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Preventing criminal minds: Early education access and adult offending behavior☆

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ABSTRACT

In this paper we estimate the impact of higher early preschool access on offending behavior later in time, exploiting a nationwide public preschool expansion that took place in Spain over the 1990s. This is the first study providing evidence on the relationship between preschool and crime using a large-scale, universal-access program rather than targeted initiatives reserved for disadvantaged children. Our identification strategy relies on the staggered implementation of the Spanish preschool expansion across regions and birth cohorts. We link the education data to a unique administrative crime dataset recording the offenses committed in the region of Catalonia over the period 2009–2014. On average, a 1 percentage point increase in preschool access among the age 0–3 population yields 1.6% fewer crime actions during youth and young adulthood. Larger effects on impulsive-crime categories provide suggestive evidence for non-cognitive-skills improvements being important drivers of our results.

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"Educate the children
and it won't be necessary to punish the men."
– Pythagoras

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1. Introduction

Early childhood is recognized by various fields of research as a key stage in life, carrying the power to shape future outcomes through youth and well into adulthood. There is wide consensus about early childhood assessments being good *predictors* for educational achievements, labor market success and health outcomes.[1] Even more importantly, a growing number of studies shows that *policy interventions* on early education are able to produce significant impacts in the short and even in the long run, especially when disadvantaged children are targeted.[2] The majority of studies looks at cognitive skill formation and derived outcomes, while along the *non-cognitive* and *behavioral* dimensions, rigorous large-sample evidence is scarce and mixed, often due to the lack of high-quality data on outcomes (Duncan and Magnuson, 2013). However, pedagogical theory clearly indicates that pre-kindergarden circumstances and experiences are able to forge an adult's spheres of anxiety and self-control, self-esteem and motivation, socio-emotional intelligence and aggressive behavior.[3] Moreover, evidence from psychology, criminology and economics suggests that reducing early externalizing behavior reduces crime (Heckman et al., 2013).

Building on these premises, our paper studies the causal relationship between the access to good-quality early childhood education and criminal propensity later in life, and is the first to do so looking at a universal public preschool program. Previous evidence is based on small-scale, highly selective initiatives – such as Perry, Abecedarian and CARE in the US, which count significant crime reduction among their benefits. These programs are characterized by high per-pupil costs and intensive interaction with participating families. It is far from obvious that a 'standard' preschool program, ran on nationwide scale and open to all households, would still yield appreciable benefits in terms of adult offending behavior.

Starting in the early 1990s, a Spanish national education reform boosted the availability of public preschool for 3 year-old children. Due to the autonomy enjoyed by regional governments on educational matters, the reform was implemented in a staggered manner, and substantial variation in public preschool access for 3-year olds arose both across birth cohorts and across Spanish regions. For crime outcomes, we employ an exclusive dataset of highly-detailed administrative data obtained from police forces, recording offenses committed in the region of Catalonia over the period 2009–2014. Catalonia is populous, wealthy and has an attractive labor market, so that its resident population includes internal migrants coming from all other Spanish regions. We construct offending rates between ages 15 and 30 by region of origin and birth cohort, and relate the variation in offenses to the variation in preschool enrolment rates characterizing each region and birth cohort back in the 1990s. We estimate the causal impact of early preschool access on crime prevalence through an intuitive and robust fixed effects model.

We find a negative impact of early public preschool access on crime rates in youth and young adulthood: a 1 percentage point increase in age 0–3 preschool access back in the early 1990s yields on average 1.6% fewer crime actions in the 2000s. These magnitudes are smaller than those identified by evaluations of the selective US preschool programs; however, our cost-benefit analysis suggests that the monetary value of cost-of-crime savings produced by the Spanish public preschool expansion exceeded expenses by multiple times.

Our high-quality crime dataset allows us to observe detailed information about the type of offense committed. Exploiting this data, in the second part of our analysis we assess the impact that the national preschool expansion has had on different types of crime separately, which helps us to shed light on the potential mechanisms at work behind the main results. The crime types on which we estimate the highest preschool impacts are those of more impulsive and imprudent nature – those crimes for which literature has found poor socio-emotional and behavioral skills to be important drivers. Our findings suggest that the non-cognitive skills channel plays an important role in explaining the reduction in crime which followed the increased public preschool access in Spain. These results are in line with the existing evidence from the US, which our paper extends to a context of large-scale and universal-access.

We test the exogeneity of the Spanish preschool expansion pattern with respect to our regression model, by showing the absence of correlations with relevant region-cohort characteristics and by conducting placebo tests. The latter prove that the relationship between early preschool access and crime exists only within correctly matched cohorts and regions, while it breaks down when associating access rates of only slightly older peers, different cohorts or different regions of origin. We provide statistical evidence to support the remaining assumptions behind our estimation strategy, including those on internal migration behavior and the crime-age distributions. We discuss the threats to our empirical setup, highlight its advantages with respect to existing work, and perform a set of robustness checks to corroborate our findings.

The remainder of this paper is structured as follows. Section 2 reviews the literature. Section 3 presents the institutional background on preschool education in Spain, and illustrates the public preschool expansion occurred in the 1990s. Section 4 presents the data employed and discusses the characteristics of our sample of analysis. Section 5 explains our empirical strategy. Section 6 presents our main results, discusses potential channels behind these and illustrates our cost-benefit exercise. Section 7 reviews the main assumptions underlying our empirical strategy and offers empirical evidence to back these up. Section 8 provides further considerations about strengths and shortcomings of our analysis, and concludes.

[1] Among others, see Burchinal et al. (2010), Nelson and Sheridan (2011), Le et al. (2005), Duncan et al. (2010), Nguyen et al. (2016), Case et al. (2002).

[2] Among others, see Currie and Thomas (1995), Garces et al. (2002), Ludwig and Miller, 2007, Magnuson et al. (2007), Conti et al. (2016), Campbell et al. (2002), Heckman (2006), Gertler et al. (2014), Conti and Heckman (2014).

[3] Among others, see Gunnar and Barr (1998), Kagan and Snidman (1999), Moffitt et al. (2011), Maslow (1943), Almlund et al. (2011), Phillips and Shonkoff, 2000, Robinson (2007), Tremblay et al. (2004); Hahn et al. (2007).

2. Related literature

There is a small literature studying the impact of universal preschool programs on criminal behavior later in time. In fact, most studies on the effects of preschool education are either focused on the short-run, consider strictly cognitive outcomes or are based on highly selected populations. On the other hand, studies on the relationship between education and criminal behavior typically investigate teenage-year interventions rather than early childhood ones. The remainder of this section summarizes the main findings from recent literature related to ours.

Attending high quality pre-kindergarden programs is effective both towards increasing cognitive performance of children and towards improving their socio-emotional skills. The far-reach of the *earliest* educational choices into the *adult* socio-emotional sphere has been recognized in economics only quite recently.[4] Particularly influential in demonstrating such long-run impacts have been those studies evaluating high-quality US programs such as the Perry program, Project CARE and the Abecedarian project. Studies by Heckman et al. (2010a), Heckman et al. (2013) and Conti et al. (2016) follow individuals from age 3 up to age 40 and show that the Perry and Abecedarian programs had persistent positive impacts on academic motivation and externalizing behaviors such as aggressive, antisocial and rule-breaking conduct. They show that these short-run impacts in turn translated into significant improvements in a range of adult outcomes including marriage, health, healthy behaviors and, most importantly, a strongly reduced participation in crime, quantified in experiencing approximately half as many lifetime arrests as non-participants.[5] These findings confirm those in the analysis by Schweinhart et al. (2005), which highlights crime prevention as the most beneficial lifetime impact of the Perry program. Long-run benefits from the Head Start program were documented using sibling-comparisons (Garces et al., 2002; Deming, 2009), variation in the program rollout across time and geography (Thompson, 2018) and combinations of the two strategies (Johnson and Jackson, 2019). Garces et al. (2002) find significant increases in educational attainment for both blacks and whites, as well as reduced probability of being charged with a crime for blacks; Deming (2009) however finds positive effects of Head Start on a range of young adult outcomes but *not* on criminal activity, which in his dataset is self-reported. Using more recent versions of the Head Start programs, Thompson (2018) confirms the positive impact on educational achievements and also documents increases in adult earnings and health, with larger treatment effects among blacks, children of lower-education parents, and children exposed to better funded programs. Johnson and Jackson (2019) again confirm increases in educational attainment and earnings for poor children, as well as reduced poverty and incarceration rates; they also show dynamic complementarities between exposure to the Head Start programs and later K-12 spending.

The common denominator of the research cited so far is its reliance on rather small and highly selected study populations. The access to the Head Start, Perry, Abecedarian, CARE and many other US state-level programs was and is reserved to individuals from low-income families and otherwise disadvantaged backgrounds. The majority of these programs includes mentoring for families, on top of the institutional education offered to children. It is far from obvious that the strong impacts on crime reduction identified in such contexts would hold if scaled up to encompass larger and more diversified populations, and ran on a standard, nationwide basis.

Most studies on universal or very-large-scale preschool initiatives have limited their attention to short-run behavioral outcomes or, when contemplating the long horizon, to strictly academic or labor market skills.[6] An exception to the rule and close to our study is the recent contribution by Baker et al. (2019), who study the long-run effects of an universal preschool program introduced in Quebec starting in 1997 and exploit the fact that eligibility was staggered across birth cohorts. However, the Quebec program can be seen as an outlier in the preschool literature, since it has been found to be of particularly low cost and quality; earlier studies on its consequences had documented persistent negative shocks to noncognitive development of children and very little impact on cognitive skills (Baker et al., 2008; Kottelenberg and Lehrer, 2013). In fact, the recent long-run analysis shows persistence in the negative impacts on noncognitive outcomes throughout teenage years, and concludes that cohorts with increased child care access have worse health and lower life satisfaction later in life, as well as displaying *higher* crime rates between ages 12 and 20. Beyond the Quebec study, looking at impacts of *universal* preschool programs on criminal behavior there is a working paper by Smith (2015). The author focuses on a universal program started in Oklahoma in 1998 and leverages the birthdate cutoff for its first year of implementation. The sample is rather limited in terms of age, since it includes only individuals aged 18 and 19, but the results clearly point to very large reductions in criminal behavior among black children and no detectable impacts for white children. It is evident that further evidence on the impact of universal programs on criminal behavior is needed, and the Spanish preschool experience offers the characteristics of being nationwide, of universal access for all socioeconomic backgrounds, of good quality and sufficiently long-established to allow the analysis on crime outcomes for fifteen different birth cohorts.

The second body of literature enclosing our paper investigates the impact of education on criminal behavior in youth or adulthood. On this avenue, the vast majority of studies looks at the results of changing the minimum school leaving age,

[4] The medical and pedagogical literature have been strongly supporting the effectiveness of school-based programs to reduce or prevent violent behavior for a longer time. For a systematic review, see Hahn et al. (2007).

[5] These more recent studies refined and reinforced the findings by Heckman et al., 2009a,b, 2010, in which crime reduction was first identified as a major benefit of the Perry program, the main channel of the effect was enhancing non-cognitive and behavioral skills. Some years earlier, Campbell et al. (2002, 2008) had found that both Project CARE and the Abecedarian Project induced reduced marijuana consumption. However, at odds with the other studies, males were actually found *more likely* to report breaking the law.

[6] Appendix A provides a short review of studies in these two strands of literature.

or at the results of attending higher- vs. lower-quality schools. Appendix A provides a brief summary of relevant literature in this area. The effectiveness of large-scale education policies in influencing offending behavior is generally supported, but all based on initiatives targeted at individuals aged between 14 and 18, for whom the effects of the additional education received often mingles with the pure crime-incapacitating effect of spending more time at school. Our paper extends the literature on the impact of education policies on crime by looking at a large early-years intervention – and more specifically, at one changing the start of formal education rather than its end.

3. Institutional background

Up until the early 1990s, Spanish preschool was organized into 'kindergarden' for ages 0 to 3 and 'nursery school' for ages 4 to 5, and the vast majority of children was starting preschool education at age 4. The public offer of nursery schools was adequate, and enrolling a child into nursery school meant securing a spot for primary education in the same center (Berea, 1992). However, mainly due to funding restrictions, the offer of public kindergardens for children aged 3 and below was utterly deficient and unable to meet the demand of families.[7] For this age group, offer of private institutions was somewhat larger than the public, but was unregulated, expensive, and lacking any quality scrutiny on standards or staff credentials. Moreover, both public and private kindergardens were viewed as mere 'day-custody' centers (*"guarderías"*), typically carrying very little educational content (Berea, 1992; González, 2004). Informal daycare arrangements through extended family members or friends were the only alternative resource for many working mothers (Del Campo Urbano, 1982).

In 1990 the educational system was deeply reformed through a new regulatory framework known as LOGSE – *Ley Orgánica General del Sistema Educativo* (Ley Orgánica 1/1990), which incorporated a significant increase in the importance given to preschool education. Preschool was restructured so that kindergarden was reduced to cover ages 0 to 2 and the start of nursery school was anticipated to age 3. These changes triggered a rapid expansion of early public preschool services and especially boosted enrollment of 3-year-old children, which rose from around 5% to around 65% between 1987 and 2000. This was an entirely supply-driven expansion, as documented by the many authors reporting the severity of excess demand for public preschool spots throughout the Spanish context over that period (Análisis de la demanda de servicios para la primera infancia, 1992; Felfe et al., 2015; González, 2004; Gonzälez and Quiroga, 2003; González and Vidal Torre, 2005, 2006). The reasons of the insufficient public provision was mainly due to chronic scarcity of funding (Calero and Bonal, 1999; Rambla and Bonal, 2000; Bonal et al., 2005). Based on this evidence, we will refrain from including any demand-side considerations in this analysis.

Figure 1 shows the evolution of national preschool enrollment rates over those 14 academic years, and distinguishes between 0–2 year-olds and 3-year-olds from 1993 onward, when age-specific data became available. There are two important takeaways from this figure: first, over this period the growth in preschool enrollment was entirely driven by the public sector, while the private offer remained virtually unchanged; second, the growth in preschool enrollment was almost entirely driven by 3-year olds. In fact, we will focus our empirical analysis solely on the public sector and overlook the 0–2 age group, as will be discussed later on.

Public preschool spots are open to all families and heavily subsidized by regional governments, so that parents are charged on average only around 12% of the total cost of a preschool spot[8], with additional public subsidies available to low-income families, but without ever being completely free-of-charge.[9] Both low and middle-income families are the most common users of public preschool centers. This pool of users diverges both in terms of size and in terms of characteristics from those populating the smaller-scale Perry, Abecedarian and CARE programs in the US, making the contribution of our analysis all the more innovative.

It is important to note that through the new legal framework, the role of preschool transitioned from the previous 'day-custody' perspective to a proper educational stage, with class sizes limited to 20 pupils and the requirement of university degrees for teachers. Importantly, the new Spanish preschool recognized the importance of non-cognitive skills for children's comfort and success in social environments, so that national guidelines put particular emphasis on non-cognitive skills development. Children were promoted in their self-awareness, personal autonomy, knowledge of the environment, communication and representation skills, social, affective and personal advancement.

