Education Resources and Student Performance: Evidence from Arab municipalities in Israel

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Abstract

This paper is offering a new identification strategy in order to estimate the effect of education resources on students' achievements at the municipal level. We use the appointment of an external accountant to financially distressed municipalities in Israel as an instrumental variable for independent education expenditures, and control for selection bias. We find that the decline in independent education expenditures per capita in Arab municipalities did not affect the share of matriculation certification recipients. This result suggests that changes in aggregate education resources might not affect students' achievements.

Keywords: Education resources, students' achievements, municipalities

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I. Introduction

Do education resources affect student performance? The answer to this question would probably be positive when looking on studies conducted at the student or class level. However, much less is known at the school, school district, county or country levels. We provide results which rely on a micro-econometric approach applied to municipal data. We isolate a plausibly exogenous component of education expenses, and find that student achievements are not affected by education resources at the municipal level.

The last two decades brought a multitude of papers aiming at estimating the effect of different education policies on student achievements. These papers are mostly done at the student or class level, where it is easier to identify the exogenous component of the policy, holding student attributes fixed. Many policies were shown to affect student achievements. These include, among others, the effect of class size (Angrist and Lavy 1999, Krueger 1999), teachers' quality (Rivkin et al. 2005), special education programs (Hanushek et al. 2002), monetary incentives to students (Lavy 2008a), and incentive programs for teachers and principals (Lavy 2002, Lavy 2008b, Goodman and Turner 2012).[1]

This great body of literature can help in evaluating the effectiveness of specific programs, which are usually done in a specific setting. However, education practitioners are sometimes interested in a more aggregated assessment, namely the effect of school resources on student performance, or even the effect of education expenditures at the school district, county or country level. However, very few studies are done at aggregated levels and the results suggest a very limited link, if any, between education resources and performance (Hanushek et al. 1996). Although some suggest that

[1] Goodman and Turner (2012) emphasize that the design of incentive programs for teachers can dictate the success or failure of the program.

education resources at aggregated levels increase student performance (Figlio 1997, Lee and Barro 2001) others disagree (e.g. Hanushek et al. 1996, Hanushek and Luque 2003). International comparisons tend to find little, if any, connection between resources and performance (Hanushek & Kimko 2000, Gundlach and Woesmann 2001, Woesmann 2003b, Altinok & Bennaghmouch 2008, Glewwe et al. 2011).

Estimating the effect of economic resources on student achievements at aggregated levels (the "macro" level) is challenging for at least two reasons. Firstly, students' achievements, in addition to being affected by education expenditures, are also affecting them. For example, low test scores might increase voters' demand for education, leading to a rise in education expenditures. Secondly, many forces are at play at the macro level, many of them unobservable, making it extremely hard to isolate exogenous components of education spending.

The econometric challenges mentioned above are but one explanation for the difference between results obtained by micro and macro studies. Another possible explanation comes from differences in the education production function. While micro-level studies tend to focus on specific programs with well defined inputs, macro-level studies tend to analyze aggregate educational resources. However, aggregate educational resources are not merely the aggregation of specific programs (Pritchett and Filmer 1999). Another difference is that macro studies tend to analyze cases where no changes were made to the education production function, while micro studies tend to analyze the effect of new programs which might altered the production function. Yet another potential difference lies in the functional form of the education production function (Figlio 1999). Figlio (1999) claims that school-level studies wrongly assume a production function which is additive. He estimates a translog production function and

finds that school inputs do affect student performance, though the effect tends to be very small.

In order to try and explain the gap between the micro and the macro level we focus on an intermediate level, the municipal level. Israeli municipalities control and run the education system at the local level and their responsibilities are comparable to counties in the US. In addition, in order to avoid endogeneity issues, we use an IV approach. Our instrument for municipal education expenditures is the appointment of an external accountant (EA) to municipalities in financial distress. External accountants tend to cut independent expenditures (Steklov 2008), mainly independent education expenditures. The appointment of an external accountant is determined by the ministry of interior, based on contemporary financial performance of the municipality. Since changes in financial performance are mainly determined by the managerial performance of the mayor, as well as by macro shocks, they are arguably uncorrelated with student achievements. In addition we deal with selection bias - the possibility that municipalities with EA are different than those without an EA. This is done by using municipalities which will be appointed an EA in the following years as a control group for municipalities which were appointed an EA in a specific year.

Using external accountants as an instrument for independent education expenditures we find that education expenditures have no effect on students' test scores, measured by the share of matriculation certification recipients. Our results are in line with other studies at the macro level, which find no effect of education expenditures on test scores. We suggest that the micro-macro divide is not a result of methodological differences. Rather, our findings support the conclusion of Hanushek (2003) that without policy reform additional funding to education would not necessarily result in better outcomes.

There are several caveats to our analysis. Firstly, independent education expenditures are not directed in their entirety to enhance education achievements. The municipalities have to finance school maintenance, cleaning etc., which might not affect students' achievements directly. However, Cellini et al. (2010) find that investments in infrastructure do affect test scores. Secondly, investments in education take time to affect students' achievements. Unfortunately, due to the short time span, we can only analyze the effect of education resources on achievements in the short and medium run. Lastly, unobserved confounding factors, such as parental effort, might affect our results. We assess this issue in section IV.

II. Data

a. Municipal data

We begin with a short description of Israeli local governments. During the sample period there were 252 municipalities in Israel.[2] We use data on the 77 Arab municipalities, for the years 2000-2009.[3] We restrict the sample to Arab municipalities for three reasons. Firstly, the vast majority of external accountants were appointed in these municipalities. Secondly, external accountants which were appointed in Jewish municipalities did not reduce education expenditures. Therefore, external accountants are a very weak IV for education expenditures in Jewish municipalities. Finally, Arab and Jewish municipalities are different in many dimensions (Lavy 1998, Reingewertz 2010, Brender 2005, Ben Bassat and Dahan 2012, Ben Bassat, Dahan and Klor 2012).

[2] During the sample period several municipal amalgamations were made. See Reingewertz (2012) for more details.

[3] Arab (Jewish) municipalities are defined as municipalities with a majority of Arab (Jewish) residents. Arab residents are mainly Muslim, but some are Christians and some are Druze. There are three Arab regional municipalities that are not included in our sample (Abu Basma, El Batuf and Bustan El Marj).

