

Machielsen, Bas; van Besouw, Bram

Working Paper

Dynamics of Democratization and the Organization of Education

Suggested Citation: Machielsen, Bas; van Besouw, Bram (2026) : Dynamics of Democratization and the Organization of Education, ZBW - Leibniz Information Centre for Economics, Kiel, Hamburg

This Version is available at:

<https://hdl.handle.net/10419/342692>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

Dynamics of Democratization and the Organization of Education

Bas Machielsen

Bram van Besouw*

August 12, 2026

Abstract

We study the fiscal and educational consequences of franchise extension using the administratively arbitrary sorting of Dutch municipalities into nine voter-eligibility classes by the 1896 Electoral Law. Larger suffrage expansions contracted municipal expenditure and revenue and cut education spending, the largest budget heading. Public-school fees fell sharply while public pupils, schools, and class size did not change, so a smaller budget was spread over unchanged public provision and the saving passed to parents. Administrative records for 1905–1907 show that private pupil and teacher stocks expanded and the private share of teaching staff rose, while public stocks stood still inside a growing system. Neither a rise in the appeal of confessional schooling nor a fall in public teachers per pupil accounts for this. Teacher vacancy advertisements show private schools raising salaries while public councils increasingly rested on the statutory minimum. Democratization reorganized primary education rather than enlarging or shrinking it.

JEL Classifications: N43, H75, I22, D72

*Both Utrecht University School of Economics, Utrecht University, Kriekenpitplein 21–22, 3584 EC Utrecht, the Netherlands; e-mail: a.h.machielsen@uu.nl, b.vanbesouw@uu.nl

1 Introduction

Franchise extension changes public policy through the identities and interests of the citizens it admits. The canonical redistribution model predicts expansion when poorer voters enter the electorate (Meltzer and Richard, 1981), but that prediction need not hold when the marginal voters are ratepayers who bear the cost of local government (Acemoglu and Robinson, 2000). Cross-national panels of nineteenth- and early twentieth-century reforms have accordingly returned mixed and setting-dependent verdicts on whether democratization enlarged the state (Lindert, 1994; Persson and Tabellini, 2004; Aidt et al., 2006; Aidt and Jensen, 2013; Aidt et al., 2022). Nor can a budget response alone reveal what democratization does to public services. A smaller education budget may mean fewer schools and pupils, less intensive provision, lower user prices, or a reallocation toward providers outside the municipal sector. Distinguishing these margins requires evidence on prices, quantities, and providers rather than on spending alone, and it is necessary to understand the real consequences of political inclusion.

This paper studies these consequences around the Dutch Electoral Law of 1896. The law assigned municipalities to nine income classes that set thresholds for voter eligibility. The classification came from a tax statute, was not designed around political outcomes, and sorted otherwise similar municipalities into different threshold regimes. We use class rank as an instrument for the change in eligible municipal voters between 1896 and 1897, conditional on the scale, income, demographic, religious, and provincial characteristics that informed administrative classification. Using an administrative eligibility rule to isolate the effect of the franchise follows a body of work on suffrage restriction and extension in other settings (Naidu, 2012; Bernini et al., 2023; Larcinese, 2024). The resulting design isolates differential enfranchisement within one national institutional setting. It also isolates a shock that proved durable: the instrumented 1896–1897 extension is followed by no detectable endogenous growth in the local electorate through 1903, so later outcomes reflect the cohort the reform admitted rather than a moving electorate. That null bears on models of self-reinforcing enfranchisement (Jack and Lagunoff, 2006) and on evidence that democratic consolidation operates through gains realized over horizons longer than the ones observed here (Acemoglu et al., 2025).

The reform added taxpaying ratepayers rather than the poorest residents. Larger suffrage expansions subsequently reduced both municipal expenditure and municipal revenue per capita by similar proportions, so that municipalities operated at a smaller fiscal scale rather than moving into deficit. Education, the most important municipal service for human-capital formation, shared in that contraction. These results are the fiscal signature of ratepayer restraint rather than of redistributive expansion, in line with the retrench-

ment hypothesis advanced for nineteenth-century England and Wales ([Aidt et al., 2010](#)), but they do not by themselves establish whether municipalities reduced educational provision.

We answer that question with new municipal evidence at two horizons. The short-run evidence comes from the annual national report on the state of education, compiled from the returns of the school inspectorate and digitized here at the level of the individual municipality. For every municipality it records the number of schools, the number of pupils and classes, how many of those pupils attended, and the fees charged, separately for public and for privately organized schools. Public schools were a municipal responsibility and municipal councils set their fees, while confessional schools relied on national subsidy and private organization. In both 1899 and 1900, a larger suffrage expansion sharply lowered the public-school fee, while public pupils, schools, pupils per class, and attendance show no detectable reduction and the fee at privately financed schools did not move. Ratepayer enfranchisement therefore lowered the price of public schooling without measurably reducing its observed quantity, which means the smaller education budget was spread over the same number of public places.

The medium-run evidence comes from a second, newly digitized source that answers the question the accounts cannot: the education tables of the provincial reports for 1905 through 1907, which record public and private schools, pupils, and regular teachers for every municipality in six provinces. Where the reform induced a larger suffrage expansion, private pupil and teacher stocks expanded and the private share of the teaching staff rose by roughly three percentage points, while public schools, public pupils, and public teachers per thousand residents did not move. The shift was not public retrenchment: total enrollment and the total teaching establishment both grew, and the private sector absorbed that growth while the public sector stood still. Nor was it a wave of entry into places that had no private school before the reform, since municipalities without one in 1896 were no more likely to acquire one and the total number of establishments did not detectably change. Democratization thus reorganized primary education rather than enlarging or shrinking it, producing a relative decline of public provision without an absolute contraction.¹

A simple model of public and private provision, developed in [Appendix C](#), narrows the interpretation of this pattern by elimination. Capacity is not the margin, since public schools and pupils do not move at either horizon. Neither is the incidence of the cost

¹Advertisements and notices in the confessional and public education periodicals for 1903 through 1909 point the same way, with confessional teacher vacancies and school openings rising while public vacancy activity does not. Because they read a school's allegiance where the administrative tables cannot, they supply the confessional identification the provincial source lacks; because they are selected flows of advertised demand rather than a census, they support rather than carry the result. [Appendix E.4](#) reports them together with an audit of the corpus.

between parents and ratepayers: the public fee fell by roughly the same proportion as spending per public place, so the council passed the saving through rather than shifting the burden. A fall in the price of those resources cannot account for it either: a place obtained more cheaply at unchanged content gives no pupil a reason to leave it, so a cheaper public school cannot by itself produce the private expansion that followed. That leaves a rise in the relative appeal of confessional schooling, which we test directly with a third new source, the annual Protestant “A School with the Bible” collection, digitized here as a municipality-by-season panel running from 1890 through 1917. Where the reform induced larger suffrage expansions, local fundraising presence and receipts show no differential increase after 1897.² The private expansion is therefore not a consequence of a shift in preferences over religious education.

The explanation that survives concerns the real resources standing behind each public place, and the administrative records identify the form the reduction took. Councils did not economize by thinning the teaching establishment: regular public teachers per hundred public pupils do not fall, and the estimate is precise enough to exclude a substantial decline. What a council adjusted instead were the terms on which each place was staffed and equipped, meaning what a teacher was paid and at what grade a council was willing to appoint, together with the materials, the upkeep and heating of the buildings, and the ancillary staff. One of those terms is directly observed, and it moves. Where the reform admitted more ratepayers, private schools raised what they offered regular teachers and public councils did not follow, compressing the entry grade of the public salary schedule and resting more often on the statutory minimum, and they did so without any deterioration in how readily public posts were filled. Ratepayer enfranchisement therefore left the size of the public establishment intact and economized on its content. Because families weigh what a school delivers against what it costs them, a public place that is cheaper to attend but thinner in what it provides leaves some of them better served by a confessional school financed from national salary subsidy and private contribution rather than from the municipal budget.

These findings contribute to four related literatures. First, they show that the fiscal effect of democratization depends on who receives the vote (Acemoglu and Robinson, 2000; Aidt and Mooney, 2014; Chapman, 2018, 2024). The Dutch ratepayer reform produced restraint, in contrast to the increase in poor relief after the English reform studied by Chapman (2026), and the contrast adds a within-country case to the growing evidence that democratization outcomes vary with institutional setting (Abou-Chadi and Orlowski, 2015; Marcucci et al., 2023; Cassan et al., 2025). Second, they connect democratization

²Appendix Tables A.29, A.30, and A.31 report the mechanism and heterogeneity estimates from the collection panel and the same test restricted to Protestant-majority municipalities, where the null also holds.

to the real content of local public finance by following a fiscal shock into prices and quantities rather than treating spending as the final outcome, at a granularity that aggregate national panels cannot provide. Third, they add the organization of provision to the political economy of human capital: the political margin here operated neither on the number of schools nor on the teachers standing in front of pupils, but on which sector accommodated a growing school population, a response that counts of enrollment alone cannot reveal. Fourth, the setting shows how mixed public-private finance conditions political responses to suffrage. The same electorate could reduce the municipal footprint and public fees while organized religious groups expanded outside the municipal sector.

The paper proceeds as follows. Section 2 describes the franchise laws and the public-confessional school system. Section 3 introduces the data and research design. Section 4 establishes the fiscal contraction and its education component. Section 5 studies the short-run price and quantity of public schooling, the medium-run organization of provision, and the mechanism that survives. Section 6 concludes.

2 Historical Background

2.1 Politics of Franchise Extension

The Dutch franchise was a central political conflict of the late nineteenth century. Liberals supported a substantial but limited expansion, confessional parties sought a broader male electorate that could end their political disadvantage, and socialist and labor organizations demanded universal suffrage (Lijphart, 1968). The conflict was unusually protracted, and it was resolved in stages whose timing owed as much to fiscal law as to electoral principle.

The constitutional revision of 1848 introduced direct election of the Lower House, the provincial states, and the municipal councils, and it made the vote a function of tax assessment. Under the Electoral Law of 1850 a man qualified by paying at least a stated amount in direct taxes, and the statute fixed that amount separately for each municipality, from twenty guilders in the least expensive places to one hundred sixty guilders in Amsterdam. Roughly one adult man in nine could vote at mid-century. Because the qualifying amounts were nominal and written into statute, the size of the electorate afterwards drifted with prices, incomes, and changes in tax law rather than following from any deliberate decision, and every attempt to reset it deliberately failed: bills to lower the tax qualification were introduced in 1870, 1872, 1874, 1877, and 1882, and the proposal of 1874 was lost by a single vote. The constitutional revision of 1887 abandoned the tax census as the constitutional criterion, replacing it with marks of capability and social standing whose content

ordinary legislation would define.³

The first broad use of that delegation produced the deepest political crisis of the period. In 1892 Tak van Poortvliet, as minister of the interior, proposed that every adult man who could read and write and did not live on public relief should have the vote, which would have enfranchised something like three quarters of adult men. In March 1894 the chamber carried an amendment requiring a voter to occupy a whole dwelling rather than a single room. Tak declared the amended bill unacceptable, withdrew it, and obtained a dissolution. The election that followed cut across party lines, with liberals standing against liberals and antirevolutionaries against antirevolutionaries; the opponents of the bill won a clear majority, the cabinet fell, and both the Liberal Union and the Antirevolutionary Party split. The 1896 Electoral Law, or *Kieswet*, drafted by Van Houten, was the compromise that closed the crisis. It admitted men aged at least 25 who could demonstrate taxation, income, rent, savings, or education, roughly doubling the electorate to about half of men of qualifying age, yet stopping short of universal male suffrage. That final step came only in 1917, in the constitutional settlement that simultaneously ended the school conflict by placing confessional and public schools on an equal financial footing.⁴

What made the franchise so resistant to change was that it was welded to taxation. Eligibility was defined by tax assessment, so the electorate could not be redrawn without either redefining the tax base or severing the vote from taxation altogether, and the connection ran in both directions. A change in taxation moved the electorate mechanically, as when the abolition of the patent tax in 1893 removed one of the assessments on which eligibility had rested. A change in the franchise, conversely, threatened the principle that political voice belonged to those who paid for government. On that logic, which inverted the revolutionary slogan into “no representation without taxation,” admitting men who contributed little was not simply a transfer of political power but an attack on the justification of the fiscal system itself. The obstacle was therefore that the suffrage question could not be separated from a tax system in which they had a direct stake, and once the two were detached, wider electorates were followed here, as elsewhere in Europe, by growth in public budgets (Lindert, 2004; Machielsen, 2026).

The composition of the new electorate is central to the expected fiscal response. The marginal voter in 1896 was not an unpropertied recipient of redistribution. He was a modest taxpayer or renter who paid local taxes and, often, school fees. Broadening political participation among this group could therefore strengthen demands for lower municipal

³Appendix Table A.2 sets out this sequence, giving for each reform the statute that governed it, the voting age, the principal eligibility rule, and the qualification that most restricted its reach.

⁴The men who legislated these reforms changed with them: measuring the personal wealth of Dutch politicians from probate inventories, Machielsen (2026) finds that the wealth of successive parliaments fell over the transition from oligarchy to democracy.

costs even as it increased access to local services. This logic differs from extensions that enfranchise poorer voters with little local tax exposure, for which redistribution models predict greater spending (Meltzer and Richard, 1981; Acemoglu and Robinson, 2000).

2.2 Legal Basis of the Reforms

The *Kieswet* created several paths to voter eligibility for male residents aged at least 25 (*Kieswet*, 1896). Article 1a admitted men assessed in qualifying national direct taxes. Article 1b admitted otherwise ineligible men who met municipality-specific income or weekly-rent thresholds. The variation in those thresholds followed a nine-class table inherited from the Personal Tax Law enacted five months earlier (*Wet tot regeling der Personele belasting*, 1896). Class 1 contained the highest-cost municipalities and imposed the highest thresholds; Class 9 contained the lowest-cost municipalities and imposed the lowest. A higher numerical class therefore admitted more men from a given local income distribution.

