School, what is it good for?

The politics and economics of public education in 19th century Habsburg Empire

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Abstract: The rise of education has featured prominently in the debate on the sources of modern long-term economic growth. Existing accounts stress the role of public education and the importance of political support for its provision. We argue that such an explanation for the spread of schooling is probably a poor fit for many nations' schooling histories and provide an example, using detailed data on schooling supply from the Habsburg Empire. We show that while economic development made schooling more affordable and widespread, the politics of demand for schools was not motivated by expectations of economic development but by the ongoing conflict between nationalities within the Empire. We find that public schools were scarcely useful from an economic point of view, yet they did enjoy significant political and financial support from local political elites, if they offered the "right" language of instruction. Our results suggest that, for some countries at least, the main link, historically, went from economic development to public schooling, not the other way round.

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1. Introduction

Modern economic growth has been correlated with concurrent increase in schooling and human capital accumulation. It has been investigated in numerous Barro-style cross-sectional regressions and proposals regarding the provision of schooling count among core policy recommendations issued to developing countries around the world (Sala-i-Martin, 1997). The strong correlation between human capital and growth appears not only in the cross-section but also across time. It has become an integral part of theoretical modeling of industrialization, long-term growth and the accompanying demographic transition (Galor, 2011).

There is less certainty about how the human capital is accumulated and what kind of human capital exactly matters for growth. Formal schooling has long been considered one of the most efficient channels (Easterlin, 1981). This line of argument, recently revisited by Lindert (2004), Go and Lindert (2010), Mariscal and Sokoloff (2000) and others, sees modern growth as a consequence of (among other things) the rise of national public systems of education which themselves were the product of increased political voice and, eventually, electoral support for tax-based schooling. On closer inspection, any of these causal links becomes more complicated. While Becker et al. (2009) claim that Prussia caught up with UK only thanks to her schools, Mitch (1999) argues that Britain's industrialization was orthogonal to her educational system. Sandberg (1979) cites Sweden as a case of "impoverished sophisticate" where human capital reportedly stood entirely out of proportion to the country's level of income. As for the link from political voice to education, the impulse for nation-wide education may come (and has come in many instances) not from savvy voters but from the ruling elites whose motives had more to do with political control than economic development (Van Horn Melton, 1988). Even democratic politics must contend with questions about who may or may not enroll, who pays for the schools,

what is to be taught and how and it is far from obvious that the answer has always been "more, better, broader" (Naidu, 2012; Palma & Reis, 2012).

We introduce more nuance to the argument by investigating these details of public education decisions. Our research questions lie at the heart of the Easterlin-Lindert story. Using data from the Habsburg Empire, we ask: how well was its educational system responding to (and thereby aiding) economic development? How did the provision of schooling infrastructure interact with the Empire's economic development? How important was educational politics vis-à-vis economic factors? How did individuals respond to the public schooling provision and to the economic development in making their decisions about investing in human capital? In place of the Easterlin-Lindert story of credit-constrained but newly enfranchised poor parents wisely voting themselves more public school provision to be financed by stingy elites to further the economic fortunes of their children and their country, we describe a system where the local elite foists a politically-motivated and economically irrelevant education on lukewarm masses while making them pay for it mostly out of their own pockets.[1] Our hypothesis is not new (Lindert, 2004: 100-103) but, as far as we are aware, ours is the first attempt to empirically test it using historical statistical evidence.

2. Explaining the rise of schooling

A frequent point of departure for the literature on the provision of schooling and economic growth is the high cross-sectional variation among nation states (Easterlin, 1981). Lindert (2004) opens by noting that Britain, the leader in school enrollment in early 19th century, was overtaken by 1880 by France and Germany (Prussia). His explanation is that for a widely

[1] Easterlin (1981: 10-11), citing the example of Church-controlled Spanish educational system, is aware of the fact that not all kinds of formal schooling are equally useful, yet he still sees the rise of schooling primarily in the context of democratization of opportunity and of the political life, noting that absolute monarchies are usually suspicious of mass education's subversive potential.

available schooling system to develop, three ingredients had to come together: local autonomy so that local decision-makers could appropriately respond to local economic developments, political voice, i.e. a mechanism whereby broad public support for tax-based schooling could be converted into actual policy, and low-cost provision which amounted to cheap, abundant teaching staff. The reason why schooling almost always ended up being publicly financed in spite of being among the most profitable investments was that most of the population was credit-constrained and positive externalities were too weak to interest moneyed local elites in generating critical mass of schooling through philanthropic activity. The argument was further developed in Go and Lindert (2007, 2010) where it was tested on enrollment and schooling data from US censuses of 1840 and 1850. This county-level analysis used information on votes cast in presidential elections and property restrictions on eligibility to state legislatures as a measure of political voice and showed a positive effect of political voice on enrollment and on public spending per pupil. Studies in similar vein have appeared or are under way for Britain, Brazil, Russia, India and China (Mitch, 2012; Chaudhary et al., 2011; Musacchio et al., 2012).

One recurring problem that these studies encounter is that they have to work around a lack of suitable reliable data. An analysis of schooling provision would ideally require data that are both sufficiently broad in scope to encompass all the necessary economic, political and educational indicators and sufficiently detailed geographically so as to capture the local variation. As it stands, educational statistics (enrollments, attendance records, age-schooling profiles) are often unreliable or incomplete; economic statistics, such as GDP per capita or real wages, rarely exist on the sub-national level; and measures of political voice are often hard to construct and interpret. As a result, both Chaudhary et al. (2011) and Musacchio et al. (2012) mostly have to stay at the level of federal states or corresponding units, which is a considerably greater level of aggregation than Go and Lindert's (2010) US counties. Go and Lindert (2007,

2010), on the other hand, have no economic variables on the local county level and given that the whole education-growth nexus is riddled with endogeneity anyway, they propose to sidestep the issue and estimate the demand (and supply) of education in reduced form.

There is a further issue with measures of political voice. Go & Lindert's (2010) choice of votes cast in presidential elections makes good sense in the context of American political institutions but for most other countries such measure is too restrictive, if it exists at all. Many European countries at the time of Industrial Revolution had scarcely any democratic institutions and the variation in suffrage, where it existed, was small across localities. This does not mean that various special interests and segments of population did not have a way to voice their concerns; it does mean, however, that their political voice was much less formalized and thus much harder to measure.

How exactly that political voice would shape educational policies is also far from obvious. The cited studies usually posit the issue in the form of a dichotomy between elites who were ambivalent about educating the masses and strongly opposed to having pay for it and the general population, which would demand more educational infrastructure if only it had more political clout. Musacchio et al. (2012), for example, show that exogenous positive shocks to various states of the Brazilian federation had differential impact on local public spending on education depending on whether the state's institutions were more or less democratic. Chaudhary et al. (2011) present case studies of the BRIC countries, arguing along similar lines. But in many cases, among which the Habsburg Empire is one, the original and continuing impetus for the spread of primary schooling came decidedly from the top of the political hierarchy. Palma and Reis (2012), using Portugal as their example, go so far a to argue that an authoritarian state may be, for various reasons, more effective in achieving literacy than a republican regime. In other cases, the dichotomy between centralization and decentralization is false, as many educational systems

settled for some hybrid arrangement. Such would be the case in Prussia (as well as the Habsburg Empire) where the oversight over content of education was relatively centralized and tightly controlled while the school financing was local. Under such circumstances, the local popular demand for more educational infrastructure will likely depend on what kind of education the state deems allowable. Ficker (1873) documents, for example, that for the whole first half of the 19th century, the Austrian government pushed for the spread of primary schooling but resisted the growth of secondary, particularly technical, schooling.