3.1. Staggered expansion

The decentralization of educational competences from the national to the regional level started in Spain during the early 1980s and protracted itself until the late 1990s, going hand in hand with the wider political decentralization process occurring in those years. As the new educational regulations were approved in 1990, Spanish regions (*Comunidades Autónomas*)

[7] Being voluntary, preschool ranked low in the priorities of the public education agenda. Funding was limited and directed primarily to compulsory schooling stages, and became especially scarce over the 70s and 80s decades, as the 1960s baby-boom led to an overstretch of national educational budgets.

[8] Based on data from the region of Andalusia for the school year 1994 (*Temporeros y educación*, Temporeros y educación (1997))

[9] Access to public spots is regulated by a point system, which prioritizes children with older siblings already attending the same education center, or whose parents live or work in its catchment area. Extra points are attributed for low income status and to large families.

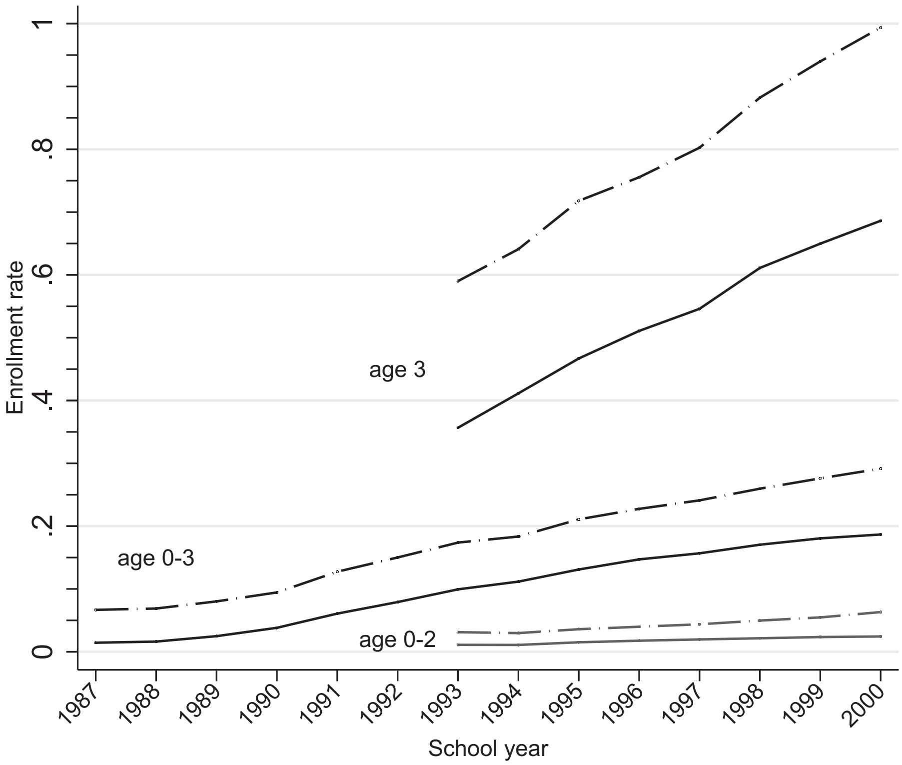

Fig. 1. Early preschool enrollment rates in Spain, 1987–2000 *Note:* Early preschool enrollment rates in Spain, by school year. Source: Statistics of the Ministry of Education. Public preschool rates are solid and total (public + private) ones are dash-dotted. The upper two series trace age 3 enrollment, the middle two trace the ages 0–3 average enrollment, the lower two trace ages 0–2 average enrollment. Before 1993, enrollment rates were not collected separately by age by the Ministry of Education.

were in the process of taking over educational responsibilities.[10] As a result of their decisional and budgetary autonomy, the timing of the expansion in the public supply of early preschool varied considerably across the eighteen Spanish regions; Fig. A.1 shows the regional evolution of early preschool enrollment rates between school years 1987 and 2000.

As in several other cases of uncoordinated reform rollouts, the Spanish preschool expansion was staggered across time and space but not the result of an actual random assignment procedure (among several others, see Blanden et al., 2016; Duflo, 2001; Felfe et al., 2015; Havnes and Mogstad, 2011; Hoynes et al., 2011; Johnson and Jackson, 2019). Several studies on the Spanish case have failed to identify any salient determinant explaining the timing in the adoption of the 1990 educational reform across regions, or justifying the territorial differences in public preschool supply (Bonal et al., 2005; González and Quiroga, 2003; Flaquer and Oliver, 2002). Cross-regional differences in policy adoption have been attributed to a complex blend of local wealth, political preferences, differences in local financing models, socio-economic structure, local history, demographic distribution and migration, but without identifying any salient factor among these (Bonal et al., 2005).

Given that the cross-regional and cross-temporal variation in preschool expansion underlies our identification strategy, it is important to discuss whether it might be spuriously related to our outcome variable, criminal behavior later in time. As a first step, through the analysis presented in Table A.1 we are able to confirm the absence of significant correlations between the regional pattern of preschool expansion and a range of variables proxying institutional, financial and socio-economic factors characterizing each Spanish region and birth cohort. Importantly, we also introduce proxies for criminal activity back in time to identify any spurious relationship between education and crime dynamics pre-dating the 1990s preschool expansion – and we find none. Despite these results, one might still be worried that something in the mix of regional characteristics might drive both preschool enrollment patterns and the later criminal propensity of individuals growing up there. We thus further validate the exogeneity of region-cohort specific preschool enrollment levels with respect to our model through the compelling placebo tests illustrated in Sections 6 and 7.

[10] Overall, more than 50% of the public education expenditure was administered by regional governments at the end of the 1980s, and the figure would rise to above 80% over the 1990s (Calero and Bonal, 2004). On the other hand, the involvement of the more local level (provinces and municipalities) in educational matters is, in general, quite limited in Spain.

4. Data

Data on regional preschool enrollment stems from the records of the Spanish Ministry of Education[11], which we personally digitalized, and data on resident children is provided by population series (by region of origin and year of birth) from the Spanish National Statistics Agency (INE).[12] We focus on birth cohorts 1984 to 1997, which are homogeneous in terms of educational characteristics beyond preschool access (most importantly, school leaving age) and are old enough to appear in our crime dataset later in time. Our main variable of interest is a measure of early preschool access, which we construct as follows:

$$P_{c,i} = \frac{\text{Preschoolers } (0\text{ - }3)_{c+3,i}}{\text{Total children aged } (0\text{ - }3)_{c+3,i}}$$

where c indexes the birth cohort and i indexes the Spanish region of origin. That is, we measure early preschool access for region i and cohort c as the number of 0–3 year olds attending public preschool in the academic year $c+3$, over the total population of 0–3 year olds in the region. For example, the preschool access of a child born in the region of Andalusia in 1990 is measured as the number of preschool attendees among children aged 0–3 registered in Andalusia in the year 1993, over the total number of children aged 0–3 living in Andalusia in 1993. In effect, we use the full 14-year long enrollment data for the 0–3 age group but attribute all its dynamics to 3-year olds, our focus population. Figure 1 had highlighted the stylized fact that enrollment of 0–2-year olds remained almost stationary over our period of observation, and overlooking constant quantities shall not affect our estimates. However, this choice will imply the need to rescale our point estimates when interpreting them.[13] Note that given the context of a constantly binding supply of public preschool spots, we read preschool *enrollment* rates as our preschool *access* measure.

For our criminal behavior outcomes, we use highly-detailed administrative data from police records, on offenses committed by identified authors in each municipality in the region of Catalonia over the years 2009–2014; Catalan police authority (*Mossos d'Esquadra*). Available information includes date and region of birth of the author, timing and location of the action, and a classification of the type of offense committed. We take the freedom of adopting the generic term "crime actions" to refer to these offenses, although these police records predate the judicial process that will confirm or lift the imputed charges. We construct our outcome variable, 'crime rate per 1000 inhabitants', as follows:

$$O_{c,i,m,y} = \frac{Offenses_{c,i,m,y}}{Residents_{c,i,m,y}} \cdot 1000$$

where $O_{c,i,m,y}$ measures offenses per 1000 inhabitants belonging to birth cohort c, born in region i and living in Catalan municipality m in year y. Resuming our previous example, we construct measures of crime rates among young adults born in the region of Andalusia in the year 1990 and living in the municipality of Barcelona in 2009, 2010, and so forth for all Catalan municipalities and years from 2009 to 2014.[14] The descriptive statistics in Table A.2 and the regional data in Fig. A.2 show that the variance of crime rates is multiple times higher than its mean. This is due to a large mass of zeroes and a long right tail: that is, we observe many instances of zero crimes, and few instances of very high crime rates.

Our main sample consists of 153,924 observations, each referring to a tuple (c, i, m, y) – 'birth cohort c, born in region i, living in municipality m, in year y'. We observe the 10 birth cohorts between 1984 and 1993, born across 18 Spanish regions, living across the 948 municipalities in Catalonia over the 6-year period 2009–2014.[15] Figure A.3 illustrates the composition of our sample indicating the share of observations by each region of origin and each birth cohort: as expected, the single most represented region of origin is Catalonia itself, but the remaining regions constitute a sample fraction going from around 60% for older cohorts to around 45% for younger ones.

5. Empirical strategy

In order to estimate the causal effect of early preschool on criminal behavior, we exploit the variation in preschool access both across regions of origin and across birth cohorts. Since each Spanish region expanded early public preschool following its own timeline, the preschool access faced by each individual at age 3 is given by the combination of her birth cohort and region of origin. Our identification strategy controls for systematic differences in preschool access across regions through

[11] "*Estadística de la Enseñanza en España. Niveles de preescolar, general básica y enseñanzas medias*", Ministerio de Educación y Ciencia, eds. 1987/88 - 2000/2001. We use public enrollment data for our main specifications, but also show results for total enrollment (public + private) in our robustness checks.

[12] Specific source: Municipal Register of Resident Population (*Padró municipal d'habitants*) 2000–2014.

[13] Felfe et al. (2015) instead create age 3 enrollment rates by backwards imputation, starting in 1993. However, this method is based on making arbitrary assumptions about the year-by-year growth rate of enrollment, and since this represents an important portion of our identifying variation, we prefer to keep it as unaltered as possible.

[14] This construction views offenders as part of the resident population in the municipality m in which they commit the offense, thus ignoring the possibility of having crimes "away from home". Criminology literature agrees that in the vast majority of instances, distance-to-crime is well below two miles (for a review of the evidence, see Ackerman and Rossmo, 2015). A more careful treatment of criminal mobility goes beyond the scope of this paper.

[15] Not all cohort-region combinations are found in all municipalities and in all years: that is, in some municipality m and year y combinations, there are no residents born in region i in cohort c. These instances are excluded from the sample.

regional fixed effects, and for systematic differences in access across time through cohort fixed effects, so that only their combination is treated as exogenous.

We opt for a negative binomial (NB) regression model, as it is able to accommodate the over-dispersion and the count nature of our outcomes;[16] in Appendix B we carry out a formal discussion of the NB model setup and show results on model fit to the data. Equation (1) represents our baseline model, which on the right hand side features early preschool access rates $P_{c,i}$, birth-cohort fixed effects C_c and region of origin fixed effects R_i:

$$O_{c,i,m,y} = \exp\left(\beta_0 + \beta P_{c,i} + \sum_c \gamma_{1c} C_c + \sum_i \gamma_{2i} R_i\right) \tag{1}$$

The indexes c, i, m and y respectively denote cohort, region of origin, municipality of residence and year of crime data. This simple baseline specification encloses the core of our empirical exercise: we estimate the relationship between the prevalence of criminal behavior among individuals belonging to a specific cohort and region of origin and their early preschool access rates, after accounting for birth cohort effects and region of origin effects. Our crime data allows us to carry out the analysis with high geographical detail and and yearly frequency, increasing our precision substantially with respect to a more aggregate level – as we show later in our robustness checks.

Departing from our baseline specification, Eq. (1), we gradually allow crime outcomes to depend on further elements. We introduce local fixed effects and local controls, to reduce potential bias stemming from characteristics of the local area in which crime actions are committed. We also introduce year fixed effects to reduce potential bias associated with the timing of crime actions. Finally, we introduce controls for economic conditions that individuals faced in their regions of origin during their preschool time, to alleviate concerns about time-and-region-specific factors correlating to preschool access and co-determining later criminal behavior. Our most complete specification, Eq. (2), can be illustrated as follows:

$$O_{c,i,m,y} = \exp\left(\beta_0 + \beta P_{c,i} + \sum_c \gamma_{1c} C_c + \sum_i \gamma_{2i} R_i + \sum_p \gamma_{3p} V_p + \boldsymbol{M_m}\boldsymbol{\lambda} + \sum_y \gamma_{5y} Y_y + \boldsymbol{Z_{c,i}}\boldsymbol{\delta}\right) \tag{2}$$

where V_p indicate province dummies, $\boldsymbol{M_m}$ indicates a vector of municipal control variables (unemployment rates, number of police officers per 1000 inhabitants, municipal revenues per capita, municipal expenditures per capita),[17] Y_y indicate year dummies and $Z_{c,i}$ indicates a vector of cohort-region of origin controls (GDP per capita and unemployment rates). As will be illustrated in Section 6, our results remain considerably robust throughout these additions, corroborating the validity of our parsimonious baseline specification.

The main assumption underlying the empirical strategy just described is the exogeneity of the staggered preschool expansion pattern with respect to our preschool-crime relationship. As anticipated in Section 3.1, one might fear that our fixed effects and time-varying controls are not able to account for all relevant factors characterizing the region of origin and time period in which our 3-year olds grow up. More specifically, such institutional, economic or cultural circumstances might influence both the preschool access to which children are exposed and their criminal propensity later in time. We address this concern through a compelling placebo test, in which we replace preschool enrollment rates for 3-year olds with the contemporaneous preschool enrollment rates of 4 to 5 year-olds. The intuition is that one would expect any spurious factors driving age 3 enrollment rates as well as criminal behavior later in time to *also* drive age 4–5 enrollment rates – that is, the contemporaneous enrollment of only slightly older peers. Suppose we were to find a relationship between preschool access and later offending behavior only within the exact same birth cohort, but no relationship when using the access rates of slightly-older cohorts, which should be similarly influenced by any regional- and time-specific confounders. Such a result would provide strong support to the hypothesis that age 3 preschool access influenced children's adult offending behavior *directly*, and not because of spurious correlation due to contextual counfounders. The results of this test indeed confirm our prior and are shown alongside our main results in the next section. Among the robustness checks in Section 7 we show a further placebo which corroborates the fact that results do not hold when introducing mismatches between the cohort of treated children and the young adults whose crime outcomes are measured.

Further assumptions of our empirical analysis are the following: i) preschool access does not affect internal migration behavior in significant ways; ii) preschool access does not shift the *timing* of offending behavior in significant ways; iii) region of birth corresponds to region of residence at age 3; iv) private sector dynamics can be ignored. Section 7 discusses each of these assumptions in further detail and provides evidence to support them.