Therefore, Jewish municipalities are probably not an appropriate control group for Arab municipalities, and their inclusion in the analysis might create selection bias. Appendix Table A1 provides further evidence regarding the possibility of selection bias when including Jewish municipalities in the analysis. Columns 1 and 2 provide summary statistics for the main variables, for Jewish and Arab municipalities, accordingly. We can see that in almost all dimensions there are stark differences between these two groups. For example, matriculation rate is 62% in Jewish municipalities but only 52% in Arab municipalities. Columns 3 and 4 compare between municipalities that were not appointed an EA (column 3) and those who were appointed (column 4). Both columns include both Arab and Jewish municipalities. Similarly to the comparison between columns 1 and 2, there are stark differences between these groups. These differences are mainly a result of the differences between Arab and Jewish municipalities.[4]

The budgetary data which are used in this paper are taken from the ministry of interior (see Reingewertz 2010 for an elaborate description of the data). Socio-economic data was taken from the ICBS. Table 1 provides summary statistics of the main variables. Total municipal expenditures in Arab municipalities in 2003 were 4,200 (real 2009) NIS per capita on average, while municipal debt was 3,700 NIS per capita on average. Arab municipalities are ranked 2.7 on average on a socio-economic scale of 1 to 10, which is provided by the ICBS. They are also relatively small, ranging from 1,800 to 62,700 residents, or 11,580 on average.

b. Education data

Israel's National expenditure on education amounted to 57.2 billion NIS in 2008, roughly 7% of Israeli GDP. 27% of these expenses, or 15.4 billion NIS, were directed to

[4] Table 2 provides additional evidence regarding this issue.

secondary education, which is the focus of this study. Parents paid about 25.4% of secondary education expenditures on average (ICBS 2012), although this percentage is probably much lower in Arab municipalities (Klinov 2007).

Secondary education in Israel is mainly publicly provided, and is the responsibility of local governments. However, most of the resources come from the central government, which is also in charge of the curriculum.[5] Other revenue sources are local tax collection, parents' payments and donations. All these revenue sources, other than intergovernmental transfers, will be termed independent education revenues. Private payments of parents, as well as donations, are relatively low in Arab municipalities (Klinov 2007). Therefore, the majority of independent education revenues in Arab municipalities is funded through local taxes. Municipal financial statements do not specify independent education revenues, or education spending which is independently funded, because local tax collection is not earmarked for education. In order to calculate independent education spending we deduct government education transfers from municipal education spending.

Government education transfers are given by the ministry of education, and are supposed to fund teaching hours. The municipality is providing resources for additional teaching, construction, maintenance, security, cleaning, school buses, and auxiliary professionals (psychologists, secretaries etc.). The ministry of education provides funding which amounts to 75% of a "normative" spending on education. This normative spending level is calculated by the ministry. However, municipalities are able to provide extra funding or even funding below the required level. The latter is the case in most

[5] In the last two decades curriculum decisions were partially decentralized, but this affected mainly Jewish schools. Schools in Arab municipalities tend to teach more Arabic, English, Science and math, at the expense of History, Art and Citizenship (Benavot and Resh 2003, Resh and Benavot 2009). These schools tend to offer less subjects, but about the same total hours of instruction, compared to schools in Jewish municipalities (Resh and Benavot 2009).

Arab municipalities, where central government transfers are almost 90% of actual education spending. During the sample period government education transfers to Arab municipalities were 1,247 NIS per capita on average, while independent education revenues were 157 NIS per capita on average. Arab municipalities are therefore highly dependent on government transfers. In addition to that, some Arab municipalities illegally use portions of their education grant for services other than education, and possibly even for rents. Cases where education grants do not reach schools are not unique to Israel (Reinikka and Svensson 2004). In these cases independent education expenditures are negative. Since there is not much meaning to negative education expenditures, we omit observations with negative education expenditures from the analysis.

The issue of "negative" independent education spending is further explored in Appendix Table A2. The table presents a distribution of the 77 Arab municipalities, based on the number of years in which a municipality has "negative" independent education spending. In 37 municipalities there were no negative independent education spending. In 17 and 10 municipalities there were 1 and 2 negative values, accordingly. There are six municipalities which had negative education spending in the majority of the sample period. These include the following municipalities (number of negative spending years in parentheses): Ar'ara in the Negev (5), Cseife (5), Kfar Cana (6), Lakia (7), Tel Sheva (7), and Rahat (8). Note that these are all Bedouin municipalities (except for Kfar Cana).

Summary statistics of the main education variables of interest is presented in Table 1. Data on matriculation certification rates was taken from the Israeli central bureau of statistics (ICBS) and the ministry of education. This data is unavailable for municipalities with less than 2,000 residents. The average matriculation certification

rate in Arab municipalities is 0.52, i.e., 52% of every cohort receives a matriculation certificate.[6] A matriculation certificate is a prerequisite for admission for academic post-secondary schooling. There are also many employers who make it one of the eligibility criteria for a position. Students complete the matriculation process by passing a series of national exams in core and elective subjects during high school years. Students choose to be tested in each subject at various levels of difficulty, with each test awarding the student between one and five credit units per subject, depending on the difficulty of the exam.

We will discuss an additional outcome variable which measures students' achievements. This variable is the share of students whose matriculation certificate enables them to get accepted for undergraduate studies in one of the Israeli universities.[7] We term this variable college eligibility rate. College eligibility rate in Arab municipalities is 0.32 on average, ranging from 0.12 to 0.56.

c. External accountants

The appointment of external accountants is a relatively new tool used by the Israeli central government. External accountants are appointed by the ministry of interior in order to regulate the activities of financially distressed local governments (Ben-Bassat et al. 2011). This tool was made possible through legislative amendments which were made in 2003. Therefore, no external accountants were appointed before 2003.[8] External accountants are usually appointed to municipalities in a recovery plan, or municipalities with a high annual deficit (more than 5%). This variable takes values between zero (no external accountant) and one (an external accountant was present

[6] Matriculation certification rate is measured out of the students' population, and not out of the entire cohort population.
[7] Eligibility to study in the university is obtained by having a matriculation certification which includes taking 3 units in mathematics, 4 units in English and having another subject at a 4 units level or above.
[8] Very few external accountants were appointed at the end of 2003.

during the entire year). Values below one and above zero imply that the accountant was appointed in the middle of the year. For example, if the external accountant was appointed in July 1st our EA variable will equal 0.5.