All things considered, not all demand for education takes the form of public or political action, nor does a political activity necessarily reflect widespread individual demand among the local population. Our contribution is to attempt (i) to separate individual demand for more education, driven presumably by rising returns to education, from the public/political demand for more educational facilities and (ii) in analyzing the public demand for educational infrastructure, to separate the influence of economic development from that of political clout/voice. We exploit the rich detail of schooling information in the Habsburg school census of 1865 and combine it with data on local railroad access and use of steam power that proxies for local economic development. We also have data on all secondary schools within the empire, so as to account for that part of the returns to primary schooling that consists in enabling a student to continue with his (but not her!) education. Finally, we link our education data with information on local ethnic composition to capture the political aspect of the problem: since education content was centrally determined and school provision and attendance were compulsory by law, the language of instruction was education's most prominent feature of local political import.[2] We use this factor to test the importance of political voice in schooling.

[2] Other aspects, such as content and teaching methods, were determined centrally, not locally, and local religious variation had ceased to be a political matter, given that school curriculum allowed for separate religious instruction for Protestant children.

3. Education in the Habsburg Empire cca 1865

From the start, the spread of primary education through the Empire was shaped by government policy. The schooling law of 1774 is considered the foundational act of systematic primary schooling. It introduced several basic features that survived until the next major reform of 1869, such as the 6-year compulsory schooling for both boys and girls aged 6 to 12, the stress on religion and the *trivium* in education, the distinction between two-grade country schools and 3- or 4-grade town schools, the compulsory certification of teachers and the strict control of the curriculum. An amendment of this law, promulgated in 1805 during the Napoleonic Wars, tightened government control over the schooling system and, in consequence of the Habsburgs' less than fortunate encounters with the ideas of the French Revolution, reinforced its conservative thrust. The system also betrayed a certain preference for (though not insistence on) instruction in German.[3] Situations where "Romanian children were taught by a Polish priest in German" (Prausek, 1868) were not unheard of and it opened the system to accusations of Germanization. The revolutions of 1848 produced a few changes in legislation, most notably an explicit statement in favor of instruction in one's mother tongue, freedom of teaching methods, an expansion of primary schools from two grades to three grades and an extension of teaching colleges from one year to two. But other attempts at liberal education were soon quashed through the Concordat of 1855, which put the Church yet again firmly in charge of the school supervision and teacher appointments. Even the language provisions were less than perfectly enforced, as we shall see, and the freedom of teaching method fell flat.[4]

[3] The local primary schools were explicitly called "German schools".

[4] The law recommended that teachers commit the teacher manual to memory so as to minimize deviations from it in the classroom. It explicitly stated that "the Bell-Lancaster method" of peer learning, then popular in England, "was forbidden from out class-rooms." Post-1848, teachers were no longer bound by these provisions but, for various reasons, only few teachers took up the opportunity to update their methods (Ficker, 1873: 40).

Our data capture the educational system in 1865, on the eve of major liberal reforms, which secularized the educational system and modernized its structure.[5] Its main institutional features with regards to the duties of citizens and communities to establish and maintain schools, to enforce compulsory schooling and to report to higher authorities, persisted from the 1805 legislation. We describe them in detail to motivate our statistical analysis.

The explicitly stated aim of the educational system was first and foremost to inculcate impressionable youths with loyalty to the Emperor, strong Catholic faith and public morality. Religious instruction therefore occupied a prominent place in the curriculum.[6] Pacher (2008) quotes a "recommended" school timetable where catechism and biblical history took up six out of eighteen weekly lessons for the 9-12 year-olds and four out of nine weekly lessons among the 6-8 year olds. The law stipulated that a school day should preferably begin and end with a lesson of religion (Helfert, 1860: 286). Writing, reading and counting were next in importance, apportioned equally across the remaining weekly lessons, with a few lessons spared for singing. For two-grade schools, i.e. those with one grade for younger children and another for older children, this was the full extent of the curriculum. Some communities could, in agreement with church and civil authorities, extend it by introducing third and fourth grades where subjects like Geography, Nature and Drawing were also included. Passing fourth grade was also a prerequisite for further secondary education.[7] Teachers of non-religious subjects were appointed by church authorities from among graduates of requisite teacher colleges, while religious instruction fell to the local priest.

[5] In describing the system as of mid-1860s, we rely mostly on Helfert (1860).
[6] Protestants could establish their own schools but Protestant teachers were not allowed to teach Catholic children and Protestant children could leave Catholic schools during religious classes.
[7] Children from places with two-grade schools first had to transfer to complete the fourth grade, if they wanted to continue at a *gymnasium* or any other secondary school.

The educational system was compulsory both for the students, who were to attend for six years between their 6th and 12th year of life, and for the local communities, who were responsible for school provision. This consisted of construction and maintenance of the school building, paying teacher salary and providing teacher accommodation. In principle, wherever 100 school-age children lived within half-hour of walking distance, a school was to be built with ideally 80 but certainly no more than 100 pupils per classroom.[8] Contrast these prescriptions with the reality of 1865, presented in Table 1. Column (iii), for example, shows that the new 1849 regulation that all primary schools should be raised from two grades to three was not fully carried out in any province, only in the capital. There was clearly considerable variation in teacher supply and facility provision.[9]

When a school was built, the area around it, usually corresponding to the parish, was considered "covered" (*eingeschult*) and the school-age children living there were obliged by law to enroll and attend the particular local school. Column (ix) shows the extent of that coverage: all provinces were more than 80% covered, although there were districts in the Austrian Littoral where coverage sank below 30%. Less than full coverage indicates that some communities did not meet the stipulations of the law. If a town or a parish failed to provide schooling, the district authorities had at their disposal some carrots in the form of subsidies and some sticks in the form of power to sequester a portion of the local budget and assign it to schooling. The upper echelon of both public and church administration expressed, however, a strong preference for

[8] Whenever the number of pupils present crossed 100, a new classroom was to be built and an extra teacher hired. Alternatively, pupils were to be split into morning class and afternoon class. This was certainly the cheaper way out of capacity constraints and, not surprisingly, the overwhelming favorite of poorer school districts. It also meant, however, that children in those districts received only half the school time compared to children attending full-day schools.

[9] The measure of classrooms availability in column (vi) differs from that in column (v) in that it adjusts for the fact that some schools split their instruction into morning and afternoon classes to make the same classroom go farther. The South-Slav provinces stand out as frequent users of this practice but, as the case of Carniola shows, even that was not enough to keep class size under 100 pupils.

using carrots and avoiding unnecessary conflict between district supervisors and individual communities (Helfert, 1860: 19).

Communities were expected to enforce attendance. Teachers were to maintain both a list of all school-age children eligible for enrollment as well as a careful record of actual attendance.[10] Parents of frequent truants were first notified by a letter. If that failed, they were subpoenaed by local authorities. Further steps included a fine and a one-day arrest. Local police were also obliged to bring to school any children they caught outside during school time. The fruits of the enforcement efforts are reported in column (x). With overall enrollment reaching only 70.2%, it is clear that in many school districts – even whole provinces – schooling was compulsory in name only. This was especially true of the Slav areas – both in the South and in the East – while the German-speaking Alpine provinces were generally close to full enrollment.[11]

This brings us to the ethnic/language question. After the revolutions of 1848, the "Spring of Nations", official policy no longer endorsed education in German explicitly but issues of language of instruction and of public support for non-German schools remained a sore spot practically to the end of the monarchy. Non-German nationalities continuously complained about the residual Germanizing tendencies of the educational system, which was, after all, run by an overwhelmingly German civil and church administrations and designed by German policy makers. Our 1865 dataset includes 22 school districts with zero German students which nonetheless had at least one German or bilingual school. The German public opinion held that since the financing and provision of schools relied on local communities, whatever differences there were between German and non-German districts and between German and non-German

[10] When a village or an area was not covered, the local priest nonetheless had to report to his superiors on the number of children in school age, which is how we came to know their numbers.
[11] Austrian statistics make no conceptual distinction between enrollment and attendance. The figures reported in column (x) are in fact labeled "Schulbesuch" (school attendance) in the original document. But from the context of the operation of the schools, it is clear that they were enrollment numbers.

schools within districts reflected local economic development. The broad outlines of the situation with a view to these two arguments are summarized in Table 2, which also reports simple t-tests for mean differences.