[16] The more naive OLS option is not suitable for neither the count nature of the outcome variable, nor for its over-dispersion; the more traditional Poisson alternative with robust standard errors, instead, produces unbiased coefficient estimates but fits the data decidedly worse than the NB model. Notice that, in our setting, the large mass at zero does *not* call for a "zero-inflated" modeling strategy. The basic assumption behind such strategies is the presence of an 'excess' mass of zero-outcomes, due to the fact that part of the population does not participate in the count-generating process – leading to no chance of observing any outcome different from zero for that part of the population. In our case however, crimes could in principle have been recorded in any (*birth cohort, region of origin, municipality of residence, year of observation*) tuple, so we do not wish to impose zero-counts anywhere a priori.

[17] Because of the large number of municipalities (over 900), we opt for introducing province dummies and municipal-level controls rather than municipality dummies. Following the suggestion of an anonymous reviewer, we will introduce further municipal-level controls in our robustness checks Section 7.1, in particular addressing the aspect of crime reporting rates and their relationship to local internal migration and social capital.

Table 1
The effect of early preschool access on offending rates.

	Negative Binomial					Poisson	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. MAIN RESULTS							
Public preschool rate (0–3)	−3.21***	−1.60***	−1.59***	−1.64***	−1.67**	−1.17*	−47.15*
	(0.67)	(0.60)	(0.58)	(0.63)	(0.66)	(0.63)	(25.59)
B. PLACEBO							
Public preschool rate (4–5)	−0.88***	−0.28	−0.22	−0.23	−0.28	−0.38*	−27.82*
	(0.31)	(0.20)	(0.18)	(0.20)	(0.20)	(0.20)	(15.20)
Test $H_0 : A. = B.$, p-value	0.00	0.02	0.02	0.03	0.04		
Local controls				Yes	Yes	Yes	Yes
Regional controls (at age 3)					Yes	Yes	Yes
Year of birth FE		√	√	√	√	√	√
Region of birth FE		√	√	√	√	√	√
Province FE			√	√	√	√	√
Year FE			√	√	√	√	√
Mean(y)	29.69	29.69	29.69	29.69	29.69	29.69	29.69
sd(y)	160.02	160.02	160.02	160.02	160.02	160.02	160.02
N.obs	153,924	153,924	153,924	153,924	153,924	153,924	153,924
R-squared							0.01
Pseudo R-squared	0.00	0.00	0.00	0.00	0.00	0.06	
α (overdispersion if > 0)	36.29***	35.84***	35.70***	35.24***	35.24***		

Note: Each observation represents cohort 'c' born in region 'i' living in Catalan municipality 'm' in year 'y'. Local controls are measured in crime-observation years y and include municipal unemployment rates; municipal revenue and expenditure per capita; police officers per 1000 inhabitants. Regional controls at age 3 are measured in year $c+3$ and include region of origin GDP per capita; region of origin unemployment rate. Results are not sensitive to changes in the variables used to proxy economic conditions at the region of origin during preschool age or in the Catalan municipalities during the crime-observation window. SEs clustered (1-way) by cohort-region in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Throughout the analysis, we employ one-way clustering of standard errors at the cohort-region level ($n = 252$ clusters). This choice is mainly driven by the fact that we are not able to include cohort-region fixed effects to our model, since that is the level at which our treatment varies; we believe it is particularly important to adjust standard errors for the potential residual correlations between observations belonging to the same birth cohort and region of origin, i.e. the level at which the treatment is assigned (Abadie et al., 2017). The robustness of results to different inference options, in particular targeted at accounting for residual correlation between regions or cohorts, has been verified through additional background exercises not shown but available upon request.

6. Results

Table 1 shows the estimations we obtain from the models illustrated above.

Looking at the main results in Panel A, Column 1 shows that the correlation between early preschool exposure and crime rates is strongly negative: that is, higher levels of early preschool are on average associated with lower crime rates in the data. Column 2 shows the results of our baseline model (1), which represents our preferred specification. The following columns show the results of increasingly rich specifications; the estimation of the most complete model (2) is shown in Column 5. As we can see, the estimates for public preschool rates remain robust throughout the inclusion of various local and time fixed effects, as well as time-varying controls proxying for economic conditions at the region of origin and at the municipality in which crimes are committed. This corroborates our assumption that in the framework of our model, region-and-cohort-specific variation in public preschool rates can be considered exogenously assigned.[18] Columns (6) and (7) illustrate the results of estimating our most complete specification through Poisson and Ordinary Least Squares instead of the Negative Binomial strategy.

Panel B shows the results of our placebo test. When we replace age 0–3 enrollment rates in year $c+3$ with age 4–5 enrollment rates in year $c+3$, the relationship with criminal behavior falls close to zero and loses any statistical significance. This result adds to the previous evidence that time-varying regional conditions characterizing children's early years are not explaining the relationship between preschool access and later criminal behavior. Preschool enrollment and crime are related only when they refer to the exact same birth cohort, while enrollment figures of slightly older peers – which we expect to be similarly influenced by any time-varying regional confounders – do not correlate with the outcome.

Our main results imply that, for the average birth cohort in our sample, a 1 percentage point increase in age 0–3 public preschool access turned into 1.6% fewer crime actions during young adulthood over the years 2009–2014, after taking into

[18] Another way to express this is that, conditional on birth cohort and region of origin fixed effects, potential (or 'counterfactual') criminal behavior C^0 of each group of residents is mean-independent of the level of public preschool exposure it received: $E[C^0_{c,i}|C_c, R_i, P_{c,i}] = E[C^0_{c,i}|C_c, R_i]$, where $C_{c,i} = C^0_{c,i}$ if $P_{c,i} = 0$.

account region of origin, birth cohort, time and local fixed effects, as well as a battery of time-varying controls.[19] However, an increase in enrollment among 0–3-year olds is in fact mostly driven by 3-year olds, as was discussed around Fig. 1. Now taking the simplifying assumption that enrollment among 0–2-year olds remained completely static over the period studied, we estimate that a 3 percentage point increase in public preschool access of 3-year olds yielded 1.6% fewer crime actions during young adulthood, on average.[20] That translates into a reduction of approximately 0.5 offenses per 1000 inhabitants, over a baseline mean of 29 offenses per 1000 inhabitants. We can also use our estimates to relate the cross-regional variation in enrollment to the cross-regional variation in offending rates: we find that the cross-regional variation in early-preschool enrollment rates during the years 1987–2000 explains 15% of the variation in offending behavior across regions of origin during the years 2009–2014.[21]

These impacts are quite large, but still substantially lower than those found by evaluations of the small-scale preschool programs in the US – a fact that may be explained by their exclusive focus on disadvantaged children, while our estimates reflect average effects across the whole population that benefited from higher preschool access. Also the large-scale studies estimating the effects on crime of changing minimum school-leaving ages typically find impacts that are up to five times larger with respect to ours (for a review of this literature, see Section A.2). Beyond considering shorter time horizons, crime reduction in those studies typically includes the pure incapacitation effect derived from spending time at school instead of on the streets.

6.1. Evidence from catalan cohorts

Following the suggestion of an anonymous reviewer, we have carried out an additional exercise to reinforce our main results. If the increased preschool access has a negative effect on crime propensity, one should be able to focus on the region of Catalonia – which is particularly well represented in our sample – and detect a difference in criminal propensity among adjacent cohorts of 3- and 4-year olds affected by the preschool expansion. There are two aspects to recall, in this context: the first is that there is no actual “before and after reform” in our data, given that the public early preschool expansion started in the early 1990s for Catalonia and protracted until the end of our observation window. Nevertheless, we can identify an “intensive expansion” period, covering the first half of the 1990s. The second aspect is that after the intensive preschool expansion started, *all* cohorts of children are started benefiting of higher early preschool access, although in different and increasing levels.

This said, the exercise we have carried out delivers results which confirm the priors stated above. Fig. A.4 shows the (standardized) differences in crime rates between individuals born one year apart: raw differences in Panel A and differences conditional on cohort and local characteristics in Panel B. As one would expect, during the ‘flat’ preschool access period (1980s), it does not matter whether an individual was born one year or the following, in terms of his/her crime outcomes later on. However, during the “intensive preschool expansion” period, being one year younger starts to matter, since younger individuals benefit of higher preschool access with respect to their 1-year-seniors: indeed, we can observe how during this period the crime propensity differences drop, implying lower crime for slightly younger individuals. The drop in the differences is more marked in the raw data than in the series adjusted for cohort and municipal characteristics, revealing that other institutional changes are likely to have contributed to an overall reduction in crime alongside the early preschool expansion. Finally, we can add that one would expect a deceleration in the difference between adjacent cohorts for the late-1990s, when the expansion slows down; unfortunately, these cohorts are too young to be included in our crime observation window and this hypothesis remains unverified for now.

6.2. Discussion on channels

Good quality preschool programs have been shown to improve both cognitive and non-cognitive or socio-emotional skills in children – and both of these channels may lead to a reduction in criminal activity. An improvement in cognitive skills lowers crime propensity through better economic opportunities and higher income from legal activities – as in the original model by Becker (1968). Improvements on the non-cognitive dimension, such as more self-control and lower tendency to aggressive behavior, reduce crime prevalence through criminal impulse moderation (among many others, see Brownfield and Sorenson, 1993; Burt et al., 2006; Burton Jr et al., 1998; Cauffman et al., 2005; Hirschi, 1969; Junger and Tremblay, 1999; Keane et al., 1993; White et al., 1994; Wikström and Svensson, 2010; Wikström and Treiber, 2007). Felfe et al. (2015) present evidence that the Spanish preschool expansion in the 1990s led to cognitive improvements at age 15, as measured by performance in PISA tests, but to the best of our knowledge, there are no studies measuring longer-run or non-cognitive impacts

[19] Through additional exercises not shown but available upon request, we find that most of the effect appears to be driven by a reduction of offending intensity among values higher than 0, rather than by a complete eradication of crime in some (c, i, m, y) combinations. However, we are not able to shed light on the question of whether this is an intensive- or extensive-level reduction from the individual offenders' point of view.

[20] Given that 3-year olds represent approximately one third of the 0–3-year old population, any given increase in enrollment rates in the 0–3 group implies an approximately 3 times larger increase in enrollment of 3-year olds.

[21] Over the period 1987–2000, the standard deviation of age 3 public preschool enrollment across regions was 13 percentage points; and 3 percentage points for 0–3 enrollment rates. Over the period 2009–2014 the standard deviation in offending behavior across regions of origin was 10.8 offenses /1000pax. Our estimates imply that increasing age 0–3 public preschool enrollment by 1 cross-regional standard deviation decreases offending rates by 15% of a cross-regional standard deviation approximately.

in this context. In our research setting we are not able to directly check intermediate outcomes such as behavioral improvements among young adults, a method which has been employed by earlier studies to test the channels behind the effects of preschool on crime. Instead, we propose an alternative and novel way to obtain a better understanding of the potential mechanisms at play. Our unusually detailed dataset allows us to construct different offense categories and to estimate our empirical model separately by each of these: heterogeneities in impacts may provide suggestive evidence on the channel leading from more preschool to reduced criminal behavior. Our categories, whose more detailed descriptives can be found in Table A.3, broadly follow the aggregates used in police records, with some rearrangement dictated by criminology literature. There are so-called "economic motivation" crimes (44.61%), which basically include thefts and robberies; the category of "damages to property" (4.87%) and the category "fraud and falsity" (4.54%). These first three groups roughly correspond to crimes whose underlying motivation is appropriation of material wealth and whose nature is rather premeditated. Further categories are "pure violence" (18.71%) against other individuals, such as injuries and threats; "drugs/addictions" (2.59%); "sexual" crimes (0.57%) and "rules compliance", such as violations of parole, of public order or decency and offenses to public officials (17.91%). Moreover, there are crimes committed within family boundaries, separated into "gender violence" (1.27%) and the more generic offense "against the family" (4.94%), which include non-sexual mistreatments of family members as well as failure to provide economic support or adequate child care. These offense categories tend to be less planned and rather driven by impulsive behavior, and in this sense they feature a tighter relation with the non-cognitive skill spectrum.[22]

Table 2 presents results by disaggregated categories of offenses. With few exceptions, the estimated impacts of increased preschool access are negative across all crime categories and model specifications. We observe that, overall, crime types which are relatable to non-cognitive skills show larger and more significant estimated impacts of preschool access, with drug crimes leading both in terms of size and significance of the effect. A one (three) percentage point increase in age 0–3 (age 3) preschool access reduces the number of drug crimes by between 8 and 9% in youth and young adulthood.[23] The categories of violent crimes and rules compliance also show particularly robust patterns of negative point estimates across all specifications, with magnitudes well above our average result.

Notably, our large-scale and universal-access results are completely aligned with those found on disadvantaged preschool program users in the US (Campbell et al., 2002; 2008; Heckman et al., 2010b; Campbell et al., 2014; Conti et al., 2016), where reductions in drug use and reduced participation in violent crimes are particularly salient among the estimated benefits. The influential paper by Heckman et al. (2013) proposes a decomposition of the effects of the Perry program into cognitive skill- and personality skill-related. The authors show negligible effects of program-induced increases in IQ in explaining treatment effects, among which important crime rate reductions. In fact, while IQ enhancements through the Perry program turned out short-termed, improvements in personality skills have been found persistent by earlier studies (Carneiro and Heckman, 2003; Heckman, 2000). The authors claim that the largest contributions to reduced criminal activity in young adulthood – as well as better labor market outcomes and health behaviors – are attributable to the substantial improvements in externalizing behaviors (aggressive, antisocial and rule-breaking behavior) induced by the preschool program. Our own results by offense categories also suggest prominence of the non-cognitive skills channel in explaining the reduction of adult criminal propensity. We infer that, like in several other international experiences, the Spanish public preschool expansion has succeeded in improving aspects such as socio-emotional wellbeing, self-control, aggressive behavior and anger management. In the long run, this translated into a particularly strong crime inhibition effect on the most impulsive offense categories.

As a final remark, we would like call attention to the fact that the early preschool effects that previous literature and ourselves have identified are not necessarily an exclusively *direct* effect of the additional formal education received.[24] It may well be that part of the effects of preschool expansion operate through extra-school channels, such as family resources and parental time, which are potentially affected by the expansion. For instance, one plausible story is that parents and especially mothers would be able to dedicate more hours to work due to the higher availability of preschool, so that children would benefit of a wealthier family background, which in turn would have a positive impact in cognitive and non-cognitive development and thus reduce offending behavior. On the other hand, this relationship has been found not to hold in all countries and settings (Havnes and Mogstad, 2011a), so that it would be interesting to analyze it for the Spanish case. It is also plausible that most young children would be exposed to a lower adult-to-child ratio in preschool with respect to a family-care arrangement, which has been shown to have detrimental effects on specific subgroups of children (Fort et al. (2020)). Exploring the contribution of these and other extra-school channels to the crime-reducing effects we find in our analysis would constitute a promising avenue for future research.

