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PREVENTING CRIMINAL MINDS: EARLY EDUCATION ACCESS AND ADULT OFFENDING BEHAVIOR [*]

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

ABSTRACT: In this paper we estimate the impact of a nationwide public preschool expansion that took place in Spain over the 1990s on criminal behavior later in time. We exploit variation in enrollment rates across Spanish regions and birth-cohorts, and we link education data to a unique administrative crime dataset recording offenses committed in the region of Catalonia over the period 2009-2014. We find that for the average birth cohort, Catalan municipality and year, a 1 percentage point increase in preschool exposure at age 3 yields 1.6% fewer crime actions during youth and young adulthood. We are able to account for region of origin, birth cohort, time and local fixed effects, as well as several region and time-specific controls. Leveraging detailed information on types of crime committed, we propose a categorization of offenses into those likely to have been rationally planned and driven by economic motives, and those in which emotional factors and lack of self-control play a significant role. On average, we find the benefits of preschool to be larger and more robust on crimes belonging to the latter category, suggesting that non-cognitive skills play an important role in explaining the overall effect.

JEL Codes: I26, I28, K42, J13
Keywords: Universal child care, adult crime, education reform

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1 Introduction

Early childhood years are recognized by various fields of research as a key stage in life, carrying the power to shape future outcomes all the way through youth and into adulthood. There is a wide consensus about early childhood assessments being good *predictors* for educational achievements (Burchinal et al. 2010; Nelson and Sheridan 2011; Nguyen et al. 2016), labor market success (Le et al. 2005; Duncan, Ziol-Guest, and Kalil 2010) and health outcomes (Case, Lubotsky, and Paxson 2002). What is more, a growing number of compelling studies has shown the ability of *policy interventions* in those areas to produce significant impacts on later childhood years (Currie and Thomas 1995; Garces, Thomas, and Currie 2002; Ludwig and Miller 2007; Magnuson, Ruhm, and Waldfogel 2007; Conti, Heckman, and Pinto 2016) as well as on adult ages (Campbell, Ramey, et al. 2002; Heckman 2006; Gertler et al. 2014; Conti and Heckman 2014), especially when disadvantaged children are targeted. The majority of these studies looks at *cognitive* skill formation and derived outcomes.[1] On the *non-cognitive* side, psychological and pedagogical theory postulates that pre-kindergarten circumstances and experiences are able to forge an adult's spheres of anxiety and self-control (Gunnar and Barr 1998; Kagan and Snidman 1999; Moffitt et al. 2011), self-esteem and motivation (Maslow 1943; Almlund et al. 2011), socio-emotional intelligence and well-being (Phillips and Shonkoff 2000; Robinson 2007) and aggressive behavior (Tremblay et al. 2004; Hahn et al. 2007). Nevertheless, rigorous, large-sample evidence along these dimensions is both scarce – often due to the lack of high quality data on outcomes – and decidedly mixed about the direction and size of impacts (Duncan and Magnuson 2013). The branch of research focusing on the antisocial behavior outcome best encompasses our contribution, which looks at the causal relationship between early childhood education and criminal propensity later in life.

Starting in the early Nineties, a Spanish national education reform boosted the availability of high-quality public preschool. 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 arose both across time and across space. Different birth cohorts within the same region and same birth cohorts across different regions were faced with diverse levels of preschool access. We exploit such variation in our quantitative analysis by relating it to crime rates measured, years later, for each of those birth cohorts and regions of origin. We find a negative impact of early public preschool availability on crime rates in youth and young adulthood: a 1 percentage point increase in preschool access back in the early 1990s yields 1.6% fewer criminal actions in the 2000s.

We focus on birth cohorts 1984 to 1997, which are homogeneous in terms of other important

[1] The most compelling of these studies exploit cross-sibling comparisons (Currie and Thomas 1995; Garces, Thomas, and Currie 2002) and regression-discontinuity designs that take advantage of variation in Head Start funding rates (Ludwig and Miller 2007). These studies find general evidence that Head Start participation has long-term benefits in terms of schooling outcomes.

educational characteristics[2] and are old enough to appear in our crime dataset later in time. Our outcome of interest, crime rates, is drawn from a unique administrative dataset recording all criminal actions with known offender that were committed across the Spanish region of Catalonia over the years 2009 to 2014. The offenders we focus on are aged between 15 and 30 at the moment of their offense.[3] Among other details, we observe basic information about the offender, notably his/her birth cohort and region of origin, and detailed information about the offense committed, among which its date, location and type. Exploiting this information, in the second part of our analysis we are able to assess the impact that the national preschool expansion program has had on different types of crime separately, which helps us to form a more precise opinion on the potential mechanisms at work behind our main results. Our findings suggest that the non-cognitive skills channel plays a fundamental role in explaining the reduction in crime which followed the increased public preschool access. The crime types on which we estimate the highest and most robust preschool impacts are those of more impulsive, unplanned and irrational nature - in other words, those crimes for which literature has found poor socio-emotional and behavioral skills to be important drivers.

Our outcome variable takes the shape of criminal action counts per 1,000 inhabitants, and we employ negative binomial regression (NBR) modeling to estimate the relationship between crime counts and preschool access rates. Our choice of using NBR modeling is motivated by the high overdispersion of our outcome variable, and we find NBR to perform significantly better than the more traditional Poisson choice, according to a variety of measures of fit. We discuss and provide statistical evidence to support the assumptions behind our estimation strategy, among which those on internal migration behavior and on the crime-age distribution. We perform a set of robustness checks and placebo tests to corroborate our findings.

The remainder of this paper is structured as follows. Section 2 reviews the literature which is closest to our work. Section 3 presents the institutional background on preschool education in Spain, and on the public preschool expansion occurred in the Nineties. Section 4 explains our empirical framework and the data sources we draw on. Section 5 discusses the main assumptions underlying our results, which are illustrated in Section 6 along with the potential mechanisms underlying these. Section 7 provides further discussion of results, as well as strengths and potential shortcomings of our analysis. Robustness checks and placebo tests are shown in Section 8 and Section 9 concludes. Additionally, the Appendix presents descriptive statistics (A), analysis on model fit (B), additional tables and figures which are supporting our analysis (C) as well as the information needed to perform benefit-to-cost calculations derived from our estimates (D).

[2]Most importantly, school leaving age is 16 for all of them.

[3]15 years is the minimum age at which offenses are recorded in our crime dataset.

2 Review of the closest literature

Attending high quality preschool programs has been shown effective in increasing cognitive performance, especially in the short run and in the case of individuals from disadvantaged backgrounds. The strongest evidence on these aspects is based on the evaluation of US programs such as the Perry program, Project CARE and the Abecedarian project (Heckman, Moon, Pinto, P. Savelyev, et al. (2010), Heckman, Pinto, and P. Savelyev 2013), which share the characteristics of operating on a small scale and being targeted at low-income families. The results of large scale interventions or nationwide public preschool expansions are less unanimous. A 2017 an interdisciplinary task force of experts assembled a report on the current state on the scientific knowledge on pre-kindergarten effects, again based on studies focusing on the United States (Phillips, Lipsey, et al. 2017). The consensus statement opening the report declares that convincing evidence is at hand about the effectiveness of preschool programs in preparing children for kindergarten on a *cognitive* level – while the effects on social-emotional and self-regulatory development seem more modest and are addressed by a much smaller amount of studies. The statement further highlights the lack of sufficient rigorous evidence on the *longer-term impacts* of large-scale preschool programs, on which broad conclusions are still unavailable. The following paragraphs review some of the most recent and compelling studies published so far, gradually narrowing the view down to our specific contribution.

Effects of preschool programs on cognitive outcomes In terms of cognitive outcomes and looking at larger-scale early childcare interventions, several studies find strong effects for disadvantaged groups but small or negligible effects on the remaining population (Blanden et al. 2016). In Italy, Carta and Rizzica (2018) look at the subsidized expansion of preschool places for 2-year olds but find no effects on cognitive skills later in childhood. Blanden et al. (2016) exploit the staggered implementation of pre-school 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 (2011; 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, but that these were also less likely to enroll with respect to those from advantaged backgrounds. Dumas and Lefranc (2012) evaluate the effect of a large preschool expansion program in France over the 60s and 70s, finding persistent and inequality-reducing effects on educational and labor market outcomes.

Effects of preschool programs on non-cognitive outcomes The lack of consensus about the effects of early childhood education on non-cognitive outcomes begins already in the short-term

view, when looking at a few years after being exposed to the education programs. Loeb et al. (2007) use data from the Early Childhood Longitudinal Study and through OLS, matching and IV estimation, conclude that center-based preschool education has overall negative effect on socio-behavioral measures at the start of kindergarten. On the contrary, Figlio and Roth (2009), looking at children born in Florida from 1994 onwards and using differences in access within a family, find that public pre- kindergarten participation reduces behavioral problems in elementary school, especially when the child grows up in a particularly disadvantaged neighborhood. Similar positive conclusions are drawn by Berlinski, Galiani, and Gertler (2009), who find that third-grade Argentinian students 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.

Moving to the long-run horizon, there is a handful of studies including non-cognitive traits among their outcomes of interest, and the lack of universal consensus about the effects persists. Campbell et al. (2002; 2008) compare adult benefits for participants in Project CARE with those of the Abecedarian Project, and find that the programs induced higher likelihoods of conducting healthy, active adult lifestyles and reduced marijuana consumption. Nevertheless, no positive effects are found on (self-reported) binge-drinking, driving after drinking and violence, while males were actually found *more likely* to report breaking the law. Heckman, Moon, Pinto, P. Savelyev, et al. (2010) and Conti, Heckman, and Pinto (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 behaviors), and in turn significantly improved a range of adult outcomes including marriage, health, participation in healthy behaviors and reduced participation in crime (Heckman, Pinto, and P. Savelyev 2013).

Long-run effects of preschool programs on violent behavior and crime Further narrowing the focus around our contribution, Hahn et al. (2007) provide an excellent systematic review of scientific evidence concerning the effectiveness of school-based programs to reduce or prevent violent behavior. The review looks at programs administered from pre-kindergarten to high school, and finds positive impacts through all grade levels. The most important reductions in violent behavior are found through interventions targeting the earliest ages, pre-kindergarten and kindergarten. Garces, Thomas, and Currie (2002) use comparisons between siblings to look at longer-term effects of the Head Start program. Beyond positive impacts on educational attainment for both blacks and whites, they find that black children who participated in the program are less likely to have been booked or charged with a crime by their early twenties, relative to their siblings who did not partici-

pate. Schweinhart et al. (2005) study the long-term impacts of the Perry preschool program and find particularly strong evidence on crime prevention: the program played a significant role in reducing overall arrests and arrests for violent crimes as well as property and drug crimes and subsequent prison or jail sentences, over study participants' lifetimes up to age 40. Heckman et al. (2009; 2009; 2010) confirm that crime reduction is a major benefit of the Perry program and find that program treatment effects mainly operate through enhancing non-cognitive or behavioral skills that are very predictive of criminal behavior. This more recent literature distanced itself from the traditional view and findings that non-maternal forms of daycare have adverse effects on the socio-emotional development of the child (Schwarz, Strickland, and Krolick 1974; Baers 1954), and especially on anxiety and anger (Bowlby 1958; 1973).

Studies on the Spanish context Looking at the Spanish context, to the best of our knowledge there are no studies linking preschool education and aggressive behavior or criminal outcomes. On the other hand, there are a few studies exploring the causal relationship between early childhood education and educational results. 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) use 2009 data from the General Diagnostic Evaluation prepared by the Spanish Ministry of Education, and find a positive impact of more years of preschool on academic results at 4th grade. Similarly to this paper, Felfe, Nollenberger, and Rodríguez-Planas (2015) look at the expansion of public preschool during the early Nineties, studying its impact on PISA test performance. They find a positive impact on reading and math scores, more pronounced for girls and for students with low socioeconomic background.

3 Institutional and educational background

For a good understanding of our setting, it is important to describe the Spanish institutional and educational context at the time around which the early Nineties public preschool expansion occurred.

Preschool education had been formally existing in the public educational system since 1970 (that is, since the LGE – the General Law on Education), distinguishing between two levels: *Jardín de Infancia* ("Kindergarten", for children aged below 3) and *Escuela de Párvulos* ("Nursery school" for children aged 4 to 5) - the attendance to both of which was voluntary. However, the educational regime of that time did not attribute high priority to preschool, since public funds were urgently needed to cater for children of compulsory schooling ages – who had outnumbered forecasts due to the Spanish baby-boom that had occurred in the Sixties. As a result, during the Seventies and Eighties, the public preschool offer was very limited and mostly directed to cover the most compelling needs of the most socially vulnerable categories, while *private* preschool initiatives grew in importance but lacked a legal framework or quality scrutiny by authorities (Berea 1992; González 2004).

