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Does preschool boost the development of minority children?: the case of Roma children

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Summary. Does universal preschool constitute an effective policy tool to promote the development and integration of children from minority groups? We address this question for the children of the Roma—the largest and most disadvantaged minority group in Europe. To tackle the issue of non-random selection into preschool, we exploit variation in the individual distance to the nearest preschool facility. Non-parametric instrumental variable estimations reveal significant short-term gains in terms of children's literacy. Preschool attendance also increases the prevalence of vaccinations but has no effect on other observed health outcomes. Overall, preschool also does not seem to enhance integration measured by children's language proficiency or social–emotional development, at least not in the short term.

Keywords: Child development; Non-parametric instrumental variable estimation; Universal childcare

1. Introduction

Social, economic and political exclusion remains an everyday challenge that ethnic minorities face in modern societies. A key reason for the 'vicious cycle of exclusion and poverty' is the gap in educational achievements caused by disadvantaged family backgrounds and residential segregation (Kahanec, 2014). As a consequence, numerous policy experts suggest providing minorities with unlimited access to the education system of the host country, in particular to the early education system. This paper uses recent cross-country data and a non-parametric instrumental variable (IV) approach to investigate whether or not participation in universal preschool helps to boost the development and the integration of children belonging to one of the largest and most disadvantaged minorities—the Roma (O'Higgins, 2012).

The focus on early education is motivated by the findings of recent research demonstrating strong beneficial effects of preschool interventions targeted at children with special needs, such as those from disadvantaged backgrounds or minority groups. See for instance the literature on Head Start—Currie and Thomas (1995) or Currie *et al.* (2002)—or the Perry Preschool Program—Belfield *et al.* (2006) or Heckman *et al.* (2009). But whether a widely accessible or universal childcare programme is appropriate for improving the development and integration of children from minority groups is an open question. Evidence from the growing body of research discussing the effects of universal preschool on children of the majority population

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may be misleading as the needs and reactions of children from minority groups may differ from those of children from the majority population. (See Apps *et al.* (2013), Baker *et al.* (2008), Berlinski *et al.* (2009), Bernal and Keane (2009), Cascio (2009), Drange *et al.* (2014), Felfe *et al.* (2012), Fitzpatrick (2008), Gormley *et al*. (2008), Havnes and Mogstad (2011a) and Magnuson *et al.* (2007).) The evidence on the effect of large-scale programmes on children from minority groups, however, is quite scarce and limited to a few regional cases. In particular, there are a couple of studies that focus on the effects of Oklahoma's universal pre-kindergarten programme (targeting 4-year-old children). Using a regression discontinuity design based on the fact that only children who were born before a specific cut-off date are entitled to the programme, these studies have found positive effects on the cognitive development of children from different ethnic groups (Gormley *et al.*, 2005; Gormley and Gayer, 2008). Drange and Telle (2015) analysed the introduction of free preschool targeting 5-year-old children in Oslo (Norway) on later school performance of immigrant children. Employing a difference-in-difference strategy, they found beneficial effects for girls, but not for boys. Dustmann *et al.* (2013) analysed the consequences of an expansion in preschool facilities in one administrative district (Weser-Ems Kreis) in Germany. Relying on regional variation in the expansion of preschool facilities and employing a linear IV strategy they found that public childcare attendance reduces language and motor skill problems and improves overall school readiness for immigrant children.

Our paper focuses on Roma children across several European countries. The estimated number of the Roma population in Europe lies somewhere between 10 million and 12 million people (Brüggemann, 2012). Integrating this minority group represents a serious challenge: about 90% of Roma households live below national poverty lines, less than a third of people have paid employment, only 15% of young Roma adults complete upper secondary schooling and, even more worrisome, up to 37% of Roma lack primary education and thus the basic skills in reading and mathematics (Brüggemann, 2012).

Research and the design of appropriate integration policies are hampered by a severe lack of reliable data. To overcome this barrier the United Nations Development Programme (UNDP), the World Bank (WB) and the European Commission (EC) have joined forces to conduct the first cross-country survey on Roma—the so-called UNDP–WB–EC regional Roma survey. The survey contains basic socio-economic data on households as well as individual household members in 12 countries of central and south-east Europe. A specific module on early childhood provides information on children's health, cognitive and non-cognitive development and participation in preschool. Hence, for the first time one can assess the effects of preschool attendance on the development and integration of Roma children.

Identification of the causal effects of preschool attendance on child outcomes is challenging. The decision to send a child to preschool is likely to be confounded with parental efforts to enhance their child's development. In addition, preschools may select children on the basis of information that is not readily observed by the econometrician. We tackle these issues as follows: the UNDP–WB–EC regional Roma survey contains detailed information on the neighbourhood in which the household is located including the distance to the nearest preschool. The neighbourhood is obviously an important determinant of children's development and thus variation in preschool availability is unlikely to be exogenous. Variation conditional on neighbourhood and household characteristics, however, is a more promising source of exogenous variation. Indeed, basically all correlations between preschool availability and family background turn insignificant and economically meaningless once we control for the full set of control variables. Our identification strategy therefore relies on the individual distance to the nearest preschool as an instrument for preschool attendance while controlling for features of the child, the household and the neighbourhood as well as regional fixed effects. We implement this strategy by using a

fully non-parametric IV estimator: the inverse probability weighting (IPW) approach that has been discussed in Frölich (2007).

Our results reveal considerable returns to preschool attendance in terms of children's literacy: when attending preschool, Roma children are 31.0 percentage points (PPs) more likely to know 10 letters of the alphabet, 20.6 PPs more likely to be able to write their own name and 42.4 PPs more likely to recognize the symbols for the numbers 1–10. They are also 20.7 PPs more likely to be vaccinated. There are, however, no short-term gains in any other observed health outcomes. Evidence for improvements in integration measures, such as language proficiency and peer relations, is not robust.

This paper advances our understanding about the effect of universal preschool on minority children in several dimensions. First, it focuses on a so-far understudied, but sizable, minority group—the Roma. Second, it investigates the effects of preschool not only on children's cognitive development, but also on health and important integration measures. Such a broad focus is crucial if we want to understand whether universal preschool is indeed an effective integration policy. Third, our paper does not focus on a limited geographical region but rather on all of south-east Europe, which is the main region of residence of the Roma. Although a cross-country analysis must deal with much more heterogeneity in population and institutions, it allows for a more widely valid statement about the effectiveness of the policy under study. Finally, this research is the first study on the effectiveness of universal preschool that applies a non-parametric estimation method. In contrast with parametric methods, a non-parametric method can cope with non-linearities and effect heterogeneity. These issues are likely to be severe in the analysis of the effectiveness of universal preschool, not least because the treatment—preschool—and the counterfactual treatment—the alternative care mode—are likely to vary strongly in quality.

The remainder of the paper is structured as follows. The next section provides an overview of the preschool system in the countries under study. Section 3 introduces the data. Section 4 discusses the empirical framework with a focus on the underlying identification strategy. Section 5 presents our results and several robustness checks. Section 6 finally concludes and discusses the policy implications.

2. Background

This section provides an overview of the early childcare systems in the countries under study as well as a discussion about the existing integration efforts involving the Roma population. All details about the early childcare system are taken from European Commission *et al.* (2009, 2014). In addition we draw on the knowledge from local experts, in particular experts working at the statistical offices of the countries under study, the Roma Education Fund and the Institute of Romani Culture in Albania. The aim is to clarify what our treatment—preschool attendance—means and to what extent it is comparable across the countries under study.

Early childcare covers children who are between 0 and 6 or 7 years old: the age when primary school becomes mandatory. It is usually provided in care centres that are run by either a public or a private entity. Such centres mostly operate separately for younger and older children—the age break is usually at 3 years; the care centres for younger children are called nurseries; those for older children kindergarten or preschool. In what follows, we refer only to preschool. For data reasons we do not consider children younger than 3 years in our analysis. Table 1 provides an overview of the preschool system in the countries under study. (Table 1 and the discussion in this section provide national averages. Despite regulation at the national level, there is obviously much heterogeneity within countries. Most heterogeneity, however, arises on implementation

Table 1. Institutional background†

Country	*Legal entitlement*	*Compulsory attendance*	*Attendance, average (%)*	*Attendance, Roma‡ (%)*	*Hours per week*	*Group size*	*Child–staff ratio*
Albania	—	—	64	34	30	22	1:18
Bosnia and Herzegovina	Age 3 years	—	50	12	30	20–23	1:14
Bulgaria	—	Age 5 years	87	49	40	22	—
Croatia	—	Age 5 years	71	23	33	14–23	—
Czech Republic	Age 5 years	—	88	38	30	24	—
Hungary	Age 3 years	Age 5 years	95	78	35	25	—
Macedonia	—	—	21	26	40	15–20	1:11
Moldova	Age 2 years	Age 5 years	80	23	30	22.5	1:11
Montenegro	—	Age 3 years	40	12	40	25	1:15
Romania	Age 5 years	—	82	41	25	20	1:17
Serbia	—	Age 5 years	48	20	30	21.5	1:14
Slovakia	—	—	77	31	34	20–22	1:21

†Source: European Commission *et al*. (2009, 2014) and Brüggemann (2012). In addition we draw on the knowledge from local experts, in particular experts working at the statistical offices of the countries under study, the Roma Education Fund and the Institute of Romani Culture in Albania.
‡Numbers on the attendance rate of Roma children are based on the data used for this study (see Section 3 for details).

at the preschool level. As a result, heterogeneity is strongest between preschools in rural and urban areas.)

Nowadays preschool is not only a place where children receive care, but it is also an institution where children are taught the skills that are needed for primary school and life in society. For this reason, central governments issue official educational guidelines to help preschools to meet this goal. Examples are the 'National strategy for preschool education' in Bosnia and Herzegovina which was established in 2005 and postulates that preschools have the task to ensure optimal and equal conditions so that every child, from their birth to the start of schooling, develops and achieves all the intellectual and physical capabilities as well as competencies that are necessary for primary school and life in society (United Nations Educational, Scientific and Cultural Organization, 2006). Another example is the Preschool Education Act in Croatia which was established in 1997 and requires preschools to create conditions for the comprehensive and harmonious development of children's personalities (United Nations Educational, Scientific and Cultural Organization, 2010). Similarly, Slovakia has a state educational programme for preprimary education which has the aim of attaining the optimal perceptual, motor, cognitive and socioemotional development as the basis for readiness for school education and life in society (United Nations Educational, Scientific and Cultural Organization, 2010). Summing up, official objectives generally are to support children to develop personal, emotional and social skills as well as language and communication skills. Moreover, preschools engage in physical and health education. In the year before primary school, preschools focus on fostering children's literacy and numerical and logical reasoning, as well as adapting children to school life.