[22] It is worth noting that crime actions might be driven by multiple or mixed motivations. For example, not paying spousal support to the divorced partner – which in our classification is part of the offenses against the family – may be driven by financial reasons or be an emotional reaction to the separation. However, criminology literature supports the fact that specific offense categories – such as violence outbursts, drug use or sexual assaults – are more relatable to the non-cognitive skill spectrum than others – such as fraud, theft and robbery. We ignore three types of crime that occur very rarely in our Catalan dataset: crimes against the environment (0.32%), murder (0.27%) and arson (0.06%). Beyond their low counts, it is difficult to categorize these items across our groups.

[23] It is important to note that the Catalonian drug offense data we employ is mostly related to consumption and petty dealing tied to consumption, and that this does not include organized crime distributing drugs on a large scale.

[24] We thank an anonymous reviewer for pointing out this aspect.

Table 2
Impacts on disaggregated offense categories.

	Negative Binomial				
	(1)	(2)	(3)	(4)	(5)
Economic motivation					
PPR (0–3)	−2.33***	−1.58**	−1.62**	−1.41	−0.62
	(0.35)	(0.77)	(0.79)	(0.93)	(0.99)
Damages to property					
PPR (0–3)	0.54	1.82	1.81	1.71	−0.06
	(0.56)	(1.57)	(1.65)	(1.93)	(2.33)
Fraud and falsity					
PPR (0–3)	−6.77***	−3.21**	−2.64	−3.26*	−3.39
	(0.66)	(1.61)	(1.65)	(1.87)	(2.36)
Pure violence					
PPR (0–3)	−2.86***	−1.58	−1.86*	−2.76**	−2.32*
	(0.33)	(0.98)	(1.00)	(1.17)	(1.36)
Rules compliance					
PPR (0–3)	−4.57***	−1.36	−1.13	−0.72	−2.37*
	(0.33)	(1.10)	(1.06)	(1.07)	(1.32)
Drugs					
PPR (0–3)	−3.22***	−6.98**	-7.65***	-8.22***	-9.15**
	(0.92)	(2.94)	(2.91)	(3.06)	(3.71)
Against the family					
PPR (0–3)	−5.79***	−1.43	−1.08	−2.22	−0.41
	(0.50)	(1.64)	(1.72)	(2.22)	(2.70)
Gender violence					
PPR (0–3)	−5.13***	4.34	2.76	0.54	−1.47
	(1.03)	(2.74)	(3.15)	(3.36)	(3.63)
Sexual crimes					
PPR (0–3)	1.87	0.87	0.10	4.43	−5.36
	(1.96)	(4.82)	(5.09)	(5.44)	(6.36)
Local controls				Yes	Yes
Regional controls (at age 3)					Yes
Year of birth FE		√	√	√	√
Region of birth FE		√	√	√	√
Province FE			√	√	√
Year FE			√	√	√
N.obs	153,924	153,924	153,924	153,924	153,924

Note: The table reports early preschool exposure coefficient estimates for different types of offenses. Each cell of the table comes from a separate regression: each line refers to a different sample of a specific offense category; each column represents a different model specification employed, replicating those of our main results Table 1. SEs clustered (1-way) by cohort-region in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.3. Benefit-to-cost calculations

Our disaggregated results allow us to provide an approximate cost-benefit analysis of the Spanish preschool expansion, in which we compare the economic value of crime reduction to the cost of the expansion. Being limited to the sole crime dimension, our exercise is much less comprehensive than the one performed by Heckman et al. (2010b) for the Perry Preschool Program, which considers a range of benefits to society beyond crime reduction – such as better education, employment, earnings and health. Further, rather than addressing the full, lifetime rates of return of the program, we limit the exercise to calculating the average annual benefit-to-cost ratio over our period of observation (2009–2014).[25] We recover the unit cost of crime from recent international data, for two thirds of our offenses. We obtain an assessment of the annual monetary worth of preschool-induced crime reduction, using the point estimates from our main results. We then relate those benefits to the cost of the expansion program, based on unit costs of one extra preschool spot in Spain. In Appendix C we illustrate the precise data used and the calculations performed.

For the case of Catalonia in the year 2000, a 1 percentage point increase in public preschool enrollment rate would imply increasing age 0–3 enrollment rates from 22.3% to 23.3%, which translates into 4710 new spots, with an associated yearly cost of EUR 26.8 million (approx. 0.1% of the regional budget). The lower-bound estimate of associated benefits from crime reduction is EUR 113.9 million, which yields a benefit-to-cost ratio of around 4.25 at the very least.[26] One could further decompose that ratio distinguishing between “public” and “private” costs of crime. Looking at the reduction in public costs

[25] Calculating lifetime rates of return would imply the need to compute present values of *lifetime* costs and benefits of preschool expansion. In our data we observe crime committed by individuals aged 15–30: even though it corresponds to the age range where crime generally peaks, it still represents an incomplete share of the potential criminal life on an individual.

[26] The figure represents a lower bound estimate because it ignores around one third of offense on the benefits side, and because we use the more conservative crime cost estimates when several ones are possible. Through simple imputation of our average unit cost to the offense for which we are missing this information, our benefit-to-cost ratio would raise to 6.33.

only (EUR 37.1 million), we estimate the benefit-to-cost ratio at around 1.4. Looking at the reduction in private costs only (EUR 76.8 million), the ratio would be around 2.9. As a comparison, for the US Perry Program, Heckman et al. (2010b) report a benefit-to-cost ratio of 7.3 only in terms of crime reduction,[27] while Rolnick and Grunewald (2003) obtain a ratio of 5.7 for the same program.

Although recognizing the limitations of the simplified approach taken in this exercise, we believe it helps us providing a useful ballpark estimate of the economic advantages brought about by the Spanish preschool expansion. Considering nothing else than the advantages in terms of crime reduction, the monetary value of benefits to society as a whole exceed costs fourfold at the very least, and is well worthwhile even with reference to the 'public budget' only.

7. Assumptions and robustness

7.1. Internal migration

7.1.1. Effects on sample composition

The fact that we only observe offenses committed in the region of Catalonia implies that our crime rates by region of origin are based on the population of internal migrants – i.e. those individuals that have migrated from their region of birth to Catalonia during their youth or adulthood. The late 1980s and 1990s were periods of intense internal migration for Spain, due to the economic upturn and associated social dynamism the country witnessed, so that at the beginning of the 2000s almost a quarter of the population was residing in a region different from their birth (Romero-Valiente, 2003). Besides labor market incentives, migration was driven by residential preferences and the last return flows following the end of Franco's regime. During those years, Catalan provinces registered positive net migration flows which were above the national average although not exceptional. There are two aspects to internal migration that might affect the interpretation of our estimates. First, if preschool access affects internal migration decisions as well as offending behavior, we might run into an issue of sample selection. Specifically, our negative impacts on crime counts might reflect changes in the sample of Catalan residents we observe, rather than true reductions in offending propensity. Second, internal migration might jeopardize external validity, if internal migrants are intrinsically different with respect to the general population. We address these concerns by investigating whether our preschool access measure has any predictive power on the quantity or quality of internal migrants that we observe in Catalonia, and how the observables of internal migrants compare to non-migrants.

Internal migrants are individuals belonging to our birth cohorts (1984–1997) that reside in Catalonia during our observation window (2009–2014) and were born in a different Spanish region. We address the migration *quantity* aspect in Table A.4, which shows OLS regressions of the share of internal migrants on potential migration determinants, including preschool exposure. After accounting for cohort and region of birth fixed effects, we do not find any evidence for preschool exposure affecting the probability of migrating to Catalonia. Second, we look at the *quality* of internal migration. We identified our birth cohorts in 2011 Census data[28] and we classified individuals into migrants or stayers according to their residence and birth information. We look at individual characteristics of these two groups, such as education level and labor market activity, and cross-tabulate these against quintiles of preschool access. We would be worried if higher/lower preschool access were predictive of a gap in characteristics opening between migrants and stayers, as this would indicate an effect of preschool access on internal migration behavior. Fig. A.5 illustrates our exercise graphically and Table A.5 shows the detailed figures underlying the graphs. Differences in education or labor market activity between internal migrants and stayers seem i) not very large in general, and ii) not to exhibit trends or specific regularities across the preschool access distribution. Fact i) reduces external validity concerns; fact ii) fails to reveal any systematic effects of preschool access on migration choices, in line with our previous findings on migration quantity.[29] In conclusion, our results indicate that despite remaining a limitation of our empirical setup, migration-induced selection appears to be mild in our context of study.

7.1.2. Timing of migration

A further aspect to the migration to Catalonia is that its timing may be heterogeneous across individuals and regions of origin, occurring during early childhood for some and during young adulthood for others. While this is harmless if following a random pattern, it would be a source of concern if there are systematic features to this heterogeneity, which might correlate with unobserved confounders characterizing regions of origin. In particular, one could be worried that the environment faced at the region of origin *beyond age 3* continues to play a relevant role towards shaping criminal behavior later in time.

We do not have information on the year in which individuals moved from their regions of origin to Catalonia. However, we are able to mitigate the above-mentioned concerns by adding controls specific to each region-of-origin and birth cohort

[27] See their Table 6. The case that best approximates our scenario is the '0% discount rate' and Low Murder Costs. In fact, our analysis does not include murders, whose costs to society are estimated to be very high in their analysis. Excluding murders further reinforces the lower-bound nature of our benefit-to-cost estimate.

[28] We employ the 5-percent Census microdata sample available for public use (from the website of the National Statistics Agency, www.ine.es.)

[29] In a similar exercise shown in Table A.6 we perform logit and ordered logit regressions of migrants' observables on preschool exposure, cohort and birth region fixed effects. This strategy also fails to spot any correlation between migrants' characteristics and preschool. We repeat the quintile-based exercise for internal migrants to Catalonia only, which yields noisier results due to very small sample sizes for migrants. The graphical results are reported in Fig. A.6.

for ages beyond 3. In Table A.9 we show the results of augmenting the vector of region-cohort specific controls $Z_{c,i}$ in our most complete specification, Eq. (2), with control variables spanning further childhood years (row C1, ages until 12) as well as adolescence years (row C2, ages until 18). In fact the estimate of our coefficient of interest becomes somewhat larger and stronger with respect to our main results, indicating that if anything, the bias stemming from non-random timing of internal migration to Catalonia may be downplaying the effect of early preschool on offending behavior in our more parsimonious specification, which only includes controls for conditions faced at age 3.

7.1.3. Local social capital

Internal migration may directly or indirectly affect the level of social capital and institutional trust recorded at the local level.[30] This aspect deserves discussion in our setting, given that it is plausible to think that crime reporting rates may vary with local social capital and trust and thus be connected to the local internal migration rates that provide the base for our identifying variation.

If local immigration rates change local crime reporting rates because of the lower integration, different trust in institutions, different culture or different social capital characterizing internal migrants, other variables capturing local social capital and institutional trust, should be affected in a similar way as reporting rates.

The two variables we use to proxy local social capital and institutional trust are local association formation and local election turnouts, both of which have been extensively endorsed by literature on this topic. Associations are legal entities registered following The Catalan Associations Act, and they are defined as non-profit organizations, voluntarily formed by three or more individuals to serve a general or specific interest, through the sharing of personal resources, temporarily or indefinitely. There are different types of associations based on their main activities (e.g., cultural, civic rights, scientific knowledge, health and social welfare, etc.). Very common are those that promote civic rights in a particular place - typically, neighborhood associations.

We gather information on the association density per municipality and year and we compiled election turnout shares over various elections held over our observation window (2009–2014). We use standardized turnout rates at municipal level for two regional elections (2010 and 2012), two European elections (2009 and 2014) and one general election (2011); for the year 2013 we have imputed the values of the regional election held in 2012.

We re-estimate our models including the new local social capital proxies among our municipal controls and show the results Table A.9 (row C3). Reassuringly, our main results remain robust to the inclusion of the new social capital / institutional trust proxies and, if anything, the point estimates of the effect of early preschool access increase slightly.

7.2. Crime-age distributions

The interpretation of our main results may change if preschool merely changed the *timing* or the *black* age dispersion of offending behavior among youths, rather than its intensity. Similarly to the concerns about migration behavior, if preschool affects the timing of crime, our results might reflect a change in the observed sample rather than a true treatment effect. Through the regression analysis shown in Table A.7 we verify whether characteristics of the crime-age distributions are correlated with our preschool access measure. The dependent variables are peak crime age, low crime age, peak-to-low ratios, standard deviation and kurtosis of the crime-age distribution; independent variables are preschool access and the usual cohort and region fixed effects. We do not find preschool to carry any predictive power on the shape characteristics of observed crime-age distributions, further mitigating concerns about compositional effects underlying our results.

7.3. Region of birth and region of residence

Given that both our data on offenders and our data on population residing in Catalonia provide region of birth but not region of residence at age three, we assume that individuals were still residing in their region of birth at age three, and were thus exposed to the preschool expansion pattern that occurred in that region. This assumption, and in general the assumption of negligible mobility of children across the geographical areas at which analysis is conducted, is very common in papers with a structure similar to ours (including Blanden et al., 2016; Duflo, 2001; Felfe et al., 2015).

Those individuals who were *not* residing in their region of birth at age three are allocated a wrong preschool access level in our analysis. If completely at random, the presence of such mismatches weakens any true relationship that there might be between outcome (crime) and predictor (preschool); nevertheless, a non-random misallocation could potentially bias our results in any direction. Mobility of children under the age of 3 is in fact quite low across Spanish regions, as we can observe in Table A.8, however we correct for it following the weighting method by Card and Krueger (1992). We construct migration probability matrices between Spanish regions, using cohort and age-specific migration data from residence variation records kept by the Spanish National Statistics Agency (*Encuesta de Variaciones Residenciales* 1988–2000, INE).[31] For each birth cohort c, the matrix P^c cross-tabulates region of birth (i) and region of residence at age three (j), and its elements p^c_{ji} indicate the

[30] Thanks to an anonymous reviewer for bringing up this point.

[31] Complete yearly migration data is available for birth cohorts 1988 onward. Migration records for earlier cohorts are missing for ages 0 (1987), 0–1 (1986), 0–2 (1985) and 0–3 (1984). To the missing records, we impute the age-specific migration rates of the immediately younger cohort – which are likely to represent slight over-estimates of the true rates, given that mobility trends are increasing over time.

probability of residing in any region j at age three, while being born in region i. Based on these probabilities, for each birth cohort c and region of origin i, we construct mobility-weighted preschool access measures $P^{*}_{c,i}$ as follows:

$$P^{*}_{c,i} = \sum p^{c}_{j,i} P_{c,j}$$

where $P_{c,j}$ are the age three preschool exposure rates for cohort c in region j. We then re-estimate our main regression models using $P^{*}_{c,i}$ instead of the original, unweighted $P_{c,i}$, and report results in Table A.9, row C4. As we can see, estimates show only marginal changes with respect to the original ones, confirming the inconsequential role played by preschool-age mobility in our context.

