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Public Good Provision, Commuting and Local Employment*

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ABSTRACT

This paper assesses the differential impact of the local availability of grammar schools on local employment depending on the openness of a jurisdiction, measured by commuting costs. Commuting costs matter as they influence workers' reservation wage. While the reservation wage depends on public good provision in jurisdictions with high commuting costs, it does not so in jurisdictions with low commuting costs as workers' outside option is to commute and not to move away. We test these predictions using local grammar school closures in East Germany after 2000. In line with the predictions we find that school closures reduced employment only in jurisdictions with high commuting costs. Reassuringly, house prices responded, however, similar in both types of jurisdictions which rules out that differences in preferences are driving our results.

JEL Classification: J2, R1, R5, H4

Keywords: local schools, employment, commuting, house price

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I. INTRODUCTION

In 2050 almost 70% of the world population will live in urban areas, up from 30% in 1950 (United Nations (2014)). The driving force behind this trend is seen in urban amenities (Ahlfeldt *et al.* (2015)) as well as agglomeration economies which increase productivity and thus wages (Rice *et al.* (2006), Graham (2007), Graham and Kim (2008), Melo *et al.* (2009)). While adjusting house prices mitigate the trend in urbanization (e.g. Combes *et al.* (2012)), the centralization of public goods (for which economies of scale are important) in rural areas – in response to the decline in the population density – may re-enforce the urbanization trend. Our paper tests this presumption by investigating the role of the local availability of grammar schools for local employment in East Germany. Following recent literature (see, e.g., Monte *et al.* (2018)), we pay particular attention to differences in commuting patterns between jurisdictions when analyzing the employment effects. Commuting goes hand in hand with urbanization as agglomeration economies capitalize into housing prices and thus increase the incentive to commute to metropolitan areas.

The paper starts by setting out a stylized theoretical model of local employment in a (small) rural town and a large city/metropolis. Town and city differ with respect to wages, public good provision and housing prices. Since we focus in the empirical analysis on local grammar school closures in mid-sized towns, we assume that changes in the town do not affect the city. The local labor market and the housing market are modeled as follows: The wage in the town results from a bargaining between workers and employers and we assume that workers have no bargaining power. Thus, the wage in the town equals workers' reservation wage. Local employment is then determined by the aggregate labor demand for the given wage. Housing prices are a function of housing supply and the wage income of the residents in the town as well as their public good valuation.

We compare two settings: In the first setting, we assume commuting costs between town and city to be prohibitively high and thus workers in the town are not able to commute. In this case, workers' reservation wage and, therefore, the wage in the town depends negatively on public good provision in the town. This means that a reduction in public good provision increases the wage and thereby decreases employment. In the second setting, we assume commuting costs to be sufficiently low. This means workers' outside option is not longer to move to the city but to commute to the city and to work there. In this case, workers' reservation wage only depends on the wage in the city and the commuting costs. Changes in the public

good provision have, therefore, no impact on the wage in the town and consequently no impact on employment. A further prediction of our model is that house prices change similar in jurisdictions with high and low commuting costs in response to a change in public good provision if the labor demand elasticity is close to -1 .

We test these theoretical predictions using grammar school closures in mid-sized rural towns in East Germany between 1997 and 2008. Grammar schools are important as pupils finishing grammar school obtain a university entrance degree. In 2014, 41% of all pupils in East Germany went to a grammar school.¹ The school closures we exploit for identification were caused by three reasons. First, there was a substantial outflow of people to West Germany after the re-unification. Overall, about four million inhabitants (out of 16 million) moved to West Germany for at least some time. As it was mostly the younger people moving, this had a direct effect on the number of pupils attending east German schools. Second, birthrates dropped after the re-unification due to economic uncertainty (see Chevalier and Marie (2014)). Third, in the second half of the 1990s, there was a substantial migration from rural to urban areas. Saxony-Anhalt, a mostly rural area, saw its population decrease by more than 20% between 1993 and 2013 (after the initial east-west migration) and had to close about 60% of all schools in rural areas during the process.

It is important to note that the final decision to close a particular grammar school was made by the (hosting) federal state. Although (hosting) counties as well as municipalities have been involved in the decision process as they contribute to the funding of schools, the federal states had the final say as they are bearing the main financial burden and are in particular responsible for hiring and overseeing teachers as well as providing general funds per student.² According to anecdotal evidence and interviews with municipality administrations, the school closures have been announced up to two years in advance. Since we do not have the announcement dates for all school closures, we use the year of the actual school closing in the empirical analysis but do expect anticipation effects.

To lessen concerns about (adjusting) school sizes, we focus in the empirical analysis on jurisdictions with only one grammar school that was closed during our sample period and compare these jurisdictions to similar jurisdictions in which the

¹https://www.destatis.de/DE/Publikationen/Thematisch/BildungForschungKultur/Schulen/BroschuereSchulenBlick0110018169004.pdf?__blob=publicationFile

²Counties are usually responsible for pupil transportation, youth welfare services and schools for children with special needs and the hosting municipality is in charge of the school infrastructure and maintaining school buildings, facilities and equipment.

only grammar school continued to exist. For the average jurisdiction with a school closure we do not find evidence for a reduction in local employment. Splitting the sample according to the cost of commuting (proxied by the distance to the next highway and railway or the share of out-commuters) we identify clear evidence in line with our theoretical predictions. In commuter towns employment is unchanged, while in worker towns it is reduced by around 12% after four years.

To hedge against concerns that our results are driven by differences in preferences for schooling or differences in the schools that have been closed, we also investigate the impact of school closures on house prices using offer price data. Reassuringly, we find that house prices dropped in all jurisdictions with a school closure by about 12%. Thus the jurisdictions as well as the school closures are comparable. Moreover, the similar response of employment and house prices is consistent with the predictions of our stylized theoretical model.

Our paper contributes to several streams of literature: First, we add to the literature that investigates the impact of public good provision on local employment. Despite its' high relevance for local governments, the literature has so far mainly focused on the impact of "firm" public goods such as universities (Bania *et al.* (1993)) or infrastructure (Redding *et al.* (2011), Duranton and Turner (2012), Moeller and Zierer (2018)) on firm location and performance (e.g. productivity) or on the impact of "people" public goods such as schools on individuals' location decision (Buettner and Janeba (2016), Albouy and Lue (2015)). While it is indisputable that firms employ people, decisions on the size of university funding or large infrastructure projects are usually not in the hand of local governments. Further, it is not obvious whether attracting people does lead to more local employment or whether attracted people simply out-commute. Most closely related to our work are thus studies that focus on "people" public goods and/or local employment as Mofidi and Stone (1990), Dalenberg and Partridge (1995) and Gabe and Bell (2004).