The appointment of an external accountant is linked to the initiation of a recovery program for the municipality. A recovery program is a program specifically tailored for each financially distressed municipality. The program sets targets for revenues and expenditures which would lead to a balanced budget. Most of these programs include an increased target for tax collection, and spending cuts. In addition, the programs include grants to reduce municipal debt. More details on recovery programs are provided in Ben-Bassat, Dahan and Klor (2012).

Though recovery programs are highly correlated with the appointment of external accountants, they have little, if any, effect on municipal performance by themselves (Ben-Bassat, Dahan and Klor 2012). Probably the only significant effect of these programs is an increase in grants targeted to debt payments (Ben-Bassat, Dahan and Klor 2012). Therefore, there is arguably no reason to expect them to affect the results. In addition, the vast majority of Arab municipalities were in a recovery program during the sample period, including those without an EA.

d. Selection

A major concern in analyzing the data is the possibility of selection. It might be the case that Arab municipalities with an external accountant are different than those without an external accountant. If this is indeed the case then our IV is correlated with additional variables and the exclusion restriction is violated. Table 2 discusses differences between Arab municipalities which were appointed an EA during the sample period (column 1) and municipalities which were not appointed an EA (column 2). The table presents

summary statistics for 2003, one year before the use of external accountants became prevalent. There are some differences between the two groups, the main ones being: matriculation rate, college eligibility rate, education grants, municipal debt and welfare expenses. Municipalities with an EA are characterized by a lower matriculation rate (51% vs. 56% in the non-EA group), and lower college eligibility rate (31% vs. 37%). This difference is surprising considering the fact that they get higher government education grants per capita (1,296 NIS compared to 1,140 NIS). Municipalities with an external accountant also have a much higher debt (4,086 NIS per capita compared to 2,272 NIS per capita in the control group). Finally, they spend less on welfare (417 NIS per capita compared to 536 NIS), mainly due to lower welfare grants per capita. These differences, especially the difference in the matriculation rate, raise the issue of selection bias. It seems that municipalities which were not appointed an external accountant are not necessarily an appropriate control group for the treated municipalities, i.e. those with an external accountant.

In order to deal with selection bias, we introduce an alternative control group, which includes only municipalities which were appointed an EA. In this case, the control group in every year is comprised of municipalities that will be appointed an EA in the following years. For example, the control group for municipalities which were appointed an EA in 2004 is comprised of municipalities which will be appointed an EA in the years 2005-2009. Still, selection is a possibility. In order to check for selection within municipalities with an EA we compare municipalities who were appointed an external accountant during 2003-2004 (column 3), and those who were appointed an EA on 2005-2009 (column 4). Note that this comparison is a simplification of the actual treatment and control groups. We can see that most of the differences which were present between columns 1 and 2 disappear, including the differences in the

matriculation rate and college eligibility rate. In addition, the differences in the level of debt diminish, although they are still large and statistically significant. Two conflicting differences do emerge between the two groups. First, municipalities which were appointed an EA before 2005 seem to be ranked as having a higher socio-economic status. However, they also have more unemployment recipients.[9] We will control for these variable, as well as for the level of debt, in the robustness checks.

III. Methodology

a. The first stage

We use the appointment of an external accountant to municipalities in financial distress as an instrumental variable for education expenditures. The first stage is described in equation (1):

$$(1)\ edu_{it} = \alpha_{it} + \beta EA_{it} + x_{it} + C_i + T_t + \varepsilon_{it}$$

where edu_{it} is the natural log of independent education expenditures per capita in municipality i at time t, EA_{it} is a variable which equals one if municipality i had an external accountant in year t.[10] X_{it} is a set of control variables which include: population, average wage of residents, number of unemployment beneficiaries, average class size, percent of population above 65 and percent of population below 14. C_i and T_t are municipality and year fixed effects, respectively. α_{it} is the intercept and ε_{it} is the residual.

[9] The share of unemployment recipients in Arab municipalities is positively correlated with Socio-economic status.

[10] As noted above, EA can take values below one if the external accountant was appointed in the middle of the year.

In order to be a valid and not a week instrument external accountants have to be highly correlated with education expenditures. This is tested in Table 3, which describes the results of the first stage. We experiment with different lags of the external accountants variable since the effect of EA on municipal spending might take time to materialize. An external accountant cuts independent education expenditures by 12% in the first year of his appointment (Table 3, column 1). This is followed by a 14% cut in the following year (column 2). However, the big decline in independent education spending comes two years after the appointment of the external accountant, and amounts to a 53% decrease (column 3). This decrease is also highly statistically significant, and the F-test for excluded instruments equals 19.66, which is higher than the common threshold of 10 (Angrist and Pischke 2009). Using both the first and second lag of an external accountant (column 4) still gives an F-test larger than 10, but the instruments are clearly weaker than in column 3. Therefore, our preferred instrument will be the one presented in column 3, namely a two-year lag of an external accountant. This specification is presented in equation 2, which is identical to equation 1 except for the second lag for the external accountant variable:

$$(2)\ edu_{it} = \alpha_{it} + \beta EA_{it-1} + x_{it} + C_i + T_t + \varepsilon_{it}$$

Table 3 provides evidence suggesting that our IV is not weak. We now turn to discuss the credibility of the exclusion restriction. There are several reasons why external accountants need not be correlated with student achievements. Firstly, the decision regarding the appointment of an external accountant is made by the ministry of interior, not by public officials within the municipality. This decision is based on changes to the financial performance of the municipality, not on students' performance or socio-economic attributes which tend to be relatively fixed. Secondly, when restricting the sample to municipalities which were appointed an EA the control and

treatment groups become very similar (Table 2). This means that the timing of appointment, which dictates our control and treatment groups and our IV, is probably uncorrelated with the municipalities attributes, such as student achievements. We will use this restricted sample in order to correct for the possibility of selection bias.