Over the late Seventies and Eighties, public preschool access for 4 and 5-year olds increased quite rapidly. Preschool classes for children of those ages were typically annexed to existing primary schools, and they were in high demand as families became increasingly aware of the advantages of early education, and as this form of early schooling became a way of securing a spot in their preferred primary school center (Berea 1992). Nevertheless, the predominance of the unregulated private sector over daycare for children aged 3 and below continued, with great heterogeneity in type of ownership, skills and credentials of staff, and prices. These nurseries were mainly viewed as 'day-custody' centers (*guarderías* in Spanish) and generally carried very little educational content (González 2004).[4] Besides this scarce and disordered formal offer, informal daycare arrangements through members of the extended family or friends were the only available resource for many working mothers - who characterized around 18% of Spanish families by the early Eighties ("Situación social de la mujer en España" 1986; Del Campo Urbano 1986).

Age three preschool expansion

It was not until 1990 that the educational system was deeply reformed. The new educational law, LOGSE (*Ley Orgánica General del Sistema Educativo*, October 1990), aimed at embracing the evolving socio-economic reality of Spain, as well as adapting to the increasingly decentralized allocation of sectoral competencies which had been taking shape over the previous decade.

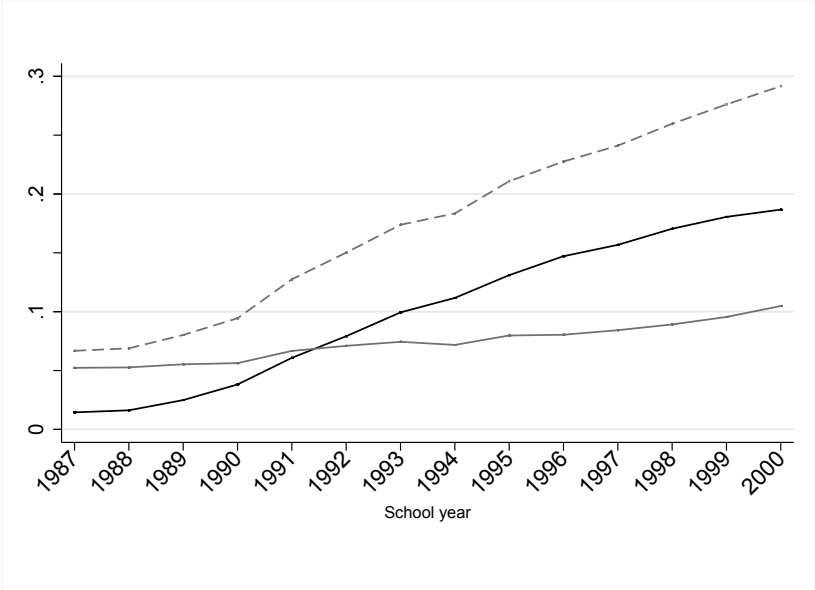
The new regulation classified preschool years into two voluntary education cycles: the first for ages 0 to 2 and the second for ages 3 to 5. None of the two cycles was offered as a completely free public service, but they were heavily subsidized, so that families covered only around 12% of the total cost of a preschool spot, and the rest was sustained by local governments.[5] As a result, the fee users were facing for a public spot was multiple times lower than the ones prevalent on the private sector. The transition between the last year of preschool and the first year of primary school was made automatic: children who attended the last preschool year in a specific education center acquired the right to remain in that center during primary school years (ages 6 to 11). In fact, this policy shifted the moment of primary school choice back to the start of the second cycle of preschool, i.e. to age three. As a consequence, educational centers offering exclusively preschool cycles gradually

[4]Sociologists and historians have provided several explanations for the slow expansion of public daycare for the very young in Spain. By some, it has been interpreted as the result of an explicit social rejection of all kind of measures that could be associated with the earlier Franco dictatorship, which had tried to invade private family lives through fertility policies ((Iglesias de Ussel 1994; Valiente Fernandéz 1997; González 2004)). Another explanation proposed was the lack of measures for gender equality and women's labor participation in the Spanish political agenda. Finally, cultural aspects may have also played a role, such as the widespread opinion that the best care for early childhood came from maternal care ((Valiente Fernandéz 1997; González 2004)).

[5]Data from the region of Andalusia for the school year 1994 ("Temporeros y educación", 1997). In terms of number of available spots and access conditions, we note that Article 7.2 of LOGSE states "Preschool education is voluntary. The public administrations guarantees the existence of a sufficient number of places to ensure schooling for those who request it". However, the degree of compliance to this rule may have varied across different regions. Also, regions were the ones deciding upon access priorites and exact fees (see González and Vidal Torre 2005) .

started losing demand, in favor of centers offering second cycle preschool as well as primary school ("*centros de infantil y primaria*") – which increasingly offer new matriculation to 3-year olds and rarely to older children. The 1990 LOGSE reform triggered a significant and rapid expansion of early public preschool services, and especially so for three-year-old children. Figure 1 shows the evolution of early preschool enrollment rates at national level, and Figure 2 separates rates for 0-2 year-olds and 3-year-olds - for those years in which enrollment data started to be collected separately by exact age.

Figure 1: Early preschool (age 0-3) enrollment rates Spain, 1987-2000

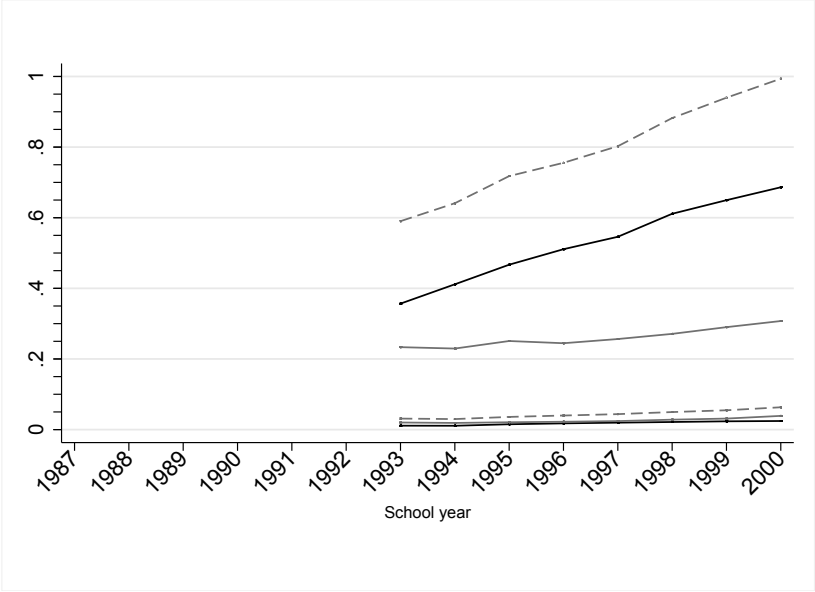


Note: Age 0-3 enrollment rates in Spain, by school year. Public preschool rates in solid black, private ones in solid grey and total ones dashed.

Under the new legal framework, the institutional view about the role of preschool was also revisited, transitioning from the previous 'day-custody' perspective to a proper educational stage – the goals of which were set to promote the physical, intellectual, social, affective and personal development, and to compensate for any existing inequalities. National preschool education guidelines instructed to teach children self-awareness, personal autonomy, knowledge of the environment, communication and representation skills. In other words, beyond preparing children for later academic stages on a cognitive level, the reformed Spanish public preschool heavily promoted the development of non-cognitive skills, recognizing their importance for children's comfort and success in social environments. Finally, through the new norms preschool classes for three-year olds were limited to a maximum of 20 pupils and their teachers were required to possess a university degree.

In their article, Blanden et al. (2016) observe that three important factors may influence the results

Figure 2: Preschool enrollment rates for ages 0-2 and 3 separately, 1993-2000



Note: Enrolment rates in Spain for age groups 0-2 (bottom three lines) and 3 (top three lines) separately, by school year. Public preschool rates in solid black, private ones in solid grey and total ones dashed.

of an expansion of childcare, in terms of children's development: the intensity of the additional provision, the quality of the newly provided childcare and the quality of the counterfactual care. Following this useful structure and based on what was described in this Section, we categorize the Spanish public preschool expansion during Nineties as high-intensity, high quality, and arisen in a context of relatively low-quality alternative options.[6] Given the quality characteristics of the renovated public system, one would expect a positive impact of the Spanish preschool expansion on the development of 3-year old children. Regarding the intensity aspect, we agree with Blanden et al. (2016) in saying that the effects are more difficult to predict - given that preschool access for young children generates a tradeoff between losing time with parents, gaining professional education time, and probably enjoying better economic opportunities through increased labor market participation of parents (Brewer et al. 2014; Berlinski and Galiani 2007). We will come back to this point when discussing our results.

[6] Although it did contribute towards the regulation of minimum standards to be met by private childcare providers, which had been, as described earlier, completely unregulated until then.

3.1 The role of subnational governments and the staggered preschool expansion

The decentralization of educational competences from the national to the regional level started in Spain during the early Eighties and protracted itself until the late Nineties - going hand in hand with the gradual, wider political decentralization process that occurred in those years. As the new educational regulations were approved in 1990, Spanish regions (*Comunidades Autónomas*) were in the midst of the process of taking over educational responsibilities - with substantial heterogeneity in the degree to which the transition was complete, ranging from 'not yet started' to 'fully transitioned'. Overall, more than 50% of the public education expenditure was administered by regional governments at that time - and the figure would rise to above 80% over the opening decade (Calero and Bonal 2004).[7]

As a result of their decisional and budgetary autonomy, and of the cross-regional differences in those matters, the timing of the expansion of public supply of early preschool places varied considerably across the eighteen Spanish regions. Figure 3 shows the regional evolution of early preschool enrollment rates, which we take as our measure of preschool access. Interpreting enrollment rates as access rates makes the implicit assumption the availability of public preschool was below the demand for it - that is, the supply side was binding. This assumption is supported by the fact that new educational requirements dictated by LOGSE were heavily underfunded, and that non-compulsory education stages such as preschool suffered the heaviest consequences, struggling to meet society's needs and expectations (Calero and Bonal 1999; Rambla and Bonal 2000; Bonal et al. 2005). Preschool-specific literature confirms how how excess demand for public preschool places was and still is an ever present feature of the Spanish context (Felfe, Nollenberger, and Rodríguez-Planas 2015; González and Vidal Torre 2006; González and Vidal Torre 2005; González 2004; González and Quiroga 2003; Berea 1992).

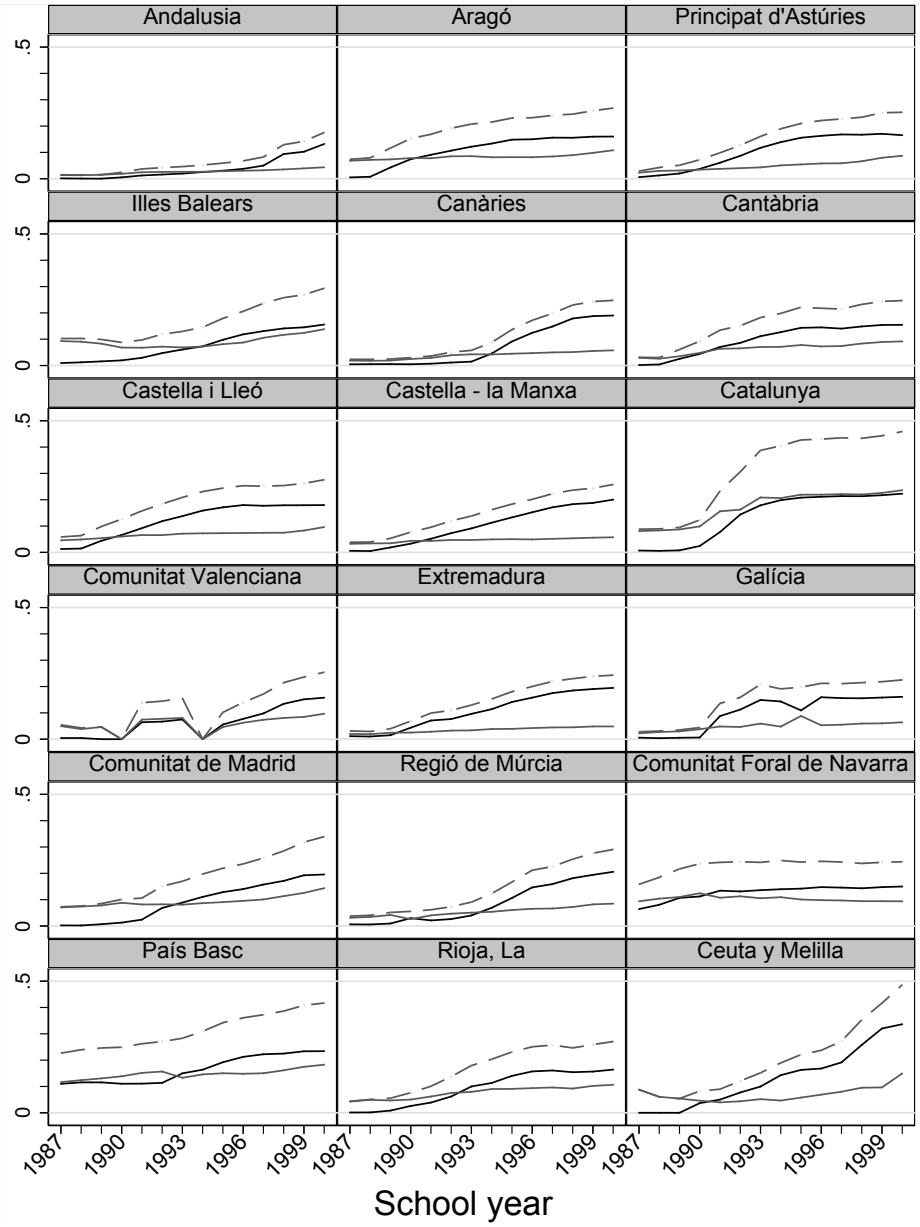
3.1.1 Determinants of the cross-regional heterogeneity

Given that the cross-regional variation in preschool access lies at the heart of our empirical identification strategy, it is important to understand its origins – and to establish whether these might be directly or indirectly related to our outcome of interest, crime prevalence by region of origin.

