Compulsory Education in Argentina: Effects on Crime

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September 16, 2018

Abstract

Researchers have long been interested in the links between education and crime but evaluating this relationship causally is challenging. Evidence from developed countries indicates a negative relationship. In this paper, I analyze this relationship using data of Argentina by exploiting a legal change which lengthened compulsory education. I use difference-in-differences and a synthetic control method to compare trends in early adopting areas to those in later adopting areas. I find a significant decrease in crime rates in Buenos Aires and Córdoba, the two provinces which implemented the law in 1996, as compared to the controls which adopted later. Evidence suggests these impacts may be due to incapacitation effect.

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I would like to thank Emily Oster, Anna Aizer, Bryce Steinberg, Leopoldo Tornarolli, and Cecilia Velazquez for their helpful conversations and comments. I am grateful to seminar participants at the Applied Microeconomics Seminar at Brown University for many helpful comments. All errors and opinions are my own.

1 Introduction

The analysis of the impact of an educational reform on crime is relevant because it lies on several facts. The extension of the period of compulsory schooling for teenagers forces students to stay in the educational system for more years which limits their available time to engage in risky activities.[1] Additionally, there is evidence (Damm and Dustmann, 2014) showing that young people are very responsive to social interactions and exposure to risky and illegal activities could influence their subsequent criminal behavior. Recent evidence for Peru (Sviatschi, 2018) indicates that this channel is important: individuals exposed to illegal labor markets in their early teens are 30 percent more likely to be incarcerated for violent crimes as adults. In that sense, if the extension of the period of compulsory education generates changes in students, their entire potential criminal path could be affected. In the literature of education and crime, several authors have found that reforms that increased years of compulsory education in developed countries have a negative effect on individual participation in criminal activities (see for example, Lochner and Moretti, 2004 for the United States; Machin, Marie, and Vujic, 2010 for the United Kingdom; and Hjalmarsson, Holmlund and Lindquist, 2015 for Sweden). However, causal evidence of the link between an educational reform and crime is still scarce for developing countries (Nishijima and Pal, 2017).

This paper offers evidence on this link for Argentina, by exploiting an educational reform that increased compulsory years of schooling. Even though this analysis is not new in the literature, this study contributes by providing evidence on this relationship for a developing country where the enforcement of law, in general, is weak. In 1993, the National Congress of Argentina passed a law, the Federal Law of Education (Ley Federal de Educación, LFE),[2] that implied a deep reform in the educational system of the country. The most important change introduced was the extension of the period of compulsory education to the last year of pre-primary education (which affected attendance rates of 5-year-old children) and to the first 2 years of high school education (which affected attendance rates of teenagers aged 13 to 15 years). As a result, the period of compulsory education was extended from 7 to 10 years.

To estimate the effect of the reform, I exploit the fact that the LFE was implemented at different

[1] See Jacob and Legfren (2003) and Dalh and DellaVigna (2009) for other examples of "incapacitation effect".

[2] It was replaced in 2006 by a new education law ("Ley Nacional de Educación") which made compulsory all secondary education.

times across Argentinean provinces along the period 1996-2000 by using a difference-in-differences framework and synthetic control method. The intuition is that differences among provinces in the timing of application of the law implied that individuals of the same age but living in different provinces had different levels of exposure, and this could have a differential effect between provinces in crime rates. First, I compare reported crime rates of provinces that fully implemented the reform in 1996 and 1997 (highly exposed) and provinces that implemented the reform in a late way or never implemented it, before and after the implementation. Second, to provide additional evidence, I compare trends of the two most highly exposed provinces with a comparison unit for each one constructed using the synthetic control method (SCM) with a donor pool composed by the provinces the did not fully implement the reform in 1996 or 1997. This method allows me to create a better comparison group for the first provinces that implemented the law by exploiting the time series data and the existence of many potential control groups.

I use a panel dataset at the provincial level with demographic data and reported crime rates to analyze the impacts of the law on crime. These crime rates include all types of crime committed by identified and unidentified individuals that were reported by victims in public security agencies. Although there might be some concerns on the representativeness of this data on juvenile crime (which would be the ideal data to analyze the impact of the LFE), data on juvenile crime is not available because of the legislation on juvenile crime, but data used in this paper is still representative because it includes all types of victims' reports who are not always able to identify the age of their offenders.

My results indicate that the extension in the period of compulsory education in Argentina had a negative impact reported crime rates. Estimates in the difference-in-difference approach indicate a decrease in total crimes of 10 percent in highly exposed provinces in the post-treatment period, although these results are non-significant. For that reason, I provide additional evidence by using the SCM on Buenos Aires and Cordoba, provinces that fully applied the law in 1996, the first year of implementation. In these provinces, there was a reduction in total crime rates of 15 and 12 percent, with respect to their synthetic control units in the post-treatment period. Given some usual concerns on this non-experimental methodology, I perform standard robustness checks that provide support to my main findings.

I present estimations to test the presence of two mechanisms that could explain my results: incapacitation and human capital effects. The incapacitation effect is generated by the fact that students should stay more time in school, limiting their available time to commit crimes (Black, Devereux and Salvanes, 2008; Jacob and Lefgren, 2003) and the human capital effect could be present because more years of compulsory education imply higher levels of educational achievement, then the law would imply better wage prospects and higher opportunity cost of being tied to criminal activities (Lochner and Moretti, 2004), and as a result, lower engagement on crime. To test the incapacitation effect, I estimate the evolution of the attendance rates for cohorts directly affected by the reform, comparing them to the attendance rates in the comparison group. I also estimate the probability of imprisonment for the age groups that, after the implementation of the law, are forced to stay more years in school. To test the human capital effect, I estimate the probability of being in prison by educational attainment and cohort of birth. The results suggest the presence of incapacitation effect, and I cannot rule out human capital effect.

From a policy perspective, it is very important for Argentina to reduce the participation of teenagers in crime, given that criminal groups consider teenagers as a valuable tool. That is mostly explained by the treatment of adolescents in the justice system: people younger than 16 years old cannot be prosecuted and the prosecution of criminals aged 16-18 years old depends on the discretional decision of each judge. Although I do not estimate the precise economic contribution of the reform, my results together with evidence reporting that the economic cost of total crime in Argentina represents 3 percent of the GDP each year (Jaitman, 2017), suggest that the impact of the educational reforms on crime rates (both, in the short run and in the long run) should not be neglected in its cost-benefit analysis.

The paper proceeds as follows: Section 2 presents a background and the institutional setting where the LFE was applied. Section 3 describes datasets included in the analysis. Section 4 explains the empirical strategy. Section 5 shows results, analyzes potential mechanisms, and includes robustness checks. Section 6 concludes.

2 Background and Institutional Setting

This section is intended to present information that is relevant to understand how the implementation of the law could have affected crime rates and why is important to study the relationship between education and crime in Argentina. First, I will describe the main changes that the LFE introduced in the national educational system and how the timing and the intensity of implementation varied across provinces. Next, I will analyze crime trends in Argentina, showing the relevance of studying the potential effect of the LFE on crime.

2.1 The Federal Education Law

The LFE, which was passed in 1993, introduced several changes in the educational system of Argentina, the most important was the extension of the compulsory education period. More specifically, the LFE determined that the total years of compulsory education would increase from 7 years (the duration of primary education in Argentina) to 10 years (1 year of pre-primary education, 7 years of primary education and 2 years of secondary education).[3]

Although the law was passed by the National Congress, the provincial governments were in charge of deciding how and when to implement the law in their territories. This seemingly contradictory situation is explained by a simple fact: in Argentina, primary and secondary education are decentralized at the provincial level, with the National Ministry of Education in charge of providing basic national guidelines. In that sense, the provinces had to cover the costs associated with the application of the law (the teachers and the infrastructure needed to absorb a higher number of students). As a result of that decentralized system, there were substantial differences across provinces in the timing and the intensity of implementation of the law.