Another approach to assess the validity of the exclusion restriction is presented in Table 4. This approach uses tests which estimate the association between our instrumental variable, the external accountants, and several socio-economic factors. Each cell provides the coefficient from a regression where the dependent variable is the socio-economic variable and the independent variables are the second lagged external accountant, and municipality and year fixed effects. Column 1 deals with the entire sample of Arab municipalities, whereas column 2 restricts the sample to Arab municipalities that were appointed an external accountant during the sample period. In column 1 we can see that the existence of an external accountant is not associated with most socio-economic variables. However, an EA is associated with a decrease in population of about 4%, a decrease in unemployment of about 9%, and a marginally significant decrease in the average wage of about 2%. The negative association between population and EA is a result of a smaller fertility rate in the municipalities with the EA, and not a result of outward migration (results available upon request). When the sample is restricted to municipalities which were appointed an EA, the difference in population and unemployment remain (column 2). In addition, there is a stark difference in the level of debt, which drops by 15% after the appointment of the EA. This is to be expected considering the mandate of the external accountant. Two additional differences are marginally statistically significant – a decrease in welfare expenses of about 6% and an increase in total municipal spending of about 5%. These differences are small in terms of economic magnitudes and therefore cannot drive our results.

b. The second stage

The second stage is depicted by equation (3):

$$(3)\ Mat_{it} = \lambda_{it} + \mu Edu_{it} + x_{it} + C_i + T_t + \upsilon_{it}$$

Where Mat_{it} can be one of two variables: a) matriculation rate - the percentage of matriculation certificate recipients out of the entire students' cohort in municipality *i* in year *t*, and b) college eligibility rate - the share of students which their matriculation certificate allows them to get accepted to Israeli universities, out of the entire students' cohort in municipality *i* in year *t*. Edu_{it} is the predicted value of log independent education expenditures per capita, from the first stage (equation (2)). x_{it} is the set of control variables which was described above. C_i and T_t are municipality and year fixed effects, respectively. λ_{it} is the intercept and υ_{it} is the residual.

IV. Results

a. Main results

The main findings regarding the effect of independent education spending on matriculation certification rate and college eligibility rate are presented in Table 5 and Table 6. Table 5 is using a sample of all Arab municipalities, while Table 6 only uses Arab municipalities which were appointed an EA. We start with Table 5, which consists of two panels: panel A presents results for matriculation rate as the dependent variable, and panel B presents results for college eligibility rate as the dependent variable. For expositional purposes we start with the OLS estimator (Column 1). The coefficient of education expenditures equals 0.018 in both panels and is statistically significant at the 1% level. Therefore, an increase in independent education expenditures of 10% is

associated with an increase in matriculation exam recipients of 0.18 percentage points.[11] This is a very small effect compared to the average matriculation rate which is 52%. The OLS estimator probably suffers from the multiple endogeneity issues which were discussed above. Therefore, we move to the Fixed Effects estimator, which deals with some of these concerns by controlling for municipality and year specific effects. The Fixed Effects estimator is giving non-significant coefficients of -0.005 and -0.001 (Column 2, panels A and B, respectively). The fixed effects estimator is in line with other macro studies which find no effect of education expenditures on student performance. However, this estimator probably suffers from endogeneity issues as well. For example, it might be the case that when student performance deteriorates the mayor choose to divert more resources to the education system. In this case there will be simultaneity between expenditures and performance due to the negative correlation between performance and resources, and the estimator will have a downward bias. Therefore, Column 3 presents the IV estimator from equation 3, using the second lag of external accountants as an IV for independent education expenditures. The coefficient on independent education spending equals -0.002 for matriculation rate and 0.005 for college eligibility rate, and is statistically insignificantly different from zero in both cases.

Possibly the main concern with the results outlined above has to do with selection bias. As can be seen in Table 2, external accountants are not appointed randomly. Quite the opposite, external accountants are appointed to municipalities in financial distress and with relatively poor education performance.

In Table 6 we try to correct for selection bias, using different control groups. Similarly to Table 5, Table 6 presents two panels, with the dependent variable being

[11] Note that eeducation expenditures are in natural logs and matriculation exam recipients are in percents.

matriculation rate in panel A and college eligibility rate in panel B. To facilitate the comparison, column 1 of Table 6 presents the baseline results of Table 5 (column 3), where the control group is the entire population of Arab municipalities which were not appointed an external accountant. We start the discussion of the different control groups with the matriculation rate results (panel A). Column 2 of panel A presents results for a subsample which includes only municipalities which were appointed an external accountant. The control group in this case is comprised out of municipalities which would be appointed an EA in the future. The coefficient doesn't change much compared to column 1, and is now equal to -0.006. Column 3 presents results for a sub-sample which includes only municipalities which are ranked 3 or 4 in the socio-economic index of the ICBS. This sub-sample tries to correct for differences in socio-economic status, which is highly correlated with education achivements. The coefficient in column 3, -0.005, hardly changes compared to column 2. Finally, in column 4 the sample is restricted in order to correct for two issues: selection and amalgamation. Similarly to column 2, only municipalities which were appointed an external accountant are included. In addition, this sub-sample excludes municipalities which were amalgamated during the sample period. In 2003 seven Arab municipalities were amalgamated into three municipalities. The amalgamation resulted in a decrease in expenditures and might have affected the education system (Reingewertz 2012). The coefficient in column 4 is slightly closer to zero, at -0.001.

Panel B of Table 6 presents the results for college eligibility rate, using the same selection corrections as in panel A. The coefficient in column 2, which includes only municipalities which were appointed an EA, equals 0.035, much higher than the baseline results but still statistically insignificant. Column 3 includes only municipalities which are ranked 3 or 4 in the socio-economic index and the coefficient

changes the sign and equals -0.024, still statistically insignificant. Finally, column 4 includes only municipalities which were appointed an EA and were not amalgamated, and the coefficient is positive and equals 0.043, though still statistically insignificant. Note that in all the specifications presented in Table 6 the F-test for excluded instruments is above 10, ranging from 13.71 to 19.66. This means that the results probably do not suffer from a weak instruments bias. The stability of the results suggests that using external accountants as an IV for independent education expenditures is not associated with selection bias.

