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Estimating the impact of language of instruction in South African primary schools: A fixed effects approach

Stephen Taylor[a,b,c,*], Marisa von Fintel[b,c]

[a] Department of Basic Education, 222 Struben Street, Pretoria 0002, South Africa
[b] Department of Economics, University of Stellenbosch, South Africa
[c] Research on Socio-Economic Policy (ReSEP), University of Stellenbosch, South Africa

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ABSTRACT

For many children around the world, access to higher education and the labour market depends on becoming fluent in a second language. In South Africa, the majority of children do not speak English as their first language but are required to undertake their final school-leaving examinations in English. Most schools offer mother-tongue instruction in the first three grades of school and then transition to English as the language of instruction in the fourth grade. Some schools use English as the language of instruction from the first grade. In recent years a number of schools have changed their policy, thus creating within-school, cross-grade variation in the language of instruction received in the early grades. Using longitudinal data from the population of South African primary schools and a fixed-effects approach, we find that mother tongue instruction in the early grades significantly improves English acquisition, as measured in grades 4, 5 and 6.

1. Introduction

Low quality education characterised by flat learning trajectories over time is a feature of education systems in many developing economies (Pritchett, 2013). Economists have pointed out that the causes of low quality schooling in these countries are at least partly those related to public service provision in general, such as provider absence and weak incentives (Kremer, Brannen, & Glennerster, 2013), but another factor more commonly cited amongst educationists is that children in these countries often face language disadvantages at school.

Questions regarding the impact of language on education outcomes are extremely relevant to South Africa, a country with 11 official languages[1] and large inequalities within the public school system. International surveys (such as the Progress in International Reading Literacy Study surveys of 2006 and 2011, as well as the Trends in International Mathematics and Science Study surveys of 1995, 1999, 2003 and 2011) have consistently demonstrated that South Africa's performance is amongst the lowest of all participating countries. The extent to which language factors contribute to this low performance is not clear, given that language disadvantages are so strongly correlated with other confounding factors such as historical disadvantage, socio-economic status, geography, the quality of school management and the quality of teachers.

* Corresponding author at: Department of Basic Education, 222 Struben Street, Pretoria 0001, South Africa. Tel.: +27 12 3574156.
E-mail address: taylor.s@dbe.gov.za, stephentaylor17@gmail.com (S. Taylor).

[1] This list includes nine African languages (isiZulu, isiXhosa, isiNdebele, Sepedi, Setswana, Sesotho, SiSwati, Xitsonga, Tshivenda) as well as the two European languages (English and Afrikaans, which evolved from the Dutch spoken by the early Cape settlers).

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Although there are 11 official languages in South Africa, Afrikaans and English are the only languages with a developed academic literature and in which it is possible to write the secondary school leaving examinations. In addition, the advantages of English proficiency have been demonstrated by Casale and Posel (2011), who show that, in South Africa, English proficiency also improves labour market returns directly. Using a traditional earnings function methodology controlling for an individual's amount of education, they find a significant wage premium for black South Africans associated with being able to read and write English fluently.[2]

According to the 2011 census, only about 23% of South Africans speak Afrikaans or English as their first language (Statistics South Africa, 2012). In order to achieve educational as well as labour market success, the majority of South African children therefore need to become fluent in either English or Afrikaans. In reality, the vast majority choose to learn English rather than Afrikaans as the second language, given its status as a global language, as well as the fact that English is widely perceived to be the language of upward mobility, leading to a preference for instruction in English from as early as possible.

This situation presents a difficult policy question to South African policy makers: when and how should the teaching of English be introduced in schools, and when and how should a transition to English as the primary language of instruction in non-language subjects occur? Several models exist in theory, each with numerous variations that have been applied in different parts of the world.[3]

Pedagogical theory appears to be stacked more heavily in favour of using first language as language of instruction until a level of academic proficiency has been attained in that language (which may take three to six years) rather than using a second language from the start of school (Hakuta, Butler, & Witt, 2000).

However, there are often practical realities which may influence the relative effectiveness of alternative models such as logistical difficulties, a lack of teachers who are proficient in the various home languages, and the general capability and motivation of the teacher force. Similarly, the transition to English that must occur in bilingual models may be extremely disruptive and educationally damaging if a high quality of support materials and teacher expertise does not exist to manage this phase effectively – a concern that is often expressed in South Africa (e.g. Van der Berg et al., 2011).

There may be numerous other political or ideological motivations behind a particular language in education policy, such as using a single language to promote national unity or developing a diverse cultural heritage (World Bank, 2005). However, the question of which approach leads to better educational outcomes in a particular context is ultimately an empirical one.

South African legislation and education policy does not prescribe which of the 11 official languages should be used, but leaves the choice of language of instruction to School Governing Bodies, which are comprised by a parent majority as well as the school principal, several staff members and, in the case of secondary schools, pupils (South Africa, 1996). Currently, most schools in which the majority of pupils are not English- or Afrikaans-speaking opt to use first language in grades 1, 2 and 3 and then transition to English as the language of instruction in the fourth grade. This approach, though not compulsory in policy, has been encouraged by the national and provincial departments of education. Some schools, however, have chosen to go "Straight-For-English" as the language of instruction from the first grade.[4]

As the next section will show, there is a dearth of empirical work using credible methods to identify the causal impact of alternative language-of-instruction models on second language acquisition or on other educational outcomes. There is an even more acute shortage of such research done in developing countries, especially those in Africa. Consequently, as Slavin, Madden, Calderon, Chamberlain, and Hennessy (2011) observe, "ideology has often trumped evidence" in language policy debates.

Hulstijn (1997) explains the major limitation in the vast field of research studying second language acquisition, namely that confounding variables affect the comparability of groups who underwent different second language learning experiences. He argues that, "*One of the most difficult methodological challenges is to keep all such variables constant. This is almost impossible in "normal" classrooms with real L2 learners. It comes as no surprise, therefore, that the outcomes of studies conducted in natural learning environments, including classrooms, often form the object of considerable disagreement.*"

A further challenge that must be overcome in order to produce meaningful empirical evidence on the relative effectiveness of alternative language-of-instruction regimes is that studies must span several years. This is because the "treatment", which is either instruction in first language or instruction in second language, lasts for several years. Furthermore, the outcome of interest is not English proficiency at the end of the "treatment period" but at a later stage once those in a bilingual programme have transitioned to English as language of instruction. The outcome of interest is really educational outcomes, in particular second language acquisition, in the long run. The vast majority of studies have not used data with a long enough time span to address this fundamental research question.

In this regard, the South African system offers a unique opportunity to evaluate the impact of language policy on

[2] This echoes previous findings from a study by Angrist and Lavy (1997) examining the impact of French language acquisition on labour market outcomes in Morocco.

[3] The interested reader is referred to the working paper version of this article, where these alternative models are discussed in greater detail. The working paper version is available online at www.ekon.sun.ac.za/wpapers/2013/wp212013.

[4] The Curriculum and Assessment Policy Statements (CAPS) also prescribe that the teaching of English as a subject should be introduced from grade 1 in all schools (Department of Basic Education, 2011). Consequently, all schools should have some English being taught from the first grade, but for some schools English is also the language of instruction from grade 1 whereas in most schools this is only the case from the fourth grade.

the acquisition of a second language. We take advantage of changes in the language policies of particular schools, which offer exogenous variation which may be used in the estimation of the impact of language policies. In addition, we use a unique dataset that was constructed by combining several datasets covering all of the years from 2007 to 2012 for the entire population of South African schools. The final subsample of data used after restricting the sample to the relevant section of the school system, contains 827 745 individuals in 9 180 primary schools. We estimate the impact of English instruction relative to first language instruction in grades 1, 2 and 3 on English proficiency in grades 4, 5 and 6. We use a school-fixed effects model to exploit within-school variation in the language of instruction in grades 1, 2 and 3 caused by historical changes in the language of instruction at specific schools. This deals with the major source of endogeneity bias caused by systematic unobserved differences between schools that adopt different language policies. We also include several individual-level and grade-specific characteristics to control for any differences across grades within a school that may be systematically related to the language of instruction. We find that three years of English instruction in the Foundation Phase (grades 1, 2 and 3) relative to three years of first language instruction is associated with a negative effect on English performance in grades 4, 5 and 6 of approximately 17% of a standard deviation in test scores. We argue that this estimate can be interpreted causally.