As is clear from Panel A of the table, the majority German districts did have almost everything better and by a significant margin: more classrooms and teachers per 1000 school-age children, more public spending per child and better coverage by school infrastructure. Only the curriculum extent, measured by the average number of grades per school, was comparable between German and non-German districts. The German districts fell behind, however, in provision of education in minority languages, i.e. in building schools for the local Slav minorities, while German minorities in non-German districts had almost certain access to instruction in their mother tongue.[12] Making those same comparisons with respect to the presence of steam engines or railroad (Panels B and C) shows that more developed districts reported better coverage, slightly broader curriculum, greater public support and higher enrollment rates even though the educational infrastructure of teachers and classrooms was not as dense on the ground as in the less developed districts. The availability of minority schools also does not seem to be significantly different in districts with steam engines relative to those without.

The German element undoubtedly had the strongest political voice among all the nationalities, although in the Habsburg context it would be counterproductive to try to measure it using electoral statistics. As of 1865, the Empire had had experience with mere two nation-wide elections (1848 and 1861), the suffrage was highly constrained and too complicated to interpret easily, falling into four different electoral colleges of unequal importance; and in any case, it encompassed only a tiny fraction of the public. Yet Panel A of Table 2 suggests that the German political voice, however informal and unobservable directly, may have had measureable

[12] Note that these measures are calculated from a subsample of districts with more than 100 minority students, i.e. those where a minority school may be reasonably justified.

impact on the disbursement of public funds in matters of schooling and especially minority German schooling. Recall that the district authorities – the political elites in our case – were not in a strong position to prevent a school from being built by a determined community, but they could make it significantly easier and cheaper by providing a subsidy to a within-district minority who may not have been big enough to support a school on its own.

An important part of our empirical analysis is to see whether these plain differences in mean along ethnic and economic lines survive when we control for other local factors. The main lesson from our brief outline of the Austrian educational system is that – unlike in the Easterlin-Lindert story, where political voice is an ally of economic development – in our case the two forces are, if not set against each other, then certainly not pushing in the same direction. If the pro-German advantages do not survive controlling for economic variables, then apparently political voice mattered less than development. If economic variables turn out not to matter in the presence of the ethnic variables, then it suggests that the school was not so much an engine of growth as a cultural battlefield.

4. Data

Our data come from several sources. One is the school census of 1865. The descriptive statistics for relevant variables are reported in Table 1. We do not have any data on Hungary and the rest of the eastern half of the Empire (Transleithania). We also lack certain important education measures for Galicia, Bukowina and Dalmatia in the Cisleithanian part and so the estimation from this point on uses not the 730 school districts specified in Table 1 but only 546 in those provinces with full slate of school statistics. Average district encompassed an area of about 363 square km and was a home on average to 3.036 children aged 6-12.[13]

[13] Considering that these age cohorts usually represent about 13-14% of the total population, we can infer that the average district had about 22 – 24.000 inhabitants. Unfortunately, we cannot

We merged this schooling dataset with information from the steam engine census, published in 1863. It was an empire-wide survey, which also provided a retrospective on steam engines in 1852. In both years, we know precise location, sector of employment and horsepower of each engine. Column (xii) of Table 1 reports the average steam engines per district and its standard deviation. As of 1863, when the steam engine survey took place, about 270 of the 546 school districts still used no steam power at all.

Our final source is the annual yearbook of Austrian railroads from which we draw information about the railroad network. We use it to calculate each district's distance to the closest railroad. Means and standard deviations are reported in column (xiii) of Table 1.

5. Estimation

We separate our empirical analysis into three parts. Firstly, on the individual level, economic development, as measured by the spread of steam engines, may potentially affect one's demand independently of the provision of schooling. That is, whether parents choose to enroll their child in a school, given some existing publicly provided supply, depends on the expected returns to education. Secondly, on the district level, the provision of schooling may respond to economic development through two channels: by increasing the tax base which will make public financing of local schools, as stipulated by law, easier and presumably through greater demand for such provision. Finally, to assess the relative importance of political voice relative to the economy, we look for differences in the treatment of German minorities in non-German districts and non-German minorities in German districts along the ethnic boundaries within the Empire.

5.1. Individual demand for education

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be more precise because the school districts in 1865 did not overlap with census districts for either the 1857 census or the 1869 census.

We investigate how enrollment in a district responded to economic changes, conditional on the existing supply of schools. Ideally, one would like to know the expected returns to primary education for individual children and see how these varied with development. We do not have such detailed information and so we exploit the difference in post-primary school careers open to boys and to girls. For each district, we construct a dependent variable that measures enrollment of boys and girls separately. Since this variation took place within districts, we are able to include district-level fixed effects that will capture district-specific levels of law enforcement, supervision and other characteristics of the school supply. Since boys and girls face the same school supply and primary schooling was overwhelmingly coeducational in terms of extent and content, any differences between boys' and girls' enrollment will be due to differences in their expected returns to education: the post-primary-school prospects.

Girls did not continue with their education beyond primary school and all secondary schools were for boys only. We use the sum of all secondary schools' entering classes within 10km of a district to measure the prospects of secondary education for boys; for girls, this value is set to zero. Girls and boys also benefited differentially from economic modernization in their employment prospects after graduation. While industrialization generated new employment opportunities for men and women alike, some sectors were more feminized than others. For example, the 1869 report by the Prague Chamber of Commerce indicates that almost no women were employed in metalworking and machinery, while about half or more of the workforce in textiles and paper mills and tobacco factories was female. Our measure of economic development, number of steam engines in a given district, allows us to separate engines by sector of employment and so we construct a measure of steam engines relevant for each gender. Specifically, for girls, the steam engine totals do not include engines in metalworking, machinery,

mining, railroad stations, chemical industry and glass industry. In all of these sectors, female employment did not exceed twenty percent of the workforce.

School attendance was compulsory by law and even though its enforcement was far from perfect, the actual observed enrollments were presumably at least partially affected by the existing threats of fines and other penalties. However, in all the districts in our dataset, actual enrollments included children who were not under legal compulsion. First, there were children who lived in villages currently not covered by school provision (*nicht eingeschulte Ortschaften*) and it is clear from the data that in many places these did actually enroll in whichever school was closest.[14] Second, while compulsory education extended to age 12, all but three districts report positive enrollment among children over 12. For these two groups, the enrollment was voluntary. We therefore use, as our dependent variable Y_{is}, the number of enrolled pupils per 100 covered school-age children, for which the descriptive statistics are reported in column (xi) of Table 1. The presence of the two groups of voluntary enrollees explains why in many districts this measure would exceed 100.

Our specification is:

Eq. (1) $$Y_{is} = \beta_1 + \beta_2 EC_{is} + \beta_3 SE_{is} + \beta_4 SE_{is}^2 + \delta_i D_i + \varepsilon_i$$

where EC_{is} stands for secondary school entering classes within 10 km of the school district, SE_{is} is the number of steam engines and D_i is a vector of school district fixed effects. Subscript *i* indexes school districts, subscript *s* genders. Since steam engines may potentially be endogenous to enrollment, we instrument for them using steam engines located in a school district in 1852, i.e. 13 years before the enrollment data. It is unlikely that location of steam power was affected by the anticipation of school enrollment of children who were not even born in that year and if

[14] Coverage (*Einschulung*) meant that each child was assigned to a particular school. Teachers were required to turn away pupils who were assigned to a different school but the law is silent on children who were not assigned to any school.

steam engines of 1852 affect 1865 enrollment, it is likely going to be through subsequent economic development, as measured here by steam engines in 1863.

