On-Line Appendix A: Robustness checks

In this section, we discuss a number of checks and extensions to our main results. These are presented in On-Line Appendix Table 1. To start with, in Column (1) we restrict pupils' choice set to only include schools within 2.5km from a pupil's residence (a number that varies by pupil; on average 5.9 each, with a standard deviation of 1.9). In this case there is a much bigger semi-elasticity – approximately 33% – implying that the relative increase in a converting school's probability of being chosen first within a local choice set is substantially larger than the corresponding increase compared to the overall LA choice set. We discuss these issues in Section 6.1, where we study the interaction between home-school distance and academisation.

We next check that our results are not affected by families moving in the years preceding secondary school in order to gain admission to over-subscribed academies which rank admissions by home-to-school distance. To do so, we drop from our analysis all students that move their residential address between grade 3 (right after KS1, and at the beginning of the second stage of primary school) and grade 7 (at the beginning of secondary school). As shown in Column (2), dropping these potentially strategic movers does not affect our conclusions.

The next two columns investigate whether other institutional features of the English admission system affect our estimates. In Column (3) we drop pupils with special education needs (SEN), as these are prioritised for admissions to their preferred schools. This exclusion does not affect our findings. In Column (4), we focus on rules that prioritise children with siblings at the school for admission and exclude from our analysis all pupils who are admitted to schools on the basis of the sibling criterion. When we apply this restriction, we still find a positive effect of conversion on parental preference, with an implied effect of approximately 14%.

Finally, Column (5) adds more school controls to the specification, namely: the share of SEN pupils; the ratio of pupils to SEN-support teachers; and the percentage of school sessions missed because of absences. This extension does not affect our results. We also experimented with replacing the control for average end-of-secondary (KS4) school attainments with a measure of average primary-to-secondary (KS2-to-KS4) value-added. This also did not substantially affect our results.

On-Line Appendix B: Estimating Academies' Effect on Pupil Value-Added

In this section, we discuss two complementary approaches we use to estimate the impact of academy conversion on pupil KS2-to-KS4 value added.

First, we follow the method used in Eyles et al. (2017) and compare the KS4 attainments of pupils that enrolled for secondary education (grade 7) in academies prior to their conversion – i.e., 'legacy enrolled' students – to the attainments of pupils enrolled in schools that will become academies after our observation window – i.e., students in 'future converter' control schools. This approach is essentially a difference-in-difference (D-i-D) method that exploits differences in grade 11 (age 16) attainments among the following three groups of students: *(i)* pupils that start secondary education (grade 7) in schools that convert to academies within our observation window, but are not exposed to academy teaching – i.e., they complete secondary education before the school converts; *(ii)* pupils that start secondary school in converter academies prior to conversion and are potentially exposed to one, two, three or four years of academy education; and *(iii)* children that start

secondary education in schools that will convert to academy after the end of our observation window and so are not exposed to any academy year.

We fix attendance to the school in which students are enrolled grade 7 and prior to school conversion to by-pass the endogeneity of choice/mobility in relation to academisation. By concentrating on where pupils start secondary education, we measure an 'intention-to-treat' (ITT) exposure to academy education. However, we can also follow pupils through their subsequent moves and identify actual academy exposure. We then use ITT exposure to instrument for actual exposure and estimate Instrumental Variable (IV) D-i-D models to identify the causal effect of academy attendance on end-of-secondary school KS4 attainments

In our second approach, we consider the school in which pupils sit for their KS4 examination and compare students that are 'endogenously' enrolled in an academy in their grade 11 to pupils who are enrolled in a school that is not an academy at that point in time – but will convert after the end of our observation period. Endogeneity arises from the fact that individuals might: *(i)* choose to stay in the school where they started secondary education (in grade 7) after it becomes an academy and take their KS4 (grade 11) exams at that school; or *(ii)* choose to move away/move to an academy to sit for their KS4 examination. It should be noted that the time-series of the available data does not allow us to consider the KS4 attainments of pupils who chose to start secondary education in a school that was already an academy when they applied for it.

The first approach above should yield an unbiased causal estimate of the impact of academy conversion on a randomly picked student. The second method yields a potentially biased estimate containing a 'match-specific gain' component – which might however be relevant for parental preferences.