Several studies on the topic have failed to identify any single most important determinant explaining the timing in the adoption of the 1990 educational rules across Spanish regions, or justifying

[7] On the other hand, the involvement of the more local level (provinces and municipalities) in educational matters is, in general, quite limited in Spain. Using budgetary data from the Spanish Ministry of Economics for 2001 we can compute the share of public expenditures devoted to education by local governments . For instance, municipalities in Andalusia devoted on average 3.5% and provinces 2.3% of their total expenditures to education, while in Catalonia it was 5.5% and 7.5% respectively. *URL: http://serviciostelematicosext.minhap.gob.es/SGCAL/entidadeslocales/ (accessed November 2018).*

Figure 3: 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.

the territorial differences in public preschool supply (González and Quiroga 2003; Flaquer and Oliver 2002). Differences in education policy have been attributed to a complex blend of local wealth, political opinions on priorities, differences in local financing models, as well as differences in the local socioeconomic structure such as past history, economic development, demographic distribution and migration (Bonal et al. 2005). Other studies have related public childcare supply to population size and its concentration, to private households' income and to women's presence in local government (González and Vidal Torre 2005).

Based on this literature, we want to assess whether we are able to identify any significant relationship between public early preschool expansion and regional characteristics in our setup and with our data, in order to adequately control for these in our empirical model. In Table 1, we regress early preschool enrollment rates at the regional level for the period 1987-2000 on a number of potential determinants: to proxy for institutional and financial factors, we insert a dummy variable for the 1990 LOGSE reform; the regional public capital expenditure as % of GDP; the per capita stock of public infrastructures devoted to education. To account for socio-economic factors, we introduce regional GDP per capita; youth (16-19) unemployment rate and the education level of active population. We account for the expansion of the female employment rate with data from the Economically Active Population Survey (INE),[8] although it is not clear whether female labor force participation should be viewed as a determinant or an outcome of early childcare expansion. We also introduce a dummy variable indicating the regional government with a left party in government, in order to account for political preferences of regional governments towards educational policies.

Beyond these 'usual' factors, it is important for our setting to assess the presence of any correlation between early preschool access and contemporaneous crime rates, back in the Nineties. If such a correlation existed, it would become harder to argue that any impact of preschool found on future crime rates does not actually reflect a spurious relationship that has been existing historically. For the 1990s, there is no availability of detailed crime data of the same sort we employ for our main analysis on the 2000s; however, we retrieved data from the General Attorney's Office from the Spanish Ministry of Justice, recording regional court activity, which constitutes a fairly good proxy for crime rates.[9]

After controlling for regional and year fixed effects, only the LOGSE reform dummy variable shows a statistically significant relationship with early preschool enrollment rates. In line with previous literature, we do not detect any further significant correlation between the regional expansion pattern of public preschool and the potential expansion determinants we have considered.[10]

[8] Figure A.4 in the Appendix shows the positive correlation between the activity rate of women with at least one child 0-2 and the percentage of children aged 0-2 schooled per province in 2001.

[9] 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.

[10] We have performed various robustness exercises to the results presented in Table 1 and we obtain very similar results using as dependent variable total preschool enrollment rates, and using as potential determinants current

Table 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/1,000p		–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	✓	✓	✓	✓	✓
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: * p<0.10, ** p<0.05, *** p<0.01. Data sources: GDP > Spanish Ministry of Finance and Public Administration; stock of public capital in education and education of active population > Instituto Valenciano de Investigaciones Económicas, IVIE; GDP per capita and youth unemployment > Source: Spanish National Statistical Institute (INE). The education level of active population is defined as the percentage of working-age population with only primary education or less.

expenditures as a share of GDP (instead of capital expenditures) or the education level of the employed population (instead of the education level of the active population).

4 Empirical strategy and data

4.1 Early preschool access

Our main variable of interest consists in a measure of early preschool access, which we construct as follows:

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

where c indexes birth cohorts and i indexes Spanish regions of origin. For example, the early preschool exposure of a child born in 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. Data on regional preschool enrollment stems from the records of the Spanish Ministry of Education[11] 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]

Only from the academic year 1993/94 onward the Ministry has been collecting enrollment information by each age separately, while until that point it was done by age groups 0-3 and 4-5. This makes it unfeasible for us to use original enrollment figures for three-year olds only, so we were faced with the choice of using rates for the 0-3 age group or resorting to imputation strategies to single out 3-year olds. We chose the former option because in our setting it is crucial to work with *unaltered* evolutions of regional enrollment rates, and any arbitrary imputation choice may cause unwanted distortions. Additionally, Figure 2 showed how enrollment rates of 0-2 year olds are both very low and quite stationary over our period of analysis. As a consequence, by using the overall 0-3 enrollment rates, we are certainly underestimating the true preschool access of 3-year olds, but are unlikely to significantly alter the over-time variation in rates – which is what mostly matters for our empirical analysis.

Returning to our definition of preschool access, and in the light of the previous considerations, we measure early preschool access for cohort c born in region i as the enrollment rates for 0-3 year olds which were recorded in that region i in school year $c+3$ – that is, when our reference birth cohort c is three years old.

[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 (see Section 8).

[12] More specifically, from the Municipal Register of resident population (2000-2014).

4.2 Criminal outcomes

We have administrative data on crime actions committed by identified offenders in each municipality in the region of Catalonia over the years 2009-2014.[13] Available information includes date and region of birth of the crime author, timing and location of the crime action, and a classification of the type of crime committed. Our outcome variable consists in crime rates constructed as follows:

$$C_{c,i,m,y} = \frac{Actions_{c,i,m,y}}{Residents_{c,i,m,y}} \cdot 1,000$$

with count of crime actions at the numerator and count of residents at the denominator. That is, $C_{c,i,m,y}$ measures crimes per 1,000 residents belonging to birth cohort c, born in region i and living in Catalan municipality m in year y. Resuming the example used for preschool access, 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 2009-2014.

4.3 Overdispersion and negative binomial regression

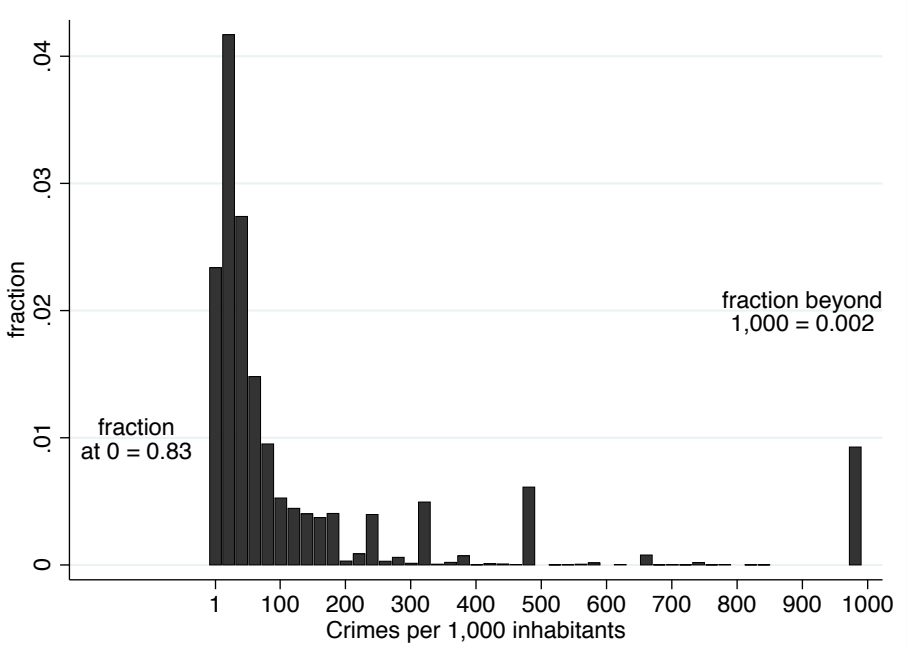
Our outcome variable is count data – number of criminals per 1,000 residents – and is characterized by high dispersion. This can be observed looking at descriptive statistics in Table A.1, in which crime rate variance shows multiple times higher than its mean, as well as in Figure 4 here below, revealing a large mass at zero[14] and a long right tail. Given the need to accommodate both count data features and high dispersion in the outcome variable, we opt for negative binomial (NB) regression as our preferred estimation strategy: negative binomial regression models the number of occurrences of an event when the event has extra-Poisson variation, that is, when overdispersion occurrs. The remainder of this Section illustrates our modeling strategy more formally, following Cameron and Trivedi (2010), Long and Freese (2014) and Hilbe (2011).

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 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 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 overdispersion

[13] Source: Catalan police authority (*Mossos d'Esquadra*).

[14] 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 – or in other words, for part of the population there is no chance to observe any outcome different from zero. In our case however, we do not have any reasons to assume or impose zero-counts, a priori, for any specific (*birth cohort, region of origin, municipality of residence, year of observation*) tuple.

Figure 4: Crime rates distribution



Note: The figure plots the distribution of crime rates in our sample. The fraction at 0 (83%) and the fraction beyond 1,000 crimes per 1,000 inhabitants (0.2%) are omitted from the graph for the sake of clarity.

or shape parameter.[15] 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

$$e^{v_j} \sim \text{Gamma}\left(1/\alpha, \alpha\right)$$

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.[16]

We always compare our preferred NB specifications to the naive OLS option – which does not appropriately account for neither the count nature of the outcome variable nor for its overdispersion – and to the Poisson alternative with robust standard errors which produces unbiased coefficient estimates but whose fit to the data is decidedly inferior to NB (see Appendix B).

4.4 Estimation approach

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, individuals belonging to the same birth cohort are more or less likely to have attended preschool at age three, depending on which region they spent their early childhood in. At the same time, individuals from the same region of origin faced different probabilities of age three preschool attendance, depending on the birth cohort they belong to.

In our baseline specification, the covariate vector $\boldsymbol{x_j}$ simply includes early preschool access and fixed effects for birth-cohort and region of origin. That is, our empirical model equation is the following:

[15] The higher α, the larger the overdispersion; with $\alpha = 0$ we are back to the Poisson regression model.

[16] 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 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 invaried to such additions.

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

where C_c indicate cohort dummies and R_i indicate region of origin dummies. This baseline specification represents the core of our identification strategy: variation in criminal behavior is associated to variation in early preschool access over time and space, after accounting for birth cohort effects and region of origin effects.

In order to test the robustness of our estimates against a number of potential concerns, we gradually allow expected outcomes to depend on further elements. We introduce local fixed effects and controls to reduce potential omitted variable bias arising 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 conditions faced by individuals during childhood in their regions of origin, beyond the level of early preschool access, to alleviate concerns about other time-varying factors correlating to preschool exposure and co-determining later criminal behavior. Our most complete specification can be illustrated as follows:

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

where V_p indicate province dummies, $\boldsymbol{M_m}$ indicates a vector of municipal control variables (unemployment rates, number of police officers per $1,000$ 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 more parsimonious baseline specification.

Recall that in our setup, each observation refers to a tuple 'birth cohort c, born in region i, living in municipality m, in year y'. Our main sample consists of 153,924 such (c, i, m, y) combinations. We look at the 10 birth cohorts between 1984 and 1993, there are 18 regions in Spain,[18] and 948 municipalities in Catalonia, which we observe over 6 years (2009-2014). However, not all cohort-region combinations are found in all municipalities and in all years: that is, in some municipalities and in some years, there are no residents born in region i in cohort c. Figure A.2 in the Appendix 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 (almost

[17] Because of the large number of municipalities (over 900), we opt for introducing province dummies and municipal-level controls rather than municipality dummies. We always cluster standard errors at the municipality level.

[18] 17 Autonomous Communities plus the two Autonomous Cities of Ceuta and Melilla considered jointly.

all Catalan-born cohorts are represented in every Catalan municipality and year), but the remaining regions constitute a sample fraction going from around 60% for older cohorts to around 45% for younger ones (younger cohorts born in other regions have had less time to migrate to Catalonia and are thus less likely to occur in our sample).

5 Assumptions behind the empirical setup

This section discusses the main assumptions on which our identification approach is based. The first concerns the correspondence between region of birth and region of residence at preschool age. The second concerns the possible impact of preschool access on cross-regional migration patterns and on crime-age profiles.[19] Next, we describe each assumption in more detail, we present statistical or descriptive evidence backing it, and we discuss how its failure would affect our results.