Table 3 presents the results of the two-stage estimation. First-stage F-statistics suggest that our instrument is sufficiently strong. For comparison, we also present a model with no fixed effects in column (i) and with diocese (but not district) fixed effects in column (ii). Our baseline specification is in column (iii). Overall, the coefficients on steam engines do not indicate a very large impact on enrollment. Using the standard deviation from column (xii) in Table 1, increasing the number of steam engines by 12 will increase enrollment by 1.1 percentage point. Even the secondary school prospects have a weak impact: a standard deviation increase of 78 extra slots in entry classes of local secondary schools would increase enrollment also by about 1.1 percentage point. To see how robust these coefficient results are, we re-estimated the same specification on various subsamples, as presented in columns (iv) – (ix). Perhaps the biggest worry, given our definition of the dependent variable, would be that districts with already full coverage would inevitably show considerably less variation in our enrollment measure because voluntary enrollment can only happen there along the age margin, not the coverage margin. The last two columns of Table 3 give some credence to this. The difference in the steam engine coefficients is a factor of eight. Still, even the large steam engine coefficients in column (ix) would imply an increase in enrollment of 4.1 percentage points as a result of 12 more steam engines in the district. The effects, in short, are relatively small in any of the subsamples. Moreover, the standard errors around the coefficients in most regressions suggest that these are relatively precisely estimated zeros. For example, a 95% confidence interval around the coefficient in column (iii) would cap it at 0.3.

5.2. Determinants of school supply

The individual demand for education therefore does not seem to have responded very strongly to economic development. Either the education offered in school was not a strong complement to modern industry, or schools – and the enforcement that went with it – were built "ahead of demand", thereby placing a more or less binding constraint on who enrolled. Since we do not have any direct measures of the strength of enforcement (and it would be subsumed under the district fixed effects) and we know that an established teacher in a school was the "first instance" of enforcement of compulsory attendance, this question ultimately speaks to the determinants of the supply of schools.

We choose four school characteristics to capture the extent and quality of school provision. These are all defined on the level of a school district. Our main measure of curriculum quality is the average number of grades per school, reported in column (iii) of Table 1. The extent and density of school infrastructure is measured by the number of (non-priest) teachers per 1000 school-age children and the number of classrooms per 1000 school-age children. We also include annual public expenditure per school-age child in a district, measured in gulden of Austrian currency (column (vii) of Table 1).[15]

This estimation of the public supply of educational infrastructure is our first attempt here to look at the intersection between economy and politics. In our specification, we put the ethnic make-up of a district in a horse race with the economic variables:

Eq. (2) $$Y_i = \beta_1 + \beta_2 SE_i + \beta_3 SE_i^2 + \beta_4 DRR_i + \beta_5 1_i(G) + \gamma X_i + \sum_d \delta_d D_i + \varepsilon_i$$

Here, in addition to steam engines and another economic variable, district *i*'s distance to railroad (in 100s of km), DRR_i, we include an indicator for a German majority, $1_i(G)$ in order to see

[15] Our public spending is equal to the payment that the communities paid to the teacher. In case the pay was insufficient, the community and the teacher could agree that the teacher levy an additional fee from attending pupils, called *Schulgeld*, reported in column (viii) of Table 1. We do not have information on spending on maintenance of infrastructure, but teacher salaries make up the bulk of public spending on education (Go & Lindert, 2010).

whether the economic variables can account for the differences in school provision along ethnic lines reported in Table 2. Finally, X_i are further exogenous variables and D_i are a set of 22 diocese fixed effects to capture any local effect specific to the operation of school supervision in a given bishopric (diocese).[16]

The change in dependent variable from enrollment to measures of the stock of school supply warrants, in our opinion, a change in the instrument for the potentially endogenous variables, SE_i and SE_i^2. Many of the schools appearing in the 1865 census were already built and functioning decades earlier, so already in 1852 the steam engines may have been built in response to local human capital, which would compromise the exogeneity of this potential instrument. We therefore instrument the steam engines with local urban population, defined as the number of people living in towns of 5.000 inhabitants or more.[17]

Table 4 shows the results. Most of the coefficients have the expected signs, with the exception of distance to railroad, where a consistently positive coefficient suggests that closeness to railroad led to worse outcomes in every aspect of school provision. But that effect is negligibly small.[18] The steam engines exhibit a diminishing impact but the quadratic term is mostly small and unimportant over the relevant range. Unlike in Table 3, the steam engine coefficients are not only statistically significant but also economically meaningful. An increase by one standard deviation in the number of steam engines brings about extra 1.9 teachers and 0.46 classrooms per 1000 school-age children, raises the school quality by 0.45 of a grade and adds 1.0 gulden of public spending per school-age child. This amounts to more than the standard deviation of per-child public spending. Apparently, if nothing else, economic development made the raising funds

[16] In a few cases, a province overlapped with a single diocese but most provinces had more than one bishopric.
[17] The town populations are taken from the 1857 census.
[18] Note that, in contrast to Table 1, for the regression purposes the distance to railroad was measured in 100s of km, so its standard deviation is 0.2.

for public schools easier. In columns (v) to (viii) of Table 4, we investigate whether the effects survive excluding Vienna from the estimation sample, given that the empire's capital was a clear outlier in all respects (see Table 1). With the exception of the effect for teacher availability, which weakens somewhat, all other effects survive even though their statistical significance declines, as could be expected.

The results for the German dummy variable suggest that economic variables are unable to account for differences in school provision between nationalities. The German effect is sizeable and in case of teacher and classroom provision, they actually trump the economic variables twice over. Only for grades per school is the German effect negligible. While these results are suggestive, we should not jump to the conclusion that what is at work here is nothing but German advantage in political influence. For example, many of the German districts in modern-day Austria lay in Alpine provinces where settlements were more scattered than in lowlands, which may have generated a school infrastructure with fewer school-age children per school and therefore more teachers and classrooms per school child. (We will try to rule out some of these effects and isolate the impact of German political voice in the next section.)

The results in Table 4, showing positive a significant response to economic development, may seem to contradict to our earlier conclusion, which showed little impact of steam engines on demand for education. Why are the developed communities building more schools if they at the same time do not see the returns to schooling improve with development? Is the industrialization merely making compliance with the schooling law easier? Or are the schools built sufficiently in lockstep with economic development, so that there is no "residual" demand that would show up in regressions in Table 3?

To investigate this question, we estimate, in Table 5, a system of simultaneous equations where we allow the local steam engines to depend on local stock of human capital, proxied by the supply of teachers.[19] The system is specified as follows:

Eq. (3a) $T_i = \alpha_1 + \beta_1 SE_i + \gamma_1 SE_i^2 + \delta_1 DRR_i + \theta_1 1_i(G) + \lambda_1 X_i + DFE + \varepsilon_{i1}$

Eq. (3b) $GR_i = \alpha_2 + \beta_2 SE_i + \gamma_2 SE_i^2 + \delta_2 DRR_i + \theta_2 1_i(G) + \lambda_2 X_i + DFE + \varepsilon_{i2}$

Eq. (3c) $P_i = \alpha_3 + \beta_3 SE_i + \gamma_3 SE_i^2 + \delta_3 DRR_i + \theta_3 1_i(G) + \lambda_3 X_i + DFE + \varepsilon_{i2}$

Eq. (3d) $SE_i = \alpha_4 + \delta_4 DRR_i + \zeta_4 T_i + \lambda_4 X_i + DFE + \varepsilon_{i3}$

Eq. (3e) $SE_i^2 = \alpha_5 + \delta_5 DRR_i^2 + \zeta_5 T_i^2 + \lambda_5 X_i + DFE + \varepsilon_{i4}$

where T_i is the number of teachers per 1000 school-age children, GR_i is the average grades per schools, P_i is the public spending per school-age child and is X_i a vector of exogenous variables which ensure that the order conditions of identification be satisfied. *DFE* stand for diocese fixed effects.

The results in Table 5 suggest that most results carry over well from Table 4. As before, excluding Vienna from the sample weakens the steam engine coefficients in the teacher equation but other school provision variables remain mostly unaffected.