The Personal Tax Law classified municipalities to calibrate exemptions and deductions for observable signs of prosperity, including rental value, fireplaces, furniture, servants, horses, and bicycles. The electoral law reused that administrative classification rather than constructing a political one. It thus converted a coarse tax schedule into differential electoral eligibility. National assignment across more than one thousand municipalities was necessarily imperfect, and contemporaries complained that economically dissimilar places shared a class while similar places fell on opposite sides of class boundaries. Conditional on the observable scale and wealth measures that guided classification, this administrative coarseness is the source of the empirical variation used below.

Class rank also altered the point at which a household became liable for the national Personal Tax. This creates a compound statutory treatment, but not a direct municipal-budget channel. The tax accrued to the national government, and the associated household liability was small. Appendix B argues that the additional tax liability arising from this is minimal, and develops the legal details and the historical sequence.

2.3 Public and Confessional Schooling

Dutch primary education was divided between *openbare* public schools and *bijzondere* private schools, most of which were Protestant or Catholic. Their unequal public financing lay at the center of the *schoolstrijd*, the conflict over school funding that organized Dutch politics from the middle of the nineteenth century until the constitutional settlement of 1917 (Kossmann, 1978; Lijphart, 1968). Municipalities built, operated, and financed the public schools, subject to national regulation and to partial national support for teacher salaries,

and it was the municipal council rather than the national government that set the fees paid by public-school parents (van Gijlswijk, 2016; Boekholt and de Booy, 1987). Confessional schools were founded and governed by private religious associations and received essentially no municipal funding.

Two statutes fixed the terms that prevailed in 1896. The education law of 1878 raised national requirements for buildings, staffing, and teacher salaries but financed compliance only in the public sector. It thereby worsened the competitive position of confessional providers and provoked the mass petition campaign out of which organized confessional politics grew (Kossmann, 1978; Boekholt and de Booy, 1987). The education law of 1889 answered that grievance in two ways that matter here. It granted confessional schools a national contribution toward teacher salaries, which covered roughly one third of their costs and left the remainder to fees, donations, and denominational organization. It also obliged municipalities to charge a school fee at their public schools, ending the free public tuition that some councils had adopted as a competitive weapon against confessional schools, while exempting the poor (Boekholt and de Booy, 1987; van Gijlswijk, 2016). The public-school fee was therefore a live municipal instrument in the period we study: a price set by the same council that voted the education budget, subject only to a statutory obligation that some fee be levied. Equal public financing of the two sectors arrived only with the settlement of 1917 and the legislation that followed it, and has structured Dutch school finance since (Ritzen et al., 1997).

This financing asymmetry rested on dense Protestant and Catholic organization. Local associations founded schools, recruited teachers, and raised the funds that the national salary contribution did not cover, while denominational periodicals and national unions bound these local efforts into what later became one of the pillars of Dutch social life (Lijphart, 1968; Wintle, 2000). The consequence for a municipal fiscal shock is that the two sectors were exposed to it very differently. A council could change what it spent on a public place and what it charged for one, but it had no comparable lever over confessional schools, whose resources came from outside its budget. Religious provision elsewhere has been shown to move when public provision does (Gruber and Hungerman, 2007), and historical differences in confessional resistance to state schooling still explain how large the privately operated sector is today (West and Woessmann, 2010; Squicciarini, 2020).

Before turning to the evidence, it is worth separating the margins on which a municipality could respond, because a fall in education spending is consistent with several quite different things happening in the classroom. Five matter. The first is how many public places the council maintains (capacity K), which is what counts of schools and pupils measure. The second is the resources actually devoted to each place: the teaching staff per class, the materials, and the upkeep of the building, none of which a count of schools or

pupils registers. Call that g . The third is the price the municipality pays for those resources, that is, for teachers, for materials, and for building work (the cost shifter ϕ). Part of that price a council did command, since it wrote the salary schedule its own teachers were paid on. What matters for the argument is not whether a council could lower that price, but that buying the same place more cheaply leaves what the school delivers unchanged, while devoting fewer resources to it does not. Only the second can move pupils toward the private sector. The fourth is how much of the resulting cost the council recovers from parents rather than from ratepayers (the fee share η). The fifth is how attractive confessional schooling is relative to public schooling in the eyes of local families (relative preference θ). Spending falls if the council maintains fewer places, if it devotes fewer resources to the places it keeps, or if the same place happens to have become cheaper to provide. The fee falls if the place becomes cheaper to provide or if the council simply charges parents less of what it costs.

Two comparisons then do the analytical work. Because the counts of public schools and pupils turn out not to move, the contraction cannot have run through the number of places, so lower spending must reflect either a public place that receives fewer resources or one that costs less to provide. Spending alone cannot separate those two, since both reach the budget through the same channel. The fee, however, can be compared with spending: a fee that fell by more than the cost of providing a place would imply that the council had also shifted the burden of that cost away from parents.⁵

What discriminates between a public place that receives fewer resources and one that costs less to provide is the movement of the private sector, and the intuition is straightforward. Families weigh what each kind of school delivers against what it costs them (Epple and Romano, 1998; Cohen-Zada and Justman, 2003). If the same classroom, with the same staff, the same materials, and the same upkeep, is obtained at a lower price, no family is worse served than before, and if the fee falls the public option is cheaper as well, so nothing gives a family a reason to leave it. Thinning what a public place contains does give families such a reason, and because the two kinds of schooling are substitutes, private enrollment and with it private teacher demand rise even when nothing has changed in how families feel about religious education. Growth in the private sector is therefore evidence of a real reduction in public inputs, unless the relative appeal of confessional schooling rose at the same time for reasons of its own. That alternative is not a residual to be waved away, and Section 5.3 tests it directly against annual Protestant fundraising. The argument

⁵The comparison is made in proportional terms. The change in the public-school fee is set against the change in municipal primary-education spending per capita which, because the number of public pupils per capita does not detectably move, is also the change in spending per public place. If the two proportional changes coincide, the share of the cost charged to parents is unchanged; only a fee falling by more than the cost of a place indicates a shift of that burden from parents to ratepayers.

identifies a reduction in g collectively rather than naming which input fell, and it is silent about ϕ : a council may also have bought what it bought more cheaply, but no amount of that alone would move a pupil to a confessional school. The evidence below narrows g from two sides. The number of regular teachers standing before public pupils does not change, so what was thinned was not the count of the staff. What a council offered a newly appointed teacher does change, and that is the one component of a public place we observe being reduced directly. Appendix C states the argument formally and derives the propositions.

3 Data and Empirical Strategy

3.1 Data

The analysis joins several bodies of municipal evidence, all linked at the level of the historical municipality: the statutory class table that supplies the instrument, the annual provincial reports that supply the franchise, the municipal accounts and the medium-run education outcomes, predetermined population and tax records, the national education returns that supply the short-run schooling outcomes, and two corpora of the digitized press. Appendix A documents the sources, the record linkage, and sample construction in detail.

The instrument is the nine-class schedule printed in the 1896 law, transcribed here for every municipality. Class rank was fixed by statute before any outcome studied below was observed, and it is the same schedule that governed national Personal Tax liability, as Section 2 describes.

The provincial reports. One printed series supplies the treatment, the fiscal outcomes, and the medium-run education outcomes, which makes it the backbone of the analysis. Each Dutch province published an annual report whose statistical appendices ran to several hundred pages of tables, with municipalities as the rows. We read those tables from the page images of the originals rather than from any prepared electronic version.⁶ Three of the appendices matter here.

The first lists the number of men eligible to vote in municipal elections. The counts for 1896 and 1897 define the franchise expansion that is the endogenous variable throughout, expressed as the increase in eligible voters divided by the 1896 population. The count for

⁶Appendix Figures A.1 and A.2 reproduce one such opening, the realized expenditure accounts of Groningen for 1903, and enlarge the block of columns from which municipal primary-education spending is taken.

1903 measures whether the local electorate continued to grow after the reform, which the design requires it not to have done differentially.

The second reproduces the municipal balance sheets for 1903 through 1907. It reports revenue and expenditure not as single totals but under the many numbered headings of the municipal accounts, grouped under printed titles that name the branch of activity: government and administration, tax collection, police and fire service, public works, municipal trading enterprises, poor relief, debt service, and education, arts and science. We reconstruct each total from its component headings, treating a total as missing only when every component is missing, and we form annual per-capita values before averaging across years. Because the accounts survive in disaggregated form, the same source follows the fiscal response into individual headings, including the column for primary education, which is the schooling municipalities directly operated and paid for.

The third is the education table, and it supplies the paper’s principal medium-run outcomes. For 1905 through 1907, six provinces printed municipality-level counts of day schools, pupils, and regular teachers, separately for the *openbare* public and the *bijzondere* private sector. Three properties of the source shape its use. It is not national, so the medium-run estimates rest on those six provinces. It divides schools only into public and private, so its private figures include confessional provision but do not name a confession, and we accordingly write private rather than confessional when reporting them. One of the six provinces does not print teacher counts in a comparable form, so the teacher outcomes rest on the other five. Ratios and population rates are formed within each report year and then averaged over the years available, and a structurally absent or incomparable cell is treated as missing rather than as a zero. Where the national returns supply a comparable measure for 1896, the pupil and school shares condition on it; there is no comparable pre-reform teacher count, so teacher outcomes are post-period levels. The inclusion of province-years, the field mappings, and the primary outcome were all fixed before any municipal value in these tables was read.

Predetermined characteristics. These come from the Historical Database of Dutch Municipalities, which assembles municipal population statistics and the returns of the 1889 census (Boonstra, 2020): population and its growth, land area, tax receipts recorded before the reform, and the demographic and religious composition of the population. These are the observable measures of scale and wealth that informed administrative classification, and they constitute the control set used throughout.

The national education returns. The annual national report on the state of higher, secondary, and primary schools in the Kingdom of the Netherlands, compiled from the school

inspectorate’s returns, supplies the short-run schooling outcomes. We use the municipal observations for 1900 in the main text ([Ministerie van Binnenlandse Zaken, 1901](#)), the last year for which the source provides the required local detail, with 1899 as an adjacent replication. The returns separate public from private schools and report schools, pupils, classes, attendance, and mean fees. Public pupils per capita, public schools per capita, pupils per class, attendance, and the public-school fee are the direct measures of public provision and its price.⁷ Private fees are a useful placebo because municipalities did not set them. Returns from 1894 and 1895 supply predetermined outcome controls, and the 1896 volume supplies the comparable baseline for the medium-run composition shares. Two limits follow from the source itself. It measures observed stocks, enrollment, and prices, not instructional quality or every costly input, and it groups Protestant, Catholic, and other private schools under the single historical *bijzonder* category.

The digitized press. Two further corpora, digitized for this paper, observe what the official statistics never recorded. The first is the annual report of the Protestant Unie collection, “A School with the Bible”, printed in *De Standaard* and read here for every season from 1890 through 1917. Its printed lists name each local collection committee and the sum it remitted, which yields a municipality-by-season measure of whether local organizers were active and of how much they raised. This source carries the paper’s test of whether the reform raised the relative appeal of confessional schooling itself. It observes mobilization annually from seven years before the reform to twenty years after, so pre-reform levels and trends can be examined before any post-reform comparison is drawn.⁸ The second corpus consists of education periodicals, whose advertisements and notices yield dated signals of teacher vacancies, appointments, and school openings during 1903 through 1909, classified by the affiliation of the school. These are selected flows of advertised demand rather than administrative stocks, and the titles differ in orientation, reach, and propensity to print a vacancy notice, so they enter the paper as supporting evidence only.⁹

The sources differ in the dates they cover and in the unit they record, and neither difference is incidental. The accounts, the national returns, and the provincial education tables report stocks and prices: an account balance for a year, a count of schools, pupils, or teachers, a fee charged per pupil. The press corpora report dated flows: a post advertised, a school opened, a collection remitted in a given season. A stock registers a change

⁷Appendix Table [A.1](#) reports the distribution of each of these measures across municipalities in 1900, separately for the public and the private sector.

⁸These are measures of Protestant mobilization, not a census of private schools and not evidence on Catholic activity. Appendix [E.5](#) gives the construction, the quality-control limits, and the results.

⁹Appendix [E.4](#) reports their construction, coverage, estimates, and the source-selection audit.

only once it has accumulated, while a flow registers it as it happens, so the two speak to the same response at different horizons. Table 1 lists every variable used in the main text with its source and the years for which it is available, grouped by the role it plays: the instrument, the franchise measures, the predetermined controls, the fiscal outcomes, the short-run schooling outcomes, the medium-run administrative outcomes, and the mobilization measures.

[Table 1 here]

3.2 Empirical Strategy

For municipality i in province j , define the treatment as the increase in eligible municipal voters between 1896 and 1897 divided by the 1896 population. We estimate

$$\begin{aligned}\Delta S_{ij} &= \alpha_j + \pi C_i + \Gamma X_i + \nu_{ij}, \\ Y_{ij} &= \alpha_j + \beta \widehat{\Delta S}_{ij} + \Lambda X_i + \epsilon_{ij},\end{aligned}$$

where $C_i \in \{1, \dots, 9\}$ is class rank, X_i contains predetermined controls, and α_j are province fixed effects. Fiscal specifications control for log population, pre-reform tax revenue per capita, log surface area, demographic and religious principal components, and population growth. Education specifications use log population and density with the same income, demographic, religious, and provincial conditioning. Unless stated otherwise, inference is heteroskedasticity robust. We report effects for a one-percentage-point larger franchise expansion, so coefficients on logged outcomes are approximate percentage changes.