5.1 Region of birth and region of residence at preschool age

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. Another way to view this is we assume that individuals born in regions different from Catalonia have migrated to Catalonia after the age of three.

Mobility of children under the age of 3 is quite low across Spanish regions, as we can observe in Table 2. For each Spanish region, Table 2 shows the discrepancies between the number of children born in a specific year and the number of 3-year olds residing in the same region three years later in time. Discrepancies are expressed in % differences and are averaged across cohorts 1984 to 1997. The average 3-year old resident cohort is only 0.22% smaller than the respective birth cohort; the maximum discrepancy is found in the Autonomous Cities of Ceuta and Melilla (-1.47%) and in the Basque Country (-1.44%). Internal migration of very young children thus seemed quite limited for the cohorts we work with.[20]

Those young adults who were *not* residing in their region of birth at age three are allocated a wrong preschool exposure 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

[19] A further implicit assumption in our analysis concerns the construction of our outcome variable, criminal behavior (see section 4.2). Our municipal crime rates $C_{i,c,m}$ view criminals as forming part of the resident population in the municipality in which they commit the crime, thus ignoring the possibility of having crimes "away from home" (i.e., in another municipality). Nevertheless, this assumption ought not to play a particularly important role: if criminals commit crime actions in municipalities different from the one of residence, 'true' crime rates will be inflated in the former but deflated in the latter, merely contributing to estimation noise. A more careful treatment of this kind of criminal mobility goes beyond the scope of this paper.

[20] Even more so if we note that cohort sizes are eroded over time also by migration abroad and child deaths.

Table 2: 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 size of the 3-year old residents cohort and the size of the respective birth cohort (average across birth cohorts 1984-1997). Source: municipal residence records, elaborated by the Spanish National Statistics Agency (INE).

(preschool), and thus works against being able to pin down any significant effect. Nevertheless, a non-random misallocation could potentially bias our results in any direction. Therefore, despite the low childhood mobility we document, we correct for it in Section 8 by constructing a between-region transition probability matrix and weighting our estimates accordingly, following the method by Card and Krueger (1992). We show that our main results remain essentially unaltered.

5.2 Impacts of preschool exposure on cross-regional migration and on the crime-age distribution

The changes in criminal activity we observe across Catalan municipalities may reflect an actual modification of criminal attitudes in individuals that were exposed to the public preschool program, i.e. a real treatment effect. On the other hand, the observed changes may potentially encompass compositional effects as well, if preschool access affects cross-regional migration decisions or the age at which crime is committed. We address these concerns with a battery of exercises investigating any relationship between preschool exposure, migration behavior and crime-age distributions.

One might argue that increased preschool access at their regions of origin decreases the likelihood for children and young adults to migrate to Catalonia - while their crime propensity has not changed. Then, we would observe reduced crime rates in Catalonia and misinterpret these as a treatment effect when they instead embody mere changes in the observed sample which correlate with preschool access. Therefore, we first check whether our preschool exposure measure has any predictive power on the quantity of internal migrants that we observe in Catalonia. Internal migrants are individuals belonging to our birth cohorts (1984-1997) that we observe residing in Catalonia during our observation window (2009-2014) and were born in a different Spanish region. Table 3 shows OLS regressions of the share of internal migrants on a number of 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.

Table 3: 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	1,428	1,428	1,428
R-squared	0.22	0.86	0.87

Note: * p<0.10, ** p<0.05, *** p<0.01.

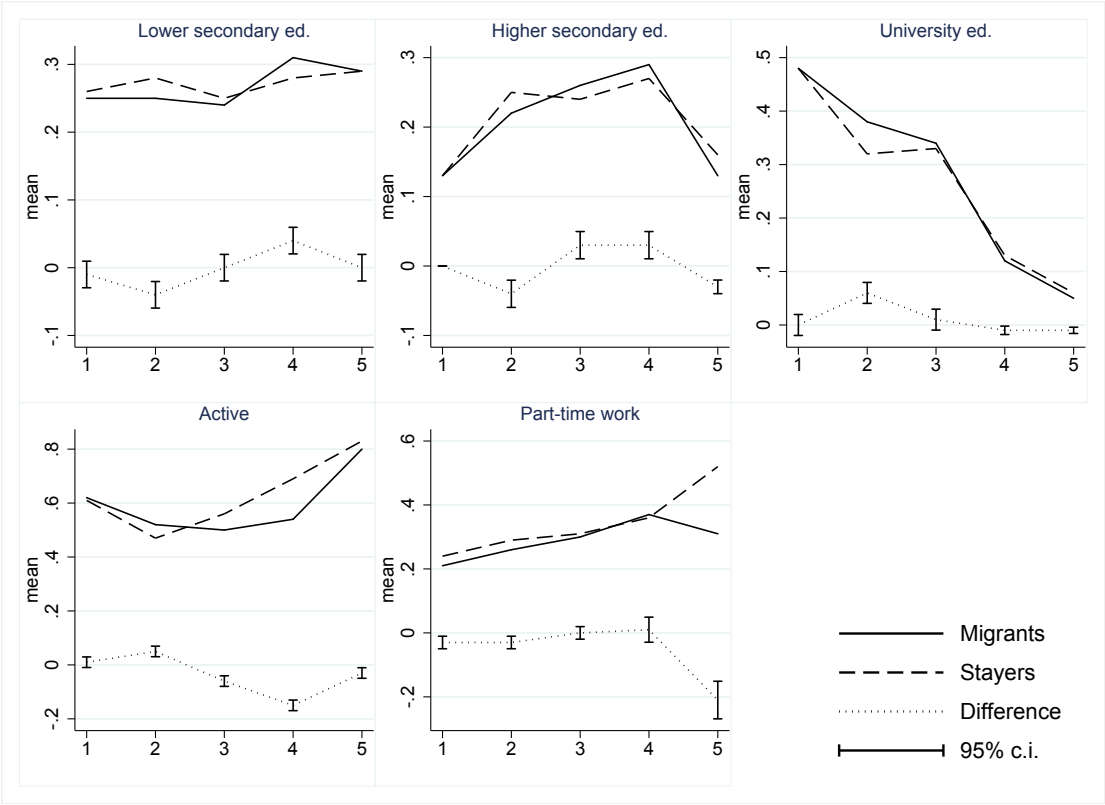
Our second exercise aims at understanding whether preschool access might affect not the *quantity* but the *type* of internal migration. To do so, we look for our birth cohorts in 2011 Census data[21] and we classify individuals into migrants or stayers according to their residence and birth information. We then cross-tabulate the individual characteristics of the two groups against quintiles of preschool exposure, looking for any significant trends in the differences between migrants and stayers. Figure 5 illustrates our exercise graphically and Table A.6 in Appendix C shows the detailed figures underlying the graphs. According to our results, differences in education or labor market activity do not seem to correlate with preschool access in any systematic way.[22]

Finally, the interpretation of our main results may change if preschool merely changed the *timing* or the *age dispersion* of criminal behavior among youths - rather than its intensity. Similarly to the arguments regarding migration behavior, if preschool affects the timing of crime, our results might be

[21] We employ the 5-percent Census microdata sample available for public use (INE.es).

[22] In a similar exercise shown in Table A.6, also in appendix C, 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 Figure A.5.

Figure 5: Characteristics of internal migrants by preschool exposure quintile



Note: For internal migrants and stayers, the figure plots shares of individuals by completed education level (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. The difference in shares is dotted, and 95% confidence intervals are capped. Source: 2011 Census microdata (5-Percent Public Use Microdata Sample). Table A.6 in the Appendix shows the figures on which the graphs are drawn.

reflecting a change in the observed sample rather than a true treatment effect. With the regressions shown in Table 4 we verify whether characteristics of the crime-age distributions are correlated with our preschool exposure 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 exposure 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, mitigating concerns about compositional effects of the sort described underlying our results.

Table 4: Characteristics of the crime age distributions

	(1) Peak age	(2) Low age	(3) Peak / Low	(4) sd	(5) 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	1,512	1,512	954	1,512	1,482
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: SE clustered by region of origin in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

6 Main results and potential mechanisms

Table 5 illustrates our main results. Column 1 shows that the most basic relationship between early preschool exposure and crime rates, without accounting for neither birth cohort nor region of origin effects, is negative and significant: that is, higher levels of early preschool are on average associated with lower crime rates. Column 2 shows the results of our baseline model (1), which represents our preferred baseline specification. Columns 3 to 5 show the results of increasingly rich specifications, as previously described in Section 4.4, and Column 5 corresponds to our most complete model (2). As we can see, the estimates for public preschool rates remain robust to the inclusion of various local and time fixed effects and controls, corroborating our assumption that, in this Spanish context we are examining, variation in public preschool rates can be considered as good as randomly assigned

after conditioning on birth cohort and region of origin.[23]

Table 5: The effect of early preschool access on crime rates

	Negative Binomial					Poisson	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Public preschool rate (0-3)	–3.21***	–1.60***	–1.59***	–1.64***	–1.37*	–1.00	–52.54**
	(0.22,)	(0.53,)	(0.54,)	(0.63,)	(0.72,)	(0.64,)	(25.32,)
Unemployment/1,000p				0.02***	0.02***	0.02***	0.47***
				(0.00,)	(0.00,)	(0.00,)	(0.08,)
Police/1,000p				0.18***	0.18***	0.13***	4.78***
				(0.04,)	(0.04,)	(0.03,)	(1.25,)
Mun. revenue pc				–0.19	–0.19	–0.32*	–7.63*
				(0.12,)	(0.12,)	(0.19,)	(4.09,)
Mun. expenditure pc				0.18	0.18	0.33*	8.59*
				(0.13,)	(0.13,)	(0.20,)	(4.46,)
GDP pc (age 3)					–41.27	–33.21	203.93
					(33.55,)	(32.32,)	(,865.68,)
Unemployment (age 3)					–0.01	–0.01	–0.44
					(0.01,)	(0.02,)	(0.59,)
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	
alpha	36.29***	35.84***	35.70***	35.24***	35.24***		

Note: SE clustered by municipality in parentheses. Each observation represents cohort 'c' born in region 'i' living in Catalan municipality 'm' in year 'y'. * p<0.10, ** p<0.05, *** p<0.01.

In terms of magnitudes, our main results imply that for an average birth cohort of children, a 1

[23]Another way to express this is that, conditional on birth cohort and region of origin controls, 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 $P_{c,i} = 0$.

percentage point increase in public preschool exposure at the region of origin yields 1.6% fewer crime actions during young adulthood, in the years 2009-2014. That is after taking into account region of origin, birth cohort, time and local fixed effects, as well as a battery of local time-varying controls.[24]

The literature on preschool impacts points at two main potential mechanisms behind our results: an improvement in cognitive skills and an improvement in non-cognitive or socio-emotional skills, both of which have the potential to lead to a reduction of criminal activities. An improvement in cognitive skills lower crime propensity through better economic opportunities and higher income from legal activities - as outlined in the groundbreaking economic model by Becker (1968). Somewhat less salient in the Economics literature but widely documented in criminology, medical and psychological literature, improvements on the non-cognitive dimension such as more self-control and lower tendency to aggressive behavior reduce criminal occurrences through criminal impulse moderation (among many others, see Wikström and Svensson 2010; Wikström and Treiber 2007; Burt, R. L. Simons, and L. G. Simons 2006; Cauffman, Steinberg, and Piquero 2005; Junger and Tremblay 1999; Burton Jr et al. 1998; White et al. 1994; Keane, Maxim, and Teevan 1993; Brownfield and Sorenson 1993; Hirschi 1969).

In order to obtain a better understanding of which mechanism is more likely to be at play in our context, we enter a more detailed analysis of crime categories. Given that we are able to exploit an unusually rich and accurate crime dataset, we classify crime actions in types according to their most likely kind of underlying motivation. Subsequently, we estimate our empirical model separately by crime categories and verify which of them are most responsive to preschool access.

An initial and broad classification, typically found in the literature, is between property and violent crime. This division, although loose, can be viewed as moving towards our purpose: for property crime actions, the economic reward is typically the main goal for offenders. On the other hand, violent crime often arises from situations in which the economic reward is not the primary trigger. Property crime is the most common type of offense recorded in our dataset (54.01%)[25] and is mainly composed by theft, robberies, damages to property (vandalism), motor vehicle theft, misappropriation, fraud, falsity and house occupation. Violent crime (25.49%) includes injuries, threats, gender violence, mistreatments within the family, evasion of family responsibilities and sexual crimes. Finally there is third typology of actions that is hard to classify as either property or violent crimes, which we label as "other crimes", accounting for 20.50% of total recorded crimes in our dataset: these are mostly composed by offenses against road safety, drug offenses and public-order offenses.[26]

[24] Garces, Thomas, and Currie (2002) find that, after controlling for mother fixed effects, children who attended Head Start are 8,5 percentage points less likely to have been booked or charged with a crime by age 30 with respect to their siblings who did not attend the program.

[25] Property crime is a crime to obtain money, property, or some other benefit. This may involve force, or the threat of force, in cases like robbery or extortion. Since these crimes are committed in order to enrich the perpetrator they are considered property crimes.