One problem with the steam engines as dependent variables in equations (3d) and (3e) is that they are truncated: half of the districts reported zero steam power in use. To see whether and how that may affect the estimate, we provide a separate estimation for a subsample where only districts with positive number of steam engines are included. These are reported in columns (iii) and (iv). The answer is that the estimates for grades per school and public spending per

[19] We do not include classrooms per 1000 school-age children among the endogenous variables in this system of equations because it is highly correlated with the teacher measure. Also, Tablr 5 does not report coefficients for all the exogenous variables. Full estimation report is available from the authors upon request.

child are mostly unaffected while – perhaps, predictably – the estimates for teachers per school child and for number of steam engines are. The evidence suggests that the stock of human capital has some effect on the local number of steam engines, even if that effect is not stable across specifications. Including or excluding Vienna from the sample also makes considerable difference. Statistically, the reason why the effect of steam on teachers weakens while the effect of teachers on steam strengthens as one moves from column (i) column to (iii) or from column (ii) to column (iv) is that the districts with steam engines show lower variation in teacher supply and greater variation in steam engines (obviously) than the rest of the sample. The coefficient on teacher availability in column (iv) implies that if the number of teachers per 1000 school-age children increased from 10 to 11, it would bring an extra steam engine to the district. This is not an overwhelming but certainly not a negligible impact.

Overall, the evidence in Tables 4 and 5 indicates a strong interaction between the local economy and the local school supply. The extent of industrial development is positively and non-trivially correlated with investment in school quality as well as school quantity and given that the spread of steam power responds to the stock of human capital present, the communities are apparently not building the schools just to make good on the schooling law. At the same time, the differences between schools in German and non-German districts remains quite sizeable even after controlling for the economic environment.

5.3. Treatment of ethnic minorities: a spatial discontinuity regression

To isolate the impact of political voice as cleanly as possible, we go below the level of school districts and compare the treatment of German minorities in non-German districts against the treatment of non-German minorities in German districts. As mentioned before, district or higher authorities had scarcely any official means to prevent a school from being established; in fact, they were expected to enforce a law, which required full school coverage and full attendance.

They could, however, be more or less cooperative in providing a subsidy to small communities who expressed a desire to build a school but claimed to lack sufficient means to sustain it. Such could reasonably be the case of local ethnic minorities. If political voice mattered in this way, then we would expect – since public administration was thoroughly German – that German minorities would be more successful in securing such aid than non-German ones.

To explore this idea, we exploit the fact that the 1865 school census reported the language of instruction for each school in each district, together with the number of students of each mother tongue. Even when some minorities were too small to have their own schools, the record noted if a local school was bilingual, providing at least a parallel class in the minority language. To control for as much variation in other characteristics of these districts, we look for German and non-German districts straddling long-standing ethnic boundaries within the Empire. We are confident that these are exogenous to schooling provision, as they were result of mediaeval settlement patterns, and in the opinion of 19th century demographers they scarcely moved (Rauchberg, 1905; Ficker, 1864; Czoernig, 1855). The boundaries were also quite sharp, so much so that there were pairs of districts, no more than 15 miles apart, on either side of an ethnic boundary that reported no minority students at all. However, we were able to locate 34 pairs of German and non-German districts such that they both contained a linguistic minority that either had a school operating in its own language or was big enough that it should have had one (i.e. it numbered over 100 students of a given mother tongue). Of these 34 matches, 27 are German-Czech in Bohemia, Moravia and Silesia, 5 are German-Slovene in Styria, Carinthia and Carniola and 2 are German-Italian in South Tyrol.

The 34 matches consist of 68 districts, each appearing in exactly one match, and each district contributes two observations: one for the local majority schools and one for local minority schools.

Eq. (4)

$$Y_i = \beta_1 1_i(G) + \beta_2 1_i(Min) + \beta_3 1_i(GMin) + \beta_4 SE_i + \beta_5 SE_i^2 + \beta_6 1_i(Min) \times SE_i + \beta_7 DRR_i + \gamma 1_i(U) + \sum_j \delta_j D_{ji} + \varepsilon_i$$

Our regression specification includes dummy variables for German majority, $1_i(G)$, for minority status, $1_i(Min)$, for a German minority school, $1_i(GMin)$, as well as 34 pair fixed effects, D_j. Since the districts are immediate neighbors (average distance between them being 11 miles), any local specifics, such as the mountainous geography mentioned in the previous section, are likely to operate in both matched districts and will be captured by the match fixed effect. We also include number of steam engines in linear and quadratic form and an interaction between minority status and number of steam engines. Given this set-up there are several ways in which the importance of political voice can play out: (i) if $\beta_2 = \beta_3 = 0$, then we find no conclusive evidence of minorities of either kind receiving any systematically different treatment, so it will be difficult to argue that district authorities were playing favorites; (ii) if $\beta_2 \neq \beta_3 = 0$, then minorities of any kind are treated differently and the advantage of political voice rests with district majorities rather than with Germans; (iii) if $\beta_3 \neq 0$, then the German minority is clearly getting a different treatment and so political voice matters. The coefficient β_6 will capture the possibility that minorities may respond differently to economic development – perhaps alleviate their disadvantage thanks to economic growth.

The results are presented in Table 6. First, the instrument performs much worse in this subsample than in Table 4. Second, with 34 fixed effects and 8 other variables in a regression counting 136 observations, statistical significance is inevitably going to suffer. Yet in spite of that, some clear patterns emerge. Across the different specifications, we see β_1 either positive or a statistical zero, indicating that German-majority districts maintain their advantage, already seen

in Tables 4 and 5, in this specification also. Next, $\beta_2 < 0$ and is statistically significant in three out of the six regressions, while $\beta_3 > 0$ in five out of six regressions and statistically significant again in three of them. The bottom of Table 6 shows a series of t-tests for $\beta_2 + \beta_3 = 0$, to see whether the German minorities had enough clout to outweigh the otherwise negative effect of minority status. As it turned out, $\beta_2 + \beta_3 > 0$ unambiguously in several specifications, i.e. the German minorities were actually doing better in terms of the supply of teachers, classrooms and public funds per school-age child than the local majorities (although they were still worse, on average, than German schools in German districts, $\beta_2 + \beta_3 < \beta_1$). Overall, these results add up to a fairly consistent evidence of significant advantage for the Germans.

Considering the coefficients on the economic variables, the large negative values on the number of steam engines in columns (iii) and (iv) immediately jump out. While this result is consistent with the numbers presented in Table 2, it is the opposite of what we see in Tables 4 and 5. This is likely a statistical byproduct of moving the analysis onto a sub-district level. When a school is being established in a small community, which does not have many school-age children, a teacher and a classroom will represent fixed indivisible items because that are reported in whole units. In an analysis on the district level, such as in Table 4, this indivisibility is not so much of a problem because each district is likely to have some small communities and some large ones and in calculating the ratios of teachers or classrooms per 1000 school-age children, this indivisibility "averages out" in each district. However, moving the analysis to a sub-district level, especially with a view to minorities, such indivisibilities will become more prominent. It is telling that in columns (i) and (ii) in Table 6, the steam engine coefficients do not flip: average number of grades is calculated per school, not per school-age child, and public spending per school-age child consists mainly of teacher salary which somewhat depended on the number of children to be taught. To further illustrate that this is likely going on, we present

columns (v) and (vi) where the teacher and classrooms supplies are calculated per section.[20] This adjustment makes the signs positive, i.e. consistent with the results in Tables 4 and 5, indicating that, after controlling for various local characteristics, more developed districts will indeed have a better, denser teacher and classroom supply.

Having said this, steam engines positively affect both the quality of the average school (measured by number of grades taught) and public financing of schools, although the coefficients are only imprecisely estimated. At least in case of the grades regression (column (i)), economics can be said to be stronger than politics, in that the impact of one standard deviation increase in the local number of steam engines more than outweighs the negative effect of a minority status. In fact, minorities would be especial beneficiaries of economic development, since in column (i) $\beta_4 + \beta_6 > 0$ even at 5% level of significance. Same argument also applies to public spending per school-age child although the coefficients are even less precisely estimated than in column (i). Finally, note that the change in definition of dependent variables from per school-age child to per section in columns (iii) – (vi) eliminates the German advantage and strengthens the importance of economic variables. This suggests that the louder German political voice found its expression not so much along the intensive margin (investing more in existing schools, providing more specialized teaching staff, more special classrooms and more extensive curriculum) but along the extensive margin, that is, in that they were able to establish own schools for smaller groups of school-age children, relative to non-Germans.