The significance of this study is twofold. Firstly, it illustrates the power of fixed effects estimation to address concerns about unobserved factors when estimating the causal impacts of educational interventions. Secondly, it is the first South African study (and one of a very few international studies) to bring robust empirical evidence to the current policy debate around language of instruction.[5]

The next section reviews previous empirical studies and thus further establishes the relevance of this study, given its unique design. Section 3 describes the data that is used and the derivation of the treatment variable, namely years of English instruction in the Foundation Phase. It also presents descriptive statistics for Straight for English schools and mother tongue schools as well as for schools that switched their language policy at some stage. Section 4 explains the identification strategy and statistical model used to measure the impact of the alternative language of instruction regimes. Section 5 presents the results, including numerous variations on the basic model to examine heterogeneous effects across different parts of the school system. Section 6 reports on a number of robustness checks that were conducted to address potential concerns regarding the causal interpretation that we offer. Section 7 concludes by discussing the significance of these findings for the literature on second language acquisition and for South African education policy.

2. Previous empirical studies

Several meta-analyses reviewing the literature on alternative language-of-instruction regimes have been conducted, mainly pertaining to the question of language policy for Spanish speaking children in the United States. Rossell and Baker (1996) argued that the weight of evidence from studies of sufficient methodological quality suggested no significant difference between bilingual education approaches and English-immersion approaches. However, subsequent re-analysis of the same studies (Greene, 1997; Cheung & Slavin, 2005, 2012) has demonstrated that many of these studies had serious methodological flaws and that the most credible studies in fact favoured bilingual approaches. Cheung and Slavin (2012) calculate a mean effect size of 0.21 standard deviations in favour of bilingual approaches amongst the studies they review.[6]

Cheung and Slavin (2012) find 13 studies that met their methodological criteria for inclusion. We would contend that even these 13 studies do not all provide strong grounds for causal inference. For example, Cheung and Slavin (2012) regarded matching techniques as sufficient for inclusion even though matching cannot control for unobservable characteristics, which may well determine both selection into programme and educational outcomes. Secondly, many of the studies reviewed contain samples that are really too small for precise measurement. For example, the randomised control trial conducted by Slavin et al. (2011), which Cheung and Slavin (2012) laud as the "only multiyear randomized evaluation of transitional bilingual education" only included six schools. Slavin et al. (2011) concede that the small sample was a weakness and that, therefore, in their calculation of standard errors they did not adjust for clustering at the school level.

The empirical literature on language in education in developing countries, especially those in Africa, is even less well developed. Only a few studies have provided robust empirical evidence of the impact of language of instruction, although these studies are not specifically focussed on estimating the impact on second language acquisition.

Angrist and Lavy (1997) make use of a difference-in-difference approach to estimate the impact of a shift in the language policy in Morocco on French language skills and labour market outcomes. They find a decline in French writing skills in individuals exposed to the policy introducing Arabic as language of instruction in the place of French.[7] Contrary to this finding, Angrist, Chin, and Godoy (2008) make use of a triple-difference approach in Puerto Rico, and find that greater exposure to English

[5] One exception is the study by Eriksson (2014). However, the period under study in her paper reflects the impact of language of instruction as implemented in the 1950s (a time period during which apartheid policies were still implemented), and it may therefore be argued that her results are historical in nature and do not directly feed into the current policy debate.

[6] Two studies that used observational data are worth mentioning given their strong influence in the literature, namely Ramìrez, Pasta, Yuen, Ramey, and Billings (1991) and Thomas and Collier (1997), both in the United States.

[7] Although the study is worth mentioning for the fact that it is an empirical study within the field of language use in schools, its aim was to address a different policy question than the one examined in our paper.

instruction in primary school seemed to have no impact on the English proficiency of children later in life. Most recently, Ivlevs and King (2014) use the change in language policy[8] in Latvia to estimate the impact of native language instruction on the academic performance[9] of learners in secondary schools. They find that the change in language policy had significant negative effects on the performance of the Russian-speaking minority.

The strong majority of studies on language of instruction in African countries are written by linguists in favour of mother tongue instruction. Some are quantitative, but tend to lack a methodology allowing for causal inference (for example, the study by Walter and Chuo (2012) in Cameroon; Piper and Miksic (2011) in Uganda and Kenya; Alidou et al. (2006) in Nigeria; and Benson (2000) in Mozambique).[10]

Empirical studies on the impact of language of instruction in South Africa are even scarcer. Most existing studies have been small-scale qualitative studies. Brock-Utne (2007), for example, shows, using observations from two classes, that IsiXhosa speaking children learn better when being instructed in their home language. Vorster, Mayet, and Taylor (2013) use a nationally representative dataset (albeit excluding one of the nine provinces) to estimate the disadvantage of writing a test in English versus in mother tongue. This study makes use of a dataset containing two sets of test scores for the same children in the same year on the same test administered in English on one occasion and in mother tongue on another occasion. These studies, however, do not really address the policy question on when the language of instruction should switch to English.

It is therefore clear that existing empirical evidence about the causal impact of bilingual transitional programmes relative to English immersion programmes on learning outcomes, specifically on second language acquisition, is insufficient. This is true internationally, but is especially true for African countries and South Africa.

One exception is the study by Eriksson (2014), where the change in language policy under the Bantu Education Act of 1953 (which extended the number of years for which black children received instruction in their mother tongue) is used as an identification strategy. Eriksson (2014) makes use of a difference-in-difference approach using the variation across time periods as well as the variation in exposure to mother tongue instruction across provinces induced by the exogenous change in the language policy. Using the 1980 census data, she is able to estimate the long-run impact of the shift in language policy for the cohorts affected by the change. She finds that increased exposure to mother tongue instruction has positive and significant impacts on income, literacy and numeracy, years of education and the ability to speak English for individuals living in predominantly English areas.

We would argue that our results are a contemporary extension of the conclusions by Eriksson (2014), whose study has a historical focus and relates to the time period up to 1980, during which apartheid policies were being implemented.

3. The data

We constructed a unique dataset by merging information from the Department of Basic Education's Annual Surveys of Schools (ASS) from 2007 to 2011 with the Annual National Assessments (ANA) data for 2012.

The national Department of Basic Education administered the ANA standardised assessments in grades 1–6 and 9 in all public ordinary schools in 2012. All children wrote a mathematics test and a language test. For grades 1, 2 and 3 the language test was administered in the language that the school taught as the first language. In grades 4, 5 and 6 English- and Afrikaans-speaking pupils wrote English or Afrikaans on the first language level, while pupils with a different first language wrote a test for English as a "first additional language" or Afrikaans as a "first additional language".[11] Consequently, the majority of children in poor, majority black schools wrote English as a first additional language in grades 4, 5 and 6. The tests contained a combination of multiple choice items, open-ended items to provide short written answers and longer open-ended questions requiring a creative response. The grade 4 test was out of 20 marks, the grade 5 test was out of 30 marks and the grade 6 test was out of 40 marks. The ANA dataset also includes several individual characteristics, such as gender, age and population group.[12]

The ASS is conducted on the first Tuesday of March every year. It is completed by each school's principal and contains extensive administrative information about the numbers of children enrolled in each grade and about the teachers in each grade. The principal also indicates which language is used as the language of instruction in each grade. It is therefore possible to identify for pupils who were in grade 4 in 2012, what the language of instruction was when they were in grade 1 (in 2009) and in grade 2 (in 2010) and in grade 3 (in 2011). The same historical reconstruction applies to those tested in grade 5 in 2012 (using ASS data from 2008, 2009 and 2010) and those in grade 6 in 2012 (using ASS data from 2007, 2008 and 2009). This assumes that children had not repeated any grades prior to being observed in grade 4, 5 or 6 and had

[8] From instruction in their home language, Russian, to what is essentially bilingual instruction (60% instruction in Latvian and 40% instruction in Russian).

