0.058)	(0.062)	(0.063)
Draft	-0.029	-0.046	0.047	-0.001
Eligible*Dictatorship	(0.084)	(0.086)	(0.091)	(0.094)
Controls	No	Yes	No	Yes
Observations	1,133	1,133	1,133	1,133

Notes: Standard errors clustered at the ID-cohort level are in parentheses. All models include cohorts' fixed effects. The set of controls includes the district of origin dummies and all variables listed in Table 2. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Figure A1. Population and sample distributions of parents' education

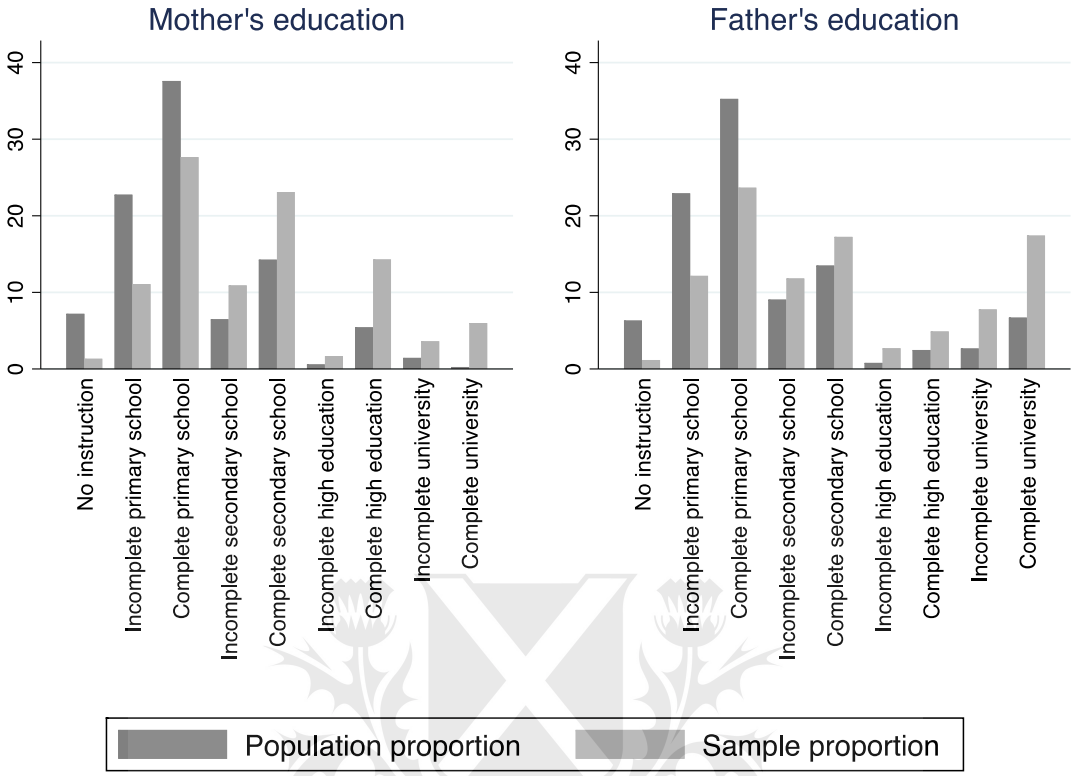
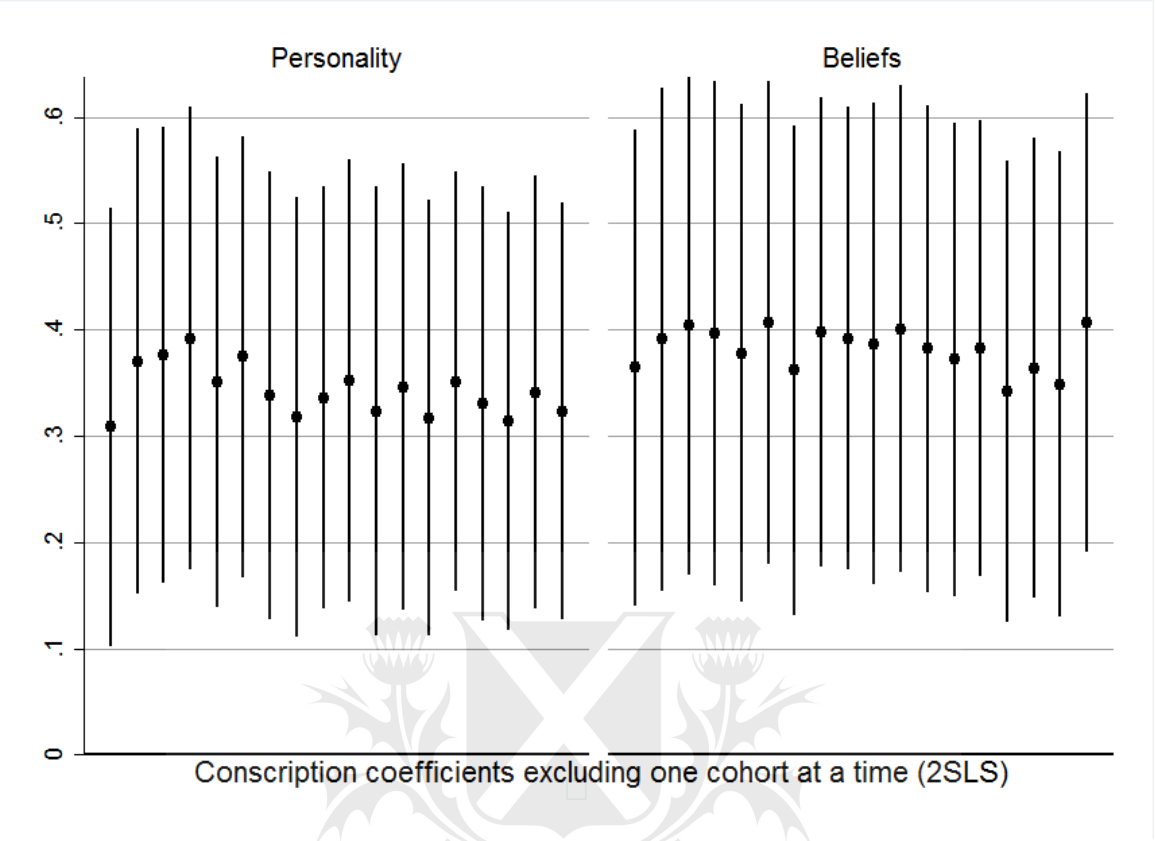


Figure A2. Robustness check: results excluding one cohort at a time



Chapter 2*

Impact of ICTs Training Program on Formal Employment: Experimental Evidence from the City of Buenos Aires

I. Introduction

Concerns that an adequately skilled workforce does not meet changing skill needs associated with new technologies, has placed the issue of skill mismatch (the incongruence between skill supply and skill demand) high up in the agenda of policymakers (Honorati and McArdle 2013; Cappelli 2015; Polacheck et al. 2017). Despite the existence of numerous training programs developed to address the skill mismatch problem, the evidence on their effects is not conclusive. In this study, we present new experimental evidence on the causal effect of a training program in the city of Buenos Aires, Argentina, on formal employment.

The government of the city of Buenos Aires implemented in 2017 the training program "*Programa Codo a Codo*" *(*PCC), focused on basic programming languages and Java software management. The main objective was to improve employability in the labor market of Information and Communication Technologies (ICTs). In global terms, in the last decades ICTs has been one of the most dynamic labor markets, with an increase in the demand of workers that generated an excess labor demand in the ICTs market (Dapozo et al. 2014; Hüsing, Korte, and Dashja 2015). Argentina is not an exception to

* This chapter was written with Nicole Aranovich. We are grateful to "Programa Codo a Codo" for sharing with us the administrative databases on the program. We thank Santiago Pellegrino, Adrian Romero, Matías Busto, Diego Jorrat, Marco Di Natale, Laura Contreras, Alan Acosta, Christian Ruzzier, Martín Rossi, Florencia Hnilo, and Lucio Wasserman for useful comments and suggestions.

this global phenomenon (Jacinto 1999; Jacinto and Millenaar 2012; Jacinto and Gallart 1998). The excess labor demand in the Argentine ICTs market motivated the launch of PCC. Through several meetings with leading companies, such as Mercado Libre and Accenture, the designers of PCC were able to elaborate a program that covered most of the basic skills required by ICTs companies.

We study whether PCC improved employment opportunities in the formal labor market. In particular, we explore if the participation in the program increases the probability of having a formal job. To address potential endogeneity concerns, we exploit the random assignment into the program.

We use administrative data on the random assignment and administrative data on formal employment for the population of applicants. We find that participation in PCC decreases the probability of having a formal job. We explore potential underlying reasons to this undesired result, and we find no evidence of individuals switching to the informal sector nor evidence of individuals motivated to follow further education.