[26] We have labelled as public-order offenses basically resistance and disobedience to the authority, prevarication, obstruction to justice, conspiracy and those offenses committed by public servants.

Table A.2 presents the descriptive statistics for the different types of crime analyzed.

Table 6 shows the results of estimating our empirical model separately for property crime, violent crime and other types of crimes; the set of controls used in Columns 1 to 7 correspond to those used in the respective columns of the main results Table 5. Following a 1 percentage point increase in preschool access, depending on the specification, violent crime is reduced by between 1.1% and 1.97%, crime against property is reduced by around 1.4%, while the reduction in other types of crime lies between 1.7% and 2.6%. The impact on property crime is the most stable across specifications and the most statistically significant, while the impacts on violent crime and 'other' crimes are potentially larger in magnitude but less precisely estimated. The analysis performed on this traditional crime categorization indicates responses to preschool across all crime categories, and may suggest somewhat higher ones among crime types which are tied to motivations beyond the economic reward, but lacks the precision and the flexibility to draw stronger conclusions.

Given that our data allows us to do so, we go further and bring our analysis to a more detailed level, beyond the traditional, broad classification. We propose an original categorization of crime actions according to their relation with the cognitive and non-cognitive skill spectrum.[27] The cognitive-skill aggregate is composed of 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%). We expect crime rates in this aggregate to be particularly responsive to increased preschool access if the main channel at work is an improvement in cognitive skills and consequently in labor market outcomes.

Our second crime aggregate relates to the non-cognitive skill range, including those crime actions behind which socio-emotional wellbeing, self-control, aggressive behavior, anger management and related aspects are likely to play a key role. The categories included in this aggregate are "pure violence" (18.71%) against other individuals such as injuries (physical violence) and threats (verbal violence); "drugs/addictions"[28] (2.59%); "sexual" crimes (0.57%) and "rules compliance" (17.91%), i.e. crimes committed against the authority or various sets of rules and norms. Moreover, there are crimes committed within family boundaries, separated into "gender violence" (1.27%) as defined by police records, and the more generic crimes "against the family" (4.94%), which include mistreatments of family members as well as failing the most basic family responsibilities. If preschool helped reducing crime rates by endowing children with stronger non-cognitive skills, we would expect crimes in this second aggregate to be particularly responsive to increased preschool access.[29]

[27] From the initial dataset we have deleted three typologies of crime with very small crime counts: crimes against the environment (0.32%), murder (0.27%) and arson (0.06%). Beyond the small number of occurrences, it is difficult to categorize these types across the typologies we propose.

[28] Children's behavior at age 3 is surprisingly predictive of the risk of developing addictive behaviors like problem gambling or drug misuse during adulthood, according to data from the Dunedin Study on nearly 1,000 people in New Zealand (Slutske et al. 2012).

[29] The percentages reported (also presented in Table A.2) refer to the whole 2009-2014 period. Aggregated data also show the total number of crimes divide into 50.52% of 'non-cognitive' crimes and 49.48% of 'cognitive' crimes.

Table 6: The effect of early preschool access on crime rates, by type of crime (traditional categories)

	Negative Binomial					Poisson	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A) PROPERTY							
Public preschool rate (0-3)	–2.48***	–1.36*	–1.35*	–1.46*	–1.18	–1.03	–32.86*
	(0.29,)	(0.70,)	(0.71,)	(0.85,)	(0.93,)	(0.86,)	(17.17,)
B) VIOLENT							
Public preschool rate (0-3)	–3.45***	–1.10	–1.15	–1.97*	–1.44	–0.11	–7.45
	(0.29,)	(0.90,)	(0.91,)	(1.09,)	(1.30,)	(0.92,)	(9.58,)
C) OTHER							
Public preschool rate (0-3)	–4.46***	–1.70*	–1.48	–1.14	–2.59**	–2.18*	–14.71
	(0.32,)	(1.03,)	(0.98,)	(0.98,)	(1.24,)	(1.18,)	(8.98,)
Municipal controls				✓	✓	✓	✓
Region of origin controls					✓	✓	✓
Year of birth FE		✓	✓	✓	✓	✓	✓
Region of birth FE		✓	✓	✓	✓	✓	✓
Province FE			✓	✓	✓	✓	✓
Year FE			✓	✓	✓	✓	✓
N.obs	153,924	153,924	153,924	153,924	153,924	153,924	153,924

Note: SE clustered by municipality in parentheses. Each observation represents cohort 'c' born in region 'i' living in Catalan municipality 'm' in year 'y'. * p<0.10, ** p<0.05, *** p<0.01.

Table 7 presents the results for these more disaggregated categories of offenses. Estimated impacts of increased preschool access are negative across all crime categories and model specifications, with some exceptions within "damages to property", "gender violence" and "sexual crimes". In these three categories, positive but imprecisely estimated coefficients appear until region of origin controls are added to the model.

We observe that, in general, the crime types which can be related to non-cognitive skills 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. A one percentage point increase in preschool access reduces the number of violent crime actions by proportions around 2.5% and drug crimes by proportions around 8% in youth and young adulthood. In sum, the results presented in Table 7 suggest a powerful impact of the non-cognitive skills channel in explaining the reduction of

Table 7: Cognitive versus non-cognitive channels

	Negative Binomial					Poisson	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
COGNITIVE							
Economic motivation	–2.33***	–1.58**	–1.62**	–1.41	–0.62	–0.81	–25.14*
	(0.35,)	(0.77,)	(0.79,)	(0.93,)	(0.99,)	(0.94,)	(14.61,)
Damages to property	0.54	1.82	1.81	1.71	–0.06	0.18	–0.54
	(0.56,)	(1.57,)	(1.65,)	(1.93,)	(2.33,)	(1.81,)	(3.46,)
Fraud and falsity	–6.77***	–3.21**	–2.64	–3.26*	–3.39	–2.95	–7.20
	(0.66,)	(1.61,)	(1.65,)	(1.87,)	(2.36,)	(1.85,)	(4.68,)
NON-COGNITIVE							
Pure violence	–2.86***	–1.58	–1.86*	–2.76**	–2.32*	–0.79	–11.53
	(0.33,)	(0.98,)	(1.00,)	(1.17,)	(1.36,)	(1.02,)	(7.84,)
Rules compliance	–4.57***	–1.36	–1.13	–0.72	–2.37*	–2.13*	–12.24
	(0.33,)	(1.10,)	(1.06,)	(1.07,)	(1.32,)	(1.22,)	(8.69,)
Drugs	–3.22***	–6.98**	–7.65***	–8.22***	–9.15**	–2.89	–2.47
	(0.92,)	(2.94,)	(2.91,)	(3.06,)	(3.71,)	(3.52,)	(2.10,)
Against the family	–5.79***	–1.43	–1.08	–2.22	–0.41	0.78	1.26
	(0.50,)	(1.64,)	(1.72,)	(2.22,)	(2.70,)	(1.64,)	(3.76,)
Gender violence	–5.13***	4.34	2.76	0.54	–1.47	5.64	3.23*
	(1.03,)	(2.74,)	(3.15,)	(3.36,)	(3.63,)	(3.67,)	(1.95,)
Sexual crimes	1.87	0.87	0.10	4.43	–5.36	–1.20	–0.40
	(1.96,)	(4.82,)	(5.09,)	(5.44,)	(6.36,)	(5.67,)	(0.71,)
Local controls				✓	✓	✓	✓
Region of origin controls					✓	✓	✓
Year of birth FE		✓	✓	✓	✓	✓	✓
Region of birth FE		✓	✓	✓	✓	✓	✓
Province FE			✓	✓	✓	✓	✓
Year FE			✓	✓	✓	✓	✓
N.obs	153,924	153,924	153,924	153,924	153,924	153,924	153,924

Note: The table reports early preschool exposure coefficient estimates for different types of crime. The model specifications (columns) are the same used for our main results. SE clustered by municipality in parentheses. * p<0.10, ** p<0.05, *** p<0.01

crimes rates we observe as a consequence of the improved access to a high quality public preschool system.

7 Discussion

As reviewed in Section 2, the effectiveness of high-quality preschool programs in reducing crime during youth and young adulthood is supported by several analyses of several US programs such as the Head Start, the High Scope / Perry, the Abecedarian and the CARE projects. Due to the reduced sample sizes and the necessarily disadvantaged socioeconomic backgrounds characterizing participation in those studies, it is difficult to perform a direct comparison between their effect magnitudes and our results, but all four programs were found to have led to strong and significant reductions both in lifetime arrests and in self-reported misbehavior. Garces, Thomas, and Currie (2002), who evaluate the long-term effects of the Head Start program through sibling comparisons, find that self-reported brushes with the law by age 30 are significantly lower for black participants who participated to the program - but do not distinguish between different types of misdemeanors. Schweinhart et al. (2005) looks at the Perry program and finds significant differences between participant and control groups in crime rates, arrest rates and prison time, and across a wide range of crime types: violent, property and drug crimes. These differences are found at various times of life: starting already in adolescence, continuing over early adulthood and through midlife. Our particularly strong result on drug crime reduction is in line with findings by Campbell et al. (2008; 2002), who identify large and significant reductions in marijuana and other illegal substance abuse among young adults who participated to the Abecedarian and CARE preschool projects. On the other hand, these authors report no significant findings on self-reported violent behavior in their sample of analysis.

In line with the objective of the second part of our analysis, an influential paper by Heckman, Pinto, and P. Savelyev (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 had turned out short-termed, improvements in personality skills had 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.

Compared to Heckman, Pinto, and P. Savelyev (2013), our analysis of the Spanish context has both advantages and disadvantages. The most significant disadvantage is the unavailability of skill data for our population of analysis: in this sense, we are missing an empirical measurement of 'intermediate outcomes' between preschool access in early childhood and criminal activities in young

adulthood. Being able to provide 'first-stage evidence' showing that the new Spanish public preschool system introduced in the Nineties was able, like the Perry program, to boost non-cognitive skills in its participants would represent a clear gain for this study. Instead, our focus goes directly to the subsequent outcomes of improved socio-emotional competencies, and in particular to criminal behavior. On the other hand, we are proposing a novel approach to infer the importance of the cognitive versus non-cognitive skills channel in explaining effects on crime, taking advantage of the high-quality crime data at our disposal. Contrary to the analyses by Heckman and coauthors, and to other papers evaluating preschool programs in the same spirit, our crime categories are much finer and more detailed, allowing to relate them to either skill group with a certain degree of 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 contexts in which lack of intermediate skill outcomes is a limitation and rich crime data is available instead.

A further aspect about our approach which is worth discussing is the fact that crime actions can sometimes 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 a heated reaction to the separation events. Depending on the particular case thus, the causes of failure to pay spousal support should be identified within the cognitive sphere or among the non-cognitive set of skills. The classification we use, presented in Tables 7 and A.2, is guided by criminology literature as well as common sense, and should be viewed as our proposal on how to interpret empirical results.

Finally, it is worth dedicating some thoughts to the distribution of our treatment effect across the population of Spanish children which was affected by the preschool expansion program. More specifically, existing literature strongly suggests that children raised in rather disadvantaged socioeconomic backgrounds are the ones benefiting the most from the introduction of good quality preschool programs. In the Spanish case we are analyzing, the public preschool offer was not limited to specific socio-economic categories but open to all families. Unfortunately we are unable to carry out an empirical heterogeneity analysis on this dimension, due to the unavailability of sociodemographic information on offenders in our dataset. However, reaching back to the three factors determining the results of a childcare expansion program, outlined by Blanden et al. (2016) and already introduced in Section 3, we can develop some useful considerations. The first factor, the quality of provided public childcare, is unlikely to help forming conjectures about which parts of the user population benefited the most from the program: quality of childcare was the same for all users of the public system, as rules and curricula were established centrally. On the other hand, the second and third factor, i.e. quality of the alternative care solutions and quantity and quality of (lost) time with parents, are more likely to give us hints about the direction of heterogeneity in our effects. We agree with Blanden et al. (2016) in their observation that presumably the quality of alternative care received

by children at home or with friends and the extended family differs by background, and that we expect effects of a high quality formal childcare service to be more positive for children from families of lower socio-economic status. The same conclusion applies if we consider the lost time with parents, which has been found to be of both higher quantity and quality for children raised in more advantaged contexts (Guryan, Hurst, and Kearney 2008; Kimmel and Connelly 2007). In conclusion, based on these considerations and even though we are unable to show this empirically, there are reasons to think that in the Spanish context, like in others before, the public preschool expansion program helped reducing inequalities in society by benefiting disadvantaged children more than those from wealthier families. In turn, we conjecture the reduction in crime to be driven mainly by those more disadvantaged children later in time, that is, by a reduction of criminal activity at the hand of individuals raised in lower socio-economic backgrounds.

7.1 Cost-benefit calculations

The results obtained for the more disaggregated types of crime allows us to provide a back-of-the-envelope cost-benefit analysis of the Spanish preschool expansion. In particular, we aim at relating the economic value of crime reduction to the cost of the expansion.[30]

Without addressing thorough rates of return of the program,[31] we limit ourselves to calculating the average annual benefit-to-cost ratio over our period of observation (2009-2014). In Appendix D we present the data used and the calculations performed to obtain an annual benefit-to-cost ratio for a 1 percentage point expansion of a preschool enrollment rate. More precisely, we recover the unit costs of crime by type and combine these with our estimates on the impact of increased preschool access on crime actions, obtaining an assessment of the monetary value of reduced crime. We then relate those benefits to the cost of the expansion program, based on unit costs of one extra preschool spot in Spain.

