QUEEN MARY UNIVERSITY OF LONDON

Empirical Essays on Economics of Education and Labour Economics

by

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A Thesis submitted for the degree of
Doctor of Philosophy (Ph.D) in Economics

in the

School of Economics and Finance
Queen Mary University of London

September 2015

Declaration of Authorship

I, Barbara Masi, declare that this thesis titled "Empirical Essays on Economics of Education and Labour Economics", and the work presented in it are my own. I confirm that:

- This work was done wholly or mainly while in candidature for a research degree at this University.
- Where any part of this thesis has previously been submitted for a degree or any other qualification at this University or any other institution, this has been clearly stated.
- Where I have consulted the published work of others, this is always clearly attributed.
- Where I have quoted from the work of others, the source is always given. With the exception of such quotations, this thesis is entirely my own work.
- I have acknowledged all main sources of help.
- Where the thesis is based on work done by myself jointly with others, I have made clear exactly what was done by others and what I have contributed myself.

Acknowledgements

First and foremost I want to thank my supervisor Marco Manacorda. I am very grateful for his invaluable academic advice, constant support and endless patience, which made this thesis possible. It has been an honor to be his Ph.D. student.
I am as well particularly indebted to my second supervisor Barbara Petrongolo for numerous feedbacks and useful discussions.

Beside my supervisors, I would like to thank present and past members of Queen Mary's faculty for sharing their knowledge. A special thanks goes to Ghazala Azmat, Erich Battistin, Francesca Cornaglia, Francesco Fasani and Andrea Tesei for many insightful comments on the material of this thesis. I also would like to thank Olmo Silva for his suggestions and for sharing the data.

Parts of the dissertation were presented at different stages at the following seminars and conferences from which I got valuable comments: IZA 18$^{\text{th}}$ summer school, XI Jornadas de Economía Laboral and QMUL Economics Reading Group.

For the non-academic side of the thesis, I particularly want to thank Pedro for making the summer of my writing up one of the best of my life. Thank you for every hour you spent and will spend with me.

My time at Queen Mary and in London was made enjoyable in large part due to the many friends that became a part of my life. A special mention goes to my colleagues and friends Kostas, Jinu, Michael, Nils, Nathalie and Tommi for making of the office and, more generally of London, my home. Laura and Marcella have been great roommates and friends and our chats in front of a nice dinner and a bottle of wine will always be among my best memories. Thanks to Ale, Ceci, Daniele and Mart who became my second family. My life without you guys would be much more boring. I am grateful also to two very special people, Marco and Paolo, who have been there for me for the past thirty years, sharing all (and I mean it) best and worst times.

Last, but by no means least, I want to thank my mum Mara and my dad Paolo (Poppo). They taught me how to be curious, be open to new experiences and accept challenges. Without them and their love this thesis, as many other achievements, would not have been possible.

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Introduction

This Ph.D. thesis consists of three essays on Labour Economics and the Economics of Education, having the goal of contributing to the scientific discussion and shed new light on a number of empirical questions.

The remaining of the chapter presents a general motivation for the study, together with the main findings and policy implications, which are fully developed throughout the thesis.

Motivation

There is an ongoing debate in Economics of Education on the merits and drawbacks of school choice as opposed to a community-based model, where schools only serve the local neighbourhood.

Advocates of school choice base their arguments on the economic theory of market efficiency. First, a more market oriented education system should improve the match between pupils and schools. In this sense, allowing families to select schools on the basis of their preferences and teaching needs should result in an improvement in the average academic achievement. Moreover, increased choice should help breaking the link between residential and school segregation induced by a community-based model, with wealthier families living in more affluent neighbourhoods also attending the best schools. The benefits of choice should be even more pronounced for low income children who are typically segregated in poor neighbourhoods served by low quality schools (Gibbons et al., 2008). Second, school choice is believed to have beneficial effects also on school performance. Indeed, community-based schools operate in an almost monopolistic market, implying little incentives to innovate and improve teaching performance. In a world where parents have strong preferences for quality, a choice based model would increase competition among schools with the ultimate result of boosting performance (Hastings et al., 2005; Burgess et al., 2009; Gibbons and Silva, 2011).

On the other hand, scholars in favour of a community-school model claim that teachers are more likely to perform well in a more stable environment with relatively low turnover. Moreover, greater choice would replace the link between neighbourhood and school segregation with sorting across schools on the basis of family background characteristics. In this sense, they advocate that it would be more desirable to stick to a community-based model and improve the performance of lower quality schools via redistribution of resources.

The first two chapters of this thesis aim at shading additional light on the advantages and disadvantages of school choice models. Specifically, I explore the effects of a programme introduced in the UK, which aimed at increasing choice among low income families, on both students' choices and school behaviour.

The third chapter addresses a different empirical question. Typically, when workers are rewarded on the basis of team effort the possibility arises that individuals free ride. However, past literature emphasised the importance of externalities when groups of agents are concerned. Specifically, group effects such as social pressure or shame may be strong enough to completely offset free riding (Kandel and Lazear, 1992; Mas and Moretti, 2009). Using Italian social security data on private sector employees, the last chapter contributes to the existing literature by exploring externalities in workers' shirking, which I recover from information on sick leave episodes.

Main results

Chapter 1: One the Way Down: the Unintended Consequences of School Transport Subsidies

In the past, especially in the US, free transport to school has been used as a tool to decrease school segregation and improve the quality of education among disadvantaged students (Billings and Rockoff, 2014; Katz et al., 2001).

The chapter examines a policy reform that occurred in England in academic year 2007/2008 (Free Transport policy), which provided monetary incentives to low socio-economic status (SES) students to attend further away secondary schools. In particular, the policy supplied free transport to any of the three closest schools at a distance of at least two miles from home, with the explicit goal of improving choice and, eventually, the quality of the school attended by low income pupils.

A simple model shows that while this policy should create incentives for low SES students to attend further away schools, its effect on the quality of the school attended is ambiguous and it might even reduce it. This happens if households are willing to trade school quality for lower travel cost. The mechanism is further enhanced by the fact that families are *de facto* not free to choose. Due to over-subscription of most popular schools and distance-based admission criteria, the ultimate "compliers" may indeed be those ending up in low quality institutions.

Using confidential panel school micro data, providing information on the postcode of both schools and students' residence, I identify the effect of the policy on school choice through a difference-in-difference approach comparing students who are eligible for the programme with those who are not. Consistent with the intended objectives of the policy, I find strong evidence of an increase in enrolment into more distant schools. Interestingly, though, there is no improvement in the quality of the school attended.

Chapter 2: School Competition and Performance

The chapter investigates the effects of the Free Transport policy on schools and school behaviour. Simple economic reasoning and empirical evidence suggest that an increase in school choice should lead to an improvement in performance through competitive pressure. Clearly, this happens if money follows the pupil, i.e. incentives are enough high power. In order to assess the impact of the policy on school performance, I investigate precisely these three margins.

This chapter proceeds by first testing whether the introduction of free transport has an effect on school enrollment, i.e. if higher competitive pressure results in an actual decrease in student numbers. Estimates suggest that enrollment declines more in schools which were more affected by the policy. Second, the chapter looks at the impact of the policy on school resources. Results confirm that funds do decrease in response to the decline in enrolment numbers almost perfectly, suggesting that schools do operate in a quasi-market environment and have financial incentives to attract additional students.

Finally, I explore the impact of increased competition on school performance. I show that the policy has the effect of slightly improving quality, measured as test scores of students in Year 11. These results are consistent with the argument that competition among schools improves performance, though findings are economically modest.

Chapter 3: Externalities at the Workplace: Evidence from the Italian Private Sector

This chapter uses matched employer-employee microdata on a sample of Italian private sector workers to estimate how co-workers' behaviour affects the individual propensity to shirk. The analysis is closely related to the paper of Mas and Moretti (2009), providing evidence of positive spillovers among workers in a large supermarket. Differently from their work, however, I focus on the more common context of team work, where workers contribute to production jointly rather than individually.

Identifying shirking is challenging, as obviously it is not recorded in the data. Nonetheless, there is a large amount of evidence showing that, when sick leave is covered by public insurance as in Italy, workers' absences often cover shirking. I hence circumvent the problem by using workers' absence rate as a proxy.

Empirically, the identification of group effects relies on the arguably exogenous variation in groups' absenteeism due to new co-workers joining the establishment (movers). Employing a two stages approach, I estimate movers' absenteeism net of other characteristics and use it as a proxy for co-workers' shirking. Results show that workers tend to emulate their peers, increasing their absence rates in response to more absenteeist co-workers.

Conclusions and policy implications

All in all, the goal of this thesis is to contribute to two main strands of the existing literature: the one on school choice and competition and the one on externalities at the workplace. The empirical evidence provided carries important policy implications.

The first chapter shows how well meaning policies aiming at increasing school choice among disadvantaged pupils may have the unintended effect of lowering the average quality of the school attended. In order to achieve the goal of improving access to high quality schools, it is essential to carefully determine *a priori* what are the incentives families will face, as well as the institutional barriers that may prevent the success of such policies. Specifically, in the case under analysis two concerns emerge. First, given the subsidy's conditionality on distance, the policy creates incentives to apply to further away schools even if there is no gain in terms of quality. Second, high quality schools are likely to be oversubscribed. As distance is the main criteria for admission, the choice of school for students eligible for free transport may *de facto* be limited to poor quality schools. In light of this, future research may include exploring alternative ways to promote school choice. One may think, for instance, of extending the subsidy to *any* secondary school in order to avoid the distortionary incentives induced by distance conditionality. Alternatively, free transport could be restricted to more distant schools which are of higher quality compared to the one nearby. Finally, to overcome the oversubscription problem, one might consider the introduction of special quotas for low income students in most popular, high quality schools.

The second chapter outlines how increased competition among schools does not improve teaching quality significantly, results being small and economically modest. In particular, competition does not seem to lead to the desired effect of boosting substantially schools' performance. Though the small results could be explained by the limited "bite" of the policy under analysis, these findings are in line with other studies exploring the UK context. Overall, the evidence provided in this chapter confirms the empirical evidence that competitive pressure may not be a powerful tool to improve school quality. In this sense, policies aiming at promoting school accountability may represent a potentially more promising ground.

Finally, the last chapter shows the presence of positive externalities in shirking. In particular, workers respond to absenteeist co-workers by increasing their absences. Overall, results suggest that when monitoring is not perfect, a negative shock in the composition of peers may have significant repercussions on workers' productivity. This is especially true in large establishment, where the employment protection legislation is more stringent. In this sense, if the goal is to discourage workers from shirking , a more transparent law regulating sick leave may be desirable.

Altogether, this thesis should be viewed as a departing point, which I hope will foster the economic debate on the role played by the aforementioned factors.

Chapter 1

On the Way Down: The Unintended Consequences of School Transport Subsidies

1.1 Introduction

According to the National Transport Survey (NTS)[1], in 2009 more than 50% of British households in the bottom quintile of the income distribution did not own a car or van, compared with only 10% in the top income group. Low rates of car ownership imply that families will need to rely on public transports if their children are enrolled in schools beyond walking distance, with a significant impact on both the time and monetary cost of attending school.[2] The high cost of travelling, together with distance-based admission criteria, mean that low income students residing in isolated neighbourhoods *de facto* do not have access to the best institutions.[3]

[1] The NTS is the primary source of data on personal travel patterns in Great Britain. It is designed to monitor long-term trends in personal travel and to inform the development of policy. The survey collects information on how, why, when and where people travel as well as factors affecting travel (e.g. car availability and drivers' holding). https://www.gov.uk/government/collections/national-travel-survey-statistics.

[2] On average, tickets fares for children aged under 16 are £1 for a single short journey, £1.20 for a medium length journey and £1.40 for a long journey.

[3] In principle, school admission policies are not based on geographic zoning, implying that students could potentially apply to and attend any secondary school in the country. Nonetheless, low income students usually attend the school nearby, which is typically of lower quality compared to the national average. Gibbons et al. (2012) provide compelling evidence of how house prices are correlated with school quality: using a regression discontinuity approach, they show that a one standard deviation increase in the school's value added or raw test scores increases house

Improving access to good schools seems to be a promising tool to decrease segregation and promote social mobility. Indeed, though pupils' innate ability and parental background explain a large share of academic achievement, the quality of the school attended is believed to be crucial in determining academic success and future labour market outcomes (Card, 1992; Dearden et al., 2002a; Kramarz et al., 2009; Chetty et al., 2011).[4]

One way to achieve this goal is to decrease the cost of transport to school. The focus of this paper is a unique policy innovation which occurred in England in the academic year 2007/2008, providing monetary incentives to low income students to attend schools beyond walking distance. Although transport subsides have always existed in the UK, in 2007/2008 they became particularly generous for low socio-economic status (SES) students -i.e. those eligible for free school meals (FSME) or whose parents are in receipt of benefits.[5] In particular, it extended the right to free transport to any of the three closest schools at a distance of at least 2 miles and no more than 6 miles from home. The rationales of this policy (Free Transport policy), though with important differences, resemble two kind of programmes adopted in the past: the US desegregation policies, aiming at reducing school segregation of racial minorities, and school choice programmes, having the objective of increasing families' choice set.

prices by 3%. For additional evidence on the link between housing market prices and school quality see also Black (1999), Hoxby (2000), Rothstein (2006), Fack and Grenet (2010) and Machin and Salvanes (2010).

[4]Compelling evidence comes from the newly introduced academy schools in England, which are showed to improve the share of pupils achieving at least five grades in range A*-C in their GCSE/GNVQ (Machin and Wilson, 2009; Machin and Vernoit, 2010). More recent literature focuses on the impact of the newly introduced charter schools in the US. These schools aim at promoting teaching quality emphasizing traditional reading and math skills, extended instruction time and selective teachers hiring. Abdulkadiroglu et al. (2011) show that oversubscribed charter schools in Boston increase the test scores of low income students by a third of a standard deviation per year -enough to eliminate the black-white test score gap in a few years of attendance. In a follow-up of this paper, Abdulkadiroglu et al. (2014) show that Boston charter attendance boosted SAT scores sharply, along with the probability of taking an Advanced Placement examination. Similar effects have been found in New York City (Dobbie and Fryer, 2011). For additional evidence on the benefits on charter schools see also Hoxby and Murarka (2009), Dobbie and Roland G. Fryer (2011) and Abdulkadiroglu et al. (2011). However, the literature on charter schools is not completely unanimous. Both Ravitch (2010) and Rothstein (2004) criticize the external validity of studies on charter schools, pointing out that those schools are more likely to select students from the top of the ability distribution those children with innate intelligence and well motivated parents. Other studies using as a proxy of school quality by various observable indicators, such as teacher/pupil ratio, teachers' educations and per-pupil expenditures, find mixed results on the link with students' achievement (Hanushek, 1986, 2003; Krueger, 1999, 2003; Chetty et al., 2014).

[5]Benefits include: income-based Job-seekers Allowance, Income-related Employment and Support Allowance, Support under Part VI of the Immigration and Asylum Act 1999, Child Tax Credit (provided one is not also entitled to Working Tax Credit and has an annual gross income of no more than £16,190) and the guaranteed element of State Pension Credit.

Concerning the former, past literature generally connected the implementation of school desegregation programmes with a number of positive outcomes (Guryan, 2004; Reber, 2010; Billings and Rockoff, 2014).[6] Nonetheless, there are important exceptions: the Moving to Opportunity relocation of low SES families across the US, for instance, did not seem to be effective in improving children's academic achievement. (Katz et al., 2001; De Luca and Rosenblatt, 2010; Ludwig et al., 2013). Though the Free Transport policy shares with these policies the ultimate goal of improving access for low SES families to high quality education , it differs from the majority of school desegregation programmes as it is not conditional on attendance of a pre-assigned school.

With respect to school choice interventions, there is plenty of evidence showing how English pupils from disadvantaged families are disproportionally sorted in poorly performing institutions (Burgess et al., 2004; Allen, 2007; Burgess et al., 2008, 2010; Fitz et al., 2003; Allen and Vignoles, 2006; Gibbons and Telhaj, 2007), though little is known on whether improved school choice would help promoting access to best schools. Past literature exploring parents' preferences revealed that, on average, families do value academic attainment as one of the most important school characteristics (Hastings et al., 2005; Burgess et al., 2009; Gibbons and Silva, 2011), suggesting that expanding families' choice set should translate into a higher fraction of students attending high quality institutions.[7] Empirical evidence on this, however, is mostly limited to the US context. Among others, Cullen et al. (2005) explore the impact of introducing open enrolment within the Chicago Public Schools (CPS). Roughly half of the students opt out of their assigned high school to attend career academies and other high-achieving schools, and these students

[6] Guryan (2004) finds a 3 percentage points reduction in drop out rates for black students, while no effect is found for white students. Similarly, Reber (2010) shows that schools desegregation increased graduation rates among black students by 15 %. Ashenfelter et al. (2005) report a positive effect of desegregation on long term outcomes of black students, finding that blacks who finished their schooling just before effective desegregation occurred fared poorly compared to blacks who followed just a few years behind them at school. Finally, Billings and Rockoff (2014) show that the rezoning following the end of busing sensibly widened racial inequality despite the effort of local schools to mitigate the impacts of increased segregation through an increase in the resources invested in education. Students reassigned to high minority schools displayed persistently lower grades at graduation, lower college attendance and higher crime rates. Concerning studies outside the US, Lavy (2010) studies the effect of the end of inter-district busing in Tel-Aviv public schools. Similarly to the US, before 1994 students' assignment to secondary schools was motivated by social and ethnic integration and included busing of some pupils across the city's schooling districts. The 1994 programme terminated the previous system and granted families access to all secondary schools, both within and outside the district. He finds that affected students displayed lower drop out rates and significantly higher cognitive achievement than unaffected children. Moreover, non-academic outcomes, such as students' satisfaction and social acclimation, improved as a result of the better match between students and schools.

[7] Families also value pupils' composition and distance, the latter being generally more relevant for students with lower socio-economic backgrounds.

are much more likely to graduate than those who remain in their assigned schools.[8] Similarly, Deming et al. (2014) explore the effect of winning an admissions lottery to attend a public high school in Charlotte-Mecklenburg (CMS), showing that lottery winners are more likely than lottery losers to graduate from high school and to attend college, and that the positive impacts of choice are strongly predicted by gains on several measures of school quality.[9] [10] With respect to the UK, Gibbons et al. (2008) show that pupils who have a wider choice of schools at their place of residence perform no better than those with more limited choice. Though closely related to school choice programmes, however, the Free Transport policy differs from these policies in being the first intervention of this kind conditioning choice on distance to school.

A simple model shows that, while this policy should create incentives for low SES student to attend schools further away, its effect on the quality of the school attended is ambiguous and it might even reduce it. This follows from the fact that some students might be induced to trade school quality with savings in the cost of transport. The mechanism is further enhanced by school over-subscription and distance-based admission criteria that could *de facto* limit choice to lower quality, less popular schools.[11]

Using a unique dataset on the universe of England's students providing information on both pupils' postcode of residence and school history, I identify the effect of the policy on school choices through a differences-in-differences approach, comparing

[8] Cullen and Jacob (2007) examine whether expanded access to sought-after schools in the CPS can improve academic achievement. Using lottery data, they find that winners attend on average higher quality schools than lottery losers. However, they do not find that winning the lottery systematically confers any evident academic benefit.

[9] For additional evidence on the effects of CMS open enrolment see, among the others, Hastings et al. (2006) and Hastings et al. (2007).

[10] A different strand of the literature examines the impact on school choice of school vouchers, which decrease the cost of attending private schools. In 1990 Wisconsin began providing a small number of low income families with vouchers to attend non sectarian private schools. Greene et al. (1996, 1997) and compare the test scores of students who won the lottery with those who lost, finding significant gains in both math and reading scores. Rouse (1998) compares the test scores of students selected to attend a private school with those of all other students from Milwaukee public schools. She finds that the program had a positive impact on math score gains of selected students. Other studies on the effects of the Milwaukee Voucher Program include: Witte (1992), Witte et al. (1995), Witte and Thorn (1996), Witte (1997). Finally, Angrist et al. (2002) explore the effects of a voucher programme in Columbia, offering vouchers which partially covered the cost of private secondary school for students who maintained satisfactory academic progress. Three years after the lotteries, winners were about 10 percentage points more likely to have finished 8th grade, primarily because they were less likely to repeat grades, and scored 0.2 standard deviations higher on achievement tests.

[11] Compelling evidence on the relevance of the proximity criterion is provided by Burgess et al. (2010), showing that it accounts for up to two thirds of the overall observed difference in the quality of the school attended.

low SES students living in postcodes eligible for free transport (i.e. with at least one of the 3 closest schools over 2 miles and below 6 miles) in the post reform period with those ineligible (i.e. those for whom the three closest schools are all below 2 miles). As eligibility for the programme is based on walking distances, I computed the shortest route between pupils' postcodes and schools' postcodes using the Geographic Information System (GIS). Furthermore, I use students' postcodes measured prior to the entrance into secondary school (i.e. in their last year of primary school), to alleviate the concern stemming from families' endogenous mobility.

Consistent with the intended objectives of the policy, I find strong evidence of an increase in the probability of FSME students enrolling at more distant schools, in the order of 2 percentage points. This, however, does not result in an improve in quality, with eligible students enrolling at schools between 0.02 and 0.03 standard deviations lower in quality than ineligible ones. Exploring the potential mechanisms, my results show that a crucial role is played by school over-subscription which limits the access to more distant, high quality schools. Overall, these findings suggest that the introduction of free transport did not yield the desired effect of improving the quality of the school attended by low SES students.

This chapter unfolds as follows: in sections 1.2 and 1.3 I briefly discuss the institutional background and present basic descriptive evidence. Section 1.4 introduces a simple model of school choice with free transport to school. Sections 1.5 and 1.6 present the identification strategy and show results of the effect of the programme on the outcome variables of interest. The last section summarizes and concludes.

1.2 Background

This paper focuses on public school students in their transition from primary to secondary school. Compulsory primary education in England covers ages 5 to 16.[12] The National Curriculum is divided into four Key Stages: Key Stage 1 (ages 5 to 7), Key Stage 2 (ages 7 to 11), Key Stage 3 (ages 11 to 14) and Key Stage 4 (ages 14 to 16).[13]

[12] There is no grade retention in England, so age corresponds to school grade.

[13] A second route available to students consists of a three tier track with students enrolling in primary school at age 6-9, in middle school at age 9-13 and in secondary school from then on. However, even if very popular in the 80's, the number of middle schools started declining already in the early 90's and nowadays only a negligible fraction of students follows this path (roughly 5% of the whole population).