To estimate these models, we use KS4 attainment data on all pupils in England (not just our eight LAs) for the academic years 2005/2006 to 2013/2014. As in Eyles et al. (2017), we identify converter academies as 'operative' for the academic year *t/t+1* if they open by December of year *t* so that they have two full terms of academy teaching before impacting students' KS4 exams in May of *t+1* (still part of the same academic year t/*t+1*).[1] To clarify, consider the following example: a student who starts secondary school in September of 2007 – in the academic year 2007/2008 – who takes his/her KS4 tests in May 2012 – in the academic year 2011/2012 – and whose school converts to academy in December 2010 – i.e., during the academic year 2010/2011. If the student does not change school, he/she will have ITT and actual academic exposures equal to two years. If instead the student changes school in September 2011, he/she will be assigned to two years of ITT exposure, although actual exposure will only be one year. Note that approximately 95% of the pupils we observe in converter academies do not change school.

Throughout the analysis, we use standardized KS4 test scores as our dependent variable. All regressions control for student demographics (FSM eligibility, SEN status, gender, English as first language and White British ethnicity) and end-of-primary KS2 attainments. Because of this, all specifications are 'lagged dependent

[1] Academies opened by December of year *t* are essentially all approved by October of year *t*, as in the timeline in our main analysis. Academies that open after December of year *t* – predominantly in April/May of year *t+1* – are deemed 'too late' to influence their students' attainment in that period and assigned to students' KS4 tests for the academic year *t+1/t+2*. Note that we experimented with alternative timelines and found that the results are fairly robust.

variable' models measuring academies' impact on students' test score progression. More restrictive value-added models yielded similar findings. Throughout our analysis, we cluster standard errors by school.

Besides estimating academies' average effectiveness, we investigate heterogeneous impacts by pupil sub-groups – e.g., FSM vs. non-FSM eligible pupils – and across schools. These models are estimated by pooling all pupil observations but interacting all controls included in the specification (including year effects) with the relevant sub-group indicators. In essence, these models only restrict school fixed-effects to be the same for the various strata. Split-sample models yield almost identical estimates. As we found no evidence of significant heterogeneity across pupils with different background, these results are not tabulated or used in our analysis.

We also estimate models (pooling all pupil types) that allow us to recover standardized school-specific policy-on academy effects. These are obtained by interacting each school identifier with a dummy capturing whether the school is open as an academy in time to influence KS4 attainments of that academic year (irrespective of how many academic years the academy has been open for).

On-Line Appendix Figure 1 graphically presents our estimates of the effectiveness of converter academies obtained using the 'legacy enrolment' approach. These are estimated separately for schools rated 'outstanding', 'good' and 'satisfactory/inadequate' at the latest inspection prior to 2010. We follow this approach since different schools had different eligibility criteria to convert to academy depending on their inspectorate rating (see Section 2.4). The top left-hand side plot displays the average impact of converting to academy up to four years after conversion (for 'outstanding' schools; up to three for the other two groups as not enough schools with these ratings convert early enough to impact KS4 attainments in the academic year 2013/2014 – the last year we use in our analysis) and up to five years before conversion – with the omitted group being the year just before academisation. The other diagrams instead present histograms displaying standardized KS4 school-specific policy-on academy effects.

Our findings show that 'outstanding' converters display no significant pre-policy effects (i.e., the standard D-i-D parallel-trend assumption is likely to hold) and a significant positive impact on pupils' KS4 attainments in the first three years after opening – before dropping somewhat four years after conversion and becoming insignificant. This dip is most likely explained by the fact that very few schools converted by December 2010 in time to have four full years of impact by the end of the academic year 2013/2014 – and should be somewhat discounted. However, we find a much less neat picture for the other two group types. To begin with, there is some evidence of pre-trends in KS4 in the years leading up to conversion – in particular for good schools – which complicates causal inference. Furthermore, we find little evidence to suggest that converter academies in these two groups improve the attainments of their students following conversion.

The other panels show that average performance measures hide substantial heterogeneity in school-specific effectiveness. The average school-specific policy-on impact for outstanding schools is 3% of a standard deviation with a standard deviation of 10.6%. On the other hand, the average school-specific policy-on impact is 0.4% for 'good' schools and negative 3.7% for schools in the residual group. Both estimates display substantial amounts of variation with 12.7% and 13.9% standard deviations, respectively.

Finally, On-Line Appendix Figure 3 presents scatter plots of the school-specific estimates we obtain using the 'legacy enrolment' approach against those obtained using the potentially endogenous school where students

sit for their KS4 exam. The left-hand side panel presents estimates for all schools in England – once again broken down by OFSTED ratings; the right-hand side panel instead presents estimates for the schools we consider in our sample. As clear from the diagrams, the correlation between the two set of estimates is very high and always above 0.85.