On average, preschools provide children with care for 30 or more hours a week allowing parents to work at least part time. Most countries provide regulations regarding the group sizes and child–staff ratio: groups should comprise between 20 and 25 children; the exceptions are Croatia and Macedonia where groups contain between 14 and 23 children and 15 and 20 children respectively; the child–staff ratio should range between 11 and 18 children per staff member. In practice, however, few childcare centres comply with these guidelines.

To comply with the goals of the European Union's presidency in 2002—'to provide child care by 2010 to at least 90% of children between 3 years old and the mandatory school age and at least 33% of children under 3 years of age'—many European countries have committed themselves to providing a preschool slot to all children. In particular, they either established a legal entitlement to a place in preschool or made attendance compulsory for some years before primary school entrance. By 2010–2011, in Bosnia and Herzegovina, Hungary and Moldova children were legally entitled to a slot in preschool from age 3 years onwards (in the case of Moldova from age 2 years onwards). In the Czech Republic and Romania, there is a legal claim from age 5 years onwards. In Bulgaria, Croatia, Hungary, Moldova and Serbia attendance is compulsory in the last year before primary school entrance; in Montenegro attendance is compulsory from age 3 years onwards. Unfortunately, we lack information on expansion efforts in the countries under study, yet what we observe is that despite the legal commitments attendance rates are still low. In fact, Hungary is the only country reaching the target that was set by the European Union presidency: in 2010–2011 95% of all 3–5-year-old children were attending preschool. Bulgaria, the Czech Republic, Moldova and Romania are getting close: in all four countries more than 80% of all 3–5-year-old children are attending preschool. In Bosnia and Herzegovina, Montenegro and Serbia attendance rates are still quite low—below 50%—despite political efforts to stimulate attendance. In Macedonia only one in five children are attending preschool.

The integration of Roma children in the education system is a highly debated topic not only at the national level but also at the international level. In fact, there are several European Union initiatives which focus on the education of Roma children in different countries. For example, there is the 'Roma good start initiative', which is a joint European Union–United Nations Children's Emergency Fund project whose objective is to promote early childhood development as an entry point for fostering the sustained social inclusion of Roma children in the Czech Republic, Hungary, Macedonia, Romania, Serbia and Spain. Another major initiative is the Roma Education Fund, which is an organization operating in central and south-east Europe. The particular goal of the Fund is to expand Roma children's access to preschool education through such efforts as information campaigns, assisting with the registration process and conversations with parents. To reach this goal the Roma Education Fund supports national initiatives. One example for such a national initiative is the project 'Inclusion of Roma children in public preschools' in Macedonia. This project not only aims to support the integration of Roma children by increasing the number of Roma children in preschools; it also seeks to improve the attitude of the Roma parents towards the local education system, to improve the use of the Macedonian language and to increase the competence of educators to help to overcome prejudices and stereotypes about the Roma in public preschools.

Despite these efforts integration of Roma children in the preschool system is far from being accomplished. With the exception of Hungary and Bulgaria, where 77% and 49% of all 3–5-year-old Roma children are attending preschool respectively, attendance rates of Roma children lie well below 50%. In Bosnia and Herzegovina and Montenegro only one out of eight Roma children are attending preschool. Thus, although preschool is broadly understood as a policy tool to integrate Roma children into the education system and society, to reach full integration of Roma children into preschools, major efforts are still needed. Moreover, whether participation in preschool is indeed an effective policy tool to foster the development and the integration of Roma children is still an open question.

3. Data

The data underlying this study are the 2011 UNDP, the WB and the EC cross-country survey

Table 2. Descriptive statistics on key variables†

Variable	*Non-Roma*	*Roma*	*Difference*	*Preschool*	*No preschool*	*Difference*
(a) Child outcomes						
Name 10 letters (dummy)	0.568	0.298	−0.269‡	0.506	0.206	0.300‡
Write name (dummy)	0.431	0.191	−0.239‡	0.378	0.108	0.270‡
Read 4 words (dummy)	0.371	0.175	−0.196‡	0.335	0.103	0.232‡
Recognize numbers 1–10 (dummy)	0.703	0.417	−0.286‡	0.655	0.310	0.345‡
Understand local language (dummy)	0.842	0.714	−0.127‡	0.837	0.660	0.177‡
Self-confidence (dummy)	0.921	0.799	−0.122‡	0.894	0.757	0.138‡
Peer relation (dummy)	0.951	0.884	−0.067‡	0.950	0.855	0.095‡
Health (from 1 to 5, from bad to good)	4.562	4.405	−0.156‡	4.391	4.412	−0.021
Long-standing illness (dummy)	0.028	0.053	0.025‡	0.043	0.058	−0.016
Limited by health (dummy)	2.938	2.919	−0.019	2.929	2.914	0.014
Vaccination card (dummy)	0.962	0.893	−0.068‡	0.93	0.876	0.055§
Vaccine: tuberculosis (dummy)	0.981	0.97	−0.011	0.97	0.971	−0.001
Vaccine: polio (dummy)	0.974	0.893	−0.082‡	0.918	0.881	0.037
Vaccine: diphtheria (dummy)	0.983	0.897	−0.086‡	0.936	0.878	0.057§
Vaccine: measles (dummy)	0.981	0.887	−0.094‡	0.927	0.867	0.060§§
(b) Treatment						
Preschool attendance (dummy)	0.567	0.309	−0.268‡	—	—	—
(c) Family characteristics						
Age of child (years)	4.448	4.452	0.004	4.966	4.222	0.744‡
Child is a boy (dummy)	0.499	0.508	0.009	0.504	0.510	−0.006
Household head: no education (dummy)	0.016	0.271	0.255‡	0.169	0.316	−0.147‡
Household head: primary education (dummy)	0.065	0.275	0.210‡	0.253	0.285	−0.031
Household head: secondary education (dummy)	0.852	0.450	−0.402‡	0.576	0.394	0.182‡
Household head: works (dummy)	0.649	0.376	−0.272‡	0.417	0.358	0.058§
Household head: working hours per week	26.202	12.407	13.795‡	13.466	11.927	1.539§§
Mother: works (dummy)	0.131	0.075	−0.055‡	0.102	0.017	0.085‡
Mother: working hours per week	4.238	2.013	2.226‡	2.691	0.514	2.177‡
Household members	4.852	6.213	1.361‡	5.992	6.312	−0.319‡
Home: appartment (dummy)	0.151	0.071	−0.080‡	0.081	0.067	0.014
Home: new house (dummy)	0.290	0.127	−0.163‡	0.145	0.119	0.025
Home: older house	0.453	0.429	−0.025	0.478	0.407	0.071§
Home: slum or Roma camp (dummy)	0.090	0.351	0.262‡	0.278	0.384	−0.106‡
Electricity available (dummy)	0.974	0.876	−0.099‡	0.903	0.863	0.040§
Connection to sewerage (dummy)	0.819	0.522	−0.297‡	0.531	0.518	0.013
Toilet available (dummy)	0.821	0.486	−0.335‡	0.532	0.465	0.067§
Kitchen available (dummy)	0.961	0.71	−0.251‡	0.795	0.672	0.123‡
Bed for each available (dummy)	0.895	0.528	−0.367‡	0.657	0.470	0.188‡
Radio available (dummy)	0.793	0.529	−0.264‡	0.498	0.543	−0.045
Television available (dummy)	0.984	0.869	−0.116‡	0.909	0.851	0.058‡
Computer available (dummy)	0.561	0.165	−0.396‡	0.219	0.141	0.079‡

(*continued*)

of the Roma—the so-called UNDP–WB–EC 2011 regional Roma survey. It is the result of an important effort to produce a socio-economic profile of large Roma populations throughout Europe and is based on samples of Roma living in Roma settlements or areas of compact Roma populations (areas where the share of Roma people is at least as large as the share of Roma in the national census). The samples represent about 85% of the Roma population in the following

Table 2 (*continued*)

Variable	*Non-Roma*	*Roma*	*Difference*	*Preschool*	*No preschool*	*Difference*
(d) Location characteristics						
Location: capital (dummy)	0.202	0.159	−0.043§§	0.130	0.171	−0.041
Location: city (dummy)	0.188	0.157	−0.031	0.138	0.165	−0.028
Location: town or village (dummy)	0.598	0.651	0.054	0.706	0.627	0.079
Location: unregulated area (dummy)	0.012	0.033	0.021§	0.026	0.036	−0.01§§
Location: majority Roma (dummy)	0.148	0.648	0.500‡	0.601	0.668	−0.067
Next city less than 1 km (dummy)	0.097	0.064	−0.033‡	0.054	0.068	−0.014
Municipality office less 1 km (dummy)	0.267	0.242	−0.025	0.292	0.22	0.073§
Bus stop less than 1 km (dummy)	0.694	0.604	−0.090§	0.689	0.566	0.123‡
Preschool less than 1 km (dummy)	0.534	0.475	−0.060§	0.587	424	0.163‡
Observations	569	3334		1030	2304	

†'Non-Roma' is the average for all non-Roma children, 'Roma' corresponds to the Roma sample average, 'Preschool' refers to all Roma children with preschool exposure and 'No preschool' refers to all Roma children with no preschool exposure. 'Difference' is the difference between 'Roma' and 'Non-Roma' or the difference between 'Preschool' and 'No preschool'. Results of a hypothesis test of no difference between the respective means when clustering at the region level are indicated.
‡$p < 0.1$.
§$p < 0.05$.
§§$p < 0.1$.

12 central and south-east European countries: Albania, Bosnia and Herzegovina, Bulgaria, Croatia, the Czech Republic, Hungary, Macedonia, Montenegro, the Republic of Moldova, Romania, Serbia and Slovakia.

The survey was conducted in a three-stage random representative sampling process: first, in each country 110 random clusters of approximately 30 households from areas of compact Roma populations were selected; second, in each cluster, seven households were randomly chosen and the respective head of the household answered questions about the household; third, one random household member older than 15 years was selected to answer a battery of questions on status and attitudes. In addition, the primary care giver of all children below school age answered questions of a special module on early childhood education and care. In each country, the survey interviewed about 750 Roma households as well as 350 non-Roma households close to the Roma households.