Mofidi and Stone (1990) assess the impact of educational spending on manufacturing employment on the state level in the U.S. between 1962 and 1982 and find a positive impact. Dalenberg and Partridge (1995) study employment in 28 metropolitan areas between 1966 and 1981 and find a positive impact of educational spending as well. Gabe and Bell (2004) use data on firm locations between 1993 and 1995 in Maine in the USA. They are able to distinguish between different types of educational spending on the municipality level and identify for some spending categories such as school transport or administration negative and for others such

as the availability of schools that offer education to the twelfth grade a positive impact on the the number of businesses that locate in a particular municipality. The impact is, however, mainly identified using variation within counties as there is not sufficient variation over time. We contribute to this literature by examining the impact of the local availability of grammar schools on employment exploiting variation over time. Our strategy is thus robust to time-invariant unobservables.

Second, we add to the literature on local labor markets (for a review see Moretti (2011)). In a recent contribution Monte *et al.* (2018) highlight the role of commuting differences between jurisdictions for observed local employment elasticities. The authors show that employment effects in response to labor demand shocks are increasing with jurisdictions' commuting openness as moving people is more costly than moving goods. Their main measure of commuting openness is the share of residents who work where they live, which is strongly correlated with our proxy for commuting costs. We believe our paper adds an important dimension as it shows that commuting openness may also have a downside for governments since public goods directed to individuals cannot longer be used to stimulate employment in jurisdictions with close links to other jurisdictions.

Lastly, we contribute to the literature that investigates whether the availability of schools and school quality capitalizes into prices. A substantial part of this literature is concerned with the capitalization of schools and school quality into housing prices and finds a substantial impact (for a recent overview see, e.g., Black and Machin (2011)). In contrast to most work, in which the reasonableness of the estimates for school quality are hard to judge, our approach has the advantage that it allows us to assess the plausibility of our estimates. More precisely, using our estimates we are able to back out the additional costs for families due to the school closures. Our paper speaks, however, also to the literature on the capitalization of public goods into wages. The empirical literature is scare at best, mainly due to data availability.³ One recent exception is Buettner and Janeba (2016). They show that higher spending for theaters and thus lower ticket prices reduces the required wage by high skilled workers in Germany. Our result are in line with their findings as they suggest that also the local availability of grammar schools is capitalized into wages (although not in all jurisdictions but only the ones with high commuting

³For example, while the data in Fuest *et al.* (2018) can in principle also be used to study the impact of public goods on wages, this data can only be used with a special permission by the German Federal Ministry of Finance. Unfortunately, we have not been granted such a special permission yet.

costs).

The remainder of this article is structured as follows. In Section II, we set out a stylized theoretical model to motivate our empirical analysis. The empirical strategies, the data and our results are presented in sections III and IV. Section V concludes.

II. THEORETICAL MOTIVATION

In this section, we outline a stylized theoretical model to understand the mechanism at work. Our set up in mind is that there are many jurisdictions. For simplicity, however, we model only two jurisdictions, a rural town (no subscript) and a large metropolis (m). The latter is assumed to be sufficiently large such that changes in the town do not affect the metropolis. In this sense, we believe that the large metropolis can also be seen as the sum of all other jurisdictions.

We focus on two settings. In the first setting, commuting between town and metropolis is not possible due to high commuting costs. Thus, a worker can either work and live in the town or work and live in the metropolis. In the second case, we allow for commuting. We start with the non-commuting case.

There are N potential workers that supply one unit of labor and are willing to live in the town.⁴ All workers maximize a Cobb-Douglas utility function over consumption (C) and housing (H) (see equation (1)). We abstract from modeling the goods market similar to Suárez Serrato and Zidar (2016) and assume that the price index for good consumption is 1. We include local public good provision in our model by assuming that workers maximize their utility over gross housing, defined as net housing (H^N , housing consumption) times public good valuation ($v(g)$, with $v' > 0$ and $v'' < 0$), but pay only for their net housing (see equation (2) for workers' budget constraint). The idea behind is that most public goods are non-exclusive. Thus, workers always have access to the public goods but may incur costs to do so, e.g., commuting costs to use the closest public swimming pool, the closest playground or the closest higher education school. Since it is not possible to buy these local public goods in the goods market, we believe that linking them to housing is most appropriate. Intuitively this means that the value of net housing increases with the value of the locally provided public goods.

⁴This means that these workers have a sufficiently high preference for living in the town. While this could be modeled explicitly as in Monte *et al.* (2018), we believe it would only complicate the model without adding further insights as the main channel at work is the wage channel.

$$U = \left(\frac{C}{\alpha}\right)^\alpha \left(\frac{H}{(1-\alpha)}\right)^{(1-\alpha)} \quad (1)$$

s.t.

$$w = C + p \frac{H}{v(g)} \quad (2)$$

For simplicity, we work in the following with workers' indirect utility function, which is given by:

$$\tilde{U} = \frac{w v(g)^{(1-\alpha)}}{p^{(1-\alpha)}} \quad (3)$$

Three dimensions are important to workers: Wages (w), house prices (p) and the value of the public good ($v(g)$). Public good provision is the policy tool we are interested in and we assume, therefore, that it is exogenously determined. Wages are set in a bargaining process between workers and (aggregate) employers. The parties bargain over the public good valuation adjusted wage ($w v(g)$) since the budget constraint (after re-arranging) does not depend on the “gross” but on the adjusted wage.⁵ Based on the assumption that firms have all the bargaining power⁶, the wage in the town equals workers' reservation wage. The wage in the town can thus be obtained by equalizing the (exogenous) public good adjusted wage in the metropolis ($w_m v(g_m)$) and in the town and solving the expression for the wage in the town:

$$w = \frac{w_m v(g_m)}{v(g)} \quad (4)$$

Local employment is given by aggregate labor demand for the given wage. We assume that the labor demand elasticity equals -1 . While this is at the lower end of a recent study for Germany by Lichter *et al.* (2013), we believe it is still a good

⁵While there is evidence that wages adjust to changes in housing prices (see, for example, Winters *et al.* (2009)), this does not mean that wages are set taking housing prices into account. More realistic is that wages and housing prices are driven by common factors as, for example, public good provision or employment (e.g. agglomeration economies).