In the Spring at the end of each Key Stage (KS) students are assessed in three compulsory subjects, mathematics, English and science, either by teacher assessment (in Key Stage 1 and Key Stage 3) or by standard national tests (SATS, in Key Stage 2).[14] At the end of KS4, though not mandatory, most students take the General Certificate of Secondary Education (GCSE),[15] the minimum requirement being to sit national examinations in mathematics, English and science.[16]

School admission to both primary and secondary schools is based on the principle of free parental choice: parents can apply to any school, regardless of their Local Authority (LA) of residence (roughly comparable to New York City's Boroughs).

The only limit to parents' free choice is over-subscription of the most popular schools. In this case admissions are determined on the basis of the schools' own criteria, which must be non-discriminatory according to the Department for Education's guidelines. Generally, schools give priority to: (1) pupils with special education needs (SEN), (2) students who have siblings already at the school and (3) students who live close by.[17] Some schools, namely grammar schools, may select students on the basis of their ability. However, the share of these schools is negligible.

Every year LAs' websites publish an up-to-date list of the schools available within their boundaries, along with all the steps needed to complete the application process.[18] Parents are provided with very rich information on the characteristics of available schools. In particular, every school is required to publish on its website detailed information on past performances ("performance tables"), typically Key Stage 2 and Key Stage 4 attainment measures, and additional statistics, such as the pupil/teacher ratio and pupils' ethnic composition. Even if the criteria to complete the performance tables have been reviewed almost every year, measures of pupils' achievement in both mathematics and English have always been

[14]Evaluation of Key Stage 3 became teacher-assessed in the academic year 2008/2009.

[15]Roughly 95% of students in Key Stage 4 take the final examinations. This is also an essential requirement to access higher education. Moreover, virtually all universities set requirements on additional subjects to be taken at GCSE level, as well as on minimum grades.

[16]To pass the GCSE all students are required to take the examination in first level (*core*) science (Single Award). Students can also choose to pursue a Double Award (*core* and *additional*) or a Triple Award (biology, chemistry and physics).

[17]Distance for the purpose of admission is the linear (crow flies) distance between the pupil's house and the school.

[18]Applications open the Fall before the student is due to start school. Families need to submit their completed application (on-line or on paper) by the 15^{th} of January for primary schools and 31^{th} of October for secondary schools, including at least three and a maximum of five options. Results of the application will be confirmed by the 16^{th} of April for primary schools and by the 1^{st} of March for secondary schools.

included. Additional to performance tables, schools' websites must include a link to Ofsted's website, an independent body producing detailed reports on perceived schools' quality on the basis of students' and parents' satisfaction.[19]

This study focuses on the unique policy change, which aimed at increasing school choice among low income families through the provision of free transport to school. Since 1996 a duty exists for Local Authorities to provide free transport to all students aged 11-16 years old attending their nearest available school, provided this is more than 3 miles (and less than 6 miles) walking distance from their home.[20] Free transport can take different forms: school buses ("yellow buses"), free tickets for public transport, private cars and taxis or car mileage bonuses for parents. The provision of free transport only covers the travels to and from schools for the whole duration of the academic year and it is up to the LA to determine case by case the most suitable transport arrangement.[21] [22] [23]

In academic year 2007/ 2008 the Free Transport policy extended the benefit for low income students aged 11-16 to any of their *three* nearest schools over 2 (and below 6) miles walking distance from their homes. In practice, this means that starting from 2007/2008, FSME students with the first closest school below 2 miles but the second or third closest school between 2 and 6 miles can access free transport to any of the more distant two schools. If the second or third nearest school is over-subscribed and the pupil is not granted admission, the right to free transport extends to the next available school. In order to be eligible for the programme, parents need to be in receipt of benefits- the same criterion required for free school meal status. Families can apply to their Local Authority at any time during the academic year and need to provide initial evidence of their receipt status. The Local Authority would then be in charge of verifying the existence

[19] All past reports can be consulted at www.ofsted.gov.uk.

[20] To the best of my knowledge, the vast majority of Local Authorities employ the Geographic Information System (GIS) to compute the walking distance. Usually Local Authorities also provide a free of charge service through which parents can compute the home to school distance in a similar way.

[21] Local Authorities have the discretionary power to provide travel arrangements to ineligible students, usually charging a fee, but priority is to be given to eligible children.

[22] The Education Act 1996 states "As a general guide, transport arrangements should not require a child to make several changes on public transport resulting in an unreasonably long journey time. Best practice suggests that the maximum each way length of journey for a child of primary school age to be 45 minutes and for secondary school age 75 minutes".

[23] Unfortunately, to the best of my knowledge, there are not official data on what form of free transport LAs provided to families.

of the eligibility status on a yearly basis.[24] The policy change did not affect non-FSME students, with the exception that starting from 2007/2008 children living between 2 and 3 miles from the nearest school became eligible for free transport to that school.[25]

1.3 Data

To assess the effects of the Free Transport policy, I employ a differences-in-differences identification strategy, comparing FSME students eligible for free transport (defined on the basis of distance) with those ineligible before and after the policy. The empirical analysis covers academic years 2004/2005-2010/2011 and only considers students who do not reside in London. This decision follows from the fact that first, since August 2005, all students living or attending a secondary school in London have been entitled to free of charge transport or reduced fares on public transports with no distance or income constraints. As such, London Local Authorities are not subject to the duties of the Free Transport policy. Second, London secondary schools display different trends in terms of performance compared to the rest of English schools. Table 1.1 shows how the eligible and ineligible groups are constructed. The first two columns report the distance to the nearest and second nearest school respectively, the third and fourth columns report the eligibility for free transport before and after 2007/2008 and the last column reports the percentage of the total sample. For simplicity, and without great loss of generality, I restrict the analysis to students who leave less than 2 miles from the nearest school and assume that families can only choose between the 2 nearest schools. The ineligible group is then defined as pupils who leave less than 2 miles from the second nearest school, while the eligible group is formed by pupils whose second nearest school is over 2 (and below 6) miles from home. As shown in the last

[24]Local Authorities are asked to publish detailed information on how the eligibility for free transports would be assessed and what kind of assistance they would be providing.

[25]The fact that non-FSME students are now eligible for free transport to the nearest school (between 2 and 3 miles) from home may potentially have an impact on FSME students as well, through an increase in competition for schools between 2 and 3 miles. To rule this out, table 1.A1 in the Appendix reports results for a regression on non-FSME students of the form

$$y_{ipt} = \gamma_0 + \gamma_1 T_{pt} + \eta_p + \eta_t + \omega_{ipt}$$

where T_{pt} is equal to 1 if the nearest school is between 2 and 3 miles from home in the post reform period, η_p are postcode fixed effects and η_t are year fixed effects. Estimates show that the changes in the requirements for free transport for non-FSME students did not have a significant impact on their choices.

column of the table, overall these two groups count for 91% of the total number of English students.

The core dataset used in the analysis is the Pupil Level Annual Census (PLASC), carried out every year at the end of January. This is a Census of English state school pupils, covering roughly 95% of the whole population.[26] It includes information on student demographics such as gender, ethnicity, language spoken at home, special education need status (SEN), eligibility for free school meal, the unique identifier of the school attended and pupils' postcode of residence. There are 900,609 postcodes in my data. A postcode includes roughly 20 households (a block) located on the same side of a street and identifies on average less than 2 students per year in the data. I focus on students due to start secondary school in academic years 2004/2005 to 2010/2011.[27]

A minor concern relates to the time at which the eligibility for free transport is determined. As mentioned above, parents can apply for free transport at any time during the academic year . Hence, one may worry that families may move (or avoid to move) in order to gain eligibility for free transport to their preferred school. To temper this concern, I consider students' postcode measured during the last year of primary school, that is, before the eligibility for the programme is assessed.

I use administrative data on schools, which report the exact address of every establishment, to match each pupil to his two nearest secondary schools determined on the basis of linear distance ("crow flies", which determines admission) from the student's postcode of residence. I exclude from the sample of schools institutes for SEN students (special schools). I do so because these schools may follow a different curriculum from the national one and pupils studying below GCSE level may take a different qualification altogether in one or more subjects.

To determine eligibility for free transport, I measure walking distance from the pupil's postcode to each school using the Geographic Information System (GIS), which computes the shortest route available excluding motorways and major roads. Figure 1.1 provides an example of how walking distances to school are computed: the straight line reports the linear distance to the second nearest school, while the blue-dotted line reports the shortest walking distance. In the example the student would not be eligible for free transport if we were to consider the linear distance;

[26] About 5% of English students are enrolled in private schools.

[27] As I am interested in the transition between primary and secondary education, I exclude from the analysis the small fraction of students (roughly 5%) enrolled in middle schools.

however, he falls into the eligible group when considering walking distance to the school.[28]

Finally, I use data on students' test scores at KS4 (Year 11) from the National Pupil Database (NPD) to obtain a measure of the quality of school attended. The data include information on individual GCSE test scores in all subjects for the academic years 2004/2005-2010/2011. One may worry that schools based in different neighbourhoods may experience different trends in performance (for instance because Local Authorities invest more resources in schools based in more deprived areas). If this process differs between the eligible and ineligible group, the estimates of the effect of the programme on the quality of the school attended may be biased. In order to alleviate this concern, I define a time invariant measure of school quality computed as the average of English and mathematics test scores over the whole period of analysis and standardize it at the school level to have a mean of zero and a unit standard deviation, such that the average school quality in the period is zero. It is worth mentioning that this measure is constructed based on the test scores of students who enrolled before the policy was implemented (2007/2008) and is hence pre-determined.

Figure 1.A1 in the Appendix summarises the timing of the data building. In October, at the beginning of the last year of primary school (Year 6), families fill the application form to enrol at secondary school. In January of the following year, at the time of the Census, I observe the residential address of the student and measure the walking distance to each of the two nearest schools. In September the student starts secondary school (Year 7) and, finally, in January I observe the unique identifier for the school attended and assign the corresponding measure of school quality to each student.

1.3.1 School characteristics

There are 3,323 secondary schools in England in the period of analysis.[29] Panel A of table 1.2 reports schools' basic characteristics. Among them 50.23% are

[28] As school have some discretionary power in determining the walking route to the school, there is still some risk of measurement error in determining the eligibility for the programme. Specifically, schools consider the "safe" shortest route from the pupil's house to the school, implying that they are allowed to discard some routes when they do not find them appropriate for the pupil. As the policy does not provide schools with objective criteria to define safety, I am not able to control for this.

[29] This number does not account for secondary schools based in London and schools dedicated to special education needs students ("special schools"), which have been excluded from the analysis. Moreover, schools changing denomination are counted as separate schools.

community schools, which are run and financed directly by the local government.[30] On average each school enrols roughly 147 new students every year, going from a minimum of 2 in the bottom decile of the distribution to almost 275 in the top decile.

The last row of Panel A reports statistics on school quality. The top 10% of schools perform 1.4 standard deviations better than the average and 2.4 standard deviations above the bottom decile.

Panel B displays schools pupils' composition. In the average establishment almost 80% of first year students are white British, more than 88% speak English as a first language and roughly 20% of them are eligible for free school meals. As for the number of new enrolments, students' characteristics differ widely among schools, suggesting that there is significant sorting of pupils based on ethnicity and parental income. The fraction of white British students goes from 16% in the bottom decile to a maximum of over 98% in the most "white" schools. Very similar patterns emerge with respect to English speakers: in 10% of schools the proportion of students speaking English as a first language is in the order of 36%, while in the top 10% of the distribution it is virtually 100%.

Lastly, there is significant variation also with respect to students' family income. In the most wealthy schools, the percentage of FSME pupils is less than 2%. This is well below the national average of 20%. On the other hand, FSME pupils account for 57% of students in the most disadvantaged schools.

Overall, these figures show that there is large variation in both the quality and students' body composition of schools, including ethnic and income composition.

1.3.2 FSME students' characteristics

There are 416,366 FSME students starting secondary school between academic years 2004/2005 and 2010/2011. Panel A of table 1.3 reports the basic characteristics of the sample. The first column reports statistics for the whole sample, the second for students eligible for free transport (on the basis of distance) and the last for ineligible students.

[30]There are several types of secondary schools in England, which differ regarding the degree of freedom in setting their own curriculum. The most common are: community schools, controlled by the local council; foundation schools, with slightly more freedom than community schools; voluntary controlled and voluntary aided school, run by a foundation or a trust and academies, comparable to US charter schools.

Eligible students are more likely to be white British and to speak English as a first language compared to the rest of the population: 87.5% of them report to be of white British ethnicity and 95% are native English, compared to 74% and 84% respectively among the ineligible.

Figure 1.2 shows the distribution of students by distance to the two nearest school from home. The majority of FSME students have at least two schools within 2 miles, with less than 10% of them having to travel more than 2 miles to reach the closest school. However, more than 15% of FSME students have the second nearest school above 2 miles from home, meaning that, starting from 2007/2008, they would be eligible for free transport. Panel B of table 1.3 shows the statistics relative to school availability and choice of school separately for eligible and ineligible students. The average distance among all children to the nearest school is 0.9 miles while the distance to the second nearest is 1.8 miles, increasing to 1 and 2.9 miles respectively for the sample of eligible students.[31]

Most students attend either the nearest or the second nearest school from home: more than 70% of eligible pupils attend one of these two schools, compared to roughly 63% of other pupils. Interestingly, eligible students attend schools that are, on average, of higher quality than the ineligible group (of the order of 0.11 standard deviations).

Figure 1.3 shows the distribution of the quality of the nearest and the second nearest schools by distance to the second nearest school for FSME students (i.e. the programme eligibility variable). Strikingly, on average, the second nearest school is always of higher quality than the nearest one, the gap increasing with distance. Even more interestingly, the quality of both schools decreases with distance as long as pupils live within 2 miles from the school and it increases sharply above the 2 miles threshold. The average standardized test scores of the nearest school are in the order of -0.04 for both eligible and ineligible students, while the same figures for the second nearest school are in the order of 0.14 for eligible students and 0.07 for ineligible ones. This suggest two margins of residential segregation. First, FSME students are generally segregated into neighbourhoods served by low quality schools surrounded by affluent neighbourhoods with high quality schools. Second, among FSME students, those who are more isolated are surrounded by

[31] 3 miles is the "statutory walking distance" for ineligible students and 2 miles the "statutory distance" for low income students, i.e. the maximum distance students are expected to walk to school according to the DfE.

neighbourhoods served by higher quality schools than other disadvantaged students (possibly wealthy residential areas).[32] Overall, these figures suggest that, by pushing students to enrol at more distant schools, the Free Transport policy could in principle have beneficial effects on the quality of the school attended by eligible students.

1.4 Theoretical framework

For the sake of simplicity, consider a world with only two schools. Note, however, that the implications do not change if the model is extended to more than two schools. A family decides whether to enrol their children at the nearest school (S_1) or at the more distant school (S_2). The utility of enrolling at S_1 and S_2 is given, respectively, by

$$U_1 = Q_1 - \beta_1 dist_1 + e_1$$

and

$$U_2 = Q_2 - \beta_1 dist_2 + e_2$$

where Q_1 and Q_2 are school quality measured as test scores, $dist_1$ and $dist_2$ are the distance costs of attending the further away school and e_1 and e_2 are idiosyncratic error terms. The parameter β_1 captures the utility cost per mile of travelling to school, embodying both the monetary cost of transport and the leisure loss. The family will choose to enrol their children at the school delivering the highest utility. Hence, the probability of attending S_2 will be given by

$$P(S_2 = 1) = P(U(S_2) > U(S_1)) = F(\Delta Q + \beta_1(dist_1 - dist_2))$$

Where F is the cumulative distribution of $e_2 - e_1$ and $\Delta Q = Q_2 - Q_1$. Note that students may enrol at the more distant school even if this is of lower quality

[32] Figures 1.A2 and 1.A3 provide a visual representation of these two stylized facts for the city of Manchester. Figure 1.A2 maps the difference in quality between the second nearest and the nearest school (on the left) and the proportion of FSME students on the territory (on the right). Figure 1.A3 shows the difference in quality between the second nearest and the nearest school (on the left) and the proportion of FSME students living between 2 and 6 miles from the second nearest school (on the right).

compared to the nearest one (i.e. $P(S_2 = 1) \neq 0$ even if $\Delta Q \leq 0$). This captures preference heterogeneity across families.[33] In particular, as test scores are *de facto* only a proxy of true quality, a family utility function may take into account other characteristics, for instance peer composition, the quality of the neighbourhood or more targeted programmes for disadvantaged pupils.

The Free Transport subsidy *de facto* reduces the cost $dist_2$ of attending the distant school. All else equal, the main implications of the programme on the choice of school can be summarized as follows:

1) A positive impact on the probability of enrolling at S_2;

2) A larger effect the higher the distance to S_1;

3) A smaller effect the higher the distance to S_2.

The second relevant question concerns the effects on the average quality of the school attended. The expected quality can be written as

$$E(Q) = Q_1 P(S_1 = 1) + Q_2 P(S_2 = 1)$$

This is equivalent to

$$E(Q) = Q_1 + \Delta Q F(\Delta Q + \beta_1(dist_1 - dist_2))$$

The effect is ambiguous and depends effectively on the distribution of school quality (ΔQ) among those who took up the policy. In particular, given the design of the programme, marginal students may be pushed to enrol at more distant schools even if there is no gain in terms of quality. This follows from the fact that, as mentioned, families have different preferences and take into account school characteristics other than test scores. This implies that some families would prefer to enrol their children at S_2 even if $\Delta Q \leq 0$, but are constrained by the distance cost. The decline in $dist_2$ may hence move these students away from S_1 towards S_2. Second, the subsidy may imply that now for some families $dist_2 < dist_1$. In, particular, the free transport subsidy provides monetary savings for students whose nearest school is beyond walking distance and would have to pay public

[33] Though I do not report statistics here, the data at hand confirm that a non negligible fraction of students attend a more distant school even if it is of lower measured quality compared to the nearest one.

transport out of their own pocket if attending the closest school. Hence, in the post reform period these students may decide to enrol to the more distant school even if $\Delta Q \leq 0$, in order to save on transport costs.

The overall potential effect on $E(Q)$ is shown in figure 1.4. The y-axis reports the expected quality of the school attended $E(Q)$ and the x-axis the difference in the quality of the two schools ΔQ. The solid line plots the distribution of $E(Q)$ for a given $P(S_2 = 1)$ before the policy change: the larger ΔQ, the higher $E(Q)$. The effect of the policy $E(Q)$ is shown by the dashed line. Free transport has the effect of boosting the distribution of $E(Q)$ for values of ΔQ greater than 0 and pushing it down for values lower than zero.

Indeed, although ex-ante $\Delta Q > 0$ (see table 1.3), meaning that FSME children could potentially gain from the policy, I show that, due to school oversubscription, students responding to the programme are disproportionally those for whom $\Delta Q \leq 0$, so that $E(Q)$ declines as an effect of the policy.

1.5 Empirical strategy

In order to identify the effect of the policy on FSME students' choice, I use a differences-in-differences strategy based on the eligibility for free transport as shown in table 1.1. In practice, I compare the choice of eligible students (i.e. FSME pupils with the first school below 2 miles and the second nearest school above 2 miles) and ineligible students (i.e. FSME students with both schools below 2 miles) before (up to 2008) and after the implementation of the policy (2007/2008 onwords).

Ignoring other covariates, I estimate the model in reduced form

$$y_{ipt} = \beta_0 + \beta_1 D_{pt} + \eta_p + \eta_t + \epsilon_{ipt} \tag{1.1}$$

where D_{pt} is a variable that takes the value of 1 if the second nearest school to student i's postcode is between 2 and 6 miles walking distance in the post reform period, η_p is a postcode fixed effect, η_t are time fixed effects and the β_1 parameter captures the effect of the programme. The outcome variable y_{ipt} is either the probability of attending a given school or the quality of the school attended.[34]

[34]Though the analysis relies on a linear probability model, results are consistent and comparable when estimating a conditional logit model.

Equation 1.1 leads consistent estimate of the intent to treat parameter under the assumption that, in the absence of the programme, the changes in the outcome variables would have been the same for eligible and ineligible postcodes. In other words, the eligibility for the programme should be "as good as random", implying that $Cov(D_{pt}, \epsilon_{ipt}|\eta_p, \eta_t) = 0$.

One violation of this assumption may occur if distance to the second nearest school (the treatment variable) is correlated with other unobservable characteristics of the pupils. This might arise from endogenous mobility or, in general, from the non random allocation of households across neighbourhoods. As the identification strategy is a differences-in-differences (across postcodes over time), the real concern is whether such selection is correlated with the policy reform, as in practice the diff-in-diff is able to control for non random location as long as it is time invariant. One might indeed think of circumstances where households respond strategically to the policy. Consider, for instance, a household with very strong preferences for a (good) far away school, say school A. In the pre policy period this household would have moved near to the school in order to maximize the probability of admission and minimize the cost of travel. If that school is centrally located (better schools tend to be close to each other), then this household would have been classified as ineligible in the pre policy period, as the second nearest school would also have been within 2 miles from home. However, this household might decide not to move in the post reform period in order to take advantage of the subsidy. It would now be classified as eligible while still attending school A. Under this set of circumstances one would find that households further away from the second nearest school are more likely to attend school A in the programme vs the pre programme period, but this would be a pure compositional effect, rather than a genuine effect of the policy. There are three arguments that suggest that this selection should not be a major source of concern. First, as discussed in section 1.3, all distances to schools are predetermined and, as such, do not depend on residential choices in response to the policy. Second, low income households are typically immobile, especially considering that house prices are highly correlated with proximity to good schools. Third, this can be empirically tested. Though, for simplicity, I do not report this here, a regression of the number of household by postcode on the treatment variable shows no significant correlation between the policy change and students' residential choices, suggesting that endogenous mobility is not a major source of concern.

Aside from endogenous mobility in response to the policy, another potential source of bias in the estimates might result from latent time trends in school attendance among children in populated vs isolated areas. If those living in more populated areas are increasingly more likely to attend closer and possibly better schools compared to those in isolated areas, this might confound the effect of the policy. In theory this seems to be unlikely. Also, if this were the case one could expect a smooth trend across treatment and control areas over time. Figures 1.5 and 1.6 report the treatment effect at different leads and lags from the implementation of the policy.[35] Overall, there is no evidence of the presence of pre policy trends in the outcome variables. Moreover, there is a change in the gradient precisely at the time of the policy change, reassuring on the validity of the identification strategy.