Table 1 contains information on the timing and the intensity of implementation. Regarding the timing, the first 2 provinces in implementing the law were Buenos Aires and Córdoba, in 1996. Most provinces implemented the LFE in the next 2 years, and a few of them in 1999 and 2000. The reform was never put into practice in 2 districts, CABA (Ciudad Autónoma de Buenos Aires) and Río Negro. The differences in the intensity of implementation derives from the way in which the

[3]The equivalences between the old and the new system are shown in Table 1 Appendix 1.

reform was implemented in each province: some provinces decided to increase compulsory education from 7 to 10 years in all schools (full implementation), while other provinces decided to increase the years of compulsory education by 1 year at a time and/or did not apply the reform in all schools at the same time (gradual implementation).[4]

2.2 Crime Trends in Argentina

While crime has always been a major concern in most Latin American countries, Argentina was historically a country with very low crime rates, however, crime rates started to increase in the 1990s, following Latin American trends (Di Tella and Schargrodsky, 2013). Figure 1 exhibits the evolution of crimes rates in Argentina in the period 1992-2008. The rates of different crimes have a positive trend, but it can be noticed that there is a spike in total crime rates in 2001 and 2002. During that period Argentina faced a deep institutional and economic crisis that led to extremely high levels of poverty and unemployment.[5] Even when crime rates went down with the recovery of economic activity after 2003, those rates plateaued at a very high level after 2005. According to the results of the National Victimization Survey (INDEC) of 2016, 27.5 percent of Argentinean households were victims of a crime during the 12 months before the survey and Jaitman (2017) estimated that the annual cost of crime in Argentina is a 3 percent of the GDP.[6]

[4] There is no detailed information about how the intensity of the reform varied across provinces, for that reason I am not exploiting that source of variation much further.

[5] In Figure 1 Appendix 1, it can be observed how was the evolution of growth rate of poverty by province, to show the crisis affected the entire country.

[6] This estimation includes public expenditure on police, justice and prisons, private expenditure on security, victimization social cost that comprises not perceived income by victims of crimes and lost profit of inmates, Jaitman (2017).

3 Data

In my main analysis, I use several data sources covering the period 1991-2008[7] to create a panel with aggregated information at the provincial level. First, I use aggregated reported crimes rates at the provincial level (committed in each year). Second, I use data on socio-economic variables relevant to the analysis. The next subsections describe the content of both datasets.

Crime Data

The data on crime rates (per 100,000 inhabitants, aggregated at the provincial level per year) was obtained from annual reports of the National Criminal Information System (SNIC, Ministry of Justice and Human Rights), includes only reported crimes and it ranges from 1991 to 2008. It is important to note that this data is based on victims' reports in all type of police stations (local, federal, etc.) and I cannot identify the age of the perpetrator. I am considering total crime rates which include the following crimes: property crimes (burglary and attempted burglary, aggravated robbery and attempted robbery resulting in injuries and/or death, theft and attempted theft, and other property crimes), crimes against people (attempted murder, wrongful death in a car accident, homicides caused by other events, grievous bodily harm, negligent injury in traffic accidents and other crimes against people), intentional homicide, crimes against sexual integrity and honor, crimes against freedom, crimes against the state and the community, crimes against the civil status, drug-related crimes, and other offenses included on under special laws.[8]

As I mentioned above, in Argentina young people (less than 16 years) cannot be prosecuted. This could cast some doubts regarding the analyzed data in this paper (total reported crimes) due to the fact that people could not report crimes if they were victims of younger offenders. It can be argued that those crimes included in the data are representative of the juvenile crime for several reasons. First, most of these crimes are committed by non-identified individuals so they include

[7] The analysis is limited to that period mainly for two reasons. First, previous years of crime data are not available because not all the provinces submitted the reports to the national agency in charge, and after 2008, the national government decided not to continue publishing such information. There were some controversies on that decision because it could reflect a worsening in the statistics. Up to now, the current government was not able to reconstruct the series of crime for those missing years. Second, in 2006 the national congress passed a new educational reform that made compulsory all years of secondary school. Given that posterior years of crime data could reflect the impact of the new law on crime rates it seems reasonable not to include that period.

[8] The provinces of Salta and Jujuy were excluded from the analysis because they do not report crimes in some consecutive years and their trends were not clear to extrapolate such values.

juveniles even if they cannot be prosecuted. Second, no prosecution does not mean victims don't report their offenders, if they are identified they are incarcerated until a judge decides their release, and victims can reclaim their belongings (in case of a property crime). Also, there is no official data about the percentage of total crimes committed by juveniles, but a subsample of identified offenders called "Sistema de Alerta Temprana" SAT (Early Alert System),[9] indicates that almost 26 and 28 percent of robberies in 2003 and 2008 respectively, were committed by individuals under 18 years old.

In addition, I use the latest national census collected in 2010 by the National Institute of Statistics (INDEC), where there is a module that about collective dwellings. The surveyor must select the type of dwelling and prison is one category, then each person who spent the night there (before the census day) were asked demographic questions such as educational attainment. An important remark is that the census does not collect information about the reasons for which a person is in prison at that time (or in particular the type of conviction), which could be a limitation of this data. I use this information to compute the probability of imprisonment, mainly to motivate part of my analysis. I am not able to exploit this dataset further given that I do not have access to other demographic variables in the census that would allow me to provide a cohort of birth analysis.

Provincial Level Data

In order to control for differences at the provincial level, I construct a panel (province/year) with information on the evolution of spending obtained from the National Direction of Macroeconomic Policy (Dirección Nacional de Política Macroeconómica) of the Ministry of Economy and Public Finance (MECON), it ranges from 1993 to 2008. In particular, I use information on per capita expenditure on security and I compute the growth rate in that variable to control for level differences among provinces. Projections on population growth for each year, to compute per capita terms, comes from the National Institute of Statistics (INDEC).

Also, I gather information on demographic characteristics by province (available from 1992 to 2008), from the Socio-Economic Databases for Latin America and the Caribbean (SEDLAC),[10]

[9] This system uses information from each Police Department and Security Forces about homicides, and crimes against property. There are crimes included in SNIC but not informed to SAT, for that reason the crimes in SAT are lower than SNIC.

[10] A CEDLAS (Universidad Nacional de La Plata) and the World Bank's LAC Poverty Group (LCSPP) project.

I compute school attendance, gender ratio (men per 100 women), poverty levels, and an income inequality measure (Gini index). I use this data to motivate my analysis and also, to include demographic characteristics in my estimations in Section 5.

4 Empirical Strategy

In this section, I explain in more detail the methodology used to estimate the impact of increasing compulsory years of education on crime. As it was already discussed, the LFE was not implemented at the same time throughout the country and my identification strategies rely on those differences among provinces.

More specifically, the units of analysis will be the Argentinean provinces. They are classified in two groups according to when and how the LFE was implemented: highly exposed/treated provinces (those provinces in which the LFE was fully implemented in 1996 or 1997) and less exposed/comparison provinces (those provinces that did not fully implement the reform or did it after 1997).[11] The identification strategy exploits the fact that the differences in the timing and the intensity of application of the LFE implied that individuals of the same age but living in different provinces had a different level of exposure to the law. For example, at the beginning of 1997 a 13-year-old child living in Buenos Aires or Córdoba was exposed to the law for a whole year, while children living in any other province were not exposed to the law at all.