[9] However, the paper by Ivlevs and King (2014) did not examine the performance in language subjects.

[10] For a more detailed discussion of these studies, the interested reader is referred to the working paper version of this article, available at www.ekon.sun.ac.za/wpapers/2013/wp212013.

[11] More information about the Annual National Assessments is available online in the official ANA Report of 2012 available at http://www.education.gov.za/LinkClick.aspx?fileticket=tcz0a0RHVhs%3d&tabid=358&mid=1325.

[12] South African education researchers and policy-makers remain interested in analysis by population group because this characteristic still serves as a proxy for language dynamics, historical disadvantage under the apartheid era and current poverty. Moreover, schools were formerly segregated on the basis of race and administrated by separate education departments. Consequently, institutional and managerial weaknesses persist in the historically black section of the school system (Van der Berg, 2008).

not changed schools over the period - assumptions that are obviously not true for all children. This introduces random noise into the statistical models due to measurement error. In the school fixed-effects model this can be expected to cause a degree of attenuation bias (Angrist & Pischke, 2009), which would shrink the size of our estimates toward zero.

We therefore derive a "treatment" variable for the number of years of English instruction in the Foundation Phase, which varies from zero to three. If the treatment variable takes on a zero value, it therefore signifies the fact that these children were never exposed to English as language of instruction. Similarly values of one, two and three signify the number of years for which children were exposed to English instruction (three would mean that these children were instructed in English for the entire duration of the Foundation Phase, i.e. grades 1–3). For each pupil in grades 4, 5 and 6 in 2012 we thus impute a value on this variable.[13] For those years in which the language of instruction was not English it was one of the "African" languages. For most pupils this would have been their first language, though for some pupils this also would not have been their first language. For the identification of the students in our sample, we ignored all cohorts that received instruction in two languages during one or more years of their Foundation Phase.[14] The proportion of the sample excluded for this reason is small: 6.8% of grade 4, 5.26% of grade 5 and 7.51% of grade 6 children in the sample.

We restrict the sample to pupils in public schools, schools that wrote the English first additional language paper in 2012 (thus excluding schools with predominantly Afrikaans-speaking and English-speaking children as well as the few schools that opted to write the Afrikaans first additional language paper), schools in which at least 80% of children are black, and schools officially categorised as being in the bottom three wealth quintiles (quintiles 1, 2 and 3).[15] The resulting sample of 827,745 individuals in 9180 primary schools represents the population of schools in which the challenge of English acquisition as a second language is applicable. It is also the section of the South African school system that is seriously underperforming. Children in these schools typically find themselves in a poverty trap where low quality education outcomes lead to weak labour market prospects (Van der Berg et al., 2011). We also restrict out analysis to estimating the impact of the language of instruction on English proficiency. We therefore conduct our analysis only on the English test scores.[16]

Table 1 reports selected descriptive statistics for all pupils by treatment intensity. More than 80% of the sample received instruction in an African Language for all three years of the Foundation Phase. Schools in which three years of first language instruction were provided were smaller schools on average.[17] The sample almost exclusively consists of black children. Interestingly, those schools with three years of first language instruction were more

Table 1
Descriptive statistics by treatment intensity.

Years of English	0	1	2	3
	Mean (standard deviation)			
Total school enrolment	357.32	415.19	393.11	453.38
	(253.98)	(283.01)	(282.42)	(279.71)
Percentage black	99.80	99.79	99.75	98.95
	(4.46)	(4.54)	(5.00)	(10.18)
Class size	38.94	43.80	43.85	43.76
	(14.98)	(14.74)	(13.83)	(12.15)
Percentage quintile 1	39.34	38.63	36.15	24.71
	(48.85)	(48.69)	(38.04)	(43.13)
Fees (% > R100)	2.10	2.70	5.85	18.94
	(14.34)	(16.20)	(23.48)	(39.18)
Percentage of sample	85.77	7.39	3.33	3.51
Number of children	709,985	61,113	27,574	29,053

Source: ASS data (2007–2011) and ANA data (2012).
Notes: Descriptive statistics for estimation sample by treatment intensity.

[13] It should be noted that our definition of treatment is simply based on the response of school principals to the question as to what language is used as the medium of instruction in each grade. Therefore, within each response category schools would no doubt vary substantially in how the language of instruction was used and in characteristics such as the availability of materials, the quality of such materials, the extent of code switching and the language proficiencies of teachers. However, it is worth noting several aspects of the context in order to get a better sense of the treatment variable under consideration. First, the National Curriculum Statement of 2002 (relevant to the time period under analysis) specified that the school day is 7 h long and that, in grades 1–3, 40% of time be allocated to languages, 35% to numeracy and 25% to Life Skills. Thus, teachers could decide what proportion of the 40% to spend on Home Language or an Additional Language, but 60% of time would be taught in the chosen language of instruction, which is the key variable of interest in this paper. In grades 1–3 classes are almost always taught by a single teacher. Second, The NEEDU Report (NEEDU, 2012) pointed to a shortage of home language reading materials. Therefore, if any difference in materials across treatment status, there would be more materials available to support English instruction. However, it is likely that in both situations classrooms would be constrained by a lack of reading materials. Teachers are not paid any differently depending on the language of instruction that is used. Third, regarding the language proficiency of teachers, the ASS includes a question to teachers about their confidence in teaching English. However, the meaningfulness of this self-reported question is uncertain, especially in view of the high levels of reported confidence observed. The question is on a 4 point scale with 4 being "very confident". About 5% of teachers gave themselves a 1 or a 2. About 54% of teachers gave themselves a 3 (confident) and about 41% of teachers gave themselves a 4 (very confident). Interestingly, there was no significant difference between non-switching schools, schools that switched towards English and schools that switched away from English in terms of their average self-reported teacher confidence.

[14] The ASS questionnaire includes a question to school principals regarding the percentage of students in each grade who received instruction in English. If this percentage was not indicated as being either 0 or 100, we excluded the cohort from our estimation sample since it is not certain what the treatment is in a situation where children were instructed in a double medium (the terminology used in South Africa to describe schools where more than one language of instruction was used) school.

[15] Schools are categorised according to a measure of the poverty in the surrounding community. This is used to inform pro-poor public expenditure on non-personnel education spending. There are five poverty quintiles of schools, although these are not equal in size due to substantial reclassification of schools over time, mainly towards the lower quintiles. Quintiles 4 and 5 schools include historically advantaged schools, which serve many children whose first language is English or Afrikaans.

[16] The analysis on the mathematics test scores has been included in the working paper version available at http://www.ekon.sun.ac.za/wpapers/2013/wp212013.

[17] This probably reflects the pattern that remote rural schools usually opt for mother tongue instruction and are often small.

Table 2
Descriptive statistics by switching and non-switching schools.