⁶Relaxing this assumption does not affect the model implications qualitatively. If workers' also receive part of the profits, the question is how strong they change in response to a change in employment. If the profit per worker decreases with employment and the impact on bargaining strength is independent of employment, this would - in case of a reduction in public good provision - predict a smaller increase in wages and thus a lower reduction in employment in jurisdictions with high commuting costs.

approximation as East Germany is characterized by less “manufacturing” jobs (see, for example, Kluge and Weber (2015)) and estimated labor demand elasticities are larger for these type of jobs.⁷

$$L = w^{-1}. \quad (5)$$

From workers’ utility maximization, we know that each worker spends $(1 - \alpha) w v(g)$ for housing and that there are L residents in the town (as commuting is not possible and thus population equals the number of employees). Using Q as housing supply, housing prices are then given by:

$$p = \frac{(1 - \alpha)wv(g)L}{Q} \quad (6)$$

Change in Public Good Provision: Non-Commuter Case

Based on this model set-up, we now derive the impact of a marginal change in public good provision on housing prices, wages and employment. We start with the impact on house prices. Since we will estimate all specifications in logs, we also derive the predictions for the logarized variables. Replacing L in equation (6) with equation (5) shows that wages and employment do not matter for housing prices. Assuming housing supply to be fixed, the first order derivative of $(\ln)$ housing prices with respect to public good provision is given by:

$$\frac{\partial \ln(p)}{\partial g} = \frac{\partial \ln(v(g))}{\partial g} \quad (7)$$

$(\ln)$ Housing prices change, therefore, one to one for a change in $(\ln)$ public good valuation. $(\ln)$ Wages in contrast decrease one to one for an increase in $(\ln)$ public good valuation:

$$\frac{\partial \ln(w)}{\partial g} = -\frac{\partial \ln(v(g))}{\partial g} \quad (8)$$

Finally, the impact on employment has - based on our assumption of a labor demand elasticity of -1 - the opposite sign to the change in wages.

$$\frac{\partial \ln(L)}{\partial g} = \frac{\partial \ln(v(g))}{\partial g} \quad (9)$$

⁷To the extent that the employment demand elasticity is smaller (larger) than 1 in absolute terms, this would predict a smaller (larger) response in employment and in house prices.

Change in Public Good Provision: Commuter Case

We now turn to the case where commuting is possible since worker-independent commuting costs c are sufficiently low. Allowing for commuting affects workers' outside option, which is now not longer to move away but to start commuting (based on the assumption that wages are higher in the metropolis). Equalizing the public good adjusted wage of workers and of commuters in the town and solving for the wage in the town gives the wage in the town:

$$w = \frac{w_m}{c} \quad (10)$$

This equation reveals the most important difference to the non-commuting case. Since wages are independent of public good provision in the commuting case, a change in public good provision does not affect wages and, therefore, also not employment.

House prices in the commuting case depend both on the number of workers *and* commuters as well as on their respective wages and the public good valuations. Since we assumed that there are N potential workers willing to live in the town, the number of commuters is given by $N - L$. Further, they earn the same (net-of-commuting cost) wage as the workers in the town (as the wage in the town equals workers' reservation wage). Given that wages, and the population is unchanged, we see that $(\ln)$ house prices changes – as in the non-commuter case – one to one for a change in $(\ln)$ valuation of public good provision.

$$\frac{\partial \ln(p)}{\partial g} = \frac{\partial \ln(v(g))}{\partial g} \quad (11)$$

To sum up, based on a very stylized theoretical model, we expect that a reduction in public good provision reduces employment only in jurisdictions with high-commuting costs as in these jurisdictions wages are a function of the locally provided public goods. In jurisdictions with low-commuting costs, wages are independent of public good provision. As wages remain unchanged, also local employment is not affected. House prices change, however, in jurisdictions with high and low commuting costs to the same extent in response to a change in public good provision if the labor demand elasticity is close to -1 (which we believe to be a realistic assumption for rural labor markets in Eastern Germany).

III. EMPIRICAL STRATEGY AND DESCRIPTIVE STATISTICS

A. Local Employment

To identify the effect of local grammar school closures on local employment we rely on a simple difference-in-differences estimator (DiD). Thus, we compare jurisdictions (municipalities) with and without the closure of their only grammar school before and after the closing. Our baseline fixed effects estimation equation reads as follow:⁸

$$Y_{i,t} = \alpha_i + \beta_1 TR_i * Close_{i,t} + \gamma X_{i,t} + \nu_t + \lambda_i + \epsilon_{i,t} \quad (12)$$

Our dependent variable ($Y_{i,t}$) is (ln) number of employed residents, which includes residents employed in the jurisdiction (residential employment) as well as residents employed in other jurisdictions (out-commuters).⁹ We prefer using the number of employed residents in the baseline specification as also in high commuting cost jurisdictions some worker still commute to close by towns or villages. In additional specifications, we use, however, also a more precise measure for local employment, namely (ln) residential employment.

Our treatment group (TR_i) consists of all jurisdictions with one grammar school in 1997 that was closed until 2008. The data for grammar schools is hand-collected and cross-checked with the number of grammar schools available on the county level for Germany. We focus on jurisdictions with one grammar school as we believe this provides the most credible variation for our analysis. In total, we observe 38 towns that saw their only grammar school closed between 1997-2008 with roughly 6 school closures per year between 2002 and 2008. In principle, school closures in cities with more than one grammar school could also be used if these closures cause excess demand for grammar schools. However, we do not have information on the number of students on the school level and thus are not able to account for school size (changes).¹⁰ Since not all school closures happened in the same year, the reform variable ($Close_{i,t}$) is jurisdiction-specific and is one for all years for which the jurisdiction had one higher secondary education school less. The geographical

⁸In a robustness analysis, we also estimate a Poisson model. Results are basically unchanged and available upon request.

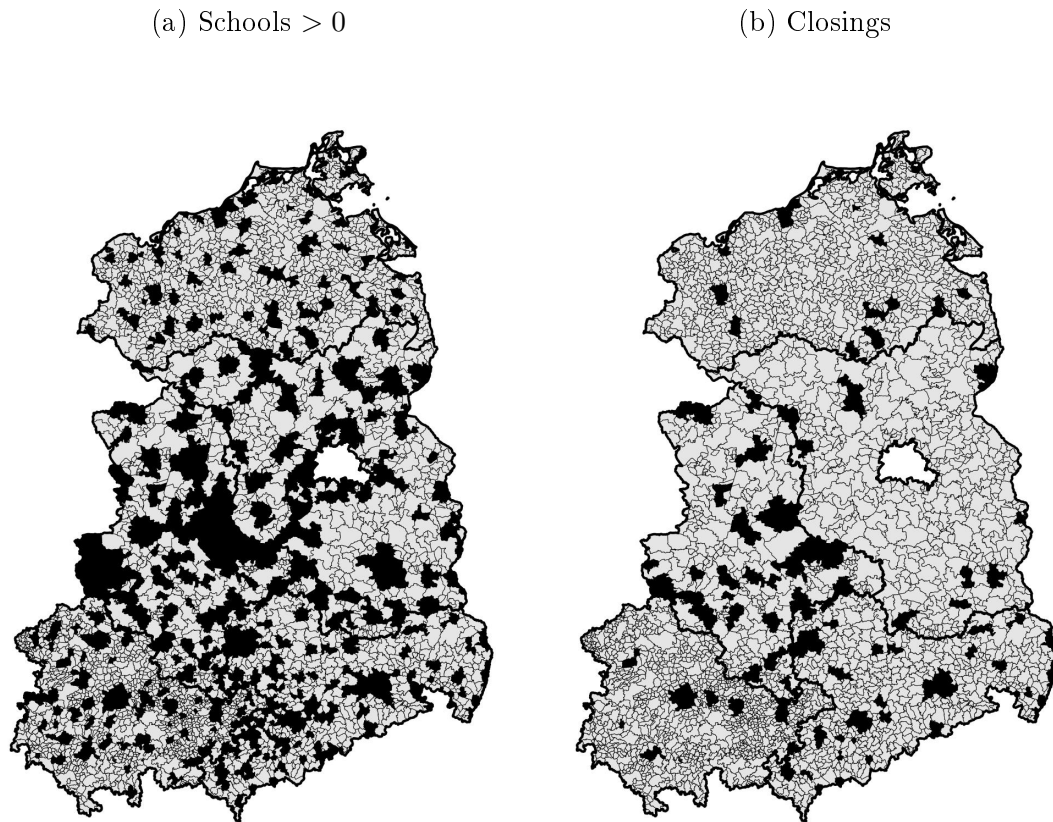
⁹The data for this variable comes from the German Federal Employment Agency.