The issue of selection bias is further emphasized through Appendix Table A3. This table describes the results of the IV estimator (equation 3), for three different samples: the entire sample of Israeli municipalities – both Arab and Jewish ones (Column 1); only Jewish municipalities (Column 2); and only Arab municipalities (Column 3). Panel A describes the results for matriculation rate. We can see that when both Arab and Jewish municipalities are included in the analysis, independent education spending positively affects matriculation rates (Column 1). In the case of Jewish municipalities the IV is very weak and is no longer valid, since the F-test for excluded instruments is zero (Column 2). Finally, in the case of Arab municipalities, education spending seems to have no effect on matriculation rates (Column 3). Panel b of Table 5 shows a similar picture. The only difference is that here the entire sample of Jewish and Arab municipalities still yields a non-significant effect of education spending on student achievements. Comparing the results presented in Column 1 (Jewish and Arab municipalities) and Column 3 (only Arab municipalities) suggests that ignoring the issue of selection can bias the estimates upward, leading to a false positive relation between education spending and students' achievements.

b. Robustness checks

We provide a set of robustness checks that explore issues other than selection bias (Tables 7 and 8). All the robustness checks are done on the subsample which was introduced in Table 6, column 4. This subsample includes only Arab municipalities which had an external accountant during at least one year, excludes the amalgamated municipalities, and presumably corrects for selection bias. As can be seen in Table 6, different corrections for selection give very similar results. Restricting the sample to municipalities which were appointed an EA, and excluding the amalgamated municipalities from both treatment and control groups, probably corrects for selection in the most rigorous way. The robustness checks are done for the matriculation rate as the dependent variable (results for college eligibility rate are presented in Appendix Table A4, and are consistent with the results in Table 7). Table 7 is divided to panel A and panel B, both of them use the matriculation rate as the dependent variable. Panel A is adding additional control variable to the specification of Table 6, column 4. Panel B discusses changes to the specification.

We will first discuss panel A (Table 7). The specifications in this panel try to control for additional variables that might be correlated with our IV and with student performance, thus possibly violating the exclusion restriction. Every column introduces a different set of control variables. Column 1 in Table 7 introduces the baseline control variables, which are described in equation 3, to the estimation. In Column 2 we add education grants per capita as a control variable. If external accountants are correlated with education grants they do not satisfy the exclusion restriction and the results will be biased. Column 3 includes welfare spending per capita as a control variable, since welfare spending might be affected by the appointment of an EA, and might affect private expenditures on education. Finally, Column 4 includes debt per capita as a

control variable, since debt is significantly different between treated municipalities and non-treated ones. The estimated coefficients in Columns 1-4 do not change much compared to the baseline results, and range from -0.005 to -0.009, all statistically insignificantly distinguishable from zero. This suggests that the exclusion restriction is a reasonable assumption.

We now turn to panel B. The first column deals with the possibility of non-linearity in the effect of resources on education outcomes. Indeed, theory suggests a decreasing marginal effect of education resources on students' outcomes, and empirical findings seem to support it (Loken et al. 2012). Therefore, we add education expenditures squared as an additional endogenous variable. In order to do that we use an additional IV - the first lag of an external accountant (similar to Table 3, column 4). Adding an additional IV results in a weaker instrument (First stage F-tests are 7.29 and 7.91). In addition the coefficients are highly insignificant and the R-square is slightly lower. All these suggest that this specification is miss-specified.

Column 2 in Table 7, panel B, uses independent education spending per capita (i.e. without the natural logarithm transformation) as the endogenous variable. The results do not change much compared to the baseline results. Column 3 omits outliers, observations where independent education expenditures are lower than the 1^{st} percentile or higher than the 99^{th} percentile. The coefficient declines to -0.027 but is still statistically insignificant.

Finally, column 4 in panel B tackles the issue of municipalities which had "negative" independent education spending (see a discussion in Section II.b). The sub-sample introduced in column 4 omits municipalities which had negative independent

education expenditures during at least half the sample period (i.e. at least 5 out of the 10 years). The results are not much changed and the coefficient is -0.012.

In addition to the robustness checks which are reported in Table 7 we used a method described in Conley et al. (2012), in order to assess the importance of the exclusion restriction. Conley et al. (2012) provide a method which allows us to relax the assumption of complete exogeneity which is needed for the IV analysis. Instead of complete exogeneity their method requires that the instrumental variable be "plausibly exogenous" (Conley et al. 2012). In other words, their methodology tries to correct for small violations of the exclusion restriction, through a "Union of Confidence Intervals" instead of a single estimator. Applying their methodology to our case yields a confidence interval of -0.008 to 0.022, which includes most of our point estimates (the confidence interval for college eligibility rate is -0.019-0.009).

Table 8 explores the time dimension, by introducing different time lags between education expenditures and students' achievements. Since education expenditures might take some time to affect students' achievements we use two different lags of education expenditures: one year and two years. Column 1 presents the baseline results of Table 5 (i.e. with no lag between resources and achievements), column 2 uses a one-year lag of education expenditures, and column 3 uses a two years lag of education expenditures. A one year lag of education resources gives a coefficient of -0.01 while a two-year lag gives a coefficient of -0.037, both are statistically insignificant.

c. Additional confounding factors

In addition to selection bias and other concerns which were discussed in the robustness checks, we discuss other factors which might affect our results but cannot be assessed quantitatively at the municipal level. These confounding factors include parental

spending on education, migration and school choice, dropout rates and other unobservable factors.

Parental effort and other family attributes are arguably the most important confounding factor in studies on education spending and education outcomes (Houtenville and Conway 2008). We will focus on parental spending on education, though parental efforts are also non-monetary in nature. Parents' spending on education might respond to changes in public education spending, violating the exclusion restriction. For example, parents might decrease education spending as a result of an anticipated increase in public education spending (Das et al. 2013). Parental spending on education can be divided to two groups: payments to schools and spending on education at home (e.g. private lessons). Payments of parents to schools comprise the majority of their expenses on education (Klinov 2007). These payments do not affect our estimates, since our definition of independent education expenditures includes payments by households. Therefore, the sharp decline in own expenditures after the appointment of an external accountant happens despite an offsetting effect, if any, made by parents' payments.

Parental education spending at home can take many forms. We will touch what might be the dominant component in parental education spending, private lessons, which are only observable at an aggregated level.[12] Klinov (2007) reports private spending on private lessons in elementary schools in Israel. According to her findings the average Arab student receives 36 NIS worth of private lessons a year. This is an order of magnitude below spending on private lessons in Jewish households. It is also

[12] Other parental spending on education includes books, office and learning materials, computers etc. Regarding computers, there is no evidence for an effect of home computers on student achievement (Fairlie and Robinson 2013)

an order of magnitude below parents' payments to schools in Arab municipalities. Therefore, private lessons probably do not have a considerable effect on our results.