7.4. Private sector

We address any residual skepticism about the potential influence of the sluggish private sector dynamics mentioned earlier, when describing Fig. 1. Given the characteristics of our empirical model, and that it was the public sector expansion driving the variation in our data, we do not expect that ignoring the private sector is relevant to our results. We estimate our models using total preschool access (public and private), instead of public only. Indeed, as we see in Table A.9, row C5, coefficients slightly decrease due to the higher mean of the new predictor variable, but all result patterns remain unaltered.

7.5. Additional robustness and placebo

We test whether our conclusions are sensitive to a refinement to our Negative Binomial model, that is accounting for different amounts of exposure. The likelihood of observing offending behavior might change across observation units, depending on the amount of population represented by each of them: in our dataset, we are more likely to observe criminal behavior on larger resident groups than in less-represented groups, for any given underlying population behavior. In Table A.9, row C6, we thus show results of our exposure-augmented model, in which the exposure variable is given by log of the number of residents for each cohort-region-municipality-year combination. In fact, the estimated impact of preschool access on offending behavior is actually somewhat larger once exposure is accounted for, with respect to our more conservative main specification.

In the following rows of Table A.9 we test the robustness of our results against the exclusion of specific regions from our dataset. In row C7 we exclude regions which are islands or territories (Canary Islands, Balearic Islands, Ceuta and Melilla): beyond being both culturally and institutionally different from the Spanish mainland in several aspects, they are small and present the highest variability in offending behavior (see Fig. A.2). In row C8 we exclude the two largest regions in our sample, Catalonia and Andalusia, to verify that our results do not hinge exclusively on these. In row C9 we exclude those regions which present irregular enrolment rate patterns, potentially due to missing data or misreporting issues (Comunidad de Valencia, Galicia; see Fig. A.1). For all three sample restrictions, the estimated relationship between preschool access and crime remains negative and sizeable; however when excluding the two largest regions we lose statistical significance as well as around 30% of magnitude.

We allow for changes in the level of geographical aggregation of our data, in order to rule out any spurious correlations specific to the municipal level of analysis. Row C10 and C11 in Table A.9 show results at the provincial and regional level respectively. Crime rates by birth cohort and region of origin, as well as local economic controls, are now computed at these two higher geographical levels (see descriptives in Table A.10). Point estimates remain in the same ballpark as the original ones at the municipality level, but standard errors inflate. These checks confirm the absence of any spurious correlations specific to our municipal level of analysis. At the same time, they reiterate the remarkable value of the unusual level of geographical detail in the crime data we are able to employ, which allows to pin down effects with greater precision.

Finally, we conduct a second placebo test to remove concerns of accidental correlation between preschool access (P) and offending behavior (O) across time or across space. We repeatedly simulate results from our model after replacing the true birth cohort or region of origin with random different ones. The intuition is that if no genuine relationship between P and O existed and our main results were the result of chance, we should be able to obtain similar estimations from a portion of those simulations. Fig. A.7 shows the outcomes of estimating Eq. (1) one-hundred times, with a placebo cohort (Panel (a)) or a placebo region of origin (Panel (b)) assigned to each group of residents, and compares these to our original results. In both experiments, it is striking how our original estimates are far off any simulation obtained from fictitious birth cohorts or regions of origin. In the case of random birth years, estimates are concentrated around zero; in the case of random birth regions, if anything, estimates tend to reach statistical significance on the positive side of the spectrum. These results once again confirm that the negative and significant relationship between preschool access and later offending behavior exists only when cohorts and regions of origin are correctly matched between treated children and observed young adults.

8. Conclusions

Our analysis on the Spanish preschool expansion has shown that the effectiveness of good-quality preschool programs in reducing crime during youth and young adulthood scales up and holds even in the context of nationwide, universal-access programs. We find that on average, crime types which can be related to the non-cognitive skills sphere show the largest and most significant estimated impacts of preschool access, with drug crimes and pure violence leading both in terms of size and significance of the effect. We interpret this as evidence suggesting that the influence of preschool on the non-cognitive skill dimension is strong, persists into adulthood and is powerful enough to display effects on antisocial behaviors. Our estimated effect magnitudes are smaller with respect to those found on reduced samples and disadvantaged contexts in the US, but our back-of-the-envelope calculations suggest that the economic value of crime-reduction is still multiple times larger than the program cost.

Compared to the existing US analyses, our study of the Spanish context presents a few disadvantages. The probably most significant one is the unavailability of skill data for our population of children or young adults: in this sense, we are missing an empirical measurement of 'intermediate outcomes' between preschool access in early childhood and criminal activities in young adulthood (such as in Heckman et al., 2013). Being able to provide 'first-stage evidence' on cognitive and/or non-cognitive skills improvements among program participants would represent an obvious gain for this study. Alas, being able to track individual program participants and to test their skills is unusual in the context of universal-access programs, and more typical in contexts characterized by a restricted and supervised participation. On the other hand, we are proposing an outcome-driven approach to infer the importance of the cognitive versus non-cognitive skills channels in explaining effects on crime, taking advantage of the high quality crime data at our disposal. With respect to the analyses by Heckman and coauthors, and to other papers evaluating preschool programs in the same spirit, our crime categories are finer and more detailed, allowing to relate crimes to either skill group with some confidence. We view this as a novel and valuable contribution to the literature which, beyond being applied to the evaluation of the Spanish preschool experience, may prove useful as a research method in other similar contexts. Furthermore, despite the fact that our sample is the result of internal migration, we trust that our study offers a wider angle on the relationship between preschool and adult criminal behavior with respect to analyses based on small-sample and restricted-participation programs.

Finally, it is worth dedicating some thought to the distribution of our treatment effect across the population of Spanish children which was affected by the preschool expansion program. Existing literature strongly suggests that children raised in disadvantaged socio-economic backgrounds are those benefiting the most from the introduction of good quality preschool programs (Guryan et al., 2008; Kimmel and Connelly, 2007). Regrettably, the lack of socio-demographic information on offenders makes us unable to analyze heterogeneity in the effect across individual characteristics and empirically validate this prediction for the Spanish case. As a second-best attempt, in Table A.11 we show heterogeneity of the impact of preschool access on crime by relative economic advantage of entire birth cohorts. We find weak evidence for a larger effect for cohorts who found themselves in a relatively disadvantaged economic context at age 3, our indicators of disadvantage being a) below-median GDP per capita; b) above-median unemployment rates; c) hardest-hit by the 1993 Spanish economic crisis. However, these estimates are quite noisy, and unsurprisingly so, since the importance of the heterogeneity in average socio-economic background across cohorts does not compare to the heterogeneity across individual characteristics *within* cohorts. However, all things considered, there are reasons to trust that also in Spain the public preschool expansion program helped reducing inequalities in society by benefiting disadvantaged children more than those from wealthier families, with sizeable repercussions on offending behavior in youth and young adulthood.

Declaration of Competing Interest

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Appendix A. Literature review supplement

A1. Preschool education and adult behavior

We present a short review of studies on universal or very-large-scale preschool initiatives looking at short-run behavioral outcomes. In fact, even in the short-run, evidence on behavioral impacts of preschool obtained from large populations is discordant. Loeb et al. (2007) use data from the Early Childhood Longitudinal Study and conclude that center-based preschool education has overall negative effect on socio-behavioral measures at the start of kindergarden. On the contrary, Figlio and Roth (2009) use differences in access within a family and find that, in Florida, public pre-kindergarden participation reduces behavioral problems in elementary school – especially in disadvantaged neighborhoods. Also Berlinski et al. (2009) find positive results in Argentina, where third-graders improved attention, participation, discipline and effort following attendance of pre-primary education. Gupta and Simonsen (2010) find that in Denmark, compared to home care, enrollment in preschool programs at age three does not lead to any significant differences in children's outcomes at age 7. Felfe and Lalive (2018) find that public preschool rollout in one German state induced significant socio-emotional skill improvement measured at school-entry age, but especially so for children with a lower propensity to attend preschool in first place.

Next, we summarize research contemplating long-horizon effects of large-scale preschool initiatives, focusing on strictly academic or labor market skills. Several studies find strong cognitive-skill effects for disadvantaged groups but small or negligible effects on the remaining population (Blanden et al., 2016). In Italy, Carta and Rizzica (2018) find no effects on school performance following a subsidized expansion of preschool places for 2-year olds. Blanden et al. (2016) exploit the staggered implementation of preschool for three-year olds across Local Education Authorities in England in the early 2000s, finding small improvements in attainment at age 5, but no apparent benefits by age 11, and attributing the failure to low quality of some preschool centers. Universal child care programs in Norway have been evaluated by Havnes and Mogstad (2011b, 2015), who find positive earning effects for children from low-income families but actual losses for upper-class children. Cornelissen et al. (2017) look at an universal preschool program in Germany and exploit its staggered implementation across municipalities, finding that school entry examination gains from the program were particularly high for children from disadvantaged backgrounds. In Europe, Dumas and Lefranc (2012) evaluate the effect of the expansion implemented in France over the 1960s and 70s, finding persistent and inequality-reducing effects on educational and labor market outcomes. Finally in Spain, Hidalgo-Hidalgo and García-Pérez (2012) find a positive impact of preschool on results in language, mathematics and reading using TIMSS-PIRLS data; Santín and Sicilia (2015) find a positive impact of more years of preschool on academic results at 4th grade; Felfe et al. (2015) look at the same public preschool expansion we exploit and find a positive impact on PISA reading and math scores, especially for girls and for students with low socio-economic background.

A2. Large-scale education policies and offending behavior

What follows is a brief summary of studies analyzing the impacts on offending behavior stemming from large-scale education policy initiatives, all of which were targeted to teenage populations. Lochner and Moretti (2004) show that between the 1960s and 1980s in the US, completing high school reduced the probability of incarceration by about.76 percentage points for whites and 3.4 percentage points for blacks, with the biggest impacts on murder, assault, and motor vehicle theft. Deming (2011) finds that black males who won access to their first-choice high school through a lottery commit 50% fewer crimes over the 7 years following assignment. Anderson (2014) finds that minimum dropout age requirements have a significant and negative effect on property and violent crime arrest rates for individuals 16 to 18 years old, between 1980 and 2008. He suggests the incapacitation effect to be the main driver, but notes that other channels such as human capital impacts cannot be ruled out. Again looking at changes in compulsory schooling laws in the US, Bell et al. (2016) conclude that credible causal estimates of the education–crime relationship cannot in general be identified for the more recent 1980–2010 period, though they can for some groups with lower average education levels (in particular, for blacks). Machin et al. (2011, 2012) focus on a 1972 change in the UK school leaving age and estimate that a 1% increase in the proportion of men staying on at school after the compulsory school leaving age reduces male youth crime by between 0.85 and 1.7%, with stronger effects on property than on violent crime. They also pin down significant impacts on other productivity-related economic variables such as qualification attainment and wages, demonstrating that the incapacitation effect of additional time spent in school is not the sole driver of the results. The paper by Buonanno and Leonida (2009) look at Italy over the period 1980–1995 and shows that graduating from high school, as well as attending more years of education in general, has a negative effect on crime rate over and above its effect through labour market returns. In the Netherlands, Groot and van den Brink (2010) show that the probability of committing crimes like shop lifting, vandalism and threat, assault and injury decreases with years of education – but find the opposite result for tax fraud.

Appendix B. Negative binomial modeling and fit to data

Here we illustrate our modeling strategy more formally, following Long and Freese (2014), Hilbe (2011) and Cameron and Trivedi (2010). In negative binomial regression, the outcome variable y_j is assumed to follow a Poisson distribution whose parameter μ_j^* – representing expected counts and their variance – is a function of a covariate vector $\boldsymbol{x_j}$, a parameter vector $\boldsymbol{\beta}$ and an unobserved component ν_j. The unobserved component it is such that e^{ν_j} follows a gamma distribution with mean 1 and variance α, where α is known as the over-dispersion or shape parameter.[32] More formally, the NB regression model can be expressed as follows:

$$y_j \sim \text{Poisson}\left(\mu_j^*\right)$$

where j indexes single observations, and

$$\mu_j^* = \exp\left(\boldsymbol{x_j}\boldsymbol{\beta} + \nu_j\right) = \exp\left(\mu_j + \nu_j\right)$$

and

$$e^{\nu_j} \sim \text{Gamma}(1/\alpha, \alpha)$$

This model allows for dispersion to depend on the expected mean of each observation. However, we do not allow the dispersion *parameter*α to vary across observations, that is, we are estimating a single α for our entire crime dataset.[33]

[32] The unobserved component ν_j is what differentiates the NB model from the Poisson model, allowing for over-dispersion in observed outcomes by adding an observation-specific element. The higher α, the larger the over-dispersion; with $\alpha = 0$ we are back to the Poisson regression model.

[33] In our main analysis we are also abstracting from the so-called 'offset' variables, which can be included into the NB model to allow for outcome events y_j to be observed over different amounts of exposure, and whose coefficients are constrained to 1. We thus consider the amount of exposure to

We assess how well the baseline specification of our negative binomial (NB) model, Eq. (1), is able to fit the distribution of our crime data, and how it performs compared to the more traditional Poisson choice. As a first approach, Table A.12 shows descriptive statistics of the predicted distribution of count probabilities generated by the two modeling options, and compares it to the actually observed counts. While the *mean* of the count distribution is well approximated by both the NB and the Poisson models, the fit of the former is clearly superior to the fit of the latter when dispersion measures are taken into account. The large standard deviation, skewness and kurtosis of actual counts are very well replicated by our NB specification, while the Poisson model fails to do so.

Model fit for count models can also be assessed through a rootogram (Tukey, 1972; Tukey, 1977; Wainer, 1974; Kleiber and Zeileis, 2016), which compares the empirical distribution of a variable to a theoretical distribution. The two theoretical distributions we consider are the Poisson distribution and the NB distribution, characterized by our parameter estimates as in Eq. (1). Count residuals are computed as the difference between observed counts and the counts predicted by the theoretical model. The rootograms in Fig. A.8 plot the square root of these count residuals,[34],[35] with positive bars indicating underestimation of actual counts and negative bars indicating overestimation of actual counts. The Poisson specification (right panel) massively underestimates zero counts and considerably overestimates small counts. The NB model (left panel) performs significantly better, predicting zero counts quite well and over- or underestimating the remaining counts to a lesser degree with respect to the Poisson model. Finally, Table A.13 reports AIC and BIC for both model choices. Unsurprisingly, these criteria too favor using the NB model over a Poisson specification in our context.

Appendix C. Benefit-to-cost calculations

To the best of our knowledge, estimates for the social costs of crime are not available for Spain, and calculating these from scratch are beyond the scope of this paper. Instead, we make use of the estimates for the UK provided by Heeks et al. (2018), which should serve as a good proxy: these are estimates of how much a single crime action costs to society, including costs in anticipation of crime (defense expenditure and insurance administration), costs as a consequence of the crime (value of property stolen, physical and emotional harm, lost output, health services and victim services), and costs in response to crime (police costs and judiciary costs). These unit cost estimates are provided separately by crime type, and may be split into ‘private’ costs – associated with personal defense expenditure, insurance administration, value of property stolen, physical and emotional harm and lost output– and ‘public’ costs – associated with health services, victim services, police costs and judiciary costs. Matching the crime categories available in Heeks et al. (2018) to those present in our analysis,[36] we obtain unit cost estimates for around two thirds of the total offenses recorded in our sample, while the remaining third was not considered by Heeks et al. (2018).