This paper relates to an abundant literature on the impact of training programs. In a meta-study of training programs in Europe and the United States, Card, Kluve, and Weber (2010) indicate that these programs have, at best, a moderate impact. González-Velosa, Ripani, and Rosas-Shady (2012) and Vezza (2014) review the available evidence for Latin America, and they highlight the fact that most programs in the region are either not evaluated or evaluated with non-experimental tools. Among the few experimental evaluations available in the region, Alzúa, Cruces, and López (2016) study the effect of a life-skills and vocational training program for low-income youth in Argentina. They find positive and highly significant short-term (18 months) effects on employment, although this effect dissipates in the medium and in the long term. Attanasio, Kugler, and Meghir (2011) study a youth vocational training program in Colombia. They find a

positive and significant impact of the program on women's formal employment, but no impact on men's formal employment. Card et al. (2011) present the evaluation of a youth training program in the Dominican Republic. This program targeted young people who had not completed high school. They find no significant impact on employment. Ibarrarán et al. (2015) present a follow-up of the same cohort, and while they still fail to find any effects on average employment, they report significant impacts on the probability of holding a formal job in the longer term.

The program we study here is different from those previously evaluated in Latin America. First, while most programs in the region focus on the youth, PCC only required participants to be at least 18 years old. Second, while most programs in the region focus on basic skills, vocational and technical training, entrepreneurial skills and soft skills, PCC focused on programming skills. Finally, while most programs facilitate labor placement of students through agreements with local companies or small businesses, this was not the case for PCC.

II. Data

The Program

PCC is a formal education program that offers capacitation in different programming languages and use of new technologies. Its main objective is to provide the necessary basic skills to apply for jobs in the growing technological industry, which requires knowledge in software and programming skills.

The Sub-secretary of Professional Technical Training and Teaching Career and the former Sub-secretary of Innovation and Smart-City implemented PCC in 2017.[18] The

[18] The Sub-secretary of Innovation and Smart-City depended on the Modernization, Innovation, and Technology Ministry, that dissolved after PCC was created. The program is currently driven by the Secretary of Science, Technology, and Innovation, within the Education Ministry of the city of Buenos Aires.

course outline consisted of nine hours a week of on-site classes in one of the 35 study points around the city of Buenos Aires. Every classroom had all the necessary facilities to guarantee theoretical and hand-on learning of each student. The course included four elementary learning modules: programming techniques, object-oriented programming, database, and software development. The program focused on Java programming language.

PCC required participants to be at least 18 years old and to have completed secondary school. First, applicants had to complete an online inscription from February 22, 2017 to March 10, 2017. Second, applicants had to validate the inscription information in a personal appointment. Once those in charge of enrollments validated the candidates list, a specialized software randomly assigned the available places in each commission.[19]

We evaluated the program's first edition, which started in March 2017 and finished in December 2017. The program received 7,588 on-line applications, but 462 did not meet the Program's basic requirements. Therefore, the final number of enrollments was 7,126.

Because of physical restrictions, there was a limited number of individuals considered for each commission. The total sum of the commission's capacity limit from all the final centers was of 2,732 students.

The random assignment of 2,732 beneficiaries was made on March 11, 2017. In order to cover any eventual vacancy, the software generated a complete list that provides the order of selection to the program of the 7,126 eligible individuals. The rule in order

[19] The educational centers were spatially spread throughout the city of Buenos Aires. To guarantee the best possible site for each candidate, the software's algorithm matched candidates to the location closest to his/her address.

to cover eventual vacancies was that if a selected participant did not show up to the first lesson, PCC would contact the next person on the waiting list.

From the 2,732 randomized to treatment group, 1,248 decided not to participate in the program. The final number of participants was 2,352, of which 1,484 were originally drawn in the first randomized list and 868 were added later.

Data

To study if participation in the program increased the probability of having a formal job, we use two administrative databases provided by PCC. The first database includes baseline information about the applicants from the program's application form and information about whether or not the individual participated in the courses. The second database, provided by the National System of Tributary and Social Identification (SINTyS), includes individual information on formal employment 7 months after the intervention.[20]

From the 7,126 eligible individuals, PCC could not get information of 13 of them because of typos and mistakes in their applications (specifically on their ID numbers). Thus, the final number of individuals in the sample is 7,113. Thus, we conclude that attrition is unlikely to bias the main result.

Table 1 provides descriptive statistics of the data.

An implication of random assignment is that pre-treatment characteristics should be orthogonal to randomization status. We perform tests of balancing of pre-treatment characteristics by treatment status. As reported in Table 2, 8 out of 9 pre-treatment characteristics are balanced between those assigned to the treatment group and those assigned to the control group, thus validating the lottery assignment.

[20] Throughout a legal agreement, PCC accessed administrative data from SINTyS for the population of eligible individual.

We conclude that random assignment into the program, the balancing of pre-treatment characteristics, and the insignificance of attrition indicated in the results presented below are not subject to significant sources of selection bias.

Another potential source of bias comes from non-compliance. As is the case for most social programs, non-compliance was not perfect in the case of PCC, since many applicants originally assigned to the treatment group did not participate in the program, and some applicants originally assigned to the control group ended up being treated (see Table 3).

Even though compliance is not perfect, first-stage results indicate that *Randomized to treatment* is a strong instrument for *Participant*. As shown in Table 4, being randomly assigned to participate in the program significantly increases the probability of actually participating in the program in approximately 35 percentage points.

III. Empirical strategy and results

We want to estimate the causal impact of participating in the program on the probability of having a formal job. Formally, we want to estimate the following equation:

$$Formal_i = \alpha + \beta\ Participant_i + \delta X_i + \varepsilon_i \quad (1)$$

where *Formal* is a variable that takes the value of one for those individuals who have a formal job, *Participant* is a dummy variable that takes the value of one for participants in the program, X is a vector of pre-treatment characteristics, and ε is an error term. The coefficient of interest is β.

As a benchmark, in Table 5 we report Ordinary Least Squares (OLS) estimates of equation (1). OLS estimates in columns (1) and (2) show a negative correlation between participating in the program and having a formal job.

Reduced-form estimates reported in columns (3) and (4) of Table 5 anticipate a negative causal effect between participating in the program and the probability of having a formal job.

Since participating in the program is potentially endogenous in a model for formal employment, we estimate equation (1) by Two Stage Least Squares (2SLS), where we use *Randomized to treatment* as an instrument for *Participant*.[21] The 2SLS estimator recovers the average treatment effect for lottery compliers. That is, for those who participate in the program because they were randomly assigned by the lottery, and would not have participated otherwise.

Table 6 also reports the preferred 2SLS estimates, with and without controls. All coefficients in the 2SLS regressions are negative and statistically significant at the 1% level, indicating that participating in the program has a negative impact on the probability of having a formal job.

The magnitude of the estimated coefficients is economically relevant. Focusing on mean effects in 2SLS estimates, the probability of having a formal job is 16.4 percentage points lower (or 30.7% relative to the mean of those randomized to control group) for those who participate in the program.

Overall, our results indicate that the participation in the program has negative effects on the probability of having a formal job seven months after the intervention.

IV. Further results

To explore further results, we use social and motivational information from the people who had signed up for PCC using a web-based survey that the program conducted through the month of August 2018 (precisely from August 6 to 10). PCC sent an email

[21] *Randomized to treatment* is a dummy variable that takes the value one for applicants randomly assigned to participate in the program, and zero otherwise.

invitation to participate in the survey to the 7,113 individuals in our sample.[22] PCC received 1,100 answered surveys (the response rate was 15.5%). We combine the survey database with the randomization results and the data from the program's application form.

A natural concern when using a survey is potential selection into the sample. If selection into the sample were nonrandom, our estimated treatment effects may be biased. To validate our sample, we proceed in two ways. We first check whether our main results can be reproduced in the sample of respondents to the survey. We obtained similar conclusions, in the sense that the coefficient of interest is negative and significant. However, the coefficient is higher than in our main results (see Table A1 in the appendix). We also perform balancing check of pre-treatment characteristics by treatment assignment status, and most pre-treatment characteristics are balanced between the treated group and the control group (see Table A2 in the appendix).

The survey allows us to test some potential mechanisms driving the negative effect between participating in the program and having a formal job. As shown in column (1), (2), and (3) in Table 7, we find that participation in the program has a negative and significant effect on having a job, a positive and significant effect on being unemployed, and a positive but small increase in informal labor. Overall, we conclude that there is no evidence of individuals switching from formal to informal market. In addition, as shown in column (4) in Table 7, we find no effect of the program on motivation to follow further education.

V. Final remarks

[22] To encourage participation in the survey, participants were included in a raffle for multiple pairs of tickets for any show of the *Complejo Teatral Buenos Aires*.

We provide experimental evidence on the impact of an ICTs training program in the city of Buenos Aires on the probability of having a formal job. Using administrative data on formal employment seven months after the intervention, we find that participants in the program are less likely to have a formal job. The magnitude of the estimated coefficient is statistically significant and economically relevant.

We also use survey data of a sample of applicants to explore some potential underlying reasons to our unexpected result. We find no evidence of individuals switching to the informal sector nor evidence of individuals motivated to follow further education. Further research is needed in order to understand why an ICTs training program can decrease the probability of having a formal job.