	Switch to English	Switch from English	No switch
	Mean (standard deviation)		
Language homogeneity	0.83	0.91	0.91
	(0.22)	(0.19)	(0.18)
Total school enrolment	470.62	377.62	355.19
	(305.25)	(282.47)	(248.37)
Class size	43.71	42.93	38.75
	(43.71)	(14.81)	(15.02)
School fees (% > R100)	3.54	3.51	2.69
	(18.49)	(18.40)	(16.18)
Number of schools	545 (5.94%)	1311 (14.28%)	7324 (79.78%)

Source: ASS data (2007–2011) and ANA data (2012).
Notes: Descriptive statistics for estimation sample by language switch.

Table 3
Selected descriptive characteristics for switching schools before and after the switch.

Description	Sub-sample	Pre-switch		Post-switch	
		Mean	Standard error	Mean	Standard error
Proportion black enrolment	All switchers	0.996	0.000	0.997	0.000
	To English	0.996	0.000	0.998	0.000
	From English	0.997	0.000	0.998	0.0010
Total enrolment	All switchers	314.206	0.368	302.671	0.364
	To English	342.088	0.710	343.168	0.722
	From English	272.281	0.477	253.713	0.463
Class size	All switchers	45.547	0.029	40.235	0.031
	To English	44.218	0.046	41.864	0.045
	From English	47.480	0.046	39.637	0.040

Source: ASS data (2007–2011) and ANA data (2012).
Notes: Descriptive statistics for estimation sample before and after the language switch.

likely to be quintile 1 (i.e. poorest) schools and less likely to charge fees in excess of R100.[18] This already indicates why it is not valid to simply compare educational performance across schools with differing language policies.

Analysis of ASS data indicates that a considerable number of schools changed the language of instruction in at least one of the Foundation Phase grades between 2007 and 2011. Table 2 reports that 79.8% of children were in schools that experienced no change in policy during the period. However, 5.9% of children were in schools that switched towards English as language of instruction and 14.3% of children were in schools that switched from English to an African language during the period. There were some differences between the schools that switched in either direction and the non-switching schools, particularly with respect to school size. However, these are clearly not substantially different groups of schools along the dimensions that are known to be educationally important in South Africa. Interestingly, the variable describing the language homogeneity of the school (defined as the proportion of pupils that speak the most common home language in the school) was somewhat lower in schools that switched to English as language of instruction. This may indicate that language homogeneity of a classroom is one factor taken into consideration by schools when deciding on the language of instruction.

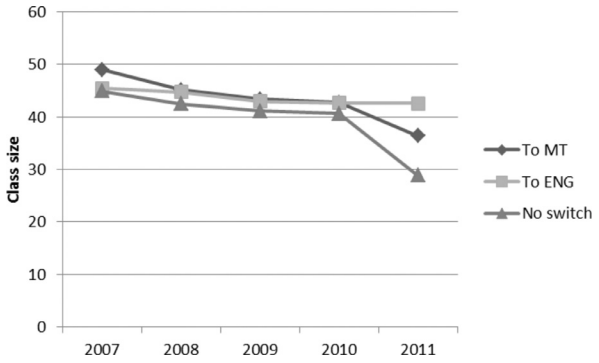

Fig. 1. Trends in class size (grades 1–3) for switching schools and non-switching schools.
Source: ASS data (2007–2011).

Table 3 shows selected descriptive statistics for schools before and after a switch in the language policy occurred. The table shows this information separately for schools that switched to English and schools that switched away from English, in case a switch in one direction was linked with other changes in school characteristics. Only in the case of class size was there a statistically significant decline in the class size within schools that switched to an African language.

Fig. 1, however, confirms that a decline in class size was consistent with the trend amongst the large group of non-switching schools. Instead, the fact that schools switching to English retained similar average class size is the divergent trend. One possible explanation for this is that parents may have been attracted to schools that switched to English due to the perception that English is the language of upward mobility.[19]

Fig. 2 confirms that the trends in class size were largely driven by slight changes in enrolment patterns rather than by teacher recruitment. Schools switching to English experienced a stable average level of enrolments over the period, while the other two groups of schools saw slight declines in total enrolment.

Average school fees, though somewhat higher amongst non-switching schools, declined in all three groups over the period. It should be noted here that the high school fees in 2010 for schools that switched to first language is driven primarily by 4 schools which indicated their school fees to be higher than R1 500.[20] Once these outliers are removed, the trend for schools that switched to first language is similar to the other two groups. The main point

[18] Most of these schools are "no-fee schools" and receive a higher government subsidy as compensation for not charging fees.

[19] We have some concern about the quality of the class size data for 2011, since the drop in average class size appears suspiciously large.

[20] These outliers report school fees of R1500 or less for 2011, which leads us to suspect that the 2010 data entry might have been a mistake.

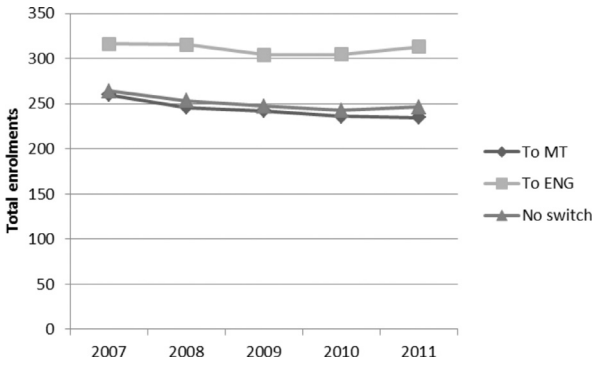

Fig. 2. Trends in total enrolments (grades 1–3) for switching schools and non-switching schools.
Source: ASS data (2007–2011).

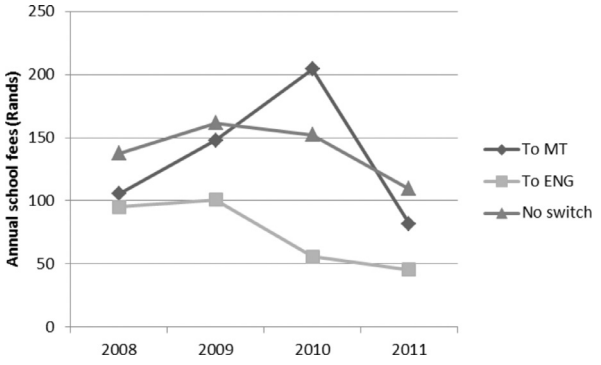

Fig. 3. Trends in school fees for switching schools and non-switching schools
Source: ASS data (2007–2011).

from Figs. 1–3 is that these time trends were fairly consistent across the three groups.

4. Identification strategy

We first estimate the baseline effects of exposure to English as language of instruction by adopting a simple OLS estimation strategy using the following model:

$$Y_{igs} = \alpha + G_g + \beta\, T_{igs} + \delta' X_i + \tau' Z_s + \nu_s + \varepsilon_{igs} \quad (1)$$

where Y_{igs} is the standardised English test score of child i in grade g in school s. These standardised test scores are from the ANA tests written by grades 4, 5 and 6 children in 2012. Since our model includes children in grades 4, 5 and 6, we include grade fixed effects G_g to control for the overall differences in performance between grades. The impact of exposure to English instruction is estimated by including T_{igs}, an indicator variable equal to the number of years for which the child received instruction in English during their foundation phase (i.e. when they were attending grades 1, 2 and 3). This variable takes on the values 0, 1, 2 or 3. For children who were never instructed in English (because the switch in language from mother tongue to English took place after they had completed grades 1–3 or because their school never switched from mother tongue to English), the treatment variable would be coded as $T = 0$. On the other hand, for children who were instructed in English throughout their first three years of school (either because the switch to English occurred before they entered school or because their school never switched from being a Straight-for-English school), the treatment variable would be coded as $T = 3$. In the model, we also control for individual child characteristics by the inclusion of $\boldsymbol{X_i}$, a vector of individual child covariates, while $\boldsymbol{Z_s}$ is a vector of school characteristics. The error term can be thought of as consisting of two components: that driven by unobserved school characteristics (represented by ν_s) and secondly the remaining random error component (represented by ε_{igs}).