1.6 Results

This section begins by showing the overall effect of the program on the choice of school (subsection 1.6.1). Second, it looks at the effects on the quality of the school attended (subsection 1.6.2). Third, it checks the identifying assumptions and whether the main findings are robust to the alternative specifications (subsections 1.6.3 and 1.6.4). Finally, it analyses heterogeneous effects in the impact of the programme (subsections 1.6.5 and 1.6.6).

1.6.1 The effect of the policy on the choice of school

Figure 1.7 shows the probability of attending the nearest school before and after 2008 by distance to the second nearest school. Data only refer to FSME students. Observations on the left of the vertical line (i.e. with distance to the second nearest school less than 2 miles) identify the ineligible group, those on the right (i.e. with distance to the second nearest school greater than 2 miles) the eligible group. The dashed line reports data for the pre policy period, while the solid line reports data for the policy period. The difference between the outcome of the eligible and ineligible groups before and after the policy identifies the effect of the programme. As it is clear, the proportion of eligible students attending the nearest school falls significantly after the implementation of the policy, while it is virtually unchanged

[35] The figures plot the coefficients ψ_{1t} from the following regression (where η_s=1 if $t = s$):

$$y_{ipt} = \psi_0 + \sum_{s=2005/2006}^{2010/2011} \psi_{1s}(D_p * \eta_s) + \eta_p + \eta_t + u_{ipt}$$

where D_p is a variable that takes the value of 1 if the second nearest school to student i's postcode is between 2 and 6 miles walking distance.

for the ineligible group. This suggests that free transport had the effect of decrease the fraction of low income students attending the closest school.

Table 1.4 shows the corresponding estimates of the effect of the programme on the probability of attending each of the two nearest schools (row 1 and row 2) or any other school (row 3). The first column controls only for Local Authority fixed effects, time fixed effects and students' background characteristics. These include: gender, student's first language and a dummy for whether the student identifies himself as "white British". Standard errors are clustered at the Local Authority level. Results show a clear negative, though small, effect of the programme on the probability of attending the nearest school from home, with a coefficient of -0.027 (significant at the 1% level). These results imply that being eligible for the programme decreases the probability of attending the nearest school by 2.7 p.p. in the post reform period, corresponding to a 5.6% decrease over the mean of 48%. The decrease in the probability of attending the nearest school is counterbalanced by a 1.2 p.p. increase in the probability of attending the second nearest school and a 1.6 p.p. increase in the probability of attending other schools.[36] These represent, respectively, an increase of 6.8% and 4.6% over the corresponding means of 17.6% and 34.6%.

The specification in columns 2 and 3 further controls for potential time varying endogenous sorting within Local Authority. Specifically, families can endogenously choose their location with respect to schools on the basis of unobserved characteristics which affect both the probability of being eligible for free transport and the choice of the school. If this process is not time invariant, estimates would be biased. In an attempt to control for this, I include in the regression a polynomial of the second order for the distance to the second nearest school (column 2) and to the nearest school (column 3). The coefficients are slightly smaller than the ones presented in column 1 but still statistically significant.

Finally, the specification in column 4 controls for postcode fixed effects. This regression compares eligible and ineligible students in the pre and post reform periods absorbing all time invariant unobservable characteristics of the student's postcode of residence. Though the specification is highly demanding, the estimates on the probability of attending the nearest and the second nearest school remain significant and similar in magnitude, implying a 1.8 p.p. decrease in the attendance of the nearest school and a 1 p.p. increase in the attendance of the second nearest school, corresponding to a 3.8% decrease and a 5.7% increase over the

[36] Note that, by construction, the three rows add up to zero.

mean, respectively. Interestingly, the coefficient on the probability of attending other schools remains positive, but is not significant at standard confidence levels, confirming the intuition that the choice of school among disadvantaged students is largely between the nearest and the second nearest schools.

1.6.2 The effect of the programme on the quality of the school attended

The crucial question of the paper is whether the shift in school choice had any effect on the average quality of the school attended by eligible students.

Table 1.5 shows the estimates of equation 1.1 where the dependent variable is the quality of the school attended, using the same specifications as in table 1.4.[37]As mentioned, quality is standardize over the whole period to have a mean of zero and a standard deviation of one.[38] It is worth reminding that this measure is constructed based on GCSE test scores of students who were not affected by the policy (as they enrolled before 2007/2008) and is hence pre-determined.

Estimates show that eligible students choose *lower* quality schools with respect to the pre-policy period than ineligible ones. On average, the quality of the school attended is between 0.021 and 0.022 standard deviations below the pre-policy period. Families whose children are eligible for FSM typically follow in the bottom 20% of the income distribution, implying that a household composed of two working parents will have post taxes earnings of roughly £16,000 (at year 2008)[39] . Estimates shown in table 1.5 suggest that, on average, families are willing to trade 2.2% of a standard deviation of quality in exchange for the subsidy. As the average transport cost to school for a child aged 11-16 is between £330 and £440 per academic year, the subsidy corresponds to approximately 2-3% of the family annual income.[40] This implies that a household would be willing to enrol their children at a school nearly 70% of a standard deviations worse if the subsidy was 100% of

[37]This measure is based on the average quality for the school's existence period. Since the panel is unbalanced due to school openings and closures, different spans of time may be considered for schools with different existence periods.

[38]Results do not change if quality is defined as the average test scores at baseline year, i.e. at 2004/2005.

[39]Statistics from the HM Revenue and Customs, available at https://www.gov.uk/government/statistics/percentile-points-from-1-to-99-for-total-income-before-and-after-tax.

[40]The average cost of a monthly ticket is between £30 and £40 and the academic year goes from September to the end of July.

their initial annual income (corresponding to approximately £32,000, enough to move from the bottom 20% to the median of the income distribution).

Overall, these results suggest that the policy did not have the desired effect of improving the quality of the school attended among FSME students. First, as discussed in section 1.4, as a result of the programme some FSME students may decide to enrol to more distant schools, even when there is no gain in measured quality. Second, as families' access to high quality schools is rationed, eligible students are *de facto* able to attend distant schools only as long as they are not very popular (and presumably high quality). The two effects combined may explain why the policy did not improve the average quality of the school attended.

So far I have assumed that the only measure of school quality considered by parents is given by students' standardized test scores. Nonetheless, as discussed in section 1.4 other characteristics may also be relevant in the choice of school. Rows 2 to 4 of table 1.5 report the estimates of equation 1.1 for schools' student composition, measured as the percentage of white British students, the percentage of FSME and the percentage of native English speakers. Similarly to school's quality, all the three variables are constructed as a mean for the whole period of Year 11 students' characteristics and hence are pre-determined. Row 2 and row 4 report the estimates of the percentage of white British students and English speakers in the school. Overall, all estimates are very close to zero and not statistically significant. Interestingly, a significant and positive, though rather small, effect emerges with respect to the percentage of students eligible for free school meals in the school (row 3): students eligible for free transport enrol at schools with between 0.4 and 0.6 percentage point higher fraction of pupils with a similar background.

1.6.3 Robustness checks

As stated in section 1.5, the identification strategy relies on the assumption that the assignment to the eligible and ineligible group is as good as random. I attempt to prove the validity of this assumption showing the presence of pre policy parallel trends and probing the robustness of the estimates to the inclusion of (observable) students' characteristics. Nonetheless, one may still be concerned about the presence of latent trends. One way to deal with this is to make the treatment and control groups the more closely comparable as possible. I do so by restricting the sample to families who live closer to the 2 miles threshold. Specifically, I redefine the eligible group as students with the first nearest school between 1 and 2 miles from home and the second nearest school between 2 and 3 miles from home. Similarly, the control group is defined as pupils with both the first and the second nearest school between 1 and 2 miles from home.

The first panel of table 1.6 reports the corresponding estimates of equation 1.1 Results are very close in magnitude, however, they are not statistically significant. This should not be surprising, as the sample is reduced by two thirds and, once including postcode fixed effects, there is little variation left.

There are two concerns remaining. First, a (small) number of school opening and closures which might be correlated with the treatment variable.[41]. Second, school conversions, which are *de facto* treated as two separate schools (i.e. when school A converts to school B I treat these as two separate schools). As in the case of school openings and closures, this may generate bias if it is correlated with the treatment variable. In an attempt to rule this out, the second panel of table 1.6 shows estimates for the sub-sample including only postcodes which are not subject to school openings/closures or school conversions, i.e. for which the school identifier of the two nearest schools is the same for the whole period of analysis. Reassuringly, estimates are robust and very close to the ones presented above, suggesting that these concerns are of second order.

1.6.4 Falsification tests

As an additional way of checking the validity of the identification strategy, in the remainder of this section I present a number of falsification tests.

The top panel of table 1.7 reports regressions of the probability of attending the nearest, the second nearest or any other school and of the quality of the school attended for the city of London. As mentioned, London is not subject to the duties imposed by the Free Transport policy, as all students are provided with discounted fares on any public transport since 2005. Hence, if the identification strategy is valid, one should not observe any change in the choice of school following the implementation of the programme. Columns 1 to 3 show estimates for the choice of the school attended. Reassuringly, I find no evidence of an effect of the Free Transport programme on the choice of school among students living in London: estimates are virtually zero and not significant across all specifications. Columns

[41] The case of new school openings should not be an issue as I restrict the sample to years previous to academic year 2011/2012, i.e. before the mass academy conversion took place.

4 reports estimates on the quality of the school attended as defined in table 1.5. Again, estimates are not significant at the standard levels.

The second panel of table 1.7 reports estimates for non-FSME students. As higher income students are not entitled to free transport, there should be no effect of the programme on their choice of school. All estimates are close to zero and non significant at the standard levels, with the exception of the one on school quality. Note, however, that the coefficient is substantially smaller than the one found for FSME students and statistical significance may simply follow from the considerably larger number of observations.

Overall, these falsification tests lend reassuring support to the findings the previous sections.

1.6.5 Non-linear effects

As in figure 1.7, figure 1.8 reports the attendance of the nearest school before and after the reform separately for students for whom the first school is close and far away. The left graph focuses on pupils whose nearest school is located between 1 and 2 miles from home, the right graph on students whose first nearest school is within 1 mile. According to the predictions of the theoretical model, the effect of the programme should be larger the higher the distance to the nearest school and the lower the distance to the more distant school. Consistently, the effect of the policy is significant only for the sub-sample of students whose nearest school is above 1 mile from home. Moreover, results seem to be driven by pupils whose second nearest school is located closer to the 2 miles threshold.

Table 1.8 reports the corresponding estimates of equation 1.1. The top panel shows the results of two separate regressions by distance to the nearest school (i.e below 1 mile or between 1 and 2 miles). Column 1 reports the estimates for the probability of attending the nearest school. Estimates are very close to zero and not significant for students living below 1 mile from the nearest school, but in the order of 2.5 p.p. and significant for those living more than 1 mile from the nearest school. The second column reports the coefficients for the probability of attending the second nearest school: estimates are small and not significant for students living closer than 1 mile to the nearest school, while a positive and significant effect in the order of 1.5 p.p. is found for those whose nearest school is above 1 mile from home.

The second panel shows the estimates on two separate regressions by distance to the second nearest school. Specifically, I divide the eligible group in 1) students whose distance to the second nearest school is above 2 but below 3 miles; 2) students whose distance to the second nearest school is above 3 miles. Results are significant only for students whose second nearest school is located closer to the 2 miles threshold, i.e. between 2 and 3 miles from home, while no effect emerges for students with the second nearest school above 3 miles.

1.6.6 Heterogeneous effects

The first six columns of table 1.9 report estimates of the probability of attending each of the nearest schools by quality of the two available schools, by region of residence and LAs Income Deprivation Affecting Children Index (IDACI).[42] [43]

Columns 1 and 2 investigate heterogeneities based on the region of residence. I define "urban" and "rural" areas according to the 2011 UK Census classification. Rural areas are more likely to be characterized by a lower coverage of public transport, meaning that, compared to urban areas, the time cost of travelling to school would be generally higher. Most Local Authorities conformed to the Free Transport policy introducing a school bus service collecting pupils directly from their homes. This substantially reduces not only the monetary cost of travelling to school by public transport, but also the time cost, especially for families living in less populated areas. Unsurprisingly, the larger effect of the policy is found in less dense regions: pupils living in rural areas are 2.2 p.p. less likely to attend their nearest school and 1.9 p.p. more likely to enrol at the second nearest, while virtually no effect is found for students living in urban areas.

Columns 3 and 4 report results for two separate regressions for Local Authorities with a IDACI score below (less deprived) or above the median (more deprived). Though coefficients are negative for both sub-samples, the effect is significant only for students living in more deprived areas and in the order of 2.6 p.p. Estimates of the probability of attending the second nearest school are also larger and significant

[42]The Index measures locally the proportion of children living in low income households.

[43]Figure 1.A4 in the Appendix provides graphical evidence of the heterogeneity of results across different sub-groups of the population. All sub-figures report the probability of attending the nearest school from home as a function of the distance to the second nearest school (i.e. the eligibility variable) before and after the reform. Sub-figures a) and b) show the the effect separately for a) students whose second nearest school is of higher quality compared to the nearest one and b) students whose second nearest school is of lower quality compared to the nearest. Sub-figures c) and d) show the same exercise for the sub-samples of urban and rural areas and sub-figures e) and f) for less and more deprived areas.

only for IDACI scores above the median. Overall, this suggests that the programme has a larger effect in those areas where children are more likely to have a deprived background. This is consistent with the intuition that only constrained families respond to the monetary incentives of the subsidy, while wealthier ones will be more likely to enrol their children at the best school regardless of free transport.

Finally, columns 5 and 6 of table 1.9 show the estimates for the sub-sample of students whose second nearest school is of higher quality than the nearest and the sub-sample of students whose second nearest school is of lower quality than the nearest. Interestingly, the coefficient on pupils whose second nearest school is of lower quality is considerably larger and statistically significant at standard levels. Similarly, the probability of attending the second nearest school increases significantly only for those students whose second nearest school is of lower quality. These findings support the argument that over-subscription of good schools may *de facto* prevent families to enrol their children at more popular institutions.[44] This mechanism is further enhanced by distance-based admission criteria, implying that more isolated students (i.e. those eligible for free transport) will have lower chances to be accepted.

To prove this point, I use data on school capacity in year 2005/2006 to construct a proxy for schools' over-subscription. Note that the decision to use school capacity at baseline follows from the fact that changes in school choice induced by the programme may have an independent impact on schools over-subscription. I define a school as "over-subscribed" if the total count of students enrolled in the school in equal or exceeds the number of places available (i.e. school capacity).[45]

Columns 7 and 8 of table 1.9 show separate estimates for the sample of students who have the second nearest school not oversubscribed and over-subscribed, respectively. As predicted, results are larger and significant only for students whose further away school is not full or over capacity. Specifically, students who are eligible for free transport are 2.7 p.p. less likely to attend the nearest school after 2008 and 1.8 p.p. more likely t o attend the second nearest. In contrast, estimates are virtually zero and not significant at the standard levels for students whose second nearest school is oversubscribed.

[44] Though I do not provide evidence here, over-subscription is strongly correlated with school test scores. Estimates are available on request.

[45] In academic year 2005/2006, 36% of English secondary schools were oversubscribed.

1.7 Summary and conclusions

This chapter investigates how the provision of free transport to attend schools further away affects the school choices of low income families. I explore a unique policy change that occurred in England in academic year 2007/2008, which expanded the right to free transport for low SES students to any of the three nearest school to home, subject to distance thresholds. While a simple theoretical model shows that monetary incentives should push families to enrol their children in more distant schools, the effect on school quality is ambiguous, as constrained parents may be induced to choose schools further away even without a gain in terms of quality. Moreover, over-subscription of high quality schools may *de facto* limit parents' choice to less popular schools.

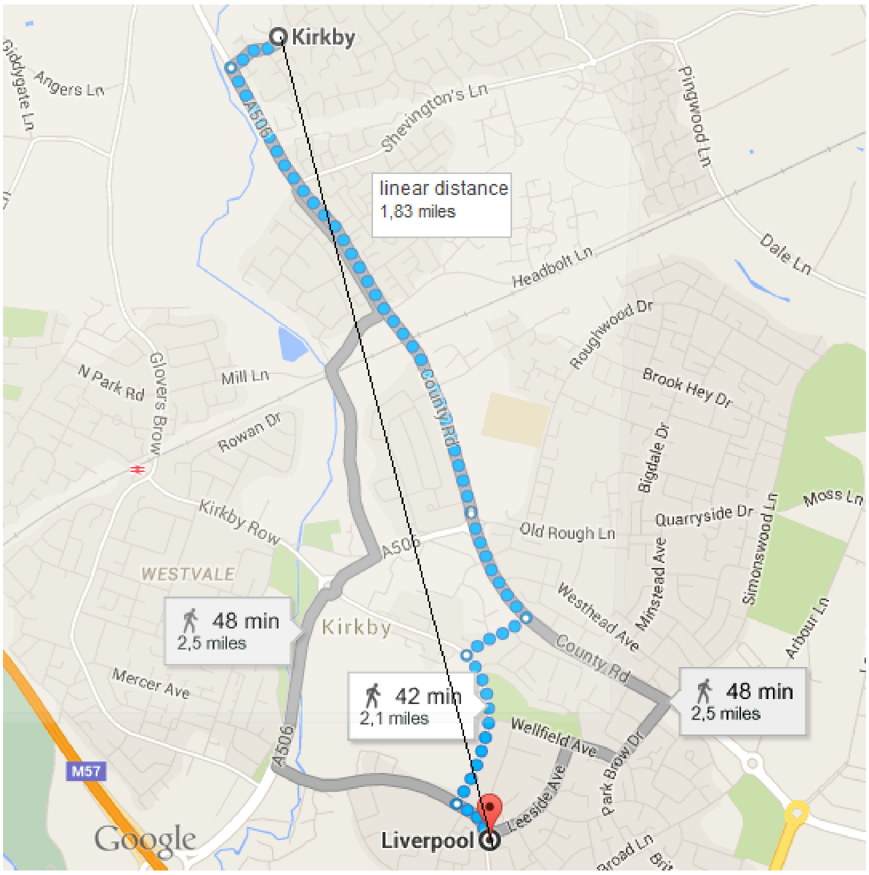
Using confidential administrative data for the period 2004/2005-2010/2011 on the universe of English students, I identify the effect of the programme through a differences-in-differences approach, comparing low SES students living in eligible postcodes in the pre and post reform period with those who are ineligible. As the Free Transport policy is based on walking distances, I compute the shortest available route for each pupil using the Geographic Information System (GIS).

Results show that, consistently, students eligible for free transport enrol at more distant schools; the effect being larger the more distant the nearest school and the more deprived the region of residence. However, the programme does not seem to lead to the intended outcome of improving the quality of the school attended by low SES students: the effect on the quality of the school attended is negative and robust to alternative specifications.

Though the direct objective of the Free Transport policy was to improve the quality of the school attended by low income families, it may still be possible that the programme succeeded under different dimensions. Specifically, though I do not address this question here, low income pupils may gain from higher choice, despite attending lower quality schools. Students may take advantage of the subsidy to escape the poor environment where they are living, benefiting from having peers with less disadvantaged backgrounds. The policy may hence result in higher average achievement, even if there is no improvement in the quality of the school attended.

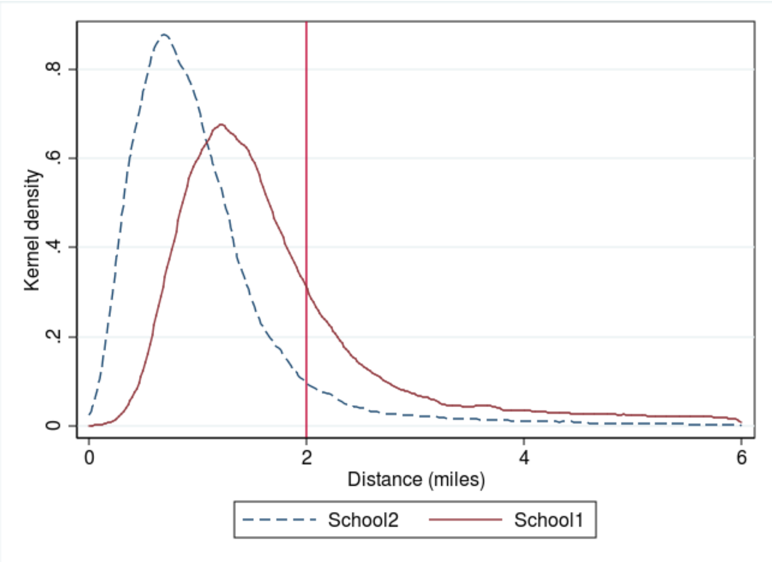
Tables and figures

Figure 1.1: Linear and walking distance to the second nearest school



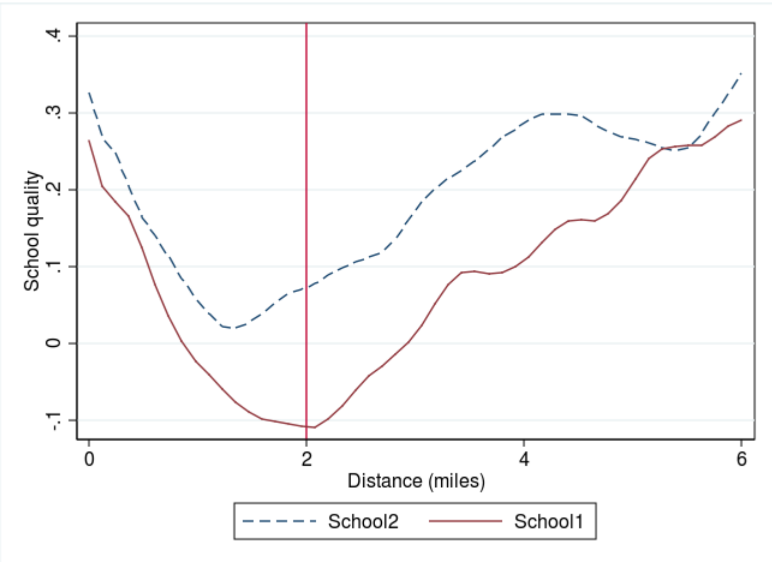
Notes: Author's calculations on PLASC data. The map reports the linear (black line) and walking distance (blue and grey lines) between the pupil house and the second nearest school from home.