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Table 1. Summary statistics

	Observations	Mean	Standard deviation
Pre-treatment characteristics			
Age	7,111	31.173	8.888
Gender	7,113	0.625	0.484
Argentine born	7,113	0.857	0.350
Level of education			
Complete secondary school	7,113	0.393	0.489
Incomplete high education	7,113	0.101	0.301
Complete high education	7,113	0.105	0.306
Incomplete university	7,113	0.267	0.443
Complete university	7,113	0.134	0.341
Has "MI BA"	7,113	0.353	0.478
Treatment			
Participant	7,113	0.331	0.470
Randomized to treatment	7,113	0.120	0.486
Outcomes			
Formal job	7,113	0.526	0.499

Notes: Pre-treatment characteristics are observed in the inscription of the first edition of the program. Argentine born is a dummy variable that takes the value one for applicants who were born in Argentina. Complete secondary school is a dummy variable that takes the value one for applicants who has finished secondary school. Incomplete high education is a dummy variable that takes the value one for applicants who has not finished high education. Complete high education is a dummy variable that takes the value one for applicants who has finished high education. Incomplete university is a dummy variable that takes the value one for applicants who has not finished university. Has "MI BA" is a dummy variable that takes the value one for applicants who has an account on the Government Platform "Mi BA". Randomized to treatment is a dummy variable that takes the value one for applicants that were assigned to participate in the program. The outcome is observed on July 27, 2018. Formal job is a dummy variable that takes the value one if the person was registered as a formal worker in SINTyS.

Table 2. Pre-treatment characteristics, by treatment assignment status

	Randomized to treatment	Randomized to control	Difference	P-value
Age	31.290	31.060	0.230	0.292
	(9.150)	(8.61)	(0.215)	
Gender	0.631	0.623	0.008	0.472
	(0.483)	(0.485)	(0.012)	
Argentine born	0.821	0.836	0.015	0.107
	(0.371)	(0.371)	(0.009)	
Level of education				
Complete secondary school	0.399	0.385	0.014	0.247
	(0.490)	(0.487)	(0.012)	
Incomplete high education	0.088	0.105	-0.018	0.015
	(0.283)	(0.307)	(0.007)	
Complete high education	0.102	0.106	-0.004	0.630
	(0.303)	(0.307)	(0.007)	
Incomplete university	0.272	0.147	0.125	0.180
	(0.445)	(0.437)	(0.011)	
Complete university	0.140	0.147	-0.007	0.420
	(0.347)	(0.354)	(0.009)	
Has "MI BA"	0.342	0.345	-0.003	0.785
	(0.474)	(0.475)	(0.012)	

Notes: P-value corresponds to a test of differences in means by treatment assignment status.

Table 3. Non-compliance for participant

	Non-participant	Participant	Total
Randomized to treatment	1,248	1,484	2,732
Randomized to control	3,513	868	4,381
Total	4,761	2,532	7,113

Table 4. First stage

	(1)	(2)
	Participant	
Randomized to treatment	0.345***	0.344***
	(0.011)	(0.011)
Controls	No	Yes
Observations	7,113	7,111

Notes: Robust standard errors are shown in parentheses. The set of controls includes all pre-treatment characteristics listed in Table 1. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 5. OLS and reduced-form estimates

	(1)	(2)	(3)	(4)
	Formal job		Formal job	
Participant	-0.019	-0.027**		
	(0.013)	(0.012)		
Randomized to treatment			-0.057***	-0.055***
			(0.012)	(0.012)
Controls	No	Yes	No	Yes
Observations	7,113	7,111	7,113	7,111

Notes: Robust standard errors are shown in parentheses. The set of controls includes all pre-treatment characteristics listed in Table 1. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 6. 2SLS estimates

	(3)	(4)
	Formal job	
Participant	-0.164***	-0.160***
	(0.036)	(0.035)
Percentage change	-30.7%	-30.4%
Controls	No	Yes
Observations	7,113	7,111

Notes: Robust standard errors are shown in parentheses. The set of controls includes all pre-treatment characteristics listed in Table 1. Percentage change is calculated with respect to the mean of the control group. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 7. Further results: 2SLS estimates

	(1) Work	(2) Informal	(3) Unemployed	(4) Interested in education
Participant	-0.156**	0.094	0.153**	0.017
	(0.079)	(0.078)	(0.076)	(0.085)
Constant	0.829***	0.196***	0.142***	0.622***
	(0.047)	(0.047)	(0.045)	(0.052)
Controls	No	No	No	No
Observations	1,010	1,010	1,010	1,010

Notes: Robust standard errors are shown in parentheses. All models are estimated by 2SLS and include an intercept. All dependent variables are observed in August, 2018. Work is a dummy variable that takes the value one for respondents that reports were working. Informal is a dummy variable that takes the value one for respondents that reports were working but actually they were not registered in the formal labor market. Unemployed is a dummy variable that takes the value one for respondents that reports they were not working but they are seeking for a job. Interested in education is a dummy variable that takes the value one for respondents that reports they were interested in continuing education (academic degree). The instrument for Participant is Randomized to treatment. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Appendix

Table A1. First stage and 2SLS estimates

	(1)	(2)	(3)	(4)
	Participant		Formal job	
Randomized to treatment	0.361***	0.372***		
	(0.028)	(0.028)		
Participant			-0.266***	-0.231***
			(0.091)	(0.086)
Controls	No	Yes	No	Yes
Observations	1,010	1,004	1,010	1,004

Notes: Robust standard errors are shown in parentheses. The set of controls includes all pre-treatment characteristics listed in Table 1. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table A2. Pre-treatment characteristics, by treatment assignment status

	Randomized to treatment	Randomized to control	Difference	P-value
Pre-treatment characteristics				
Age	32.510	32.510	0.000	0.991
	(9.512)	(9.345)	(0.605)	
Gender	0.572	0.571	0.001	0.989
	(0.495)	(0.495)	(0.032)	
Argentine	0.298	0.274	(0.024)	0.025
	(0.446)	(0.446)	(0.029)	
Level of education				
Complete secondary School	6.066	5.876	0.190	0.157
	(2.445)	(2.447)	(0.157)	
Incomplete high education	0.112	0.104	0.008	0.087
	(0.316)	(0.306)	(0.020)	
Complete high education	0.337	0.343	-0.006	0.633
	(0.473)	(0.475)	(0.030)	
Incomplete university	0.171	0.113	0.058	0.286
	(0.377)	(0.373)	(0.024)	
Complete university	0.081	0.113	-0.032	0.901
	(0.273)	(0.316)	(0.019)	
Has "MI BA"	0.440	0.492	-0.052	0.075
	(0.497)	(0.500)	(0.032)	

Notes: P-value corresponds to a test of differences in means by treatment assignment status.

Chapter 3*

Peron: A Synthetic Control Analysis

I. Introduction

After more than 40 years from his death, Juan Domingo Perón is still inspiring love and hate in Argentinian society. He was President of Argentina three times and he was the founder and leader of Peronism, a political movement and party that have set up the current political structure of the country. Even though there is consensus that Peronism brought a fundamental change in the life of the country, there is great controversy on its long-term impact (Page 1988; Cortés Conde 2009).

This paper studies Argentine economic performance in the twentieth century, with a focus on the long-term impact of Peronism on economic growth. Using a synthetic control approach, we find a large negative effect of Peronism on GDP per capita. Our results suggest that for the entire post-Peronism period (1946-1999), the gap between Argentine GDP per capita and its counterfactual increased by almost $23,000. This implies that GDP per capita for Argentina by the end of the century was less than a half of the GDP per capita the country would have had in the absence of Peronism.

The economic performance of Argentina in the twentieth century have attracted the attention of numerous historians, political scientists, and economists. The case of Argentina is generally regarded as exceptional. One of the most cited quotes in economics, originally attributed to Simon Kuznets, says that there are four kinds of countries: developed countries, underdeveloped countries, Japan (nobody knows why it grows), and Argentina (nobody knows why it does not grow). In the span of the twentieth

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century, Argentina went through numerous and contradictories experiences. During the first half of the twentieth century, the country performed well. By 1947, Argentina was among the wealthiest countries in the world. Argentine GDP per capita was higher than in any country of Western Europe, except UK, and it was as high as in Australia, Canada, and the US. During the second half of the century, Argentina fell behind, and the difference between Argentina and more developed countries became evident (Cortés Conde 2009). As discussed below, though there is some controversy on the real level of development of the country during the first half of the twentieth century, many authors consider that Argentina is the only country that was once developed and is now a solidly developing country (see, for example, Reyes and Sawyer 2015).

Our paper relates to a large literature studying Argentine exceptionalism. The first explanation directly challenges the exceptionalism hypothesis (for a discussion, see Glaeser, Di Tella, and Llach 2018 and Campante and Glaeser 2018). According to this view, Argentina was not truly developed during the first half of the twentieth century and it is not particularly underdeveloped now. The second explanation highlights the fundamental importance of institutional environment in shaping the path of economic growth and development (North 1990; Weingast 1997). The third explanation states that Argentina faced a series of negative external shocks to its terms of trade. The fourth explanation focuses on poor policy choices. Díaz Alejandro (1970), for example, argue that the key to understand Argentine decline is its populist tradition, which has fueled bad policies and political instability. Some authors consider these four explanations are related one with the other. For example, Galiani and Somaini (2018) highlights the interaction between institutions, shocks, and policies.