¹⁰In fact, we observe 83 school closures in towns with more than one grammar school. Note, that we also see a small number of school openings during the observation period. We do not use those small set of observations, because it is mostly very small private grammar schools with a limited number of pupils that have been opened.

location of schools and school closures in East Germany is depicted in Figure (1). The majority of school closures took place in Saxony and Saxony-Anhalt.

Our control group consists of jurisdictions with one grammar school in 1997 which was open at least until 2008. We include only jurisdictions with one grammar school to ensure that treatment and control group jurisdictions are comparable in size without further sample restrictions. In our sample, jurisdictions with one grammar school have between 500 and 30,000 inhabitants.

Figure 1: Schools in 1997 and School Closings between 1997 and 2008 in East-Germany



Notes: The left figure shows whether a jurisdiction in East Germany had at least one grammar school in 1997 and the right figure shows in which jurisdictions in East Germany at least one grammar school was closed between 1997 and 2008. The white area in the middle is Berlin and is not included in the analysis. The darker lines show the borders of the federal states in East Germany.

Our set of control variables, captured in the matrix $X_{i,t}$ includes (ln) population in 1997 and (ln) area size, both interacted with year dummies, the local business tax (which is set by the municipality) and (ln) number of higher education schools within 10km distance. Further, we include (ln) number of employed residents of jurisdiction with a similar size (based on 1997 population quintiles) that are located in the same state and (ln) population of jurisdictions with more than 30.000 inhabitants (in 1997) within 80km radius. With the inclusion of the latter two variables we aim to control for a differential trend of municipalities related to their size and location with respect to larger jurisdictions. Further, we account for municipality mergers that happened during our sample period by including an indicator variable if a merger took place in a particular year, interacted with state dummies.¹¹ All variables stem or are constructed based on data from *Statistik Lokal*, which is provided by the Federal Statistical Office. The variables ν_t and λ_i in equation (12) represent time and municipality fixed effects. We report heteroscedasticity-robust standard errors clustered at the municipality level.

To account for a potential different impact of school closures on local employment in jurisdictions with low and high commuting costs as predicted by our theoretical framework, we construct an indicator variable (C_i) that is one for jurisdictions with low commuting costs. Since commuting costs are not observed, we assume that jurisdictions that are closer to a motorway (in 1997) or a railway station (in 1890) than the median jurisdiction have low commuting costs. In a robustness test, we follow Monte *et al.* (2018) and use the share of out-commuters in 1998 as the splitting criteria.¹² We prefer the first measure as it is less likely to capture unobserved heterogeneity between jurisdictions. The indicator variable is interacted with the treatment and reform interaction (see equation (13)). To address the concern that the additional interaction effect simply captures heterogeneity between jurisdictions, we also include interaction effects between the commuting cost indicator variable and the control variables in additional specifications (except for population and area which are already interacted with year dummies). Finally, we also estimate a more flexible specification which includes 5 leads and 4 lags for the DiD variables (with

¹¹The results are not sensitive to this control variable. This may be not surprising given that we account for the mergers when constructing the data set. More precisely, if two jurisdictions merged in 2005, we treat them from 1997 as one jurisdiction using 1997 population weighted variables. We interact the merger indicator variable with state dummies as mergers differ between states, e.g. some states incentivized voluntary mergers, others not.

¹²We use 1998 data as out-commuters are not observed for all jurisdictions in 1997. Further, we do not have data on railways in 1997 and thus use the 1890 data.

$t + 3$ as excluded baseline category) to inspect the validity of the common trend assumption.

$$Y_{i,t} = \alpha_i + \beta_1 TR_i * Close_{i,t} + \beta_2 TR_i * Close_{i,t} * C_i + \gamma X_{i,t} + \nu_t + \lambda_i + \epsilon_{i,t} \quad (13)$$

Descriptive statistics for treatment and control group for 1997 are reported in Table (1). The average control group jurisdiction is larger than the treatment group jurisdiction, with respect to the population as well as the number of employed residents. Comparing jurisdictions with respect to their commuting costs gives a similar picture. Commuter towns are somewhat larger in terms of employed residents as well as population.

Table 1: Descriptive Statistics Local Employment Sample for 1997.

Commuting Costs	Control Group		Treatment Group	
	High	Low	High	Low
Employed Residents in 1000	4.44 (2.61)	4.93 (2.54)	3.17 (1.02)	3.62 (1.42)
Population in 1000	12.04 (7.11)	13.32 (7.25)	8.67 (3.27)	9.87 (4.64)
Area in km ²	79.68 (79.90)	69.59 (60.09)	93.67 (66.77)	63.48 (44.17)
# Schools (0 – 10km)	0.25 (0.42)	0.51 (0.67)	0.05 (0.19)	0.73 (0.62)
Local Business Tax in 100 Points	324 (37)	327 (39)	330 (42)	339 (33)
Observations	42	166	14	24

Notes: Table shows descriptive statistics for the local employment sample for treatment and control group with high and low commuting costs in 1997. The treatment group includes jurisdictions in which the only grammar school was closed between 1997 and 2008 and the control group jurisdictions in which the only grammar school was not closed between 1997 and 2008. Jurisdictions have low commuting costs if they are closer to a railway station or a motorway than the median jurisdiction. Employed residents include residents that are employed in the jurisdiction (residential employees) as well as in other jurisdictions (out-commuters). *Source:* Authors' calculations based on data from Federal Employment Agency and Statistic Local, 1997 – 2008.