After classifying the provinces in those two groups, the impact of the LFE on crime is estimated first by comparing the post-treatment outcomes of the two groups with a difference-in-differences approach. This strategy removes biases that could be explained by permanent differences between those groups, also it considers biases that could be the result of trends (Imbens and Wooldridge, 2007). In this case, the assumption is that crime rate trends would have been the same in both groups in the absence of the reform (Pischke, 2005). Although neither the text of the law nor the context in which it was enacted provide any evidence suggesting that the provinces decided the timing and the intensity of adoption of the law considering their levels of crime, I analyzed if there was a correlation between crime levels and the timing and the intensity of implementation of the law across provinces. Table 3 in Appendix 1 shows the means in some socio-economic variables and

[11] The provinces in each group can be observed in Table 2 in Appendix 1.

in the crime rates for each group of provinces, and the difference between the two groups in the pre-treatment period. It can be observed that school attendance rates for the cohort affected by the reform (13 to 15 year-old teenagers) are lower for the highly exposed/treated group, and homicide crime rates are higher for the same group. It is important to highlight that provinces implementing the reform required to devote more financial resources to education. In that sense, if the timing of implementation of the LFE were related to factors that in turn affect the variable of interest, such as poverty or unemployment, this could result in an invalid identification strategy since the LFE would be endogenous (Alzúa et al. 2015).[12]

To provide more precise estimations with the available data, I use an additional methodology that does not rely on an exogenous source of variation that determines the treatment assignment: the synthetic control method. This method is useful to create a better comparison group for the highly exposed/treated provinces. The basic idea is to build the weighted average (synthetic control) of less exposed/comparison provinces that mimics the characteristics (trend and predetermined covariates) of each highly exposed/treated province over time, prior to the implementation of the LFE. The method computes the effect of the treatment on post-intervention outcomes by comparing the results of the treated unit with the results of the synthetic control unit. In this case, the synthetic control unit of each treated unit is constructed by using weights that reproduce the rates of total crimes that were reported in the unit of interest. The treated units are the two provinces that adopted the law in 1996 (first year of implementation): Buenos Aires and Córdoba. The donor pool comprises all those provinces less (or never) exposed to the reform.

Abadie, Diamond, and Hainmueller (2010) discuss requirements and provide recommendations to construct an adequate donor pool. A first requirement is that units included in the donor pool should not be affected by the intervention under study. In the case of the LFE in Argentina, all provinces but two implemented it at some point in time. In other words, most of the units in the donor pool were (at least partially) exposed to the treatment a few years after the implementation in Buenos Aires and Córdoba. Although this implies that the donor pool is not perfect, the violation of this requirement does not invalidate the estimations. In any case, it will reduce the estimated effect

[12] Another potential estimation strategy is an instrumental variable approach using the LFE as a source of exogenous variation on years of education to estimate its impacts on crime (a similar approach was followed by Lochner and Moretti, 2004). But, I cannot follow this approach because I do not have access to a dataset with individual information on years of schooling and criminal records.

and the results will represent a lower bound of the true effect. The second requirement mentioned by these authors is that the treatment should not affect the outcome variable before its implementation ("no anticipation effect"). In this case, there are no reasons to suspect of any impacts of the LFE on crime rates before the implementation in each province, given that the reduction of crime rates in the short run was not an intended outcome of this the reform. Finally, the authors state that an implicit assumption is that the outcome should be independent across provinces ("no interference between units"). In this case, it implies that crime rates in highly exposed/treated provinces should not affect crime rates in provinces in the donor pool. Although available data do not allow to test this assumption, it is highly unlikely that the implementation of the LFE in Buenos Aires and Córdoba have provoked the migration of families or individuals to other provinces.

Validity of the LFE

To analyze the validity of the LFE, I analyze if the reform was associated with changes in security expenditures at the provincial level, following Lochner and Moretti (2004). It could have happened that after the implementation of the reform, provinces started to devote more resources to prevent crime; if that is the case, reductions in crime could be associated with the increase in police on the streets for example. In order to test this concern, I estimate the correlation between the LFE and security expenditure, results are shown in Table 2. I do not find evidence that the LFE is associated with higher level expenditure on security, on per capita security expenditure, or the growth rate of the per capita security expenditure at the provincial level, although in this case, the correlation is positive but non-significant.

5 Results

In this section of the paper, I present and discuss the results of the exercises that I use to identify the relation between the extension of the period of compulsory education and reported crime rates in Argentina. First, I briefly discuss an exercise showing that the LFE had a positive effect on the attendance rates of teenagers aged 13-15 years old. Next, I analyze the estimates that demonstrate that the LFE had a negative impact on crime rates. Then, I perform some additional estimations

aimed to shed some light on the mechanisms behind the relation between compulsory education and crime. Finally, I conduct some standard robustness checks and describe the results.

5.1 LFE and Attendance Rates

I use census data to produce further evidence on the impact of the LFE on educational level. I defined a dummy variable identifying individuals affected by the LFE (equal to one for those that were 12 years old or younger at the moment that the LFE was implemented in her province of residence,[13] and zero otherwise). Then, I estimated a regression of the educational level on the LFE dummy, including other demographic controls and province and year effects. The results in Table 3 indicate that individuals affected by the reform tend to obtain higher educational levels. Given that each category on the dependent variable capturing educational level represents 3 years of education, the results in column 1 of Table 3 mean that, on average, the LFE increased years of education by 0.25 years.

5.2 LFE and Crime

5.2.1 Difference-in-differences

As mentioned before, the year of implementation of the LFE determines the level of exposure to the reform in each province, by defining a pre- and post-treatment period a difference-in-differences estimator can be applied. I follow the classification of groups of provinces defined above (see Table 2 in Appendix 1) as a dummy variable of "treatment" where highly exposed provinces were set to 1, and I define a post-treatment variable dummy as follows: equal to 1 in period 1997 to 2008, in which crime rates should reflect the impact of the reform, at that point all the provinces that implemented the reform should have finished the process of implementation[14], and the dummy was set to zero in a pre-treatment period, from 1991 to 1995.

In Panel A of table 4, I estimate the difference-in-differences estimator, the interaction of the two dummy variables: "treatment" and "post-treatment", to analyze the impact of the LFE on my

[13] Given that my census data doesn't have information about mobility, I am assuming that the individual attended school in the same province where they were living in 2010.

[14] I am not considering years 2001 and 2002 due to the crisis faced by the country, crime rates experienced an unusual peak at that time (see Figure 1).

dependent variables: total and property crimes rates (reported crimes per 100,000 inhabitants, in police stations or other public security agencies). Column 1 indicates that the group of provinces early affected by the reform had a decrease in total crimes in the post-treatment period (423 crimes per 100,000 inhabitants, 11 percent with respect to the comparison group in the post-treatment period) and in column 3, it can be observed a decrease in reported property crimes (229 crimes per 100,000 inhabitants, 10 percent fewer crimes with respect to the less exposed provinces). None of these results are statistically significant. Column 2 and 4 incorporate covariates at the provincial level, I include a gender ratio (measure the number of men per 100 women in each province), provincial school attendance rates for teenagers between 13-15 years (those who must attend the first two years of high school), a measure of income inequality in the province (Gini index), and the per capita security expenditure on provincial budgets. By adding these covariates, it can be observed that interaction terms remain negative, the magnitude is lower, and continue being non-significant.

5.2.2 Synthetic Control Method

In this sub-section, I present and discuss the results of the SCM in the 2 provinces that fully implemented the educational reform in 1996, Buenos Aires and Córdoba, which are also the most important provinces in economic terms.[15] The variable of interest in the analysis is the aggregate crime rate at the provincial level (rates per 100,000 inhabitants).[16] [17] The variables included as predictors to build the synthetic control units are the following: gender ratio, provincial school attendance rates for teenagers between 13-15 years, a measure of income inequality in the province (Gini index), the growth rate of security spending on provincial budgets (to control for differences in levels) and the crime rate in the province one year before the implementation of the LFE.[18]

[15] In Figure 5 in Appendix 1 other provinces in the early treated group (those that adopted the LFE on 1997) are shown, although their results are less conclusive because the year of implementation was closer to the year of implementation in the units in the donor pool.

[16] Figure 2 shows levels of the variable of interest for Buenos Aires, Córdoba and the rest of the provinces. It can be observed that pre-reform period of rest the country cannot be a good comparison group for the provinces that are the objective of this section.

[17] Results for property crimes are shown in Figure 3 and Figure 4 in Appendix 1.