6. Conclusions

The evidence reveals two important features of the Habsburg educational system. First, we find stronger support for the claim that economic development enabled a more extensive supply

[20] For example, large schools may have two or more parallel sections of the same grade. Small schools will have only one section in each grade and – importantly – a section of 40 children will be reported exactly the same as a section of 80 children: as one section.

of educational facilities, perhaps through broadening of the tax base, than for the notion that economic development generated a strong individual demand for public education, such as through raising returns to primary education. Only one of our results, in a simultaneous equations framework, suggested that economic development (as measured by steam engines) responds to the local stock of human capital but it was sensitive to the exclusion of outliers from the sample. Combined with what we know about the content and teaching methods at the time, it seems the Austrian schools were not providing education that would generate useful human capital for economic growth.

Second, political voice seems to have played a role. Accounts of political history show unequivocally that education, its extent, availability and language of instruction were highly politicized matters. We find evidence that this nationalist politics impacted educational choices made on the ground, sometimes acting much more strongly than the economic forces.

Overall, this adds up to a different picture to that painted regarding the modern rise of public education. While all the elements of the usual story – the industrialization, the public provision of schools, the political voice of important pressure groups – are present in the Austrian case, they combine in a way very different from how, for example, Go and Lindert (2007, 2010) have described the rise of American public schooling. Rather than education and human capital accumulation being among the drivers of economic growth, we see how economic development provides the resources for the Habsburg Empire's own version of "culture wars" whereby the school district elites – far from withholding public resources from education – actively subsidize that kind of schooling which corresponds to their ethnic preferences. For those who lacked political voice – in our case, the non-German nationalities – the road ahead did not pass first through enfranchisement to public education and eventually to economic development but exactly the other way: economic growth allowed them to catch up (at least in some respects) in

matters educational which – a generation later (and outside the scope of our paper) – led to their political self-assertion. And while this order of causation does not in anyway refute the more traditional account, at least as it applies to the United States, for example, it highlights that the interplay of education, politics and development can be much more varied.

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Table 1 - Descriptive statistics - means and [standard deviations]

		(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)	(x)	(xi)	(xii)	(xiii)
		Number of school districts	Number of school-age children	Average number of grades	Non-priest teachers per 1000 school-age children	Classrooms per 1000 school-age children	Classrooms per 1000 attending pupils (adjusted)	Public expenditure per school-age child	Schulgeld	Percent school-age children covered by school provision	Percent school-age children enrolled	Enrolled pupils per 100 covered school-age children	# steam engines in school district	Distance to railroad (km)
Alpine provinces	Vienna	1	37958	3.91	15.39	12.17	12.68	5.22	4.93	100.0	86.1	96.0	156.0	0.0
	Lower Austria	46	3235 [1338]	2.37 [0.39]	12.05 [1.69]	10.69 [1.41]	8.51 [2.1]	2.31 [0.57]	2.07 [0.37]	96.7 [3.56]	89.9 [3.4]	101.4 [4.85]	5.4 [8.35]	14.3 [19.92]
	Upper Austria	29	2583 [955]	2.17 [0.3]	11.47 [1.73]	10.88 [1.42]	8.54 [2.03]	2.09 [0.65]	1.67 [0.24]	96.6 [3.47]	94.6 [3.65]	102.9 [6.52]	0.9 [1.51]	13.7 [13.76]
	Salzburg	12	1167 [330]	2.17 [0.26]	16.78 [4.64]	14.41 [2.05]	14.36 [2.22]	2.53 [0.83]	1.72 [0.25]	94.1 [7.52]	92.4 [4.12]	106.0 [9.88]	0.3 [0.62]	30.0 [24.19]
	Styria	67	1583 [1020]	2.49 [0.43]	10.71 [4.06]	10.24 [3.92]	10.88 [5.07]	1.57 [0.89]	1.32 [0.48]	90.9 [11.75]	79.9 [14.39]	96.2 [15]	2.5 [7.31]	17.8 [18.9]
	Carinthia	25	1251 [407]	2.62 [0.49]	12.98 [4.23]	13.07 [3.71]	16.21 [5.24]	2.01 [0.93]	1.94 [0.77]	95.9 [3.4]	72.4 [13.13]	82.8 [16.38]	2.1 [6.58]	19.6 [20.19]
	Tyrol	67	1374 [701]	1.76 [0.5]	25.94 [6.78]	24.53 [6.61]	23.27 [6.31]	1.93 [0.87]	0.68 [0.53]	99.4 [2.7]	91.1 [5.35]	104.2 [5.81]	0.1 [0.4]	26.0 [23.66]
	Vorarlberg	6	2012 [676]	1.82 [0.52]	29.11 [7.3]	28.45 [7.1]	20.96 [2.19]	2.34 [0.58]	0.54 [0.75]	98.6 [2.84]	89.8 [6.82]	109.4 [8.32]	3.2 [4.58]	29.3 [18.16]
South-Slav provinces	Carniola	21	2048 [739]	2.06 [0.45]	8.05 [4.6]	7.30 [2.71]	9.72 [2.26]	1.91 [1.62]	1.35 [1.1]	89.3 [12.44]	58.7 [15.48]	73.3 [22.89]	1.0 [1.58]	14.5 [13.97]
	Austrian Littoral	43	1484 [1201]	2.38 [0.64]	6.24 [3.76]	7.97 [3.2]	16.48 [6.64]	1.75 [1.31]	0.77 [1.76]	80.1 [21.23]	41.7 [19.57]	55.7 [20.2]	1.0 [3.31]	31.8 [30.97]
	Dalmatia	34	853 [838]		20.06 [17.24]						37.2 [24.64]		0.1 [0.24]	310.1 [96.4]
Czech provinces	Bohemia	128	4984 [1892]	2.03 [0.36]	10.37 [2.37]	9.74 [2.06]	9.57 [2.27]	2.58 [0.59]	1.99 [0.34]	99.3 [1.76]	91.5 [3.57]	99.8 [3.84]	9.3 [17.48]	24.8 [24.23]
	Moravia	80	3150 [994]	2.16 [0.29]	11.06 [2.13]	9.97 [1.93]	9.47 [1.89]	2.39 [0.61]	1.80 [0.45]	99.0 [2.56]	92.3 [3.98]	100.2 [4.3]	7.1 [13.06]	15.7 [17.81]
	Silesia	21	2841 [1117]	2.11 [0.35]	11.05 [3.03]	10.21 [1.96]	9.46 [2.52]	2.08 [0.65]	1.19 [0.28]	99.6 [0.76]	91.5 [4.63]	98.6 [3.27]	12.0 [22.11]	13.9 [12.54]
Eastern provinces	Galicia	138	3990 [2689]		5.97 [3.49]						28.6 [14.68]		0.9 [3.34]	56.9 [52.59]
	Bukowina	12	5233 [3163]		2.84 [1.27]						11.6 [6.7]		0.4 [0.67]	240.0 [34.59]

All provinces	730	3036 [2514]	2.17 [0.48]	11.62 [7.82]	12.04 [6.2]	12.34 [6.24]	2.16 [0.9]	1.15 [0.99]	95.7 [9.69]	70.2 [29.53]	95.2 [16.92]	4.0 [12.01]	44.8 [75.44]