The exclusion restriction is conditional. Class assignment was built from municipal scale and cost-of-living proxies, so unconditional comparisons are not credible. The identifying assumption is that, after conditioning on those determinants and on province, the residual administrative placement of municipalities across classes affects later outcomes only through enfranchisement. Balance tests are reported in Appendix E.1. Two features of the setting limit the most direct alternative channels. The classification originated in a national tax law and was reused for electoral eligibility rather than calibrated to local political outcomes, and its associated tax liability was small and accrued nationally rather than to municipal budgets.

The later outcomes are cross-sectional responses to a single persistent reform shock. Appendix D shows that the 1896–1897 extension was not followed by detectable endogenous local suffrage growth through 1903.¹⁰ We consequently interpret schooling in 1900,

¹⁰Appendix Table A.3 reports those estimates. The same appendix sets out why ordinary least squares is mechanically biased toward reversion in this comparison, since the 1897 count enters the earlier change positively and the later one negatively.

the administrative education outcomes for 1905 through 1907, and the periodical flows for 1903 through 1909 as responses to the initial reform, not to a sequence of local franchise changes.

4 Fiscal Consequences

4.1 First Stage

Table 2 establishes that class rank shifted the electorate as the statute intended. A one-step increase in class rank raises the 1896–1897 change in eligible voters per capita by 0.013 to 0.019, depending on controls. Eligible voters per capita averaged 0.149 in 1896, so a single step of the statutory classification is worth between roughly nine and thirteen percent of the pre-reform electorate, and between roughly twelve and seventeen percent of the average expansion the reform produced between 1896 and 1897. The relationship remains stable after conditioning on population, pre-reform tax receipts, surface area, demographic and religious composition, population growth, and province fixed effects. The table reports the F-statistic, which clears the conventional threshold in every column; its level varies with the control set and with the sample an outcome is available on, so the appendix specifications report it alongside each estimate.

Two dispersions govern how the coefficients below should be read, and they are not the same quantity. The expansion municipalities actually experienced has a standard deviation of 6.4 percentage points and an interquartile range of 6.1, around a mean of 11.0. The expansion the class schedule *induces*, once scale, composition and province are held fixed, is far tighter: a standard deviation of about 0.7 to 0.8 percentage points and an interquartile range of about 0.85 to 0.95, depending on which control set a specification uses. It is the second that identifies the instrumental-variable estimates, because the first stage is conditional by construction and the unconditional spread is dominated by the very characteristics the design holds fixed. The one-percentage-point convention used throughout is therefore close to an interquartile move in the variation the design exploits, and coefficients should be read at that scale rather than against the full spread of realized expansions.

Because the class schedule was designed around municipal scale and wealth, this is not an unconditional comparison. The appendix shows that the predetermined covariates that predict administrative rank are absorbed by the preferred controls.¹¹

¹¹Appendix Table A.4 reports those balance regressions. Population, surface area, and pre-reform tax receipts predict class rank strongly without controls, as administrative assignment implies, while the remaining predetermined characteristics have limited joint predictive power once scale and province are held fixed.

[Table 2 here]

4.2 Municipal Finances

The first fiscal result is a contraction of the municipal footprint. Table 3 presents expenditure and revenue together. In the preferred specification, a one-percentage-point larger suffrage expansion reduces expenditure per capita by approximately 11.2 percent and revenue per capita by approximately 11.5 percent. The near symmetry is economically informative. Municipalities did not finance unchanged activity through deficits, nor did they cut expenditure while maintaining their revenue base. They operated at a smaller fiscal scale.

[Table 3 here]

This response is consistent with enfranchisement of ratepayers who bore local fiscal costs (Aidt and Mooney, 2014; Chapman, 2018, 2024). It contrasts with the expansion of poor relief after an English reform that admitted a poorer electorate (Chapman, 2026). The comparison underscores that democratization has no invariant fiscal sign. Its effect depends on the economic position of the marginal voter and the incidence of the services and taxes under local control.

Detailed accounts in Appendix E.2 show that the contraction was not confined to one bookkeeping category. Several expenditure and revenue components decline, and the budget-gap estimate does not indicate a movement toward deficit. Those decompositions involve category-specific missingness and occasionally weaker first stages, so the aggregate symmetry is the more reliable description.¹²

4.3 Educational Expenditure

Education was part of the fiscal contraction, and it was the largest single heading in the municipal accounts, absorbing on average about a third of what a municipality spent. Table A.5 reports the average share of municipal expenditure and of municipal revenue accounted for by each accounting category over 1903–1907; it also shows that school fees returned only about two percent of municipal revenue, so parents met directly a small fraction of a bill that dominated the budget. Panel A of Table 4 estimates that a one-percentage-point larger suffrage expansion lowered municipal education expenditure per capita by approximately 8.1 percent. Panel B separates a small set of substantively distinct

¹²Appendix Tables A.6 and A.7 report the two full decompositions, and Appendix Table A.8 reports the absolute gap between recorded revenue and expenditure.

ledger headings. The precisely estimated reduction is concentrated in primary education, which municipalities directly operated and which accounted for the relevant mass of local schooling. Estimates for other institutions and arts and science are imprecise. Secondary and higher education are recorded for a much smaller set of municipalities and have a weak component-specific first stage, so that coefficient is not used for inference.

[Table 4 here]

The education result makes the budget contraction substantive, but expenditure alone cannot distinguish fewer school places from places that cost less to provide or places that receive fewer real resources. The next section turns to direct observations of what municipalities charged for schooling, of how much public and private schooling they contained, and of who taught in it.

5 Consequences for Education

This section follows the fiscal response into the price of schooling, into the quantity and organization of the schooling municipalities contained, and into the mechanism that connects the two. Section 2.3 set out the five margins on which a council could respond: the number of public places it maintained, the real resources devoted to each of those places, the price it paid for those resources, the share of the resulting cost it recovered from parents, and the relative appeal of privately organized schooling. Counts of public schools and pupils measure the first. Lower expenditure at unchanged capacity means either that each public place absorbed fewer real resources or that those resources had become cheaper to buy. The public fee falls for either of those reasons, or because the council recovered a smaller share of the cost from parents. Private expansion can follow from weaker public inputs or from a rise in the relative appeal of privately organized schooling, but not from cheaper provision alone. The estimates below do not identify these margins separately. They discipline which combinations of them can rationalize the evidence, and the three subsections take the margins in turn: the short-run price and quantity response, the medium-run organization of provision, and the mechanism that survives.

5.1 Price and Quantity of Public Schooling

Table 5 reports the national education returns for 1900, the last year for which that source gives the required municipal detail. It measures the public sector municipalities directly operated, at a date three years after the reform and before the fiscal accounts used above begin. Class rank is a strong instrument for the franchise expansion in every column.

[Table 5 here]

The clearest short-run result is the price of public schooling. A one-percentage-point larger franchise expansion lowers the public-school fee by approximately 8.6 percent. The fee at private schools, which municipalities did not set, is small and imprecisely estimated, which is the placebo the design calls for. The 1899 estimate for the public fee is similar in direction and magnitude, at about 7.4 percent, and the 1900 estimate remains precise under both heteroskedasticity-robust and spatial inference.¹³ Both post-reform years therefore support the fee result.¹⁴

The public quantity measures do not show a detectable short-run reduction. Estimates for public pupils, public schools, pupils per class, and attendance are individually imprecise and close enough to zero that the data do not reject preservation of observed provision. These nulls do not establish exact zero effects, and they do not measure the resources devoted to each place. They do rule against describing the fiscal contraction as an observed collapse of public-school stocks or enrollment. Nor is the result an artifact of municipalities dropping out of the sample when they close their last public school, since public provision was very nearly universal in 1900.¹⁵ The private share of pupils rises by about two percentage points, which is consistent with composition already beginning to shift toward the privately organized sector within three years of the reform.

The short run therefore delivers a cheaper public school of unchanged observed size. What it cannot show is where the money went, or whether the composition shift it hints at continued. For that the analysis turns to a later and richer administrative source.

5.2 The Organization of Primary Education

Table 6 reports the education tables of the provincial reports for 1905 through 1907. Three features of the source govern how the estimates should be read. It covers six provinces rather than the whole country. It divides schools into the printed *openbare* and *bijzondere* categories, so the private figures include confessional provision but do not identify a confession, and this subsection accordingly speaks of private rather than confessional schooling. Teacher counts are unavailable for one province, whose reports do not print

¹³Appendix Table A.10 reports the 1899 replication in full, and Appendix Table A.13 sets heteroskedasticity-robust against spatial standard errors for the outcomes that discriminate between mechanisms.

¹⁴Controlling for the pre-reform level of the public fee leaves the 1900 estimate similar in direction but smaller and only marginally significant, so the fee result weakens, but does not disappear, once the shorter pre-reform record is absorbed. Appendix Table A.11 reports it, and Appendix Table A.12 applies the same control to 1899.

¹⁵Appendix A reports the sample restrictions and the count: of the municipalities in the estimation sample observed in the 1900 returns, slightly more than one percent record no public school at all.

them in a comparable form, so the teacher outcomes rest on the other five. Ratios and population rates are formed within each report year and then averaged across the years available. Where the earlier national returns supply a comparable 1896 measure, the pupil and school shares condition on it; no comparable pre-reform teacher count exists, so the teacher outcomes are post-period levels.

[Table 6 here]

Panel A reports what each sector did, and it is the panel that forecloses reading anything that follows as public retrenchment in disguise. Private teachers and private pupils per thousand residents both rise, and both estimates are precise. Public teachers, public pupils, and public schools per thousand residents are all close to zero and none is statistically distinguishable from it.

Panel B translates those sectoral movements into relative composition. A one-percentage-point larger franchise expansion raises the private share of teaching staff by approximately 3.3 percentage points. The private-minus-public gaps widen by about twenty pupils and six tenths of a teacher per thousand residents, and the school gap moves the same way with less precision. The pupil and school shares are positive once conditioned on their 1896 level and smaller than the corresponding post-period estimates reported in Appendix Table A.9, so part of the cross-sectional difference reflects composition that already existed before the reform. Read alongside Panel A, the composition moved because the private sector grew, not because the public sector shrank.

Panel C places that growth inside an expanding system. Total primary enrollment rises by about fifteen pupils per thousand residents and the total teaching staff by about four tenths of a teacher per thousand, while the total number of schools does not detectably change. Larger franchise expansions were therefore followed by more pupils and more teachers without a detectable increase in the number of establishments, with the private sector absorbing the growth. Panel D asks where within the private sector the increase appears. Among municipalities with no private school in 1896, the probability that one had appeared by 1905 does not rise, so the expansion is not a wave of entry into places that previously had no private provision at all. Conditional on private provision existing, pupils per school, teachers per school, and teachers per pupil are all indistinguishable from zero, but these are the least well powered estimates in the table, and the private school count is positive without being precisely estimated. The data are therefore consistent with the additional private pupils being absorbed either by larger schools or by more of them.

Panel E turns to the staffing ratios, which speak most directly to the resources behind a public place. Regular public teachers per hundred public pupils, declared as primary before any municipal value was extracted, does not move, and the interval around it is

narrow enough to rule out any substantial decline. The system-wide staffing ratio and the private-public difference in it are likewise indistinguishable from zero. Public teachers per school do fall, and precisely so, by about three tenths of a teacher against a mean of a little over four, or roughly seven percent. Read together with the unchanged number of teachers per public pupil, that estimate locates the reduction in the size of the average public establishment rather than in the staffing behind a public place, since the same arithmetic implies a public school held proportionally fewer pupils as well as fewer teachers. Municipalities that enfranchised more ratepayers ran a public sector of smaller units at an undiminished staffing intensity, which is the public-side counterpart of the private growth reported above.¹⁶ Whatever the education budget bought less of, it was not regular teachers standing in front of public classes.

Two further bodies of evidence point in the same direction and are reported in the appendix rather than here. Advertisements and notices in the education periodicals for 1903 through 1909 show confessional teacher vacancies and school openings rising with the suffrage shock while public vacancy activity does not increase, and they can read a school's allegiance where the administrative tables cannot. They are selected flows of advertised demand rather than a census, and the titles differ in orientation, reach, and propensity to print a vacancy notice, so they corroborate the administrative stocks rather than establishing anything on their own.¹⁷

5.3 Interpretation: Which Margin Moved

Section 2.3 set out five margins on which a council could respond, and the model in Appendix C shows that only one of them can produce the pattern in the data. Capacity K , the fee share η , and the relative appeal of confessional schooling θ are each ruled out by direct evidence. A pure cost saving, a fall in ϕ , is not ruled out, but it cannot by itself move a pupil into a confessional school. What remains is g , the real resources standing behind each public place.

Capacity is not the margin. Public schools and public pupils move neither in the 1900 returns nor in the administrative records for 1905 through 1907, so councils did not respond by maintaining fewer places. With capacity fixed, lower education spending is lower spending per place.

The incidence of the cost between parents and ratepayers is not the margin either.

¹⁶Appendix E.3 varies the baseline and the post-period coverage rule in Table A.9 and reports the alternative inference. The private teacher share is stable when all three report years are required.

¹⁷Appendix E.4 reports those estimates in Table A.19, together with the source-selection audit: alternative frames, allegiance bounds, leave-one-source-out and leave-one-province-out diagnostics, and the municipality-by-source-year construction.

Municipal spending on primary education, the heading councils directly operated, falls by roughly eight percent per capita, and because the number of public places is unchanged, so does spending per public place. The public fee falls by roughly the same proportion. Had the council shifted the burden away from parents, the fee would have fallen by more than the cost of a place did. It did not. The saving was passed through to parents rather than reallocated between them and ratepayers.