[18] Using all the pretreatment years of the dependent variable as individual predictors could cause all other covariates to become irrelevant (Kaul et al., 2017).

Buenos Aires

Table 5 contains the weights that the SCM assigned to each province in the donor pool to construct the synthetic unit that best reproduces the levels of crime of Buenos Aires in the years before the reform. Total crime rates in Buenos Aires are best reproduced by a combination of total crime rates in Formosa, San Luis, and Tucumán. Table 6 presents statistics on some pre-treatment characteristics that are usually considered as having a causal effect on crime. The results in the table show that the synthetic Buenos Aires performs better, in reproducing the pre-treatment values of Buenos Aires in the explanatory variables under consideration, than the simple average of provinces in the donor pool.

The most important results for Buenos Aires are displayed in Figure 4. In Panel A, it can be observed trends in total crime rates for Buenos Aires and its synthetic control, while Panel B presents the differences between both trends. The results are clear: the LFE had a negative effect on the total crime rate in Buenos Aires, although this effect appeared a few years after the implementation of the reform. A potential explanation of this result is that even though school attendance rate increased in Buenos Aires as a consequence of the LFE, global level of crime in that province increased 91 percent in 2000 with respect to 1995 levels, and given that crime rates I am considering includes not only juvenile crime, the increase in crimes committed by adults could offset reductions generated by the LFE. Given data limitations, I cannot prove this hypothesis.

The magnitude of the effect for Buenos Aires can be computed with a difference-in-differences estimator. This exercise shows that, the adoption of the LFE in Buenos Aires implied an average reduction of 15 percent (significant at the 10 percent level) in the total crime rate with respect to the synthetic Buenos Aires.

Córdoba

Table 7 is similar to Table 5, but in this case it presents the weights that the SCM assigns to provinces in the donor pool to generate the synthetic control unit for Córdoba. The total crimes rate for this province is best reproduced by a combination of CABA, Mendoza, Neuquén, and Tucumán. Table 8 is analogous to Table 6, it presents average pre-treatment characteristics that are thought to be key determinants of crime.

Figure 5 presents the main results of the estimations for Córdoba. Panel A exhibits the trend in the total crime rate for the treated province and for its synthetic control unit, while Panel B contains information on the difference between the trend in real Córdoba and the trend in synthetic Córdoba. Panel B shows that the LFE had a negative effect on the total crime rates in Córdoba, and contrary to the case of Buenos Aires, this effect appeared immediately after the reform was implemented in the province. In this case, the difference-in-differences estimator shows that, on average, the LFE reduced total crimes rates in Córdoba by 12 percent in the post-treatment period (significant at the 10 percent level).

5.3 Potential Explanations

In this sub-section, I explore two channels that are usually considered in the literature as the most important mechanisms connecting education with crime. First, I analyze if the mechanism behind my results is the "incapacitation effect" of spending more years in school as a result of the LFE. According to this effect, the reduction in the crime rates after the extension of the period of compulsory education would be explained by a simple fact: after the law, teenagers must stay longer in the educational system and this, in turn, limits their time to engage in risky activities (Black, Devereux and Salvanes, 2008). Second, I evaluate if the "human capital effect" is present. This effect states that when individuals reach higher educational levels, their wage prospects increase accordingly, which in turn implies that being tied to criminal activities will have a higher opportunity cost (Lochner and Moretti, 2004). In that sense, if increasing the number of years of compulsory education results in individuals reaching higher educational levels, the human capital effect could be another potential explanation of the connection between the LFE and the reduction of crime.

Incapacitation effect

To test if the incapacitation effect is one of the mechanisms explaining the reduction on the crime rates after the implementation of the LFE, I use the SCM to analyze if the LFE affected the school attendance rates. Data on school attendance rates was obtained from a household survey which was primarily designed to collect information on employment conditions. For that reason, the survey

does not have enough power to compute statistics of school attendance disaggregated by provinces and age groups. To overcome this problem, I consider the combination of Buenos Aires and Córdoba as my treated unit. The results are shown in Figure 6, Panel A. It can be observed that immediately after the implementation of the law there was a sharp increase in school attendance rates in Buenos Aires and Córdoba, but after a few years the synthetic unit catches up. This result is explained by the fact that some units in the donor pool started to implement the reform.

Additionally, I use the census dataset to estimate the probability of imprisonment by cohort of birth. Figure 6, Panel B, shows how younger cohorts of birth, in particular, those who were affected by the reform, have a lower probability of imprisonment after controlling for province of residence. Although it can be stated that young people are less likely to be in prison because they had less time to commit a crime, and being caught, the decline is observed just after the first cohort affected by the LFE.

Human Capital Effect

To provide some evidence on this channel I use census data to show changes in the probability of imprisonment by cohort of birth and educational level. If this is a potential channel through which the LFE decreases crime, a decline in such probability should be observed for affected cohorts. Specifically, I estimate the probability of imprisonment in 2010 with respect to those individuals aged 40, by educational level. Results are shown in Figure 7, there is an increasing trend in the probability of imprisonment, but for those cohorts born after 1984, there is a decrease in that probability with respect to older individuals. This fact can be explained by the exposition to the LFE, younger individuals born in 1984 and after, were affected by the implementation of the reform. In addition, even though the law was aimed at individuals aged 13-15 years old, it could happen that those attending higher levels of education have been incentivized to achieve a higher level than before, because after the reform the mandatory threshold was pushed forward, reducing even more their probability of imprisonment. This is not enough evidence to conclude that the decline in total crime rates was driven by changes in human capital per se, but it is indicative that education played a role.

5.4 Robustness Checks

In the SCM, a standard robustness check is to apply the method to the provinces in the donor pool as a falsification test. I show these results in Figure 8.[19] Just for comparison purposes, I include the estimations for Buenos Aires and Córdoba, respectively (orange lines). The synthetic method provides a worse fit in the crime rates in most provinces included in the comparison group. As an additional robustness check, I compute the ratio of the post-reform mean squared prediction error (MSPE) over the pre-reform mean squared prediction error (Figure 9). The MSPE indicates the squared difference between the treated unit and the synthetic unit. Ideally, it would be expected to find the higher ratios for the treated unit, in this case, the ratios of interest (red bars) are among the highest ones.

To sum up, robustness checks shown in this subsection provide some support to the results found previously, although it is important to highlight the limitations imposed by the aggregated data used in this paper.

6 Conclusions

I analyzed the impact of an educational reform in Argentina on crime rates. The most important change imposed by the Federal Law of Education (LFE) was the extension of the period of compulsory education from 7 to 10 years, affecting attendance rates of teenagers aged 13 to 15 years old.

I exploited the fact that there was significant variability between Argentinean provinces in the timing of implementation. I separated the provinces into two groups: highly exposed to the reform (treated provinces) and less exposed to the reform (comparison provinces). I used two non-experimental methodologies: Difference-in-differences and Synthetic Control Method. In the first one, I compare early vs. late treated groups of provinces, and in the second one, I construct synthetic control units for Buenos Aires and Cordoba, the first ones to fully implement the law. The results indicate that after the implementation of the LFE there was a decrease in reported crime rates. I performed robustness checks that provide additional support to my findings. Also, I explored

[19] The difference main with respect to the previous subsection is that I add Buenos Aires (Panel A) or Córdoba (Panel B) to the original donor pool.

potential mechanisms behind the results, incapacitation and human capital effects. The evidence is more suggestive for incapacitation effect.

Even though my results are not novel in the economics of crime, I demonstrate that reforms affecting years of education reduce crime rates even in a developing country, such as Argentina, where the enforcement of educational laws tend to be weak, and in case of not attendance there are no mechanisms to punish teenagers or their families. Datasets used in the estimations are far from being the ideal ones and I could not show additional tests to provide stronger evidence on the mechanisms. Future work is needed on this direction.