B. House Prices

To strengthen our analysis of the impact of local grammar school closures on local employment, we complement it by an investigation of the impact of grammar school closures on house prices using the same strategy, namely a DiD estimator. If jurisdictions are comparable and the nature of the school closures are the same in

jurisdictions with high and low commuting costs (and the labor demand elasticity is close to -1) we expect that house prices drop independent of the commuting openness of a jurisdiction.

The data for the house price analysis stems from the Empirica AG and includes offer price data for a wide range of objects from newspapers as well as online ads covering the years 2004 to 2008.¹³ We merge municipality information to this data set using information on the location of the properties. The following types of properties are included in the analysis: single family homes, semi-detached houses, and terraced houses. We focus on purchase offers and not rental offers as housing markets in mid-sized towns in East Germany are mainly purchase markets. Although the offer price data has the potential disadvantage of selection driving the results, we believe that this is addressed by our estimation strategy, which accounts for municipality and time fixed effects.

As houses are usually sold with land, we use the natural logarithm of the overall price as the dependent variable and control for the amount of land, the floor space and the interaction between the two. In addition, our analysis controls for the type of property (single house, detached or terraced house) as well as the condition of the property (high quality, newly built, renovated, in need of renovation) and the availability of balcony, garage and basement. Although we have additional information for some properties (e.g. location in the city, close to public transport or not, etc), we decided not to use them as adverts differ strongly regarding the non-essential information included in the ad. Thus, we are concerned that additional property characteristics would not increase the precision of the estimates but only cause a selection bias. Besides the property controls, we also include the average construction year of properties in the municipality (as it is missing for many properties) as well as the same control variables as used in the analysis of local employment.

Descriptive statistics for the house price sample are shown in Table (2). A house in the sample costs on average between 120,000 and 155,000 Euro and comes with between 780 and 1,180 square meter of land and with between 133 to 140 square meter of living space. The houses in the treatment group are substantially less expensive (139 to 155 compared to 121 to 126 thousand Euro).

¹³Given that this is a private database, we drop properties with floorspace or amount of land in the top and bottom 1% to ensure that outliers are not driving the results.

Table 2: Descriptive Statistics House Price Sample for 2004 to 2008.

Commuting Costs	Control		Treatment	
	Low High	High Low	Low High	High Low
Price	139,513 (86,683)	155,585 (97,155)	126,882 (75,829)	121,188 (66,180)
Amount of Land	906 (947)	779 (746)	1,182 (1,160)	787 (713)
Living Space	137 (62)	136 (57)	140 (60)	133 (52)
Single Family House	0.79 (0.41)	0.78 (0.41)	0.84 (0.37)	0.79 (0.41)
Detached House	0.13 (0.34)	0.13 (0.33)	0.12 (0.32)	0.12 (0.32)
Terraced House	0.08 (0.26)	0.09 (0.28)	0.05 (0.21)	0.09 (0.29)
Balcony	0.09 (0.29)	0.09 (0.28)	0.09 (0.29)	0.08 (0.28)
Garage	0.56 (0.50)	0.53 (0.50)	0.56 (0.50)	0.53 (0.50)
Basement	0.17 (0.37)	0.24 (0.43)	0.21 (0.41)	0.22 (0.41)
High Quality	0.10 (0.30)	0.13 (0.33)	0.09 (0.29)	0.09 (0.29)
Renovated	0.23 (0.42)	0.18 (0.39)	0.19 (0.39)	0.18 (0.39)
In Need of Renovation	0.23 (0.42)	0.20 (0.40)	0.24 (0.43)	0.19 (0.39)
Newly Build	0.07 (0.26)	0.07 (0.25)	0.05 (0.22)	0.07 (0.26)
Population in 1000 (1997)	14.41 (6.00)	15.72 (6.83)	8.57 (2.59)	11.98 (4.68)
Area in km ²	80.57 (63.65)	71.28 (53.14)	83.89 (43.10)	70.25 (35.25)
# Schools (0 – 10km)	0.37 (0.64)	1.68 (2.98)	0.05 (0.22)	1.33 (1.24)
Local Business Tax Multiplier	334.11 (39.47)	338.65 (40.68)	348.24 (37.47)	354.36 (31.26)
Property Tax Multiplier	340.63 (26.77)	362.34 (41.91)	362.35 (31.45)	372.88 (38.99)
Observations	8,624	49,372	1,666	4,004

Notes: Table shows descriptive statistics for the house price sample for treatment and control group with high and low commuting costs for 2004 to 2008. The treatment group includes jurisdictions in which the only grammar school was closed between 1998 and 2008 and the control group jurisdictions in which the only grammar school was not closed between 1998 and 2008. Jurisdictions have low commuting costs if they are closer to a railway station or a motorway than the median jurisdictions. *Source:* Authors' calculations based on data from Federal Employment Agency, Statistic Local and Empirica AG 1997 – 2008.

IV. RESULTS

A. Local Employment

In the following we present the results of our DiD strategy. We start with the results using $(\ln)$ number of employed residents as the dependent variable (see Table (3)). In the first two columns we do not distinguish between jurisdictions with high and low commuting costs and here do not find any evidence that school closures affect local employment, neither when only controlling for jurisdictions' size (col. (1)) nor with our full set of control variables (col. (2)). In col. (3) to (6) we include the additional interaction term between the DiD variable and the commuting costs indicator variable. The results in these specifications, which differ with respect to the inclusion of additional control variables, are statistically and economically very similar and in line with the theoretical predictions: School closures only reduce employment in jurisdictions with high commuting costs but have no impact in jurisdictions with low commuting costs. Quantitatively, the effect size is substantial as it suggests that the average jurisdictions with high commuting costs in which a grammar school was closed loses between 6 and 8% of its employed residents.

Col. (3) to (6) differ as follow: Col. (3) shows the baseline specification with our full set of control variables. From col. (4) onwards we additionally control for state-year fixed effects. Starting in col. (5) we also add interaction effects between the commuting costs indicator variable and the control variables to assess whether our variable of interest solely captures heterogeneity between jurisdictions. This is, however, not the case. Finally, in col. (6) we address the fact that there are substantially more schools in neighboring jurisdictions of commuter jurisdictions (see Table (1)). This could bias our results if the additional commute due to the school closure is less in commuter jurisdictions. Thus, we include interactions effects of the DiD variables with the number of schools in neighboring jurisdiction. While the point estimates for our variables of interest changes little, an interesting effect emerges. In worker towns, a higher number of grammar schools in neighboring jurisdictions does mitigate the negative impact on employment. This relationship is, however, absent for jurisdictions with a low commuting costs.