FIGURE 1.2: FSME students' distribution by distance to the first and second nearest schools



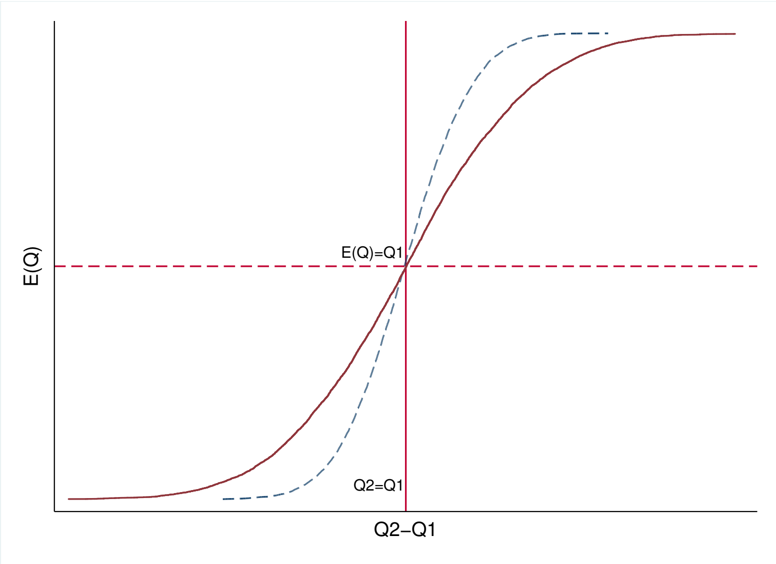
Notes: Author's calculations on PLASC data for the period 2004/2005-2010/2011.

FIGURE 1.3: Average school quality by distance to the second nearest school- FSME students



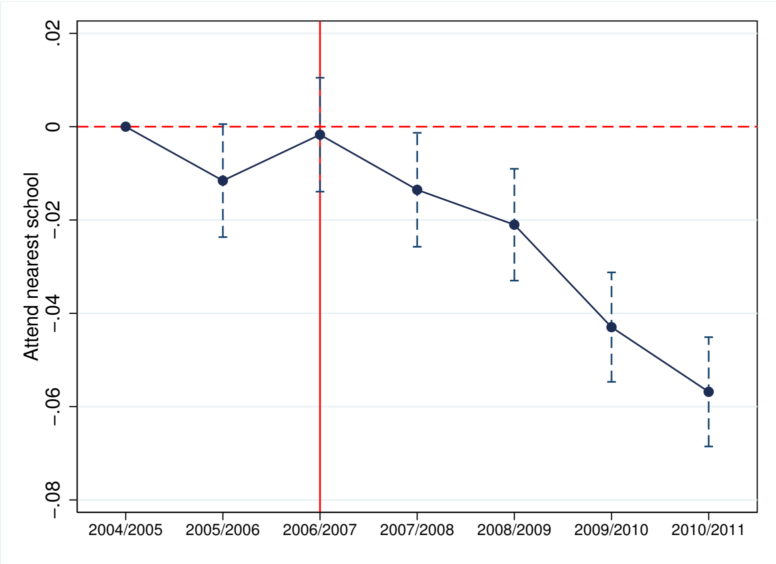
Notes: Author's calculations on PLASC data for the period 2004/2005-2010/2011. Local mean smoothing.

Figure 1.4: Predicted effect of the policy on the quality of the school attended



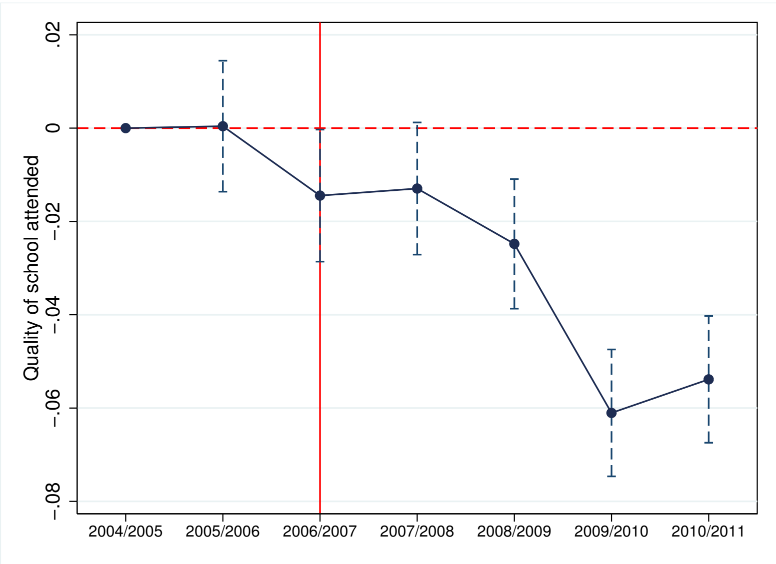
Notes: The figure plots the expected quality of the school attended on the y-axis and the difference in the quality of the two nearest school on the x-axis. The solid line represents the distribution of school quality before the policy change, the dashed line after the policy change. See also text for details.

FIGURE 1.5: Treatment effect at different leads and lags from the implementation of the policy: school attended



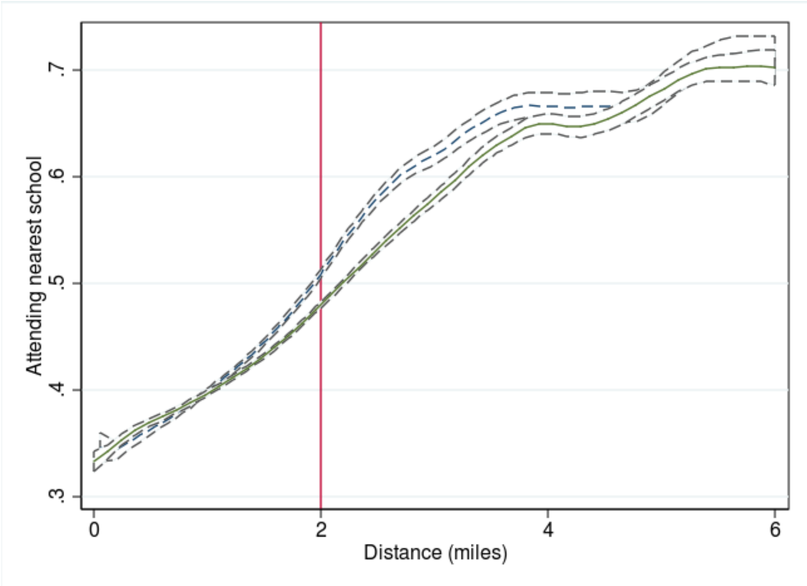
Notes: The solid line displays the coefficients of a regression of a dummy for attending the nearest school on the interaction between the year dummies and the eligibility dummy. 90% confidence intervals. Omitted category: year 2004/2005.

FIGURE 1.6: Treatment effect at different leads and lags from the implementation of the policy: quality of the school attended



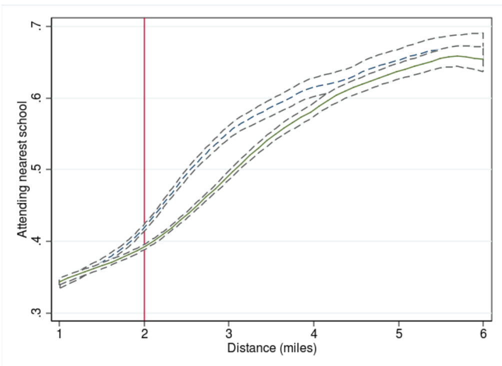
Notes: The solid line displays the coefficients of a regression of the quality of the secondary school attended on the interaction between the year dummies and the eligibility dummy. 90% confidence intervals. Omitted category: year 2004/2005.

FIGURE 1.7: Probability of attending the nearest school by distance to the second nearest school

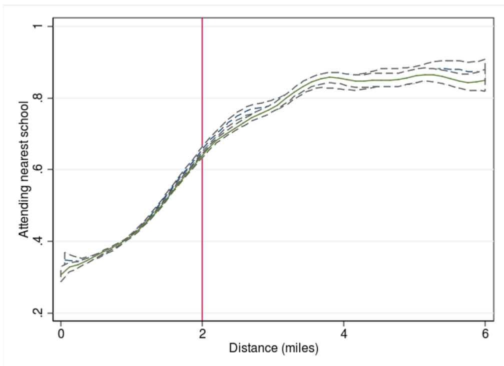


Notes: Author's calculations on PLASC data for the period 2004/2005-2010/2011. Local mean smoothing with 95% confidence interval. The dashed lines refer to the pre policy period, the solid lines to the post policy period.

FIGURE 1.8: Probability of attending the nearest school by distance to the second nearest school: non-linear effects



(a) Distance nearest school>1



(b) Distance nearest school<1

Notes: Author's calculations on PLASC data for the period 2004/2005-2010/2011. Local mean smoothing with 95% confidence interval. The dashed lines refer to the pre policy period, the solid lines to the post policy period.

Table 1.1: Free transport to school

	$dist_1$	$dist_2$	PRE 2007/2008	POST 2007/2008	SAMPLE %
INELIGIBLE	< 2	< 2	**NO**	**NO**	**73.31**
ELIGIBLE	< 2	> 2	**NO**	**YES** ($School_2$)	**17.60**

Table 1.2: School characteristics

	All schools	Bottom decile	Top decile
Panel A: Schools			
Number of schools	3,323		
Community schools (%)	50.23		
Academies (%)	7.52		
Foundation schools (%)	23.14		
Voluntary schools (%)	2.29		
Other schools (%)	16.28		
Number of new enrolments	147.19	2.07	274.86
Average exit cohorts' test scores	0.21	-0.78	1.60
Panel B: Students' composition			
White British (%)	79.24	15.58	98.23
FSME (%)	19.29	1.33	56.63
Females (%)	48.72	10.59	90.45
English speakers (%)	88.49	36.00	99.86

Notes: Author's calculations on PLASC data. The table reports summary statistics for the period 2004/2005-2010/2011. School quality is defined as the average of test scores of Year 11 students over the whole period. It has been standardized at the school level such that school quality in the period has an average of zero and a unit standard deviation.

TABLE 1.3: FSMS students' characteristics

	All	Eligible	Ineligible
Panel A: Demographics			
White British (%)	76.34	87.42	73.68
Pakistani (%)	6.91	2.06	8.07
Indian (%)	1.4	0.44	1.63
Bangladeshi (%)	1.78	0.53	2.07
Black African (%)	2.16	0.94	2.45
Other ethnic group (%)	11.42	8.61	12.09
Females (%)	49.46	49.00	49.57
English speakers (%)	85.76	94.79	83.59
Panel B: Available schools			
Distance to nearest school (miles)	0.88	1.08	0.83
Distance to second nearest school (miles)	1.77	2.86	1.51
Attending nearest school (%)	47.83	65.84	43.50
Attending second nearest school (%)	17.60	8.16	19.87
Quality of school attended	-0.06	0.03	-0.08
Quality of nearest school	-0.04	-0.04	-0.04
Quality of second nearest school	0.08	0.14	0.07
N	416,366	80,589	335,777

Notes: See table 1.2. Eligible students are defined as FSME students having the second nearest school between 2 and 6 miles from home.

Table 1.4: The effect of the Free Transport policy on school choice

	[1]	[2]	[3]	[4]
Attend:				
1. School 1	-0.027***	-0.024***	-0.023***	-0.018*
	(0.007)	(0.007)	(0.006)	(0.007)
2. School 2	0.012**	0.009*	0.009*	0.010*
	(0.004)	(0.004)	(0.004)	(0.005)
3. Other schools	0.016*	0.015*	0.014*	0.009
	(0.007)	(0.007)	(0.007)	(0.006)
Time Fixed Effects	X	X	X	X
LA Fixed Effects	X	X	X	X
Additional controls	X	X	X	X
$dist_2$		X	X	X
$dist_1$			X	X
Postcode Fixed Effects				X
N	416,365	416,365	416,365	416,365

Notes: OLS estimates of equation 1.1. See text for details. Clustered (at the Local Authority level) standard errors in parenthesis.

Table 1.5: The effect of the Free Transport policy on the quality of the school attended

	[1]	[2]	[3]	[4]	N
School characteristics:					
1. Test scores	-0.022**	-0.021*	-0.021**	-0.022**	413,691
	(0.008)	(0.008)	(0.008)	(0.007)	
2. % White British	-0.224	-0.182	-0.185	-0.130	413,744
	(0.308)	(0.298)	(0.296)	(0.270)	
3. % FSME	0.643***	0.698***	0.610***	0.369**	413,744
	(0.153)	(0.150)	(0.150)	(0.127)	
4. % English	-0.337	-0.299	-0.301	-0.118	413,744
	(0.242)	(0.233)	(0.231)	(0.210)	
Time Fixed Effects	X	X	X	X	
LA Fixed Effects	X	X	X	X	
Additional controls	X	X	X	X	
$dist_2$		X	X	X	
$dist_1$			X	X	
Postcode Fixed Effects				X	

Notes: See table 1.4. See text for details.

TABLE 1.6: Robustness checks

	Attend:			**School quality:**
	$School_1$	$School_2$	Other schools	Test scores
	[1]	[2]	[3]	[4]
Restricted sample around 2 miles threshold:				
	-0.021	0.012	0.009	-0.009
	(0.010)	(0.008)	(0.010)	(0.010)
N	129,853	129,853	129,853	129,131
Restricted sample schools continuously present:				
	-0.015**	0.010**	0.005	-0.022***
	(0.006)	(0.004)	(0.006)	(0.005)
N	255,462	255,462	255,462	253,789

Notes: See table 1.4. The first panel focuses on the restricted sample of postcodes with the first nearest school between 1 and 2 miles from home and the second nearest school between 1 and 3 miles from home. The second panel focuses on the restricted sample of postcodes that were not subject to school openings/closures or school conversions. See text for details.

TABLE 1.7: Falsification tests

	Attend:			School quality:
	$School_1$	$School_2$	Other schools	Test scores
	[1]	[2]	[3]	[4]
London:				
	-0.007	0.005	0.002	-0.037
	(0.017)	(0.013)	(0.019)	(0.019)
N	131,979	131,979	131,979	131,979
Non-FSME sample:				
	-0.004	0.001	0.003	-0.010**
	(0.003)	(0.001)	(0.003)	(0.003)
N	2,092,691	2,092,691	2,092,691	2,065,935

Notes: See table 1.4. The first panel focuses on the restricted sample of students residing in London. The second panel focuses on the sample of non-FSME students. See text for details.

TABLE 1.8: Non-linear effects

	Attend:			
	School$_1$ [1]	School$_2$ [2]	Other schools [3]	N
By dist$_1$:				
$dist_1 < 1$	-0.007	0.006	0.002	266,428
	(0.009)	(0.005)	(0.010)	
$dist_1 > 1$	-0.025**	0.015*	0.010	149,937
	(0.009)	(0.007)	(0.009)	
By dist$_2$:				
$2 < dist_2 < 3$	-0.019**	0.011*	0.008	393,379
	(0.008)	(0.006)	(0.008)	
$3 < dist_2 < 6$	-0.013	0.005	0.008	358,763
	(0.010)	(0.005)	(0.009)	

Notes: see table 1.4. The first panel shows estimates of equation 1.1 for a) the sample of students living within 1 mile from the nearest school and b) the sample of students living more than 1 mile from the nearest school. The second panel shows estimates of equation 1.1 focusing on a) eligible students living within 3 miles from the second nearest school and b) eligible students living more than 3 mile from the second nearest school. See text for details.

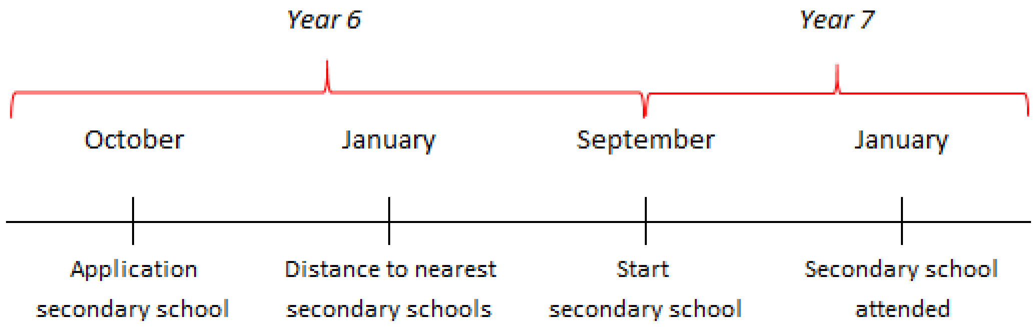
TABLE 1.9: Heterogeneous effects

	By region:		By IDACI:		By quality:		By over-subscription:	
	Urban	Rural	Least deprived	More deprived	$Q_1 < Q_2$	$Q_1 > Q_2$	Oversubscribed	Non oversubscribed
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
School 1								
	-0.013	-0.022*	-0.009	-0.026**	-0.014	-0.021*	-0.005	-0.027***
	(0.009)	(0.011)	(0.012)	(0.009)	(0.011)	(0.009)	(0.011)	(0.009)
School 2								
	0.004	0.019*	0.001	0.016**	0.008	0.011	-0.001	0.018**
	(0.006)	(0.008)	(0.008)	(0.006)	(0.008)	(0.006)	(0.008)	(0.007)
Other schools								
	0.009	0.003	0.008	0.010	0.006	0.011	0.006	0.010
	(0.009)	(0.008)	(0.010)	(0.010)	(0.010)	(0.008)	(0.010)	(0.008)
N	229,538	174,969	237,832	178,533	139,407	274,575	253,782	146,851

Notes: See table 1.4. Refer to text for details on the definition of the different sub-samples.

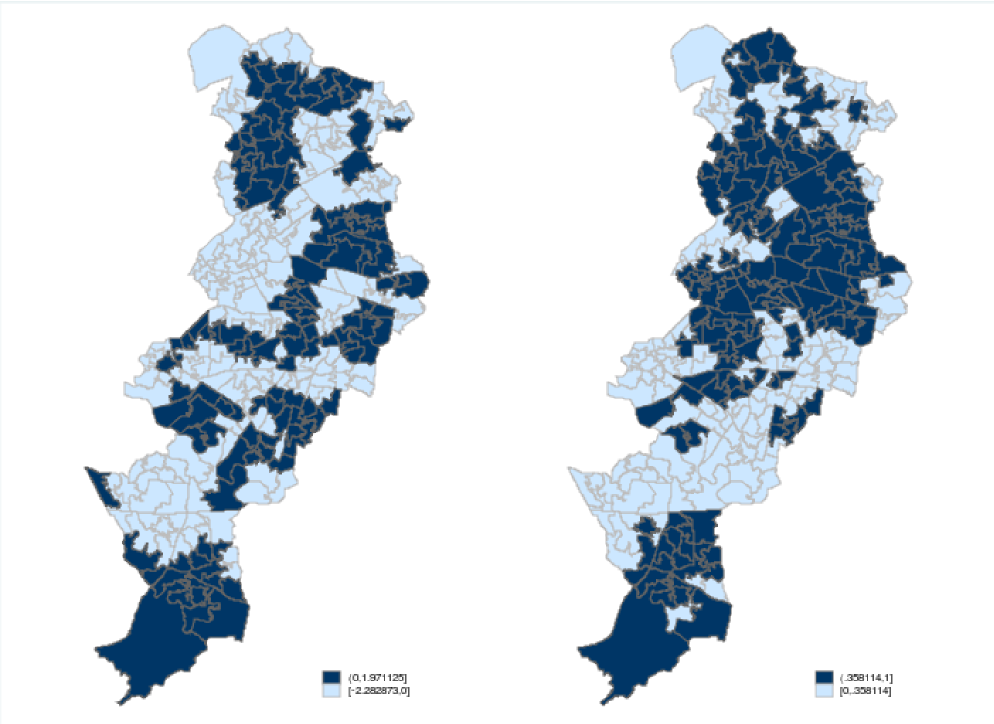
Appendix A: Supplementary tables and figures

FIGURE 1.A1: Timeline of data building



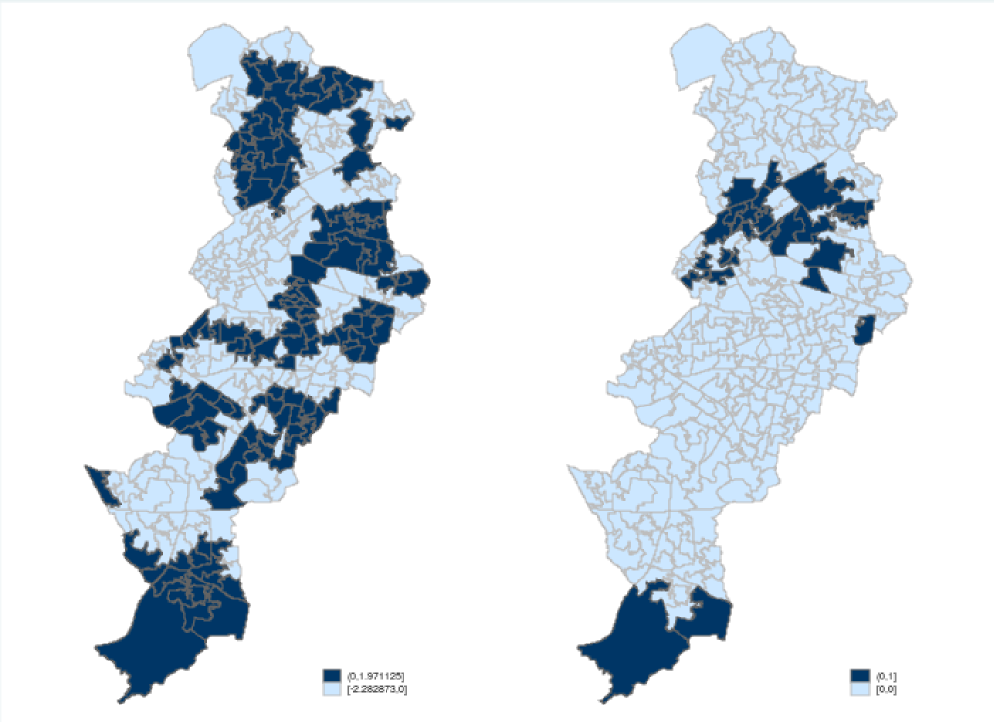
Notes: The figure shows the time-line of data building. In January of the last year of primary school (Year 6) the Pupil Census reports the address of students and each pupil is matched to his three nearest secondary schools (and corresponding distances). One year after, the Census reports the information relative to the secondary school attended and the variable "quality of school attended" is determined.