For the case of Catalonia, a 1pp increase in public preschool enrollment rate would imply to increase enrollment rates from 22.3% (for instance in the year 2000) to 23.3%, which translates into 4,710 new posts, with an associated yearly cost of 26.8M€ (a 0.1% of the regional budget in 2004). Considering the information reported in Appendix Table A.8 and the fact that we are able to use only approximately two thirds of the overall crime volume we use in the rest of the paper, the lower-bound estimate of associated benefits is 113.9M€. That is, merely considering benefits from crime

[30] Being limited to the sole crime dimension, our exercise is much less comprehensive than the one performed by Heckman, Moon, Pinto, P. A. Savelyev, et al. (2010) for the Perry Preschool Program, which considers a range of benefits to society beyond crime reduction – such as better education, employment, earnings and health. Moreover, we limit ourselves to the computation of cost-benefit ratios and do not

[31] This 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. Engaging in more demanding imputation exercises in order to approximate lifetime criminal activity, in the spirit of Heckman, Moon, Pinto, P. A. Savelyev, et al. (2010), lies beyond the scope of this paper.

reduction, we estimate the benefit-to-cost ratio at around 4.25 at the very least.[32] As a comparison, only in terms of crime reduction, Heckman, Moon, Pinto, P. A. Savelyev, et al. (2010) report a benefit-to-cost ratio of 7.3[33] for the Perry Program, while Rolnick and Grunewald (2003) obtain a ratio of 5.7 for the same program.

Keeping in mind the limitations of the quite simplified approach taken in this exercise, we believe it nonetheless helps us providing a useful lower-bound estimate of the economic advantages brought about by the Spanish preschool expansion. Considering nothing else than the positive consequences in terms of crime reduction, the monetary value of benefits to society exceed costs fourfold at the very least.

8 Robustness checks and placebo tests

In this section we present a battery of robustness checks and placebo tests aimed at ruling out any potential confounding elements which were not addressed in our main analysis.

In the first robustness check we estimate coefficients on total preschool access (public and private), instead of using public preschool access only. The over-time average of total preschool access is twice as high as the public one (see Table A.1), but as we described in Section 3, it was the expansion of the public component mainly driving the cross-regional variation across our period of analysis. As a result, we would not expect important changes in our results, and as we see in Table 8, row C1, these remain indeed qualitatively unaltered. Coefficient magnitudes decrease as expected, reflecting the higher mean of the new predictor variable.

In the second robustness check, we account for potentially nonrandom cross-regional mobility of children between birth and age three, as anticipated in Section 5.1. Since we only have information on the region of birth of offenders and population resident in Catalonia, but not on their residence at age three, our cohort- and region of origin- specific analysis is developed under the assumption that children born in a specific region still reside in that region at age three. Back in Table 2 we gave evidence for low mobility between ages 0 and 3 for our cohorts of analysis. Nevertheless, for the sake of completeness, here we follow Card and Krueger (1992) and construct full 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*

[32] One can decompose that ratio distinguishing between "public" and "private" costs of crime – as described in the Appendix section. Looking at the reduction in public costs only (37.1M€), we estimate the benefit-to-cost ratio around 1.4. Looking at the reduction in private costs from crime (76.8M€), the ratio would be around 2.9.

[33] 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. Similarly to the fact that we have cost estimates for only two thirds of overall crimes, excluding murders makes of our benefit-to-cost estimate a clear lower bound of the real one. Simply imputing our average unitary crime cost to the remaining third of crimes for which we are missing unit cost information, we would obtain a benefit-to-cost ratio to 6.33.

Table 8: Preschool exposure estimates under different robustness checks and placebo tests.

	Negative Binomial					Poisson	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
ROBUSTNESS CHECKS							
C1) Total enrolment	–1.86***	–1.27***	–1.24***	–1.35***	–1.36***	–1.09***	–43.04***
	(0.14,)	(0.29,)	(0.30,)	(0.34,)	(0.40,)	(0.37,)	(14.21,)
C2) Mobility adjusted	–3.21***	–1.59***	–1.58***	–1.63**	–1.35*	–0.99	–52.55**
	(0.22,)	(0.54,)	(0.55,)	(0.64,)	(0.73,)	(0.65,)	(25.58,)
C3) Province level (Note)	–2.00	–1.66	–1.90	–1.32	–1.17	–2.04	–14.57
	(1.46,)	(1.45,)	(2.39,)	(2.49,)	(2.42,)	(1.43,)	(,107.44,)
C4) Catalonia level (Note)	–0.73*	–1.51	–1.45	-	–1.61	–2.32	–233.78*
	(0.44,)	(1.87,)	(1.90,)	-	(1.93,)	(1.69,)	(,127.02,)
PLACEBO TESTS							
D1) Later preschool	–0.88***	–0.28	–0.22	–0.23	–0.27	–0.38*	–27.82*
	(0.31,)	(0.20,)	(0.18,)	(0.20,)	(0.19,)	(0.20,)	(15.20,)
D2) Random regions or cohorts: *See Figure 6.*							
Local controls				✓	✓	✓	✓
Region of origin controls					✓	✓	✓
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

Note: For provincial level specifications SE are clustered at provincial level. For regional level specifications, no local fixed effects are included and SE are clustered by region of origin. See descriptives for these two levels of aggregation in Table A.3. * p<0.10, ** p<0.05, *** p<0.01

*Residenciale*s 1988-2000, INE).[34] For each birth cohort c, the matrix P^c cross-tabulates region of

[34]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.

birth (i) and region of residence at age three (j), and its elements p^c_{ji} indicate the 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:

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

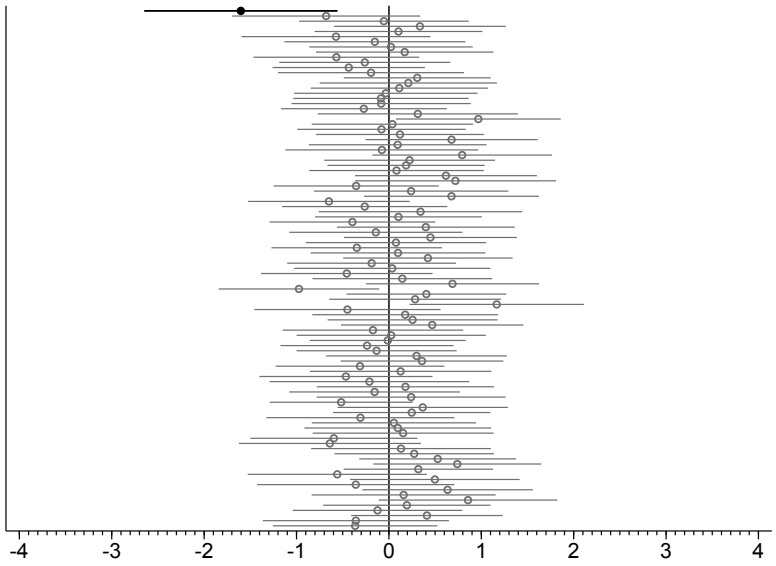
where $P_{c,j}$ are the age three preschool exposure rates for cohort c in region j. We then re-estimate our main regression models using $P^*_{c,i}$ instead of the original, unweighted $P_{c,i}$, and report results in Table 8, row C2. As we can see, estimates show only marginal changes with respect to the original ones, confirming the inconsequential role played by preschool-age mobility in our context.

Our third and fourth robustness checks aim at testing whether our conclusions are sensitive to changes in the level of geographical aggregation of our data. Row C3 and C4 in Table 8 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. 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 employ, which allows us to gain the necessary precision to pin down statistically significant effects.

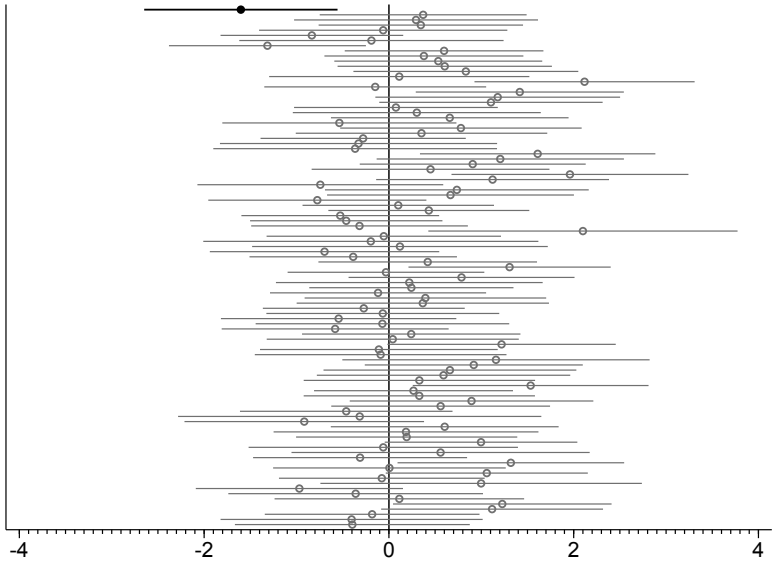
We conduct two placebo tests which have the aim of further convincing that our results reflect genuine causality between preschool access and criminal behavior later in time. One concern may be that the effect we find be driven by some omitted factors characterizing the region of origin - birth year dimension, and later influencing crime. Our main analysis already made an effort towards accounting for such factors by including region of origin and year-specific controls. If such factors existed, be they economic, social or of any other nature, they would arguably influence not only age 3 preschool rates but also age 4-5 preschool rates, in any given year and region of origin. Therefore in our first placebo test, on the right hand side we use the age 4 to 5 preschool exposure rates that were recorded in each region of origin when our cohorts were aged 3. Row D1 of Table 8 shows absence of any impact or empirical correlation with later criminal behavior of our 3-year olds. That is, an impact of preschool access is found only when we employ the age-specific access rates which were actually relevant to our cohorts back in their time and place, while no statistical relationship shows when employing the access enjoyed by slightly older peers at the same time and place.

Our second placebo test addresses a more generic concern of accidental correlation happening in our analysis. To check what type of results we would obtain in absence of any genuine relationship between our preschool exposure rates and later criminal behavior, we run simulations by swapping the real birth cohort or the real region of origin with a random different one, and show the results in

Figure 6: Random birth cohort or region of origin assignment



(a) Random birth cohort assignment



(b) Random region of origin assignment

Note: The two figures 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.

Figure 6. In both experiments, we see that our original estimations (the first on top, in black) is far off with respect to any of those simulated with fake years or regions of birth. 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.

9 Conclusions

In this paper we have estimated the impact of a nationwide preschool expansion program on crime rates two decades later, using administrative education data for Spain and administrative crime data for the region of Catalonia. The fact that the expansion was staggered across birth cohorts and Spanish regions provides the key source of variation to our analysis, which is based on a multiple-fixed-effects model.

Our main results indicate that for the average birth cohort of children, a 1 percentage point increase in public preschool access at the region of origin yields 1.6% fewer crime actions during youth and young adulthood. These results are in line with findings on several preschool initiatives based in the US, such as the Perry, Head Start, Abecedarian and CARE projects – which have been shown to lead to significant reductions in lifetime arrests and self-reported misbehavior. Our results represent an important contribution to the knowledge on the topic, since we are demonstrating that such benefits are not limited to well-known small-scale programs with highly selective access rules, but instead remain sizable even in the circumstance of a nationwide, open-to-all public preschool initiative.

Additionally, we have proposed a novel approach to infer the importance of the cognitive versus non-cognitive skills channel in explaining the effects of increased preschool access on crime, taking advantage of the unique level of detail of the crime data at our disposal. We relate each crime type committed to either the cognitive or the non-cognitive skills sphere, according to which set of skills is more likely to have influenced it and we look at our impact evaluation results separately by categories. This approach may prove helpful in other research contexts in which direct measures of cognitive and non-cognitive skills are not available.

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.