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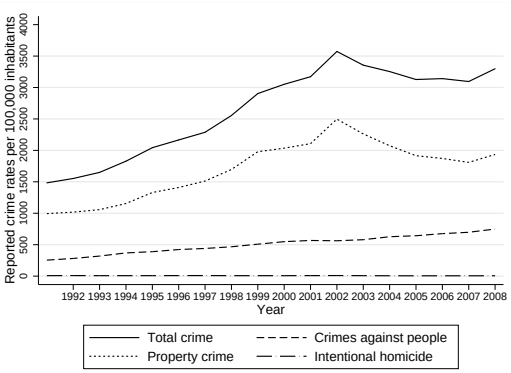
Main Figures and Tables

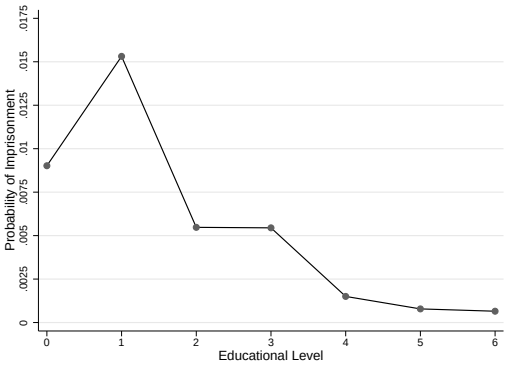
Figures

Figure 1: Reported crime rates, Argentina, 1992-2008



Notes: Rates per 100,000 inhabitants. Source: Ministerio de Justicia y DDHH de la Nación (SNIC).

Figure 2: Probability of Incarceration by Level of Education



Notes: Dependent variable is a dummy variable with value 1 if the individual is in prison during the census day, and 0 otherwise. Independent variables are educational levels where 0=Initial level or no instruction; 1=Incomplete primary education; 2=Complete primary education; 3=Incomplete secondary education; 4= Complete secondary education; 5=Incomplete tertiary/superior education; 6=Complete tertiary/superior education. The sample is restricted to individuals aged 20-40. Regression include province (where the census was collected) and age fixed effects. Source: Census 2010. National Institute of Statistics (INDEC).

Figure 3: LFE and School Attendance 1992-2008

Notes: Dependent variable is average school attendance (children aged 13-15). Vertical line indicates the first year in which the reform was implemented. Provinces in each group are shown in Appendix 1 Table 2. Source: Socio-Economic Database for Latin America and the Caribbean-SEDLAC, CEDLAS and the World Bank (2012).

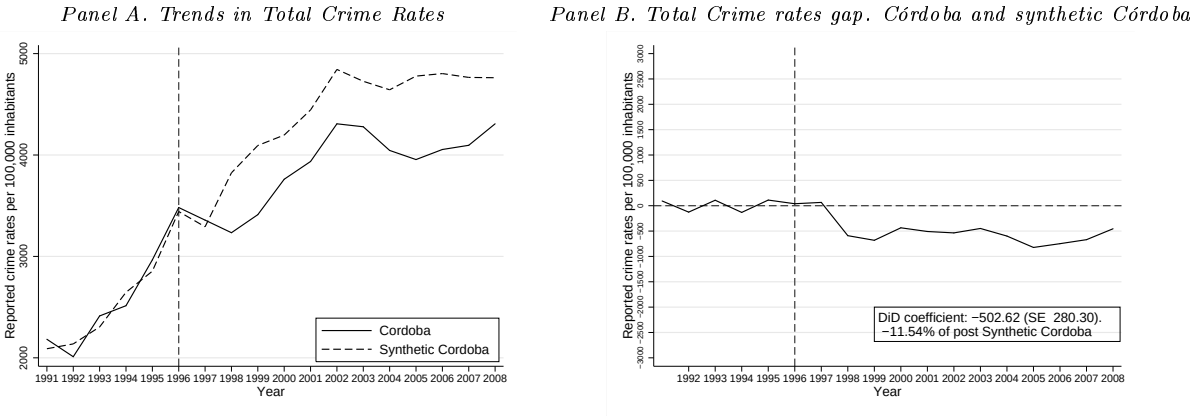
Figure 4: Synthetic control method: Buenos Aires

Panel A. Trends in Total Crime Rates

Panel B. Total Crime rates gap. Buenos Aires and synthetic Buenos Aires

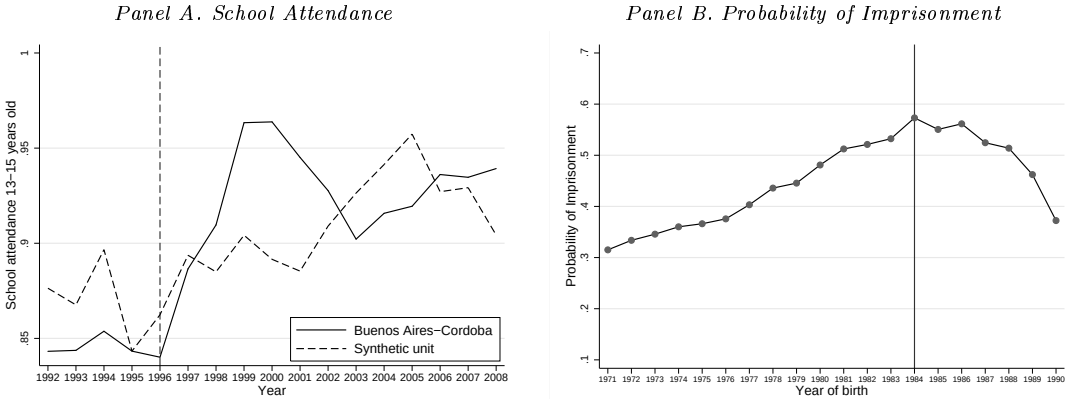
Notes: Rates per 100,000 inhabitants. Vertical line indicates the year in which the reform was implemented. In Panel B: DiD coefficient indicates the difference in differences estimator obtained by considering a regression of the real unit vs. the synthetic one, where the post variable is 1 after the implementation of the law. The percentage number indicates the ratio DiD coefficient/mean of synthetic unit in the post treatment period. Source: Ministerio de Justicia y DDHH de la Nación (SNIC).

Figure 5: Synthetic control method: Córdoba



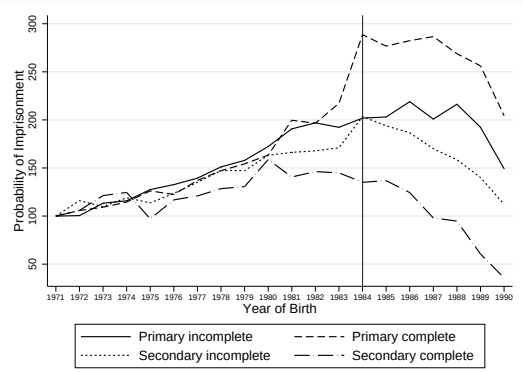
Notes: Rates per 100,000 inhabitants. Vertical line indicates the year in which the reform was implemented. In Panel B: DiD coefficient indicates the difference in differences estimator obtained by considering a regression of the real unit vs. the synthetic one, where the post variable is 1 after the implementation of the law. The percentage number indicates the ratio DiD coefficient/mean of synthetic unit in the post treatment period. Source: Ministerio de Justicia y DDHH de la Nación (SNIC).

Figure 6: Incapacitation Effect.