FIGURE 1.A2: School quality and FSME students distribution by neighbourhood. City of Manchester



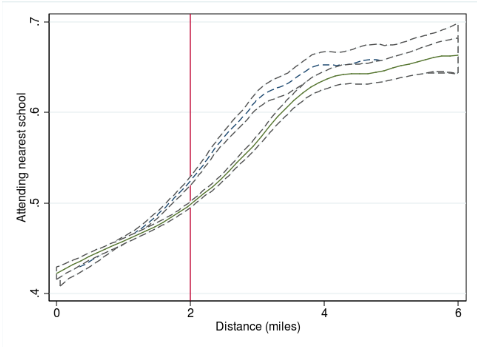
Notes: Author's calculations on PLASC data for the period 2004/2005-2010/2011. The map on the left shows the difference in the quality of the second nearest and the nearest school (ΔQ) by LLSOA (Lower Layer Super Output Area). The maps on the right shows the proportion of FSME students by LLSOA. The dark areas represent regions with a level above the median, the lighter below.

Figure 1.A3: School quality and eligible students distribution by neighbourhood (FSME only). City of Manchester

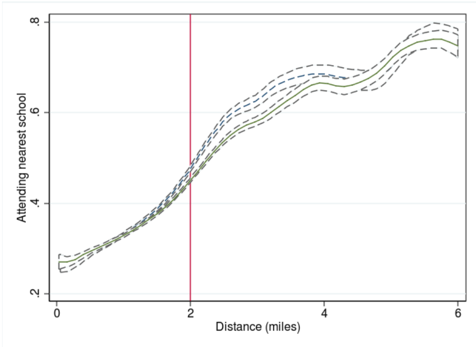


Notes: Author's calculations on PLASC data for the period 2004/2005-2010/2011. The map on the left shows the difference in the quality of the second nearest and the nearest school (ΔQ) by LLSOA (Lower Layer Super Output Area). The maps on the right shows the proportion of FSME students by LLSOA. The dark areas represent regions with a level above the median, the lighter below.

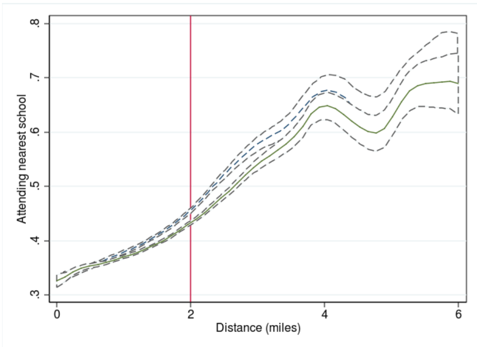
FIGURE 1.A4: Heterogeneous effects: probability of attending the nearest school by distance to the second nearest school



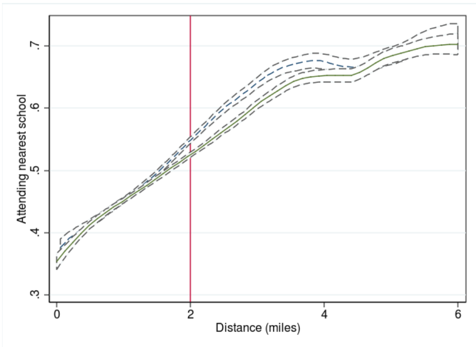
(a) $\text{Qual}_2 > \text{Qual}_1$



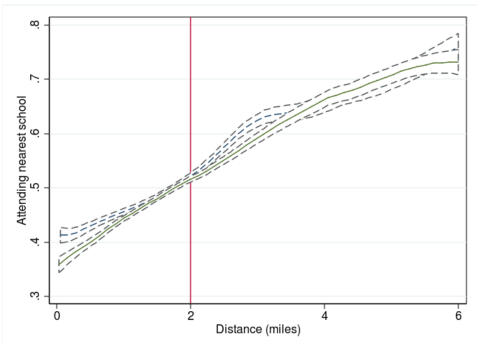
(b) $\text{Qual}_2 < \text{Qual}_1$



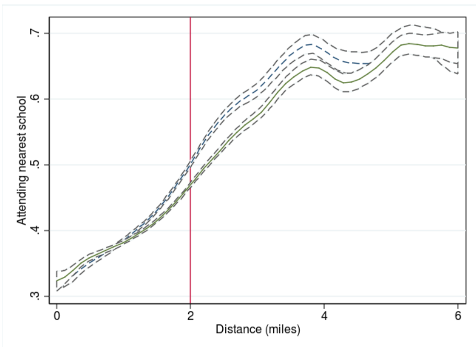
(c) Urban areas



(d) Rural areas



(e) Less deprieved



(f) More deprieved

Notes: Author's calculations on PLASC data for the period 2004/2005-2010/2011. Local mean smoothing with 95% confidence interval. The dashed lines refer to the pre policy period, the solid lines to the post policy period.

TABLE 1.A1: Estimates for the sample of non-FSME students

	Attend:			**School quality:**
	$School_1$	$School_2$	Other schools	Test scores
	[1]	[2]	[3]	[4]
$2 < dist_1 < 3$	0.002	0.001	-0.003	-0.008
	(0.003)	(0.002)	(0.004)	(0.005)
N	2,547,817	2,547,817	2,547,817	2,512,175

Notes: OLS estimates of the following equation: $y_{ipt} = \gamma_0 + \gamma_1 T_{pt} + \eta_p + \eta_t + \omega_{ipt}$, where T_{pt} is equal to 1 if the nearest school is between 2 and 3 miles form home in the post reform period and η_p are postcode fixed effects. See text for details.

TABLE 1.A2: Estimates for the sample including students with the nearest school above 2 miles from home

	Attend:			**School quality:**
	$School_1$	$School_2$	Other schools	Test scores
	[1]	[2]	[3]	[4]
	-0.013	0.010*	0.003	-0.022**
	(0.007)	(0.005)	(0.008)	(0.007)
N	458,008	458,008	458,008	454,570

Notes: see table 1.4.

Chapter 2

School Competition and Performance

2.1 Introduction

In recent years there has been great emphasis on how school choice can act as a costless tool to promote school quality. The idea has been particularly pushed in the UK, where there is increasing attention on families' right to choose the preferred school (Le Grand, 1991, 1993, 2003; Machin and Vignoles, 2006). Advocates of this model claim that it helps improving market efficiency through two channels. First, it boosts students' outcomes trough a better matching of pupils to schools. Second, it generates competition among institutions to attract students, implying that schools failing to meet parental preferences on teaching quality, among other things, may face the risk of closing. Clearly, this happens if funding is closely related to enrolment, i.e. if schools have a financial incentive to attract additional students.

On the other hand, opponents to greater choice argue that it results in higher students' turnover, with potentially detrimental effects on teachers' performance (Hanushek et al., 2004). Moreover, choice could come at the cost of increased school segregation, with the result that the gains may be disproportionally concentrated in wealthier schools. They hence argue that "leveling the playing field" by appropriate resource based policies may be a more promising tool to promote quality.[1]

Empirical evidence on the effects of school choice and competition is, however, not unanimous. In her pioneering study, Hoxby (2000) uses natural boundaries in metropolitan areas as instruments for school choice, showing that greater choice translates into higher school productivity and less private schooling.[2] Promising findings come also from studies exploring the effect of charter school penetration in the US on public school students' outcomes (Bifulco and Ladd, 2006; Carnoy et al., 2006; Booke et al., 2008). The evidence is less conclusive, however, in studies exploring the competition effect generated by openings of private and religious

[1] There is little evidence, however, of resources being effective in improving school outcome. Early evidence comes from the so-called "Coleman Report" (Coleman, 1966), arguing that the effect of additional resources on student achievement is small compared to the impact of family background. Over the years, however, many studies have criticized the Coleman's Report, arguing that its findings could not be interpreted as causal (see, among the others, Bowles and Levin, 1968, Kain and Hanushek, 1972, Hanushek, 1979 Sorenson and Morgan, 2000). Since then, several studies have confirmed the modest role of school resources in determining student achievement (Barr and Dreeben, 1983 and Hanushek, 1996). Nonetheless, evidence is mixed and other studies find a significant positive effect on pupils' performance. For instance, Murnane (1975) reports that a broad range of school resources are positively associated with student outcomes, suggesting that relatively small increases in spending may lead to significant gains in test scores. Similarly, Greenwald et al. (1996) find that an increase in school spending is associated with higher students' achievements. With respect to the UK, evidence mostly focuses on primary schools and generally suggests the presence of a positive relationship between resources and student outcomes, though the magnitude of the effect varies significantly across studies (Holmlund et al., 2010; Machin and McNally, 2011). Similar evidence comes from studies making use of natural experiments. Among the others, Machin et al. (2007) explore the effect of higher school spending in City Technology Colleges, finding a positive impact on students' English and science test scores, but not in mathematics. More recently, Machin et al. (2010) evaluate the impact of resources on students' outcome in the context of a policy initiative targeting inner-city secondary schools. Results show a positive impact on school attendance and performance in mathematics. For a complete summary of past literature see Machin and McNally (2011). The literature is more unanimous on the effects of a more targeted increase in school resources, such as raising the teacher-pupil ratio and promoting teachers' quality. In his pioneering study, Krueger (1999) explores the effects of a randomized experiment assigning students to reduced size classes in grades 1 through 3 (the project is widely known as the Tennessee STAR Project). He finds that attending smaller classes improved student performance in the standardized tests and that this gain grew in the subsequent years. More recently, Chetty et al. (2011) investigate the effects of the same programme on long term outcomes, finding that students assigned to small classes are more likely to attend college and exhibit significant improvement on other long-term outcomes. Positive effects on both test scores and future earnings are found also with respect to English schools (Dearden et al., 2002b; Iacovou, 2002) (for additional evidence on the impact of class size on student achievement refer to Finn et al. (2005), Muennig et al. (2011) and Angrist et al. (2015)). Finally, direct evidence on the beneficial impact of good teachers comes from Gordon et al. (2006) showing that the average student assigned to a teacher who was in the bottom quartile of the grade distribution during his or her first two years loses on average 5 percentile points relative to other students. In contrast, the average student assigned to a top-quartile teacher gaines 5 percentile points. Lastly, Chetty et al. (2014), who shows how students assigned to better instructors are more likely to attend college, earn higher salaries, and are less likely to have children as teenagers.

[2] In a comment to this paper, however, Rothstein (2007) challenges Hoxby's results.

schools, finding from significant beneficial (Hoxby, 1994; Couch et al., 2003; Card et al., 2010) to zero effects (Arum, 1996; Jepsen, 2003). A different strand of the literature explores the effect of school vouchers to attend private schools on public school performance. Direct evidence on voucher-based competition comes from a study of Chilean schools by Hsieh and Urquiola (2006). Using comparisons across municipalities, they find no significant relationship between private school enrolment and average district gains in school performance. Consistent with theoretical analyses by Epple and Romano (1998) and Nechyba (2000), however, they show that the introduction of vouchers led to an increase in the stratification of socio-economic status (SES) groups across schools. With respect to the UK, most of the literature focuses on the indirect effects on competition induced by the compulsory publication of school performance tables. These studies emphasize how competitive pressure resulting from increased accountability improves school performance in terms of students' achievement (Levacic, 2004; Burgess et al., 2010). A second approach evaluates the effects of secondary school admission reforms, like the introduction of free parental choice, finding a positive but modest impact on school performance (Bradley et al., 2000, 2001). Finally, Gibbons et al. (2008) show that higher competition among schools does not have beneficial effects in terms of school outcomes, except potentially for a small positive effect among faith schools.[3]

This paper aims at shading additional light on the potential impact of a choice model on school performance. In particular, it explores the effect of the Free Transport policy discussed in chapter 1, which *de facto* increased school choice among FSME students, on schools and schools behaviour.

The chapter proceeds by first exploring the effect of the increase in school choice on enrolment. Consistent with the results showed in chapter 1, I find that enrolment declines more in schools affected by the policy. I then investigate whether incentives are powerful enough for competition to have potential beneficial effects on school performance. Specifically, for this to be the case, school resources should be closely linked to enrolment, or, in other words, "money should follow the pupil". Despite this being the principle regulating school funding in the UK, both anecdotal and statistical evidence suggest that the mechanism is far from

[3] Figure 2.A2 in the Appendix provides descriptive evidence of the relationship between school choice and school quality in England. It shows the average school choice index by LA on the left and the average school quality by LA on the right.

perfect. Nonetheless, I find evidence that the decrease in enrolment in most affected schools leads to a almost proportional change in available funds, providing supporting evidence that resources do follow pupils.

In the second part of the paper I focus more directly on the effect of the Free Transport policy on school performance, measured as the GCSE test scores of students in Year 11. Results show a positive and significant effect on school quality; however, estimates are economically modest.

This chapter unfolds as follows: in sections 2.2 and 2.3 I briefly discuss the institutional background and present basic descriptive evidence. Sections 2.4 introduces the empirical strategy. Sections 2.5 shows results of the effect of the policy on enrolment, resources and school quality. Section 2.7 explores heterogeneous effects across schools. The last section summarizes and concludes.

2.2 Background

The analysis carried out in this chapter strongly relies on the institutional background introduced in chapter 1 (refer to section 1.2). In what follows I will hence focus only on how resources are allocated to schools, a crucial determinant of whether higher competitive pressure may have an effect on school performance.

2.2.1 School funding and teachers' pay

The process of school funding involves four main stages. Initially, the central Government sets the total amount of revenues to be allocated to education spending in England. This budget is set in the context of the spending review cycle, which usually takes place every 2 or 3 years. The Government then divides the total amount allocated into the proportion to be spent for schools (roughly 70 % of the total) and to be spent on other education sectors, such as universities and further education.

At the next stage, the Department of Education and Skills passes the funds for schools to the Local Authorities. This is divided into different grants, each allocated according to different criteria and subject to different constraints on how the money can be spent. Local Authorities then retain the share of the school budget devoted to "central services" which are provided directly to students, including Pupil Referral Units and Special Education needs.[4] The remaining part is transferred to the school individual budgets according to a "fair funding formula", which has the aim of providing the same amount of resources to schools with similar characteristics.[5]

Though formulae vary, the Government imposes precise constraints on Local Authorities. Importantly, Local Authorities need to allocate at least 75% of the resources based on pupil numbers.[6] Local Authorities then set the other criteria for allocation of funds. Typically, these include: indicators of social deprivation (for example the number of pupils who are FSME), the number of pupils with special education needs (SEN), the number of pupils with English as an additional language, site and school factors (such as square meters taken up by school buildings).

Though funding is primarily based on school intake, it is worth mentioning that statistical evidence suggests that resources are correlated more with historical levels of enrolment and deprivation rather than with yearly changes. For instance, a report from the CfBT Education Trust (Sibieta et al., 2008) shows that an increase in the number of FSME students enrolled in a school brings little, if any, extra funding above the basic per-pupil amount. However, though the mechanism is not perfect, it seems safe to say that it is closely resemble a quasi-market environment in which resources are allocated on the basis of parents' demand.

Teachers' pay is also closely related to performance. Starting from 1999, the UK Government introduced a performance-related pay scheme for teachers, with pupils' attainment as one of its key criteria. Specifically, to be eligible for bonuses and apply for higher salaries, teachers have to demonstrate that they had reached acceptable standards in five areas including knowledge and understanding of teaching, teaching management and assessment, wider professional effectiveness professional characteristics and pupil progress.

[4] In the UK, a Pupil Referral Unit (PRU) is an establishment maintained by a local authority which is specifically organised to provide education for children who are excluded, sick, or otherwise unable to attend a mainstream or special maintained school.

[5] The funding through the fair funding formula makes up roughly 80% of school budget, while the remaining 20% comes from specific grants from the central Government. Examples include: the School Standards Grant, the School Development Grant and other Standard Fund grants.

[6] Generally speaking Local Authorities provide flat-rate amounts for the number of pupils in each Key Stage, with more provided for later Key Stages. There are significant variations in the absolute and relative amounts provided to different Key Stages, but secondary schools generally receive more than primary schools for an additional pupil.

2.3 Data

The data used for the empirical analysis are in large part analogous to the one used in chapter 1 of this thesis, where I provide a detailed description. I here summarise only the main characteristics of the datasets, focusing on the the most relevant for the purpose of this chapter. I use the Pupil Level Annual Census (PLASC), a Census of English state school pupils, covering roughly 95% of the total population of students. I focus on students in their transition between primary and secondary school (i.e. between Year 6 and Year 7). The dataset reports information on a number of individual characteristics, such as ethnicity, gender, first language, special education needs status and eligibility for free school meals. Importantly, it reports pupils' postcode of residence in the last year of primary school and the unique identifier for the school attended.

Second, I use administrative data on English secondary schools reporting the exact address of the establishment to assign to each student his two nearest schools. I determine students' eligibility for free transport computing the walking distance from their postcodes to each of their three nearest schools. The dataset also reports information on basic school characteristics, such as opening and closure dates, total number of students and teachers, phase of education and type of school. Unfortunately, information on total number of students enrolled and number of teachers is available only for academic years 2005/2006-2008/2009.

As in chapter 1, I use the National Pupil Database (NPD) on students in Year 11 to construct a measure of school performance. The data report individual standardized test scores (GCSE) in all subjects for the years 2004/2005-2010/2011. For the purpose of this study, I define school quality as the average of English and mathematics test scores in the last year of secondary school and standardize it yearly to have a mean of zero and a unit standard deviation. It is worth mentioning that, in using test scores of students who were already enrolled at the time of the reform, I alleviate the concern stemming from changes in student body composition resulting from the policy change.

Finally, I use publicly available data on school budgets to obtain information on school yearly revenues and expenditures. The data provide school balances and those as a percentage of school total revenues. From these I am able to infer school total revenues and spending. As a shortcoming, I am not able to infer revenues and expenditures when schools have a balance of zero (roughly 6% of the total sample of schools).

2.3.1 Schools' characteristics

Panel A of table 2.1 reports schools' characteristics.[7] As shown in chapter 1, more than 50% are community schools, which are controlled and financed directly by the Local Authorities. A large share of the remaining schools are foundation schools run by a governing body or a charitable foundation, which has primary responsibility for hiring staff and admitting students. On average, each school enrols roughly 150 new students every year, going from a minimum of 2 in the bottom decile of the distribution to almost 275 in the largest schools.

The next row of Panel A reports school quality. There is large variation in school performance: the best schools perform 1.4 standard deviations better than the average and above 2 standard deviations above the bottom decile.

Finally, the last two rows report school per-pupil revenues and spending. Schools receive £4,988 per student and spend on average £4,766 per student. However, as mentioned, per-pupil resources depend highly on students' characteristics and the area deprivation index. This implies that there is high heterogeneity in the actual amount of resources available. Schools in the bottom decile of the distribution receive on average £2,825 per students, compared to the almost £8,000 received by those in the top 10%. Similarly, school expenditures vary largely, with richest schools spending almost £5,000 more per student than the ones in the bottom decile.[8]

Panel B of the table shows schools' pupil composition. In the average school 80% of first year students are white British, more than 88% are English speakers and roughly 20% of them are FSM eligible. Students' characteristics vary widely, suggesting that there is significant sorting of pupils across schools. The share of white British students enrolled in the first year goes from 16% in the bottom percentile to a maximum of over 98%. Similarly, in 10% of schools the proportion of students speaking English as a first language is 36%, while in the top 10% of the distribution is very close to 100%.

Student composition also varies greatly in respect of parental income. In the wealthiest schools, the percentage of FSME pupils is as low as 1%. On the other hand, more disadvantaged schools enrol nearly 60% of FSME students.

[7] This table is partially borrowed from chapter 1 of the thesis.

[8] Figure 2.A3 in the Appendix shows the average amount of per-pupil spending by LA on the left and the average school quality by LA on the right.

Overall, these figures suggest that English schools are highly heterogeneous in terms of performance, pupils' composition and available resources.

2.4 Empirical strategy

The identification of the effects of school competition on performance relies on the arguably exogenous shock generated by the Free Transport policy. I define the treatment variable at the school level as the percentage of students eligible for free transport in a school catchment area. Empirically, I estimate a reduced form differences-in-differences model comparing schools with different shares of eligible students before and after the programme. To this end, I define eligible students as in chapter 1 and a school catchment area as all children due to enrol in secondary school for whom the school is the closest from home.[9] Ignoring other covariates, the empirical specification takes the form

$$y_{it} = \delta_0 + \delta_1(FT_i * d_t) + \delta_2(FT_{-i} * d_t) + \eta_i + \eta_t + \upsilon_{it} \tag{2.1}$$

where FT_i is the percentage of students eligible for free transport in school i's catchment area and d_t a dummy for observation post reform. The variable FT_i is measured pre treatment (the average of years 2004/2005-2006/2007) to avoid potential bias due to migration. Similarly, the term FT_{-i} is the percentage of students due to enrol in secondary school living in the catchment area of schools other than school i who are eligible for free transport to school i. This term aims at controlling for the decrease in competitive pressure due to students from other catchment areas who could now potentially flee to school i. The outcome variable y_{it} is either school enrolment, school resources or a measure of school performance, defined as above. Finally, the terms η_i and η_t are school and time fixed effects, respectively.[10] The parameter δ_1 identifies the effect of the programme.

[9] Figure 2.A1 in the Appendix reports the average share of eligible students by Local Authority.

[10] One could argue that, as the student composition may itself change as a result of the policy, the estimates of school performance may be biased. However, it is worth emphasising again that the analysis on school performance relies on the test scores of students who hence enrolled in secondary school before 2008 and were not affected by the Free Transport Policy in their choice of school. Though students could potentially change school during their studies in order to benefit from the free transport, this seems highly unlikely. To test this directly, table 2.A1 in the Appendix shows the results of a regression of the form:

$$x_{it} = \gamma_0 + \gamma_1(FT_i * d_t) + \gamma_2(FT_{-i} * d_t) + \eta_i + \eta_t + \nu_{it}$$

The key identifying assumption of the differences-in-differences requires that, in the absence of the policy, changes in the outcome variables would have been the same before and after 2007/2008 in schools with a high and a low fraction of eligible students. Unfortunately, this hypothesis is not directly testable. As an attempt to control for potential latent trends in school behaviour, in the regressions I include a vector of school observable characteristics, including the fraction of white British pupils, the fraction of FSME students, the fraction of native English students enrolled in Year 11 and the total number of students in the school catchment area. Finally, I include Local Authority X time fixed effects, which absorb all time variant characteristics at the Local Authority-year level.

2.5 Results

2.5.1 Enrolment

This section begins by testing whether the introduction of free transport has an effect on school enrolment, i.e. if the increase in competition pressure on schools results in an actual decline in student numbers. Note that, while chapter 1 estimates the *differential* probability of enrolling at the nearest school as a result of the Free Transport policy, this chapter focuses more directly the general equilibrium effects on schools. In particular, it is not obvious that, as eligible students are less likely to attend the nearest school in the post reform period (as shown in chapter 1), these schools would actually experience an overall decline in enrolment.