We use our baseline preschool impact estimates to approximate the number of crimes avoided as a result of the preschool expansion,[37] limiting ourselves to those offenses we have cost estimates for. Lastly, we combine number of crimes avoided with unit costs and obtain our estimates of cost of crime savings - which we report in Table A.14. The table shows estimated total and public cost savings separately by crime type. Focusing on public unit costs of crime for thefts is 900.7€ (46.4% of total costs); robberies 7,669.8€ (48.1%); car thefts 5,629.2€ (38.9%); criminal damage 830.3€ (43.7%) and for fraud and falsity is 422.2€ (23.3%). Regarding non-cognitive crimes, for violent crimes we have injuries 4,813.0€ (24.3%); threats 3,293.1€ (39.5%); sexual and gender violence 2,181.3€ (23.8%). For sexual crimes we have used the conservative cost not including rapes whose estimated cost to society is six time higher.

In Table A.14, the Multiplier column shows the inflation factors we use to obtain estimates of the number of crimes actually occurred, based on our information on crimes reported and whose offender is known. Note that both the likelihood of being reported to police and of having the offender ascertained vary greatly by crime type. Using a second dataset for total reported crimes in Catalonia, we obtain a first inflation factor from crimes with known offender to crimes reported. Using multipliers calculated by Heeks et al. (2018) based on victimization surveys, we account for the difference between reported and real crime rates. In Table A.14, we show the final result of this two-step inflation procedure.

To complete our cost-benefit exercise, we compare the savings due to crime reductions with a cost estimate of the public preschool expansion. We take the average cost of a preschool post in 2004 in Catalonia from the analysis of Tarrach et al. (2011), who value it at 5,690.27€. This estimate is consistent with the reported costs in Madrid in 2013.[38] The main results of the benefit-to-cost estimations of a preschool expansion are reported in the main text, Section 6.3.

be homogeneous across our crime counts. In other robustness specifications (not shown, but available upon request), we introduce age as the exposure variable: individual age during our observation window 2009–2014 may determine the likelihood of being observed engaging in criminal behavior. Our results remain unvaried to such additions.

[34] The square root transformation prevents smaller counts to be obscured by larger counts in the graph.

[35] To improve readability, we show counts up to 100 crimes per 1000 inhabitants.

[36] We have matched what Heeks et al., 2018 define as “violence without injury” to our “threats”. When computing the cost reduction impact of a preschool expansion we use for both injuries and threats the results we obtain for our pure violence category. Similarly, for thefts, robberies and car thefts we use the estimated results for our economic motivation category. Finally, we have assigned the unit costs of other sexual offenses to our gender violence typology of crime.

[37] We use our point estimates and ignore the uncertainty around it.

[38] This unit cost is consistent with that reported in the news for Madrid in 2013 of 5100€ see https://www.elmundo.es/elmundo/2013/08/27/madrid/1377605557.html (last accessed April 2020). Moreover informal reports from 2017 point out to a cost of 7000€ per year and pupil in public preschools

Appendix D. Figures

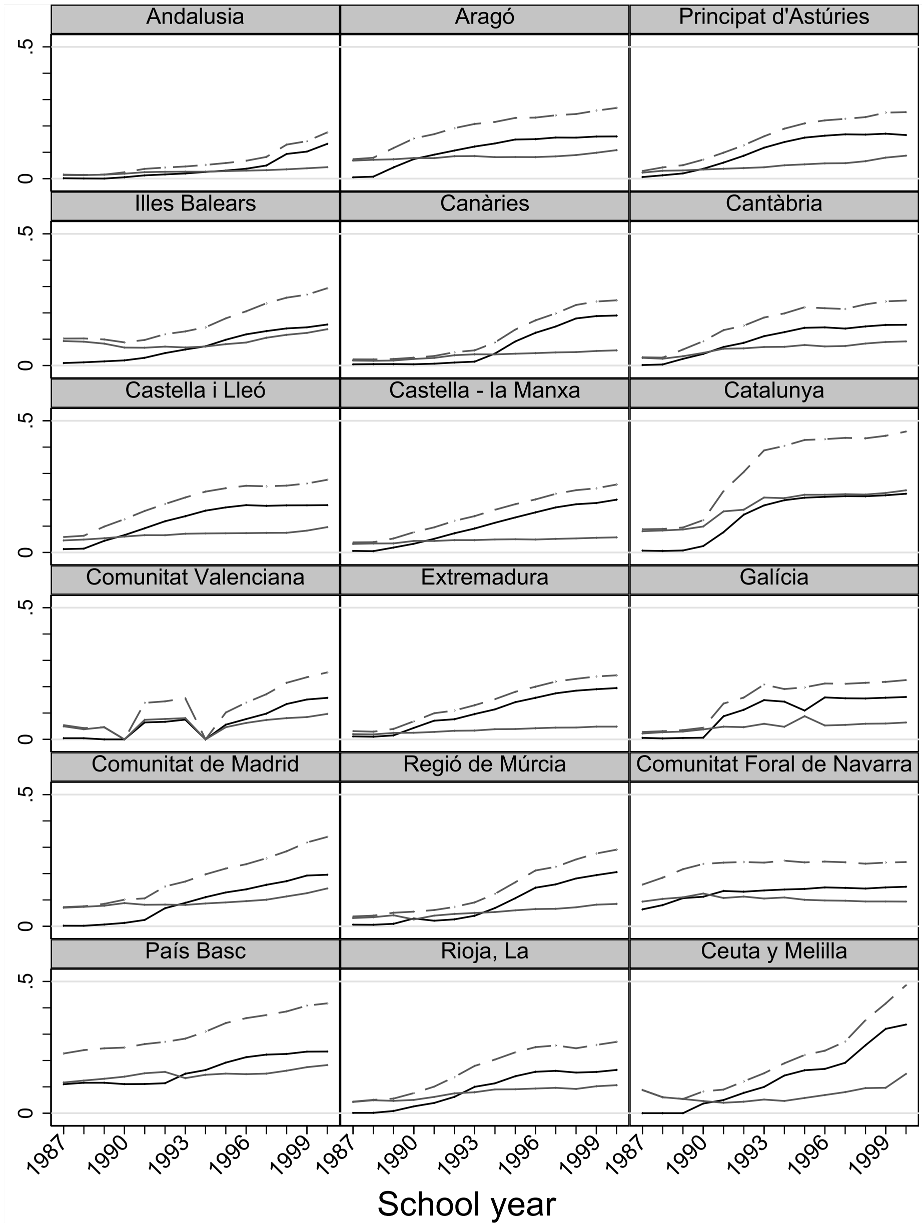


Fig. A.1. Early preschool (age 0–3) enrollment rates in each Spanish region, 1987–2000. *Note:* Age 0–3 enrollment rates by Spanish region and school year. Public preschool rates in solid black, private ones in solid grey and total ones dashed.

in Barcelona, see http://nadaesgratis.es/admin/tarifacion-social-de-las-guarderias (last accessed April 2020). As a reference, the estimated cost of the Perry Program per child in 2006 was $17,759 (see Heckman et al. (2010b)).

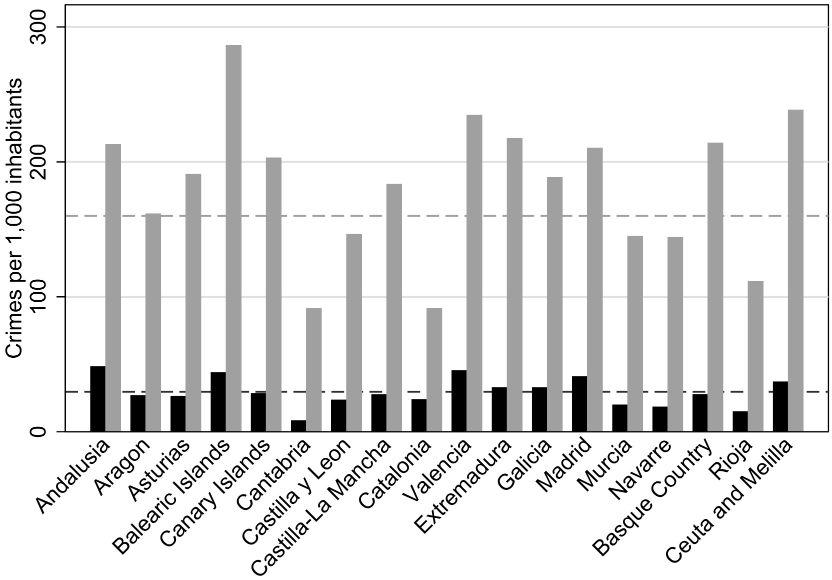


Fig. A.2. Crime rates by regions of origin. *Note:* The figure plots means (black) and standard deviations (gray) of crime rates by region of origin. Dashed lines indicate overall mean and standard deviation.

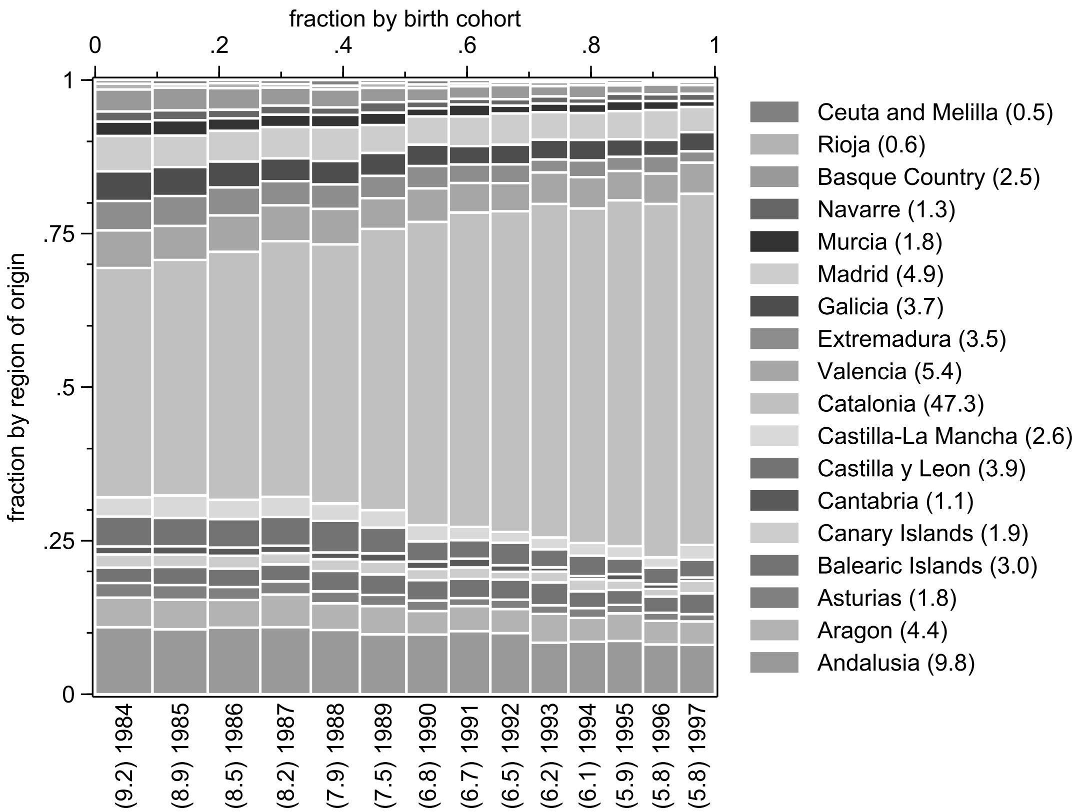


Fig. A.3. Representation of birth cohorts and regions of origin in our sample. *Note:* The figure plots sample fractions by birth cohorts (x-axes) and by region of origin (y-axes). Overall percentage representation of each cohort and region is indicated in parentheses next to its label.

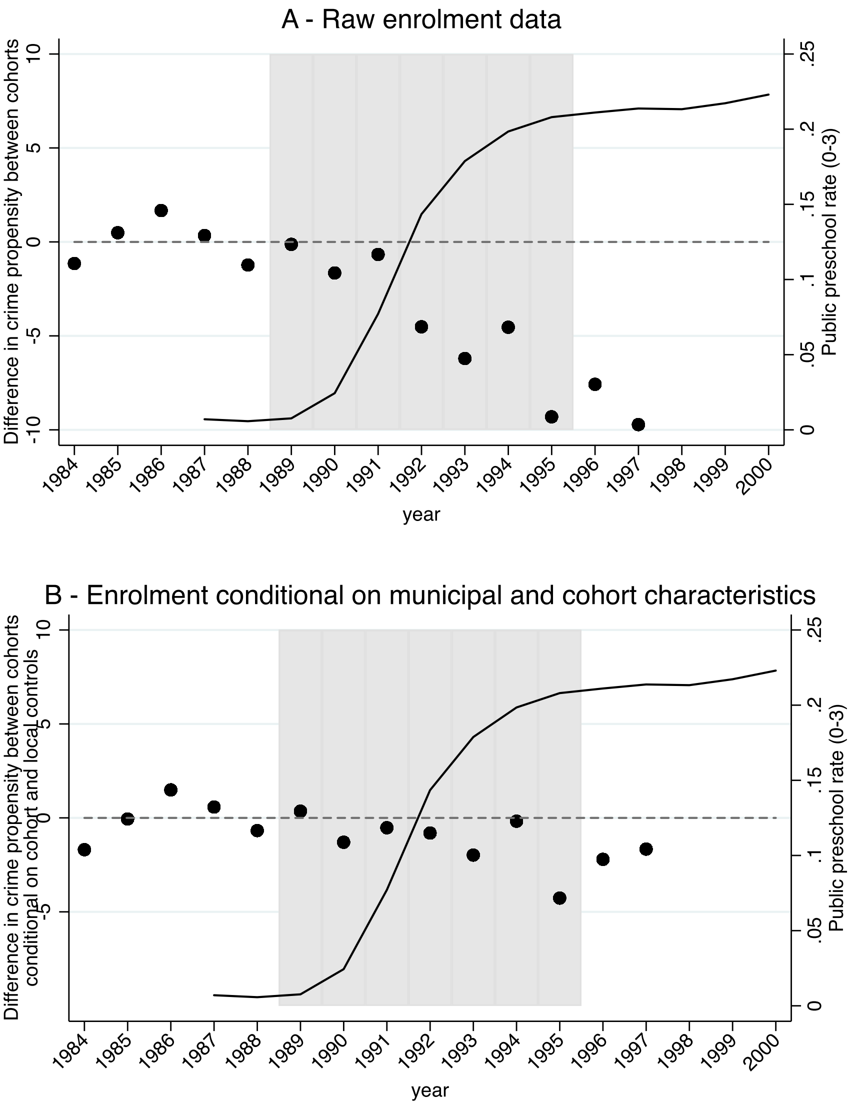


Fig. A.4. Differences in crime rates between adjacent cohorts of Catalans. *Note:* The figure scatters pairwise differences in crime rates between adjacent cohorts of Catalans, the younger cohort with respect to the older (black dots), averaged across all municipalities in our sample. Highlighted in gray is the period of most intensive public preschool expansion (solid line). Panel A shows raw differences per 1000 inhabitants; Panel B shows the residuals of the differences regressed on cohort and municipal characteristics. All pairwise differences have been standardized to the pre-expansion period average.