Since the validity of the DiD approach is based on the common trend assumption, we investigate this now in detail by estimating a more flexible specification, which includes five leads and four lags. The excluded baseline category is $t + 3$ as school closures have been announced up to 2 years in advance. The point estimates are

Table 3: Estimation Results for Local Employment

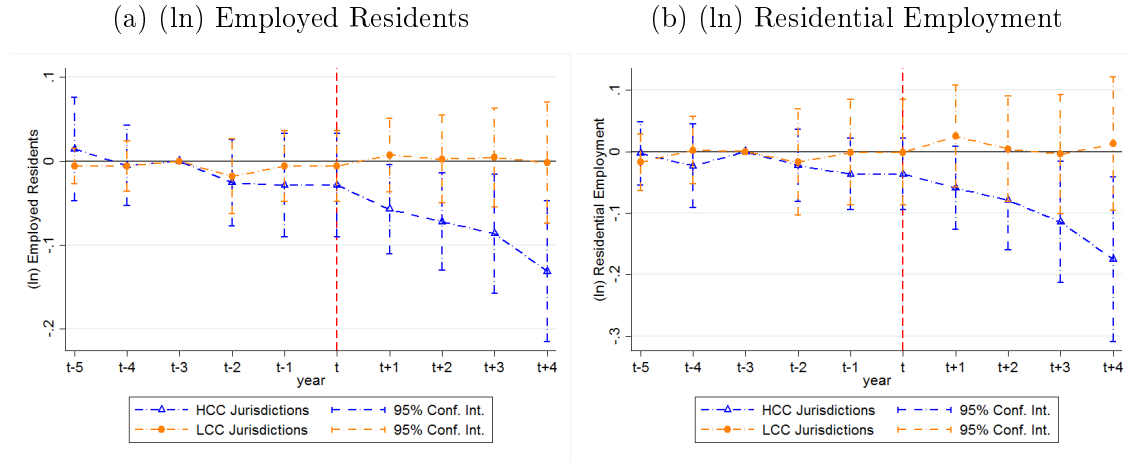
	<i>Dependent Variable: (ln) Employed Residents</i>					
	(1)	(2)	(3)	(4)	(5)	(6)
TR X Close	-0.019 (0.018)	-0.022 (0.018)	-0.087*** (0.025)	-0.079*** (0.024)	-0.065*** (0.023)	-0.071*** (0.024)
TR X Close X Low Commuting Costs (LCC)			0.101*** (0.030)	0.089*** (0.030)	0.071** (0.029)	0.052* (0.030)
TR X Close X (ln) # Schools (0 – 10km)						0.077*** (0.026)
TR X Close X (ln) # Schools (0 – 10km) X LCC						-0.060** (0.030)
(ln) # Schools (0 – 10km)		-0.014 (0.025)	-0.012 (0.025)	-0.015 (0.025)	0.128*** (0.029)	0.127*** (0.029)
(ln) # Schools (0 – 10km) X LCC					-0.157*** (0.035)	-0.160*** (0.035)
(ln) Local Business Tax		0.035 (0.055)	0.042 (0.055)	0.013 (0.051)	-0.050 (0.115)	-0.048 (0.116)
(ln) Local Business Tax X LCC					0.076 (0.125)	0.076 (0.125)
Observations	2,653	2,653	2,653	2,653	2,653	2,653
R^2	0.60	0.64	0.65	0.67	0.68	0.68
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓
(ln) Population (1997) X Year	✓	✓	✓	✓	✓	✓
(ln) Area X Year	✓	✓	✓	✓	✓	✓
Control Variables		✓	✓	✓	✓	✓
State X Year Fixed Effects				✓	✓	✓
Control Variables X LCC					✓	✓

Notes: Table shows regression result for the impact of grammar school closures in jurisdictions with high and low commuting costs on (ln) employed residents. TR is an indicator variable that is one if a jurisdiction experienced a school closure between 1997 and 2008. Close is an indicator variable that is one if the jurisdiction has a grammar school less compared to 1997. LCC is an indicator variable that is one if a jurisdiction has low commuting costs. Jurisdictions have low commuting costs if they are closer to a railway station or a motorway than the median jurisdictions. In col. (1) and (2) we do not distinguish between treated jurisdictions with high and low commuting costs but do so in col. (3) to (6). In col. (1) we only control for jurisdiction size and from col. (2) onwards we use our full set of control variables. From col. (4) onwards we additionally include state-year fixed effects and from col. (5) onwards also interaction effects between the control variables and the low commuting cost indicator variable. In col. (6) we additionally control for a differential treatment effect depending on how many schools are in neighboring jurisdictions. Robust standard errors clustered at the municipality level in parenthesis. *, **, *** denote significance at the 10%, 5% and 1% level. *Source:* Authors' calculations based on data from Federal Employment Agency and Statistic Local 1997-2008.

illustrated on the left hand side of Figure (2). Reassuringly, the point estimates for the leads suggests a common trend before the school closures. Moreover, the point estimates for the lags show that the treatment effect is increasing. More precisely, our estimation results suggest that the number of employed residents is by around 12% lower 4 years after the school closure.

To rule out that the results are driven by the response of out-commuters, we use in a robustness test $(\ln)$ residential employment, which includes only residents employed in the jurisdiction. The point estimate for this specification are shown on the right hand side of Figure (2). The pattern of the point estimates is very similar: While there is a common trend before the school closures, residential employment decreases only in jurisdictions with high commuting costs. The main difference between using employed residents vs residential employment is that the effect size is somewhat larger for the latter and suggests a reduction in residential employment by around 15% 4 years after the school closure.

Figure 2: Estimation Results for Leads and Lags using Employed Residents and Residential Employment as Dependent Variable



Notes: The figures depict the point estimates and 95% confidence intervals using a regression with five leads and four lags of the DiD variables for the school closures and the same control variables as in col. (5) in Table (3). The dependent variable in the left figure is $(\ln)$ number of employed residents (residents employed in the jurisdiction or elsewhere) and in the right figure $(\ln)$ residential employment (residents employed in the jurisdiction). HCC stands for high commuting costs jurisdictions and LCC for low commuting cost jurisdictions. A jurisdiction has low commuting costs if it is closer to a motorway or a railway station than the median jurisdiction. *Source:* Authors' calculations based on data from Federal Employment Agency and Statistic Local 1997-2008.

In another robustness check, we use $(\ln)$ working age population (population aged

between 20 and 65 years) as dependent variable. The point estimates are shown on the left hand side of Figure (3). While the general pattern is again very similar, two aspects are worth mentioning. First, there is some evidence that the working age population decreases also in treated jurisdictions with high-commuting costs but this effect is only temporary. One potential explanation for this is sorting of families. While some families dislike commuting and thus prefer moving away, others' don't mind and thus move into the jurisdiction. Since the effect sizes are still very different between jurisdictions with high and low commuting costs, we believe that this finding does not contradict the intuition outlined in the model. Second, the effect size is slightly smaller compared to using all employed residents as dependent variable (-10 compared to -12%). This suggests that not all workers that loose their jobs move away. To inspect this further, we use the unemployment rate in the jurisdiction as dependent variable (see right hand side of Figure (3)). While there is no evidence for a change in the unemployment rate in low commuting cost jurisdictions, the unemployment rate increases after the school closures in jurisdictions with high commuting costs. The results suggest that around 83% of the workers that lost their jobs move away and 17% of the worker stay in the jurisdiction but are unemployed.