Table 2.2 shows the estimates of equation 2.1 on school total enrolment in the first year of secondary school (columns 1 to 4) and on FSME students enrolment (columns 5 to 8) in the first year of secondary school. In the first specification (columns 1 and 5) I control only for the number of students in the catchment area, time fixed effects and school fixed effects. Results show a clear negative effect of the policy on schools' first year enrolment, with a coefficient of 0.470 (significant at the 1% level). The same regression on the enrolment of FSME students shows that a fair share of the decline in enrolment is due to a decrease in the number of FSME pupils.

where x_{it} are the characteristics of students attending the last year of secondary school in school i at time t. Estimates suggest that the (observable) characteristics of last year students did not change as a result of the programme, speaking in favour of the fact that no migration took place.

These results imply that an increase of 1 percentage point (p.p.) in the school's percentage of eligible student in their catchment area results in a decrease in first year total enrolment of 0.47 students. Similarly, a 1 p.p. increase in the percentage of eligible students is associated with a 0.13 decrease in the enrolment of FSME in the first year. To give a sense of the magnitude of these estimates, this implies that moving from the average of 5 to 7 students eligible for free transport in a school catchment area will result in a decline in first year enrolment of 0.47 students, 0.13 of which are FSME.

Consistently, both coefficients on the term $FT_{-i} * d_t$, capturing the potential increase in enrolment after the reform, are positive and on the order of 0.056 and 0.021, respectively. This implies that an increase of 1 p.p. increase in the percentage of eligible students in other schools' catchment areas should lead to an increase of 0.056 in total enrolment and 0.021 increase in FSME enrolment. However, estimates are not precise and non significant at conventional levels.

As mentioned, the crucial assumption of the differences-in-differences strategy is that, in the absence of the programme, changes in the outcome variables would have been the same in schools with high and low shares of eligible students. Hence, some caution is needed in interpreting the results in columns 1 and 5, as the specification may not fully control for potential latent trends in the outcome variable. In other words, the estimates could capture effects other than the genuine impact of the programme. In an attempt to alleviate this concern, the specifications in columns 2 and 6 control for school time varying characteristics. Reassuringly, all coefficients are unchanged. As a further test, specifications in columns 3 and 7 include, in addition to school fixed effects, Local Authority X time dummies to control for Local Authorities time specific effects. Though the specification is highly demanding, the estimates on both total enrolment and FSME student enrolment remain significant and similar to the ones prsented above. A 1 p.p. increase in the percentage of eligible students is associated with a 0.43 decrease in first year total enrolment and a 0.14 decrease in FSME student enrolment. The fact that coefficients do not change significantly after the inclusion of Local Authority X time fixed effects speaks in favour of the validity of the identification strategy. Finally, columns 4 and 8 show results of the most saturated specification including school fixed effects, Local Authority X time fixed effects and school characteristics. As before, the inclusion of additional controls does not have a significant impact and estimates are similar to the ones in columns 3 and 7.

Overall, results suggest that the free transport programme had a non negligible impact on the enrolment rates of affected schools.

2.5.2 School resources

As discussed at length above, a significant share of school funding follows the pupil, implying that large schools should enjoy higher resources. An additional fraction of school funding is allocated depending on students' characteristics, being considerably higher if they are from a deprived background or if they have special education needs.[11]

This section explores precisely the effect of the programme on both total and per-pupil revenues, spending and teacher/pupil ratio for the academic years 2005/2006-2008/2009, years for which both data on school resources and teachers' numbers are available. The number of observations in the sample decreases from above 17,000 to just above 11,000. This is due, along with the missing years 2004/2005 and 2009/2010-2010/2011, to the to the limitations of the data, which do not allow to compute revenues and expenditures for schools reporting a balance of zero (these account for roughly 6% of the sample).[12] [13]

As one might be worried that observations are not missing at random, table 2.3 investigates the margin of selection. Specifically, it reports results of a regression of a dummy for missing values on the treatment variable, using the same specifications of table 2.2. All coefficients are zero e not statistically significant, suggesting that selection is not a major source of concern. An additional issue is that the restricted sample of schools used for this analysis may not be representative. Table 2.A2 in the Appendix shows that the results on school enrolment and school performance are virtually unchanged when restricting to schools with no missing values in revenues/expenditures, reassuring on the fact that the schools used in this analysis are still representative of the whole sample.

Column 1 of table 2.4 reports the estimates on school log revenues, using the most saturated specification of table 2.2, which include school characteristics, school

[11]The English funding system has often been compared to the US voucher system, positively discriminating in favour of more disadvantaged students. See, for instance, Le Grand (2003).

[12]See section 2.3 for details.

[13]Though information on school revenues and expenditures is available also for years 2004/2005 and 2010/2011, for consistency I restrict the sample to the years for which the number of teachers and students is available. Estimates on revenues and expenditures including all available years do not change significantly.

fixed effects and Local Authority X time fixed effects. Interestingly, an equally sized negative effect emerges on both school revenues and spending, suggesting that school funding did adapt to the new (lower) enrolment. Specifically, a 1 p.p. increase in the percentage of eligible students leads to a 0.2% decrease in the amount of total revenues and expenditures. This implies that an additional 2 students eligible for free transport in the school catchment area result in a decline of roughly £9,500 in school revenues and of £9,100 in expenditures. Considering that the average per-pupil revenues and spending are, respectively, £4,988 and £4,766, these results provide evidence on how school resources are actually closely linked to enrolment numbers. Column 3 shows the effect of the programme on the number of school full time equivalent teachers. Results suggest that the policy has a significant negative impact on the total amount of teaching resources in the school, with a coefficient of -0.08. In particular, 25 additional students eligible for free transport in the catchment area would result in roughly one less teacher employed in the school, suggesting that schools do downsize their staff in response to a decline in the enrolment numbers. Overall, these results support the argument that school resources are allocated depending largely on student numbers. Importantly to the purpose of this study, this suggests that schools do indeed have financial incentives to attract additional students and may hence respond to higher competition pressure by improving their performance in terms of teaching quality.

As an additional check, columns 4 to 6 report the estimates on the log per-pupil revenues, log per-pupil spending and teacher-pupil ratio. Per-pupil revenues and spending are defined as the total amount of yearly revenues and expenditures divided by the total number of pupils enrolled in the school. The teacher/pupil ratio is defined as the number of full time equivalent teachers per pupil enrolled at the school. A 1 p.p. increase in the fraction of eligible students is associated with a 0.1% increase in per-pupil revenues and spending. Consistent with the results in columns 1 to 3, these estimates imply a nearly zero change in per-pupil revenues and expenditures. Finally, column 6 confirms that the policy reform did not have significant effects on schools' teacher-pupil ratio.

Overall, these results suggest that the decrease in enrolment induced by the policy has a significant impact on school total resources, consistent with a resources allocation process strictly linked to student numbers.

2.5.3 Performance

The crucial question of the chapter is whether the increase in the competitive pressure generated by the Free Transport policy has any effect on the performance of affected schools in terms of students' test scores.

It is worth stressing again that school quality is based on the test scores of students who were already enrolled in the school *prior* the implementation of the Free Transport programme. Indeed, if we were to assess performance changes based on newly enrolled students, we may capture compositional effects induced by the policy (i.e. changes in student body composition), rather than the genuine effect of the Free Transport programme.

The last column of table 2.4 reports the results for school quality, using the most saturated specification of table 2.2. Estimates imply that a 1 p.p. increase in the percentage of eligible student increases school performance by 0.004 standard deviations, corresponding to roughly 2 additional students, would boost school performance by 0.4% of a standard deviation. Though these estimates might, at first sight, seem large, one should consider that extending free transport to the totality of FSME students (from 3% to 15% of school total enrolment), would still imply a modest increase of 4.8% of a standard deviation in school performance.[14]

2.6 Falsification test

The identification strategy relies on the assumption of no latent trends in the outcome variables. In an attempt to control for this, I included in the regressions school fixed effects and Local Authorities X time fixed effects, which absorb all time invariant characteristics at the school level and time varying characteristics at the Local Authority level. However, there might still be time variant trends at the school level which are not taken into account in the estimates.

In an attempt to rule this out, table 2.5 presents a falsification test for the outcome variables presented so far.

[14]Figure 2.A4 in the Appendix shows the residuals of a regression of school quality on time, school and Local Authority X time fixed effects. The graphs reports on the x-axis the fraction of eligible students in the school potential pool of first year enrolment and on the y-axis the change in performance after the implementation of the programme. There is a clear positive relationship between the change in school quality and the percentage of students eligible for free transport living near the school.

As discussed in chapter 1, the Free Transport policy does not target students residing in London and as such we should not observe any significant effect on the enrolment and quality patterns of London schools.

Table 2.5 shows the estimates for schools based in Greater London, using the same specification of table 2.2. All coefficients are virtually zero, suggesting that the policy reform does not have a significant effect on the outcomes of London schools, speaking in favour of the validity of the identification strategy.

2.7 Heterogeneous and non-linear effects

Table 2.6 reports the estimates of school quality by type of school. As mentioned, there are several types of secondary schools in England, which differ in the degree of management freedom. The main types of schools are community schools, controlled by the local council; foundation schools, voluntary controlled and voluntary aided school, and academies. While for community and voluntary aided schools the Local Authority is directly responsible for selecting the staff, in voluntary, foundation and academy schools the governing body is in charge of hiring and student admissions.[15] Intuitively, schools with higher freedom should be more likely to respond to competition and improve their performance than other schools. I hence explore heterogeneous effects depending on school managerial freedom, dividing the sample of schools into "independent" schools (i.e. foundation schools, voluntary controlled schools and academies) and "other" (i.e. community and voluntary aided schools).

Columns 1, 2, 5 and 6 of table 2.6 show the estimates for total enrolment and FSME enrolment for the two sub-samples of schools. The results are slightly larger for schools with more management freedom, suggesting that they suffered more from the increase in competitive pressure compared to other schools.

Columns 3 and 4 report the estimates of school quality. The coefficient is significantly larger for schools with management freedom than for other schools. A 1

[15] Foundation schools' land and buildings are owned by the governing body or by a charitable foundation. Voluntary Aided schools are linked to a variety of organisations. A large share of them are faith schools (typically the Church of England or the Roman Catholic Church). The charitable foundation contributes towards the capital costs of the school (typically 10%), and appoints a majority of the school governors. Voluntary Controlled schools are schools with the land and buildings often owned by a charitable foundation. However, the local authority employs the schools' staff and has primary responsibility for admissions.

p.p. increase in the percentage of eligible students in schools with more management freedom is associated with a 0.008 standard deviations increase in quality, compared to a 0.003 increase in other schools. Overall, these results suggest that schools that enjoy higher management freedom respond more to competitive pressure, though estimates are still economically modest.

2.8 Summary and conclusions

This chapter explores the effects of the Free Transport Policy described in chapter 1 on school behaviour. The policy aimed at increasing school choice for low income student, by decreasing the cost of travelling to further away schools. Advocates of choice argue that higher choice should increase competition among schools, leading to an improvement in school performance. However, this argument is valid as long as schools have incentives to attract students, typically because the amount of resources available is closely linked to enrolment numbers. This chapter explores precisely these margins.

Using the same data as in chapter 1, I define a school catchment area as the number of children in the last year of primary school (i.e. those due to enrol at secondary school) for whom the school is the nearest from home. The competition measure assigned to each school is then computed as the average percentage of children living in the catchment area who are eligible for free transport to further away schools. To avoid bias due to students' migration in response to the policy, I use a pre treatment variable, that is, the average for the pre reform years. I identify the effect of the programme through a reduced form differences-in-differences approach comparing schools with a high percentage of eligible students with schools with a low percentage of eligible students before and after the implementation of the Free Transport policy.

Consistent with results in chapter 1, estimates show that enrolment declines more in schools with a high percentage of eligible students in the catchment area. Moreover, resources are closely linked to pupil numbers, suggesting that schools do have financial incentives to attract additional students. Nonetheless, the increase in competition has only a modest effect on school performance. This might be due to the limited "bite" of the policy under analysis, which targets only a small share of low income students. On the other hand, one could also think that the beneficial effects of increased competition are more likely to emerge in the long run, when schools had the time to adjust their behaviour. However, though both arguments are valid, a modest effect is consistent with the previous literature, suggesting that competitive pressure may not be a powerful tool to improve school quality.

Tables and figures

Table 2.1: School characteristics

	All schools	Bottom decile	Top decile
Panel A: Schools			
Number of schools	3,323		
Community schools (%)	50.23		
Academies (%)	7.52		
Foundation schools (%)	23.14		
Voluntary schools (%)	2.29		
Other schools (%)	16.28		
Number of new enrolments	147.19	2.07	274.86
Average exit cohorts' test scores	0.22	-0.83	1.64
Per-pupil revenues (£)	4,988	2,825	7,933
Per-pupil spending (£)	4,766	2,699	7,616
Panel B: Students' composition			
White British (%)	79.24	15.58	98.23
FSME (%)	19.29	1.33	56.63
Females (%)	48.72	10.59	90.45
English speakers (%)	88.49	36.00	99.86

Notes: Author's calculations on PLASC data. The table reports summary statistics for the period 2004/2005-2010/2011. School quality is defined as the average of GCSE test scores in math and English. It has been standardized yearly to have a mean of zero and a unit standard deviation.

TABLE 2.2: The effect of the Free Transport policy on school enrolment

	Enrolment:					FSME enrolment:		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
$FT_i * d_t$	-0.470***	-0.467***	-0.432***	-0.430***	-0.129**	-0.130**	-0.139**	-0.140**
	(0.111)	(0.112)	(0.113)	(0.113)	(0.056)	(0.054)	(0.064)	(0.064)
$FT_{-i} * d_t$	0.056	0.055	0.084	0.083	0.021	0.023	0.024	0.025
	(0.070)	(0.070)	(0.063)	(0.062)	(0.021)	(0.021)	(0.018)	(0.018)
(Log) number of students	0.131***	0.131***	0.120***	0.120***	0.021***	0.021***	0.018***	0.018***
in catchment area	(0.018)	(0.019)	(0.017)	(0.017)	(0.005)	(0.004)	(0.004)	(0.004)
% White British		-1.846		-2.643		1.733		0.881
		(3.138)		(3.017)		(1.569)		(1.479)
% English speakers		0.846		2.084		-1.513		-1.333
		(3.261)		(2.947)		(1.498)		(1.369)
% FSME		-8.396		-4.818		15.195***		10.223**
		(5.464)		(5.308)		(5.078)		(4.338)
Time Fixed Effects	X	X	X	X	X	X	X	X
School Fixed Effects	X	X	X	X	X	X	X	X
LA-Time Fixed Effects			X	X			X	X
N	17,210	17,210	17,210	17,210	17,210	17,210	17,210	17,210

Notes: OLS estimates. Clustered (at the school's Local Authority level) standard errors in parenthesis. FT_i is the percentage of students in school i catchment area (i.e. all students for whom the school is the nearest) who are eligible for free transport. This is measured as the average for the pre policy period (2004/2005-2006/2007). FT_{-i} is the percentage of students schools $-i$ catchment areas who are eligible for free transport to school i. This is measured as the average for the pre policy period (2004/2005-2006/2007).

Table 2.3: Sample selection: missing values in revenues or expenditures

	[1]	[2]	[3]	[4]
$FT_i * d_t$	-0.000	-0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$FT_{-i} * d_t$	-0.000	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
(Log) number of students	-0.000	-0.000	-0.000	-0.000
in catchment area	(0.000)	(0.000)	(0.000)	(0.000)
% White British		0.033		0.039
		(0.040)		(0.037)
% English speakers		-0.061		-0.050
		(0.037)		(0.035)
% FSME		0.171*		0.159*
		(0.089)		(0.089)
N	17,210	17,210	17,210	17,210

Notes: See table 2.2. The dependent variable is defined as a dummy equal to 1 if school i displays missing values in the revenues and expenditures in year t.

TABLE 2.4: The effect of the Free Transport policy on school resources and quality

	(Log) revenues	**(Log) spending**	**School Teachers**	**(Log) per-pupil revenues**	**(Log) per-pupil spending**	**Teacher-pupil ratio**	**School quality**
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
$FT_i * d_t$	-0.002*	-0.002*	-0.080**	0.001	0.001	0.000***	0.004***
	(0.001)	(0.001)	(0.036)	(0.001)	(0.001)	(0.000)	(0.001)
$FT_{-i} * d_t$	0.000	0.000	0.016	-0.000	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.011)	(0.000)	(0.000)	(0.000)	(0.000)
(Log) number of students	-0.000	-0.000	0.006***	-0.000***	-0.000***	-0.000***	0.000
in catchment area	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)	(0.000)	(0.000)
% White British	0.052	0.053	1.669	-0.022	-0.022	-0.001	-0.035
	(0.044)	(0.043)	(1.147)	(0.041)	(0.039)	(0.001)	(0.024)
% English speakers	0.126***	0.139***	3.808**	0.014	0.026	-0.001	0.001
	(0.042)	(0.044)	(1.807)	(0.039)	(0.036)	(0.001)	(0.034)
% FSME	0.050	0.046	-2.259	0.092*	0.089	0.000	-0.709***
	(0.056)	(0.058)	(2.060)	(0.055)	(0.060)	(0.001)	(0.079)
N	11,043	11,043	11,043	11,043	11,043	11,043	16,939

Notes: See table 2.2. School quality is defined as the average of GCSE test scores in math and English. It has been standardized yearly to have a mean of zero and a unit standard deviation.

TABLE 2.5: Falsification test- Schools in London

	Enrolment	**FSME enrolment**	**School quality**	**(Log) per-pupil revenues**	**(Log) per-pupil spending**	**Teacher-pupil ratio**
	[1]	[2]	[3]	[4]	[5]	[6]
$FT_i * d_t$	-0.104	-0.059	0.001	-0.000	-0.000	0.000
	(0.125)	(0.055)	(0.001)	(0.001)	(0.001)	(0.000)
$FT_i * d_t$	0.007*	0.003	-0.000***	-0.001	-0.001	0.000
	(0.004)	(0.002)	(0.000)	(0.001)	(0.001)	(0.000)
(Log) number of students in catchment area	0.022	0.025**	0.000	0.000	0.000	-0.000
	(0.015)	(0.012)	(0.000)	(0.000)	(0.000)	(0.000)
% White British	9.620	-6.680	0.056	0.196	0.207	0.003
	(7.092)	(5.378)	(0.092)	(0.127)	(0.126)	(0.003)
% English speakers	-1.539	-0.798	0.013	-0.114*	-0.113*	-0.003
	(3.492)	(3.005)	(0.066)	(0.062)	(0.061)	(0.003)
% FSME	5.198	26.788***	-0.301**	0.058	0.035	-0.010**
	(10.960)	(7.596)	(0.119)	(0.087)	(0.092)	(0.004)
N	2,616	2,616	2,586	1,658	1,658	1,658

Notes: See table 2.2.

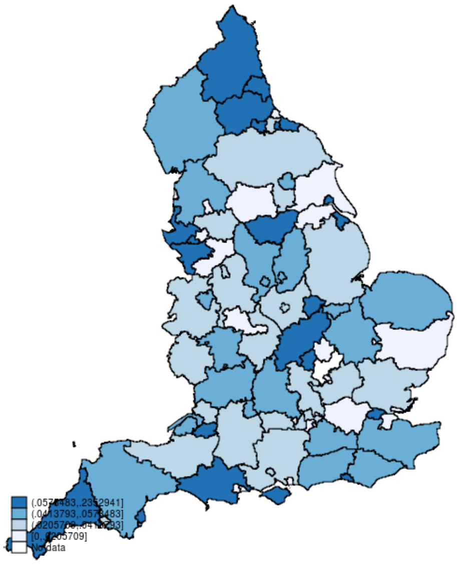
TABLE 2.6: Heterogeneous effects by school managerial freedom

	INDEPENDENT:			OTHER:		
	Enrolment	**FSME enrolment**	**School quality**	**Enrolment**	**FSME enrolment**	**School quality**
	[1]	[2]	[3]	[4]	[5]	[6]
$FT_i * d_t$	-0.528***	-0.435***	0.008***	-0.376***	-0.052	0.003**
	(0.196)	(0.147)	(0.002)	(0.142)	(0.074)	(0.001)
$FT_{-i} * d_t$	0.078	0.006	0.001	0.096	0.026	-0.000
	(0.114)	(0.037)	(0.001)	(0.001)	(0.025)	(0.001)
(Log) number of students in catchment area	0.109***	0.015	0.000	0.124***	0.018***	0.000
	(0.037)	(0.009)	(0.000)	(0.019)	(0.004)	(0.000)
% White British	-1.695	0.406	-0.104*	-2.949	0.492	-0.010
	(4.596)	(1.797)	(0.061)	(3.694)	(2.042)	(0.030)
% English speakers	1.894	-1.055	-0.001	1.915	-0.932	-0.032
	(4.388)	(1.947)	(0.059)	(3.417)	(1.702)	(0.037)
% FSME	-4.447	17.607*	-0.587***	-3.232	8.156*	-0.671***
	(10.718)	(9.645)	(0.116)	(6.514)	(4.499)	(0.109)
N	4,945	4,945	4,909	12,265	12,265	12,030

Notes: See table 2.2. Independent schools include foundation schools, voluntary controlled schools and academies. Other schools include community and voluntary aided schools.