We restrict our sample to the Roma population only: to children at preschool age (3–6 years old) and to households which have not moved during the previous 5 years (which is the case for more than 95%). This restriction guarantees that the children under study have at least some time and chance to attend preschool in the location of residence. Furthermore, we drop all observations lacking information on the outcome variables provided by the early childhood education and care module (a series of questions describing children's literacy, language proficiency, self-confidence and peer relations). Given these restrictions, our baseline sample contains 3334 Roma children at preschool age (4.5 years old on average). Information on children's health is contained in the main survey and thus is provided by the household head who is likely to be less informed about children's development than the primary care giver, who answers the questions in the early childhood education and care module. As a result, our sample shrinks when analysing health-related outcomes (2478 observations).

For descriptive comparisons we also draw on 569 non-Roma children who live close to the Roma households under study. These non-Roma children are exposed to similar regional

conditions. As such, any differences between the Roma children and the non-Roma children who were included in this sample should not result from the fact that Roma often live in regions that are most affected by poverty and unemployment. In fact, comparisons of key economic indicators, such as the employment rate, the unemployment rate or the high school dropout rate, reveal that both the Roma and the non-Roma populations that are included in the sample fare much worse than the average population in the countries under study (Brüggemann, 2012). Table 2 provides descriptive statistics of the key variables for non-Roma and Roma children, as well as for Roma children with and without preschool exposure.

There are significant (at least at the 5% level of significance) differences between Roma and non-Roma children in terms of literacy, proficiency in the local language, social–emotional development and health (see Table 2, panel (a)). It is important to point out that these differences exist despite controlling for any difference that may arise because Roma children are likely to live in a disadvantaged area. Roma children are less likely to name 10 letters from the alphabet (29.8% *versus* 56.8%), to be able to write their own name (19.1% *versus* 43.1%), to be able to read four popular words (17.5% *versus* 37.1%) or to recognize numbers between 1 and 10 (41.7% *versus* 70.3%). Roma children also lag behind in terms of their knowledge of the regional language (71.4% *versus* 84.2%). Furthermore, Roma children have less self-confidence (79.9% *versus* 92.1%) and are less integrated among their peers (88.4% *versus* 95.1%). Finally, Roma children are in slightly worse health, suffer more often from long-standing illnesses (5.3% *versus* 2.8%) and are less likely to be vaccinated (89.3% *versus* 96.2%).

Yet, there are also significant differences in terms of family background between Roma children and non-Roma children living in the same area (see Table 2, panel (c)). For instance, the household head of the Roma households, who is most likely to be the father, has less education than the household head of a non-Roma household: 45.0% of all Roma fathers have secondary education whereas 85.2% of all non-Roma fathers do so. In a similar vein, there are differences in terms of labour force participation: whereas respectively 64.9% and 13.1% of all fathers and mothers of non-Roma households are working, only respectively 37.6% and 7.5% of all fathers and mothers of Roma households are working.

Roma households are also larger than non-Roma households: Roma households consist on average of 6.2 members, whereas non-Roma households consist only of 4.9 members. Roma children live on average in worse conditions than non-Roma children: a considerable share of non-Roma children live in either apartments (15.1%) or newer houses (29.0%), whereas many Roma children live in ghettos or slums (35.1%). Moreover, Roma households are on average worse equipped than non-Roma households: Roma households have less access to electricity than non-Roma households (87.6% *versus* 97.4%), to sewerage (52.2% *versus* 81.9%), to a toilet (48.6% *versus* 82.1%), to a kitchen (71.0% *versus* 96.1%), to a bed for each household member (52.8% *versus* 89.5%), to a radio (52.9% *versus* 79.3%), to a television (86.9% *versus* 98.4%) or to a computer (16.5% *versus* 56.1%). Finally, the descriptive statistics reveal that even within the areas that were selected for the survey (areas of high Roma concentration) there is residential segregation (see panel (d) of Table 2): Roma children are more likely to live in a location that is characterized by a Roma majority (64.8% *versus* 14.8%), slightly further from the next city centre (6.4% of the Roma households live less than 1 km from the next city centre *versus* 9.7% of the non-Roma households), slightly less connected to public transportation (60.4% of the Roma live less than 1 km from the next bus stop *versus* 69.4% of the non-Roma) and finally slightly further from a preschool (47.5% of the Roma live less than 1 km from the next preschool *versus* 53.4% of the non-Roma). The difference in availability of a preschool that is close translates also into preschool attendance: among the Roma children, only 30.9% are attending preschool, whereas among the non-Roma children 56.7% are attending preschools (see panel (b), Table 2).

Does preschool attendance correlate with better development outcomes? The development gap between Roma children with and without exposure to preschool is substantial: Roma children attending preschool outperform the Roma children not attending preschool in terms of their knowledge of the local language (83.7% *versus* 66.0%), further literacy skills (the gap amounts to 30.0 PPs when naming 10 letters, to 27.0 PPs when writing down a name, to 23.2 PPs when being able to read four words and to 34.5 PPs when being able to identify the symbols for the numbers 1–10). Roma children attending preschool also have more self-confidence (89.4% *versus* 75.7%) and get along better with their peers (95.0% *versus* 85.5%). Moreover, Roma children who attend preschool are also more likely to be vaccinated than those who do not (87.0% *versus* 81.5%). However, it would be premature to conclude that preschool helps to close the gap between Roma and non-Roma children. Besides the differences in children's development described, we also observe that attendance to preschool is correlated with families' socio-economic background. In fact, Roma children who attend preschool resemble non-Roma children much more than Roma children who do not attend preschool. For instance, the household head is more likely to have secondary education (57.6% *versus* 39.4%); they live in smaller families (6.0 *versus* 6.3 household members); their households are better equipped in terms of electricity, toilets, kitchen, beds, etc. (4.0 PPs, 6.7 PPs, 12.3 PPs and 18.8 PPs respectively); they are more likely to live in an area which is well connected to public transportation (68.9% *versus* 56.6%) or to preschools (58.7% *versus* 42.4%). In other words, the socio-economic family background and the neighbourhood are on average more favourable for Roma children who attend preschool. Hence, a simple comparison between Roma children with and without exposure to preschool is likely to overestimate the effect of preschool attendance on children's development. The next section therefore discusses how we tackle the issue of endogenous selection into preschool.

4. Empirical framework

This section discusses the causal effect of preschool attendance on children's development that we aim for, the assumptions underlying our identification strategy and the estimation method.

4.1. Main effect and potential channels

The causal effect of preschool attendance—the treatment under study—on the development of a child is the (hypothetical) difference between the child's development when attending and not attending preschool. The overview that was provided in Section 2 made clear that there is substantial heterogeneity in preschools across, but also within, the countries that are under study. In addition there is likely to be substantial heterogeneity in the counterfactual treatment: the type of care that is provided to children when not attending preschool. Although it is impossible to provide an estimate for all possible combinations of treatment and counterfactual treatments, our study draws on a non-parametric estimation method which allows for effect heterogeneity (see Section 4.3 for details).

Different mechanisms are at play which shape the final direction and magnitude of the effect of attending preschool on child outcomes. First, children attending preschool are exposed to a preschool teacher. There is a broad literature on the effect of the quality of teachers on children's development which postulates a positive influence of teachers on children's development, at least when fulfilling certain quality standards (see for instance Hanushek (2011)). Roma children may be particularly likely to benefit from exposure to a teacher as that may help to boost their host-country-specific human capital such as the knowledge of the local language and culture.

Second, children attending preschool spend quite some time with their peers. Peers have a non-negligible influence on children's development, in particular on children's cognitive development (Neidell and Waldfogel, 2010). Such peer effects may be particularly relevant in the case of Roma children as they are likely to benefit from contacts with non-Roma children, especially in terms of language skills.

Third, there may be also an indirect effect through the crowding out of the alternative care mode (see Datta-Gupta and Simonsen (2010), Felfe *et al.* (2012), or Felfe and Lalive (2014) for a discussion). The direction and the magnitude of the crowding-out effect depends on the quality of the alternative care. In our context the counterfactual care mode is mostly the mother or the extended family. Although it is impossible to judge the relative quality of the care that is provided by the preschool in comparison with the care provided by the mother, in the case of Roma children it is likely that the preschool enhances children's host-country-specific skills more than the mother (through contact with the staff and peers). In terms of social–emotional development, developmental psychology, in particular so-called attachment theory (Bowlby, 1969), stresses the importance of a clear psychological parent for the development of social–emotional skills. As such the reduction of the time that is spent with the mother may have a negative effect on children's social–emotional skills.

Fourth, preschool may free mothers from their childcare duties and thus allow them to engage in paid work (see Felfe (2015) for an overview of the effect of preschools on maternal employment). As such preschool may contribute to household income which in turn is likely to have a positive effect on children's development (see Dahl and Lochner (2012) or Gonzalez (2013) for the effect of income on children's development).

Finally, parents may adjust their parenting style when being in contact with preschool teachers and other parents (Felfe and Lalive, 2012; Hsin and Felfe, 2014). This effect may go either way: on the one hand, contact with pedagogical staff or other parents may inspire parents in their parenting methods; on the other hand parents may outsource childcare and thus invest less in their children.

In this paper, we focus on estimating the overall effect of preschool. We analyse a variety of outcomes, which may hint towards potential underlying causal channels. Disentangling the mechanisms, however, lies beyond the scope of this paper.

4.2. Identification

Identification of the causal effects of preschool attendance on child outcomes is challenging. Sending a child to preschool is likely to be confounded with parents' efforts to promote their child's development. In the case of Roma families, the decision to register a child in preschool may furthermore reflect parents' integration efforts as well as their knowledge and opinion about the country's institutions. In addition, preschools may select children depending on their development and their family or ethnic background (even if, officially, they are prohibited from doing so by law). The non-random selection into preschools is in fact reflected by the unconditional mean differences in important family background characteristics, such as the household head's education or the households' equipment and infrastructure (see Table 2).

One way to tackle non-random selection is to estimate the differences in child development outcomes conditionally on a set of determinants of children's development and children's preschool attendance. The literature on the production of children's human capital has highlighted the importance of children's family background, in particular the socio-economic standing of the family, and the social environment (Cunha *et al.*, 2006). Thus, controlling for variables such as parental education, household wealth, housing conditions and neighbourhood

features is crucial in a selection-on-observables approach. Nevertheless, the criteria underlying both decision processes—the decision of parents and the decision of preschools—is unlikely to be all measurable or observable. Hence, a comparison of children enrolled and children not enrolled in preschool even conditionally on a comprehensive set of control variables may result in biased estimates of the effect of preschool attendance on child development.