The estimation of these effects using simple OLS however has the distinct disadvantage that it does not control for school quality (and other school-level) unobservables ν_s that might be correlated with both the decision of schools to adopt a specific language policy T_{igs} as well as the outcomes of the children in the school Y_{igs}. Not controlling for these unobserved school-level characteristics will bias the estimates of the exposure to English, given the large variations in the quality of schools within South Africa as well as the fact that we observe certain quality differences being correlated with the language choices that schools make.[21] In principle, we would suspect this selection bias to result in an over-estimation of the impact of English language exposure. More specifically, if we believe the anecdotal evidence referred to earlier as well as the descriptive statistics set out in the previous section, we would suspect school quality and exposure to English as language of instruction to be positively correlated. Omitting to control for school quality would therefore lead to a positive bias in the OLS estimates.

In order to control for (unobserved) school quality and eliminate this type of selection bias, we also adopt a school fixed effects approach. Including school fixed effects in the regression controls for the quality differences in South African schools and ensures that our estimates are not suffering from selection bias inherent in the South African school system. The variation in our variable of interest, T, now comes from the differences in the exposure to English as language of instruction present within each of the schools. Since this variation arises because some schools selected to change their language policy during the period 2007–2011, the estimation of β comes only from schools where there was indeed a switch in the language of instruction during the period 2007–2011.[22]

Although the time invariant unobserved school-level characteristics are now controlled for by including the school fixed effects, there might still be time varying school characteristics which may bias our estimates. One would expect these to be attributes of the school that

[21] As set out in the previous section, schools that switched to English instruction are typically richer (in a higher quintile) and more likely to charge higher school fees. In addition, these are the schools where class sizes typically increased over the period. The descriptive empirical evidence therefore seems to bear out the perception present within the South African school system that schools that teach in English are better quality schools.

[22] In other words the years in which all the children in our sample who are now in grade 4, 5 and 6 moved through grades 1, 2 and 3.

Table 4
Naïve OLS and fixed effects regressions.

	Naïve OLS regression	Fixed effects
1 year of English	0.065***	−0.063***
	(0.019)	(0.018)
2 years of English	0.148***	−0.081***
	(0.031)	(0.030)
3 years of English	0.346***	−0.170***
	(0.045)	(0.045)
School fixed effects	N	Y
Other controls	Y	Y
N	827,745	827,745
Number of clusters	9180	9180
R-squared (within)	0.054	0.062

Source: ASS data (2007–2011) and ANA data (2012).
Notes: OLS and fixed effects regression. Coefficients with standard errors (clustered at school level). Controls include child's race, gender, whether over-aged, young or correct age for grade, current grade, number of years exposed to English as language of instruction and school wealth quintile. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

change over time and are correlated with both the performance of the children as well as the decision to adopt a certain language policy. These attributes would most likely be school quality characteristics such as changes in management. To limit the potentially confounding influence of these covariates, we include a vector of class-level characteristics (varying over time within the school).

After interpreting the results from the pooled fixed effects model, we move on to investigate the heterogeneity in the results across different geographic areas as well as over time. The last part of our empirical strategy is to try and ascertain whether our results can be interpreted as being causal or not by conducting several robustness checks. These will be discussed in further detail in Section 6. In this section, our aim is to try and separate out the impact of the change in the language of instruction from other confounding changes that might be correlated with a change in the language of instruction, but are also signals of the changes in school quality.

5. Results

As set out in Section 4, the first model we estimate is the OLS model set out in Eq. (1). The results from this regression are reported in the second column of Table 4. These results seem to confirm our initial suspicion that, without adequately controlling for school quality, additional years of exposure to English as language of instruction seem to have a positive and significant effect on the performance of children.

The coefficients on the treatment variable seem to indicate that children who are exposed to one year of English instruction in their foundation phase (grades 1–3) on average score 0.07 of a standard deviation higher in the English test than children who were exposed to no years of English as instruction language. Similarly, children who were exposed to two years of English as language of instruction during their foundation phase scored on average 0.15 of a standard deviation higher in the English test than children who were exposed to no years of English as instruction language. Last, those children who were instructed in English for the entire duration of their first three grades, scored almost 0.35 of a standard deviation higher in the English test than their counterparts who were not exposed to any English during their first three years of school. These effect sizes are not small, and should be viewed in light of the literature on the impact of education interventions (Hill, Bloom, Black, & Lipsey, 2008).[23]

We introduce school fixed effects in our next regression, in which we specify the model in Eq. (1), but replace the vector of school-level covariates Z_s with school fixed effects. The results from this regression are reported in the third column of Table 4. The introduction of school-level fixed effects has a large impact on the size and sign of the coefficients on the number of years a child was exposed to English instruction. In fact, we see a reversal of the positive impact of English instruction. Table 4 indicates that, after controlling for individual and school-level characteristics, children who were exposed to three years of English instruction scored on average 0.17 of a standard deviation lower in the English test than children who received mother tongue instruction in grades 1, 2 and 3.

To understand the reason for this large reversal in sign, it is useful to think of the introduction of the school fixed effects as controlling for variables erroneously omitted from the baseline OLS estimates. Given the evidence introduced in Section 3, it is reasonable to assume that there is a positive correlation between school quality and English instruction. Without sufficient controls for school quality, we would therefore expect the coefficients on the indicator variable for English instruction to be over-estimated. Once controls for school quality (in the form of school fixed-effects) are included, the coefficients should therefore be lower than previously in order to correct the bias. This change in sign is in line with findings by Angrist et al. (2008) for Puerto Rica, where English as language of instruction was shown to be positively correlated with English acquisition, but showed no correlation once cohort-specific controls were included in the estimation.

It is possible that these results may be as a result of the fact that better educated and more motivated parents send their children to schools which switch to mother tongue instruction (or that better performing children are sent to these schools). A related concern would be that higher quality teachers are drawn to the schools which switch to mother tongue instruction. However, we do not have longitudinal student or teacher data so are not able to follow the same students and teachers as they switch between schools over time. If parents were to send their children to a school or teachers switched schools as a result of the change in the language policy of the school, we do however know from the literature (Msila, 2009) that it would rather have been to schools switching to English, and not schools switching to the mother tongue of the majority of the children. This is because of the perception by parents that schools who teach their children to speak English well are better quality schools (something which is also

[23] One year of learning is often equated to 0.4 or 0.5 of a standard deviation.

Table 5
Heterogeneous effects – urban and rural estimates.

	Urban sub-sample	Rural sub-sample
1 year of English	−0.107***	−0.054**
	(0.036)	(0.021)
2 years of English	−0.036	−0.139***
	(0.056)	(0.035)
3 years of English	−0.160**	−0.175***
	(0.074)	(0.059)
School fixed effects	Y	Y
Other controls	Y	Y
N	210,332	462,774
Number of clusters	1794	6029
R-squared (within)	0.068	0.059

Source: ASS data (2007–2011) and ANA data (2012).
Notes: Fixed effects regression. Coefficients with standard errors (clustered at school level). Controls include child's race, gender, whether over-aged, young or correct age for grade, current grade and number of years exposed to English as language of instruction. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

borne out by the descriptive statistics in Table 1 – schools who always instructed in English are typically in higher income quintiles). Any such changes in unobserved child and teacher characteristics might therefore be expected to bias our results in favour of English instruction (since this would imply that schools switching to mother tongue instruction would attract less motivated children and weaker teachers). Put differently, the finding that mother tongue instruction produced better results than English instruction is not likely to be a false conclusion driven by bias in unobservable characteristics.