In the last sensitivity check, we assess whether our proxy for commuting costs drives the results. Thus, we run the same regression using the share of out-commuters to group jurisdictions together with low and high commuting costs. The left hand side of Figure (4) shows the results using (ln) number of employed residents and the right hand side of Figure (4) using (ln) working age population as dependent variable. The patterns are again very similar. Thus, we conclude that local grammar school closures reduce local employment only in jurisdictions with high commuting costs.

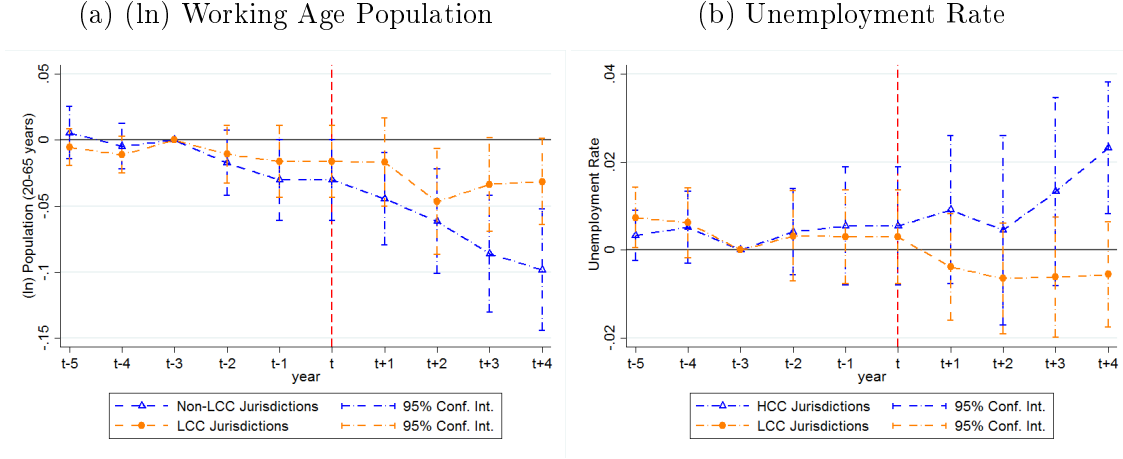
B. Housing Prices

The result for the impact of grammar school closures on house prices are reported in Table (4). Following the theoretical framework, we expect that house prices should drop to a similar extent in treated jurisdictions with high and low commuting costs if the jurisdictions as well as the school closures are comparable.

In the first two columns, we do not distinguish between jurisdictions with low and high commuting costs. To address the fact that housing markets are forward looking we include, however, two leads.¹⁴ While the treatment effect in $t+2$ is

¹⁴Since we only have four years and account for municipality fixed effects, we refrain from using

Figure 3: Estimation Results for Leads and Lags using Working Age Population and Unemployment Rate as Dependent Variable

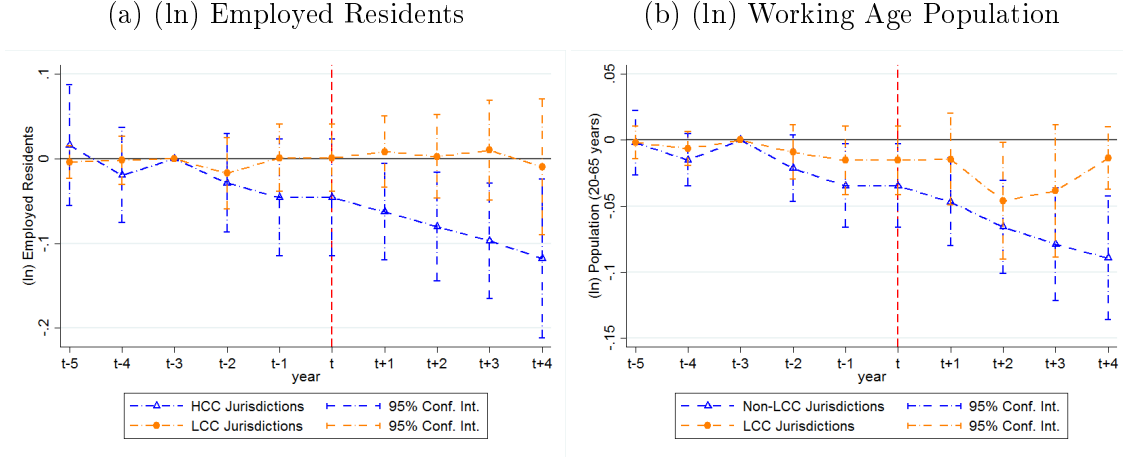


Notes: The figures depict point estimates and 95% confidence intervals using a regression with five leads and four lags of the DiD variables for the school closures and the same control variables as in col. (5) in Table (3). The dependent variable in the left figure is (ln) working age population (residents aged between 20 and 65 years) and in the right figure the unemployment rate (unemployed over working age population). HCC stands for high commuting costs jurisdictions and LCC for low commuting cost jurisdictions. A jurisdiction has low commuting costs if it is closer to a motorway or a railway station than the median jurisdiction. *Source:* Authors' calculations based on data from Federal Employment Agency and Statistic Local 1997 – 2008.

close to zero and insignificant, the treatment effect for $t+1$ and t is negative and statistically significantly different from zero. Thus, house prices respond already in expectation to the school closure, which is consistent with the fact that the school closures have been announced up to 2 years in advance. From col. (3) onwards we include the interaction effect between treatment and reform interaction and the commuting cost indicator variable. The point estimates for the interaction effects are small and insignificant. This suggests that house prices dropped in jurisdictions with low and high commuting costs to a similar extent and thus that the jurisdictions as well as the school closures are comparable. In col. (4) we include in addition state-year fixed effects, which has, however, almost no impact on the results. In col. (5) we additionally control for interaction effects between the control variables and the commuting costs indicator variable, which also has almost no impact on the results. Finally, in col. (6) we account for the fact that there are more schools in

more leads. In a sensitivity analysis, we included, however, also lags, which turn out to be close to zero and insignificant. Thus, as expected the effect takes place immediately and not with a time lag as for employment.