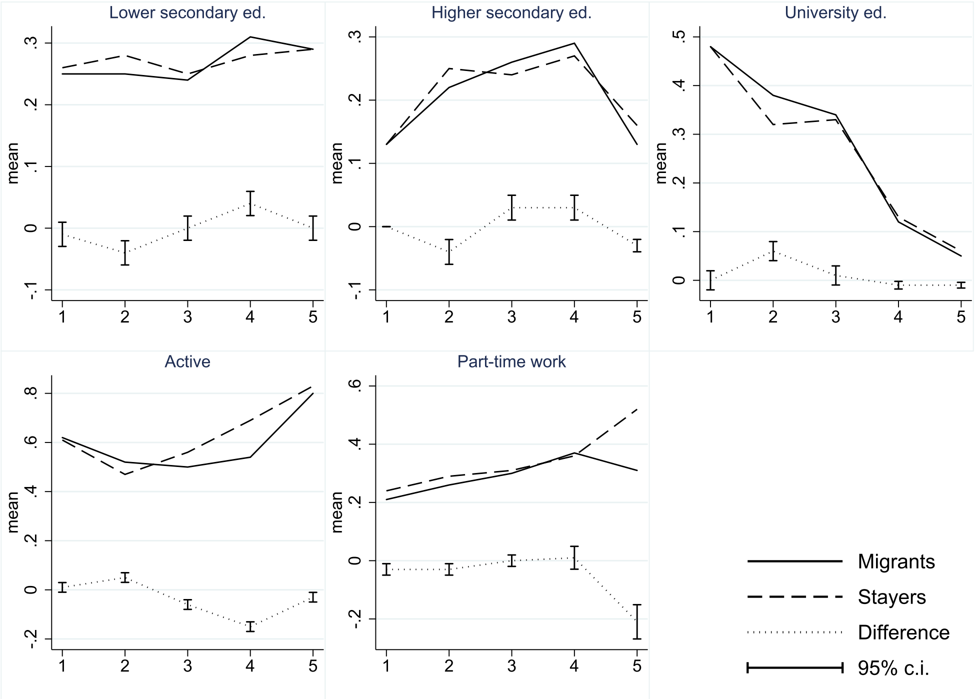


Fig. A.5. Characteristics of internal migrants by preschool exposure quintile. *Note:* For internal migrants and stayers, the figure plots shares of individuals by highest education level achieved (Lower Secondary, Higher Secondary, University or equivalent; all education level categories are mutually exclusive and the omitted category is "Less than secondary education"), activity status (working or studying versus neither) and part-time work (yes or no), in each quintile of preschool exposure (Q1 is lowest and Q5 is highest exposure). The difference in shares is dotted, and 95% confidence intervals are capped. Source: 2011 Census microdata (5-Percent Public Use Microdata Sample). Table A.5 shows the regressions on which the figure is drawn.

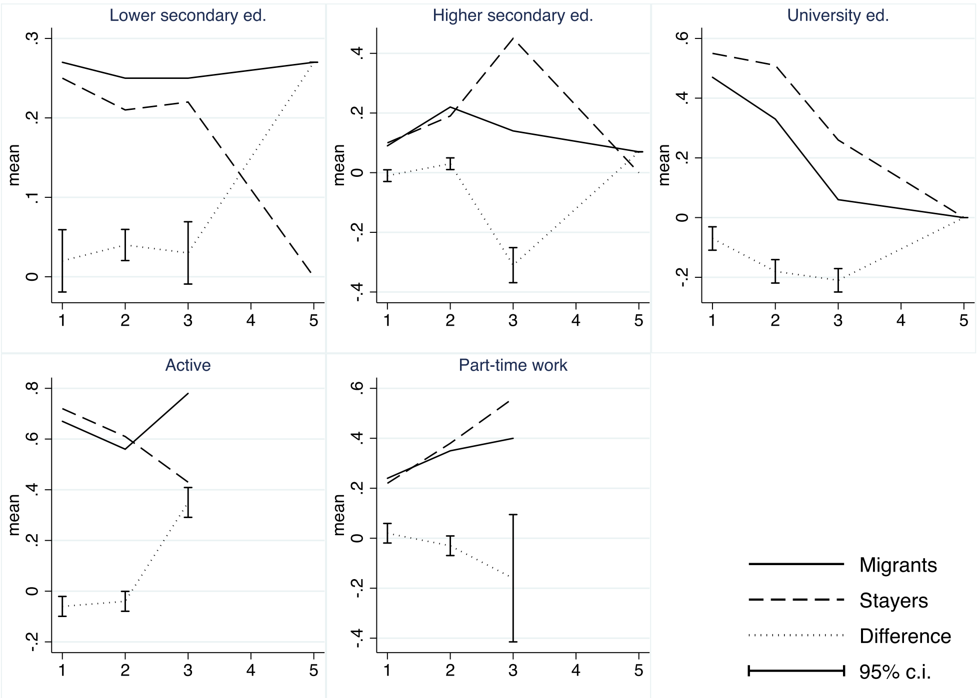
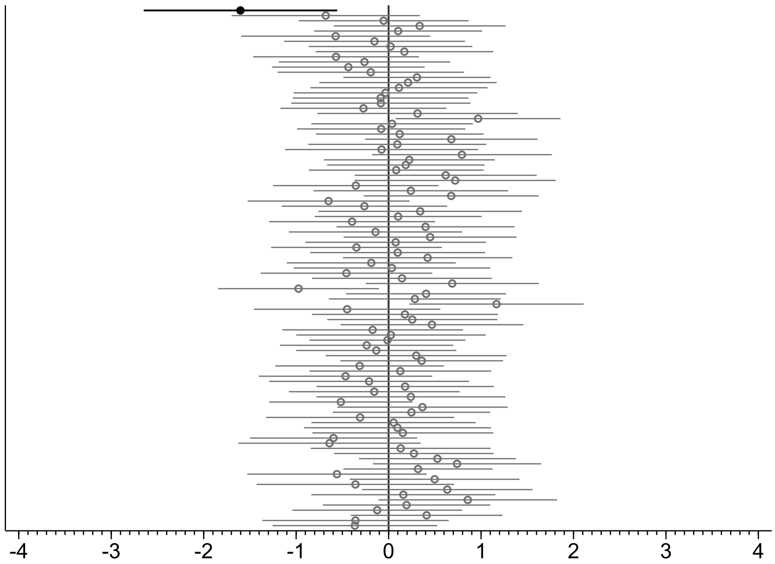


Fig. A.6. Characteristics of internal migrants (to Catalonia only) by preschool exposure quintile. *Note:* For internal migrants and stayers, the figure plots shares of individuals by highest education level achieved (Lower Secondary, Higher Secondary, University or equivalent; all education level categories are mutually exclusive and the omitted category is "Less than secondary education"), activity status (working or studying versus neither) and part-time work (yes or no), in each quintile of preschool exposure. The difference in shares is dotted, and 95% confidence intervals are capped. Source: 2011 Census microdata (5-Percent Public Use Microdata Sample). Missing data points are due to having fewer than 10 individuals in the migrants group.

(a) Random birth cohort assignment



(b) Random region of origin assignment

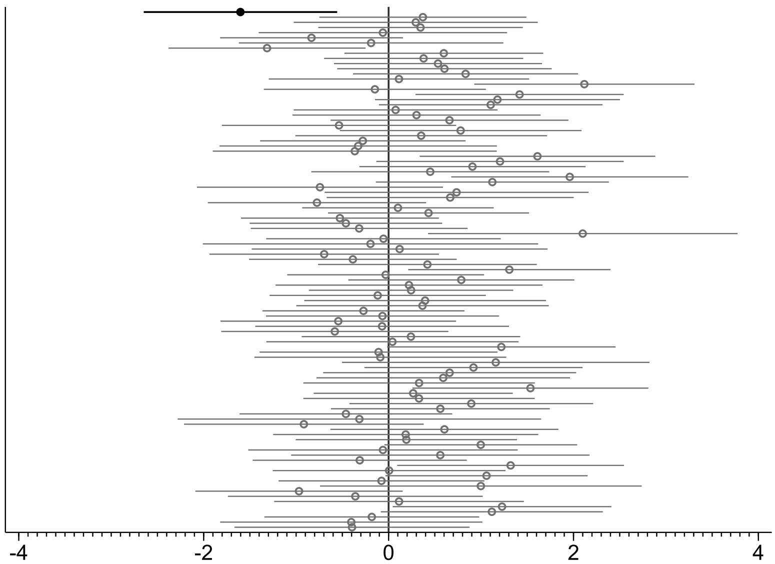


Fig. A.7. Random birth cohort or region of origin assignment. *Note:* The two panels plot early preschool exposure coefficient estimates and 95% confidence intervals obtained from our baseline model after randomizing birth cohorts (a) or region of origin (b) one-hundred times. In each sub-figure, the black marker at the top represents our original estimate.

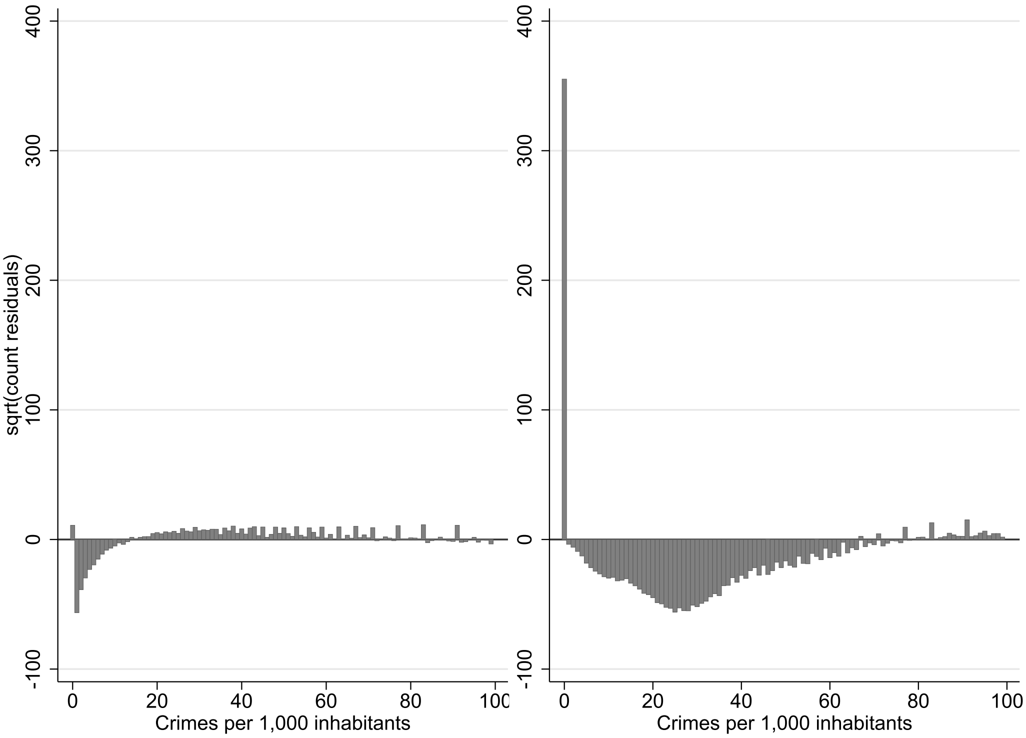


Fig. A.8. Model Fit. *Note:* The figure plots differences between observed and predicted counts produced by our baseline Negative Binomial specification (left) and the alternative Poisson specification (right).

Appendix E. Tables

Table A.1
Determinants of regional public preschool enrollment rates (0–3), 1987–2000 .

	(1)	(2)	(3)	(4)	(5)
LOGSE reform	0.21***	0.19**	0.19**	0.19**	0.19**
	(0.05)	(0.08)	(0.08)	(0.08)	(0.08)
Public capital expenditures (% GDP)	0.08	0.08	0.08	0.09	0.08
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Stock of public capital in education pc	−0.09	−0.10	−0.10	−0.11	−0.13
	(0.12)	(0.11)	(0.11)	(0.10)	(0.11)
GDP pc		−0.00	−0.00	−0.00	−0.00
		(0.00)	(0.00)	(0.00)	(0.00)
Young unemployment/1000p		−0.02	−0.02	−0.02	−0.01
		(0.04)	(0.04)	(0.04)	(0.04)
Education level active population		−0.11	−0.12	−0.17	−0.19
		(0.18)	(0.18)	(0.21)	(0.20)
Female labor force participation			−0.02	−0.00	0.02
			(0.19)	(0.20)	(0.21)
Court activity pc				−0.13	−0.12
				(0.25)	(0.26)
Left party in government					−0.01
					(0.01)
Year FE	√	√	√	√	√

(*continued on next page*)

Table A.1 (*continued*)

	(1)	(2)	(3)	(4)	(5)
Region FE	√	√	√	√	√
Mean(y)	0.10	0.10	0.10	0.10	0.10
sd(y)	0.07	0.07	0.07	0.07	0.07
N.obs	238	238	238	238	238
R-squared	0.88	0.88	0.88	0.88	0.88

Note: The Table shows the results of regressing regional-level enrollment rates for the period 1987–2000 on a number of potential determinants. Institutional and financial factors are proxied by a dummy variable for the 1990 LOGSE reform, taking the value 1 for all years after 1990; regional public capital expenditure as % of GDP ; per capita stock of public infrastructures devoted to education (Instituto Valenciano de Investigaciones Económicas, IVIE); a dummy for left party in government. Socio-economic factors are proxied by regional GDP per capita (Spanish Ministry of Finance and Public Administration); youth (16–19) unemployment rate; education level of active population, defined as the percentage of working-age population with primary education or less (IVIE). The expansion of the female employment rate is accounted for with data from the Economically Active Population Survey by INE (National Statistical Institute). As a proxy for crime rates back in time, data on regional Courts of Justice activity was retrieved from the General Attorney's Office of the Spanish Ministry of Justice. The correlation between provincial court data and provincial crime data obtained from the Spanish Ministry for Home Affairs show a correlation of 87.18% over the period 1993–1999. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Several robustness checks and change of variables did not alter results.