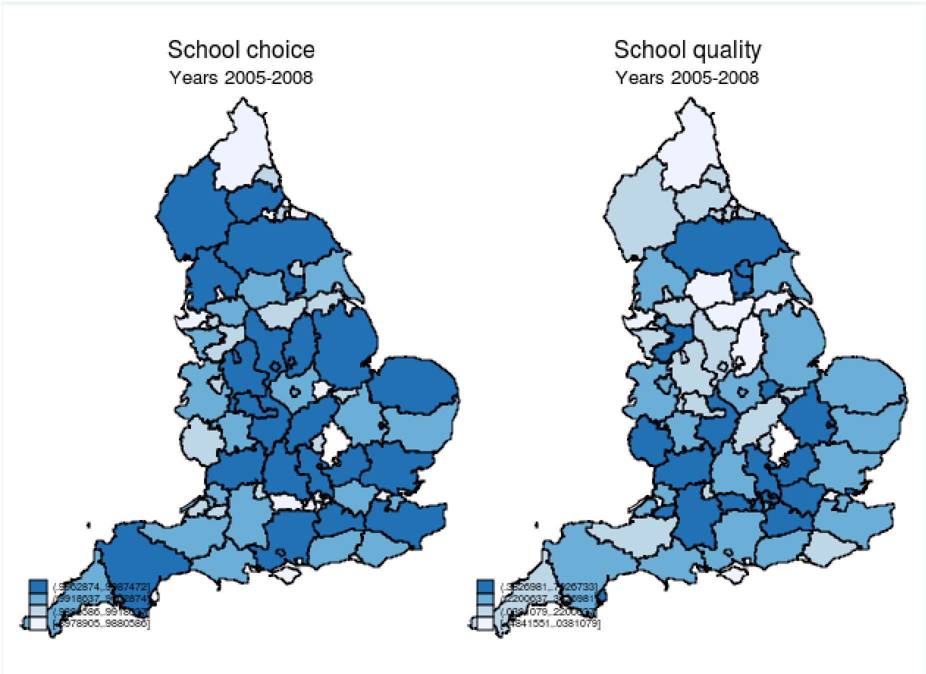
Appendix: Supplementary Figures and Tables

FIGURE 2.A1: Fraction of eligible students by LA (quartiles)



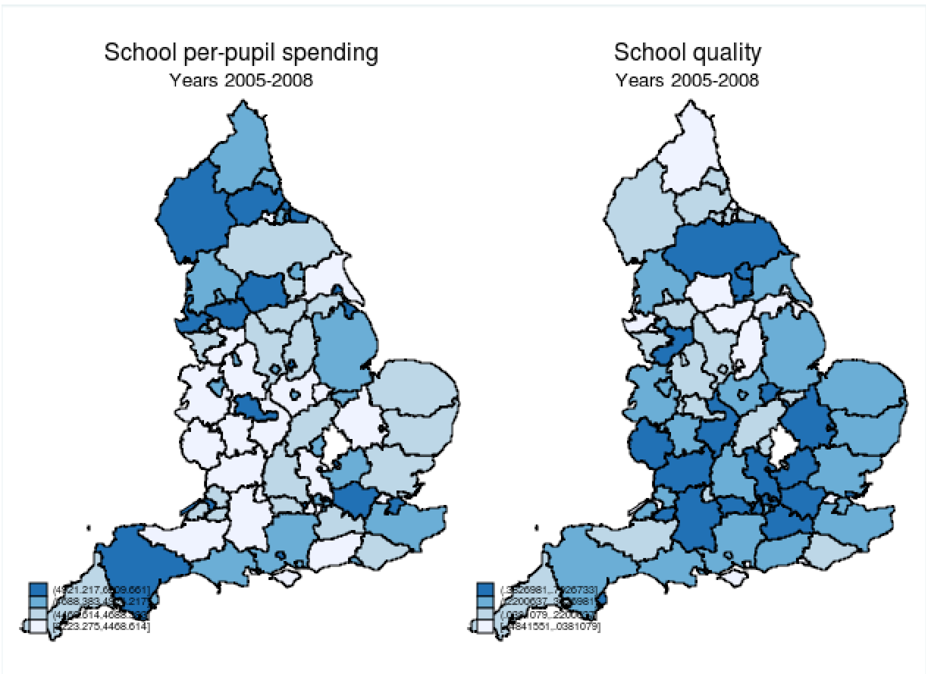
Notes: Author's calculations on PLASC data for the period 2004/2005-2010/2011.

FIGURE 2.A2: School choice index and school quality by LA (quartiles)



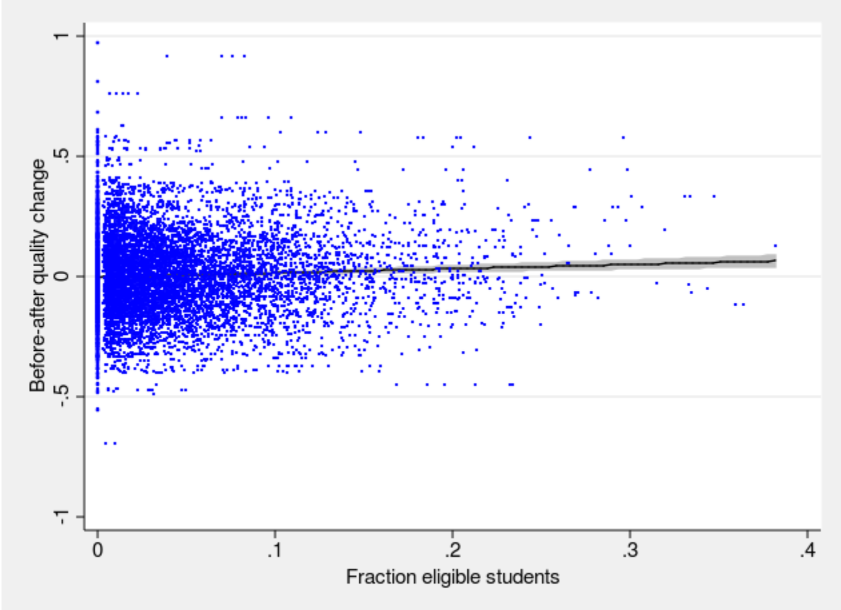
Notes: Author's calculations on PLASC data or the period 2004/2005-2010/2011. The school choice index is based on a Herfindhal index of schools' shares of the Local Authority's total enrolment: $1 - H_l = 1 - \sum_{i \in l} s_{il}^2$, where $s_{il} = \frac{enrolment_i}{enrolment_l}$.

FIGURE 2.A3: School per-pupil spending and school quality by LA (quartiles)



Notes:Author's calculations on PLASC data for the period 2004/2005-2010/2011.

FIGURE 2.A4: Change in school quality by share of eligible students (2005-2011)



Notes: Before-after change in school quality. Residuals from a regression with year, school and Local Authority X time fixed effects. 95% confidence intervals.

Table 2.A1: The effect of the Free Transport policy on school characteristics (Year 11)

	White British [1]	English speakers [2]	FSME [3]
$FT_i * d_t$	0.001	0.000	0.000
	(0.000)	(0.000)	(0.000)
$FT_{-i} * d_t$	-0.000**	0.000	-0.000
	(0.000)	(0.000)	(0.000)
N	17,210	17,210	17,210

Notes: See table 2.2.

TABLE 2.A2: The effect of the Free Transport policy on enrolment and quality: selective samples

	Sample non missing observations quality			**Sample non missing observations revenues and expenditures**		
	Enrolment	**FSME enrolment**	**School quality**	**Enrolment**	**FSME enrolment**	**School quality**
	[1]	[2]	[3]	[4]	[5]	[6]
$FT_i * d_t$	-0.423***	-0.138**	0.004***	-0.358***	-0.104	0.004***
	(0.114)	(0.064)	(0.001)	(0.100)	(0.076)	(0.001)
$FT_{-i} * d_t$	0.087	0.025	0.000	0.072	0.030	0.000
	(0.063)	(0.018)	(0.000)	(0.058)	(0.019)	(0.000)
(Log) number of students	0.119***	0.018***	0.000	0.121***	0.013***	0.000
in catchment area	(0.017)	(0.004)	(0.000)	(0.018)	(0.005)	(0.000)
% White British	-3.395	0.525	-0.035	-1.640	-0.553	-0.035
	(3.255)	(1.720)	(0.024)	(3.648)	(2.322)	(0.025)
% English speakers	0.963	-1.626	0.001	2.844	-1.134	0.002
	(3.489)	(1.671)	(0.034)	(3.734)	(1.799)	(0.034)
% FSME	-5.417	10.298**	-0.709***	0.975	7.037	-0.707***
	(5.717)	(4.738)	(0.079)	(6.555)	(5.988)	(0.079)
	(0.052)	(0.106)	(0.044)	(0.123)		
N	16,939	16,939	16,939	11,043	11,043	10,876

Notes: See table 2.2.

Chapter 3

Externalities at the Workplace: Evidence from the Italian Private Sector

3.1 Introduction

Does the work environment affect workers' productivity? Do absenteeist co-workers increase or decrease our absenteeism? The aim of this chapter is to provide evidence of the impact of group effects on workers' propensity to shirk.

When workers are rewarded based on team effort the possibility arises that individuals free ride. This reduces workers' incentives to exert effort, lowering overall productivity. Kandel and Lazear (1992) suggest that the presence of social effects, defined in their paper as peer pressure or shame, could help mitigate the detrimental effects of free riding, even in the absence of explicit economic incentives.[1] In their pioneering study Rasul et al. (2005) show evidence of social preferences by comparing workers' productivity under different pay schemes. They find that average productivity is sensibly lower under relative incentives, where individual effort imposes negative externalities on others, than under piece rate, where it does not. More recently, in an influential paper Mas and Moretti (2009) use high frequency data on cashiers of a large supermarket's to explore how the productivity of a worker is affected by the productivity of co-workers in the same shift. They find evidence of positive productivity spillovers and conclude that social pressure

[1] Past literature shows that economic incentives can restore the efficient outcome. On the role of economic incentives see, among the others, Hamilton et al. (2003).

is the main channel explaining their results.[2] Differently from the analysis in this chapter, however, their paper focuses on a specific setting where employees work individually, rather than on the more common case of team work. Moreover, results on the US may be difficult to extend to the European labour market, where the employment protection legislation is more stringent and possibly the work ethic different.

In order to investigate externalities in shirking, I use a large dataset on social security administrative records, which covers a representative sample of all private sector employees in Italy. Obviously, shirking is not recorded in the data. However, in using data for Italy, where sick leave is covered by public insurance, I circumvent the problem using workers' absence rate as a proxy. Indeed, there is a large amount of evidence that workers absences for sick leave often cover shirking.[3] Among others, Riphahn and Thalmaier (2001) show that German employees are more likely to take sick leave when job security increases. This is also found by Ichino and Riphahn (2005) using data on a large Italian bank.[4] In the context teachers' labour market, Jacob (2010) explores the effects of a policy change in Chicago's public schools, which gave principals more flexibility in dismissing probationary teachers, showing an average decrease in absence rates of 10% in the post reform period. Finally, specifically looking at Italy, D'Amuri (2011) investigates the effects of the reform of the disciplinary action on sick leave among public sector workers. The reform aimed at lowering absenteeism through financial penalties (the loss of every bonus for the first 10 days of sick leave) and stricter monitoring. Comparing private and public workers' absenteeism before and after the reform, he finds that the implementation of the policy yielded a 26% decrease in sick leave taken by public sector workers.[5]

[2] Other studies attempt to measure group effects employing lab experiments. See, for example: Falk et al. (2005); Falk and Ichino (2006) and Falk et al. (2013).

[3] There is a large body of literature (see, for example, Gruber, 2011) documenting the benefits of sick leave insurance. Among others factors, insurance makes workers more likely to engage in risky activities, to the advantage of economic production and growth. In addition, sick leave insurance is desirable also from the firms' prospective, as it reduces the contagion effects due to presenteeism (sick employees showing up at work).

[4] More generally, Lusinyan and Bonato (2007) compare a number of developed countries, showing the presence of a significant U-shaped relationship between the development of the welfare system and absence rates.

[5] For additional evidence on the unintended consequences of extensive social security refer to the literature on unemployment insurance (UI) in the US. For instance, Meyer (1990) and Gruber (1997) report that the share of workers exiting unemployment increases from 5-7% to 16.5% once the UI benefits run out. Other studies on the unintended consequences of social insurance include: Lazear (1990) on the impact of job security provisions on unemployment; Acemoglu and Shimer (1999) on the optimal level of unemployment insurance; Acemoglu and Angrist (2001) on the effects of the American Disabilities Act and Krueger and Meyer (2002) on the effects of social insurance on labour supply.

This chapter contributes to the literature on peer effect and absenteeism by investigating directly group effects in shirking. There are three main challenges in estimating social effects: the reflection problem, correlated effects and endogenous sorting (Manski, 1993). The former is the classic problem of simultaneous equation: co-workers' behaviour affects individual behaviour, but the opposite is also true. Correlated effects arise when individuals behave similarly because they share the same institutional environment and face common shocks. Finally, workers may sort endogenously into occupations and firms according to their unobservable characteristics, such as ability and motivation. Each of them can give rise to a bias in the estimates of the endogenous effects.

To deal with these issues, the identification strategy relies on arguably exogenous variation in co-workers' productivity induced by compositional changes in firms' employment due to new hirings. In the spirit of Mas and Moretti (2009), I use new co-workers' permanent absenteeism, which I recover from a regression of workers' absenteeism on individual characteristics, as a proxy for current co-workers' shirking. This addresses the simultaneity issue and reduces the risk of bias due to workers facing common shocks. I finally attempt to control for endogenous sorting by explicitly controlling in the regression for a rich array of workers' characteristics.

The empirical results show evidence of the presence of positive spillovers, with workers increasing their absence rates by at least 0.23 p.p. in response to a 10 p.p. increase in coworkers' sick leave.

The paper unfolds as follows: in sections 3.2 and 3.3 I describe the institutional background and present basic descriptive evidence. Section 3.4 introduces a simple model of teamwork with externalities. Sections 3.5 and 3.6 present the identification strategy and show results on the estimated spillovers effects. Section 3.7 explores sanctioning for shirking. The last section summarises and concludes.

3.2 Institutional Background

Sick leave insurance in Italy works through two channels: social security (*INPS, Istituto Nazionale della Previdenza Sociale*) and collective agreements (*CCNL, Contratti Collettivi Nazionali di Lavoro*). INPS provides sick leave indemnity to all salaried workers with the exception of managers and white collar workers employed in the industry sector.[6]

[6]For these workers is the firm to pay the indemnity, according to the collective agreements.

In order to be eligible for public insurance, both the firm and the employees have to pay a mandatory tax of respectively 32.7% and 9.19% of the workers' gross salary.[7] Social security covers all sick leave episodes between 4 and 180 days, remitting 50% of the gross monthly wage for the first 16 days and 66.66% for the remaining period. Most of the collective agreements include an additional payment (in charge of the employer) of up to 100% of the salary and extend the payment to the first three days of sick leave. Only when the employer or INPS can directly prove shirking, is the indemnity automatically suspended and the employer is legally entitled to dismiss the worker without notice. Alternatively, a firm can legally dismiss workers if they extend sick leave beyond the period allowed by the collective agreements (*periodo di comporto*), which usually exceeds the 180 days covered by social security.[8]

Though absenteeist workers represent a significant monetary cost for firms, it not easy for firms to dismiss absenteeist employees. Italy, like many European countries, displays a stringent employment protection legislation (OECD, 1999).[9] After a probationary period of three months, the firm is entitled to dismiss its employees only for "just cause", which the law defines as "significantly inadequate fulfilment of the employee's tasks specified in the contract" (Law No. 604 of 1966). Employment courts ultimately evaluate the cause on the basis of the evidence provided by the firm, which thus faces the risk of long and costly trials. In the absence of just cause, the firm is forced to re-hire the employee and pay the whole amount of lost wages and contributions plus a fine, amounting to 200% of lost social security. Alternatively, the worker has the option of resigning, subject to a refund of a minimum of 15 months pay. The law is slightly less stringent concerning firms with fewer than 15 employees. In cases of unfair dismissal, workers are entitled to receive a payment of up to a maximum of six months of wages, without any right to re-employment. Ichino et al. (2003) offer compelling evidence of the implications of these dismissal costs by showing that, in 17 years, a large Italian bank dismissed just over 400 workers out of a labour force of 26,000 employees.

In summary, the legislation regarding sick leave has important implications for this study. Full insurance and stringent employment protection, along with the impossibility for the employer of perfectly distinguishing sick leave from shirking,

[7]The payment, which is compulsory for workers employed in the private sector, also entitles the employee to pensions, disability indemnities, maternity leave payments and low income benefits

[8]To avoid strategic behaviour, the *periodo di comporto* applies to all episodes close in time, for which sick leave days are counted as if they were a single episode.

[9]For a detailed description of the Italian employment protection legislation see, among others, Ichino and Riphahn (2005).

imply that workers will have incentives to take advantage of sick leave. Hence, in the context analysed here, absences due to sick leave are likely to represent a good proxy for shirking.

3.3 Data

This paper uses data on Italian workers employed in the private sector in the period 1990-2002. The panel includes 146,806 workers randomly selected on the basis of their date of birth (1$^{\text{st}}$ of March and 1$^{\text{st}}$ of October) and reports information on sick leave spells and a rich set of demographic characteristics (including gender, year of birth, province of birth, gross monthly earnings, province of work and workers' CCNL).[10]

Despite providing detailed information on workers' characteristics, the dataset has some drawbacks. First, and most importantly, sick leave is defined as a dummy for whether the worker was absent at any time and for any number of days in the year. This implies that I am not able to infer the intensive margin of workers' absenteeism. Second, the sick leave variable does not include episodes shorter than three days, as they are not covered by social security. Specifically, I only observe sick leave episodes for which the worker is required to provide a medical certificate. This may undermine the identification of shirking, as absenteeist workers could strategically avoid longer periods of absence to decrease the probability of being caught.

Finally, the dataset covers only a sample of private sector workers. As I do not observe the universe of workers, I may not be able to provide a good description of co-workers' composition. Moreover, observing only a random sample of co-workers induces an error ridden measure of peers' absenteeism at the workplace. I address this issue and suggest a possible solution in the following sections.

3.3.1 Sick leave and workers' characteristics

Table 3.1 shows the probability of taking sick leave by worker characteristics. The unit of observation is the worker employed in a given establishment in a given

[10] As Italian sick leave insurance does not cover managers and white collar workers employed in the industry sector, I dropped these workers from the original dataset. This decreases the sample to a total of 145,168 workers.

year, for which the table reports means for the period 1990-2002. Panel A focuses on demographic characteristics and panel B on jobs' characteristics. The whole sample counts 1,090,034 worker-establishment-year observations in the period of the analysis. 14.25% of male workers take sick leave at least once every year, compared to only 11.89% of females. Looking at the geographic distribution of workers taking sick leave, workers born in the North display slightly higher levels of absenteeism compared to workers in the South. Similarly, more than 14% of workers employed in the Northern regions are absent at least once in the year, compared to only 13% in the Center and 11% in the South.[11]

Blue collars workers are significantly more likely to take sick leave than white collar workers with a probability in the order of 18% compared to only 4% of white collar employees. This may follows from the fact that, given the nature of the occupation, blue collar workers are usually at a higher risk of injuries and illness. On the other hand, as blue collars make a large share of a firm's workforce, monitoring their effort may be difficult and the probability of punishment for shirking could be low. This may push them to take advantage of sick leave.

Unsurprisingly, high tenured workers display higher absence rates compared to low tenured ones. As high tenured workers are protected against dismissal by law, the risk of repercussions is reasonably small, providing incentives to increase absences. Interestingly, however, the opposite relationship emerges with respect to workers' experience, as more experienced workers are roughly 3 percentage points less likely to take sick leave.

3.3.2 Movers and incumbents

In the remainder of this section I present descriptive statistics on the sample of workers that my analysis focuses on. As stated before, the identification strategy of this paper relies on the exogenous change in coworkers' composition within an establishment that follows the introduction of a new worker (*mover*). This decreases the sample to the 32.35% of workers who have at least one new co-worker in the period of the analysis.

Figure 3.1 shows the average number of new co-workers per incumbent worker per year in the data, conditional upon having a new co-worker. On average, more than 70% of incumbents has no more than one new co-worker in a year, slightly more than 10% are joined by 2 new co-workers and the remaining 20% by more than 2.

[11] These figures contrast with the findings of Ichino and Maggi (2000).

Table 3.2 reports the descriptive statistics for the incumbents and the movers. Incumbent workers are more than double as likely to take sick leave as movers, displaying a probability of nearly 15% compared to 7% of movers. However, this may be partially explained by the higher number of months worked per year among stayers, theirs being almost twice those worked by movers. Movers are also younger and on lower wages than incumbents, as well as less experienced.[12]

Looking at workers' region of birth and work, there is evidence of significant migration from the Southern to the wealthier Northern regions of Italy: while only 66% of incumbents and 62% of movers were born in the North, 83% of incumbents and 85% of movers are employed there.

3.4 A model of teamwork with externalities

I start from a simple model of teamwork shedding light on the incentives to shirk in the presence and absence of externalities. Consider a setting with 2 identical workers A and B and assume that there are no group effects.[13] Worker A maximizes his expected utility, given by the wage minus the cost of exerting effort. I assume that workers get paid a fixed salary and have a probability $P<1$ of keeping their job, which depends on their own and co-workers' effort. Note that the implications of the model do not change if I assume that workers are paid based on a function of their own effort; however, pay-to-performance schemes are quite rare in the Italian context. Defining effort as $e = 1 - a$, where a is the chosen level of absences, worker A maximizes his expected utility

$$U_A(e_A, e_B) = wP(e_A, e_B) - C(e_A)$$

where w denotes the fixed wage, $P(e_A)$ is the probability of keeping the job and $C(e_A)$ is the cost function. Assume further, as standard in the literature, that $P_1(e_A, e_B){>}0$, $P_{11}(e_A, e_B){<}0$, $C_1(e_A) > 0$ and $C_{11}(e_A) > 0$. The optimal e_A is determined by the first order condition

$$wP_1(e_A, e_B) = C_1(e_A)$$

[12]By construction, movers display 0 years of tenure.

[13]The setting is similar to the one suggested by Hölmstrom (1981)

As my empirical strategy exploits an exogenous variation in co-workers' effort, the partial derivative of optimal e_A with respect to e_B determines worker A's optimal response to a marginal change in the co-worker's effort

$$\frac{\partial e_A^*}{\partial e_B} = \frac{-wP_{12}(e_A, e_B)}{wP_{11}(e_A, e_B) - C_{11}(e_A)}$$

As $P_{11}(e_A, e_B)<0$ and $C_{11}(e_A) > 0$, the denominator of the expression above is unambiguously negative. The sign of the numerator depends on the sign of the cross derivative P_{12}. Consider the simplest case in which the two workers are substitute in the production function and the probability of retaining the job depends on the total level of output. In this case the cross derivative P_{12} will be negative, implying that worker's A marginal returns to effort in terms of probability of keeping the job decrease as the effort of worker B increases. As a result, an increase in worker B's effort will have a negative impact on worker A's optimal level of effort, leading to the classic free riding problem.[14]

Consider now the case in which workers' own cost of effort depends on co-workers' effort. For instance, workers may find it costly to exert effort when the average level of effort is low; in other words, they may be more willing to work harder when the working environment is productive. In the presence of such group effects, workers maximize the following utility function:

$$U_A(e_A, e_B) = wP(e_A, e_B) - C(e_A, e_B)$$

I assume $C_{12}(e_A, e_B) < 0$, implying that worker A's marginal cost of effort is decreasing the higher the level of worker B's effort. Worker A's optimal level of effort is given by the FOC

$$wP_1(e_A, e_B) = C_1(e_A, e_B)$$

The partial derivative of optimal effort with respect to e_B is

$$\frac{\partial e_A^*}{\partial e_B} = \frac{C_{12}(e_A, e_B) - wP_{12}(e_A, e_B)}{wP_{11}(e_A, e_B) - C_{11}(e_A, e_B)}$$

[14] If the two workers are complements in the production function the implications would be the opposite. In this case the cross derivative $P_{12}(e_A, e_B)$ will be positive, implying that workers' effort increases with the average effort in the establishment.