It is interesting to break down the results further in order to try and get a sense of whether the results are heterogeneous across different school types. One way in which to do this is to look at the differences in the results for urban and rural schools. Since there is anecdotal evidence that rural schools are the schools where teachers are less likely to be proficient in English and have the ability to teach in English, we would expect that the coefficients for these schools would be larger, pointing to one of the channels through which the exposure to a language impacts the performance of these children. However, as set out in Table 5, the results from running the main fixed effects specification on these two sub-samples seem to indicate no difference between the urban and rural schools. This provides us with some comfort that our results are not driven by the differences in the quality of teachers, but rather provide some indication of the impact of language *per se*.[24]

We also explore the possibility of parameter heterogeneity according to the language homogeneity of the school. So far we have not distinguished between non-English schools where the language of instruction is not the home language of the majority of children *versus* schools where the language of instruction is also the home language of the majority of children in the school. We would expect the effect of home language instruction to be larger in schools where the language of instruction is the same as the home language of the majority of children. In order to test this hypothesis, we first re-run the main fixed effect regression only on the sub-sample of schools where the home language spoken by all the children in the school is the same as the language of instruction (and the language of instruction was not English). The coefficients (not reported here for the sake of brevity) are slightly larger for this sub-sample, providing some evidence in support of the hypothesis that there is an added benefit to being taught in one's own home language over and above a language which is closely related to, but not exactly the same as the home language. We also repeated the main fixed effect regression on the sub-sample of schools where only 80% or less of the children spoke the same language as the language of instruction. The coefficients from this regression are smaller, and statistically insignificant. However, the sample size is also significantly smaller (only approximately 1380 schools). We are therefore wary of making any conclusions regarding parameter heterogeneity, and interpret these results as tentative evidence that the benefit of mother tongue instruction is limited to contexts where the strong majority of children speak that same home language.

6. Robustness checks

In the previous section, we dealt with the most obvious source of endogeneity biasing the results from the baseline OLS regression, namely the unobserved time-invariant school effects that are correlated both with school quality as well as the choice of language of instruction. However, we have not yet considered other factors that might be confounding the results.

One alternative explanation for our results is that we are not in fact estimating the impact of language of instruction but simply the impact of a policy switch. For example, changing the school's language policy could have a positive impact in itself if motivated individuals (teachers or parents, for example) tend to initiate a change in policy and also produce better learning outcomes. Conversely, a change in the language of instruction could have a negative disruptive impact. If this were the case, one would expect opposite signs on the treatment coefficient amongst schools that switched to English and schools that switched to Mother Tongue.[25] Table 6 shows that a negative

[24] There is a significant decrease in the number of children in the sample once we control for the geographic location of the school, since information on this is missing for approximately 29% of the children in the sample. However, for purposes of comparison, and to confirm that selection bias as a result of attrition is not driving these results, we have repeated the main fixed effects regression only on the sub-sample of children for whom we have data on the geographic location of the school, and the results are almost identical (if not stronger) for this sub-sample.

[25] More specifically, if the results were for example driven primarily by the positive input from teachers or parents (associated with a change in language policy), then we would expect to see evidence of home language instruction being beneficial in schools switching to home language and evidence of English instruction being beneficial for schools switching to English. Similarly, if a switch in language is inherently disruptive this would have caused the negative effects in the sub-sample of schools switching to English, but then we would also expect to observe a positive effect of English instruction for the schools in the sub-sample which switched to home language.

Table 6
Heterogeneous effects – two sub-samples of switching schools.

	To English	To MT	Prob>χ^2 for test of parameter equality
1 year of English	−0.043	−0.075**	0.400
	(0.029)	(0.023)	
2 years of English	0.028	−0.116**	0.087
	(0.076)	(0.033)	
3 years of English	−0.088	−0.195***	0.352
	(0.102)	(0.052)	
School fixed effects	Y	Y	
Other controls	Y	Y	
N	714,348	771,082	
Number of clusters	7869	8635	
R-squared (within)	0.062	0.061	

Source: ASS data (2007–2011) and ANA data (2012).
Notes: Fixed effects regression. Coefficients with standard errors (clustered at school level). Controls include child's race, gender, whether over-aged, young or correct age for grade, current grade and number of years exposed to English as language of instruction. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

coefficient is obtained within both sub-samples of schools. We report the results from a test for parameter equality in which the coefficients on the three treatment variables were compared across samples (the "to English" sample was compared to the "to mother tongue" sample). For none of the treatment variables are we able to reject the null hypothesis that the coefficients are equal. It therefore appears that the impact is symmetric across these two samples. Although the coefficient amongst schools switching to English is not statistically significant, there is clearly no evidence that switching in itself always leads to a positive or negative impact on learning.

Apart from the effect of switching, we have also not yet explored the possibility of time-varying school quality indicators which are correlated with the decision of the school to switch its language policy during the period 2007–2011. The difference between the OLS estimates and the fixed effects estimates indicates the need for including controls for unobserved school quality in regressions such as these, especially in situations where there are large differences which are often unobserved between schools. However, the fixed effect approach is limited in that it can only account for time-invariant unobserved differences between schools. If, however, there were changes over time which were correlated with the school's decision to change its language policy biasing the estimates, the inclusion of fixed effects would not account for this type of bias.[26] The purpose of this section is to expand the baseline fixed effects strategy so as to specifically check the robustness of these estimates when allowing for changes in school quality over time being correlated with the change in the language policy of the school. We conduct three types of robustness checks in this regard, which are set out below.

[26] Another way to look at this is to think of this type of bias as resulting in changes between grades within a single school.

6.1. Changes in school quality over time

The first set of robustness checks we perform are aimed at establishing whether some of the observed measures of school quality available in the data are correlated with the variable of interest. The rationale behind this approach is to ascertain whether there were changes in these observed quality measures (or other measures which would also influence the performance of children) over time in such a way that could bias the results. In other words, if the decision to change the language of instruction in grades 1, 2 and 3 was taken as a result of the change relating to a specific grade from one year to the next (in terms of, for example, class size or the language homogeneity of the children in the class), then what we would be observing in the regressions in the previous section would be caused by changes other than the change in the language of teaching. In addition, these observed changes in the school could also be used as proxies for unobserved changes in school quality. If there is a statistically significant correlation between these observed changes and the number of years of exposure to English as language of instruction, then we would suspect that there might also be unobserved changes in school quality which we are not controlling for and which may be driving the results.

The ASS includes some measures at the level of the class (in other words, time variant grade-level variables) that may be used as measures of school quality. These are class sizes, a measure of language homogeneity within the classroom and the fees charged by the school, as described in Section 3. We regress these measures on the full set of child-level controls as well as the measure of exposure to English instruction in a fixed effects regression after splitting the sample into schools that switched to and from English instruction. The results from these regressions are set out in Table 7.

There seems to be no significant systematic relationship between any of these measures of quality and years of English language exposure.[27] This provides us with some assurance that there are no other changes in quality that are correlated with the decision to change the language policy of the school. However, in order to confirm the robustness of the results, we also include these controls in our main fixed effects specification and report on these results in Table 8. It is reassuring to note that the inclusion of these controls does not significantly change any of the results. We again report the p-values for the test of parameter equivalence, where we compared the coefficients in the third column of Table 4 with those in Table 8. We are unable to reject the null hypothesis that the coefficients are equal to those estimated in the main fixed effects regression.

[27] One exception is the relationship between language heterogeneity and language of instruction, as this there seems to be a significant relationship between the decision to switch to English as language of instruction and the language homogeneity of the class for schools that switched to English. This is intuitive since schools with more heterogeneous classes would probably be more likely to switch to English since there is no single dominant home language within the class.