On-Line Appendix Table 1: The impact of conversion to academy on the demand for schools – Additional robustness checks

	(1)	(2)	(3)	(4)	(5)
	Keeps schools within 2.5km from home	Non-movers only	No SEN pupils	No pupils with pref's for sibling school	Extra school controls
Converter Academy	4.579 (1.137)**	0.147 (0.073)**	0.141 (0.062)**	0.142 (0.068)**	0.118 (0.055)**
Log of pupil-school distance	-0.225 (0.0190)***	-0.054 (0.004)***	-0.053 (0.004)***	-0.050 (0.004)***	-0.053 (0.004)***
Mean outcome	*0.1401*	*0.0098*	*0.0098*	*0.0101*	*0.0098*
Implied highest preference effect	*32.7%*	*15.0%*	*14.4%*	*14.1%*	*12.0%*
Implied willingness to travel	*20.3%*	*2.72%*	*2.66%*	*2.84%*	*2.23%*

Note: The table reports coefficients and standard errors in parenthesis (clustered at the school level) on a dummy for academy conversion (multiplied by 100) and on the log of home-to-school distance. All regressions control for a variable identifying whether schools become 'sponsored' academies and for: *(i)* pupil and school effects; *(ii)* school time-varying controls, and *(iii)* school 'academy propensity' × year effects. The dependent variable is a binary outcome denoting the students' highest preference. Column (1) only considers schools within 2.5km of the home address. Column (2) only considers pupils who do not change their postcode of residence between grade 3 (right after their Key Stage 1 test in the third year of primary education) and grade 7 (right after they have entered secondary education). Column (3) drops pupils with Special Education Needs (SEN) with statements. Column (4) drops pupils who express preferences for schools where they have priority admissions because of 'sibling rules'. Additional school controls in Column (5) include: school share of pupils with SEN; ratio of pupil to SEN-support teachers; school percentage of sessions missed because of absences (authorized and unauthorized). Column (1): 51,174 in 42 schools. Column (2): 916,030 in 60 schools. Column (3) 1,427,329 in 60 schools. Column (4): 1,037,258 in 60 schools. Column (5): 1,448,688 in 60 schools. Implied academy effect obtained by rescaling the coefficient by the probability of a school being the highest preference. ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

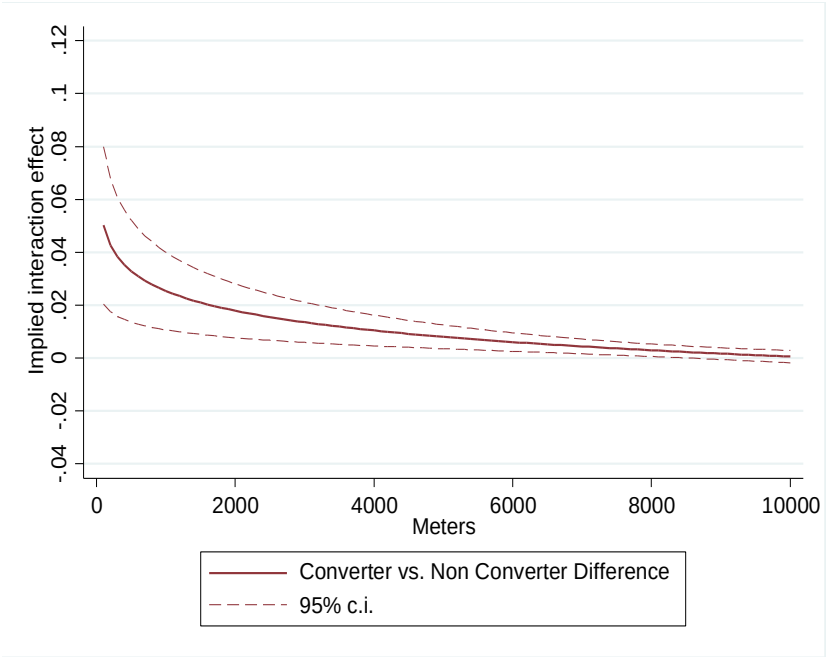
On-Line Appendix Table 2: Conversion to academy, school offers and school attended