The relative appeal of confessional schooling is not the margin. The annual Protestant collection supplies a direct test of it. Committee presence records whether local organizers participated in the annual “A School with the Bible” collection, and receipts record the funds those committees remitted, over 1,120 municipalities and 28 collection seasons from 1890 through 1917. Nationally, committee coverage rises from 35.5 percent before 1897 to 45.3 percent afterward, but common national growth is not the hypothesis: a reform-induced rise in θ requires a larger increase where the instrument induced a larger suffrage expansion. There is none. Conditional on the standard predetermined controls, province effects, and 1896 Protestant-school strength, post-minus-pre committee coverage, seat presence, and receipts per 1,000 inhabitants are all small and none is statistically distinguishable from zero, and the coverage estimate carries the opposite sign to the one the preference interpretation requires. Differential linear trends across the pre-reform seasons are not detectable, and the nulls survive restricting the comparison to seasons in which at least half of the numbered lists were observed and to the municipalities in which Protestants were a majority before the reform, where the collection has the most power to detect a mobilization response. The estimates bound any post-reform increase in committee coverage below about two percentage points and any increase in seat presence below about three and a half.¹⁸ We therefore treat θ as unmoved by the reform. The collection is a Protestant instrument, so Catholic mobilization is not directly tested.

A pure cost saving is neither excluded nor sufficient. In the model the confessional response depends on θ and on g , and not at all on ϕ : a place bought more cheaply delivers what it delivered before, so it gives no pupil a reason to leave it. Table 8 gives no sign that the more democratic municipalities obtained schooling by a cheaper technology either. Public advertising is if anything slightly shorter and less often repeated, but the private side of the same market advertised longer, matched less often, and turned over more, so the reorganized system did more searching rather than less. Those search costs fell outside the municipal accounts, so they neither confirm nor refute a saving in what a council paid for what it bought. The point that carries the argument is the model’s rather than the

¹⁸Appendix Table A.29 reports pre-period levels, linear pre-trends, post-minus-pre estimates, the high-coverage restriction, and census balance. Appendix Table A.30 reports heterogeneity by 1896 Protestant-school presence; the within-group post estimates are not significantly positive and only the differences between groups are marginally significant. Appendix Table A.31 reports the Protestant-majority restriction.

table's: whatever ϕ did, it cannot account for the confessional expansion.

What remains is g . Spending per public place falls, capacity is fixed, the share charged to parents is unchanged, and relative appeal does not move, so the resources devoted to each place must have fallen. The administrative panel shows that the reduction did not take the form of fewer regular public teachers per pupil, an outcome declared primary before the data were read and estimated precisely enough to exclude a substantial decline. Teacher offers supply the first positive measurement of a component of g . Table 7 compares what schools offered regular teachers between 1903 and 1909, using the closed specialist-periodical corpus. Where the reform admitted more ratepayers, private schools offered more and public councils did not follow; where it admitted fewer, public and private offers stayed in line. The public estimate is negative and imprecise, so this is a divergence between the sectors rather than an absolute public pay cut, and the matched-municipality and stacked comparisons both show it. Table 8 locates the public side of that divergence at the entry margin of the salary schedule: in the more democratic municipalities the ordinary-teacher floor is lower and more councils rest on the statutory minimum, while the floor for school heads does not detectably move. If public and private schools drew on the same local pool of teachers, a council offering less while the private offer rose was not buying the same teacher more cheaply, and what it saved at the entry grade is a reduction in what a public place contained rather than a lower price for an unchanged one.

[Table 7 here]

[Table 8 here]

The divergence was not accompanied by recruitment failure. Public repeated listings and advertising duration do not rise, the vacancy-to-appointment match rate does not fall, the composition of advertised public posts does not shift toward cheaper categories, and public churn does not increase. The adjustment appears in the salary schedule rather than in the difficulty of filling a post, which is entry-wage compression rather than a shortage of teachers willing to serve.

Three limits bound the conclusion. Salary levels in the 1908 survey are observed only where a municipality prints a schedule, and the treatment itself shifts that reporting margin. The estimate for the top of the schedule has a first stage too weak to distinguish an unchanged maximum from a meaningful movement. Offered salaries are not accepted wages, and neither source observes applicant quality, teacher skill, materials, building upkeep and heating, or ancillary staff. The paper measures one component of the reduction in g and rules out another; it does not measure the whole bundle.

The administrative panel supports a second and complementary reading that does not depend on the elimination. The public sector stood still while the system around it grew.

Total pupils and total teachers rise with the suffrage shock, public pupils and public teachers do not, and the private sector absorbs the difference. Municipalities that enfranchised more ratepayers did not dismantle their public schools; they declined to enlarge them as enrollment grew, and privately organized providers, financed from national salary subsidy and local contribution rather than from the municipal budget, took up the growth. Relative decline without absolute contraction is what a cheaper, thinner public place produces when families weigh what a school delivers against what it costs them (Epple and Romano, 1998; Cohen-Zada and Justman, 2003).

These results speak to a literature that has largely read the political economy of education off counts of schools and pupils. The closest parallel runs in the opposite direction. Naidu (2012) finds that disenfranchisement in the post-bellum U.S. South reduced the teacher-child ratio in black schools by ten to twenty-three percent, with no comparable effect on white schools. There the political margin operated on the staff standing in front of each pupil. Here it did not, and the response appears instead in the public-private organization of provision, which a count of enrollment does not reveal either. The sign of our estimates nonetheless cuts against the cross-sectional association between broader voting rights and more public schooling that has been documented for the nineteenth century (Go and Lindert, 2010; Gallego, 2010). The two are not in conflict, because the direction depends on who is admitted: the marginal Dutch voter of 1896 was a ratepayer and a fee-paying parent rather than a poorer non-taxpayer with little local tax exposure. Consistent with a relationship that has no fixed sign, Paglayan (2021) shows that mass primary schooling frequently expanded under non-democratic regimes. Whether schooling is financed centrally or locally conditions the response further (Cinnirella and Schueler, 2018); in the Netherlands the public school was a municipal charge, so ratepayer restraint fell on it directly while private schools drew on national subsidy and private contribution.

Taken together, the evidence supports three claims and refuses a fourth. Ratepayer enfranchisement lowered municipal education spending and the price of a public school place without reducing the number of places. It shifted the composition of primary education toward privately organized provision, and it did so within an expanding system rather than by moving pupils out of public schools. The mechanism was not a change in the relative appeal of confessional schooling, and it was not a reduction in the number of regular teachers standing before public pupils. It was a thinner public place, of which the entry pay a council offered a newly appointed teacher is the one component these sources measure directly.

6 Conclusion

The 1896 Dutch franchise reform enlarged municipal electorates among taxpaying ratepayers and contracted the local fiscal footprint. Expenditure and revenue fell by nearly equal proportions, and municipal education spending, the largest single heading in the accounts, shared in the decline. Public-school fees fell sharply in 1899 and 1900, while public pupils, schools, class size, and attendance show no detectable reduction. The immediate consequence was cheaper public schooling at broadly preserved observed capacity, not an absolute collapse of public provision.

The medium-run consequence was a reorganization of who provided schooling. Administrative records for 1905 through 1907 show private pupil and teacher stocks expanding and the private share of the teaching staff rising, while public stocks remained stable. Total pupils and total teachers increased while the number of schools did not detectably change, and municipalities without a private school before the reform were no more likely to acquire one, so a growing school population was accommodated predominantly on the private side of an existing school network. This was a relative decline of public provision within an expanding system rather than a one-for-one displacement of public pupils.

The findings qualify general claims about democratization and government growth. [Chapman \(2026\)](#) finds that admitting a poorer electorate increased English poor relief. Here, admitting ratepayers reduced the scale of municipal finance. The contrast is not a contradiction. It shows that the fiscal consequences of political inclusion depend on who enters, who pays, and which government controls the service. Related evidence on ratepayer reforms reaches the same institutional conclusion ([Aidt and Mooney, 2014](#); [Chapman, 2018, 2024](#)).

The paper's distinctive contribution is to trace a franchise shock beyond budgets into the price, organization, and public-private composition of education, and to say as precisely as the sources allow what did and did not change. Ratepayers reduced public spending and their own school fees without a detectable loss of public quantities, and privately organized providers absorbed the subsequent growth. Two rival explanations are eliminated rather than assumed away. The Protestant collection panel shows that where the reform induced larger suffrage expansions, committee presence and receipts did not increase differentially after 1897, including in the municipalities where Protestants were in the majority, so a reform-induced rise in the appeal of confessional schooling is not the explanation. The administrative records show that regular public teachers per pupil did not fall, with an interval narrow enough to exclude a substantial decline, so austerity did not work by emptying the public classroom of its teachers. What remains is what each teacher was paid and at what grade a council was willing to appoint, the materials, the upkeep

of the buildings, and the ancillary staff. The first of these is measured directly: private teacher offers rose with the suffrage expansion while public councils did not follow, and the public adjustment fell on the entry grade of the salary schedule rather than on the ease of filling a post. Because the public fee fell by about the proportion that spending per place did, the saving was passed to parents rather than shifted between parents and ratepayers, and a public school that was cheaper to attend but thinner in what it delivered left some families better served by a school financed from outside the municipal budget.

References

- Abou-Chadi, Tarik and Matthias Orlowski**, “Political institutions and the distributional consequences of suffrage extension,” *Political Studies*, 2015, 63 (S1), 1–22.
- Acemoglu, Daron and James A. Robinson**, “Why did the West extend the franchise? Democracy, inequality, and growth in historical perspective,” *Quarterly Journal of Economics*, 2000, 115 (4), 1167–1199.
- , **Nicolás Ajzenman, Cevat Giray Aksoy, Martin Fiszbein, and Carlos Molina**, “(Successful) democracies breed their own support,” *Review of Economic Studies*, 2025, 92 (2), 621–655.
- Aidt, Toke S. and Graeme Mooney**, “Voting suffrage and the political budget cycle: Evidence from the London Metropolitan Boroughs 1902–1937,” *Journal of Public Economics*, 2014, 112, 53–71.
- **and Peter S. Jensen**, “Democratization and the size of government: Evidence from the long 19th century,” *Public Choice*, 2013, 157 (3–4), 511–542.
- , **Jayasri Dutta, and Elena Loukoianova**, “Democracy comes to Europe: Franchise extension and fiscal outcomes 1830–1938,” *European Economic Review*, 2006, 50 (2), 249–283.
- , **Martin Daunton, and Jayasri Dutta**, “The retrenchment hypothesis and the extension of the franchise in England and Wales,” *Economic Journal*, 2010, 120 (547), 990–1020.
- , **Stanley L. Winer, and Peng Zhang**, “Franchise extension and fiscal structure in the UK 1820–1913: A new test of the redistribution hypothesis,” *Cliometrica*, 2022, 16 (3), 547–574.
- Bernini, Andrea, Giovanni Facchini, and Cecilia Testa**, “Race, representation, and local governments in the US South: The effect of the Voting Rights Act,” *Journal of Political Economy*, 2023, 131 (4), 994–1056.
- Boekholt, P. Th. F. M. and E. P. de Booy**, *Geschiedenis van de school in Nederland vanaf de middeleeuwen tot aan de huidige tijd*, Assen and Maastricht: Van Gorcum, 1987.
- Boonstra, Onno**, “Historische Database Nederlandse Gemeenten (Historical Database of Dutch Municipalities),” Dataset, International Institute of Social History, Amsterdam, hdl:10622/RPBVK4 2020.

- Cassan, Guilhem, Lakshmi Iyer, and Rabia A. Mirza**, “Enfranchisement, political participation, and political competition: Evidence from colonial and independent India,” *Journal of Economic History*, 2025, 85 (1), 33–71.
- Chapman, Jonathan**, “Democratic reform and opposition to government expenditure: Evidence from nineteenth-century Britain,” *Quarterly Journal of Political Science*, 2018, 13 (4), 363–404.
- , “Gradual franchise extensions and government spending in nineteenth-century England,” *Journal of Politics*, 2024, 86 (1), 369–374.
- , “Democracy, Redistribution, and Inequality: Evidence from the English Poor Law,” *Journal of the European Economic Association*, 2026.
- Cinnirella, Francesco and Ruth Schueler**, “Nation Building: The Role of Central Spending in Education,” *Explorations in Economic History*, 2018, 67, 18–39.
- Cohen-Zada, Danny and Moshe Justman**, “The political economy of school choice: linking theory and evidence,” *Journal of Urban Economics*, 2003, 54 (2), 277–308.
- Epple, Dennis and Richard E. Romano**, “Competition between private and public schools, vouchers, and peer-group effects,” *American Economic Review*, 1998, 88 (1), 33–62.
- Gallego, Francisco A.**, “Historical Origins of Schooling: The Role of Democracy and Political Decentralization,” *Review of Economics and Statistics*, 2010, 92 (2), 228–243.
- Go, Sun and Peter Lindert**, “The Uneven Rise of American Public Schools to 1850,” *Journal of Economic History*, 2010, 70 (1), 1–26.
- Gruber, Jonathan and Daniel M. Hungerman**, “Faith-based charity and crowd-out during the great depression,” *Journal of Public Economics*, 2007, 91 (5–6), 1043–1069.
- Jack, William and Roger Lagunoff**, “Dynamic enfranchisement,” *Journal of Public Economics*, 2006, 90 (4–5), 551–572.
- Kieswet*
- Kieswet*, *Staatsblad* No. 154 1896. *The Hague*.
- Kossmann, E. H.**, *The Low Countries 1780–1940*, Oxford: Clarendon Press, 1978.
- Larcinese, Valentino**, “Enfranchisement and representation: Evidence from the introduction of quasi-universal suffrage in Italy,” *Journal of Politics*, 2024, 86 (2), 565–581.