Table 7
School fixed effects models predicting potential indicators of endogeneity (including grade, race, age, gender controls).

Outcome Variable	To HL schools			To English schools		
	Class size	Language homogeneity	Log(school fees)	Class size	Language homogeneity	Log(school fees)
1 year of English	0.110	0.000	−0.218**	0.767	−0.016	−0.121
	(0.693)	(0.008)	(0.101)	(1.154)	(0.014)	(0.214)
2 years of English	0.446	0.001	−0.188	1.253	−0.021	−0.048
	(1.101)	(0.014)	(0.143)	(1.956)	(0.023)	(0.400)
3 years of English	−1.148	0.006	−0.137	2.904	−0.057*	−0.349
	(1.456)	(0.020)	(0.221)	(2.864)	(0.034)	(0.538)
School fixed effects	Y	Y	Y	Y	Y	Y
Other controls	Y	Y	Y	Y	Y	Y
N	89,484	107,479	113,397	42,844	54,991	56,663
Number of clusters	1105	1297	1311	406	538	545
R-squared (within)	0.159	0.002	0.210	0.017	0.014	0.130

Source: ASS data (2007–2011) and ANA data (2012).
Notes: Fixed effects regression. Coefficients with standard errors (clustered at school level). Controls include child's race, gender, whether over-aged, young or correct age for grade, current grade and number of years exposed to English as language of instruction. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Table 8
Fixed effects results with class-level covariates.

		Prob> χ^2 for test of parameter equality (compared to fixed effects results in Table 4)
1 year of English	−0.068***	0.678
	(0.023)	
2 years of English	−0.092**	0.591
	(0.037)	
3 years of English	−0.190***	0.844
	(0.053)	
School fixed effects	Y	
Other controls	Y	
N	529,057	
Number of clusters	6849	
R-squared (within)	0.071	

Source: ASS data (2007–2011) and ANA data (2012).
Notes: Fixed effects regression. Coefficients with standard errors (clustered at school level). Controls include child's race, gender, whether over-aged, young or correct age for grade, current grade, number of years exposed to English as language of instruction, class size, log of school fees and language homogeneity (proportion of children speaking most common home language in the school). *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

Related to the above, it may also be argued that the fixed effects approach relies on the assumption of a common trend in test scores between switching and non-switching schools prior to the switch in language.[28] Since we only consider the 2012 ANA test scores data here, we are unable to consider the trend in test scores over time to assess the validity of this assumption. However, one way in which to check the robustness of an assumption of common trends, is to consider only children who were not exposed to any switch in language (however attended a school which did switch), and introduce a placebo "treatment" variable for one of the grades. We do this by "moving back" the switch in language of instruction by one year, and then comparing two grades of children. In actual fact, neither of the cohorts was affected by the language switch, but we introduce a placebo treatment effect to the younger cohort, as if the younger cohort was affected by the switch in language.[29] Re-estimating the main fixed effects specification on all children who were not exposed to any years of English instruction, we find that the placebo dummy is small and statistically insignificant,[30] which provides some support for the fact that we are not just picking up differential trends in test scores.

However, even if there were unobserved heterogeneous time trends, Table 8 provides convincing evidence of the fact that these will at most lead to an under-estimation of the true impact of exposure to home language as the language of instruction. The coefficient on 3 years of exposure to English is marginally higher (although not significantly different) in the output in Table 8 than in the main fixed-effects model (reported in Table 4). Thus, if controlling for observable characteristics across time (grade) indicates a slight downward bias in the coefficient then one would also expect unobserved characteristics to affect the bias in the same direction, if at all. At worst, therefore, the estimates from the main fixed effects regression would be biased downwards and would be an under-estimation of the true impact.

6.2. Changes in principals

Next, we explore the possibility that there might have been a change in the management of schools during the period under investigation which is correlated with the

[28] Since there is variation between schools – switching and non-switching – as well as between grades, the fixed effects approach amounts to an approach which is similar to a difference-in-difference approach between schools and grades within schools.

[29] We constructed this placebo variable for children who were observed in schools where the switch in language only occurred in 2011, so that in 2012 the grades 5 and 6 children would have been unaffected by the change in language of instruction. We created a placebo treatment dummy variable equal to one for children who were in grade 5 in 2012 (therefore in grade 4 in 2011) and zero for children who were in grade 6 in 2012 (therefore in grade 5 in 2011), therefore essentially "moving" the switch in the language one year back.

[30] These results are not reported here. The coefficient on the placebo dummy is −0.045, with a standard error of 0.035.

Table 9
Likelihood of switching language of instruction by principal changes.

	Change in principal	No change in principal	Total
Switched to English	7.64%	6.06%	6.85%
Switched away from English	13.94%	13.64%	13.70%
No switch	78.42%	80.48%	79.46%
Total	100%	100%	100%

Source: ASS data (2007–2011).

change in the language of instruction. Since principals are part of the school governing bodies and usually take the lead in decisions taken by the school governing body,[31] we would expect a change in principal to also affect the decisions taken by the school governing body. In addition to changing the language policy, a new school principal may be a result of or the cause of other changes in school quality.[32]

We accordingly check the robustness of the results against a change in principal in the school during the period 2007-2011. Since the ASS data do not contain detail on whether the same principal remained at the school or not, we have to make use of the mobile number of the principal in order to track him or her over the period. If the mobile number provided by the principal of a school changed over the period we cannot be sure whether the principal changed or whether the same principal simply changed his or her number. However, if the mobile number of the principal did not change over the period we can fairly safely assume that there was no change in the school principal. If our main results hold within the sub-sample of schools where the principal definitely did not change then we can rule out the possibility that our results are merely driven by changes in the school leadership.

Looking at the sub-sample of schools where there may have been a change in principal, as set out in Table 9, it would appear that these schools were somewhat more likely to change their language of instruction than if there was no change in the principal during the time period. However, when we repeat the main fixed effects regression using the two sub-samples, we find that the results hold in both sub-samples, as set out in Table 10. [33] In fact, the treatment effect is slightly larger amongst schools where there was no change in the principal, thus suggesting that our estimated impact of language of instruction is not being driven by simultaneous changes in the quality of school management.

[31] Especially in the schools in our sample, as the parents at these schools are usually not highly educated themselves and often lack the confidence to participate in the decisions taken by the school.

[32] The newly appointed principal might implement other quality enhancing policies alongside the change in the language of instruction.

[33] Testing for parameter equivalence between the coefficients on the sample where principals had changed versus the sample where there was no change, we are unable to reject the null hypothesis that the coefficients are equal across these two sub-samples at the 95% confidence level for the regressions on the English test scores.

Table 10
Main treatment effects for schools with a possible principal change and schools with no principal change.

	No change in principal	Change in principal	Prob> χ^2 for test of parameter equality
1 year of English	−0.054**	−0.072***	0.606
	(0.026)	(0.024)	
2 years of English	−0.136***	−0.035	0.085
	(0.038)	(0.044)	
3 years of English	−0.197***	−0.149**	0.591
	(0.066)	(0.061)	
School fixed effects	Y	Y	
Other controls	Y	Y	
N	415,175	412,570	
Number of clusters	4583	4597	
R-squared (within)	0.062	0.062	

Source: ASS data (2007–2011) and ANA data (2012).
Notes: Fixed effects regression. Coefficients with standard errors (clustered at school level). Controls include child's race, gender, whether over-aged, young or correct age for grade, current grade and number of years exposed to English as language of instruction. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

6.3. Changes in provincial language policy directives

Last, we investigate whether the change in language policy was caused by some provincial level directive which might have been part of a provincial level improvement in administrative support. For this we decompose the proportion of grade 1 children in the sample who attend a school where English is the language of instruction by province. Fig. 4 illustrates how this proportion has changed per province over the period of interest between 2004 and 2011. Some provinces had clear shifts toward English (such as the Free State province), while others (such as the Eastern Cape and Gauteng) had clear shifts toward home language instruction.