	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)
	Pupil is non-FSM	Pupil is FSM	Pupil is White British	Pupil is other ethnicity	Pupil is White British & Non-FSM	Pupil is Non-White British Or FSM
Panel A: School offer						
Converter Academy	0.059 (0.033)*	0.019 (0.060)	0.189 (0.056)***	-0.009 (0.046)	0.202 (0.067)***	0.012 (0.034)
Implied highest preference effect	*7.11%*	*2.29%*	*22.77%*	*-1.08%*	*24.3%*	*1.45%*
Panel A: School attended						
Converter Academy	0.050 (0.036)	0.012 (0.055)	0.170 (0.057)***	-0.011 (0.043)	0.188 (0.073)**	0.006 (0.031)
Implied highest preference effect	*5.95%*	*1.43%*	*20.2%*	*1.31%*	*22.4%*	*0.71%*

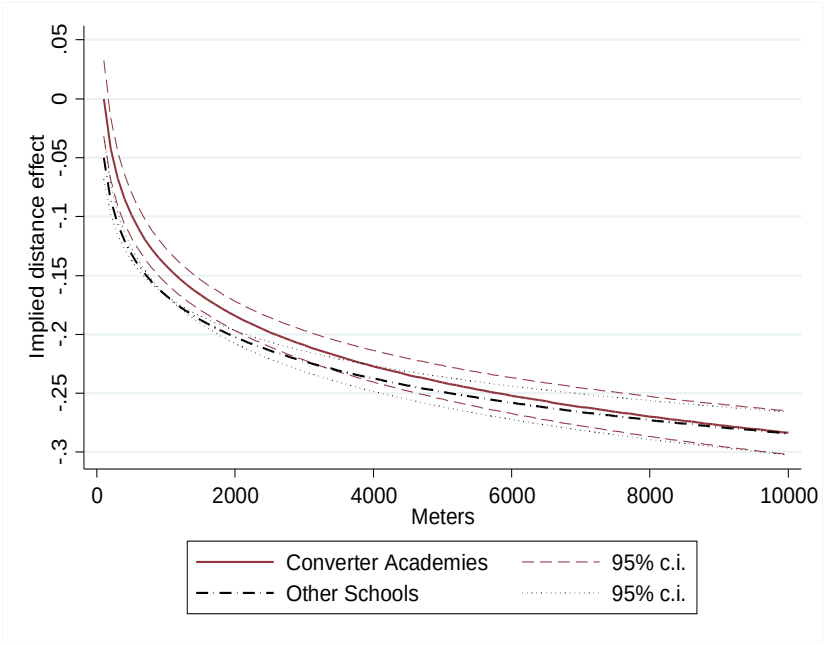
Note: The table reports coefficients and standard errors (clustered at the school level) in parenthesis (multiplied by 100). Coefficients and standard errors multiplied by 100. All regressions control for a variable identifying whether schools become 'sponsored' academies and for: *(i)* pupil and school effects; *(ii)* school time-varying controls, and *(iii)* school 'academy propensity' × year effects. The dependent variable in the top panel is a binary outcome denoting whether the student is offered a place at the school. The dependent variable in the bottom panel is a binary outcome denoting whether the student attends the school. Regressions use schools in the converters sample only (i.e. schools that are already academies at the time when pupils choose and schools that will become academies in the future). All results come from split-sample regressions run separately for the groups of pupils reported in the column headings. Implied academy effect obtained by rescaling the coefficient by the probability of a school receiving an offer or attending a school in converters sample (respectively 0.0083 and 0.0084). Number of pupils as follows (always in 60 schools). Top panel: 963,116 and 466,902 (Columns 1a and 1b); 581,412 and 848,606 (Columns 2a and 2b); 425,183 and 1,004,835 (Columns 3a and 3b). Bottom panel: 942,912 and 464,883 (Columns 1a and 1b); 577,854 and 829,941 (Columns 2a and 2b); 420,890 and 986,905 (Columns 3a and 3b). ***: significant at the 1% level; **: significant at the 5% level; *: significant at the 10% level.

On-Line Appendix Figure 1: The impact of distance for converter academies and other schools