The denominator of this expression is still negative. Focusing again on the case of substitute workers, $C_{12} < 0$ and $P_{12} < 0$, implying that the numerator can take both positive or negative sign. Hence, the sign of the cross derivative is ambiguous, implying that an increase in worker B's absenteeism could result either in an increase or a decrease in worker A's optimal level of effort.

In summary, the model has two main implications. First, in the absence of externalities, we should observe free riding. Second, externalities mitigate free riding and, if strong enough, could completely offset it.

3.5 Empirical Strategy

Consider a world with only two workers. Following the literature on spillovers, workers' absenteeism can be described by an equation of the form

$$y_{ift} = \alpha_i + \beta_1 y_{-ift} + d_t + d_f + \epsilon_{ift} \tag{3.1}$$

where y_{it} is worker's i absence rate in year t, y_{-ift} is the co-worker absenteeism, d_t are time fixed effects, d_f are firm fixed effects and ϵ_{ift} is an individual level error term. The term α_i defines individual fixed effects capturing the time invariant component of worker i's absenteeism. The parameter β_1 identifies the endogenous effects, that is how co-workers' absenteeism directly affects individual behavior.

However, the above parameter is likely to be biased, as it suffers from at least three sources of endogeneity: correlated effects, reflection problem and endogenous sorting (Manski, 1993).

Correlated effects arise when individuals behave similarly because they share the same background or face a similar institutional environment. Reflexivity relates to the classic problem of simultaneous equations: individual i's behaviour affects individual $-i$'s behaviour, but the opposite is also true. Finally, endogenous sorting arises when workers sort into firms and occupations according to unobservable characteristics. In all three cases, the exclusion restriction $Cov(y_{-it}, \epsilon_{it}) = 0$ will be violated, implying that the estimate of the endogenous parameter β_1 will be biased. Controlling for firm fixed effects, ensures that the estimate of the β_1 is not contaminated by time invariant sorting and correlated effects. However, a remaining threat to the validity of the estimates arises from reflexivity and time variant endogenous sorting.[15]

My empirical strategy consists in exploiting between-job movers' to generate a change in the composition of co-workers in the firm. The panel structure of the dataset allows then to estimate a model analogous to the one employed by Mas and Moretti (2009). The estimation strategy consists of two steps. The first step of my analysis aims at computing the fixed component of movers' absenteeism, net of observable individual characteristics. I estimate the following equation:

$$y_{-ikt} = \alpha_{-i} + X'_{-ikt}\gamma_1 + d_t + \phi_k + \epsilon_{-ikt} \tag{3.3}$$

where y_{-ikt} is the absenteeism of between job movers, X_{-ikt} are workers' individual characteristics, including age, gender, occupation, tenure and experience and ϕ_k is a vector of firms fixed effects for any establishment in which the mover worked. As a second step, I use the estimates of the fixed effects α_{-i} as a proxy for worker $-i$'s absenteeism in firm f at time t and estimate 3.1 in the following reduced form equation

$$y_{ift} = \alpha_i + \delta_1\alpha_{-i} + d_t + d_f + \epsilon_{ift} \tag{3.4}$$

The residual challenge comes from endogenous sorting, implying that workers with a given permanent level of absenteeism are not assigned at random to incumbents. Specifically, the exclusion restriction is violated if movers' permanent absenteeism is correlated with incumbents' or firms' unobservable time varying characteristics which also affect y_{ift}. Although I have no direct way of ruling out non random assignment, I attempt circumvent this problem by probing the robustness of my results to the inclusion of observable workers' characteristics.[16]

[15] To see this, consider that worker $-i$'s absenteeism can be written as

$$y_{-ift} = \alpha_{-i} + \beta_1 y_{ift} + d_t + d_f + \epsilon_{-ift} \tag{3.2}$$

This is the same expression of equation 3.1 for worker $-i$. Reflexivity implies that, as y_{-ift} is itself a function of y_{ift}, $Cov(y_{-ift}, \epsilon_{ift}) \neq 0$. Correlated effects and endogenous sorting imply that $Cov(\epsilon_{-ift}, \epsilon_{ift}) \neq 0$, leading to the violation of the exclusion restriction.

[16] The parameter δ_1 in equation 3.4 identifies the effect of an increase in co-workers' *permanent* absenteeism on incumbents' shirking. This is conceptually different from the interpretation of the parameter β_1 of equation 3.1, capturing the effect of an increase in co-workers' *contemporaneous* absenteeism on incumbents' shirking. The two models are equally plausible and have different interpretations concerning the nature of externalities. However, it is not possible to empirically distinguish between them (Mas and Moretti, 2009).

In a setting with N workers, the specification can be generalized in

$$y_{ift} = \alpha_i + \theta_1 \sum_{m=1}^{M} \frac{\alpha_m}{(N-1)} + d_t + d_f + \nu_{ift} \tag{3.5}$$

where the subscript m denotes between-job movers.

3.6 Results

3.6.1 Baseline Estimates

Table 3.3 presents estimates of equation 3.5 of workers' absenteeism on co-workers' sick leave episodes. Standard errors are clustered at the establishment level. To deal with the fact that absence rates are reported as a dummy 0-1 for the probability of taking sick leave at least once in the year and obtain a more reliable measure of true absenteeism, I define y_{ift} as the probability of taking sick leave in a given year divided by the number of months worked. It is also worth mentioning that the empirical strategy is constructed under the assumption of two workers. This rules out spillover effects among incumbents following the hiring of the new co-workers. However, in a context with N workers, the estimates of equation 3.5 are in reality a combination of a) the direct effect of movers on incumbents and b) spillovers among incumbents. In this sense, one can think at the parameter of equation 3.5 as a "reduced form" effect of movers on incumbents.

Column 1 reports results of a model controlling for individual X establishment fixed effects and time fixed effects. Results show a clear positive effect of movers' sick leave on workers' absence rates, with a coefficient of 0.023 (significant at the 10% level). As absence rates are defined as the probability of taking sick leave in a given year standardized by the number of months worked, these estimates imply that a 10 p.p. increase in average co-workers' absenteeism leads to an increase of 0.23 p.p. in the probability of taking sick leave for the focal worker. Results are consistent with the theoretical model in which workers free ride on peers, but positive externalities on the cost of exerting effort offset the incentives to free ride.

Column 2 of the table attempts to control for potential endogenous sorting. The specification includes a very rich set of workers' predetermined characteristics (that is, measured at time $t-1$), including: age and gender dummies, province of birth dummies, occupation dummies (28 categories, as coded by INPS), monthly log wage, dummies for the total number of co-workers in the sample and tenure and experience dummies. As I do not directly observe workers' true years of tenure and experience, I proxy these with the number of years of tenure and experience in my sample window. To control for left censoring, I add to the regression a dummy for whether the worker was employed in the firm and a dummy for whether the worker was in the labour market in the first year of the sample. Remarkably, results remain virtually unchanged with respect to column 1.

Finally, specifications in columns 4 and 5 control for the number of new co-workers (dummies) and the number of months incumbents have been exposed to at least one mover (dummies). The latter is measured as the number of months both the incumbent worker and the mover were employed in the establishment. These specifications control for potential effect of different exposures to new co-workers, both at the extensive and intensive margin. Both estimates are identical to the ones of columns 1.

Overall, these estimates suggest that new co-workers' absenteeism has a positive and significant effect on own absenteeism of between 0.23 and 0.24 p.p., results being robust to the inclusion of a rich set of workers' background characteristics.

3.6.2 Discussion: sampling error

Sample data can give rise to an attenuation bias in the estimates of endogenous effects. However, it is straightforward to correct for this bias when sampling is random and the true size of the underlying population is known. As the data consist of a random sample of workers drawn on the basis of their date of birth, there is no reason to believe that, conditional upon firms' establishment fixed effects, workers' characteristics should be systematically different from the ones of the underlying population.

Ammermüeller and Pischke (2009) show that in this case the OLS estimator of equation 3.5 can be adjusted for sampling bias in the following way

$$\hat{\theta}_{1Adjusted} = \hat{\theta}_1 \frac{(\overline{N} - 1)}{(\overline{n} - 1)} \tag{3.6}$$

where $\overline{n}$ is the average establishment's sampled size and $\overline{N}$ is the average establishment size in the underlying population. A shortcoming of this approach is that one needs to know the true size of firms' establishment, which is not available from the data. In order to get an estimate of the true $\overline{N}$, I employ social security data on the universe of private workers in the Italian region Veneto for the same years covered by this analysis. These data do not report information on workers' sick leave, but share all other basic features of the dataset used here. I estimate the average firm size by year and use such estimate to adjust the coefficient obtained in the previous section.

The size of the effect increases substantially, suggesting that sampling measurement error plays an important role in biasing the results towards zero. The adjusted coefficient is 0.126, implying that the probability of the average incumbent taking sick leave would increase by 1.26 p.p. in response to a 10 p.p. change in new co-workers' absenteeism.

3.6.3 Heterogeneous effects

Although I assumed that the θ_1 coefficient does not vary with individual characteristics, there is no reason why this should be the case.

In this section I allow θ_1 to vary across workers on the basis of age, gender, occupation and wage as observed at time *t-1*. I also allow all control variables to have different effects depending on the focal workers' characteristics, by running separate regressions for each group. Table 3.4 reports estimates of equation 3.5, reverting to the most saturated specification of column 4 of table 3.3.

In columns 1 and 2 I estimate equation 3.5 separately for white and blue collar workers. Only the coefficient on blue collar workers remains significant at the 5% level, while the one on white collar workers is not significantly different from zero. These results imply that a 10 p.p. increase in movers' sick leave is associated with a 0.3 p.p. increase in blue collars workers' absences.

Columns 3 and 4 repeat this exercise with respect to incumbent workers' age. Young workers are defined as those with an age below the median at time *t-1*. Estimates are significant only for older workers: a 10 p.p. increase in movers' sick leave is associated with a 0.5 p.p. increase in older workers' episodes, while no significant effect emerges in the sample of young workers. Columns 5 and 6 show results for the men-women samples. Estimates are significant only with respect to male workers, showing a 0.4 p.p. increase in absence rates in response to a 10 p.p. increase in movers' sick leave. Overall, results suggest that the presence of social effects in the baseline estimates is mainly driven by blue collar, older and male workers. Finally, columns 7 and 8 explore heterogeneous effects with respect to incumbents' wage: high waged workers, defined as those with a salary above the median, increase their absence rates by roughly 0.4 p.p. in response to a 10 p.p increase in new co-workers' absenteeism, while the coefficient drops to below 0.02 p.p. for low wage workers.

These results may simply reflect the higher propensity of these workers to emulate co-workers, as well as a lower risk of punishment. Some groups of workers may face a lower probability of being caught when shirking and face a lower risk of being sanctioned. For instance, blue collar workers usually form a large share of a firm's workforce which could make monitoring harder. On the other hand, older workers can usually take advantage of a more stringent employment protection, which makes the probability of punishment lower than for younger co-workers. A similar reasoning applies to high wage workers: assuming that wage is a reliable proxy for skills and experience, highly skilled workers may be more difficult to replace and hence less likely to be dismissed when shirking.

An additional, and potentially important source of heterogeneity in externalities if given by firm size. Unfortunately, as the data only cover a sample of workers, I do not observe the true firm size in the data. With this caveat in mind, I estimate the effect separately for firms with, on average, at least 15 workers and for firms with, on average, less than 15 workers over the sample. This gives a fairly balanced split. It is worth mentioning that, given that I have a 1:180 sample, firms with more than 15 (observed) workers are likely to be rather large firms.

Results reported in the last two columns of table 3.4 show that the effect is present only in large firms. Specifically, a 10 p.p. increase in movers' absenteeism is associated with a 0.67 p.p. in incumbents' absence rate, while no significant effect emerges with respect to firms with less than 15 workers (in the sample). One possibility is that workers in large firms are less likely to be punished for shirking. For instance, one extra absent work is likely to be less costly to large firms than to small ones, where production can be severely affected if one employee is missing. Moreover, as the Italian employment protection legislation is more stringent regarding large establishments, workers employed in these firms are less likely to be punished for shirking and hence have more incentives to take advantage of sick leave.[17]

[17] In Italy there is precisely a cutoff at 15 employees for dismissal at will.

3.7 Punishment for absenteeism

The theoretical model introduced above implies that workers' probability of retaining the job depends positively on both own and co-workers' effort. If this was the case, an increase in the average absenteeism in the firm should have a negative effect on the probability of retaining the job. The second panel of table 3.5 reports the estimates for three different measures of dismissals: (1) the probability of being in the labour market one year after the new co-workers' arrival ($t+1$), (2) the probability of being employed in the same firm at time $t+1$ and (3) the probability of being employed in the same firm at $t+1$, conditional upon being in the labour market.

Overall, there is evidence, though weak, of sanction mechanisms: a 10 p.p. increase in the average of new co-workers' absenteeism leads to decrease in the probability of being employed of 1.2 p.p.. Similarly, the effect on the probability of being working in the same firm one year after is in the order of between -0.8 and -1.8 p.p.. However, all coefficients are not statistically significant. One possible explanation for the weakness of the estimates is that the stringent employment protection prevents the employer from freely dismiss workers. On the other hand, it is not possible to exclude the presence of other sanctioning mechanisms, such as, for instance, slower career progression.

3.8 Summary and conclusions

The aim of this paper is to explore workers' response to absenteeist peers. The dataset consists of a random sample of Italian private sector employees, covering the period 1990-2002. A simple theoretical model predicts workers' free riding as long as social effects are not in place. For instance, if the probability of keeping the job is a function of collective effort, an increase in co-workers' absence rates should lead to a decrease in the worker's absenteeism. However, in the presence of externalities we could observe an increase in absenteeism in response to an increase in co-workers' absence rate. To test these hypotheses I use an identification strategy that exploits between-job movers as an arguably exogenous variation in the co-workers' sick leave episodes.

Consistently with the presence of social effects in the spirit of Mas and Moretti (2009), results show an increase in incumbent workers' absence rates in the order of 0.24 p.p. in response to a 10 p.p. increase in new co-workers' absences. Interestingly, there is wide heterogeneity depending on firm size. In particular, externalities are more pronounced in larger firms subject to a more stringent employment protection legislation, suggesting that a lower probability of punishment may push workers to respond more to absenteeist co-workers taking advantage of sick leave.

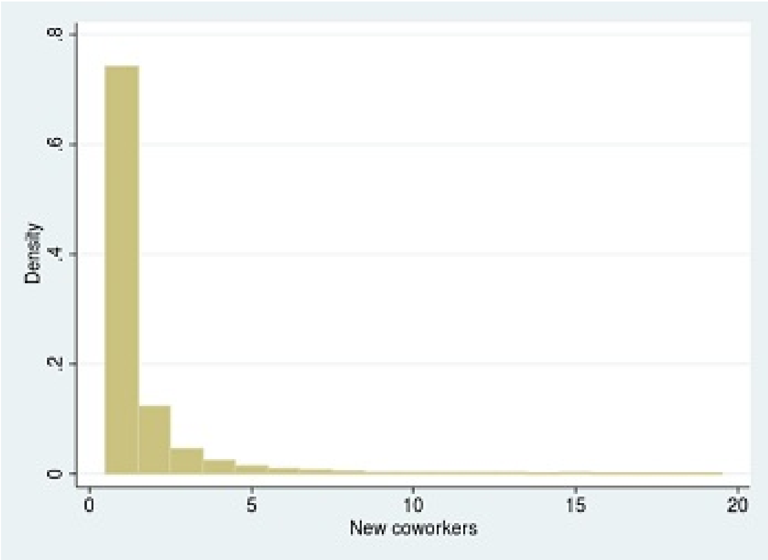
All these results however, are likely to suffer of a sample downward bias. Following Ammermüeller and Pischke (2009), I correct for the bias estimating the average firm size from a dataset collecting information for the universe of private sector workers in the Italian region of Veneto. The adjusted coefficient shows that workers increase their absence rates by 1.26 p.p. in response to a 10 p.p. increase in co-workers' absences, suggesting that measurement error plays a significant role in the analysis.

I then test whether there is evidence of punishment for shirking. In the specific, I estimate the effect of receiving absenteeist movers at time t on the probability of being employed in the same firm the year after (t+*1*). Though there is weak evidence of a negative impact of co-workers' absenteeism on the probability of keeping the job, coefficients are not statistically significant. I cannot, however, exclude the presence of other unobservable sanctioning mechanisms.

One important caveat concerns the interpretation of these results. Social security data only report absences above 3 days. As it is plausible that shirking most likely relates to shorter episodes (an example is the so called "Monday effect"), the results presented in this chapter may capture different mechanisms. For instance, it may be possible that firms employing more unhealthy workers do experience an increase in the probability of all employees being ill. Unfortunately, given the data at hand, it is not possible to rule this out. A second concern is that, given the sample structure of the dataset, the analysis focuses on relatively large firms. Italian firms are generally small and family run and my results might not necessarily extrapolate to the universe of Italian firms, most of which may differ deeply from the ones considered in this chapter. Similarly, the external validity of our findings is limited by the peculiarity of the Italian labor market. These results may hence not apply to less rigid labor markets.

Tables and figures

Figure 3.1: Average number of new co-workers per year



Notes: Author's calculations on INPS data for the period 1990-2002. The figures are conditional on having at least one new co-worker.

Table 3.1: Probability of taking sick leave by worker and job characteristics

	Sick leave=1 (%)	N
Panel A: Workers' characteristics		
Males	14.25	687,134
Females	11.89	402,900
Younger	14.63	574,527
Older	12.26	515,507
North	13.42	683,894
Center	12.77	189,710
South and Islands	13.22	345,798
Foreign	16.16	11,142
Panel B: Jobs' characteristics		
Low wage	14.06	545,017
High wage	12.69	545,017
White collar	4.41	382,922
Blue collar	18.23	707,112
Full time	13.46	1,036,143
Part time	11.73	53,891
North	14.14	844,833
Center	12.95	224,581
South and Islands	10.55	238,759
Small firm	13.77	746,814
Large firm	12.53	343,220
Paid months (≤ 6)	5.94	327,803
Paid months (> 6)	16.58	762,231
Low tenure	10.81	550,798
High Tenure	16.00	539,236
Low experience	15.00	601,576
High experience	12.05	488,458
Total number of obs		1,090,034

Notes: Author's calculations on Italian social security data. The table reports summary statistics for the period 1990-2002. All figures are means. Tenure is defined as the number of years the worker was employed in the same firm in the sample window. Experience is defined as the number of years the worker was in the labour market in the sample period. Wages are expressed as gross monthly wages (in Euros) in real terms. Low tenure, experience, wage and younger workers are defined as below the median. Small and large firms are defined on the basis of the number of co-workers in the sample (below and above the median).

Table 3.2: Descriptive statistics. Movers and incumbents samples

	Incumbents	Movers
Panel A: Workers' characteristics		
Sick leave (%)	14.76	6.98
Males (%)	61.43	59.90
Age	38.91	31.99
North (%)	65.93	62.27
Center (%)	17.81	15.63
South and Islands (%)	30.31	28.68
Foreign (%)	0.71	0.66
Panel B: Jobs' characteristics		
Wage	2,825.77	1,892.66
White collars (%)	46.91	37.88
Full time (%)	95.78	92.23
North (%)	83.42	85.12
Center (%)	20.58	18.91
South and Islands (%)	16.05	14.39
N. of co-workers in the sample	28.06	27.70
Paid months	10.32	5.27
Tenure (years)	3.48	0
Experience (years)	5.37	4.11
N	58,667	30,222

Notes: See table 3.1.

TABLE 3.3: The effect of movers' sick leave on incumbents' absenteeism

	Worker sick leave:			
	[1]	[2]	[3]	[4]
Co-workers sick leave	0.023*	0.024*	0.024*	0.024*
	(0.013)	(0.013)	(0.013)	(0.013)
Time fixed effects	X	X	X	X
Individual-Establishment FE	X	X	X	X
Background characteristics		X	X	X
Number of movers (dummies)			X	X
Months of exposure to movers (dummies)				X
N	58,667	58,667	58,667	58,667

Notes: OLS estimates. Clustered (at the establishment level) standard errors in parenthesis. Incumbents' lagged characteristics include: sick leave, age and gender dummies, province of birth dummies, occupation dummies (28 categories, as coded by INPS), monthly log wage, dummies for total number of co-workers in the sample, years of tenure, years of experience, two dummies for left censoring in tenure and experience.

TABLE 3.4: Heterogeneous effects

	Worker sick leave:									
	By occupation:		**By age:**		**By gender:**		**By (log) wage:**		**By size:**	
	Blue collar [1]	White collar [2]	Old [3]	Young [4]	Male [5]	Female [6]	Low wage [7]	High wage [8]	Small	Large
Co-workers' sick leave	0.032*	0.011	0.051***	0.006	0.038**	0.008	0.038	0.017	0.019	0.067*
	(0.019)	(0.012)	(0.019)	(0.018)	(0.017	(0.021)	(0.024)	(0.017)	(0.014)	(0.038)
N	31,466	27,201	22,627	36,040	24,357	34,310	18,030	40,637	34,775	23,892

Notes: See table 3.3. You workers and low wage are defined as below the median. Small firms are defined as establishments with an average of less than 15 employees in the sample.

TABLE 3.5: Punishment for shirking

	Dependent variable:		
	Employed (t+1)	Employed in same firm (t+1)	Employed in same firm (t+1) (conditional on employed=1)
	[1]	[2]	[3]
Co-workers' sick leave	-0.119	-0.179	-0.077
	(0.082)	(0.132)	(0.114)
N	53,575	53,575	47,723

Notes: See table 3.3. "Employed" is defined as a dummy equal to 1 if the worker is in the labour market at time $t+1$. "Employed in same firm" is defined as: in column (2) as a dummy equal to 1 if the worker is in the same firm at time t and time $t+1$ and 0 otherwise (regardless of whether he is in the labour market) and in column (3) as a dummy equal to 1 if the worker is in the same firm at time t and time $t+1$ and 0 if he is employed in a different firm.

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