It would therefore seem as if, at least in certain provinces at certain times, there may have been a province-wide emphasis towards a particular language policy. If the emphasis on a particular language policy was part of a broader change in the quality of administrative support provided by the provincial departments, or if the emphasis on a language policy was accompanied by additional learning materials or teacher training, it is possible that the results from the baseline estimates are merely reflecting a broader change in quality rather than the language policy of the school.

In order to test this hypothesis, we repeat the fixed effects regression on three sub-samples. First, we estimate the impact on the sub-sample of schools who complied with the dominant trend in their specific province over the period, first for schools switching to English and then for schools switching to Mother Tongue.[34] We then repeat the estimations on the sub-sample of schools that defied the general trend of the province in that they switched in

[34] In other words, if the dominant trend of the province was to switch to English, this group of compliers would include only schools that actually switched from home language to English during the period under consideration and vice versa for home language.

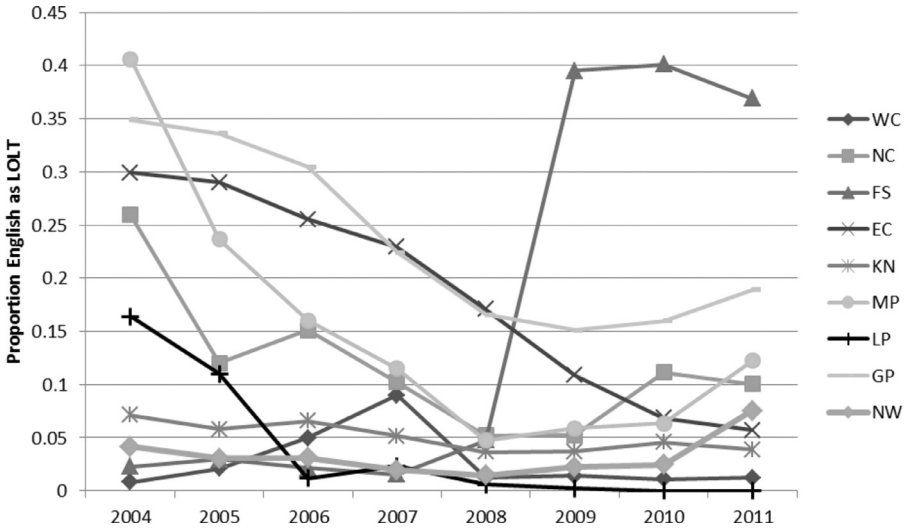

Fig. 4. Proportion of grade 1 children (in population) with English as language of learning.
Source: ASS data (2007–2011).

Table 11
Compliance.

	Non-compliers	Compliers to HL	Compliers to English
1 year of English	−0.013	−0.092**	0.076
	(0.024)	(0.039)	(0.092)
2 years of English	0.080	−0.138**	0.019
	(0.051)	(0.059)	(0.158)
3 years of English	0.074	−0.228***	−0.044
	(0.076)	(0.083)	(0.237)
School fixed effects	Y	Y	Y
Other controls	Y	Y	Y
N	734,382	66,784	26,579
Number of clusters	8031	903	246
R-squared (within)	0.061	0.049	0.085

Source: ASS data (2007–2011) and ANA data (2012).
Notes: Fixed effects regression. Coefficients with standard errors (clustered at school level). Controls include child's race, gender, whether overaged, young or correct age for grade, current grade and number of years exposed to English as language of instruction. *Significant at the 10% level. **Significant at the 5% level. ***Significant at the 1% level.

the opposite direction to the general trend in the province. These results are set out in Table 11.

Here we find a significant advantage for the schools that complied with the general trend of switching to home language, but no such advantage for the schools that switched to English, as the coefficients on the treatment variable for the sample of schools which switched to English compliantly with the provincial trend or for schools that defied the trend by switching in the reverse direction are not significant. This might be because the switch to home language was as a result of a provincial move in that direction which was accompanied by various other initiatives aimed at improving school quality (for example, additional teacher assistance and learning material). However, there are two reasons why this is unlikely to have been the case. In the first place, the largest province in this sample of compliers is the Eastern Cape, in which the quality of provincial education administration is widely thought to have been consistently weak and even deteriorating over the period.[35] Second, in the analysis conducted on the mathematics test scores (which has not been reported here), there is no significant difference between the results for the compliers and non-compliers, which seems to indicate that general quality improvements at the provincial level were not driving the results.

After conducting these robustness checks, we hold that a causal interpretation of our main results is defensible and that the results are not confounded by other changes in school quality.

7. Conclusion

The language in which children are instructed in primary school is an important input into the education production function. In many developing countries, the predominant home languages spoken by the majority of children are not well-developed for academic purposes, leading to the adoption of another language, such as English, Spanish or French as the language of instruction from an early age. This is also the case in South Africa, where most primary schools use home language education for the first three years and switch to English at the beginning of grade 4, while some primary schools use English as the language of teaching and learning, even though the majority of the children in the school do not speak English as home language. Since children with an African home language perform significantly worse than English home language speakers, one of the questions that is frequently raised is to what extent this language policy contributes to the under-performance of these children.

In this paper, we make use of longitudinal administrative data from the Annual Survey of Schools as well as

[35] In response to deteriorating provincial governance, the Eastern Cape provincial education department was placed under the administration of the national Department of Basic Education between March 2011 and April 2013.

test scores in English from standardised tests from the Annual National Assessment. The OLS estimates show that early grade instruction in English is associated with better performance in the English tests in subsequent grades. However, we find that, after controlling for school fixed effects, receiving mother tongue instruction (rather than English instruction) in grades 1, 2 and 3 leads to better English proficiency in grades 4, 5 and 6. This result does not seem to be driven by changes in school quality correlated with the change in a school's language of instruction over this period. This finding is in line with pedagogical theory which promotes the acquisition of a first language before moving on to a second language.

Several things should be borne in mind when interpreting these findings. This research tells us the *average* impact of language of instruction in South African schools *as the policy was implemented between 2007 and 2011*. Advocates of both immersion approaches and mother-tongue instruction envisage a carefully thought-through set of instructional practices implemented by high-quality teachers and supported by sufficient materials. However, we estimate the impact of the alternative models as they were implemented, within specific contexts of schools, teachers and homes. Consequently, there may be important impact heterogeneity that we do not pick up. For example, when accompanied by certain materials or when taught by certain teachers, English instruction from grade 1 may be preferable. Therefore, we maintain that the current language in education policy – to encourage the use of mother tongue instruction in grades 1, 2 and 3 but to allow schools the final decision – is a suitable approach. Our findings cannot, however, tell us what the impact would be of extending mother tongue instruction in grades 4, 5 or 6.

In addition, although our study confirms that the language of instruction is an important contributor to the academic performance of South African children, it is not in our view the main contributor. Factors such as community- and home-level poverty, weak school functionality, weak instructional practices, inadequate teacher subject knowledge, and a need for greater accountability throughout the school system all represent severe constraints to achieving better education in South Africa.

The significance of this paper is twofold: Firstly, it demonstrates the value of the school fixed effects model to, under certain conditions, overcome the bias caused by unobserved differences between schools, something which is particularly relevant in the case of South Africa. There is definitely scope for new areas of research using school, and even individual, fixed methods given the existence of administrative data on schools and school performance. Secondly, it is the first South African study (and one of a very few international studies) to bring robust empirical evidence to the policy debate around language of instruction.

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