Panel A: Implied academy-distance interaction

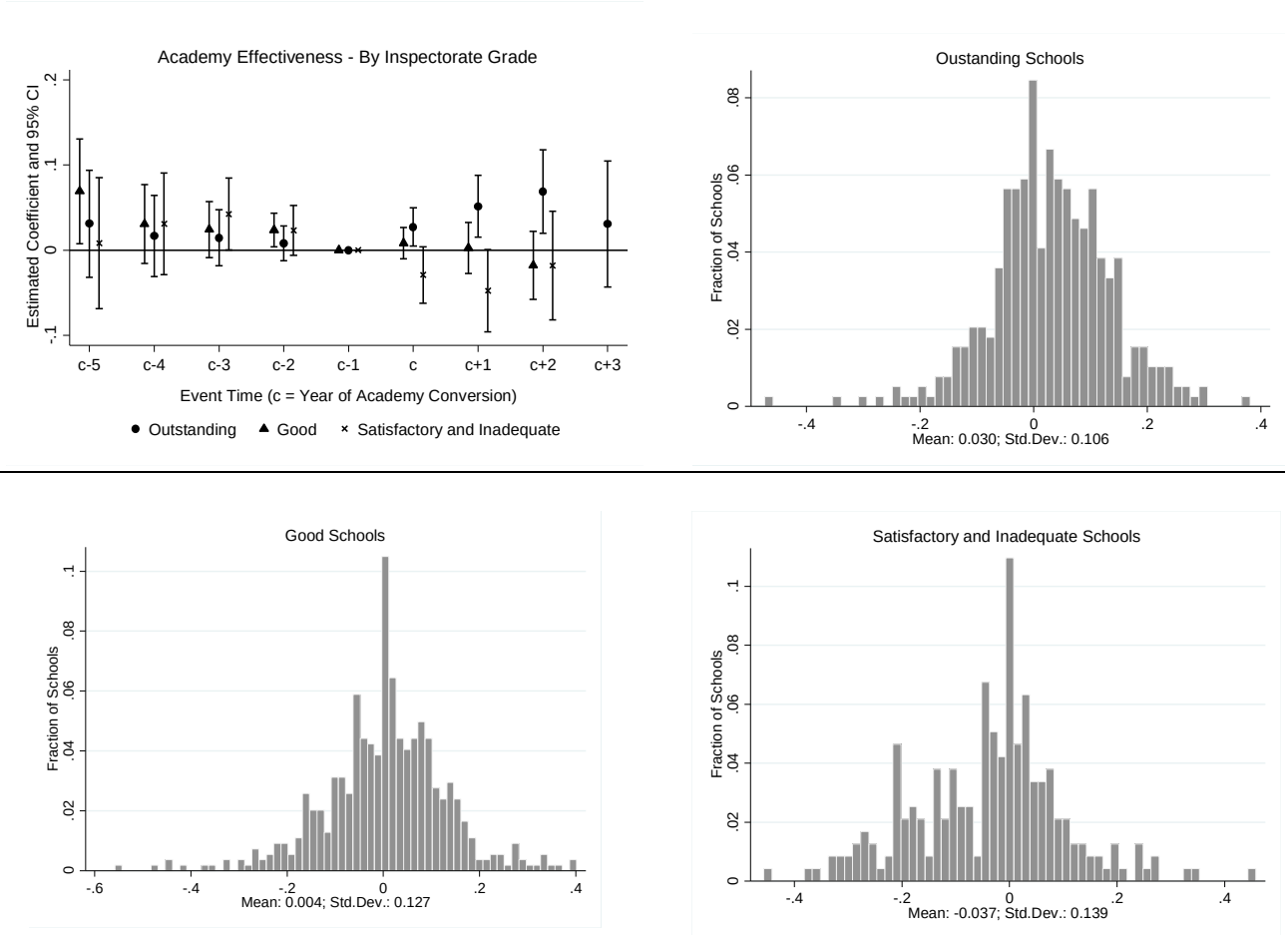


Panel B: Impact of distance by school type effect



Note: The right hand side figure (Panel A) shows the effect of distance from schools on the probability that a school is top ranked for converters and non-converters. The left hand side figure (Panel B) shows the implied (interaction) effect of converter academies at varying pupil-to-school distances. Figures obtained from specification that: use the converters sample; including all controls and school 'academy propensity'× year effects; and add an interaction between school academy conversion and the log of pupil-to-home straight line distance. Estimation sample only includes non-movers. 95% confidence intervals come from standard errors clustered at the school level. Median distance for schools chosen (ranked) by students: approximately 2.5km. Median distance for highest preference school: 1.7km.