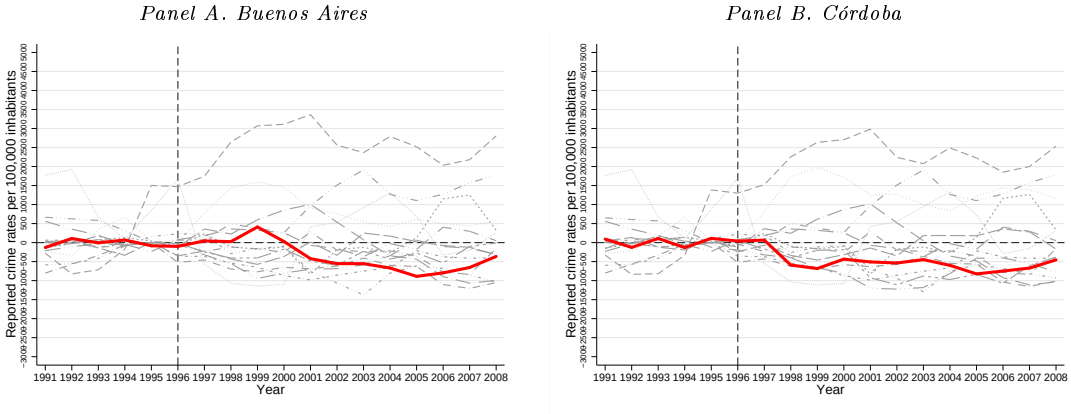
Notes: Panel A: Vertical line indicates the year in which the reform was implemented. The variables used to adjust the pre treatment are unemployment rate, Gini index and the attendance rate in year 1995. Source: Socio-Economic Database for Latin America and the Caribbean-SEDLAC, CEDLAS and the World Bank (2012). Panel B: Vertical line indicates the first cohort of birth affected by the reform (1984). Dependent variable is a dummy variable with value 1 if the individual is in prison during the census day, and 0 otherwise. The sample is restricted to individuals aged 20-40. Regression include province (where the census was collected) fixed effects. Source: Census 2010. National Institute of Statistics (INDEC).

Figure 7: Change in Probability of Imprisonment with respect to Individuals aged 40 by Educational Level.



Notes: Vertical line indicates the first cohort of birth affected by the reform (1984). Dependent variable is a dummy variable with value 1 if the individual is in prison during the census day, and 0 otherwise. The sample is restricted to individuals aged 20-40. Regression include province (where the census was collected) fixed effects. Source: Census 2010. National Institute of Statistics (INDEC).

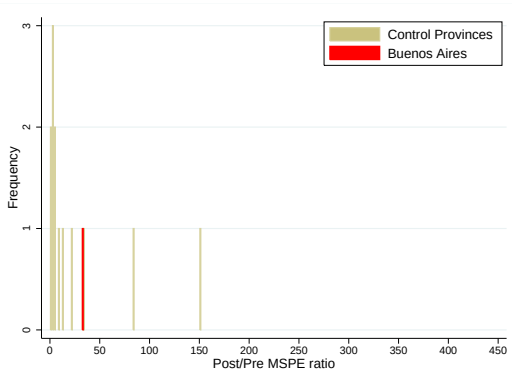
Figure 8: Permutations: Total Crime rates gap between Buenos Aires/Córdoba and synthetic Buenos Aires/Córdoba and placebo gaps in the donor pool.



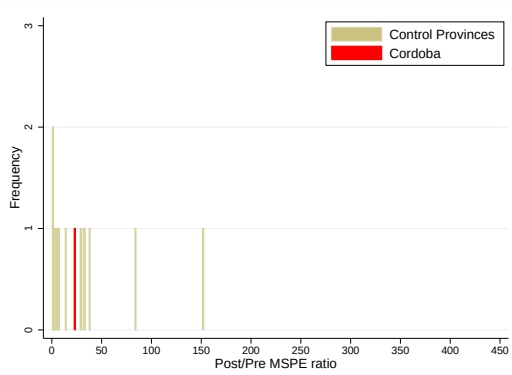
Notes: Rates per 100,000 inhabitants. Vertical line indicates the year in which the reform was implemented. Source: Ministerio de Justicia y DDHH de la Nación (SNIC).

Figure 9: Ratio of Post-Pre Reform Mean Squared Prediction Error.

Panel A. Buenos Aires



Panel B. Córdoba



Notes: Rates per 100,000 inhabitants. Source: Ministerio de Justicia y DDHH de la Nación (SNIC).

Tables

Table 1: LFE implementation.

Implementation	Intensity		
	Full	Gradual	NI
1996	Buenos Aires Cordoba		
1997	Corrientes Entre Rios La Pampa San Juan Santa Fe	Chaco	
1998	Formosa Misiones San Luis Santa Cruz Santiago del Estero Tucuman	Jujuy Neuquen Salta Tierra del Fuego	
1999		Catamarca Chubut La Rioja	
2000		Mendoza	
NI			Cdad. de Bs As Rio Negro

Notes: Type of Implementation F: Full from start; G: Gradual; NI: Never implemented the reform. Source: Crosta (2007).

Table 2: LFE and Security Expenditure.

	(1)	(2)	(3)
VARIABLES	Security Expend.	Security Expend. per capita	Growth rate (2)
LFE	-64.856	-17.904	2.943
	(66.307)	(18.952)	(2.714)
Constant	1,581.196***	1,082.353***	6.514***
	(31.904)	(14.806)	(2.263)
Observations	384	384	353
R-squared	0.745	0.939	0.463

Notes: Column (1) indicates security expenditures at the provincial level in millons of constant pesos (at 2001 prices). Column (2) indicates security expenditure in per capita terms (at 2001 prices). Column (3) indicates the growth rate of the security expenditure in per capita terms. Regressions include age and province fixed effects. Robust standard errors clustered at the provincial level. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels respectively. Source: Socio-Economic Database for Latin America and the Caribbean-SEDLAC, CEDLAS and the World Bank (2012) and Ministerio de Justicia y DDHH de la Nación (SNIC).

Table 3: Impacts of LFE on Educational Level.

	(1) All	(2) Men	(3) Women
LFE	0.082***	0.092***	0.074***
	(0.025)	(0.026)	(0.025)
Fixed effects	Yes	Yes	Yes
N	12,657,034	6,251,885	6,405,149
R-squared	0.052	0.055	0.051

Notes: Dependent variable is a variable where 0=Initial level or no instruction; 1=Incomplete primary education; 2=Complete primary education; 3=Incomplete secondary education; 4= Complete secondary education; 5=Incomplete tertiary/superior education; 6=Complete tertiary/superior education. The sample is restricted to individuals aged 20-40. Regressions include age and province fixed effects. Robust standard errors clustered at the provincial level. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels respectively. Source: Census 2010. National Institute of Statistics (INDEC).

Table 4: LFE and Crime Rates.

VARIABLES	(1) Total crimes	(2) Total crimes	(3) Property crimes	(4) Property crimes
Post*Treatment	-423.197	-362.148	-228.957	-142.303
	(309.636)	(512.773)	(221.508)	(385.319)
Treatment	-6.903	-225.512	7.402	-126.767
	(264.502)	(591.350)	(223.607)	(477.473)
Post	1,574.598***	1,232.389**	943.489***	605.058
	(266.513)	(492.954)	(184.773)	(370.646)
Constant	1,846.760***	10,501.872**	1,270.027***	6,309.203*
	(163.292)	(4,105.128)	(147.555)	(3,189.979)
Provincial Controls	No	Yes	No	Yes
Observations	330	244	330	244
R-squared	0.272	0.415	0.185	0.366

Notes: Robust standard errors clustered at the provincial level. Controls included iin columns 2 and 4 are: gender ratio, provincial school attendance rates for teenagers between 13-15 years old, Gini index, and per capita security expenditure on provincial budgets.*, **, and *** denote statistical significance at the 10, 5, and 1 percent levels respectively. Source: Socio-Economic Database for Latin America and the Caribbean-SEDLAC, CEDLAS and the World Bank (2012) and Ministerio de Justicia y DDHH de la Nación (SNIC).

Table 5: Province weights of synthetic Buenos Aires.

Donor pool	Weights	Donor pool	Weights
CABA	0.00	Neuquen	0.00
Catamarca	0.00	Rio Negro	0.00
Chaco	0.00	San Luis	0.04
Chubut	0.00	Santa Cruz	0.00
Formosa	0.49	Santiago del Estero	0.00
La Rioja	0.00	Tierra del Fuego	0.00
Mendoza	0.00	Tucuman	0.47
Misiones	0.00		

Notes: Rates per 100,000 inhabitants. Socio-Economic Database for Latin America and the Caribbean-SEDLAC, CEDLAS and the World Bank (2012) and Ministerio de Justicia y DDHH de la Nación (SNIC).