- Lijphart, Arend**, *The Politics of Accommodation: Pluralism and Democracy in the Netherlands*, University of California Press, 1968.
- Lindert, Peter H.**, “The rise of social spending, 1880–1930,” *Explorations in Economic History*, 1994, 31 (1), 1–37.
- , *Growing Public: Social Spending and Economic Growth since the Eighteenth Century*, Cambridge University Press, 2004.
- Machielsen, Bas**, “Democratization, Personal Wealth of Politicians and Voting Behavior,” *Explorations in Economic History*, 2026, 99.
- Marcucci, Annalisa, Dominic Rohner, and Alessandro Saia**, “Ballot or bullet: The impact of the UK’s Representation of the People Act on peace and prosperity,” *Economic Journal*, 2023, 133 (652), 1510–1536.
- Meltzer, Allan H. and Scott F. Richard**, “A rational theory of the size of government,” *Journal of Political Economy*, 1981, 89 (5), 914–927.
- Ministerie van Binnenlandse Zaken**, “Verslag van den staat der hooge-, middelbare en lagere scholen in het Koninkrijk der Nederlanden over 1899–1900,” Algemeene Landsdrukkerij, ’s-Gravenhage 1901.
- Naidu, Suresh**, “Suffrage, schooling, and sorting in the post-bellum U.S. South,” NBER Working Paper 18129, National Bureau of Economic Research 2012.
- Paglayan, Agustina S.**, “The Non-Democratic Roots of Mass Education: Evidence from 200 Years,” *American Political Science Review*, 2021, 115 (1), 179–198.
- Persson, Torsten and Guido Tabellini**, “Constitutional rules and fiscal policy outcomes,” *American Economic Review*, 2004, 94 (1), 25–45.
- Ritzen, Jozef M. M., Jan van Dommelen, and Frans J. de Vijlder**, “School finance and school choice in the Netherlands,” *Economics of Education Review*, 1997, 16 (3), 329–335.
- Squicciarini, Mara P.**, “Devotion and Development: Religiosity, Education, and Economic Progress in Nineteenth-Century France,” *American Economic Review*, 2020, 110 (11), 3454–3491.
- van Gijlswijk, Dick**, “Early central regulation, slow financial participation: relations between primary education and the Dutch state from ± 1750–1920,” *Paedagogica Historica*, 2016, 52 (4), 364–379.

West, Martin R. and Ludger Woessmann, “‘Every Catholic Child in a Catholic School’: Historical Resistance to State Schooling, Contemporary Private Competition and Student Achievement across Countries,” *Economic Journal*, 2010, 120 (546), F229–F255.

Wet tot regeling der Personele belasting

Wet tot regeling der Personele belasting, Staatsblad No. 72 1896. The Hague.

Wintle, Michael, *An Economic and Social History of the Netherlands, 1800–1920: Demographic, Economic and Social Transition*, Cambridge: Cambridge University Press, 2000.

Main-Text Tables and Figures

Table 1: Variables Used in the Main Text: Sources and Availability

Variable	Source	Availability
<i>Panel A. Instrument</i>		
Statutory class rank, 1 to 9	1896 Electoral Law, class schedule (<i>Kieswet, 1896</i>)	1896
<i>Panel B. Franchise Measures and the Endogenous Variable</i>		
Eligible municipal voters per capita	Provincial reports, statistical appendices	1896, 1897, 1903
Change in eligible voters per capita	Provincial reports, statistical appendices	1896–1897
<i>Panel C. Predetermined Controls</i>		
Population	Historical Database of Dutch Municipalities (<i>Boonstra, 2020</i>)	1896
Population growth	Historical Database of Dutch Municipalities	1880–1889
Population per square kilometer	Historical Database; 1896 boundary map	1896
Municipal land area, logged	1896 municipal boundary map	1896
Personal Tax receipts per capita	Historical Database of Dutch Municipalities	1889
Religious composition	Historical Database; census of 1889	1889
Demographic composition	Historical Database of Dutch Municipalities	1885–1896
<i>Panel D. Fiscal Outcomes</i>		
Municipal expenditure per capita	Provincial reports, municipal accounts	1903–1907
Municipal revenue per capita	Provincial reports, municipal accounts	1903–1907
Education expenditure per capita	Provincial reports, municipal accounts	1903–1907
Absolute budget gap per capita	Provincial reports, municipal accounts	1903–1907
<i>Panel E. Short-Run Schooling Outcomes from the National Education Report</i>		
Public school pupils and schools	National education report (<i>Ministerie van Binnenlandse Zaken, 1901</i>)	1900
Pupils per class and attendance rate	National education report	1900
Mean school fee, public and private	National education report	1900
Private share of pupils	National education report	1900
Public-private baselines for the medium-run shares	National education report	1894–1896
<i>Panel F. Medium-Run Administrative Education Outcomes</i>		
Public and private schools and pupils	Provincial reports, education tables	1905–1907
Public and private regular teachers	Provincial reports, education tables	1905–1907
Sector shares and staffing intensity	Provincial reports, education tables	1905–1907
<i>Panel G. Protestant Mobilization</i>		
Collection committee presence and receipts	“A School with the Bible” lists in <i>De Standaard</i>	1890–1917
Protestant school presence	Unie census of Protestant schools	1896

Notes: Availability gives the years used in the main text, not the full span of each source; all were digitized from the printed originals for this paper. The composition variables are first principal components: religion from the 1889 Protestant and Catholic shares and a concentration index, demography from birth and death rates over 1885–1896, infant mortality in 1889, and net migration over 1886–1890. Education expenditure is reported in total and separately for primary, secondary and higher, other institutions, and arts and science. Private in Panels E to G denotes the printed *bijzondere* sector, which neither source divides by confession. The education-periodical outcomes for 1903–1909, which do read a school’s affiliation, are reported in the appendix and are not listed here.

Denominators differ by panel. Fiscal amounts are divided by the population of the same year and then averaged over 1903–1907. Panel E uses population in 1900. Panel F forms every ratio and population rate within a report year and then averages over the years available for a municipality; its teacher outcomes exclude the one province whose reports print no comparable teacher counts. All specifications condition on log population, pre-reform tax receipts per capita, the two composition components, and province fixed effects. The fiscal models add logged land area and population growth, the first stage adds logged land area, and the education models add log density.

Table 2: First Stage: Effect of Class Rank on Suffrage per Capita

	Suffrage Expansion (1896–1897)		Eligible Voters per Capita (1897)	
	(1)	(2)	(3)	(4)
Class Rank	0.019*** (0.002)	0.013*** (0.003)	0.021*** (0.002)	0.012*** (0.003)
Log Population (1896)		0.002 (0.004)		-0.020*** (0.003)
Tax Revenue per Capita (1889)		-0.009*** (0.003)		0.011*** (0.002)
Log Surface Area (ha, 1896)		-0.001 (0.004)		0.008*** (0.003)
Demographic Composition		-0.009** (0.004)		-0.004* (0.002)
Religious Composition		0.001 (0.002)		-0.002 (0.003)
R2 Adj.	0.216	0.278	0.216	0.298
R2 Within Adj.	0.086	0.159	0.090	0.185
Num.Obs.	1120	1120	1120	1120
Province FE	Yes	Yes	Yes	Yes
Instrument F-Statistic	105.7	24.4	111.0	22.5

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: The dependent variable in columns 1 and 2 is the increase in eligible municipal voters between 1896 and 1897 divided by population; in columns 3 and 4 it is the number of eligible municipal voters per capita in 1897. Class rank is the municipality's class under the 1896 electoral law, rising from 1 in the municipalities with the highest income and rent qualifications to 9 in those with the lowest, so a higher rank admits more men from a given local income distribution. Columns 2 and 4 add the predetermined controls reported in the table. All columns include province fixed effects and use the full sample of municipalities. Standard errors are in parentheses, clustered by province in columns 1 and 3 and heteroskedasticity-robust in columns 2 and 4. The instrument F-statistic is the excluded-instrument Wald statistic computed with each column's own covariance matrix.

Table 3: Ratepayer Enfranchisement and the Municipal Fiscal Footprint

	Province Fixed Effects (1)	Preferred Specification (2)
<i>Panel A. Log Municipal Expenditure per Capita</i>		
Suffrage Expansion	-10.660*** (2.388)	-11.219*** (3.457)
Predetermined Controls	No	Yes
Province Fixed Effects	Yes	Yes
First-Stage F-Statistic	105.4	17.6
Observations	1116	1069
<i>Panel B. Log Municipal Revenue per Capita</i>		
Suffrage Expansion	-11.957*** (2.495)	-11.494*** (3.314)
Predetermined Controls	No	Yes
Province Fixed Effects	Yes	Yes
First-Stage F-Statistic	105.2	17.3
Observations	1114	1067

Notes: Two-stage least-squares estimates. The endogenous variable is the 1896–1897 increase in eligible municipal voters per capita, instrumented by class rank under the 1896 electoral law. Outcomes are log per-capita averages for 1903–1907. The preferred specification includes log population, pre-reform tax revenue per capita, log surface area, demographic and religious principal components, population growth, and province fixed effects. Heteroskedasticity-robust standard errors are in parentheses. A one-percentage-point larger suffrage expansion changes a logged outcome by approximately the reported coefficient in percent. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Ratepayer Enfranchisement and Municipal Education Expenditure

Outcome	Estimate	S.E.	First-Stage F	Observations
<i>Panel A. Aggregate Education Expenditure</i>				
All Municipal Education Expenditure	-8.117***	(3.073)	17.3	1067
<i>Panel B. Components of Education Expenditure</i>				
Primary Education	-7.808***	(2.996)	17.5	1066
Secondary and Higher Education	-19.467	(20.722)	2.7	332
Other Educational Institutions	-7.025	(10.853)	8.3	634
Arts and Science	2.583	(5.768)	14.7	901

Notes: Two-stage least-squares estimates from the preferred fiscal specification described in Table 3. Outcomes are log per-capita averages for 1903–1907. Secondary and higher education combines the two corresponding ledger headings; the other rows retain substantively distinct accounting categories. Component samples vary with ledger reporting. In particular, the first stage is weak in the smaller secondary-and-higher-education sample, so that estimate is reported for completeness and interpreted cautiously. Heteroskedasticity-robust standard errors are reported. A one-percentage-point larger suffrage expansion changes a logged outcome by approximately the reported coefficient in percent. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Short-Run Public Provision and Prices, 1900

	Public					Private	
	Pupils	Schools	Pupils per Class	Attendance	Fee	Fee	Pupil Share
Suffrage Expansion	-1.842 (1.815)	-1.426 (2.150)	-0.200 (1.720)	-0.017 (0.267)	-8.612*** (2.488)	-2.900 (3.208)	2.106** (1.067)
Effect Unit	Percent	Percent	Percent	Percentage points	Percent	Percent	Percentage points
First-Stage F-Stat	21.9	21.9	21.9	21.1	22.0	14.7	22.1
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1078	1078	1078	1057	1074	560	1091

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: Two-stage least-squares estimates for a one-percentage-point larger suffrage expansion. The outcomes are municipality-level school stocks, enrollment, attendance, and mean fees in 1900, from the national education returns. Pupils, schools, pupils per class, and fees enter in logarithms, so their effects are approximate percent changes; attendance and the private pupil share enter in levels, so their effects are percentage points. The columns do not pair each public outcome with a private counterpart, and deliberately so. Under the funding settlement in force after 1889 the municipal budget financed the public schools alone, while private schools were supported by private contributions and a national subsidy toward teacher salaries, so the public columns measure the provision on which a council's fiscal response acted directly. Public provision was also statutory and close to universal, and public quantity and price measures are accordingly defined on almost the whole sample. A private school existed in about half of municipalities, so a private counterpart to each public quantity would condition on the extensive margin that the reform may itself have moved. The private side therefore enters through the two measures that remain informative without that conditioning: the fee, a price councils did not set, observed for the municipalities where the sector existed; and the private pupil share, whose denominator spans both sectors and which is defined wherever any school operated. All columns include predetermined scale, income, religion, and demographic controls and province fixed effects. Heteroskedasticity-robust standard errors are in parentheses.