Another confounding factor is migration and school choice. If families migrate or change schools because of changes in education spending our estimates might be biased. For example, if strong students migrate due to budget cuts we would observe a decrease in achievements in municipalities which cut education spending, if only because of migration. However, the issue of school choice and migration is probably negligible in the context of the Arab education system. First, there is no school choice in the Israeli education system, and the school is determined by the residential location of the student. Therefore, students cannot change schools without migrating. Migration might be a concern if budget cuts cause the population to migrate. However, migration in Arab municipalities is relatively small, and is done mainly due to marriage (ICBS 2007). This means that migration probably does not have much effect on our results. In addition, External Accountants did not cause an increase in migration. If anything, they decreased migration (Ben Bassat et al. 2012).

Another unobservable factor is dropout rates. Students dropping from school are not accounted for in the calculation of the matriculation rate. Since students who drop from school are presumably the weakest, an increase in the dropout rate would tend to increase the matriculation rate. Dropout rates might be affected by education spending so they also represent an outcome variable in our case (Cascio et al. 2012). The dropout rate of Arab teens experienced a decline in the last few decades, followed by a mild increase in the last decade (ICBS 2010). Most of the multidecadal decline is due to girls entering schools. The decline in girls' dropout rates stabilized at a dropout rate of about 10% during the sample period since 2000. Boys show a modest decrease in dropout rates, from 20% in 2000 to 15% in 2005, coming back to 20% on 2006 (ICBS

2010). All in all, the changes during the sample period seem quite modest. If anything, there was a slight increase in dropout during the sample period.[13] If changes in dropout rates are positively correlated with the existence of an external accountant, it would bias our estimates upward. In other words, in this case our estimates are an upper bound. However, our estimates tend to be small and insignificant, despite this possible upward bias.

Yet another concern is that the appointment of external accountants is associated with other factors at the municipal level, which in turn might affect students' achievements. External accountant reduce salary payments and increase property tax revenues (Ben Bassat et al. 2012). These changes might in turn affect, e.g., parental effort. This indirect mechanism, if it exists, is not quantitatively large. For example, Ben bassat et al. (2012) find that salary payments decreased by 2.4% in the first year, and an additional 1.5% in every following year. Another possible confounding factor which might be associated with the appointment of an external accountant is corruption. It is possible that external accountants are disproportionally appointed to corrupt municipalities. In this case education spending cuts made by external accountant would not affect education outcomes if these cuts only target corruption. Although we cannot completely rule out this possibility, two important factors suggest that corruption within the education system is relatively small. First, as discussed above, some municipalities illegally divert government education transfers to other uses. If we interpret this activity as corruption it means that corruption takes place outside the education system, at least in these municipalities. Secondly, independent spending on education is relatively small in Arab municipalities. This means that the available independent funds within the education system are small and therefore corruption is probably not large in magnitude.

[13] The ICBS report also documents a difference in dropouts between villages and cities. Our sample is mostly comprised of villages, which experienced a mild increase in dropout during the sample period.

V. Conclusions and Policy Implications

The results of this paper suggest that changes to education resources of Israeli-Arab municipalities do not affect student achievements. Using the appointment of an external accountant as an IV for independent education expenditures we find that a decrease in independent education expenditures per capita did not decrease the share of matriculation recipients.

These results help in explaining the disagreement between macro and micro studies regarding the effect of education expenditures on student achievements. The results suggest that education expenditures at the municipal level might not have an effect on students' performance. Therefore, it seems that changes to education resources at the macro level might have a very small impact, if any, on students' achievements.

The results also suggest that external accountants do not harm student achievements, despite the fact that they cut education spending. This result is relevant for the debate regarding the way central government should regulate the activities of local government.

Our sample is restricted to Arab municipalities in Israel. These municipalities are the least-developed part of the Israeli economy. This means that our results do not necessarily extend to the entire education system in Israel. We believe that our results are relevant for less-developed regions within developed countries, as well as to regions within developing countries.

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Table 1. Summary statistics, 2003

	Mean	St.dev	Min	Max
Independent educ. Exp. pc	156.71	267.71	3.73	952.66
Education transfers per capita	1,247.46	460.72	417.88	2,432.69
Matriculation recipients	0.52	0.12	0.21	0.75
College eligibility rate	0.32	0.10	0.12	0.564
Average No. of students in class	29.27	2.25	24	34
Total municipal spending per capita	4,195.00	952.52	1,765.13	6,779.42
Welfare expenses	448.13	295.89	78.64	2,345.73
Municipal expenses	1,687.88	645.21	633.88	3,629.29
Debt	3,697.55	2,023.15	483.60	9,122.27
Average wage of residents	4.17	0.60	2.71	5.87
Socio-economic status	2.70	1.02	1	6
population	11.58	10.21	1.8	62.7
Unemployment recipients	6.27	2.36	1.69	12.96
N	77			

Notes: Monetary values in all tables are presented in real (2009) New Israeli Shekels (NIS).

Table 2. Selection bias – municipalities with and without external accountants, 2003

	Never had EA	Had EA	Were appointed EA in 2004	Were appointed EA after 2004
Matriculation recipients	0.56	0.51	0.50	0.52
	(0.07)	(0.12)*	(0.13)	(0.12)
College eligibility rate	0.37	0.30	0.30	0.30
	(0.09)	(0.10)**	(0.10)	(0.10)
Independent educ. Exp. pc	157.16	161.93	192.41	160.40
	(246.27)	(280.40)	(279.39)	(282.81)
Education transfers per	1,139,61	1296.34	1,334.82	1,257.84
capita	(359.49)	(478.87)*	(487.53)	(475.17)
Average No. of students in	28.68	29.46	29.15	29.75
class	(2.77)	(2.20)	(1.98)	(2.38)
Total municipal spending	4,207.96	4,231.50	4,324.83	4,138.16
per capita	(1,259.19)	(928.60)	(879.72)	(981.00)
Welfare expenses	536.27	417.19	432.36	402.02
	(501.60)	(179.73)*	(162.00)	(197.48)
Municipal expenses	1,756.00	1,681.38	1,745.00	1,613.21
	(757.88)	(625.07)	(677.00)	(568.56)
Debt per capita	2,272.45	4,085.63	4,768.79	3,402.48
	(1,669.37)	(1,923.70)***	(1,810.10)	(1,812.95)***
Average wage of residents	4.25	4.166	4.25	4.08
	(0.75)	(0.57)	(0.59)	(0.56)
Socio-economic status	2.95	2.65	2.83	2.47
	(1.35)	(0.99)	(1.09)	(0.86)*
population	9.29	12.11	12.89	11.33
	(8.37)	(10.50)	(12.26)	(8.51)
Unemployment recipients	6.20	6.23	6.92	5.52
	(2.69)	(2.26)	(2.21)	(2.11)***
N	17	60	30	30

Notes: asterisks describe significance levels (* for 10%, ** for 5% and *** for 1%). Asterisks in column 2 describe a statistically significant difference between column1 and column 2. Asterisks in column 4 describe a statistically significant difference between column3 and column 4. All the summary statistics refer to 2003.