We claim that when studying the long-term impact of Peronism, it is important to differentiate the role of bad economic policies from the role of structural changes in the

country. Peronism induced structural changes far beyond particular economic policies. Di Tella and Dubra (2018) provide an example of such structural changes induced by Peronism. They analyze Perón's speeches and find that a core aspect of his rhetoric is that neither luck nor individual effort is responsible for people's economic outcomes. Instead, Perón argues that people's poverty reflects the actions of outsiders. This example illustrates how the structural changes induced by Peronism may have affected the long-term economic performance of the country.

II. Peronism

Juan Domingo Perón was a military officer whose participation in politics began as Labor Secretariat in 1943, with the Military Government of General Ramírez. As Labor Secretariat, Perón adopted several policies in favor of labor unions and workers, policies that made him very popular among the working class. Between 1944 and 1945, Perón rose to the rank of Minister of War and then he became vice-president. Over the course of the next few years, Perón's popularity grew. On October 9, 1945, the military attempted to quash Perón's political influence by arresting and sending him to Martín García Island**.** Labor unions reacted by demanding his freedom and by calling for a general strike that led to an unprecedented popular mobilization on October 17. This mobilization forced the release of Perón and his restitution to the government. By the end of 1945, the government called for democratic elections. The elections took place in February 1946, and Perón was elected president.

Peron took on the presidency on June 4, 1946. During his first presidential term (1946–52), Perón second wife, Eva Duarte, was immensely popular among the Argentine working class. Eva died in 1952, and shortly after Perón became president for a second term, serving from 1952 until 1955. On September 19, 1955, a military coup deposed Perón and forced him to spend the next 18 years in exile. When the Peronist Héctor

Cámpora became President in 1973, Perón returned to Argentina and was soon after elected President for a third time. His third wife, María Estela Martínez (elected as Vice President) succeeded him as President upon his death in 1974.

Political economy

Peron's ideology has been described as nationalist, interventionist, and populist (Luna 1993; Di Tella and Dubra 2018). The State became a regulator, a producer, and an important employer. The State took over a great number of companies. Some of the companies already existed (such as the main oil and gas exploration and production company), some of them were nationalized (such as railroads, telephone companies, and power companies), and others were created (such as airlines and iron and steel companies).

Perón adopted a series of populist policies oriented to the redistribution of income and wealth. Between 1946 and 1950, there was a strong redistribution in favor of lower-income sectors: the participation of employees in national income increased from 39% to 46%. In addition, during the first years of Peron's presidency the overvaluation of the peso kept prices on primary products low and prevented an increase in the cost of living.

Aside from redistribution policies, Peron put in place a variety of social programs in different areas, including access to free health care, a comprehensive housing program, and a generous system of social security (see Gaggero and Garro 2009). In addition, Eva Perón Foundation (run by Perón's wife) distributed considerable amounts of social assistance (see Stawski 2005). As result of these fiscal expansive policies, for the period 1946 to 1955 expenditures were consistently greater than revenues (17.5% in 1946, 16.7% in 1955), leading to a permanent primary and total deficit.

As most Latin American countries, Argentina implemented a process of industrialization with an import substitution strategy. In Peron's view, industrialization

was a mean of achieving the goals of his nationalistic and populist policy of increasing the real consumption and employment of workers (see Gerchunoff 1989).

One of the most significant policies during the first Presidency was the nationalization of foreign trade. The IAPI (Argentine Institute for the promotion of Trade) set a purchase price of cereals and meat for producers, and directly negotiated on markets abroad. The creation of the IAPI allowed the government to have the monopoly on foreign trade.

In the first quarter of 1946, most likely at the request of Perón, the Military Government of Farrell nationalized the Central Bank and all the banking system. The nationalized Central Bank became the primary and most significant instrument for financing the goals of maintaining full employment, spreading industrialization in the country, and improving workers' real wage. Between 1946 and 1948, the Central Bank increased the nominal money supply in 49%, and reserves dropped by 52%. The increase in the nominal money supply generated a rise in the inflation rate from 9.8% in 1947 to 27.4% in 1949. In this context, people shift from domestic monetary assets to foreign assets (primarily US dollars) that protect them from inflation (Cortés Conde 2009). This habit persists until today.

III. Identification strategy

We are interested in estimating the causal effect of Peronism on GDP per capita for Argentina. In non-experimental settings, the causal effect of an intervention is usually estimated using a difference-in-differences strategy. However, it is not possible to use difference-in-differences methods to evaluate the impact of country-specific historical events. In those cases researchers used to rely on a before-and-after strategy that identifies variation in the time series and usually requires very strong assumptions to be credible.

In this paper, we use the synthetic control method (SCM) developed by Abadie and Gardeazabal (2003) and extended in Abadie, Diamond, and Hainmuller (2010). Synthetic controls resemble the difference-in-differences approach in a setting where there is only one treated unit.

The synthetic control method is a weighted combination of unaffected units that simulates the characteristics of the treated unit substantially better than any untreated unit. In our context, the methodology works by assigning a weight to each country that has not experienced the historical event under study. The algorithm computes the weights in order to minimize the difference in pre-intervention outcomes between Argentina and the pool of potential comparison countries. Using the donor countries with positive weights, the algorithm constructs a counterfactual for Argentina, that is, a synthetic Argentina.

Hence, synthetic Argentina is the weighted average of the untreated countries' GDP per capita that meets the assumption of parallel trends conditional on observable characteristics prior to the event. Therefore, under the assumption that Argentina and its synthetic counterpart would continue to follow a similar trend in the absence of the event, this approach enables us to identify the impact of Peronism on the GDP per capita for Argentina.

Formally, we have data for $J + 1$ countries ($j = 1, \dots, J + 1$). Among these, we assume that the first country ($j = 1$) is the only one exposed to the intervention. The J remaining countries serve as potential controls. The "donor pool", that is, the set of potential comparisons, $j = 2, \dots, J + 1$ is a collection of units that were not affected by the event of interest. Our sample spans T periods, where T_0 are the pre-intervention periods and T_1 are the post-intervention periods ($T = T_0 + T_1$). For each unit, j, and time, t, we observe the outcome of interest (Y_{jt}). For each unit, j, we also observe a set of k

predictors, $X_{1t}, \ldots, X_{kt}$ which may include pre-intervention values (or averages) of the outcome and which are themselves unaffected by the intervention.

Then, the effect of the event of interest for $j = 1$ in period t (with $t > T_0$) is:

$$\alpha = Y_{1t} - Y_{1t}^N$$

where Y_{1t} is the outcome of interest for $j = 1$ at time t, and Y_{1t}^N is the potential outcome of interest for $j = 1$ in the absence of the event.

The challenge is to estimate Y_{1t}^N for $t > T_0$. In our case, how GDP per capita would have evolved for Argentina in the absence of the event. This is the unobserved counterfactual outcome, so we use SCM to consistently estimate it. In particular, given a set of nonnegative weights, $W = [w_2, \ldots, w_{J+1}]$, the synthetic control estimator of the potential outcome is defined as a weighted average of GDP per capita of the countries in the donor pool:

$$\hat{Y}_{1t}^N = \sum_{j=2}^{J+1} w_j Y_{jt} \qquad \forall\, t > T_0.$$

The synthetic control method selects a set of weights in such a way that the resulting synthetic control resembles Argentina before the intervention along the values of the variables $X_{1t}, \ldots, X_{kt}$. Following Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmuller (2010), we proceed to choose the weights $W^* = [w_2^*, \ldots, w_{J+1}^*]$ by minimizing the square difference between the pretreatment values of the predictors k of Argentina and the donor pool.

Once W^* is computed, the pre- and post-intervention trends in GDP per capita for the synthetic control can be obtained by calculating the corresponding weighted average for each year, using the donor countries with positive weights. The post-intervention values for the synthetic control group serve as the estimates of the counterfactual scenario for Argentina. Therefore, the estimated effect of the intervention is given by

$$\hat{\alpha} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}.$$

III. Data and sample

We use yearly data at the country level for the period 1900 to 1999. The data were collected from Maddison Project Database.

This period includes more than fifty years of post-treatment analysis, which is a reasonable period to predict and measure the effect of Peronism.

The outcome variable is GDP per capita. In the main specification, we use, as predictors, population and several averages of GDP per capita (1901-1903, 1913-1917, 1930-1939, and 1945) as predictors. We choose values that highlight the trend of GDP per capita for Argentina before the event. Following Mercado, Cicowiez, and Coremberg (2011) we consider episodes of crisis when GDP fell for at least two consecutive years and accumulated reductions not less than 4% (1913 and 1930). Similarly, we consider episodes of growth when GDP per capita grew at an average of 3.5% annual or higher for a period of eight years and with the average exceeding at least 2% to the average of the immediately previous eight-year period (1901, 1903, and 1917). We also use as predictor the level of GDP per capita in 1945, the last year before the event under study.

The donor pool includes the following countries: Australia, Austria, Belgium, Bolivia, Brazil, Canada, Chile, Colombia, Denmark, Ecuador, Finland, France, Germany, Greece, India, Italy, Japan, Mexico, Netherlands, New Zealand, Norway, Peru, Portugal, Spain, Sri Lanka, Sweden, Switzerland, United Kingdom, United States, Uruguay, and Venezuela.