Table 6: Democratization and the Organization of Primary Education, 1905–1907

Outcome	Estimate	S.E.	Mean	First-stage F	N
<i>Panel A. Public and Private Sector Levels</i>					
Public (Openbare) Pupils per 1,000	-2.722	(3.654)	219.57	26.8	448
Private (Bijzondere) Pupils per 1,000	16.459***	(5.819)	89.69	26.4	451
Public (Openbare) Teachers per 1,000	-0.082	(0.140)	6.74	26.1	394
Private (Bijzondere) Teachers per 1,000	0.518***	(0.192)	2.89	25.9	397
Public (Openbare) Schools per 1,000	-0.035	(0.058)	1.98	27.0	449
Private (Bijzondere) Schools per 1,000	0.089	(0.055)	0.86	26.6	452
<i>Panel B. Relative Public-Private Composition</i>					
Private (Bijzondere) Pupil Share	1.518	(0.985)	25.48	25.9	436
Private (Bijzondere) Teacher Share	3.253**	(1.348)	24.51	26.1	394
Private (Bijzondere) School Share	1.091	(0.991)	26.49	26.9	436
Private Minus Public Pupils per 1,000	19.958**	(8.582)	-133.77	26.8	448
Private Minus Public Teachers per 1,000	0.604**	(0.271)	-3.96	26.1	394
Private Minus Public Schools per 1,000	0.135*	(0.078)	-1.18	27.0	449
<i>Panel C. Aggregate Educational Expansion</i>					
All Primary-School Pupils per 1,000	14.514***	(4.708)	305.37	26.8	448
All Primary-School Teachers per 1,000	0.439**	(0.200)	9.52	26.1	394
All Primary Schools per 1,000	0.064	(0.080)	2.79	27.0	449
<i>Panel D. Extensive and Intensive Private Provision</i>					
Entry of Any Private (Bijzondere) School	-0.939	(2.565)	35.92	23.9	206
Private (Bijzondere) Pupils per School	1.321	(5.268)	124.79	17.4	315
Private (Bijzondere) Teachers per School	-0.197	(0.181)	4.29	15.2	253
Private (Bijzondere) Teachers per 100 Pupils	-0.032	(0.084)	3.54	15.2	253
<i>Panel E. Public and System-Wide Staffing Intensity</i>					
Public Teachers per 100 Pupils	-0.013	(0.051)	3.27	26.2	391
Public Teachers per School	-0.301***	(0.113)	4.24	26.4	392
All Teachers per 100 Pupils	0.023	(0.057)	3.21	26.2	391
Private Minus Public Teachers per 100 Pupils	0.030	(0.095)	0.07	15.8	249

Notes: Two-stage least-squares estimates for a one-percentage-point larger 1896–1897 suffrage expansion. Ratios and rates are formed within report-year and then averaged over available years. Private denotes the printed bijzondere sector, which includes confessional provision where the reports do not split private schools by denomination. The pupil and school shares in Panel B condition on their corresponding 1896 shares; every other outcome in the table, including all of Panels A and E, uses a post-level specification. Table A.9 reports the pupil and school shares at post level alongside the baseline-conditioned estimates. Public teachers per 100 pupils is the outcome declared primary before any municipal value was extracted, and the estimate reported here reproduces the frozen design exactly. Shares and school entry are reported in percentage points. School entry is estimated among municipalities with no private school in 1896. Private-sector intensity outcomes are defined among municipality-years with private provision, and public teachers per school and per 100 pupils among municipality-years with public teaching staff. Teacher outcomes exclude Groningen because its reports do not print comparable teacher counts. All models include predetermined density, population, tax, religion, and demography controls and province fixed effects. HC1 standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: IV Estimates for Regular-Teacher Vacancy Offers

Specification	Estimate	Municipalities	First-stage F
Public	-0.612 (0.647)	722	12.9
Private	1.510** (0.588)	434	11.7
Matched private minus public	2.143* (1.245)	356	9.5
Stacked private interaction	2.122** (0.907)	799	11.8

Primary source frame is the closed specialist-periodical corpus, excluding every event touching De Tijd. Outcome is the municipality-sector mean standardized log annual offer, 1903–1909. Models use corrected annual offers between 300 and 2,500 guilders, predetermined controls, and province fixed effects. Stacked inference is municipality-clustered. Standard errors are in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Efficiency or Quality? Teacher Recruitment and Public Pay

	(1)	(2)	(3)
<i>Panel A. Recruitment frictions</i>			
Repeated-listing share	-0.018*	0.019	0.041*
	(0.010)	(0.013)	(0.022)
Log advertising duration	-0.068*	0.150**	0.259**
	(0.041)	(0.062)	(0.104)
Vacancy-to-appointment match rate	0.006	-0.029**	-0.031*
	(0.008)	(0.014)	(0.017)
Sample	Public	Private	Private – Public
Municipalities	893	530	484
First-stage F	18.2	16.6	14.0
<i>Panel B. Composition of advertised posts</i>			
Female-designated post share	0.001	0.025*	0.026
	(0.013)	(0.014)	(0.018)
Assistant or pupil-teacher post share	0.001	0.001	0.001
	(0.002)	(0.002)	(0.003)
School-head post share	-0.010	-0.024**	-0.025
	(0.013)	(0.012)	(0.017)
Sample	Public	Private	Private – Public
Municipalities	893	530	484
First-stage F	18.2	16.6	14.0
<i>Panel C. Churn: vacancies per existing teaching post</i>			
Vacancies per 100 teachers, 1905–1907	1.540	19.634*	15.964*
	(1.742)	(10.056)	(9.223)
Vacancies per 1,000 inhabitants, 1903–1909	-0.070	0.973***	1.043***
	(0.129)	(0.336)	(0.370)
Sample	Public	Private	Private – Public
Municipalities	397–1,113	257–1,113	254–1,113
First-stage F	26.1–27.0	15.9–27.0	16.7–27.0
<i>Panel D. Public salary schedules, 1908 survey</i>			
Log entry salary (schedule floor)	-0.042**	0.030	
	(0.021)	(0.019)	
Log implied maximum salary	-0.011	-0.011	
	(0.023)	(0.014)	
Rests on the statutory minimum	0.044**	-0.023	
	(0.020)	(0.019)	
Sample	Ordinary teachers	School heads	
Municipalities	378–945	455–897	
First-stage F	3.4–22.1	6.3–19.1	

Instrumental-variable estimates of the effect of the 1896–1897 expansion of the municipal-council franchise on the recruitment behaviour of public and private primary schools. Panels A to C are built from teacher vacancy advertisements printed in the education periodicals between 1903 and 1909, excluding the general newspaper *De Tijd*, and Panel D from the 1908 survey of public salary regulations. Each cell is a separate regression; the reported coefficient is on the change in eligible voters per capita, instrumented by the municipality's class rank under the 1896 Kieswet, and is scaled to a one percentage point expansion. All models include log population density, log population, tax revenue per capita in 1889, and the first principal components of religious and demographic composition, with province fixed effects and heteroskedasticity-robust standard errors in parentheses. In Panels A to C column (1) estimates the effect on public schools, column (2) on private schools, and column (3) on the private-minus-public difference within municipalities that advertise in both sectors; in Panel D column (1) covers ordinary teachers and column (2) school heads. The number of municipalities and the first-stage F statistic are reported once per panel and appear as a range where the panel's outcomes rest on different samples: in Panel C because the churn rate is defined only for the five provinces whose administrative reports print teacher counts while the population-denominated row is national, and in Panel D because the 1908 survey prints a legal-minimum indicator for every municipality that returned it, an entry salary only where a figure is printed, and a maximum only where the schedule also states its increments. The instrument is weakest for the salary-schedule outcomes in Panel D, whose coefficients should be read as suggestive. Appendix A describes the sources, the samples, and the missing-value rule behind each panel.

Reading the panels. The salary evidence is a relative divergence, private offers rising while public offers do not follow, rather than an absolute public pay cut, and the panels separate the two readings that survive it. If public pay merely absorbed rents the council had been paying, frictions (Panel A), the mix of advertised posts (Panel B) and separations (Panel C) should be unaffected and any adjustment should sit at the top of the salary schedule (Panel D). If public pay instead fell behind the outside option, the council should advertise longer and more often and fill posts less often, shift toward cheaper posts, lose staff more frequently, and cut entry pay or fall back on the statutory floor. Standard errors in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A Data Appendix

The analysis joins statutory records, provincial voter lists, accounts, and education tables, HDNG, the 1889 population census, national education returns, and two corpora of the digitized press at the historical municipality level. The fiscal sample retains municipalities observed in the statutory table, the voter lists, the predetermined controls, and the relevant account category. Annual fiscal amounts are divided by contemporaneous population and then averaged over 1903–1907. A total is missing only when all of its recorded components are missing.

Figure A.1 shows what the fiscal source looks like in print, and Figure A.2 enlarges part of the opening that follows it. Each province published an annual report whose statistical appendices ran to several hundred pages. One of those appendices, headed *Overzicht der Gemeenterekeningen*, or overview of the municipal accounts, printed every municipality's realized accounts in two parts: part A, *Werkelijke ontvangsten*, for actual receipts, and part B, *Werkelijke uitgaven*, for actual expenditure. The figures show part B as printed in the report of Groningen, a small northern province, for 1903.

Every province printed this appendix, in a layout that differs only in detail. Municipalities are the rows, listed alphabetically after the provincial capital, and the fifty-seven municipalities of Groningen therefore fit in a single block of rows that is carried across successive openings of the volume. Spending headings are the columns, numbered consecutively from the municipality name to the total and grouped under printed titles that name the branch of municipal activity: *Bestuur der gemeente* for government and administration, *Inning der belastingen* for tax collection, *Politie* and *Brandweer* for police and fire service, *Openbare verlichting* for street lighting, *Gezondheidsdienst* for public health, *Openbare werken* for public works, *Inrichtingen van nijverheid* for municipal trading enterprises such as a gasworks, an electricity plant, a waterworks, a telephone service, a tramway or a slaughterhouse, *Onderwijs, Kunst en Wetenschap* for education, arts and science, *Armwezen* for poor relief, *Beheer en onderhoud van bezittingen* for the upkeep of municipal property, *Kerkelijke zaken* for payments to ministers and church buildings, and *Gemeenteschulden* and *Pensioenen* for debt service and pensions. The final numbered column, *Totaal der Uitgaven*, gives each municipality's total expenditure, and the last row of every column gives the provincial total. Amounts are in guilders and cents. A dash means that the municipality recorded nothing under that heading, and the letters attached to some enterprise entries distinguish a service the municipality operated itself from one it had granted under concession. A page of explanatory notes keyed to the column numbers follows the table and comments on unusual items, such as the cost of a school building included in a particular municipality's public-works figure.

Figure A.2 enlarges the education block. Its columns separate spending on higher education, on secondary education, on primary education (*Lager Onderwijs*, column 23), on other teaching institutions such as infant, sewing, music, singing and swimming schools, and on the promotion of arts and science through museums, libraries, theaters and public festivities. Column 23 is the heading the paper follows, and the figures printed there are what municipal primary-education spending per capita is built from. The nine expenditure categories in Table A.5 are formed by adding the numbered columns within each of these printed groups, and the eleven revenue categories are formed the same way from part A. Reading a whole province-year therefore means reading one such sheet across its successive openings, and reading the full sample means doing so for eleven provinces, five years, and both parts of the accounts.

[Figure A.1 here]

[Figure A.2 here]

The education returns cover public and private primary schooling. Public and private counts are aggregated separately within municipality and year. Public pupils and schools are expressed relative to population, pupils per class are constructed only when classes are positive, and attendance is the number attending relative to enrolled pupils. Mean fees are recorded separately by sector. Table A.1 describes the 1900 observations. Returns from 1894 and 1895 provide predetermined controls; 1899 supplies the adjacent post-reform replication.

Three restrictions apply to the 1900 outcomes and are worth stating explicitly, because each removes observations rather than recoding them. First, attendance above one is a recording error and is set to missing, which affects ten municipalities, and attendance is then trimmed at its first percentile, which affects a further eleven. Second, the public quantity and price measures are defined only where the municipality operated at least one public school, and the private fee and private share only where the corresponding sector existed. Third, the same rule applies to the mean fee, which has no meaning for a sector with no schools.

The second restriction deserves the closest scrutiny, because closure of the public school is precisely the response that the null results on public quantities are used to rule out, and a municipality that closed its last public school would leave the sample rather than register as a decline. The number settles the question: of the 1,068 municipalities in the estimation sample observed in the 1900 returns, 12 report no public school, slightly more than one percent. Public provision was very nearly universal in 1900, so conditioning on a surviving public school cannot be concealing a wave of closures, and the extensive margin the objection points to is too thin to carry the fiscal contraction. The nulls on public pupils,

public schools, class size, and attendance are therefore statements about municipalities that kept a public school, which in this period is almost all of them.

[Table [A.1](#) here]

The medium-run administrative panel comes from the education tables of the provincial reports for Friesland, Gelderland, Groningen, Limburg, Overijssel, and Utrecht in 1905, 1906, and 1907. Its design was frozen in a dated, committed manifest before any municipal value in the target tables was read, and the order it prescribes was followed: an audit of printed headers, notes, layout, and page completeness; then a fixed list of included province-years, field mappings, and a declared primary outcome; then extraction of every included value irrespective of its relationship to class rank; then source-blind quality rules with a retained audit log; then estimation of the declared specifications. The primary outcome, regular public teachers per hundred public pupils, was selected by a rule written down in advance and was not revised once its estimate was known.

Extraction covered 482 physical source pages. Page-level quality control passed 467 of them, retained four short end-pages for review, and failed none. A source-blind audit of pupil-teacher and teacher-school ratios, which used no information about class rank or the suffrage shock, identified 53 contributing pages for stronger re-reading; that pass found genuine column-shift errors, including a pupil count that had entered a teacher field, and reduced the implausible-ratio flags from 47 to three, which are retained and visible in the audit log. All 7,954 extracted source rows link to a legal municipality, yielding 2,251 municipality-year-sector cells. Every retained value carries its province, source year, printed table heading, and source page. Seven ambiguous duplicate municipality identifiers in the merged base are dropped rather than attaching one post-reform outcome to two different treatment values. Ratios and rates are formed within each report year before averaging across years, so a municipality observed in one or two years contributes the years it has rather than being discarded. A sector absent from a municipality-year is coded zero only when the same complete source cell records the other sector; a wholly missing municipality-year stays missing. Groningen prints no comparable teacher counts and therefore enters the school and pupil outcomes but not the teacher outcomes.

The education-periodical corpus consists of eleven titles: ten education periodicals of Protestant, Catholic, public, and mixed orientation, and one general newspaper, the Catholic daily *De Tijd*, which is the only Catholic source observed before 1908. Advertisements and notices in these papers yield dated signals of teacher vacancies, appointments, and school openings. They are flows: a vacancy advertisement records staffing demand rather than the stock of teachers, an appointment records hiring, and an opening records entry. Repeated notices are linked into events across issues and periodicals using municipality, school, post, and date, so the outcome counts distinct events rather than printed

mentions. Affiliation is read from the advertisement when it is stated and otherwise assigned from the documented orientation of the periodical. Coverage matters for interpretation, because titles of different orientation enter the record in different years; Figure A.4 therefore distinguishes publication availability, actual harvesting and extraction, and the source-years that enter the pooled outcomes. The outcome window of 1903 through 1909 lies entirely after the reform, and annual variation within it is used only for timing and source diagnostics, never as an event study or a local-projection design.