Table 2 - Comparison of education variables in various subsamples				
	Means		t-test	p-value
Panel A	German (N=274)	Non-German (N=270)		
Average grades per school	2.17	2.17	-0.04	0.96
Classrooms per 1000 school-age children	13.97	10.09	-7.69	0.00
Teachers per 1000 school-age children	15.06	10.34	-8.73	0.00
Public spending per child (in fl per year)	2.28	2.05	-3.09	0.00
School coverage	96.98	94.38	-3.16	0.00
*School with minority language of instruction present	70.40	87.10	2.46	0.02
*Parallel class with minority language of instruction present	85.20	95.30	2.09	0.04
Percentage of school-age children enrolled	90.10	78.04	-8.68	0.00
Number of steam engines	4.82	5.26	0.38	0.70
1(railroad access)	37.50	30.30	1.78	0.08
Distance to railroad	22.12	19.69	-1.28	0.20
Panel B	With steam engine (N=276)	W/o steam engine (N=270)		
Average grades per school	2.24	2.10	-3.33	0.00
Classrooms per 1000 school-age children	10.24	13.88	7.18	0.00
Teachers per 1000 school-age children	11.17	14.29	5.56	0.00
Public spending per child (in fl per year)	2.42	1.90	-6.93	0.00
School coverage	97.17	94.17	-3.65	0.00
*School with minority language of instruction present	78.80	85.00	0.83	0.41
*Parallel class with minority language of instruction present	90.90	92.50	0.30	0.76
Percentage of school-age children enrolled	87.18	80.95	-4.27	0.00
Distance to railroad	13.68	28.29	8.16	0.00
Panel C	With railroad access (N=185)	W/o railroad access (N=361)		
Average grades per school	2.32	2.10	-5.22	0.00
Classrooms per 1000 school-age children	10.59	12.79	3.97	0.00
Teachers per 1000 school-age children	11.56	13.30	2.88	0.00
Public spending per child (in fl per year)	2.30	2.09	-2.80	0.01
School coverage	96.66	95.19	-1.69	0.09
*School with minority language of instruction present	71.00	90.00	2.89	0.00
*Parallel class with minority language of instruction present	87.00	95.70	1.85	0.07
Percentage of school-age children enrolled	84.58	83.85	-0.46	0.64
Note: *based on 139 districts with at least 100 German students and 100 non-German students.				

Table 3 - Individual demand for school enrollment (IV-2SLS)									
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)	(ix)
Dependent variable: Enrolled pupils per 100 covered school-age children									
Sample:	Full sample	Full sample	Full sample	Districts with steam engines	Districts w/o steam engines (OLS)	German districts	Non-German districts	Full-coverage districts	Districts w incomplete coverage
Secondary school prospects	0.006	0.009	0.014	0.0103	0.084	0.001	0.034	0.006	0.04
	[0.011]	[0.007]	[0.005]	[0.003]	[0.036]	[0.003]	[0.010]	[0.004]	[0.011]
# steam engines	0.302	0.149	0.105	0.092		0.138	0.239	0.048	0.392
	[0.130]	[0.099]	[0.098]	[0.065]		[0.082]	[0.348]	[0.091]	[0.436]
Steam engines^2	-0.002	-0.001	-0.001	0.000		0.000	-0.002	0.000	-0.004
	[0.001]	[0.001]	[0.001]	[0.001]		[0.001]	[0.003]	[0.001]	[0.004]
Constant	94.013	96.233	85.011	86.573	92.586	88.554	93.94	89.3306	89.121
	[0.646]	[2.359]	[4.666]	[3.117]	[0.418]	[2.717]	[6.954]	[3.852]	[8.358]
N	1092	1092	1092	552	540	548	544	506	586
First-stage F	610.9	503.4	307.5	157.2		137.2	22.8	187.2	20.4
Fixed effects	None	Diocese	District	District	District	District	District	District	District
Note: Standard errors are in brackets.									

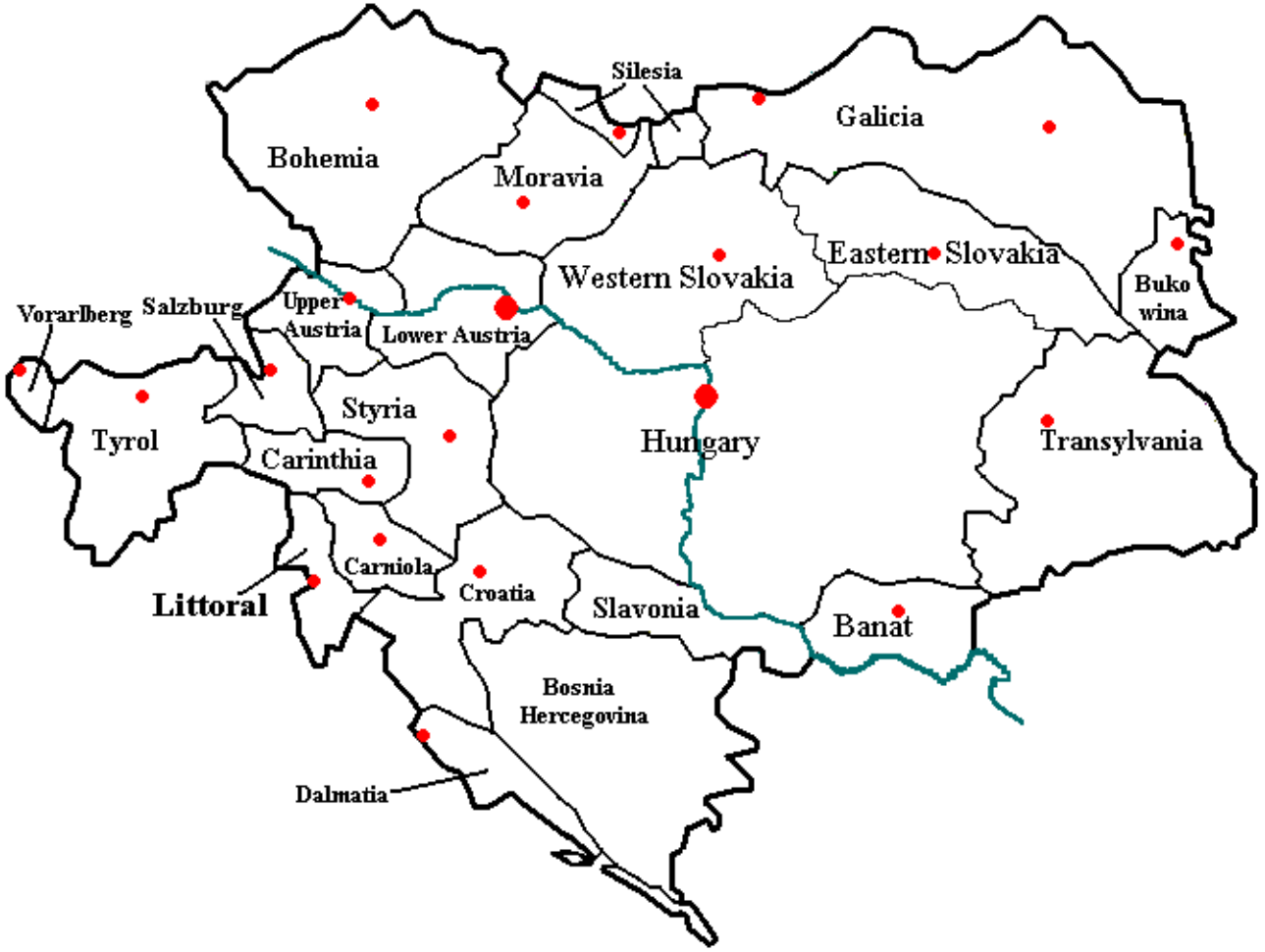
Table 4 - Determinants of the school supply (IV-2SLS)								
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
Dependent variable	Non-priest teachers per 1000 school-age children	Average number of grades	Public expenditure per school-age child	Classrooms per 1000 school-age children	Non-priest teachers per 1000 school-age children	Average number of grades	Public expenditure per school-age child	Classrooms per 1000 school-age children
# steam engines	0.173	0.038	0.088	0.041	0.121	0.061	0.095	0.031
	[0.080]	[0.009]	[0.020]	[0.069]	[0.102]	[0.013]	[0.026]	[0.089]
Steam engines2	-0.001	0.000	0.000	0.000	0.000	0.000	-0.001	0.000
	[0.001]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]	[0.001]
Distance to railroad	4.700	0.216	0.725	3.263	4.375	0.350	0.769	3.200
	[1.1]	[0.135]	[0.293]	[0.962]	[1.1]	[0.2]	[0.305]	[0.991]
1(German district)	2.657	0.054	0.307	2.369	2.688	0.039	0.303	2.375
	[0.439]	[0.054]	[0.115]	[0.381]	[0.436]	[0.060]	[0.118]	[0.384]
1(Urban district)	0.337	0.009	0.120	-0.123	0.437	-0.043	0.105	-0.104
	[0.481]	[0.060]	[0.127]	[0.417]	[0.474]	[0.068]	[0.128]	[0.418]
Constant	6.178	1.940	0.781	7.325	6.472	1.820	0.742	7.381
	[1.253]	[0.184]	[0.334]	[1.088]	[1.238]	[0.203]	[0.337]	[1.090]
First-stage F	32.3	32.4	33.1	32.3	25.8	28.0	27.6	25.8
N	546	546	546	546	545	545	545	545
Note: Distance to railroad is measured in 100s km. Columns (v)-(viii) estimate the same specifications on samples without Vienna. Standard errors are in brackets.								