Table 3. The first stage

	(1)	(2)	(3)	(4)
External accountant$_t$	-0.12	-	-	-
(std. error)	(0.11)	-	-	-
External accountant$_{t-1}$	-	-0.14	-	0.14
(std. error)	-	(0.12)	-	(0.14)
External accountant$_{t-2}$	-	-	-0.53***	-0.62***
(std. error)	-	-	(0.12)	(0.15)
R Squared	0.98	0.98	0.98	0.98
Obs.	578	575	520	520
F-test for excluded instruments (first stage)	1.11	1.49	19.66	10.11
Fixed Effects	Muni.+Year	Muni.+Year	Muni.+Year	Muni.+Year

Notes: The Table displays the effect of external accountants on education expenditures (the First Stage). The sample consists of 77 municipalities over 10 years (2000-2009) for a total of 578 observations. The dependent variable is log education expenditures per capita. The IV in the first column is an external accountant in time t. In columns 2 and 3 the IV is an external accountant at time t-1 and t-2, respectively. The IVs in column 4 are an external accountant at time t-1 and t-2.

Table 4. Balancing tests

	(1) Entire sample	(2) Had EA
Education transfers per	-0.038	-0.004
capita	(0.027)	(0.028)
Total municipal spending	0.009	0.053*
per capita	(0.024)	(0.030)
Welfare expenses per	-0.010	-0.061*
capita	(0.036)	(0.035)
Municipal expenses per	-0.008	0.057
capita	(0.037)	(0.047)
Debt per capita	-0.074	-0.155***
	(0.055)	(0.061)
population	-0.041***	-0.054***
	(0.014)	(0.015)
Average wage of	-0.021*	-0.006
residents	(0.011)	(0.015)
Unemployment	-0.093***	-0.123***
recipients	(0.032)	(0.039)

Notes: asterisks describe significance levels (* for 10%, ** for 5% and *** for 1%). All variables are in logs, except for class size. Each cell describes the results of a separate regression. The independent variable is the second lag of an external accountant. Each regression includes municipality and year fixed effects. The regressions in column 1 are run for the entire sample of 77 Arab municipalities. The regressions in column 2 only include the 60 Arab municipalities which had an external accountant during the sample period.

Table 5. Main results

	(1)	(2)	(3)
Panel a: matriculation rate	OLS	FE	IV
Independent educ. Exp. pc	0.018***	-0.005	-0.002
(std. error)	(0.005)	(0.006)	(0.026)
R Squared	0.02	0.39	0.98
Obs.	578	578	522
F-test for exc. IV(1st stage)	-	-	19.66
Fixed Effects	None	Muni.+Year	Muni.+Year
IV	No	No	Yes
Panel b: college eligibility rate			
Independent educ. Exp. pc	0.018***	-0.001	0.005
(std. error)	(0.005)	(0.005)	(0.024)
R Squared	0.02	0.51	0.98
Obs.	580	580	521
F-test for exc. IV(1st stage)	-	-	19.48
Fixed Effects	None	Muni.+Year	Muni.+Year
IV	No	No	Yes

Notes: The table displays the effect of education expenditures on matriculation rate (panel a) and college eligibility rate (panel b) (the Second Stage). The sample consists of 77 municipalities over 10 years (2000-2009) for a total of 580 observations. Column 1 presents the OLS results, column 2 includes municipality and year fixed effects. Column 3 presents the IV results.

Table 6. Results after correcting for selection bias

Panel a: matriculation rate	(1)	(2)	(3)	(4)
Independent educ. Exp. pc	-0.002	-0.006	-0.005	-0.001
(std. error)	(0.026)	(0.030)	(0.032)	(0.030)
R Squared	0.98	0.98	0.98	0.98
Obs.	522	397	302	376
No. of municipalities	77	60	41	58
F-test (first stage)	19.66	13.83	13.79	14.38
Fixed Effects	Muni.+Year	Muni.+Year	Muni.+Year	Muni.+Year
Panel b: college eligibility rate	(1)	(2)	(3)	(4)
Independent educ. Exp. pc	0.005	0.035	-0.024	0.043
(std. error)	(0.024)	(0.028)	(0.029)	(0.028)
R Squared	0.98	0.96	0.96	0.96
Obs.	521	396	301	375
No. of municipalities	77	60	41	58
F-test (first stage)	19.48	13.73	13.71	14.28
Fixed Effects	Muni.+Year	Muni.+Year	Muni.+Year	Muni.+Year
Selection correction:	No correction	Only treated	Only socio 3&4	Only treated & non-amalgam.

Notes: The Table displays the effect of education expenditures on matriculation exam recipients (panel a), and matriculation recipients with college eligibility (panel b). Column 1 presents the results without correction for selection, column 2 restricts the sample to municipalities which had an external accountant during the sample period. Column 3 restricts the sample to municipalities with socioeconomic status equal to 3 or 4. Column 4 restricts the sample to municipalities which had an external accountant during the sample period, and were not amalgamated.

Table 7. Robustness checks

	(1)	(2)	(3)	(4)
Panel a	Controls	Transfers	Welfare	debt
Independent educ. Exp. pc	-0.009	-0.006	-0.005	-0.008
(std. error)	(0.028)	(0.030)	(0.030)	(0.030)
R Squared	0.98	0.98	0.98	0.98
Obs.	385	397	395	397
F-test for excluded instruments (first stage)	15.16	14.44	14.45	13.71
Panel b	Non linearity	No-log	Outliers	No negatives
Independent educ. Exp. pc	-0.235	-0.000	-0.027	-0.012
(std. error)	(0.958)	(0.000)	(0.056)	(0.039)
Own educ. Expenses^2	0.026	-	-	-
(std. error)	(0.111)	-	-	-
R Squared	0.97	0.98	0.98	0.98
Obs.	395	397	384	379
F-test for exc. IV (1^{st} stage)	7.29, 7.91	7.21	6.66	8.56

Notes: This Table displays the effect of education expenditures on matriculation rate. The sample consists of 60 municipalities over 10 years (2000-2009) for a total of 397 observations. All regressions include year and municipality fixed effects. The robustness checks are described in the text (pages 13-15).