We therefore compare the estimates based on a selection-on-observables approach with the estimates resulting from an IV approach. We use the distance from a child's home to the closest preschool facility as an IV. To be more precise, we rely on the information that is provided by the interviewer on whether the nearest preschool facility is less than 1 km away and thus walking distance from the household. Seminal studies introducing distance-based instruments are the studies assessing the returns to college, such as Card (1995) and Kane and Rouse (1995). Distance is a widely used instrument to analyse, for instance, the effect of informal childcare on female labour supply, e.g. Compton and Pollak (2014) and Dimova and Wolff (2011), or the influence of on-line learning *versus* face-to-face courses on student performance, e.g. Bettinger *et al.* (2015) and Xu and Jaggars (2013).

An IV must satisfy particular relevance and validity conditions, at least conditionally on a range of observed characteristics. A formal definition of the non-parametric IV assumptions that our estimates rely on is given in Abadie (2003), whereas we subsequently focus on their intuition in the context of our application.

IV relevance requires the first stage of the instrument to be non-zero, i.e. there must be some 'compliers' among the Roma children in the sense that these children would attend preschool if living close by but would not do so if living further away. In other words, the distance to preschool must shift the decision of at least some parents to register their children in preschool. Availability of a preschool may shape parental awareness of the possibility to enrol their child into preschool and thus increase the likelihood of doing so. In addition, living closer to a facility reduces the costs of attending preschool, at least in terms of transportation costs. In Section 5.1 we provide empirical evidence based on our sample that living closer to a preschool facility indeed (statistically significantly) raises the likelihood of enrolling a child in preschool. At the same time, there should be no 'defiers'; that is, there should be no children who do not attend preschool if living close by but who would do so if living further away. This assumption appears plausible in our application as it is difficult to think of a reason that could induce parents who would send their child to a distant preschool facility to take him or her out of preschool altogether once a facility opened up in their vicinity.

IV validity rules out a direct effect of the mere distance to preschool on child outcomes (other than through preschool attendance). It also implies that there is no correlation between the distance to preschool and any unobservable determinants of preschool attendance and/or child outcomes, at least not when including the set of control variables. Although the absence of an association between the mere distance to preschool and child outcomes seems plausible, it is likely that there is an association between the distance to preschool and some determinants of preschool attendance and/or child outcomes. We therefore devote the remainder of this section to a discussion of whether the distance to preschool may serve as a valid instrument once conditioning on an appropriate set of control variables. The choice of control variables is again motivated by the literature on the child development production function (Cunha *et al.*, 2006). As such we control for available proxies for the household's socio-economic status, such as the level of education of the household head, proxies for the household's wealth (the type of the home, available infrastructure or appliances), as well as available proxies for the neighbourhood, such as the share of Roma people, the distance to the next city, the municipality office or public transport.

Table 3. Countries and regions†

Country	*Region*	*Observations*	*% of sample*
Albania	Central	131	3.93
Albania	North	16	0.48
Albania	South	97	2.91
Bosnia and Herzegovina	Rest of the Federation of Bosnia and Herzegovina	179	5.37
Bosnia and Herzegovina	Republika Srpska	94	2.82
Bosnia and Herzegovina	Sarajevo	56	1.68
Bulgaria	North-east	48	1.44
Bulgaria	North-west	47	1.41
Bulgaria	South-east	56	1.68
Bulgaria	South-west	36	1.08
Croatia	Central	249	7.47
Croatia	Istra i primorje	35	1.05
Croatia	Slavonija	72	2.16
Croatia	Zagreb	53	1.59
Czech Republic	Bohemia	158	4.74
Czech Republic	Moravia	91	2.73
Czech Republic	Prague	40	1.2
Hungary	Kelet–north	210	6.3
Hungary	Kozep–central	14	0.42
Hungary	Nyugat–west	32	0.96
Macedonia	East	27	0.81
Macedonia	North-west	58	1.74
Macedonia	Skopje	113	3.39
Macedonia	South-west	41	1.23
Moldova	Central	57	1.71
Moldova	North	127	3.81
Moldova	South	20	0.6
Montenegro	Central	246	7.38
Montenegro	South	36	1.08
Romania	Bucharest and Ilfov	13	0.39
Romania	Moldova	46	1.38
Romania	Muntenia	101	3.03
Romania	Transilvania	130	3.9
Serbia	Belgrad	56	1.68
Serbia	Central	183	5.49
Serbia	Vojvodina	89	2.67
Slovakia	Central	70	2.1
Slovakia	East	190	5.7
Slovakia	West	17	0.51

†UNDP–WB–EC regional Roma survey, own calculations.

In addition, we control for regional fixed effects. As described in Section 3, our sample is spread over 12 countries, between three and four regions per country (40 regions altogether) and up to 113 local clusters per region. As described in Section 2, the preschool system is regulated at a centralized level. Most heterogeneity in how the regulations are actually implemented appears between urban, peripheral and rural areas. Controlling for regional fixed effects as well as the size or character of the location (i.e. capital, city, town, village or unregulated area) should thus capture most heterogeneity between existing preschools. The exact regions are displayed in Table 3. On average, there are 156 children per region in our sample. An alternative unit on which we could define the fixed effects is local clusters. Note, however, that the local clusters do not correspond to any administrative unit, but to a unit of 30 households out of which seven

are interviewed in our sample—the median number of children included in a local cluster in our sample is 5. As such, the local cluster is not only meaningless in terms of administrative power, but also too small in terms of geographical size and number of observations.

What are the threats to validity of the distance to preschool as an instrument to preschool attendance? First, parents may choose their location of residence on the basis of the availability of preschool facilities among other amenities offered by the neighbourhood. Second, local citizens may actively lobby for the supply of preschools together with other social policies. Third, the availability of a preschool might be the result of an active policy of local politicians to attract workers. We address in turn each of these threats and provide some empirical evidence to show how our set of controls renders possible correlations between the availability of preschools and the determinants of children's development not only insignificant, but also economically meaningless (Table 4).

Regarding the first concern, the question is whether Roma families indeed (consciously) settle down in a place that is close to a preschool (or to other amenities that correlate with education). According to Vlase and Preoteasa (2012), the main driver behind migration of Roma people is employment opportunities; the attractiveness of the education system as a whole and thus of the preschool system in particular appears to play a negligible role. In addition, the Roma families that were included in our sample are (contrary to what the *cliché* suggests) rather immobile: only 4.6% of Roma families had moved into their residence at the sampling date within the previous 5 years, whereas the vast majority had lived in the same area for at least 5 years. We exclude the few Roma families that moved during the previous 5 years to guarantee that the Roma children live for a sufficiently long time at the current location. (Our set of control variables should help to mitigate potential selection bias that is introduced by dropping the small subsample of movers.)

Despite the recent immobility, one may nevertheless argue that migration or residence decisions that occurred more than 5 years previously could have been endogenous to preschool availability. As argued in Card (1995) (but in the context of college education), family background characteristics may play an important role. We argue that conditioning on our set of control variables tackles this issue. Balancing tests, shown in Table 4, support this claim. The unconditional correlations between the distance to the nearest preschool and a series of socio-economic characteristics of the family are substantial (see the second column in Table 4, panel (a)): in particular, we observe a significant correlation between the distance to the nearest preschool and the education of the household head, household size, the type of the home, the available infrastructure and a series of indicators of household wealth. Conditioning on the set of regional dummies reduces the correlations substantially and renders several of the correlations—in particular the correlation with the education of the household head—insignificant. Conditioning on the full set of control variables leads to a complete loss of magnitude and significance of the correlations between the distance to the nearest preschool and the family's socio-economic background. This is also true for a measure of the integration efforts of the family: the use of the local language in the family (see Table 4, panel (c)). Importantly, the correlation between the distance to the nearest preschool and Roma parents' labour supply is also insignificant and economically meaningless (see Table 4, panel (c)). In fact, this is also true for the correlation between the distance to the nearest preschool and non-Roma parents' labour supply (see Table 4, panel (d)). This is important evidence that, even if preschools may belong to a policy package to attract qualified workers (which is an issue that we discuss more below), this policy does not seem to be really effective. There is also no effect on Roma parents' labour supply when using our IV method. Results are available on request.

Table 4. Balancing tests†

Variable	*Unconditional*	*Conditional on regional dummies*	*Conditional on full set of controls*
(a) Family characteristics—Roma			
Household head: no education (dummy)	−0.051‡ (0.019)	−0.014 (0.020)	0.124 (0.177)
Household head: primary education (dummy)	−0.032 (0.019)	−0.018 (0.019)	0.102 (0.177)
Household head: secondary education (dummy)	0.063‡ (0.017)	0.029 (0.018)	0.111 (0.176)
Household members (dummy)	−0.012‡ (0.004)	−0.004 (0.003)	−0.004 (0.003)
Home: apartment (dummy)	0.130‡ (0.034)	0.101‡ (0.035)	−0.004 (0.055)
Home: new house (dummy)	0.016 (0.026)	0.039 (0.025)	−0.017 (0.051)
Home: older house (dummy)	0.036§ (0.018)	0.036§ (0.017)	−0.021 (0.048)
Home: slum or ghetto (dummy)	−0.081‡ (0.018)	−0.077‡ (0.018)	−0.042 (0.049)
Electricity (dummy)	0.022 (0.025)	0.012 (0.024)	−0.021 (0.022)
Sewerage (dummy)	0.076‡ (0.018)	0.043§ (0.019)	0.007 (0.017)
Toilet (dummy)	0.051‡ (0.018)	−0.010 (0.017)	−0.009 (0.021)
Kitchen (dummy)	0.067‡ (0.019)	0.053‡ (0.019)	0.035 (0.028)
Bed for each (dummy)	0.093‡ (0.017)	0.065‡ (0.017)	0.028 (0.026)
Radio (dummy)	−0.102‡ (0.017)	0.008 (0.018)	−0.005 (0.016)
Television (dummy)	0.078‡ (0.025)	0.060§ (0.026)	0.017 (0.023)
Computer (dummy)	0.105‡ (0.023)	0.096‡ (0.022)	0.038 (0.025)
(b) Location characteristics—Roma			
Location: capital (dummy)	0.132‡ (0.038)	−0.056§ (0.024)	0.000 (0.016)
Location: city (dummy)	0.111‡ (0.024)	0.119‡ (0.026)	−0.037 (0.039)
Location: town or village (dummy)	−0.113‡ (0.021)	0.000 (0.018)	−0.004 (0.042)
Location: unregulated area (dummy)	−0.227‡ (0.048)	−0.084§§ (0.048)	−0.050 (0.048)
Distance to next city	0.301‡ (0.035)	0.029 (0.035)	0.027 (0.033)
Distance to municipality office (dummy)	0.484‡ (0.018)	0.459‡ (0.019)	0.304‡ (0.019)
Distance to bus stop (dummy)	0.515‡ (0.015)	0.497‡ (0.016)	0.408‡ (0.016)
Roma majority	−0.082‡ (0.018)	0.010 (0.020)	−0.002 (0.017)
(c) Integration and employment—Roma			
Local language (dummy)	0.043§ (0.019)	−0.003 (0.023)	−0.017 (0.019)
Household head: works (dummy)	0.021 (0.018)	0.018 (0.017)	0.002 (0.015)
Household head: work hours	0.000 (0.001)	0.000 (0.000)	−0.000 (0.000)
Mother: works (dummy)	−0.002 (0.033)	−0.003 (0.031)	−0.002 (0.026)
Mother: work hours (dummy)	−0.001 (0.001)	−0.001 (0.001)	−0.001 (0.001)
(d) Employment outcomes—non-Roma			
Household head: works (dummy)	−0.014 (0.040)	−0.024 (0.042)	−0.013 (0.039)
Household head: work hours	−0.000 (0.001)	−0.001 (0.001)	−0.001 (0.001)
Mother: works (dummy)	0.023 (0.062)	0.029 (0.063)	−0.005 (0.056)
Mother: work hours (dummy)	−0.001 (0.002)	−0.001 (0.002)	−0.001 (0.001)