IV. Results

Figure 1 displays the trends in GDP per capita in Argentina and the rest of the countries in the donor pool. As this figure suggests, the average of the rest of the countries would not provide a suitable comparison group for Argentina to study the effects of Peronism on GDP per capita. Even before 1946, the levels of the time series in GDP per

capita in Argentina and in the rest of the countries in the donor pool differed notably. The GDP per capita in Argentina is higher than the average GDP per capita in the rest of the countries in all the pre-intervention period. The trends between the two series are relatively parallel until 1950, and begin to diverge thereafter. From the mid 1970s, the average GDP per capita in the rest of the countries is higher than the GDP per capita in Argentina.

Table 1 displays the weights for each donor country in synthetic Argentina from the synthetic control estimation. The main specification includes, as predictors, countries' population and the average GDP per capita for four periods (1901-1903, 1913-1917, 1930-1939, and 1945). In the main specification, the estimated weights indicate that GDP per capita in Argentina in the pre-intervention period is best reproduced by a combination of Australia, Belgium, Canada, Denmark, Finland, New Zealand, Sweden, Switzerland, United Kingdom, United States, and Uruguay. All other countries in the donor pool have zero weights. That is, the algorithm constructs a synthetic Argentina from a combination of some emerging economies with similar development indicators (Australia, Canada, and New Zealand), some leading economies with more growth potential (Belgium, Switzerland, United Kingdom, and United States), others European economies (Denmark, Finland, and Sweden), and the neighboring Uruguay.

We use the estimated weights to obtain synthetic Argentina and compare it to real Argentina in pre-treatment characteristics. Table 2 shows that synthetic Argentina is very similar to real Argentina in all covariates used in the estimation. By contrast, the simple average of all countries would not provide a suitable comparison group for Argentina.

Figure 2a displays GDP per capita for Argentina and its synthetic counterpart during the period 1900 to 1999. In contrast to the average GDP per capita for the rest of the countries, GDP per capita in the synthetic Argentina very closely tracks the trajectory

of this variable in Argentina for the entire pre-intervention period. Combined with the high degree of balance on all predictors (Table 1), this suggests that synthetic Argentina provides a good approximation to the counterfactual, that is, to the GDP per capita that Argentina would have had in the period 1947 to 1999 in the absence of Peronism.

Our estimate of the effect of Peronism is the difference between GDP per capita in Argentina and in its synthetic version after 1946. Around 1952 the two lines begin to diverge noticeably, suggesting a large negative effect of Peronism on GDP per capita. Indeed, 1952 is an important year in Argentine politics since Eva Perón died and Juan Domingo Perón started his second presidential term.

Figure 2b plots the yearly gaps in GDP per capita between Argentina and its synthetic counterpart. The magnitude of the estimated impact of Peronism in Figure 2b is substantial and gets larger over time. Our results suggest that for the entire 1946–1999 period the gap in GDP per capita was increased by almost $23,000, that is, the GDP per capita for synthetic Argentina is more than two times the GDP per capita for the real Argentina.

V. Placebo and robustness tests

To confirm that the gap shown in Figure 2b is the true causal effect, we need to conduct inference and provide evidence of the validity of synthetic Argentina as a counterfactual.

By systematizing the process of estimating the counterfactual of interest, the synthetic-control method enables us to conduct a series of robustness checks and placebo tests. As a first robustness check, we show that results remain unchanged to alternative specifications. As a second robustness check, we perform the leave-one-out test. Finally, we report the in-space placebo and the in-time placebo.

Alternative specifications

The main conclusions of an empirical study should display some level of robustness with respect to changes in the study design. An important way the design of a synthetic control study may influence results is the choice of predictors of the outcome variable. To address this potential concern, we perform five alternative specifications.

The first alternative model is the same as the main model but without including population as a predictor. The second model includes, as predictors, population, GDP per capita in 1900, 1910, 1920, 1930, and 1945. The third model uses, as predictors, population and GDP per capita every five years between 1900 and 1945. The fourth model includes, as predictors, population, GDP per capita for episodes of growth (1901, 1903, and 1917), episodes of crisis (1913 and 1930), and the average of GDP per capita for the period 1938-1945. Finally, the fifth model is the same as the previous one, but adding surface area as additional predictor.

As reported in Figure 3, the main conclusions are robust to the alternative specifications.

Leave-one-out test

Figure 4 reports the results of a leave-one-out test. In this test, we take from the sample one-at-a-time each of the countries that contribute to synthetic Argentina (that is, we omit in each iteration one of the countries that received a positive weight). All leave-one-out estimates closely track the series of GDP per capita for Argentina before 1946.

The main conclusion of this leave-out-one exercise is that the negative estimate of Peronism on GDP per capita is robust to the exclusion of any particular country.

In-space placebo

Abadie et al. (2010) propose a mode of inference for the synthetic control framework based on permutation methods. As in classical permutation tests, the intervention was reassigned to countries that were not exposed to the intervention. That

is, we iteratively apply the synthetic-control method to every other control country, shifting Argentina to the donor pool. Ideally, the estimated effect in Argentina should be larger than the estimated effect for any other country not exposed to the intervention. We construct the permutation distribution by pooling the effect estimated for Argentina together with placebo effects estimated for all the countries in the donor pool. The effect of the intervention is statistically significant when its magnitude is extreme relative to the permutation distribution. As shown in Figure 5, this is exactly the case. Compared against the gaps for the other untreated countries, the gap between Argentina and synthetic Argentina appears highly unusual. In fact, the negative effect in Argentina is by far the largest of all since 1952.

In-time placebo

Figure 6 shows the result of estimating the effect of Peronism with the intervention backdated to several years before the intervention (1911 to 1945). Two important features of the results are as follows. First, the synthetic control estimator closely tracks GDP per capita for Argentina in the period before the start of the actual intervention (1911-1945, 1912-1945, 1913-1945, etc.). This is the in-time placebo test described in Abadie et al. (2015) and Abadie (2019). The absence of estimated effects prior to the intervention provides credibility of the synthetic control estimator, since it shows that the synthetic control reproduces the trajectory of GDP per capita for Argentina before the intervention occurs. Second, a gap between GDP per capita for Argentina and its synthetic control counterpart appears around the same period as in the main results. This is the case even when the procedure uses no information on the timing of the actual intervention. The shape and direction of the gap in Figure 6 is similar to that of Figure 2a, though lower in magnitude. The fact that the estimated effect appears shortly after 1946 even when the

intervention is artificially backdated in the data provides credibility to the synthetic control estimator.

VI. Other outcomes

Development is a wider concept than just GDP per capita growth. Economic development also implies progressive changes in certain important variables, which determine well-being of the people, such as health and education. Therefore, it is important to analyze a potential trade-off between the decline in GDP growth in Argentina after Peronism and an improvement on other dimensions of development. From these multiple dimensions of development, we were only able to collect data on life expectancy and child mortality.

We test the trade-off hypothesis by exploring the impact of Peronism on life expectancy and child mortality, again using a synthetic control approach. As observed in figures 7 and 8, we find no evidence of a trade-off. Figure 7 plots the trends in child mortality between Argentina and synthetic Argentina. The difference between Argentina and its counterfactual is not significant. Figure 8 plots the trends in life expectancy between Argentina and synthetic Argentina. If anything, Argentina performed worse than synthetic Argentina. Overall, there is no evidence of a trade-off between the poor performance of Argentina in terms of GDP per capita and a good performance in these two other dimensions of development.

VII. Conclusion and discussion

Our paper contributes to a large literature studying Argentine exceptionalism. We provide evidence on the role that Peronism had on Argentine economic performance in the twentieth century. Using a synthetic control approach, we report a large negative effect of Peronism on GDP per capita. Our results suggest that Argentine GDP per capita

by the end of the century was less than a half of the GDP per capita the country would have had in the absence of Peronism.

Development is a wider concept than growth, and includes other dimensions that determine the well-being of the people. It is plausible, therefore, the presence of a trade-off between the decline in GDP growth in Argentina after Peronism and an improvement on other dimensions of development. We took some steps in this direction by exploring the impact of Peronism on life expectancy and child mortality, and we find no support to this hypothesis.

Of course, Peronism may have influenced other dimensions of development not covered in this study. Therefore, more research is needed in order to draw solid conclusions about the long-term impact of Peronism on development.