Table 6: Predictor means of Buenos Aires.

Variables	Real Unit	Synthetic Unit	Average Control
Gender ratio	96.05	99.97	101.02
Attendance rate 12-15	0.90	0.90	0.92
Gini	0.45	0.49	0.43
Expenditure Security pc	0.07	0.04	0.05
Total crime rate (1995)	681.00	720.40	1,526.67

Notes: Rates per 100,000 inhabitants. Variables are averaged for the 19991–1995 period, except total crime rates that refers to 1995 and the growth rate of expenditure on security (1994-1995). Source: Socio-Economic Database for Latin America and the Caribbean-SEDLAC, CEDLAS and the World Bank (2012) and Ministerio de Justicia y DDHH de la Nación (SNIC).

Table 7: Province weights of synthetic Córdoba.

Donor pool	Weights	Donor pool	Weights
CABA	0.19	Neuquen	0.26
Catamarca	0.00	Rio Negro	0.00
Chaco	0.00	San Luis	0.00
Chubut	0.00	Santa Cruz	0.00
Formosa	0.00	Santiago del Estero	0.00
La Rioja	0.00	Tierra del Fuego	0.00
Mendoza	0.22	Tucuman	0.32
Misiones	0.00		

Notes: Rates per 100,000 inhabitants. Source: Socio-Economic Database for Latin America and the Caribbean-SEDLAC, CEDLAS and the World Bank (2012) and Ministerio de Justicia y DDHH de la Nación (SNIC).

Table 8: Predictor means of Córdoba.

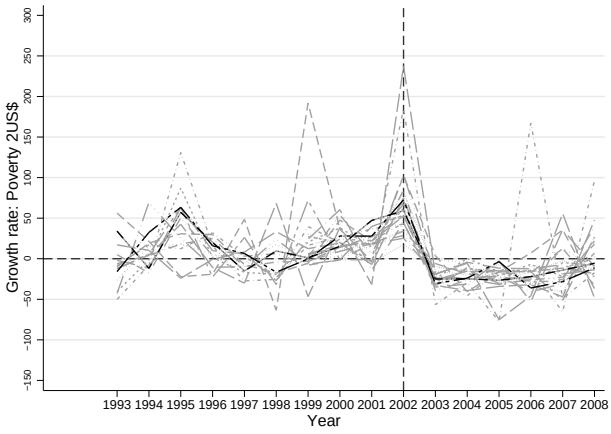
Variables	Real Unit	Synthetic Unit	Average Control
Gender ratio	95.68	95.68	101.02
Attendance rate 12-15	0.90	0.92	0.92
Gini	0.45	0.47	0.43
Expenditure Security pc	0.05	0.11	0.05
Total crime rate (1995)	2,148.00	2,082.40	1,526.67

Notes: Rates per 100,000 inhabitants. Variables are averaged for the 19991–1995 period, except total crime rates that refers to 1995 and the growth rate of expenditure on security (1994-1995). Source: Socio-Economic Database for Latin America and the Caribbean-SEDLAC, CEDLAS and the World Bank (2012) and Ministerio de Justicia y DDHH de la Nación (SNIC).

Appendix 1: Figures and Tables

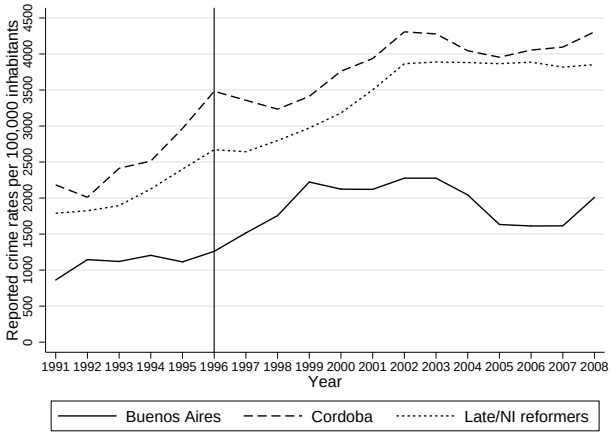
Figures

Figure 1: Poverty Growth Rate



Notes: Vertical line indicates year of the economic crisis. Source: Socio-Economic Database for Latin America and the Caribbean-SEDLAC, CEDLAS and the World Bank (2012)

Figure 2: Reported crime in Buenos Aires, Córdoba vs. donor pool 1992-2008

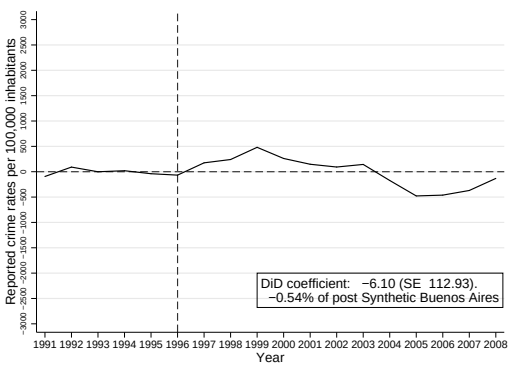


Notes: Rates per 100,000 inhabitants. Vertical lines indicates the year in which the reform was implemented. Source: Ministerio de Justicia y DDHH de la Nación (SNIC).

Figure 3: Synthetic control method: Buenos Aires. Property crimes.

Panel A. Trends in Crime Rates

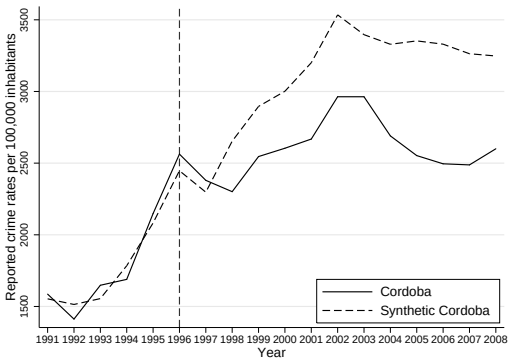
Panel B. Crime rates gap. Buenos Aires and synthetic Buenos Aires



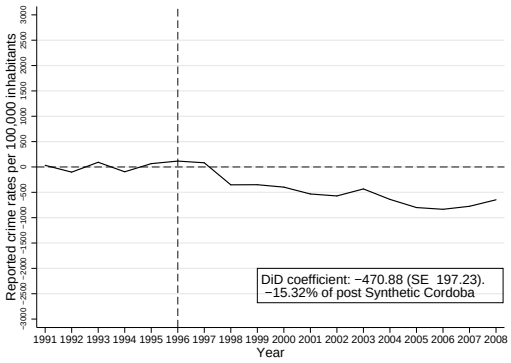
Notes: Rates per 100,000 inhabitants. Vertical line indicates the year in which the reform was implemented. In Panel B: DiD coefficient indicates the difference in differences estimator obtained by considering a regression of the real unit vs. the synthetic one, where the post variable is 1 after the implementation of the law. The percentage number indicates the ratio DiD coefficient/mean of synthetic unit in the post treatment period. Source: Ministerio de Justicia y DDHH de la Nación (SNIC).

Figure 4: Synthetic control method: Córdoba. Property crimes.