†Column 'Unconditional' displays the unconditional correlation between the instrument—the closest preschool is less than 1 km away—and the variables shown in the first column. Column 'Conditional on regional dummies' displays the respective correlation resulting from a linear regression when controlling for regional dummies. Column 'Conditional on full set of controls' displays the respective correlation resulting from a linear regression when controlling for the full set of controls—shown in Table 2 except the respective variable. Standard errors are shown in parentheses. Source: UNDP–WB–EC regional Roma survey, own calculations.
‡$p < 0.01$.
§$p < 0.05$.
§§$p < 0.1$.

Regarding the second concern, although it is unlikely that Roma families engage in lobbying activities, it may be that lobbying activities for certain social policies, such as policies targeting the integration of minorities and policies aiming at expanding the preschool system, do correlate. We argue that controlling for the set of regional dummies helps to resolve this issue: even if attitudes or preferences for childcare were selective, they are unlikely to change rapidly over

Table 5. Descriptive statistics of the complier population†

Variable	*Marginal child, complier children*	*Average child, all Roma children*	*Difference*	*p-value*
(a) Child characteristics				
Child is a boy (dummy)	0.475	0.508	−0.032	0.008
Age of child (years)	4.477	4.452	0.025	0.426
(b) Family characteristics				
Household head: no education (dummy)	0.284	0.271	0.013	0.471
Household head: primary education (dummy)	0.285	0.275	0.010	0.415
Household head: secondary education (dummy)	0.430	0.450	−0.020	0.204
Household members	5.537	5.601	−0.065	0.044
Home: apartment (dummy)	0.056	0.071	−0.015	0.056
Home: new house (dummy)	0.120	0.127	−0.007	0.377
Home: older house (dummy)	0.395	0.429	−0.034	0.004
Home: slum, Roma camp (dummy)	0.400	0.351	0.049	0.807
Electricity available (dummy)	0.847	0.863	−0.016	0.031
Connection to sewerage (dummy)	0.497	0.522	−0.025	0.040
Toilet available (dummy)	0.444	0.486	−0.042	0.002
Kitchen available (dummy)	0.662	0.710	−0.048	0.000
Bed for each available (dummy)	0.464	0.528	−0.063	0.000
Radio available (dummy)	0.532	0.529	0.003	0.985
Television available (dummy)	0.843	0.869	−0.025	0.003
Computer available (dummy)	0.148	0.165	−0.017	0.116
(c) Location characteristics				
Location: capital (dummy)	0.139	0.159	−0.020	0.047
Location: city (dummy)	0.158	0.157	0.001	0.596
Location: town (dummy)	0.267	0.263	0.004	0.781
Location: village (dummy)	0.394	0.388	0.006	0.788
Next city less 1 km (dummy)	0.046	0.064	−0.018	0.004
Municipality office less than 1 km (dummy)	0.199	0.242	−0.043	0.001
Bus stop less than 1 km (dummy)	0.506	0.604	−0.098	0.000

†The column 'Average child' displays the descriptive statistics of all Roma children for the set for the full set of control variables, whereas the column 'Marginal child' does so for the complier population. The descriptive statistics for the complier population have been calculated by using the 'κ'-formula of Abadie (2003); see his theorem 3.1. Source: UNDP–WB–EC regional Roma survey, own calculations.

time. Hence, controlling for regional fixed effects should capture such unmeasured preferences and local actions. As we can see in Table 4, panel (b), there are substantial correlations between the neighbourhood conditions and the distance to the nearest preschool. As we can see in the third column of Table 4 within regions most of the correlations are economically meaningless and insignificant. The exception is the distance to the municipality office and the bus stop. Nevertheless, these correlations are basically a reflection of the location of preschools which are quite central and connected to public transport.

Regarding the third concern—whether preschools are a policy to attract workers and thus implicitly to induce the settlement of Roma—the question is what are the types of worker whom local politicians want to attract? The Roma are by far less educated than the national average—in fact, only 15% of Roma adults complete upper secondary schooling and 37% of Roma lack primary education (Brüggemann, 2012)—and exhibit much higher unemployment rates than the national average—less than a third work in paid employment. Therefore, it is unlikely that Roma people are the target of local policies to attract young workers. Empirical evidence confirms this claim: the correlations between the availability of preschools and labour

force participation of members of the Roma households are all economically meaningless and insignificant. Importantly, the same is true for members of non-Roma households, providing evidence that preschools do not act as an effective policy to attract qualified workers. This is in line with the results of several recent studies investigating the effect of preschools on maternal labour supply, for instance, Cascio (2009), Fitzpatrick (2010), Goux and Maurin (2010) and Havnes and Mogstad (2011b).

Taken together, our IV method essentially relies on variation in the distance to preschool across Roma children living in the same region and sharing similar family and neighbourhood characteristics. Conditionally on the set of control variables mentioned, we believe that individual distance to preschool is arguably exogenous—neither correlated with Roma families' location choice, lobbyism or local preferences, nor local politicians' activities to attract workers. To strengthen this argument, we provide the results of the test on exogeneity and monotonicity that was suggested by Huber and Mellace (2015) (for details refer to Section 5.2).

Under the previous conditions, our approach identifies the local average treatment effect LATE of preschool on child development in the subgroup of compliers. Hence, our estimates measure the effects of attending preschool on the development of the marginal child who only starts to attend preschool if living close by. Only if we—as is the case in standard linear IV models—impose the strong assumption of effect homogeneity (that preschool attendance exerts the same effect on everyone), our estimated effect would apply to the entire population of Roma children. Who is the marginal child and how does he or she compare with the average Roma child? Table 5 contrasts descriptive statistics of the complier population and the average population of Roma children in our sample. As we can see there are only some minor differences such as in the possession of some wealth items and the specific location of the family home. Regarding external validity it is important to bear in mind that the area under study covers 85% of all Roma people in 12 countries of south-east Europe, which is the main location of residence of Roma people. Thus, our estimates provide a useful benchmark for the effect of expanding preschool availability on the development and integration of Roma children across a large swath of Europe.

4.3. Estimation

This section briefly sketches our estimation approaches based on IPW, which are entirely non-parametric. As discussed in Section 4.2, we rely on two alternative identification strategies: our baseline estimates rely on an IV approach, whereas a selection-on-observables approach provides some benchmark estimates. In what follows we focus on the IPW estimator relying on an IV approach (see also Frölich (2007) for details). The IPW estimator relying on the selection-on-observables assumption is analogous to the IPW estimator relying on an IV approach and can be obtained by replacing the instrument by the treatment (refer to Hirano *et al.* (2003) for details). (To be more precise, in the IPW estimator relying on a selection-on-observables approach Z_i is replaced by D_i everywhere in equation (1). This implies that the denominator reduces to 1 and that an estimate of the treatment propensity score $\Pr(D=1|X=X_i)$ is used for weighting.)

The estimator underlying the IPW estimation relying on an IV is defined as follows: let D_i denote the preschool attendance indicator (i.e. $D_i=1$ if attending and $D_i=0$ if not attending) of a particular individual i in our sample of n observations. Z_i represents the binary instrument, which takes the value 1 when individual i lives less than 1 km from preschool and 0 otherwise. Y_i corresponds to one of the child outcomes of interest, whereas X_i denotes the vector of observed covariates. Finally, let $p(X_i) \equiv \Pr(Z=1|X=X_i)$, which represents the conditional

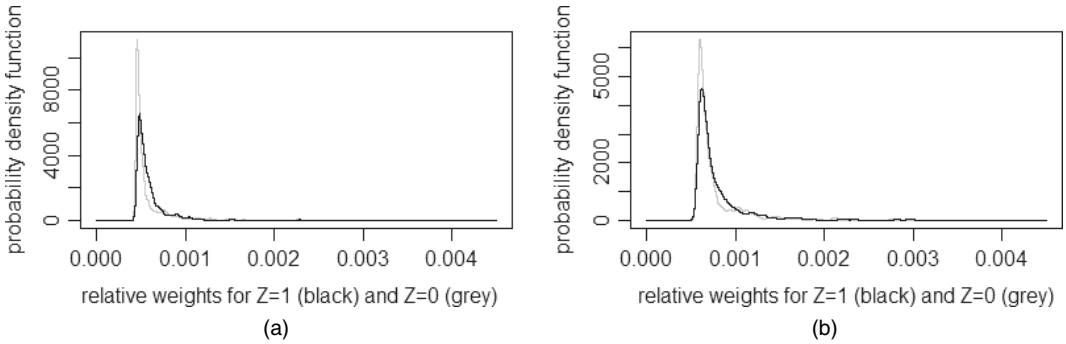


Fig. 1. Relative weights (within instrument state) for (a) literacy and social–emotional outcomes and (b) health outcomes: kernel density estimation of the relative weights, namely $Z_i/\tilde{p}(X_i)$ if Z_i and $(1-Z_i)/\tilde{p}(X_i)$ if $Z_i = 0$ (see also equation (1)), are based on the Gaussian kernel and the Silverman (1986) rule of thumb for bandwidth selection

probability of living close to preschool given the observed characteristics, which is also known as the (instrument) propensity score.