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Table 1. Countries weights in synthetic Argentina

	Weights
Australia	0.008
Austria	0
Belgium	0.006
Bolivia	0
Brazil	0
Canada	0.001
Chile	0
Colombia	0
Denmark	0.368
Ecuador	0
Finland	0.001
France	0
Germany	0
Greece	0
India	0
Italy	0
Japan	0
Mexico	0
Netherlands	0
New Zealand	0.017
Norway	0
Peru	0
Portugal	0
Spain	0
Sri Lanka	0
Sweden	0.001
Switzerland	0.528
UK	0.001
United States	0.063
Uruguay	0.002
Venezuela	0

Table 2. GDP per capita predictor means before 1946

	Argentina	Synthetic Argentina	Average other countries
GDP per capita (1901-1903)	4,904	4,898	3,139
GDP per capita (1913-1917)	5,558	5,553	3,635
GDP per capita (1930-1939)	6,678	6,672	4,255
GDP per capita 1945	7,462	7,462	4,763
Population (1900-1945)	9,882	10,409	27,598

Table 3. Countries weights in the synthetic Argentina for alternative specifications

	Alternative model 1	Alternative model 2	Alternative model 3	Alternative model 4	Alternative model 5
Australia	0.026	0.104	0.141	0.102	0.061
Austria	0.005	0	0	0	0
Belgium	0.125	0.264	0.289	0.094	0
Bolivia	0.002	0	0	0	0
Brazil	0.002	0	0	0	0
Canada	0.003	0.32	0	0	0
Chile	0.003	0	0	0	0
Colombia	0.002	0	0	0	0
Denmark	0.471	0	0.177	0.258	0
Ecuador	0.002	0	0	0	0
Finland	0.003	0	0	0	0
France	0.006	0	0	0.401	0.624
Germany	0.155	0	0	0	0
Greece	0.007	0	0.024	0	0
India	0.002	0	0	0	0
Italy	0.003	0	0	0	0
Japan	0.003	0	0	0	0
Mexico	0.002	0	0	0	0
Netherlands	0	0.313	0.169	0	0
New Zealand	0.011	0	0	0	0
Norway	0.004	0	0	0	0
Peru	0.002	0	0	0	0
Portugal	0.003	0	0	0	0
Spain	0.006	0	0	0	0
Sri Lanka	0.002	0	0	0	0
Sweden	0.004	0	0	0	0
Switzerland	0.015	0	0	0	0
UK	0.02	0	0	0	0
United States	0.103	0	0.134	0.145	0.275
Uruguay	0.007	0	0.067	0	0.04
Venezuela	0.002	0	0	0	0

Notes: The first alternative model includes, as predictors, the average GDP per capita for four periods (1901-1903, 1913-1917, 1930-1939, and 1945). The second alternative model includes, as predictors, population, GDP per capita in 1900, 1910, 1920, 1930, and 1945. The third alternative model uses, as predictors, population and GDP per capita every five years between 1900 and 1945. The fourth alternative model includes, as predictors, population, GDP per capita for episodes of growth (1901, 1903, and 1917), episodes of crisis (1913 and 1930), and the average of GDP per capita for the period 1938-1945. Finally, the fifth alternative model is the same as the previous one, but adding surface area as additional predictor.