Table A.2
Descriptive statistics.

	mean	sd	median
Crime actions /1000pax	29.69	160.02	0.00
Property c. actions /1000pax	13.05	99.81	0.00
Person c. actions /1000pax	9.68	83.25	0.00
Other c. actions /1000pax	6.95	64.82	0.00
Public preschool rate (0–3)	0.10	0.08	0.09
Mobility-adjusted public preschool rate (0–3)	0.10	0.08	0.09
Total preschool rate (0–3)	0.22	0.15	0.18
Public preschool rate (4–5)	0.62	0.12	0.60
Unemployed /1000pax	76.78	29.27	77.31
Police /1000pax	1.15	1.24	1.26
Municipal revenue pc (Thousands of E)	1.66	1.13	1.36
Municipal expenditure pc (Thousands of E)	1.63	1.12	1.35
Regional GDP pc at age 3 (Billions of E)	0.01	0.00	0.01
Regional unemployment (%) pc at age 3	18.30	5.61	18.02
N	153,924		

Table A.3
Descriptive statistics by crime typologies.

	Sample weight	N	Mean	sd
PROPERTY CRIMES	54.01%	153,924	13.69	102.15
Economic motivation	44.61%	153,924	9.87	86.59
Theft	23.80%			
Robbery	10.90%			
Car theft	2.50%			
Misappropriation	2.70%			
House occupation	2.40%			
Other property	2.30%			
Damages to property	4.87%	153,924	1.79	31.98
Fraud and falsity	4.54%	153,924	2.03	37.59
VIOLENT CRIMES	25.49%	153,924	8.65	77.98
Pure violence	18.71%	153,924	6.24	64.76
Injuries	12.50%			
Threats	6.20%			
Against the family	4.94%	153,924	1.75	32.55
Mistreatments	3.93%			
Missing family responsibilities	1.01%			
Gender violence	1.27%	153,924	0.50	16.62
Sexual crimes	0.57%	153,924	0.15	8.67
OTHER CRIMES	20.50%	153,924	7.09	65.58
Rules compliance	17.91%	153,924	6.48	62.37
Against road safety	10.11%			
Public order	6.09%			
Sentence break	1.00%			
Against public administration	0.72%			
Drugs	2.59%	153,924	0.61	18.31

Table A.4
Determinants of migration to Catalonia (2009–2014).

	(1)	(2)	(3)
Public preschool rate (0–3)	−0.04***	−0.00	−0.00
	(0.01)	(0.01)	(0.01)
GDP pc (age 3)			0.01
			(0.18)
Unemployment (age 3)			−0.00*
			(0.00)
Year of birth FE		√	√
Region of birth FE		√	√
Year FE			√
Mean(y)	0.01	0.01	0.01
sd(y)	0.01	0.01	0.01
N.obs	1428	1428	1428
R-squared	0.22	0.86	0.87

Note: OLS regressions of the share of internal migrants from Spanish regions to Catalonia over the period 2009–2014 on potential factors influencing migration decisions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5
Characteristics of internal migrants by preschool exposure quintile.

	1st Q		2nd Q		3rd Q		4th Q		5th Q	
	m	sd/SE	m	sd/SE	m	sd/SE	m	sd/SE	m	sd/SE
Education: LS										
Migrants	0.25	(.01)	.25	(.01)	.24	(.01)	.31	(.01)	.29	(.01)
Stayers	.26	(.00)	.28	(.00)	.25	(.00)	.28	(.00)	.29	(.00)
Difference	-.01**	(.01)	-.04***	(.01)	-0.00	(.01)	.04	(.01)	.00	(.01)
Education: HS										
Migrants	.13	(.00)	.22	(.01)	.26	(.01)	.29	(.01)	.13	(.00)
Stayers	.13	(.00)	.25	(.00)	.24	(.00)	.27	(.00)	.16	(.00)
Difference	.00	(.00)	-.04***	(.01)	.03***	(.01)	.03***	(.01)	-.03***	(.00)
Education: Uni										
Migrants	.48	(.01)	.38	(.01)	.34	(.01)	.12	(.00)	.05	(.00)
Stayers	.48	(.00)	.32	(.00)	.33	(.00)	.13	(.00)	.06	(.00)
Difference	.00	(.01)	.06***	(.01)	.01**	(.01)	-.01***	(.00)	-.01***	(.00)
Active										
Migrants	.62	(.01)	.52	(.01)	.50	(.01)	.54	(.01)	.80	(.01)
Stayers	.61	(.00)	.47	(.00)	.56	(.00)	.69	(.00)	.83	(.00)
Difference	.01	(.01)	.05***	(.01)	-.06***	(.01)	-.15***	(.01)	-.03***	(.01)
Part-time										
Migrants	.21	(.01)	.26	(.01)	.30	(.01)	.37	(.02)	.31	(.03)
Stayers	.24	(.00)	.29	(.00)	.31	(.00)	.36	(.01)	.52	(.01)
Difference	-.03***	(.01)	-.03***	(.01)	-.00	(.01)	.01	(.02)	-.21***	(.03)

Note: For internal migrants and stayers, the table presents shares of individuals by highest education level achieved (Lower Secondary, Higher Secondary, University or equivalent), activity status (working or studying versus neither) and part-time work (yes or no) in each quintile of preschool exposure. Source: 2011 Census microdata. In parentheses, standard deviation of the shares and standard error of the differences between migrants and stayers.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Figure A.5 shows this data graphically.

Table A.6
Preschool exposure and migrants' characteristics.

OUTCOME	Odds ratio	robust s.e.	z	*p*-value	95% c.i.		N
2001 Census							
Education level	0.03	0.11	−0.82	0.41	0.00	169.46	1132
2011 Census							
Education level	7.59	13.62	1.13	0.26	.23	255.22	2063
Active	3.67	10.51	0.45	0.65	0.73	2.06	1632
Part-time	0.02	0.09	−0.86	0.39	0.00	164.01	814

Note: The table indicates the relationship, in proportional odds ratios, between different characteristics of migrants and their preschool exposure at age 3, conditional on cohort and region of bith fixed effects. Ordered logit model estimates for education level achieved [Less than primary - Primary - Secondary - Specialization - University - Masters or higher]. Logit model estimates for being active (employed or studying) and for having part-time a job [Yes - No].

Table A.7
Characteristics of the crime age distributions.

	(1)	(2)	(3)	(4)	(5)
	Peak age	Low age	Peak / Low	sd	kurtosis
Public preschool rate (0–3)	−0.12	0.09	1.27	−0.73	0.81
	(0.14)	(0.29)	(2.49)	(2.14)	(0.66)
Year of birth FE	√	√	√	√	√
Region of birth FE	√	√	√	√	√
Mean(y)	21.05	20.76	4.10	41.02	2.21
sd(y)	3.29	5.25	3.31	46.50	0.70
N.obs	1512	1512	954	1512	1482
Pseudo R-squared	0.08	0.19	0.13	0.09	0.01
alpha	0.00	0.00	0.05***	0.28***	0.00

Note: SEs clustered (1-way) by cohort-region in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8
Residence and birth region for 3-year olds across Spain.

	% difference		% difference
Andalusia	−0.18	Valencia	0.97
Aragon	−0.03	Extremadura	−1.16
Asturias	−1.16	Galicia	0.27
Balearic Islands	−0.30	Madrid	−0.64
Canary Islands	0.02	Murcia	−0.03
Cantabria	−0.00	Navarre	0.41
Castilla y Leon	−0.74	Basque Country	−1.44
Castilla-La Mancha	0.31	Rioja	1.30
Catalonia	−0.11	Ceuta and Melilla	−1.47

Note: For each Spanish region, the table shows the percentage difference between the number of 3-year old residents and the number of births recorded three years earlier (averages across birth cohorts 1984–1997). Source: municipal residence records, elaborated by the Spanish National Statistics Agency (INE).

Table A.9
Preschool exposure estimates under different robustness checks.

	Negative Binomial				
	(1)	(2)	(3)	(4)	(5)
C1) w/ regional controls until 12					−1.87**
					(0.77)
C2) w/ regional controls until 18					−2.20***
					(0.78)
C3) Social capital controls				−1.68***	−1.70**
				(0.63)	(0.67)
C4) Mobility-adjusted	−3.21***	−1.59***	−1.58***	−1.63**	−1.35*
	(0.67)	(0.61)	(0.59)	(0.64)	(0.75)
C5) Total enrolment	−1.86***	−1.27***	−1.24***	−1.35***	−1.36***
	(0.37)	(0.31)	(0.30)	(0.33)	(0.42)
C6) w/ exposure	−4.79***	−2.31***	−2.64***	−2.49***	−1.89**
	(1.03)	(0.71)	(0.70)	(0.73)	(0.83)
C7) w/o islands and territories(x)	−3.17***	−1.62***	−1.56***	−1.57**	−1.20
	(0.68)	(0.62)	(0.60)	(0.65)	(0.79)
C8) w/o largest regions	−2.16***	−0.97	−0.95	−0.93	−0.93
	(0.62)	(1.41)	(1.44)	(1.67)	(1.64)
C9) w/o regions missing data	−3.08***	−1.68**	−1.66**	−1.72**	−1.43*
	(0.69)	(0.67)	(0.66)	(0.71)	(0.81)
C10) Province level (+)	−3.32***	−1.66	−1.90	−1.32	−1.17
	(1.14)	(1.75)	(1.85)	(1.85)	(1.85)
C11) Catalonia level (+)	−3.18***	−1.98	−2.03	−1.38	−1.11
	(0.66)	(1.83)	(1.84)	(1.99)	(1.86)
Local controls				Yes	Yes
Regional controls (at age 3)					Yes
Year of birth FE		√	√	√	√
Region of birth FE		√	√	√	√
Province FE			√	√	√
Year FE			√	√	√

Note: SEs clustered (1-way) by cohort-region in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Islands and territories are Canary and Balearic Islands, Ceuta and Melilla. (x) Largest regions are Catalonia and Andalusia. Regions missing some data points are Valencia and Galicia. (+) For regional level specifications, no local fixed effects are included. See descriptives for the provincial and regional level of aggregation in Table A.10.

Table A.10
Descriptive statistics at higher geographical aggregation .

	Provincial level			Regional level		
	mean	sd	median	mean	sd	median
Crime actions /1000pax	34.21	77.65	0	38.55	43.84	31.00
Unemployment /1000pax	61.16	10.90	63.88	63.45	5.69	64.03
Police /1000pax	1.64	0.74	1.73	1.69	0.33	1.76
Local revenue pc (Thousands of EUR)	1.64	0.38	1.58	1.52	0.14	1.50
Local expenditure pc (Thousands of EUR)	1.61	0.36	1.57	1.49	0.13	1.48
N	5801			1512		

Table A.11
Heterogeneous effects by relative economic advantage of birth cohorts.

	(1)	(2)	(3)	(4)	(5)	(6)
Public preschool rate (0–3)	−1.46**	−0.92	−1.74*	−1.30	−0.89	−0.08
	(0.68)	(0.89)	(0.96)	(1.07)	(1.19)	(1.30)
PP rate (0–3) x Low GDP	−0.39	−1.28				
	(1.02)	(1.08)				
PP rate (0–3) x High unemployment			−0.07	−0.16		
			(0.88)	(0.92)		
PP rate (0–3) x Hard crisis					−1.55	−2.15
					(1.35)	(1.41)
Local controls		Yes		Yes		Yes
Regional controls (at age 3)		Yes		Yes		Yes
Year of birth FE	√	√	√	√	√	√
Region of birth FE	√	√	√	√	√	√
Province FE		√		√		√
Year FE		√		√		√
Mean(y)	29.69	29.69	29.69	29.69	29.69	29.69
sd(y)	160.02	160.02	160.02	160.02	160.02	160.02
N.obs	153,924	153,924	153,924	153,924	153,924	153,924
R-squared						
Pseudo R-squared	0.00	0.00	0.00	0.00	0.00	0.00
alpha	35.84	35.24	35.84	35.24	35.84	35.24

Note: The table reports the results of interacting the preschool access measure with measures of relative economic advantage of birth cohorts. Low-GDP: equals 1 (0) for cohorts exposed to a GDP per capita below-median (above-median) within their region of origin at age 3. High-unemployment: equals 1 (0) for cohorts exposed to unemployment levels above-median (below-median) within their region of origin at age 3. Hard crisis: equals 1 (0) for cohorts who were aged 3 between 1993 and 1998 in regions hit hardest by the 1993 economic crisis (i.e. recording a rise in unemployment above the national average of +0.41% between 1991.T4 and 1994.T4). Each observation represents cohort 'c' born in region 'i' living in Catalan municipality 'm' in year 'y'. SEs clustered (1-way) by cohort-region in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12
Descriptive statistics of empirical and predicted count distributions.

	Empirical distribution	Negative Binomial model	Poisson model
mean	.0001	.0001	.0001
standard deviation	.0263	.0261	.0047
skewness	31.5650	31.5313	5.6219
kurtosis	997.8949	996.4473	36.1710
max |difference|		.0230	.8306
value at max |difference|		0	1
mean |difference|		.0002	.0018
N	1001	1001	1001

Note: Comparison between the empirical offense counts distribution and the distributions of offense counts predicted by the Negative Binomial and the Poisson modeling options.

Table A.13
Bayesian and Akaike Information Criteria for the two modeling choices.

	BIC	AIC
Poisson model	2.442×10^7	2.442×10^7
Negative Binomial model	4.684×10^5	4.681×10^5
Difference	2.395×10^7	2.395×10^7
Prefer	NB model	NB model

Note: Assessment of model fit through Bayesian and Akaike Information Criteria for the Negative Binomial and Poisson modeling choices applied to our context.

Table A.14
Annual benefits in terms of crime reduction, by crime type.

Crime type	Total unit cost (€)	Public unit cost (€)	Multiplier	Annual total cost reduction (€)	Annual public cost reduction (€)
theft	1,942.1	900.7	81.1	10,317,486.2	4,784,921.1
robbery	15,930.6	7,669.8	57.2	27,474,533.2	13,227,580.0
car theft	14,481.1	5,629.2	33.4	3,284,053.8	1,276,600.1
damages	1,899.9	830.3	42.4	104,583.7	45,706.9
fraud	1,815.4	422.2	306.7	38,028,039.6	8,843,730.1
injury	19,772.6	4,813.0	6.4	16,345,683.4	3,978,806.9
threats	8,345.3	3,293.1	7.3	3,885,788.0	1,533,346.4
sexual	9,175.6	2,181.3	77.9	9,656,182.5	2,295,564.8
gender violence	9,175.6	2,181.3	63.5	4,845,458.5	1,151,911.1
TOTAL				113,941,808.8	37,138,167.6

Note: Total and public costs obtained from Heeks et al. (2018) and are expressed in 2015 EUR (exchange rate used GBP 1 = EUR 1.4073 as of 31/06/2015). Crime used in this exercise account for two thirds of crime with known offender recorded in Catalonia. The Multiplier inflates crime counts from those recorded by police with a known offender to an estimate of those actually occurred. The Multiplier is composed of a first inflation factor from crimes with known offender to crimes reported to police (calculated using administrative data for Catalonia, at our disposal), and a second inflation factor from crimes reported to police to total crimes (borrowed from Heeks et al., 2018). Annual estimates of cost reductions are obtained using impact estimates shown in Column 5 in Table 2 for the period 2009–2014, and are expressed in EUR discounted to 2004.

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