Instrument assignment is as good as random once we condition on the covariates. Instead of conditioning on the covariates directly, we can also condition on the propensity score (see Rosenbaum and Rubin (1983)). In applications using semiparametric or non-parametric IV methods, controlling for the one-dimensional propensity score is often more feasible than controlling for a multi-dimensional vector of covariates. In other words, it may not be possible to find observations across instrument states that are comparable in terms of covariates for all combinations of covariate values in the data. Since distinct combinations of covariates may still yield similar propensity scores, finding comparable observations in terms of propensity scores is possible. Since the true propensity score is unknown, in practice any propensity score method relies on an estimate of this parameter, denoted by $\hat{p}(X_i)$. We estimate the latter non-parametrically by local constant kernel regression as discussed in Racine and Li (2004). This method allows for both continuous and discrete regressors and is implemented by using the package by Hayfield and Racine (2008) for the statistical software R. We rely on Kullback–Leibler cross-validation (Hurvich *et al.*, 1998) to choose the appropriate bandwidth, but we test for sensitivity when cutting the bandwidth by half (see Section 5.2). Finally, we estimate the effect of preschool attendance by IPW, using the normalized sample analogue of expression (11) in Frölich (2007) where the normalization guarantees that the weights that the observations obtain in the minuends and subtrahends of the numerator and denominator sum to 1:

$$\text{estimated LATE} = \frac{\sum_{i=1}^{n} Y_i Z_i/\tilde{p}(X_i) - \sum_{i=1}^{n} Y_i (1-Z_i)/\bar{p}(X_i)}{\sum_{i=1}^{n} D_i Z_i/\tilde{p}(X_i) - \sum_{i=1}^{n} D_i (1-Z_i)/\bar{p}(X_i)}, \tag{1}$$

with $\tilde{p}(X_i) = \hat{p}(X_i)/\{\Sigma_{i=1}^{n} Z_i/\hat{p}(X_i)\}$ and $\bar{p}(X_i) = \{1-\hat{p}(X_i)\}/[\{\Sigma_{i=1}^{n}(1-Z_i)/\{1-\hat{p}(X_i)\}]$ being the normalized propensity scores. For a representation of the relative weights in our application refer to Fig 1.

The exact selection of control variables is as discussed in Section 4.2 based on the literature on children's human capital production (Cunha *et al.*, 2006) and contains the following variables (see also Table 2, panels (b) and (c), for descriptive statistics): age (in years) and gender of the child, education of the household head (dummies for three categories), the type of home

(dummies for four categories), infrastructure of the house and the neighbourhood (dummies for the availability of electricity, a sewage system, a toilet and a kitchen), wealth items (dummies for having a bed for each household member, a computer, radio or television) and neighbourhood characteristics, such as the location (dummies for four categories), distance to the next city, municipality office and bus stop (dummies indicating whether the distance is less than 1 km). In addition we control for a set of regional dummies (40 altogether). Note that the set of control variables does not include parental employment or use of the local language as these variables are potentially endogenous to preschool attendance—so-called bad controls (Angrist and Pischke, 2008)—and thus may bias our results.

It is worth noting that, in contrast with the vast majority of IV applications, our empirical strategy is very flexible in terms of functional form assumptions. In particular, linearity in the first-stage regression or the outcome model need not be imposed, as for instance in two-stage least squares. As both preschool attendance and all except one outcome are binary, two-stage least squares appear particularly unattractive in our application because they are generally only consistent under linear probability models for both stages. Our approach is also more robust than semiparametric estimators that rely on parametric models for the propensity score (like probit or logit), which may be prone to misspecification. In our case, the functional form of $\Pr(Z = 1|X = X_i)$ is not restricted at all.

Concerning inference, we estimate the standard error of the LATE-estimator by (jointly) bootstrapping the estimation steps in the numerator and denominator of equation (1) 1999 times. We apply a cluster bootstrap, so that entire households rather than individuals are randomly drawn with replacement in each bootstrap sample. Clustering allows for dependences between unobserved characteristics of children who live in the same household, which probably arise because of exposure to similar social networks, parenting style, etc. In a sensitivity analysis that is shown in Section 5.2, we cluster instead on the level of local cluster which allows for dependences between unobserved characteristics of children who live in the same neighbourhood. Such dependences may arise due to exposure to similar institutions, infrastructure or social networks.

5. Results

5.1. Main results

What are the effects of attending preschool on the development of Roma children? Table 6 displays the estimates resulting from the two alternative approaches: the selection-on-observables method and IV method. We first discuss the estimates based on the selection-on-observables approach. As we judge the underlying assumption to be quite strong in the context of our application, the estimates serve in our opinion only as a benchmark for the estimates based on the IV approach. The estimates resulting from the IV approach are, in our opinion, more plausible for assessing the causal effects of preschool attendance.

The overall picture resulting from the estimates under the selection-on-observables assumption resembles quite closely the picture resulting from the unconditional correlations (Table 6, panel (a), *versus* Table 2, last column): children attending preschool outperform children not attending preschool. The conditional estimates are only slightly smaller in magnitude than the unconditional estimates. Children attending preschool are more likely to know 10 letters of the alphabet (by 25.2 PPs), to be able to write their name (by 24.7 PPs), to be able to read four popular words (by 21.9 PPs), to recognize the symbols of at least 10 numbers (by 28.6 PPs) and to understand the local language (by 14.7 PPs). The differences between children attending and not attending preschool regarding a couple of measures for children's social–emotional development also maintain their relevance and significance (at any conventional level) when

Table 6. Estimation results†

Variable	*ATE (IPW)*	*Standard error*	*p-value*	*ATE (IPW)*	*Standard error*	*p-value*
(a) Literacy (N = 3334)						
10 letters	0.252	0.017	0.000	0.310	0.145	0.033
Write name	0.247	0.017	0.000	0.206	0.124	0.097
4 words	0.219	0.016	0.000	0.177	0.122	0.147
10 numbers	0.286	0.018	0.000	0.424	0.152	0.005
Understand local language	0.147	0.018	0.000	0.039	0.157	0.802
First stage	—	—	—	0.109	0.016	0.000
(b) Social–emotional outcomes (N = 3334)						
Self-confidence	0.117	0.015	0.000	−0.015	0.140	0.916
Peer relations	0.076	0.013	0.000	−0.051	0.116	0.657
First stage	—	—	—	0.109	0.016	0.000
(c) Health (N = 2478)						
General health	0.027	0.036	0.454	−0.100	0.302	0.741
Long-standing illness	−0.018	0.009	0.043	0.082	0.081	0.308
Limited activity	0.006	0.014	0.658	−0.204	0.126	0.105
Vaccinated (dummy)	0.039	0.012	0.001	0.207	0.119	0.082
against tuberculosis	−0.002	0.009	0.848	−0.051	0.067	0.444
against polio	0.029	0.012	0.019	0.238	0.132	0.070
against diphtheria	0.035	0.012	0.003	0.146	0.126	0.247
against measles	0.046	0.012	0.000	0.172	0.131	0.189
First stage	—	—	—	0.114	0.019	0.000

†The results reflect the estimates resulting from separate selection-on-observables and IV estimations by using the IPW methods as discussed in Hirano *et al.* (2003) for ATE estimation and Frölich (2007) for LATE estimation respectively (see Section 4.3 for details). The set of controls corresponds to the variables shown in Table 2. Standard errors are estimated based on bootstrapping 1999 times.

conditioning on the full set of controls: children attending preschool are more self-confident (by 11.7 PPs) and integrate better with their peers (by 7.6 PPs). Regarding the series of health-related outcomes we observe that children attending preschool are slightly less likely to suffer from a long-standing illness (by 1.8 PPs) and are more likely to be vaccinated (by 3.9 PPs). Again these estimates are comparable in magnitude with the unconditional correlations displayed in Table 2.

As discussed in Section 4.2, omitted variables may bias the estimates resulting from a selection-on-observables strategy. We therefore proceed with the estimates resulting from the IV approach. Before doing so, we briefly discuss the first stage of our IV estimation. In other words, we address the question of whether the availability of a preschool nearby indeed stimulates preschool attendance among (at least some) Roma children. Doing so sheds some light on the relevance of the instrument: one of the assumptions underlying an IV estimation (see Section 4.2 for details). Table 6 shows that in the case of the sample that was available for children's literacy and social–emotional outcomes the instrument shifts the likelihood of Roma children to attend preschool by 10.9 PPs, which is statistically significant at any conventional level. Using the smaller sample that is available for children's health outcomes, the first stage is of similar magnitude—11.4 PPs—and is also significant at any conventional level of significance. In light of the rather low level of preschool attendance among Roma children—not even a third (30.9 %) of the children in our Roma sample goes to preschool—this is a substantial increase. Among the Roma children in our sample who do not have a preschool close by, attendance is even lower, amounting to

Table 7. First stages per country†

Country	*First stage*	*Standard error*	*p-value*	*Number of observations*
Albania	0.188	0.062	0.002	244
Bosnia and Herzegovina	0.056	0.048	0.248	329
Bulgaria	0.133	0.084	0.113	187
Croatia	0.113	0.053	0.035	409
Czech Republic	−0.051	0.057	0.369	289
Hungary	−0.004	0.053	0.940	256
Macedonia	0.084	0.063	0.181	239
Moldova	0.119	0.063	0.058	204
Montenegro	−0.022	0.047	0.640	282
Romania	0.064	0.071	0.364	290
Serbia	0.110	0.041	0.008	328
Slovakia	0.088	0.055	0.108	277

†Country-specific first-stage estimations (using all covariates of the main specification) based on the denominator of equation (1).

only a fifth. Hence, the mere offer of a preschool facility in the neighbourhood helps to integrate Roma children into the local educational system early in life. The effectiveness of such a policy, however, differs across countries (Table 7). Putting aside the issue of precision—note that country samples contain rarely more than 300 observations—the availability of a preschool nearby shifts preschool attendance in all except three countries. The exceptions are the Czech Republic, Hungary and Montenegro. In the next section we therefore provide estimation results when excluding these three countries, even though selecting countries on the basis of country-specific first stages may jeopardize the accuracy of the results because of pretesting bias. See also Abadie *et al.* (2015) for a discussion on this issue.