Table 5: Simultaneous equations estimation				
	(i)	(ii)	(iii)	(iv)
Sample:	Full sample	Full sample minus Vienna	Districts with steam engines	Districts with steam engines minus Vienna
Eq (3a) Dependent variable: teachers per 1000 school-age children				
Number of steam engines	0.376	0.187	0.186	0.047
	0.062	0.084	0.055	0.075
Steam engines2	-0.002	-0.001	-0.001	0
	0.000	0.001	0.000	0.001
Distance to railroad (in 100s km)	6.165	4.883	3.719	2
	0.987	1.045	1.327	1.39
1(German district)	2.073	2.56	1.431	1.898
	0.41	0.423	0.417	0.431
1(Urban district)	-0.49	0.239	0.74	1.257
	0.453	0.459	0.441	0.44
Constant	4.452	5.913	6.08	7.549
	1.174	1.18	1.202	1.22
Eq (3b) Dependent variable: grades per school				
Number of steam engines	0.045	0.06	0.032	0.041
	0.007	0.01	0.008	0.011
Steam engines2	0	0	0	0
	0	0	0	0
Distance to railroad (in 100s km)	0.231	0.328	0.226	0.294
	0.119	0.129	0.197	0.213
1(German district)	0.023	0.02	0.128	0.127
	0.049	0.052	0.061	0.066
1(Urban district)	-0.001	-0.018	0.072	0.066
	0.055	0.06	0.066	0.072
Constant	1.84	1.776	1.692	1.647
	0.161	0.168	0.221	0.23
Eq (3c) Dependent variable: public spending per school-age child				
Number of steam engines	0.131	0.113	0.102	0.076
	0.015	0.019	0.016	0.022
Steam engines2	-0.001	-0.001	0	0
	0	0	0	0
Distance to railroad (in 100s km)	1	0.887	1.306	0.901
	0.252	0.264	0.414	0.436
1(German district)	0.185	0.253	0.132	0.249
	0.104	0.107	0.138	0.143
1(Urban district)	-0.026	0.087	0.057	0.192
	0.116	0.119	0.144	0.145
Constant	0.455	0.648	0.306	0.664
	0.303	0.308	0.396	0.404
Eq (3d) Dependent variable: number of steam engines				
Teachers per 1000 school-age children	0.88	0.169	2.411	1.002
	0.349	0.331	0.749	0.722
Urban population (in 000s)	0.256	0.52	0.242	0.514
	0.017	0.039	0.022	0.053
Distance to railroad (in 100s km)	-12.17	-10.754	-20.058	-17.134

	3			
	2.02	1.862	3.901	3.597
1(Urban district)	2.434	-0.259	0.645	-0.77
	1.195	1.175	2.228	2.124
Constant	6.112	12.915	-1.964	11.623
	4.288	4.053	8.366	7.997
Eq (3e) Dependent variable: number of steam engines squared				
Teachers per 1000 school-age children	108.2	9.211	252.977	62.868
	34.34			
	7	30.417	79.432	70.349
Urban population squared	0.09	0.394	0.087	0.403
	0.003	0.025	0.005	0.035
Distance to railroad squared	-1132.			
	173	-742.097	-2122.632	-1359.821
	285.2			
	98	238.472	715.288	595.162
1(Urban district)	290.1			
	86	220.238	139.201	256.04
	114.4			
	26	98.835	233.356	200.274
Constant	-205.2			
	16	752.03	-1230.713	620.161
	426.1			
	37	373.375	888.239	779.544
N	546	545	276	275
Note: Coefficients and standard errors are reported.				

Table 6 - Regression results for spatial discontinuity regression (IV-2SLS)						
	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Dependent variable	Grades per school	Public spending per school-age child	Classrooms per 1000 school-age children	Teachers per 1000 school-age children	Classrooms per section	Teachers per section
sub-sample mean	2.191	2.559	10.852	16.684	0.59	0.917
sub-sample s.d.	0.731	1.32	4.607	7.062	0.322	0.544
1(majority German district)	0.024	0.497	1.977	3.368	-0.116	-0.133
	[0.175]	[0.251]	[0.789]	[1.221]	[0.081]	[0.130]
1(minority school)	-0.202	-0.562	-1.377	-2.749	-0.022	-0.080
	[0.197]	[0.283]	[0.890]	[1.377]	[0.091]	[0.147]
1(minority German school)	0.108	1.335	3.185	5.427	-0.009	0.065
	[0.239]	[0.343]	[1.081]	[1.672]	[0.110]	[0.179]
# steam engines	0.099	0.026	-0.431	-0.497	0.045	0.076
	[0.054]	[0.077]	[0.243]	[0.376]	[0.025]	[0.040]
# steam engines squared	-0.001	0.000	0.005	0.006	0.000	-0.001
	[0.001]	[0.001]	[0.003]	[0.004]	[0.001]	[0.001]
1(minority school)*# steam engines	0.007	0.013	0.016	0.054	-0.002	0.000
	[0.012]	[0.017]	[0.053]	[0.083]	[0.005]	[0.009]
Distance to railroad	0.000	0.012	0.008	0.068	-0.002	-0.002
	[0.011]	[0.015]	[0.048]	[0.075]	[0.005]	[0.008]
Urban dummy	0.207	-0.158	-0.490	-1.369	0.008	-0.087
	[0.238]	[0.342]	[1.077]	[1.666]	[0.110]	[0.178]
Constant	0.925	1.604	11.569	15.958	0.063	0.014
	[0.658]	[0.944]	[2.972]	[4.599]	[0.303]	[0.491]
t-tests:						
minority+mgs=0	0.27	8.87	4.89	4.48	0.13	0.01
(p-val)	0.6045	0.029	0.027	0.0342	0.7145	0.9116
se63+minse=0	3.89	0.26	2.92	1.39	3.04	3.58
(p-val)	0.0486	0.609	0.0873	0.2382	0.0813	0.0586
First-stage F:	3.46	3.46	3.46	3.46	3.46	3.46
Note: Standard errors are in brackets.						

Figure 1 – Map of the Habsburg Empire in its 1914 borders



Province	Capital
Lower Austria	Vienna
Upper Austria	Linz
Salzburg	Salzburg
Tyrol	Innsbruck
Vorarlberg	Bregenz
Styria	Graz
Carinthia	Klagenfurt
Carniola	Ljubljana
Littoral	Trieste
Bohemia	Prague
Moravia	Brno
Silesia	Opava
Galicia	Lwow
Bukowina	Czernowitz
Dalmatia	Zadar
Hungary	Budapest
Transylvania	Cluj
Western Slovakia	Nitra
Eastern Slovakia	Košice
Croatia	Zagreb
Slavonia	Osijek
Banat	Timisoara
Bosnia-Hercegovina	Sarajevo

Note: Not all provinces were in existence at all times. Bosnia-Hercegovina was an Austrian protectorate between 1878 – 1908, after which it was annexed. Source: Wikimedia commons.