[Figure A.4 here]

Periodical measures begin with issue-level notices and advertisements. Municipality, school, position, affiliation, salary, housing terms, and event type are retained when stated. Cross-issue and cross-periodical duplicates are grouped into event spells. Extensive outcomes assign zero to municipalities with no observed event in the fully processed source-years; shares and offered terms remain missing when their denominator is absent. Population in 1896 is the predetermined denominator for event rates. The corpus cannot measure unadvertised hiring, and changes in the probability that a salary or affiliation is printed can select conditional outcomes. Seven historical municipality identifiers appear twice in the merged base, yielding 1,120 rows for 1,113 distinct identifiers; removing those duplicate rows produces similar headline estimates. The headline table retains all 1,120 rows, matching the fiscal specifications. The source-selection audit uses the 1,113 distinct identifiers because its leave-one-source-out and stacked constructions require a unique municipality key; estimates on the two samples differ only in the third decimal.

Table 8 draws on three of these sources, and the rule for what counts as a zero differs across its panels. The recruitment and composition panels use the closed specialist-periodical corpus for 1903 through 1909, excluding every advertisement that appears in *De Tijd*. They are intensive margins: a municipality and sector enters only where at least one vacancy is observed, and an unobserved cell is never filled in with a zero. The vacancy-to-appointment match rate in the first of those panels is a heuristic link between an advertisement and a later appointment notice in the same corpus, and its level differs between the sectors mainly because periodicals differ in whether they print appointments at all, so only the estimated gradient within a sector carries evidence. The churn panel is instead an extensive margin, where a municipality with a positive teacher stock and no advertisement is a genuine zero, because coverage is national in every year. Its first row divides vacancies by the teacher stock recorded in the medium-run administrative panel, which prints teacher counts for five of its six provinces, while its second row is denominated by population and is national. That is why the panel reports its sample size and its first-stage strength as a range rather than a single figure.

The salary panel of the same table rests on the 1908 survey of public salary regulations, in which an absent return is unknown rather than zero. The source contains no private schedules, so no comparison between the sectors is possible there, and a municipality resting on the bare legal floor may print no salary figure at all. That printing rule governs both the panel's range and its interpretation. The legal-minimum indicator is defined wherever a municipality returned the questionnaire, an entry salary only where a figure is printed, and an implied maximum only where the printed schedule also states its increments, which is the rarest of the three and leaves the weakest instrument. The two salary levels are therefore estimated on different sets of municipalities and are conditional on a reporting decision that the franchise expansion itself affects. Throughout, the corpus records offered salaries and advertising behaviour, never accepted wages, applicant quality, or teacher skill.

B Extended Historical Background

The nine-class schedule originated in the Personal Tax Law of April 1896 and was incorporated by reference into the Electoral Law of September 1896 (*Wet tot regeling der Personele belasting*, 1896; *Kieswet*, 1896). The tax law adjusted exemptions and deductions to local costs of living. The electoral law then used the same classes to set rent and income qualifications. Table A.2 places the reform in the sequence of Dutch suffrage rules since 1848.

[Table A.2 here]

The shared classification means that rank affected both electoral eligibility and national Personal Tax liability. The electoral law adopted the classification by statutory cross-reference rather than constructing one of its own. Article 1a conditions eligibility on assessment under one or more of the first five *grondslagen* of the Personal Tax as regulated by the law of 16 April 1896, and the threshold table governing Article 1b eligibility reproduces the same nine classes without stating any classification principle. The tax law is thus the primitive institution and the electoral law is derivative.

That overlap creates a compound statutory treatment. Class rank simultaneously determines the rental-value exemption under Article 12 of the tax law, which falls from 125 guilders in Class 1 to 25 guilders in Class 9; the rental-value deduction under Article 13, which falls from 110 to 10 guilders across the same range; and the income and rent thresholds for electoral eligibility under Article 1b. Articles 12 and 13 together imply a break-even rental value above which a household is assessed for the Personal Tax and, through Article 1a, thereby enfranchised. Between adjacent classes at the low-cost end of

the schedule that break-even value falls from roughly 60 to roughly 35 guilders a year, so households renting between those amounts are enfranchised and brought into national tax liability at the same time.

The fiscal magnitude of the additional liability is small. A marginal household renting at 50 guilders a year has a taxable base of $50 - 25 - 10 = 15$ guilders under the lower-threshold schedule, after the Article 12 exemption of 25 guilders and the Article 13 deduction of 10 guilders. At the lowest bracket rate this implies an annual liability of roughly one to two guilders, against nothing at all under the adjacent higher-threshold schedule, whose break-even rental value the same household does not reach. Municipalities could levy a surcharge on the national Personal Tax, which at contemporaneous rates adds at most a further half guilder a year. The whole difference therefore remains well below two guilders a year, a negligible share of the income of a marginal voter and far too small to move aggregate municipal fiscal or schooling outcomes. The revenue in any case accrued nationally rather than mechanically entering the municipal budget. The main institutional channel is therefore the discrete change in eligibility, conditional on the economic variables that generated class placement.

Municipalities financed public schools and set their fees, while confessional associations relied on private contributions and national support. This division also shaped teacher recruitment. Public posts appeared in general or public-oriented education papers; Protestant and Catholic organizations used their own papers and association networks. The corpus preserves that source-specific structure rather than treating selective denominational evidence as a national school census.

C Full Model of Education Austerity and Confessional Substitution

The following model separates observed public capacity from costly inputs per place, the cost of those inputs, the share charged to parents, and relative confessional preferences. It formalizes why lower spending and fees can coexist with stable public stocks, and why confessional expansion rules out pure efficiency as the sole channel.

C.1 Setup and Comparative Statics

This section gives a minimal static model that rationalizes four features of the evidence: municipal education expenditure and the public-school fee fall, measured public provision does not change, confessional provision expands, and public hiring and wages need not increase. The model distinguishes measured public capacity from the resource intensity of

a public-school place. This distinction is necessary because schools and pupils can remain unchanged even when expenditure per place falls.

The political shock and its causal channels. Let Z denote the municipality's institutional exposure under the Kieswet classification and let S denote the resulting increase in the enfranchised share. The quasi-exogenous empirical variation begins with Z , which induces differential changes in S ; the municipal policy variables are responses to that suffrage treatment, not themselves exogenous shocks. Write these responses as

$$g = g(S), \quad \phi = \phi(S), \quad \eta = \eta(S), \quad K = K(S). \quad (1)$$

Here $g_S < 0$ is input austerity: the municipality uses fewer costly inputs per public place. A value $\phi_S < 0$ is instead shorthand for an induced reduction in the cost of a given input bundle, through such mechanisms as procurement, administrative reorganization, bargaining, or genuine efficiency improvement. It is not an automatic consequence of extending the franchise, and should be called an efficiency channel only when the institutional evidence supports that interpretation. Finally, $\eta_S < 0$ changes the incidence of public-school costs by reducing the share charged to parents. It lowers their user price but does not lower the resource cost c . The empirical stability of public schools and enrollment motivates $K_S \simeq 0$, rather than imposing that councils could never change capacity.

Figure A.3 displays this causal structure. Solid arrows are the municipal-policy channels in the baseline model. The dotted arrow from S to θ represents a possible causal change in the religious composition or preferences of the pivotal electorate. The dashed arrows represent a different case: an omitted confessional-mobilization factor U that changes θ and is also correlated with measured suffrage expansion. The first is an additional mechanism of the suffrage effect; the second is confounding.

[Figure A.3 here]

Households and substitutability. There are two education goods. Let q_o denote enrollment in public (*openbaar*) schools and q_c enrollment in confessional schools. A public-school place is supplied with input intensity $g > 0$, so effective public education is $e_o = gq_o$. The input g can represent instructional resources, staffing intensity, or other costly attributes not measured by the number of schools or pupils. Because g is an intensity rather than a headcount, an unchanged number of teachers per pupil is compatible with a fall in g whenever the experience or quality of those teachers, or what else the school provides,

declines. A representative household has quasi-linear utility

$$u = z + a(e_o + q_c) + \theta q_c - \frac{b}{2} (e_o^2 + q_c^2 + 2\gamma e_o q_c), \quad (2)$$

where z is the numeraire, $a > 0$, $b > 0$, and $0 < \gamma < 1$. The parameter θ captures the relative value of confessional education. The cross-partial $\partial^2 u / \partial e_o \partial q_c = -b\gamma < 0$, so effective public and confessional education are substitutes. The household faces

$$z + p_o q_o + p_c q_c \leq y,$$

where p_o is the public-school fee and p_c is the price of confessional education.

Public capacity and municipal finance. The municipality supplies a fixed number K of public-school places, so $q_o \leq K$. Let $c(g, \phi)$ be the resource cost per place, with $c_g > 0$. The parameter ϕ is a cost shifter, with $c_\phi > 0$, so a fall in ϕ represents a pure efficiency improvement. Gross municipal education expenditure is

$$X_o = c(g, \phi)K. \quad (3)$$

Suppose the fee covers a fraction $\eta \in (0, 1)$ of the resource cost of a place:

$$p_o = \eta c(g, \phi), \quad R_o = p_o K, \quad (4)$$

where R_o is municipal school-fee revenue. A ratepayer-induced austerity package can combine input austerity, $dg < 0$, with a cost-saving reform, $d\phi < 0$. It can simultaneously lower the fee share η , which lowers the price paid by parents without itself reducing c . Holding capacity fixed,

$$d \log X_o = d \log c, \quad (5)$$

$$d \log p_o = d \log R_o = d \log \eta + d \log c. \quad (6)$$

Consequently, $dc < 0$ and $d\eta \leq 0$ produce lower expenditure, a lower user price, and lower fee revenue without requiring fewer public-school places.

The policy functions in equation (1) make the reduced-form derivatives with respect to

the suffrage treatment explicit:

$$\frac{d \log X_o}{dS} = \frac{c_g}{c} g_S + \frac{c_\phi}{c} \phi_S + \frac{K_S}{K}, \quad (7)$$

$$\frac{d \log p_o}{dS} = \frac{\eta_S}{\eta} + \frac{c_g}{c} g_S + \frac{c_\phi}{c} \phi_S. \quad (8)$$

Thus the fiscal evidence identifies the combined unit-cost response, not g_S and ϕ_S separately. The difference between the fee and expenditure responses is informative about η_S , subject to $K_S \simeq 0$.

Public enrollment remains at capacity when demand for the last available place remains sufficiently high. The relevant Kuhn–Tucker condition is

$$g [a - b (gK + \gamma q_c)] \geq p_o. \quad (9)$$

If this inequality holds before and after the policy change, then $q_o^* = K$ in both equilibria. The fall in the fee helps preserve this condition even if lower input intensity makes a public place less attractive. The model therefore does not require the public quantity to respond whenever the public budget changes.

Confessional supply and equilibrium. Confessional schools face the inverse supply curve

$$p_c = m + \kappa q_c, \quad \kappa \geq 0. \quad (10)$$

Conditional on the public sector operating at capacity, the household's first-order condition for confessional education is

$$a + \theta - b (q_c + \gamma gK) = p_c.$$

Combining demand with equation (10) gives

$$q_c^* = \frac{a + \theta - m - b\gamma gK}{b + \kappa}, \quad (11)$$

for an interior equilibrium. Its comparative static is

$$dq_c^* = \frac{d\theta - b\gamma K dg}{b + \kappa}. \quad (12)$$

Allowing each policy variable to respond to suffrage gives the more general decomposition

$$\frac{dq_c^*}{dS} = \frac{\theta_S - b\gamma (K g_S + g K_S)}{b + \kappa}. \quad (13)$$

Under the empirically motivated restriction $K_S = 0$, only the input-intensity response g_S , not the pure cost shifter ϕ_S or the fee-share response η_S , generates confessional substitution in the baseline model.

An input-intensive austerity policy has $dg < 0$. It reduces effective public education gK while leaving measured public enrollment K unchanged. Because the two goods are substitutes, confessional enrollment and capacity then increase even if the general demand parameter a and the relative preference parameter θ remain fixed. A rise in the relative appeal of confessional schooling, $d\theta > 0$, reinforces the same response.

Equation (12) also separates two interpretations of the fiscal contraction. If austerity is a pure efficiency improvement, $d\phi < 0$ with $dg = 0$, it lowers expenditure and the fee but does not by itself raise confessional quantity. In that case, confessional expansion requires $d\theta > 0$, consistent with a simultaneous *schoolstrijd* preference shift. If austerity instead reduces costly inputs per public place, $dg < 0$, substitutability alone generates confessional expansion. The observed quantities cannot distinguish these two channels, but they reject the claim that a pure cost saving with unchanged inputs is sufficient to explain every result.

Preference change versus confounding. Equation (13) clarifies three empirically distinct interpretations of θ . First, the clean input-austerity interpretation imposes $\theta_S = 0$; a positive confessional response then requires $g_S < 0$ when $K_S = 0$. Second, $\theta_S > 0$ may itself be caused by the franchise extension if the new pivotal electorate places greater relative value on confessional schooling. This is a valid causal channel from suffrage to confessional expansion, but it means that the latter cannot by itself identify a reduction in g . Third, confessional mobilization may move θ for reasons outside the franchise reform. A common national shift in θ , uncorrelated with the municipality-level suffrage treatment, can explain aggregate growth but not the differential treatment gradient. By contrast, a municipality-specific shift correlated with S , represented by U in Figure A.3, confounds the estimated suffrage effect unless the Kieswet instrument is conditionally independent of U .