The IV estimates based on our non-parametric IPW approach presented in Table 6, panel (a), suggest that preschool attendance brings considerable benefits for Roma children in terms of literacy. Roma children in preschool are 31.0 PPs more likely to know 10 letters of the alphabet (significant at the 5% level), 20.6 PPs more likely to write their own name and 42.4 PPs more likely to recognize the symbols for the numbers 1–10. The estimated benefits of preschool exposure are substantial, in particular in the light of the poor performance of Roma children without exposure to preschool (see Table 2): only 20.6% of the Roma children without exposure to preschool can name 10 letters of the alphabet, 10.8% can write their own name and only 31.0% recognize the symbols for the numbers 1–10. Hence, the point estimates suggest that exposure to preschool more than doubles the performance of Roma children in the performance of such literacy tasks and, thus, helps to close the performance gap with non-Roma children (which amounts to 26.9 PPs, 23.9 PPs and 28.6 PPs respectively). As a word of caution, however, it needs to be mentioned that the IV confidence intervals are quite wide, even for the statistically significant effects. Interestingly, the estimated effects for the influence of preschool attendance on children's literacy resulting from the IV approach are comparable with the estimates resulting from the selection-on-observables approach. Therefore, omitted confounding variables do not seem to play a big role in the case of the specific measures of children's literacy. As discussed in Section 4.3 the marginal child is comparable in terms of observable characteristics with the average child. As such we also do not expect strong effect heterogeneity, at least not for this set of outcomes.

To judge further about the magnitude of these estimates, we compare them with estimates that have previously been found in the literature. We draw only on studies investigating the

effect of universal preschool on minority children. The reason for this is, as discussed in Section 1, that universal programmes may not be sufficient for addressing the needs of minority children and thus findings for targeted programmes may differ enormously from findings for universal programmes. For such a comparison it is important to express the effects in terms of a standardized measure such as in terms of standard deviations (SDs). The effects correspond to 0.65 SDs, 0.52 SDs and 0.86 SDs in the case of being able to name 10 letters, to write his or her own name and to identify the symbols of the numbers 1–10. These effects are comparable with those found, for instance, for ethnic minorities in Oklahoma (Gormley *et al.*, 2005). The estimated benefits from preschool exposure for black children varied between 0.52 and 0.74 SDs for verbal skills and amounted to 0.30 SDs for analytical skills. In the case of Hispanic children, benefits were about 0.94 SDs for analytical skills and ranged between 0.98 and 1.5 SDs for verbal skills.

The skills that have been discussed so far are the outcomes of basic literacy tests. Preschool seems to do a good job of boosting the Roma children's performance in such tests. The picture, however, changes when evaluating the effect of attending preschool on Roma children's proficiency in the local language, which is a more complex measure than the performance in basic literacy tests. In contrast with the estimate resulting from the selection-on-observables approach, the IV estimate for the effect of preschool attendance on children's proficiency in the local language is almost 0 and insignificant. These differences point towards the existence of relevant confounding variables of parents' decision to send their child to preschool and their effort to enhance children's knowledge of the local language. What explains the absence of any causal effect of preschool on Roma children's language proficiency? One possible explanation may be that Roma are likely to settle in compact areas. As such, there may also be a high concentration of Roma children in preschools and, thus, little scope for teachers and native peers to influence Roma children's proficiency in the local language. (We have experimented with splitting the sample according to the share of Roma living in the neighbourhood. Although the point estimates for Roma children living in high Roma concentration areas (above 50%) are substantially higher than the point estimates for Roma children living in low concentration areas (19.3 PPs *versus* −1.7 PPs), the confidence intervals are too big to derive a strong statement regarding the relevance of the ethnic composition of the peers.) This is particularly important when comparing our results with the results of the study by Dustmann *et al.* (2013) who analysed the effect of universal preschool on immigrant children. In contrast with our study, they found positive returns to preschool in terms of language proficiency. However, they focused on a county in west Germany with a rather low immigrant ratio (abut 10%).

Also in the case of children's social–emotional development, such as self-confidence and peer relations, the IV estimates cannot confirm the positive and significant estimates resulting from the selection-on-observables approach (Table 6, panel (b)). This difference between the results based on the alternative approaches points to the existence of important unobserved determinants of parental decision to send their children to preschool and inputs into the production of children's social–emotional development. Such confounding variables may be, for instance, parents' availability and effort to spend time with their children. In fact, a prominent theory that has been established in developmental psychology—the so-called attachment theory (Bowlby, 1969)—stresses the importance of a clear contact person for the development of children's social–emotional skills. Unfortunately, we are unaware of any study providing estimates of universal preschool on minority children's socioemotional development. Findings from studies on children of the majority population provide mixed evidence. Whereas Baker *et al.* (2008) and Kottelenberg and Lehrer (2014) found negative effects of universal care on children's socioemotional outcomes in Quebec—note, however, that the reform under study

affected mainly children from advantaged backgrounds—Datta-Gupta and Simonsen (2010) found neutral effects of universal care in Denmark.

Finally, preschool attendance causes an increase in the prevalence of vaccinations among Roma children (see Table 6, panel (c)). In contrast with non-Roma children who have almost all been vaccinated (95.3%), only 81.5% of Roma children without preschool exposure have been. When attending preschool this share rises by 20.4 PPs (significant at the 10% level of significance only) and thus leads to full coverage among Roma children. This result is driven by vaccination against poliomyelitis—a viral infection which leads to paralysis among children. This achievement is notable as poliomyelitis still prevails among the Roma population whereas it has basically been extinguished in most parts of Europe (Loewenberg, 2006). The increase in the prevalence of vaccinations may be due to several channels and is likely to vary across countries under study and even within countries across preschool facilities: preschools may provide children with vaccinations (they may be even mandatory for children enrolled in preschool) or preschool may inform parents about the importance of vaccinations. Besides the increased incidence of vaccination, there are no further gains in terms of children's health when attending preschool (see Table 6, panel (c)). This stands again in contrast with the results based on the selection-on-observables approach which indicated an association between preschool attendance and less long-standing illness. In this case, reverse causality may be at play: children suffering from a long-standing illness may be less likely to attend preschool, which is an issue which is unlikely to be detected by a selection-on-observables approach.

5.2. Tests and robustness checks

In this section we provide further evidence that supports the assumption underlying our baseline strategy—the IV approach—and provide the results of a series of alternative estimations testing the robustness of our baseline estimates to, for example, alternative clustering, the choice of bandwidth and the choice of the support area.

As discussed in detail in Section 4.2, our identification strategy relies on variation in the individual distance to preschool across Roma children living in the same region and sharing similar neighbourhood and household characteristics. Given the results of the balancing tests shown in Table 4, we argued that, conditionally on the set of control variables mentioned, individual distance to preschool is exogenous, i.e. conditionally unrelated to Roma families' choice of location, lobbyism or local preferences or political efforts of the local politicians. To strengthen this argument, we perform an additional test assessing the unconditional validity of our instrument (the validity of our instrument when not conditioning on any covariates). For this we perform the IV test that was suggested by Huber and Mellace (2015) which jointly assesses two assumptions made about the instrument: a monotonous increase in preschool attendance when the distance to the next preschool is decreasing and mean independence of the distance to the next preschool and the potential treatment and outcome states. (For a more formal discussion refer to Appendix A.) As we can see in Table 8, the null hypothesis regarding monotonicity and mean independence of the IV cannot be rejected at any conventional level. Note, however, that the test cannot detect all violations, not even asymptotically. The reason for this is that the test relies on partial identification of the mean potential outcomes of the so-called always- and never-takers in the notation of Angrist *et al.* (1996). The specific problem is that asymptotic test power decreases with the share of compliers. The latter is, however, moderate in our application.

As discussed in Section 5.1, the first stage is positive in nine out of 12 countries. In the case of the Czech Republic, Hungary and Montenegro the first stage is 0 or even negative. We therefore provide estimation results when excluding these three countries from our estimations. As we

Table 8. Testing monotonicity and validity of the IV†

Outcome	*p-value*	*Outcome*	*p-value*	*Outcome*	*p-value*
Understand language	1.000	Confidence	1.000	Vaccinated (dummy)	0.531
10 letters	0.999	Peer relations	0.997	against tuberculosis	0.626
Write name	1.000	General health	0.999	against polio	0.125
4 words	1.000	Long-standing illness	0.959	against diphtheria	0.999
10 numbers	1.000	Limited activity	0.785	against measles	0.695

†The *p*-values refer to the null hypothesis stating monotonicity and mean independence of the distance to the next preschool and the potential treatment and outcome states. The estimator was proposed by Huber and Mellace (2015). The *p*-values are based on bootstrapping 999 times.

can see in Table 9, third column, results are robust when using the countries with a positive first stage only.

As mentioned in Section 3, health-related outcomes are reported by the household head and not by the primary care giver as is the case with the remaining outcomes under study. The resulting sample that is available for health-related outcomes is substantially smaller than the sample that is available for the remaining outcomes. One may wonder whether this is due to selective responses by the household head. If this is so, the absence of basically any effect on health outcomes may be caused by selective response and thus be a biased estimate for the causal effect of preschool attendance on children's health. To exclude this possibility, we provide estimates for the remaining outcomes—literacy and social–emotional development—using the restricted sample that is available for health outcomes; see Table 9, fourth column.

Another potential issue is a lack of common support in the estimated instrument propensity scores across values of the instrument. This may for instance imply that, for children living close to a preschool and having a high propensity score, there are no children among the children who live further from a preschool with a comparable propensity score. Fig. 2 provides the propensity score distributions across instrument states and suggests that we run into common support problems as propensity score values approach 0 or 1. No observation receives a relative weight that is larger than 0.43%, implying that no data point is excessively influential in our IPW approach. For this reason, we test the robustness of the results when trimming extreme values of the propensity scores—when dropping observations with propensity scores that are higher than 98% and lower than 2% from our estimation sample. As we can see in Table 9, fifth column, the results are robust to trimming.