Figure 4: Estimation Results for Leads and Lags using Share of Out-Commuters to Proxy Commuting Costs in a Jurisdiction



Notes: The figures depict the point estimates and 95% confidence intervals using a regression with five leads and four lags of the DiD variables for the school closures and the same control variables as in col. (5) in Table (3). The dependent variable in the left figure is (ln) number of employed residents and in the right figure (ln) working age population. HCC stands for high commuting costs jurisdictions and LCC for low commuting cost jurisdictions. The main difference to Figures (2) and (3) is that jurisdictions are grouped into low and high commuting costs based on their share of out-commuters in 1998 (using the median as threshold).

neighboring jurisdictions of low commuting cost jurisdictions, but again results are unchanged.¹⁵

Quantitatively our results suggests that house prices dropped by around 12% if a jurisdictions' only grammar school was closed. This is very similar to the change in employment as predicted by our theoretical model. Given that the reduction in house prices should reflect the costs of the additional commute caused by the school closure, we can back out the additional cost using our estimates. The average house price in treated jurisdictions is 122,000 Euro, which suggests an absolute house price drop of around 14,000 Euro. Given that pupils go on average 200 days for 8 years to a grammar school, this suggest additional costs of 10 EURO per school day (using a discount rate of 3%). Thus, if we believe that the additional commute is 0.5 hour per day (and that the direct costs as, for example, for fuel are negligible) we would

¹⁵In another not reported robustness check we assess whether the point estimates are driven by the estimation on the property level. Thus, we collapse the data to the municipality level and re-estimate the specification shown in col. (2). The point estimates for the treatment and reform interaction in $t+2$, $t+1$ and t (p-value) are 0.00 (0.989), -0.054 (0.174) and -0.095 (0.066). Thus, while they are somewhat smaller, they are otherwise very similar.

derive at a hourly rate of 20 EURO, which seems very reasonable.

V. CONCLUSION

This article evaluates the importance of local schools for a jurisdictions' economy. Motivated by a stylized theoretical model, we investigate empirically the potentially differential impact of public good provision on local employment depending on the openness of a jurisdiction. For identification we focus on school closures in East Germany between 1997 and 2008 and employ a DiD estimator. We find that local grammar school closures decrease the number of employed residents after four years by roughly 12% in jurisdictions with high commuting costs but that there is no impact on local employment in jurisdictions with low commuting costs. The reason for the differential effect is the different impact of public good provision on wages. While there is a negative relationship between public good provision and wages in jurisdictions with high commuting costs, wages are independent of public good provision in jurisdictions with low commuting costs as workers' outside option is to commute and not to move away. Reassuringly, we also find that house prices dropped in jurisdictions with high and low commuting costs by around 12%. This suggest that the differential impact of the school closures is indeed related to the wage channel and not to differences in preferences or differences in the schools closed.

We believe our work has two important implications: First, our work shows that public good provision (specifically schools) can be used in jurisdictions with high commuting costs to stimulate employment. This is certainly good news, but also means that if public good provision is more and more centralized in rural areas, it will fortify the trend in urbanization. Second, we find that commuting patterns matter not only for local employment elasticities as highlighted in Monte *et al.* (2018) but also for the impact of public good provision on employment. We believe a particular important avenue for future research is to show explicitly the differential capitalization of public good provision into wages depending on the commuting openness of a jurisdiction.

Table 4: Estimation Results for House Prices

	<i>Dependent Variable: (ln) House Price</i>					
	(1)	(2)	(3)	(4)	(5)	(6)
	b/se	b/se	b/se	b/se	b/se	b/se
Close (t+2)	-0.023 (0.035)	-0.015 (0.036)	-0.016 (0.027)	-0.008 (0.030)	-0.009 (0.029)	-0.010 (0.032)
Close (t+2) X Low Commuting Costs (LCC)			0.004 (0.059)	-0.010 (0.061)	-0.007 (0.060)	-0.006 (0.062)
Close (t+1)	-0.073* (0.040)	-0.068* (0.039)	-0.084** (0.037)	-0.086** (0.043)	-0.089* (0.048)	-0.093* (0.054)
Close (t+1) X Low Commuting Costs (LCC)			0.024 (0.065)	0.016 (0.066)	0.022 (0.070)	0.026 (0.074)
TR X Close	-0.103** (0.043)	-0.109*** (0.041)	-0.120*** (0.036)	-0.133*** (0.032)	-0.139*** (0.038)	-0.129*** (0.041)
TR X Close X Low Commuting Costs (LCC)			0.018 (0.060)	0.009 (0.055)	0.021 (0.061)	0.007 (0.082)
TR X (ln) # Schools (0 – 10km)						-0.065 (0.078)
TR X (ln) # Schools (0 – 10km) X LCC						0.071 (0.093)
(ln) Property Tax		-0.048 (0.089)	-0.048 (0.089)	-0.062 (0.090)	-0.056 (0.091)	-0.030 (0.467)
(ln) Property Tax x LCC						-0.026 (0.473)
(ln) Local Business Tax		-0.002 (0.033)	-0.002 (0.033)	0.007 (0.041)	0.486 (0.464)	0.475 (0.526)
(ln) Local Business Tax x LCC					-0.489 (0.466)	-0.478 (0.528)
(ln) # Schools (0 – 10km)		-0.005 (0.033)	-0.005 (0.033)	-0.020 (0.030)	-0.275*** (0.046)	-0.275*** (0.050)
(ln) # Schools (0 – 10km) X LCC					0.274*** (0.054)	0.274*** (0.060)
N	63,666	63,666	63,666	63,666	63,666	63,666
R ²	0.24	0.24	0.24	0.24	0.24	0.24
Property Controls	✓	✓	✓	✓	✓	✓
Municipality Fixed Effects	✓	✓	✓	✓	✓	✓
(ln) Population (1997) X Year	✓	✓	✓	✓	✓	✓
(ln) Area X Year	✓	✓	✓	✓	✓	✓
Control Variables		✓	✓	✓	✓	✓
State X Year Fixed Effects				✓	✓	✓
Control Variables X LCC					✓	✓

Notes: Table shows regression result for the impact of grammar school closure in jurisdictions with high and low commuting costs on (ln) house prices. TR is an indicator variable that is one if a jurisdiction experienced a school closure between 1997 and 2008. Close is an indicator variable that is one if the jurisdiction has a grammar school less compared to 1997. LCC is an indicator variable that is one if a jurisdiction has low commuting costs. Jurisdictions have low commuting costs if they are closer to a railway station or a motorway than the median jurisdictions. In col. (1) and (2) we do not distinguish between treated jurisdictions with high and low commuting costs but do so in col. (3) to (6). In col. (1) we only control for jurisdiction size and from col. (2) onwards we use our full set of control variables. From col. (4) onwards we additionally include state-year fixed effects and from col. (5) onwards also interaction effects between the control variables and the low commuting cost indicator variable. In col. (6) we additionally control for a differential treatment effect depending on how many schools are in neighboring jurisdictions. Robust standard errors clustered at the municipality level in parenthesis. *, **, *** denote significance at the 10%, 5% and 1% level. *Source:* Authors' calculations based on data from Federal Employment Agency, Statistic Local and Empirica AG 2004 – 2008.

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