Figure 1. Trends in GDP per capita: Argentina vs. the rest of countries

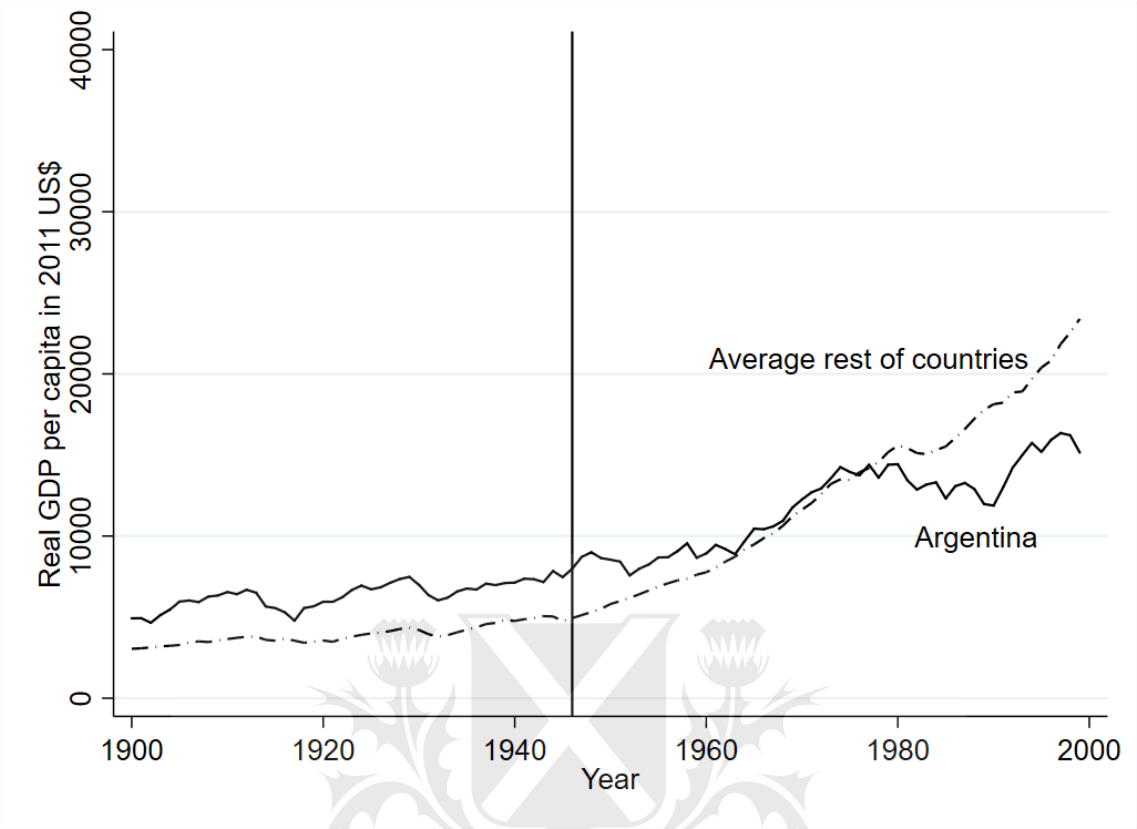


Figure 2a. Trends in GDP per capita: Argentina vs. synthetic Argentina

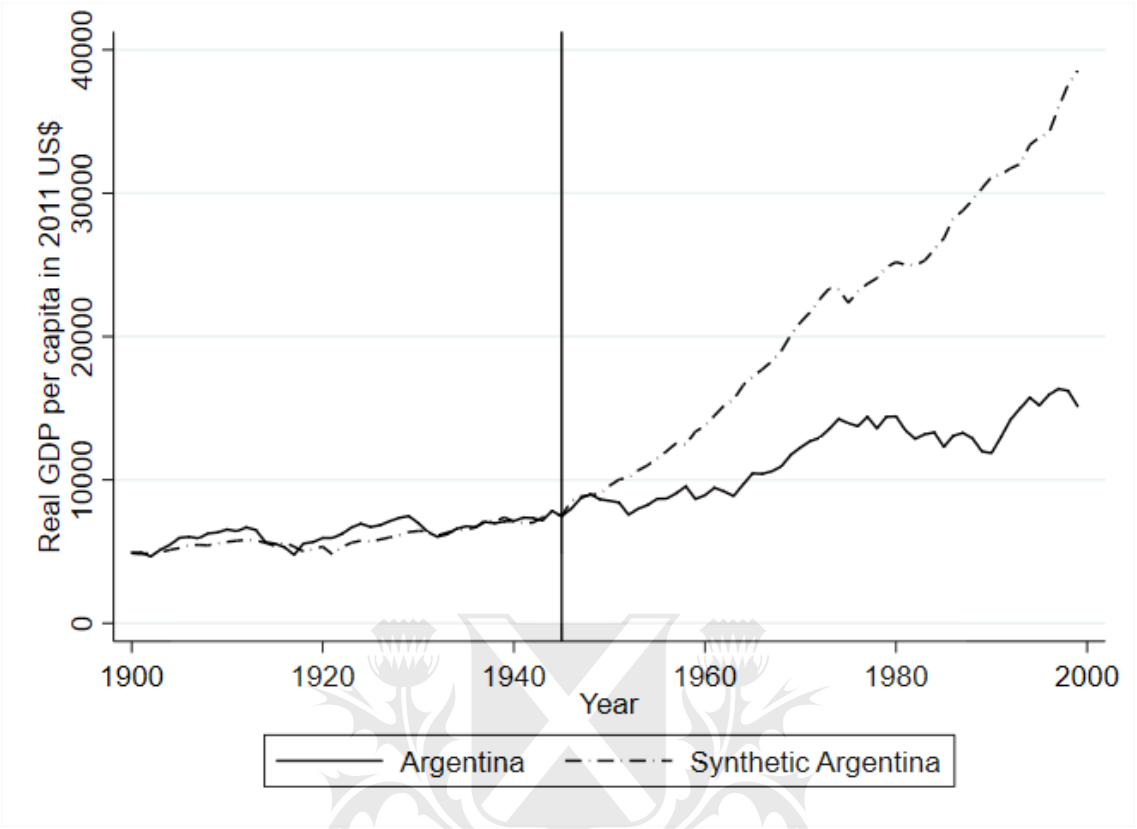


Figure 2b. Gap between Argentina and synthetic Argentina

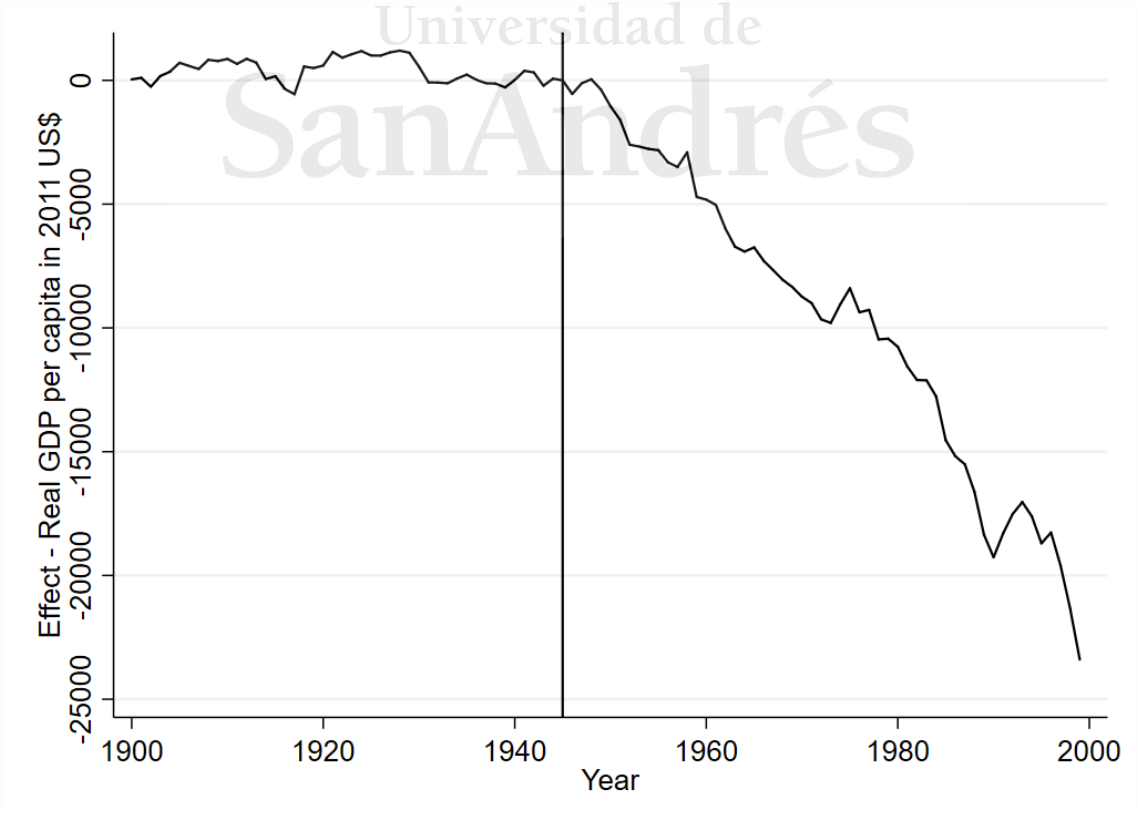
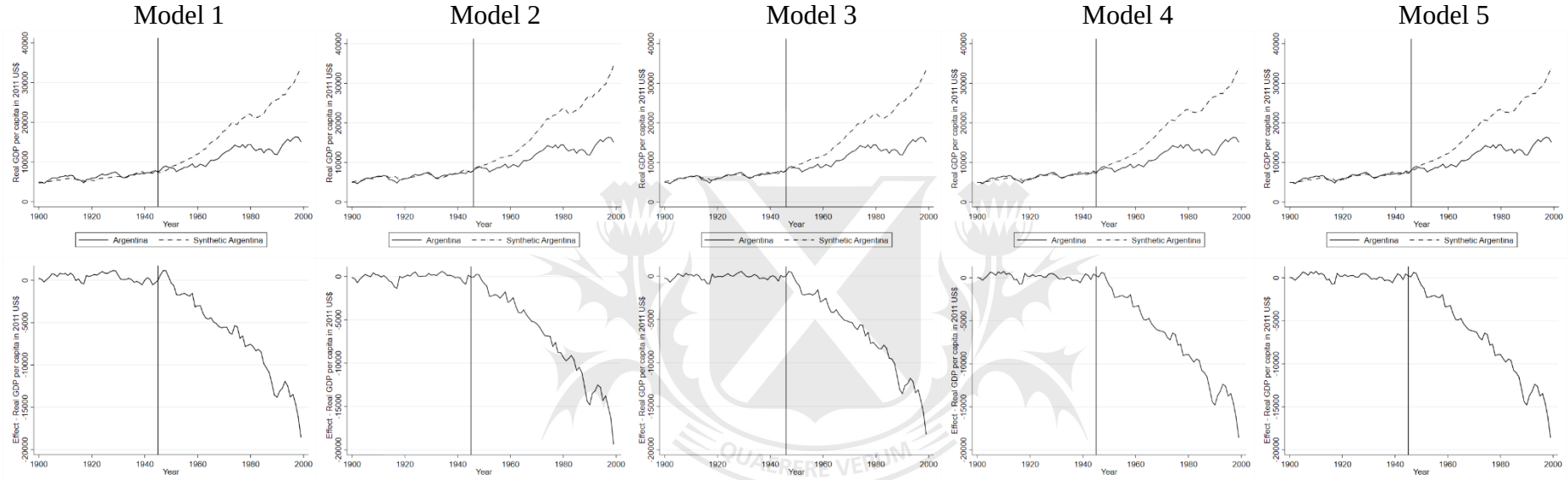


Figure 3. Robustness checks: Argentina vs. synthetic Argentina using alternative models



Notes: The first alternative model includes, as predictors, the average GDP per capita for four periods (1901-1903, 1913-1917, 1930-1939, and 1945). The second alternative model includes, as predictors, population, GDP per capita in 1900, 1910, 1920, 1930, and 1945. The third alternative model uses, as predictors, population and GDP per capita every five years between 1900 and 1945. The fourth alternative model includes, as predictors, population, GDP per capita for episodes of growth (1901, 1903, and 1917), episodes of crisis (1913 and 1930), and the average of GDP per capita for the period 1938-1945. Finally, the fifth alternative model is the same as the previous one, but adding surface area as additional predictor.