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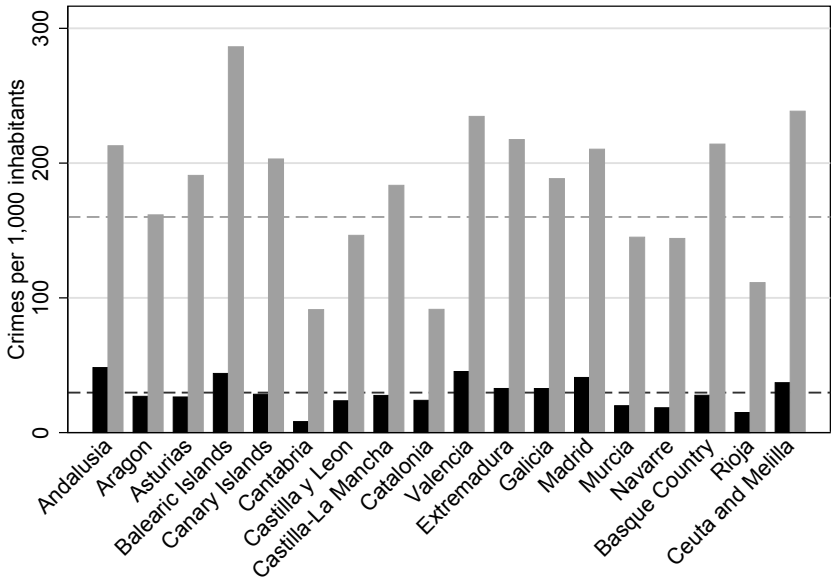
A Descriptive statistics

Table A.1: Descriptive statistics

	mean	sd	median
Crime actions /1,000pax	29.69	160.02	0.00
Property c. actions /1,000pax	13.05	99.81	0.00
Person c. actions /1,000pax	9.68	83.25	0.00
Other c. actions /1,000pax	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 /1,000pax	76.78	29.27	77.31
Police /1,000pax	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		

Figure A.1: Crime rates by regions of origin

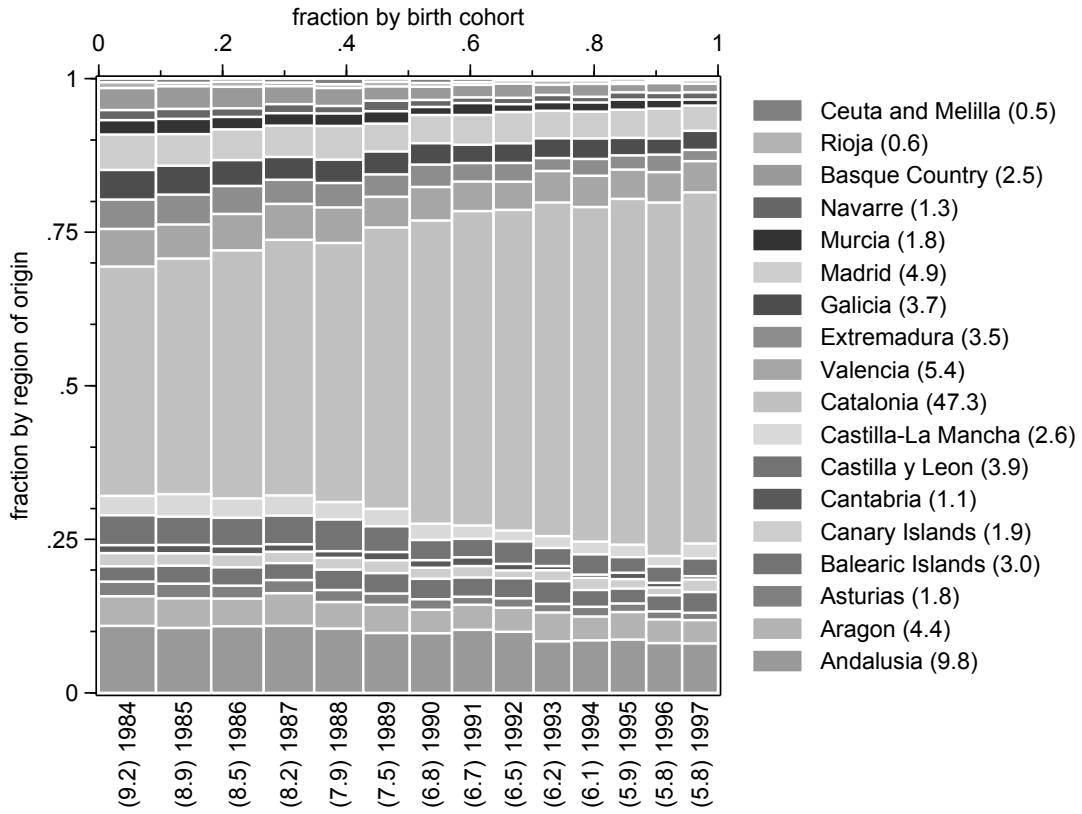


Note: The figure plots means (black) and standard deviations (gray) of crime rates by region of origin. Dashed lines indicate overall mean and standard deviation.

Table A.2: Descriptive statistics by crime typologies

	Sample weight	N	Mean	sd
PROPERTY CRIMES	54.01%	153,924	13.69	102.15
Economic motivation (COG)	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 (COG)	4.87%	153,924	1.79	31.98
Fraud and falsity (COG)	4.54%	153,924	2.03	37.59
VIOLENT CRIMES	25.49%	153,924	8.65	77.98
Pure violence (NON-COG)	18.71%	153,924	6.24	64.76
Injuries	12.50%			
Threats	6.20%			
Against the family (NON-COG)	4.94%	153,924	1.75	32.55
Mistreatments	3.93%			
Missing family responsibilities	1.01%			
Gender violence (NON-COG)	1.27%	153,924	0.50	16.62
Sexual crimes (NON-COG)	0.57%	153,924	0.15	8.67
OTHER CRIMES	20.50%	153,924	7.09	65.58
Rules compliance (NON-COG)	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 (NON-COG)	2.59%	153,924	0.61	18.31

Figure A.2: 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.

Table A.3: Descriptive statistics at higher geographical aggregation

	Provincial level			Regional level		
	mean	sd	median	mean	sd	median
Crime actions /1,000pax	34.21	77.65	0	38.55	43.84	31.00
Unemployment /1,000pax	61.16	10.90	63.88	63.45	5.69	64.03
Police /1,000pax	1.64	0.74	1.73	1.69	0.33	1.76
Local revenue percapita (Thousands of E)	1.64	0.38	1.58	1.52	0.14	1.50
Local expenditure percapita (Thousands of E)	1.61	0.36	1.57	1.49	0.13	1.48
N	5,801			1,512		

B Model fit

We assess how well our negative binomial (NB) model, in its baseline specification (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.4 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.

Table A.4: Descriptive statistics of empirical and predicted count distributions

	Empirical distribution	Negative Binomial model	Poisson model
mean	0.000,1	0.000,1	0.000,1
standard deviation	0.026,3	0.026,1	0.004,7
skewness	31.565,0	31.531,3	5.621,9
kurtosis	997.894,9	996.447,3	36.171,0
max |difference|		0.023,0	0.830,6
value at max |difference|		0	1
mean |difference|		0.000,2	0.001,8
N	1,001	1,001	1,001

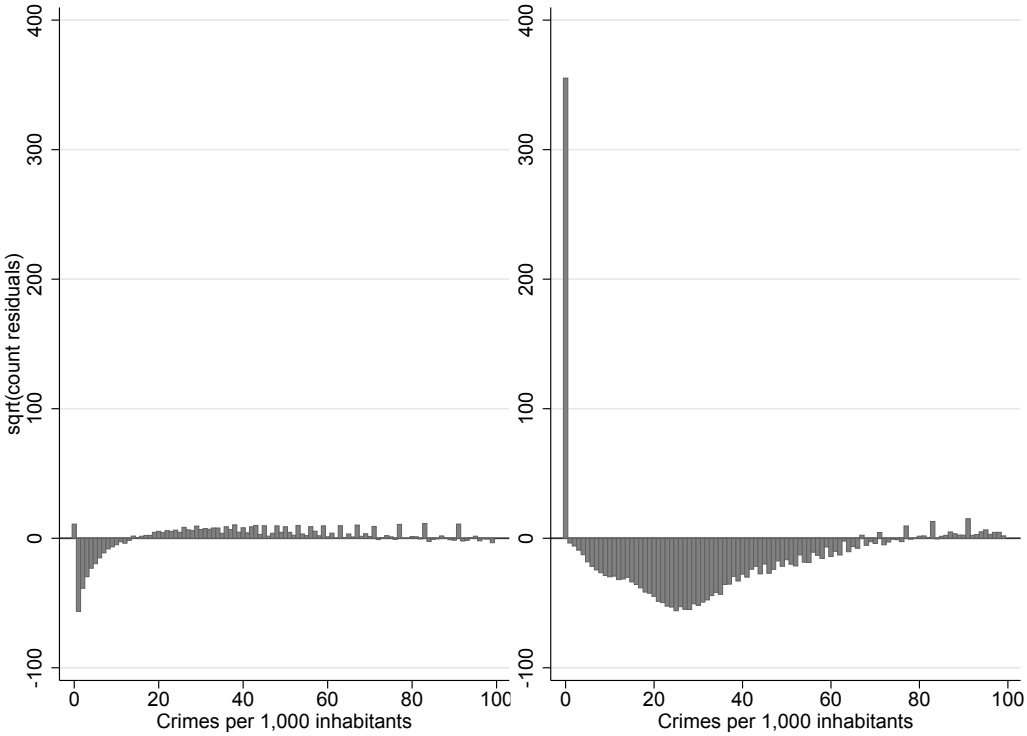
Model fit for count models can also be assessed through a rootogram (Tukey 1972, Tukey 1977, Wainer 1974, Kleiber and Zeileis 2016), which compares the empirical distribution of a variable to a theoretical distribution. The two theoretical distributions we consider are the Poisson distribution and the NB distribution, characterized by our parameter estimates as of baseline specification eq(1). Count residuals are computed as the difference between observed counts and the counts predicted by the theoretical model. The rootograms in Figure A.3 plot the square root of these count residuals,[35] [36] 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.5 reports AIC and BIC for both model choices. Unsurprisingly, these criteria too favor using the NB model over a Poisson specification in our context.

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

[36] To improve readability, we show counts up to 100 crimes per 1,000 inhabitants.

Figure A.3: Model Fit



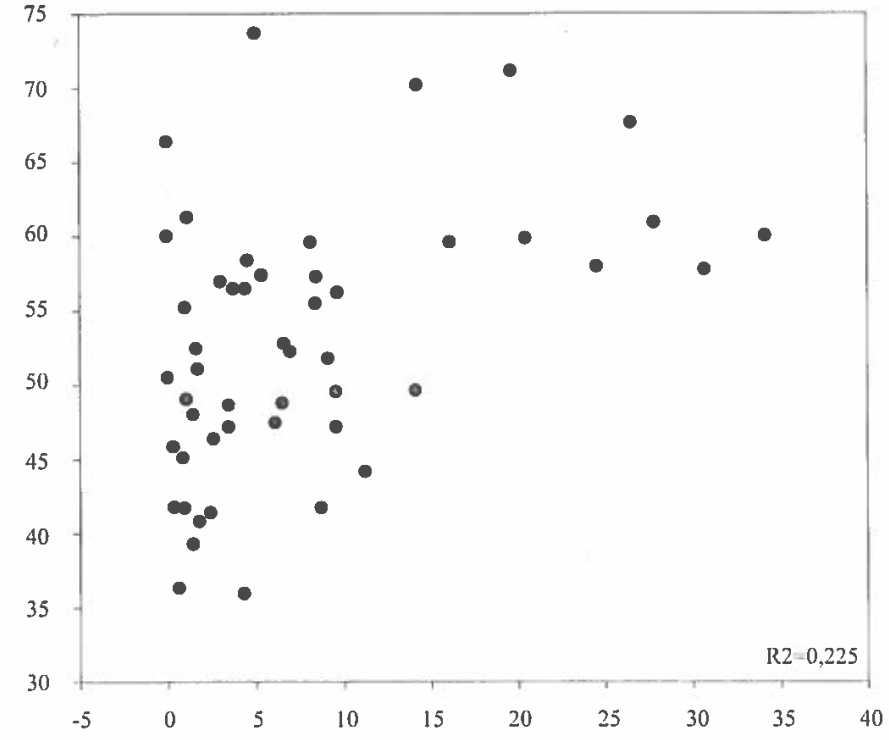
Note: The figure plots differences between observed and predicted counts produced by our baseline Negative Binomial specification (left) and the alternative Poisson specification (right).