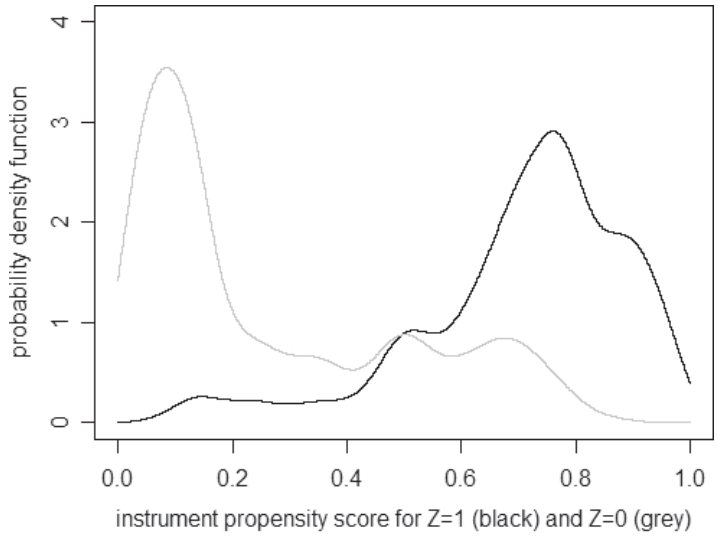
Finally, as explained in Section 4.3, we also test the robustness of our results with respect to

(a) cutting the kernel bandwidths (coming from cross-validation) by half, which implies undersmoothing (see Table 9, sixth column), and
(b) an alternative unit of clustering, namely the neighbourhood (see Table 9, seventh column).

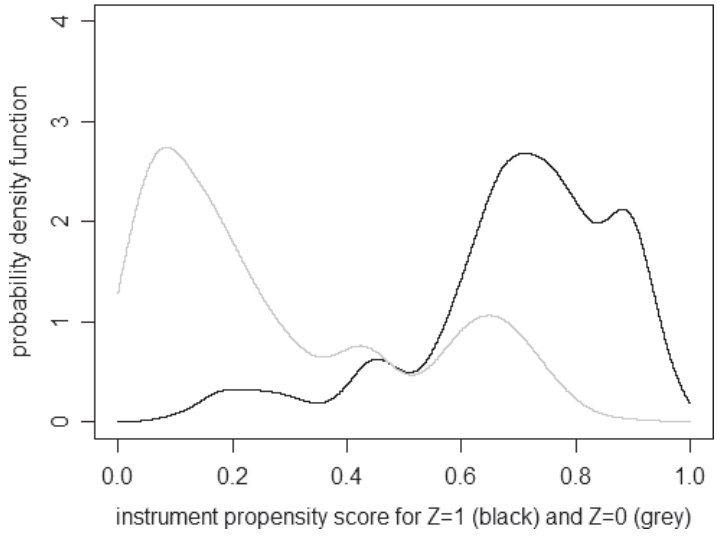
Effect estimates are quite stable across these alternative specifications, but standard errors increase slightly when clustering at the neighbourhood.

6. Conclusion

This paper addresses the question of whether exposing Roma children to preschools stimulates their development and thus contributes to closing the development gap that is observed between Roma and non-Roma children. The empirical analysis is based on a recent data set developed by the UNDP, the WB and the EC. To tackle endogenous selection into preschool, we use



(a)



(b)

Fig. 2. Propensity score distributions for (a) cognitive or personality outcomes and (b) health outcomes: kernel density estimation of the estimated propensity scores are based on the Gaussian kernel and the Silverman (1986) rule of thumb for bandwidth selection

distance to the nearest preschool as an instrument for attendance. Estimates resulting from a non-parametric IV approach highlight important development gains: preschool attendance raises Roma children's likelihood to know 10 letters of the alphabet by 31.0 PPs, to be able to write their own name by 20.6 PPs, and to recognize the symbols for numbers 1–10 by 42.4 PPs. In addition, preschool increases the prevalence of vaccinations among Roma children by 20.7 PPs. However, it must be acknowledged that, albeit the effects are significantly different from 0, the confidence intervals around the point estimates are non-negligible.

Given these results, expanding preschools appears to be a successful policy idea for diminishing early arising inequalities between children from minority and majority groups in terms

Table 9. Robustness checks†

	Baseline	*Complier*	*Health sample*	*Trimming*	*Bandwidth*	*Cluster*
(a) Literacy						
10 letters	0.310	0.242	0.359	0.441	0.248	0.310
	(0.145)	(0.134)	(0.168)	(0.151)	(0.118)	(0.178)
Write name	0.206	0.159	0.254	0.281	0.127	0.206
	(0.124)	(0.112)	(0.143)	(0.130)	(0.103)	(0.157)
4 words	0.177	0.059	0.260	0.253	0.085	0.177
	(0.122)	(0.117)	(0.137)	(0.126)	(0.101)	(0.177)
10 numbers	0.424	0.306	0.402	0.488	0.378	0.424
	(0.152)	(0.144)	(0.169)	(0.163)	(0.130)	(0.200)
Understand language	0.039	0.163	0.051	0.069	0.123	0.039
	(0.157)	(0.145)	(0.169)	(0.164)	(0.130)	(0.263)
First stage	0.109	0.123	0.139	0.112	0.136	0.109
	(0.016)	(0.021)	(0.019)	(0.017)	(0.017)	(0.024)
Observations	3334	2194	2507	3033	3334	3334
(b) Social–emotional development						
Self-confidence	−0.015	0.033	−0.063	−0.046	0.045	−0.015
	(0.140)	(0.135)	(0.151)	(0.146)	(0.117)	(0.190)
Peer relations	−0.051	−0.101	−0.055	−0.074	0.045	−0.051
	(0.116)	(0.114)	(0.129)	(0.122)	(0.096)	(0.150)
First stage	0.109	0.139	0.123	0.112	0.136	0.109
	(0.016)	(0.019)	(0.021)	(0.017)	(0.017)	(0.024)
Observations	3334	2507	2194	3033	3334	3334
(c) Health						
General health	−0.100	0.283	−0.100	0.078	−0.382	−0.100
	(0.302)	(0.318)	(0.302)	(0.324)	(0.262)	(0.386)
Long-standing illness	0.082	−0.052	0.082	0.046	0.072	0.082
	(0.081)	(0.082)	(0.081)	(0.087)	(0.067)	(0.109)
Limited activity	−0.204	−0.088	−0.204	−0.140	−0.168	−0.204
	(0.126)	(0.135)	(0.126)	(0.137)	(0.107)	(0.183)
Vaccinated (dummy)	0.207	0.156	0.207	0.162	0.212	0.207
	(0.119)	(0.133)	(0.119)	(0.122)	(0.110)	(0.197)
First stage	0.114	0.136	0.114	0.111	0.145	0.114
	(0.019)	(0.021)	(0.019)	(0.021)	(0.020)	(0.026)
Observations	2478	1808	2478	2250	2478	2478

†The second column shows our baseline estimates. The third column shows the estimates when dropping the countries with a negative first stage from the sample. The fourth column displays the estimates when restricting the sample to children for whom we observe the health outcomes. The fifth column provides the results when discarding observations with p-scores above 0.95 and below 0.05. The sixth column shows estimates when using half of the kernel bandwidths determined by cross-validation. The last column displays the estimates when clustering at the neighbourhood instead of the household level. Standard errors are estimated based on bootstrapping 1999 times and are shown below the effect estimates in parentheses.

of literacy. There is a growing amount of research relating early literacy to later success on the labour market. Duncan *et al*. (2007), for instance, showed that children's intellectual skills at school entry are key in predicting later educational achievements. Also Gregg and Machin (1999, 2001) discussed the relevance of children's early cognitive abilities for their later success in the labour market. Nevertheless, the absence of (short-term) gains in terms of language proficiency and children's social–emotional development may cast some doubts on the efficiency of preschools as a tool to enhance integration of minority children into society. Reasons for this may be the high residential segregation of Romas and thus the rather high concentration of

Roma children in preschools. Moreover, we cannot ignore the segregation that exists between Roma and non-Roma children within preschools. In addition, staff working in these facilities may belong to the ethnic minority and thus may not be able to enhance children's understanding of the local language and cultural habits. As such, the existing initiatives to foster Roma children's participation in preschool, such as the 'Roma good start initiative' or the Roma Education Fund, may not reach the desired objectives if they only promote Roma children's participation in preschool without guaranteeing that preschools exhibit a sufficient mixture of Roma and non-Roma children and foster the interaction between the different ethnicities. To enhance the effectiveness of preschools as a tool for integrating Roma children, future research should therefore dig deeper into the relevance of structural characteristics of preschools, such as peer group composition or staff characteristics.

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Appendix A: Testing the instrumental variables assumptions

We subsequently present IV tests for the unconditional validity of our instrument (i.e. when not controlling for covariates X). To be precise, the method jointly tests monotonicity of preschool attendance in distance to preschool and the mean independence of the distance instrument of both potential outcomes and treatment states as proposed by Huber and Mellace (2015), who showed that, under the satisfaction of both assumptions, the following testable mean constraints must hold:

$$\begin{aligned} &E(Y|D=1,Z=1,Y\leqslant y_q)\leqslant E(Y|D=1,Z=0)\leqslant E(Y|D=1,Z=1,Y\geqslant y_{1-q}),\\ &E(Y|D=0,Z=0,Y\leqslant y_r)\leqslant E(Y|D=0,Z=1)\leqslant E(Y|D=0,Z=0,Y\geqslant y_{1-r}), \end{aligned} \tag{2}$$

where (under our identifying assumptions) $q=\Pr(D=1|Z=0)/\Pr(D=1|Z=1)$ is the share of always-takers (those who attend preschool no matter whether they live close or further away) in the population with $D=1$ and $Z=1$. $r=\Pr(D=0|Z=1)/\Pr(D=0|Z=0)$ is the share of never-takers (those who do not attend preschool no matter whether they live close or further away) in the population with $D=0$ and $Z=0$.

As discussed in Huber and Mellace (2015) in more detail, the intuition of the test is that $E(Y|D=1,Z=0)$ and $E(Y|D=0,Z=1)$ point-identify the mean potential outcomes (as a function of D) of the always- or never-takers under $D=1$ and $D=0$ respectively. At the same time, the mean potential outcomes of the latter groups can be bounded in the (mixed) populations with $D=1$ and $Z=1$, and $D=0$ and $Z=0$ respectively, that also contain compliers. One can therefore test whether the points lie within the bounds as postulated in expression (2). We do so applying the minimum p-value-based test of Chen and Szroeter (2009) for multiple inequality constraints. The distribution of the test statistic is estimated by bootstrapping (we use 999 bootstrap draws) and relies on pre-estimating which inequality constraints are (close to being) violated to increase testing power in finite samples. See for instance Andrews and Soares (2010) and Chen and Szroeter (2014) for a more detailed discussion of moment selection based on pre-estimating which constraints are (almost) violated. The p-values of the IV tests for the various outcomes are provided in Table 8 and are never significant on any conventional level. As a word of caution, however, it should be noted that, even asymptotically, these tests cannot find all possible violations, as they rely on a partial identification approach. Specifically, asymptotic power decreases in the share of compliers (which, however, is moderate in our application).

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