Figure 4. Leave-one-out test

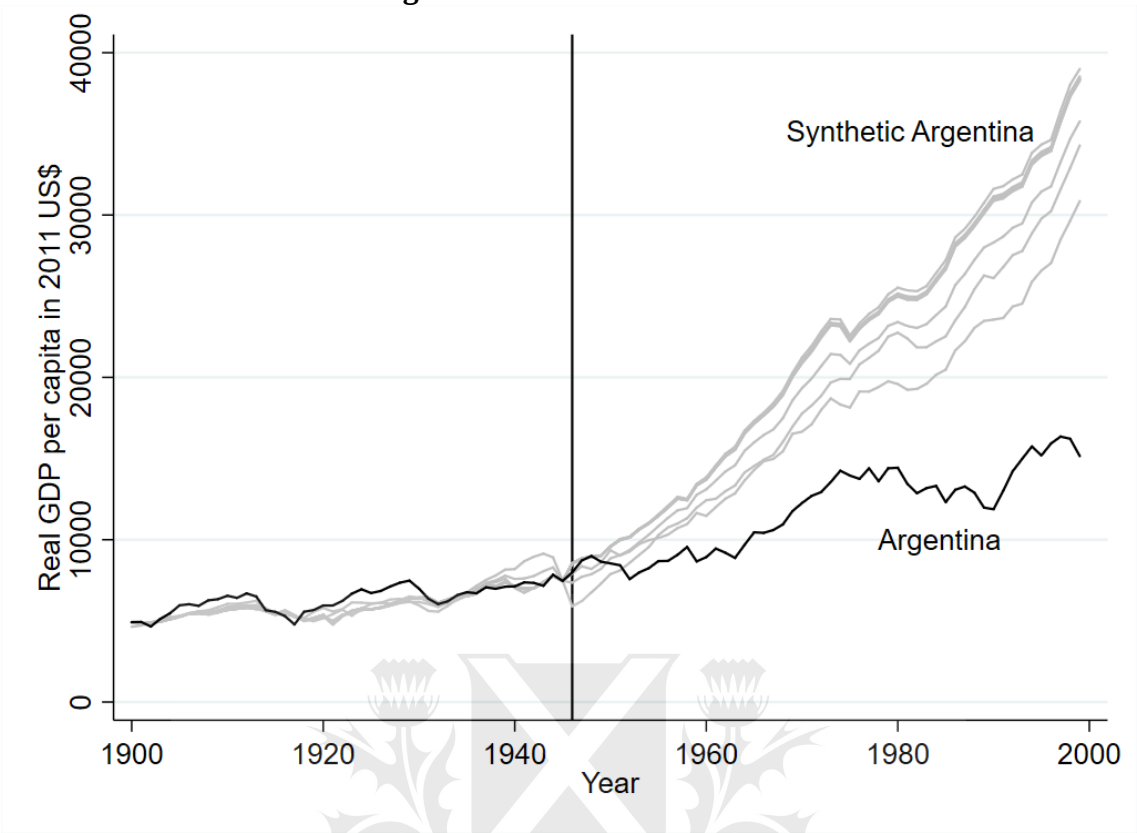


Figure 5. In-space placebo

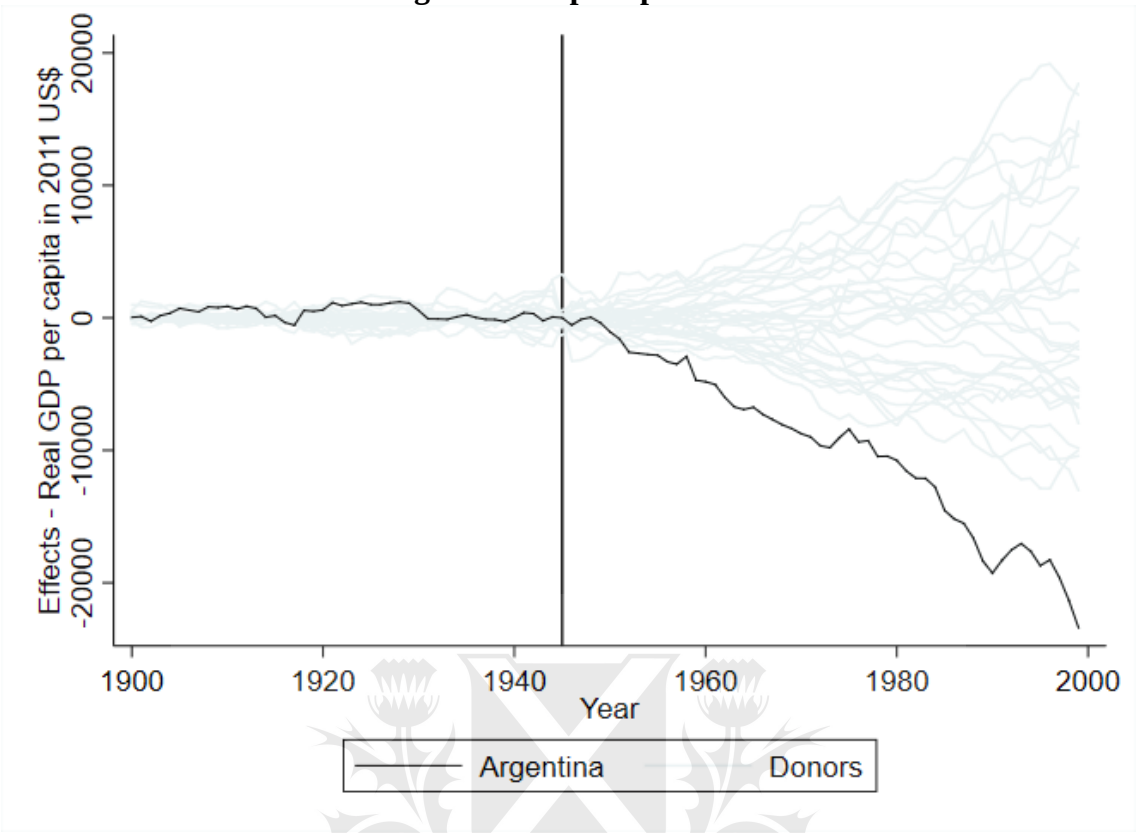


Figure 6. In-time placebo

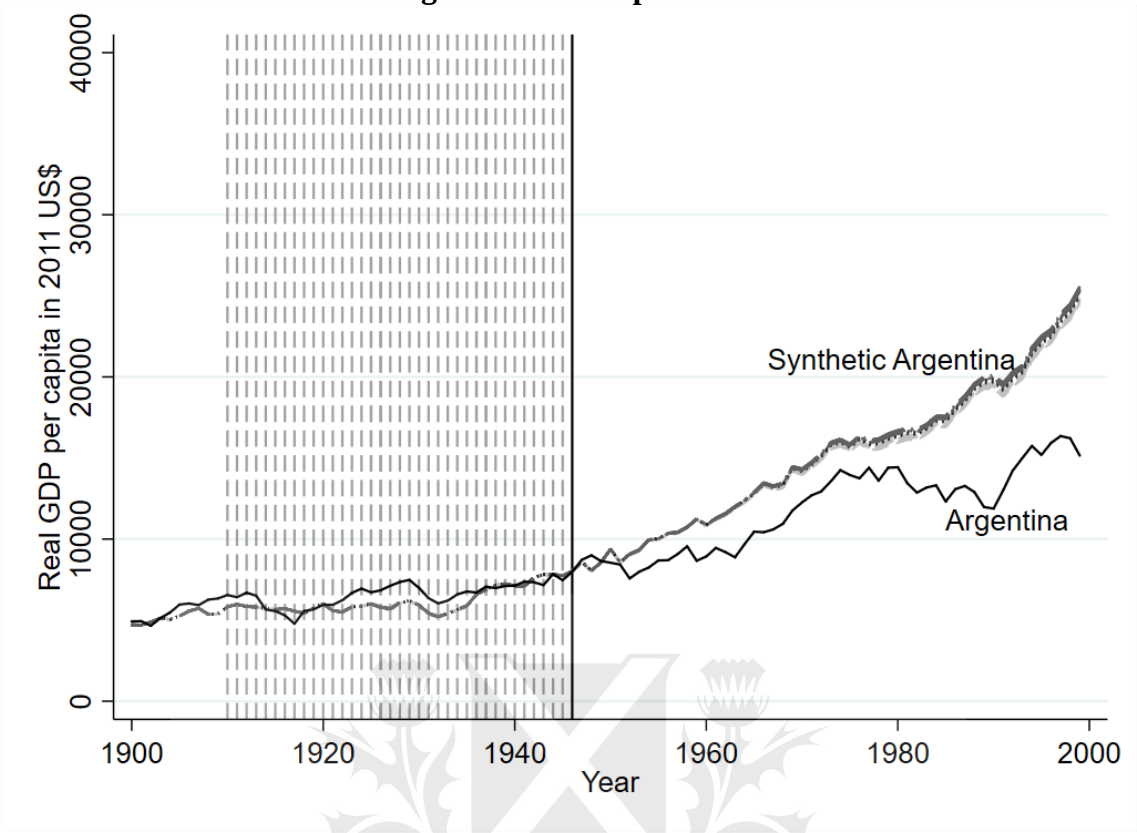


Figure 7. Trends in life expectancy: Argentina vs. synthetic Argentina

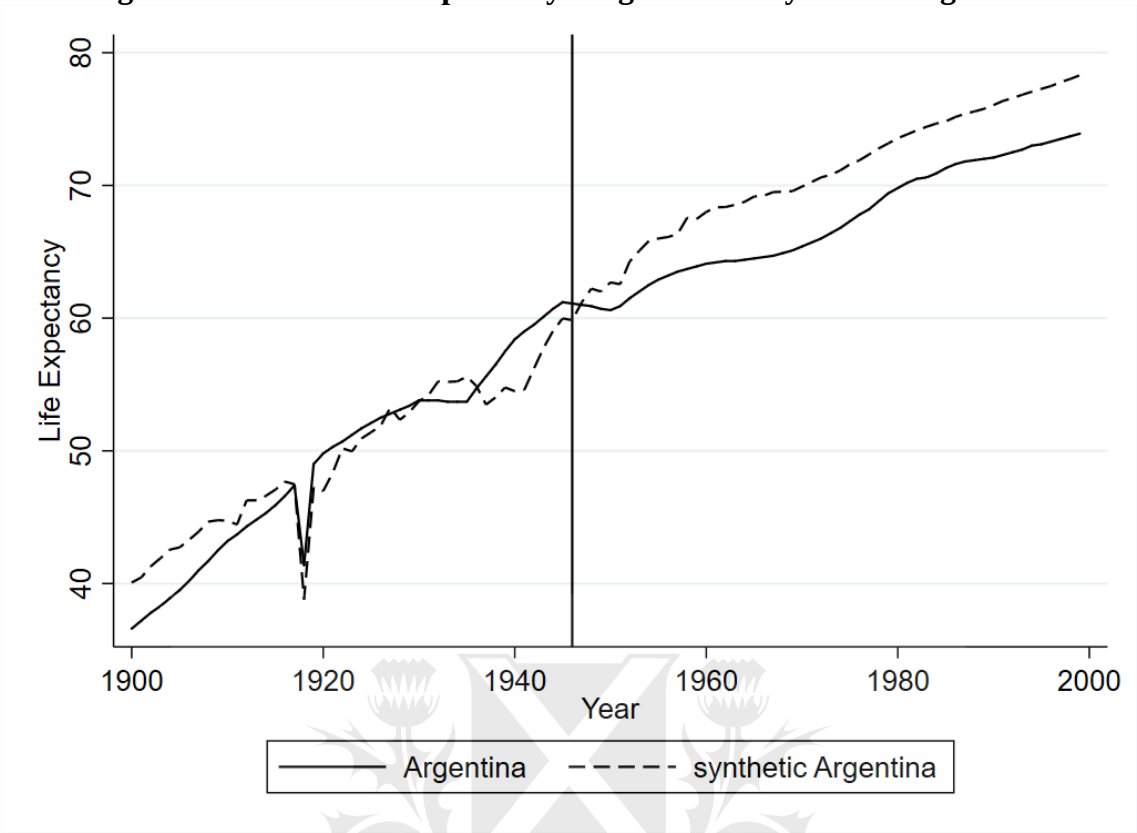


Figure 8. Trends in child mortality: Argentina vs. synthetic Argentina