Panel A. Trends in Crime Rates

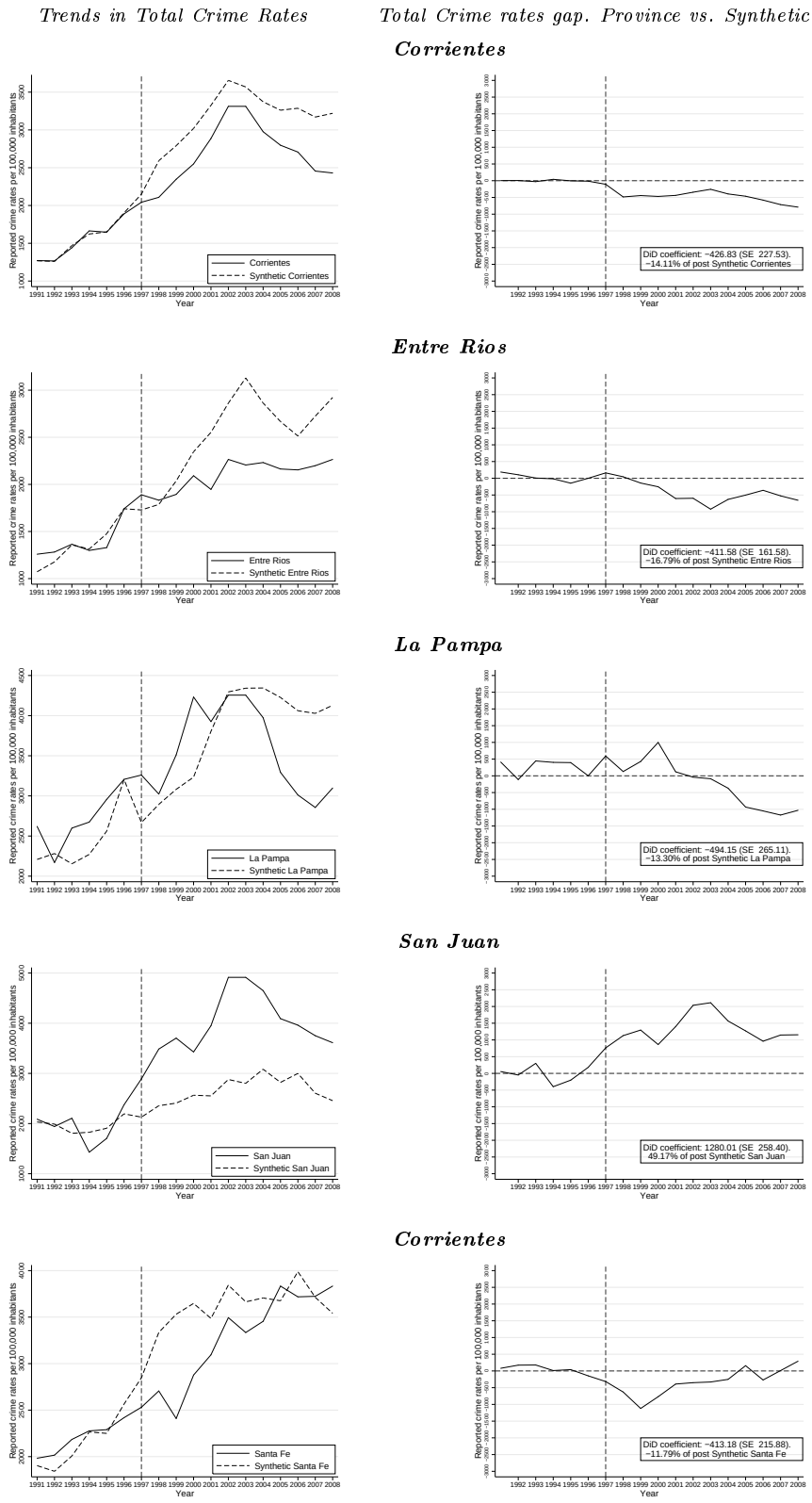


Panel B. Crime rates gap. Córdoba and synthetic Córdoba



Notes: Rates per 100,000 inhabitants. Vertical line indicates the year in which the reform was implemented. In Panel B: DiD coefficient indicates the difference in differences estimator obtained by considering a regression of the real unit vs. the synthetic one, where the post variable is 1 after the implementation of the law. The percentage number indicates the ratio DiD coefficient/mean of synthetic unit in the post treatment period. Source: Ministerio de Justicia y DDHH de la Nación (SNIC).

Figure 5: Synthetic control method



Notes: Rates per 100,000 inhabitants. Vertical line indicates the year in which the reform was implemented. In Panel B: DiD coefficient indicates the difference in differences estimator obtained by considering a regression of the real unit vs. the synthetic one, where the post variable is 1 after the implementation of the law. The percentage number indicates the ratio DiD coefficient/mean of synthetic unit in the post-treatment period. Source: Ministerio de Justicia y DDHH de la Nación (SNIC).

Tables

Table 1: Equivalencies between Educational Systems.

Previous Structure		Approximate ages	LFE Structure	
Level	Years		Level	Years
Initial	1°	3	1°	Initial
	2°	4	2°	
	3°	5	3°	
Primary	1°	6	1°	EGB1
	2°	7	2°	
	3°	8	3°	
	4°	9	4°	EGB2
	5°	10	5°	
	6°	11	6°	
	7°	12	7°	EGB3
Secondary	1°	13	8°	
	2°	14	9°	
	3°	15	1°	Polimodal
	4°	16	2°	
	5°	17	3°	

Notes: The mandatory years of schooling have been shaded. LFE: Federal Law of Education. EGB: Educación General Básica. Source: DINIECE, Ministry of Education, Science and Technology.

Table 2: Groups of Provinces

Early Treatment *1996-1997 & Full*	NI/Late Treatment *1998-2000 & Full-Gradual*
Buenos Aires	Chaco
Cordoba	Formosa
Corrientes	Misiones
Entre Rios	San Luis
La Pampa	Santa Cruz
San Juan	Santiago del Estero
Santa Fe	Tucuman
	Jujuy
	Neuquen
	Salta
	Tierra del Fuego
	Catamarca
	Chubut
	La Rioja
	Mendoza
	Cdad. de Bs As
	Rio Negro

Notes: NI: Never implemented the reform. Source: Crosta (2007).

Table 3: Differences in means. Pre LFE.

	Treatment		Control		Difference		
Variables	Mean (1)	SE	Mean (2)	SE	(1)-(2)	SE	P-value
Poverty	9.31	0.69	9.68	0.92	-0.37	2.10	0.86
Inequality	0.42	0.00	0.43	0.00	-0.01	0.01	0.19
Unemployment rate 15-18	0.34	0.03	0.28	0.02	0.06	0.04	0.16
Unemployment rate 15-18, Male	0.31	0.02	0.27	0.02	0.05	0.04	0.23
School attendance rate 13-15	0.87	0.01	0.92	0.01	-0.04	0.02	0.07
School attend. rate 13-15, Male	0.87	0.01	0.90	0.01	-0.02	0.02	0.34
Gender ratio	97.48	0.41	100.71	0.67	-3.23	1.60	0.06
Basic Educ. publ. expenditure pc	489.40	112.10	200.49	18.82	288.91	196.90	0.15
Soc. Assistance publ. expend. pc	99.33	25.17	33.37	4.62	65.96	43.16	0.14
Security-Defense publ. expend. pc	203.81	51.44	63.65	6.57	140.16	89.78	0.13
Total Crimes rate	1,861.75	109.61	2,051.14	92.06	-189.39	266.13	0.48
Property Crimes rate	337.64	28.52	412.56	18.92	-74.92	65.27	0.26
Crimes against People rate	1,283.14	87.88	1,363.89	66.55	-80.74	206.63	0.70
Intentional Homicide rate	5.74	0.45	9.69	1.14	-3.95	2.32	0.10

Notes: The provinces in each group are described in Table 2 in Appendix 1. The poverty rate is estimated by using the US$ 2.5 threshold. All the expenditure measures are calculated using the inverse hyperparabolic sine transformation as follows: $sinh^{-1}(x) = ln\left[x + \left(x^2 + 1\right)^{.5}\right]$ (expressed in constant 2001 prices). Income inequality measured by the Gini coefficient. The gender ratio between 15 and 24 year-olds measures the number of men per 100 women. Source: Socio-Economic Database for Latin America and the Caribbean-SEDLAC, CEDLAS and the World Bank (2012) and Ministerio de Justicia y DDHH de la Nación (SNIC). Rates per 100,000 inhabitants.