Table 8. Timing of education expenditures

	(1)	(2)	(3)
Lags:	0	1	2
Independent educ. Exp. pc	-0.006	-0.010	-0.037
(std. error)	(0.030)	(0.032)	(0.037)
R Squared	0.98	0.98	0.98
Obs.	397	353	299
F-test for excluded IV (1st stage)	13.83	13.45	8.33
Fixed Effects	Muni.+Yr	Muni.+Yr	Muni.+Yr

Notes: The Table displays the effect of education expenditures on matriculation exam recipients (the Second Stage). The sample consists of 77 Arab municipalities over 10 years (2000-2009) for a total of 397 observations. Column 1 presents the effect of lagged education expenditures on student performance. , using lagged external accountants as an IV. Columns 2, 3 and 4 use the second, third and forth lag of education expenditures, accordingly. Column 5 uses no lag for education expenditures, but uses a lag for the external accountants.

Appendix Table A1. Selection bias – Jewish municipalities vs. Arab municipalities, 2003

	Jewish	Arab	Never had EA (Jewish + Arab)	Had EA (Jewish+Arab)
Matriculation recipients	0.62	0.52	0.62	0.53
	(0.10)	(0.12)***	(0.10)	(0.12)***
Eligibility rate	0.51	0.32	0.51	0.34
	(0.13)	(0.10)***	(0.13)	(0.12)***
Independent educ. Exp. pc	673.09	156.72	588.74	313.95
	(300.57)	(267.71)***	(336.54)	(386.14)***
Education transfers per capita	932.36	1,2247.46	933.40	1,216.08
	(515.51)	(460.72)***	(491.50)	(508.54)***
Average No. of students in class	25.69	29.43	26.97	27.42
	(4.13)	(2.25)***	(3.41)	(4.56)
Total municipal spending per capita	5,234.71	4,194.99	4,981.40	4,635.87
	(1,551.74)	(952.52)***	(1,485.32)	(1,364.81)**
Welfare expenses	655.47	448.13	598.61	542.33
	(280.23)	(295.89)***	(313.42)	(287.99)*
Municipal expenses	2,355.40	1,687.88	2,202.34	1,961.27
	(992.36)	(645.21)***	(973.15)	(861.50)**
Debt per capita	3,965.98	3,697.54	3,125.10	4,824.05
	(3,037.75)	(2,023.15)	(2,453.68)	(2,688.73)***
Average wage of residents	7.29	4.17	7.07	4.72
	(2.37)	(0.60)***	(2.56)	(1.38)***
Socio-economic status	5.88	2.70	5.65	3.34
	(1.95)	(1.02)***	(2.22)	(1.57)***
population	43.84	11.58	43.43	15.54
	(82.83)	(10.21)***	(85.58)	(18.73)***
Unemployment recipients	12.04	6.27	10.81	8.48
	(4.03)	(2.36)***	(4.15)	(4.55)***
N	120	77	111	86

Notes: asterisks describe significance levels (* for 10%, ** for 5% and *** for 1%). Asterisks in column 2 describe a statistically significant difference between column 1 and column 2. Asterisks in column 4 describe a statistically significant difference between column3 and column 4. All the data refers to 2003.

Appendix Table A2. "Negative" independent spending on education

No. of negative values	No. of municipalities
0	37
1	17
2	10
3	1
4	6
5	2
6	1
7	2
8	1

Notes: The municipalities with 5 or more negative values are (No. of negative values in parentheses): Ar'ara in the Negev (5), Cseife (5), Kfar Cana (6), Lakia (7), Tel Sheva (7), Rahat (8). All but Kfar Cana are Bedouin municipalities.

Appendix Table A3. Main results, Jewish and Arab municipalities

	(1)	(2)	(3)
Panel a: matriculation rate	Jewish + Arab	Jewish	Arab
Independent educ. Exp. pc	0.094***	4.623	-0.002
(std. error)	(0.028)	(62.708)	(0.026)
R Squared	0.98	0.39	0.98
Obs.	1,480	958	522
F-test for exc. IV(1st stage)	37.95	0.00	19.66
Fixed Effects	Muni.+Year	Muni.+Year	Muni.+Year
IV	Yes	Yes	Yes
Panel b: college eligibility rate			
Independent educ. Exp. pc	-0.0002	1.791	0.005
(std. error)	(0.021)	(24.506)	(0.024)
R Squared	0.99	0.38	0.98
Obs.	1,479	958	521
F-test for exc. IV(1st stage)	37.55	0.00	19.48
Fixed Effects	Muni.+Year	Muni.+Year	Muni.+Year
IV	Yes	Yes	Yes

Notes: The table displays the effect of education expenditures on matriculation rate (panel a) and college eligibility rate (panel b) (the Second Stage). The sample consists of 77 municipalities over 10 years (2000-2009) for a total of 580 observations. Column 1 presents the OLS results, column 2 includes municipality and year fixed effects. Column 3 presents the IV results.

Appendix Table A4. Robustness checks for college eligibility rate

	(1)	(2)	(3)	(4)
Panel a	Controls	Transfers	Welfare	debt
Independent educ. Exp.	0.038	0.034	0.027	0.032
(std. error)	(0.026)	(0.027)	(0.027)	(0.028)
R Squared	0.96	0.96	0.96	0.96
Obs.	384	396	394	396
F-test for excluded instruments (first stage)	15.01	14.34	14.35	13.61
Panel b	Non linearity	No-log	Outliers	No negatives
Independent educ. Exp.	-0.822	0.000	0.034	0.034
(std. error)	(1.53)	(0.000)	(0.052)	(0.036)
Own educ. Expenses^2	0.098	-	-	-
(std. error)	(0.177)	-	-	-
R Squared	0.87	0.96	0.96	0.96
Obs.	394	396	383	378
F-test for exc. IV (1^{st} stage)	7.24, 7.85	7.19	6.61	8.49

Notes: This Table displays the effect of education expenditures on college eligibility rate. The sample consists of 60 municipalities over 10 years (2000-2009) for a total of 396 observations. All regressions include year and municipality fixed effects. The robustness checks are explained in the text (pages 13-15).