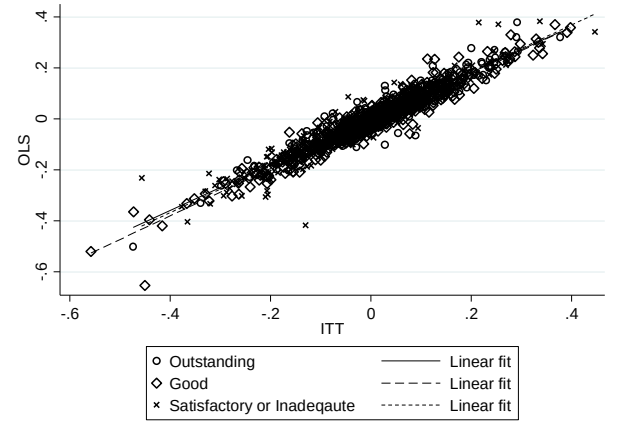
On-Line Appendix Figure 2: Academy effectiveness and variation in ITT policy-on effects – by OFSTED inspection grade



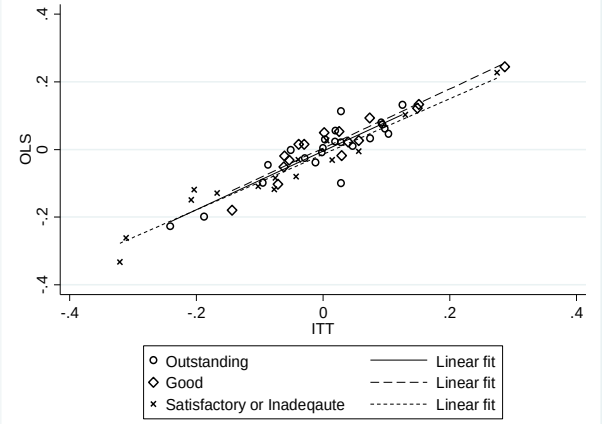
Note: Plots present estimates of the ITT impact of academy conversion on KS4 attainments. Regressions run at the pupil level with standard errors clustered at the school level. Regressions consider pupil KS4 outcomes in the academic year 2005/2006 to 2013/2014. School are considered operating as academies if they open before December of year *t* and impact on KS4 outcomes in May of year *t+1* (e.g. open by December 2011 and impact on KS4 in May 2012 – both dates referring to the academic year 2011/2012). Sample only includes: *(i)* converter academies open between September 2010 and December 2013 (treated) and converter academies open between January 2014 and March 2016 (controls); *(ii)* pupils enrolled in these schools before academy conversion (legacy enrolment). Number of pupils and schools as follows. Outstanding schools: 564,340 pupils in 395 (380 treated and 15 control) schools. Good schools: 803,039 pupils in 566 (516 treated and 50 control) schools. Satisfactory and inadequate schools: 346,684 pupils in 259 (220 treated and 39 control) schools. Top, left-hand side plot presents results for academy impact at time of conversion and up to four years after (c to c+3); and prior to conversion (c-2 to c-5). Omitted group: c-1 (year prior to conversion). There are no good schools/too few satisfactory and inadequate schools converting in the first year (up to December 2010) to present estimates for c+3 for these groups. The other plots present school-specific estimates of academy effectiveness. These are obtained from a school-specific 'policy-on' dummy indicating whether the school was open as academy at that time.

On-Line Appendix Figure 3: Academy effectiveness –
Correlation between estimates obtained using 'legacy' enrolment (ITT) and endogenous KS4 school (OLS)

All schools



Bimingham schools



Note: Plots present school-specific estimates of the impact of academy conversion on KS4 attainments. Regressions run at the pupil level with standard errors clustered at the school level. Regressions consider pupil KS4 outcomes in the academic year 2005/2006 to 2013/2014. School are considered operating as academies if they open before December of year *t* and impact on KS4 outcomes in May of year *t+1* (e.g. open by December 2011 and impact on KS4 in May 2012 – both dates referring to the academic year 2011/2012). Sample only includes: *(i)* converter academies open between September 2010 and December 2013 (treated) and converter academies open between January 2014 and March 2016 (controls); *(ii)* pupils enrolled in these schools before academy conversion (legacy enrolment). Number of pupils and schools as follows. Outstanding schools: 564,340 pupils in 395 (380 treated and 15 control) schools. Good schools: 803,039 pupils in 566 (516 treated and 50 control) schools. Satisfactory and inadequate schools: 346,684 pupils in 259 (220 treated and 39 control) schools. ITT school effectiveness is estimated comparing the performance of actual converters to the performance of future converters using only 'legacy pupils' enrolled in schools prior to conversion. OLS school effectiveness is estimated using pupils enrolled in the school at KS4 (instead of 'legacy' pupils). This approach uses the endogenous location of students at the time when they are sitting for their exams and following conversion (and captures potential 'match specific' gains). See Appendix A for more details. Left-hand side plot presents results for all schools in England. Right-hand side plot presents results for Birmingham schools only.