Table A.5: Bayesian and Akaike Information Criteria for the two modeling choices

	BIC	AIC
Poisson model	2.442 x10^7	2.442 x10^7
Negative Binomial model	4.684 x10^5	4.681 x10^5
Difference	2.395 x10^7	2.395 x10^7
Prefer	NB model	NB model

C Additional supporting tables and figures

Figure A.4: Activity rates of women with at least one child 0-2 and % of 0-2 kids schooled per province in 2001



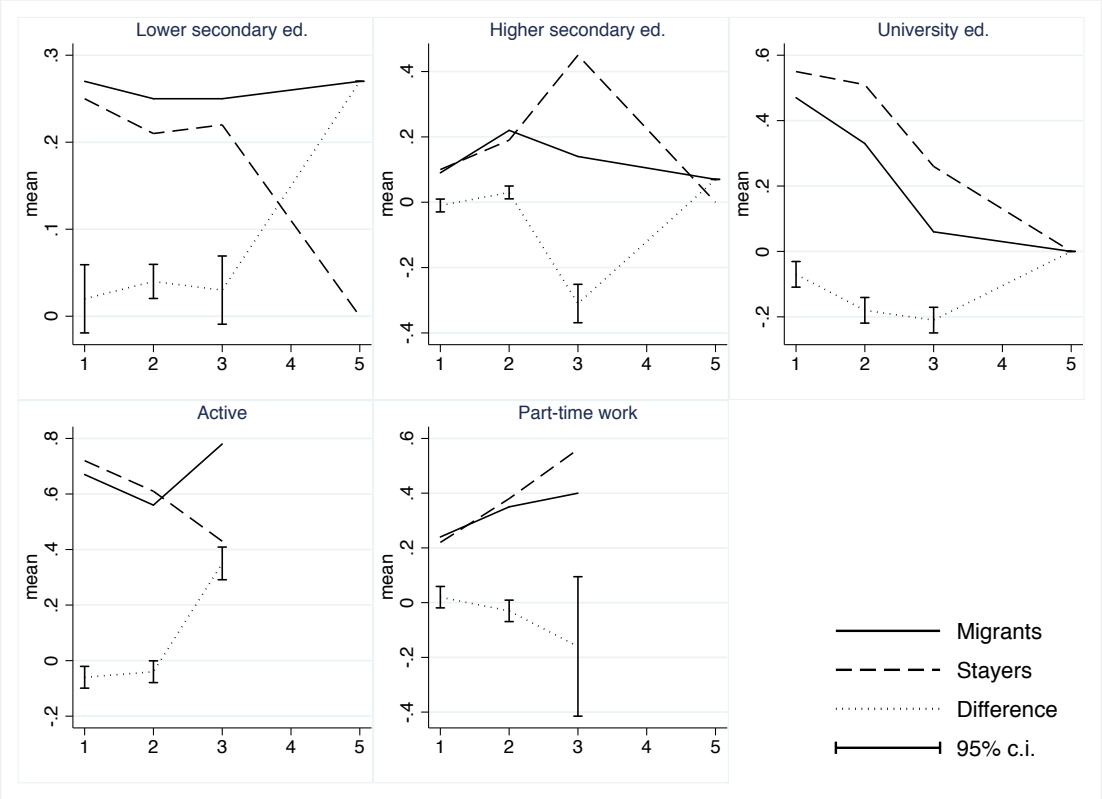
Note: Horizontal axis: 0-2 preschool enrollment rates. Vertical axis: female activity rate. Activity rates calculated with Labour Force Survey. Preschool enrollment rates from the Ministry of Education (2002) and 2001 Census. Source: González (2003), Gráfico 4, p.31.

Table A.6: 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,)	0.25	(.01,)	0.24	(.01,)	0.31	(.01,)	0.29	(.01,)
Stayers	0.26	(.00,)	0.28	(.00,)	0.25	(.00,)	0.28	(.00,)	0.29	(.00,)
Difference	-0.01***	(.01,)	-0.04***	(.01,)	-0.00	(.01,)	0.04	(.01,)	0.00	(.01,)
Education: HS										
Migrants	0.13	(.00,)	0.22	(.01,)	0.26	(.01,)	0.29	(.01,)	0.13	(.00,)
Stayers	0.13	(.00,)	0.25	(.00,)	0.24	(.00,)	0.27	(.00,)	0.16	(.00,)
Difference	0.00	(.00,)	-0.04***	(.01,)	0.03***	(.01,)	0.03***	(.01,)	-0.03***	(.00,)
Education: Uni										
Migrants	0.48	(.01,)	0.38	(.01,)	0.34	(.01,)	0.12	(.00,)	0.05	(.00,)
Stayers	0.48	(.00,)	0.32	(.00,)	0.33	(.00,)	0.13	(.00,)	0.06	(.00,)
Difference	0.00	(.01,)	0.06***	(.01,)	0.01***	(.01,)	-0.01***	(.00,)	-0.01***	(.00,)
Active										
Migrants	0.62	(.01,)	0.52	(.01,)	0.50	(.01,)	0.54	(.01,)	0.80	(.01,)
Stayers	0.61	(.00,)	0.47	(.00,)	0.56	(.00,)	0.69	(.00,)	0.83	(.00,)
Difference	0.01	(.01,)	0.05***	(.01,)	-0.06***	(.01,)	-0.15***	(.01,)	-0.03***	(.01,)
Part-time										
Migrants	0.21	(.01,)	0.26	(.01,)	0.30	(.01,)	0.37	(.02,)	0.31	(.03,)
Stayers	0.24	(.00,)	0.29	(.00,)	0.31	(.00,)	0.36	(.01,)	0.52	(.01,)
Difference	-0.03***	(.01,)	-0.03***	(.01,)	-0.00	(.01,)	0.01	(.02,)	-0.21***	(.03,)

Note: For internal migrants and stayers, the table presents shares of individuals by completed education level (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 5 shows this data graphically.

Figure A.5: Characteristics of internal migrants (to Catalonia only) by preschool exposure quintile



Note: For internal migrants and stayers, the figure plots shares of individuals by completed education level (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. 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.

Table A.7: 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	1,132
2011 Census							
Education level	7.59	13.62	1.13	0.26	0.23	255.22	2,063
Active	3.67	10.51	0.45	0.65	0.73	2.06	1,632
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].

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D 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,[37] 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 most conservative preschool impact estimates (Table 5, column 5) to approximate the number of crimes avoided as a result of the preschool expansion,[38] limiting ourselves to those

[37]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.

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

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.8. 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.8, 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.8, we show the overall 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, Serra, and Baños (2011), who value it at 5,690.27€. This estimate is consistent with the reported costs in Madrid in 2013.[39] The main results of the benefit-to-cost estimations of a preschool expansion are reported in the main text, Section 7.1.

[39]This unit cost is consistent with that reported in the news for Madrid in 2013 of 5.100€ see https://www.elmundo.es/elmundo/2013/08/27/madrid/1377605557.html (last visited March 2019). Moreover informal reports from 2017 point out to a cost of 7.000€ per year and pupil in public preschools in Barcelona, see http://nadaesgratis.es/admin/tarifacion-social-de-las-guarderias (last visited March 2019). As a reference, the estimated cost of the Perry Program per child in 2006 was $17,759 (see Heckman, Moon, Pinto, P. A. Savelyev, et al. (2010)).

Table A.8: 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 (€)
Cognitive					
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
NonCognitive					
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 1GBP = 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 7 for the period 2009-2014, and are expressed in EUR discounted to 2004.

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2013/2, Hortas Rico, M.: "Sprawl, blight and the role of urban containment policies. Evidence from US cities"
2013/3, Lampón, J.F.; Cabanelas-Lorenzo, P-; Lago-Peñas, S.: "Why firms relocate their production overseas? The answer lies inside: corporate, logistic and technological determinants"
2013/4, Montolio, D.; Planells, S.: "Does tourism boost criminal activity? Evidence from a top touristic country"
2013/5, Garcia-López, M.A.; Holl, A.; Viladecans-Marsal, E.: "Suburbanization and highways: when the Romans, the Bourbons and the first cars still shape Spanish cities"
2013/6, Bosch, N.; Espasa, M.; Montolio, D.: "Should large Spanish municipalities be financially compensated? Costs and benefits of being a capital/central municipality"
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2013/10, Jofre-Monseny, J.: "The effects of unemployment benefits on migration in lagging regions"
2013/11, Segarra, A.; García-Quevedo, J.; Teruel, M.: "Financial constraints and the failure of innovation projects"
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2013/14, Lundqvist, H.: "Is it worth it? On the returns to holding political office"
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2013/17, Guío, J.M.; Choi, A.: "Evolution of the school failure risk during the 2000 decade in Spain: analysis of Pisa results with a two-level logistic mode"
2013/18, Dahlby, B.; Rodden, J.: "A political economy model of the vertical fiscal gap and vertical fiscal imbalances in a federation"
2013/19, Acacia, F.; Cubel, M.: "Strategic voting and happiness"
2013/20, Hellerstein, J.K.; Kutzbach, M.J.; Neumark, D.: "Do labor market networks have an important spatial dimension?"
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2013/22, Lin, J.: "Regional resilience"
2013/23, Costa-Campi, M.T.; Duch-Brown, N.; García-Quevedo, J.: "R&D drivers and obstacles to innovation in the energy industry"
2013/24, Huisman, R.; Stradnic, V.; Westgaard, S.: "Renewable energy and electricity prices: indirect empirical evidence from hydro power"
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2013/26, Lambertini, L.; Mantovani, A.: "Feedback equilibria in a dynamic renewable resource oligopoly: pre-emption, voracity and exhaustion"
2013/27, Feld, L.P.; Kalb, A.; Moessinger, M.D.; Osterloh, S.: "Sovereign bond market reactions to fiscal rules and no-bailout clauses – the Swiss experience"
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2013/32, Saarimaa, T.; Tukiainen, J.: "Local representation and strategic voting: evidence from electoral boundary reforms"
2013/33, Agasisti, T.; Murtinu, S.: "Are we wasting public money? No! The effects of grants on Italian university students' performances"
2013/34, Flacher, D.; Harari-Kermadec, H.; Moulin, L.: "Financing higher education: a contributory scheme"
2013/35, Carozzi, F.; Repetto, L.: "Sending the pork home: birth town bias in transfers to Italian municipalities"
2013/36, Coad, A.; Frankish, J.S.; Roberts, R.G.; Storey, D.J.: "New venture survival and growth: Does the fog lift?"

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2015/35, Calero, J.; Choi, A.: "The distribution of skills among the European adult population and unemployment: a comparative approach"
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2015/40, Mancebón, M.J.; Ximénez-de-Embún, D.P.; Mediavilla, M.; Gómez-Sancho, J.M.: "Does educational management model matter? New evidence for Spain by a quasiexperimental approach"
2015/41, Daniele, G.; Geys, B.: "Exposing politicians' ties to criminal organizations: the effects of local government dissolutions on electoral outcomes in Southern Italian municipalities"
2015/42, Ooghe, E.: "Wage policies, employment, and redistributive efficiency"

2016

2016/1, Galletta, S.: "Law enforcement, municipal budgets and spillover effects: evidence from a quasi-experiment in Italy"
2016/2, Flatley, L.; Giulietti, M.; Grossi, L.; Trujillo-Baute, E.; Waterson, M.: "Analysing the potential economic value of energy storage"
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2016/7, Choi, A.; Gil, M.; Mediavilla, M.; Valbuena, J.: "Double toil and trouble: grade retention and academic performance"
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2016/10, Bianchini, S.; Pellegrino, G.; Tamagni, F.: "Innovation strategies and firm growth"
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2016/13, Costa-Campi, M.T.; García-Quevedo, J.; Martínez-Ros, E.: "What are the determinants of investment in environmental R&D?"
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2016/15, Matas, A.; Raymond, J.L.; Dominguez, A.: "Changes in fuel economy: An analysis of the Spanish car market"
2016/16, Leme, A.; Escardíbul, J.O.: "The effect of a specialized versus a general upper secondary school curriculum on students' performance and inequality. A difference-in-differences cross country comparison"
2016/17, **Scandurra, R.I.; Calero, J.**: "Modelling adult skills in OECD countries"
2016/18, **Fernández-Gutiérrez, M.; Calero, J.**: "Leisure and education: insights from a time-use analysis"
2016/19, Del Rio, P.; Mir-Artigues, P.; Trujillo-Baute, E.: "Analysing the impact of renewable energy regulation on retail electricity prices"
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2016/21, Ferraresi, M.; Galmarini, U.; Rizzo, L.; Zanardi, A.: "Switch towards tax centralization in Italy: A wake up for the local political budget cycle"
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2016/30, Di Cosmo, V.; Malaguzzi Valeri, L.: "Wind, storage, interconnection and the cost of electricity"

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2017/3, Bianchini, S.; Pellegrino, G.: "Innovation persistence and employment dynamics"
2017/4, Curto-Grau, M.; Solé-Ollé, A.; Sorribas-Navarro, P.: "Does electoral competition curb party favoritism?"
2017/5, Solé-Ollé, A.; Viladecans-Marsal, E.: "Housing booms and busts and local fiscal policy"
2017/6, Esteller, A.; Piolatto, A.; Rablen, M.D.: "Taxing high-income earners: Tax avoidance and mobility"
2017/7, Combes, P.P.; Duranton, G.; Gobillon, L.: "The production function for housing: Evidence from France"
2017/8, Nepal, R.; Cram, L.; Jamasb, T.; Sen, A.: "Small systems, big targets: power sector reforms and renewable energy development in small electricity systems"
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2017/10, Neisser, C.: "The elasticity of taxable income: A meta-regression analysis"
2017/11, Baker, E.; Bosetti, V.; Salo, A.: "Finding common ground when experts disagree: robust portfolio decision analysis"
2017/12, Murillo, I.P; Raymond, J.L; Calero, J.: "Efficiency in the transformation of schooling into competences: A cross-country analysis using PIAAC data"
2017/13, **Ferrer-Esteban, G.; Mediavilla, M.:** "The more educated, the more engaged? An analysis of social capital and education"
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2017/15, Schwab, T.; Todtenhaupt, M.: "Spillover from the haven: Cross-border externalities of patent box regimes within multinational firms"
2017/16, Chacón, M.; Jensen, J.: "The institutional determinants of Southern secession"
2017/17, Gancia, G.; Ponzetto, G.A.M.; Ventura, J.: "Globalization and political structure"
2017/18, González-Val, R.: "City size distribution and space"
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