The empirical task is therefore twofold. The fiscal outcomes establish that the suffrage treatment reduced public unit costs and parental fees. Measures of pre-reform confessional strength and annual confessional mobilization can then test whether the post-reform expansion was already developing where S would be large. In particular, predetermined Protestant school presence and Unie collection committee presence or receipts can be used as controls, heterogeneity measures, and pre-trend outcomes. Section 5.3 reports that test: neither the level nor the pre-reform trend of Protestant collection activity responds to the instrumented expansion, so the paper maintains $\theta_S = 0$. That panel's universe is Protestant, so Catholic mobilization is not directly tested. Under this restriction $g_S < 0$ is required by a positive confessional response, and the observable implications of input intensity that this

argument calls for have since been examined: public staffing per pupil does not fall, while the entry salary a council offered does.

Teachers and school formation. Let public teacher demand be $n_o = \ell(g)K$, with $\ell'(g) \geq 0$, and let confessional teacher demand be $n_c = \ell_c q_c$, with $\ell_c > 0$. With fixed public capacity and $dg \leq 0$, public hiring need not rise and may fall. By contrast, $dq_c^* > 0$ implies greater confessional teacher demand and, when existing confessional capacity is costly to expand at the margin, more school entry. Nothing in the model requires a change in the female share of public teachers or in the general wage level. Those outcomes require an additional change in relative factor prices or hiring technology. The offered entry salary is one such relative price. A council appointing at a lower grade while the outside offer rises is changing the composition of the input bundle behind a place, so the observed compression is read here as a movement in g rather than in ℓ .

What is held constant. The model holds the general education-demand schedule fixed by setting $da = 0$. It does not impose a fixed equilibrium total $q_o + q_c$. Imposing $q_o + q_c = \bar{Q}$ would make the empirical combination $dq_o = 0$ and $dq_c > 0$ impossible as an accounting identity. The evidence must therefore be read as allowing an extensive enrollment margin, as measuring confessional capacity and teacher demand rather than only a fixed stock of pupils, or both. This is consistent with the administrative records, in which total pupils and total teachers both rise while public stocks are unchanged. The vacancy and opening outcomes supply further flow evidence of staffing demand and school formation rather than a census of pupils.

Empirical prediction. Suppose the suffrage shock induces $dc < 0$, $d\eta \leq 0$, leaves K unchanged, and preserves the capacity condition in equation (9). If, in addition,

$$d\theta - b\gamma K dg > 0,$$

then the model predicts

$$dX_o < 0, \quad dp_o < 0, \quad dq_o = 0, \quad dq_c > 0.$$

These are the central fiscal and education patterns. The same equilibrium allows public hiring, the female-teacher share, and general wages to remain unchanged, while confessional teacher demand and school formation increase.

D Persistence of the Suffrage Shock and OLS Bias

The design treats the 1896–1897 extension as one cross-sectional shock. Table A.3 shows no detectable effect of that shock on additional local suffrage growth between 1897 and 1903. The result is used only to support design interpretation: the initial extension persisted without a detectable sequence of endogenous local franchise changes. Later outcomes are therefore medium-run responses to the initial reform, not responses in a repeated-treatment panel.

[Table A.3 here]

Ordinary least squares is mechanically vulnerable to mean reversion in this comparison. Let observed suffrage be $S_{it} = S_{it}^* + u_{it}$, where S_{it}^* is persistent suffrage and u_{it} is a transitory disturbance. Define $X_i = S_{i,1897} - S_{i,1896}$ and $Y_i = S_{i,1903} - S_{i,1897}$. The 1897 disturbance enters X_i positively and Y_i negatively. If the structural relation is

$$\Delta S_{i,1903-1897}^* = \beta \Delta S_{i,1897-1896}^* + \varepsilon_i,$$

then, under serially uncorrelated disturbances,

$$\text{plim } \widehat{\beta}_{OLS} = \frac{\beta \text{Var}(\Delta S_i^*) - \sigma_u^2}{\text{Var}(\Delta S_i^*) + 2\sigma_u^2}.$$

Even when $\beta = 0$, the probability limit is negative. Instrumenting the first change with statutory class rank removes the shared transitory component when rank is orthogonal to u_{it} . The IV estimates accordingly address persistence of the reform-induced component rather than the negative correlation created by a shared endpoint.

E Robustness Checks and Supplementary Analyses

This section collects the checks and supporting analyses behind the results reported in the main text. It covers the balance evidence for the identifying assumption, the composition of the municipal accounts and the full fiscal decompositions, the baseline and inference sensitivity of the administrative education estimates, the alternative specifications for the national school returns, the education-periodical evidence and the audit of the corpus that produces it, and the construction of the Protestant collection panel together with the mechanism tests it supports.

E.1 Identification and Balance

Class rank is strongly related to population, surface area, and pre-reform tax receipts because these variables proxy the scale and wealth used in administrative assignment. Table A.4 documents this relationship and shows that, once scale controls and province effects are included, the remaining predetermined characteristics have limited joint predictive power. These checks support conditional rather than unconditional exogeneity.

[Table A.4 here]

E.2 Fiscal Robustness and Decompositions

Table A.5 sets out what the municipal accounts of 1903 through 1907 contained, and so what the fiscal contraction was a contraction of. Each heading's share is computed within a municipality and year, averaged over the five years, and then averaged across municipalities, so the reported figure is an unweighted mean of municipal shares in which a small rural municipality counts for as much as a large city; a share of aggregate amounts would give a different, city-dominated picture. The denominator of each share is reconstructed from the accounts themselves, total expenditure being the sum of the nine spending headings and total revenue the sum of the eleven revenue headings. A heading that a municipality never reports in any of the five years is treated as missing rather than as zero, and that municipality leaves the row, while within a municipality that does report the heading a year with no entry is a genuine zero. Because the rows average over different sets of municipalities, the mean shares within a panel need not sum to one. The revenue denominator is total receipts as the accounts record them, including the carry-over balance from previous years and the proceeds of loans and asset sales; those financing items are together close to a fifth of recorded revenue, so shares of ordinary revenue are somewhat larger than the ones printed here.

Two revenue headings need definition. The statutory state grant is the general grant to municipalities under the Act of 24 May 1897 together with contributions toward higher education, and the heading that follows it collects the remaining state and provincial grants, subsidies, gifts and legacies. The surcharges on the national land and personal taxes appear in the accounts both as the number of surcharge units levied and as the guilders they raised, and only the guilder amounts enter the table. Four features of the composition bear on the analysis. Education, arts and science is the largest single expenditure heading, absorbing about a third of municipal spending on average, which is why the paper follows the fiscal shock into education rather than into any other service. On the revenue side, local direct taxation and the statutory state grant are jointly the largest headings,

at about a fifth each; their averages are close enough, and the samples behind them different enough, that they are best read as the two principal sources of municipal revenue rather than ranked against one another. School fees are a very small part of municipal revenue, about two percent, so parents met directly only a small part of a bill that absorbed a third of municipal spending. Municipal trading enterprises, finally, are a selected heading, reported by a minority of municipalities and mostly by larger places operating a gasworks, a waterworks, or a pawnbank, so the average share in that row describes those municipalities rather than the experience of a typical one.

[Table A.5 here]

Tables A.6 and A.7 display the full accounting decompositions. Governance, tax compliance, public services, finance, and primary education contribute to the expenditure contraction. Several revenue sources also fall, including local taxation, transfers, and borrowing. Category samples differ because sparse ledgers do not report every activity in every municipality, and weak first stages limit inference for some specialized headings. The decompositions establish breadth, while the aggregate two-panel result remains the preferred summary.

[Table A.6 here]

[Table A.7 here]

Table A.8 reports the absolute difference between revenue and expenditure. The estimate indicates that the fiscal contraction did not produce a larger imbalance. Because the outcome is an absolute gap rather than a directional surplus measure, it is used only as a consistency check.

[Table A.8 here]

E.3 Education Robustness

Table A.9 varies the baseline and the post-period coverage rule for the three administrative composition shares. The private pupil-share coefficient is largest in the post-level model. Conditioning on the 1896 share reduces it to about one and a half percentage points, while the strictly pre-law 1894–1895 baseline and the three-year-balanced specification give similar estimates between one and two points; the change-score estimate is smaller and imprecise. The private school share follows the same pattern, with a positive post-level estimate that attenuates once its baseline is included. The private teacher share, by contrast, changes little when the sample is restricted to municipalities observed in all three

post years, and there is no comparable pre-reform teacher count to support a baseline-conditioned specification for it. The table therefore supports the persistence of the teacher-composition result while showing that the pupil and school shares are sensitive to how much pre-existing sectoral composition is absorbed.

[Table [A.9](#) here]

Allowing spatial dependence between neighboring municipalities leaves the administrative conclusions intact. Point estimates are unchanged by construction, the nulls are unaffected, and the increase in private teachers per thousand residents remains clearly distinguishable from zero. The other private-expansion estimates lose some precision, as cross-sectional estimates on neighboring units usually do. The only estimate whose status changes is public teachers per school, which is why the main text does not build on it.

Table [A.10](#) replicates the school specification in 1899. The public-fee coefficient is close to the 1900 estimate in magnitude and direction. Table [A.11](#) adds the 1894–1895 level of each outcome to the 1900 specification; the public-fee estimate becomes smaller and less precise. Table [A.12](#) applies the same control to 1899. The pre-reform level of an outcome is a strong predictor of its 1900 value in every column, so these specifications absorb much of the cross-sectional variation the instrument acts on and attenuate the estimates generally rather than the composition estimates in particular. They are reported as a demanding check on the fee result, which survives in direction and magnitude, and not as the basis for the main-text reading of the composition estimates.

[Table [A.10](#) here]

[Table [A.11](#) here]

[Table [A.12](#) here]

Table [A.13](#) compares heteroskedasticity-robust and 25-kilometer spatial inference for the mechanism-discriminating outcomes. Spatial inference preserves the public-fee conclusion but reduces precision for the confessional composition measures and for some vacancy levels. We therefore distinguish the strong fee result and the positive confessional level effects from the more sensitive relative-share evidence.

[Table [A.13](#) here]

Table [A.14](#) reports the coverage and distribution of the vacancy offers behind the main salary result. Offers are observed for both sectors in the closed specialist-periodical frame, with missing quotes left missing rather than coded as zero. Table [A.15](#) shows that the rise in private offers and the private-over-public divergence remain positive in a stricter

homogeneous source frame, annual models with year effects, tighter admissible salary bounds, and the full corpus. The matched direct gap is less precise than the stacked interaction because it retains only municipalities observed in both sectors.

The offers remain selected observations rather than accepted wages. Table A.16 estimates the margins determining whether the corpus records a vacancy and whether that vacancy quotes a salary. The suffrage expansion predicts the presence of private vacancy activity, but the salary-reporting shares are imprecisely estimated in both sectors. Table A.17 reports modest positive correlations between public vacancy offers and the independently extracted public salary schedules. Its education-budget specifications are secondary checks and are not mediation estimates, since they require the stronger exclusion restriction that class rank affects offers only through education spending.

Table A.18 probes the interpretation of the main mechanism matrix. The salary divergence persists when advertisements mentioning housing are removed or the housing-mention rate is controlled, and the recruitment-friction pattern is similar in the homogeneous specialist frame. The text-explicit and teacher-weighted public specifications have weak first stages and carry little evidentiary weight. Panel E addresses the 1908 schedule-selection problem directly. The suffrage expansion changes whether an ordinary-teacher salary figure is printed, and the entry-floor decline survives on the common sample that prints both entry and maximum pay. Salary levels nevertheless remain conditional on a reporting decision affected by treatment, while the first stage for maximum pay is too weak for its null estimate to establish an unchanged top of the schedule.

[Table A.14 here]

[Table A.15 here]

[Table A.16 here]

[Table A.17 here]

[Table A.18 here]

E.4 Education-Periodical Evidence and Robustness

Table A.19 reports the periodical evidence that supports the administrative stocks in the main text. Vacancy advertisements increase by about 0.85 events per 1,000 inhabitants for a one-percentage-point larger suffrage expansion. Confessional advertisements increase by about 0.89, while the public estimate is -0.05 and imprecise, so there is no evidence of an absolute public contraction in advertised staffing demand; total observed activity rises because the confessional increase dominates. Confessional school openings rise by about 0.06 events per 1,000, and appointments are positive but imprecisely estimated. Among municipalities for which the corpus records at least one advertisement, the confessional

share of observed advertisements rises by about 5.5 percentage points. Because these flows read a school's allegiance where the provincial education tables cannot, they add the confessional identification that the administrative panel lacks, and they point the same way.

[Table [A.19](#) here]

The corpus's central measurement problem is source bias, which is why these estimates support rather than carry the argument. The corpus is not a census of vacancies: its periodicals differ in denominational orientation, geographic reach, years of coverage, and propensity to print vacancy advertisements, so the estimates may combine changes in educational activity with differences in what the included titles capture.¹⁹ We therefore treat the composition estimate as secondary, since the share is undefined for municipalities with no recorded advertisement: it omits 166 municipalities and makes sample membership depend on what the periodicals capture as well as on actual activity. The unconditional net rate addresses that selection problem, because it is measured over the full source frame and retains municipalities with no classified activity; the confessional-minus-public contrast is 1.04 events per 1,000 in that construction.

A series of alternative constructions probes source bias more directly. The confessional-minus-public result remains positive when the general newspaper *De Tijd* is excluded, when the corpus is restricted to the pre-specified family of titles observed throughout the window, when allegiance must be stated in the advertisement rather than inferred from the title, under worst-case reallocations of uncertain allegiance, and after deleting any single source or province. For total observed activity, a further construction first expresses each municipality's vacancy rate relative to the distribution for the same periodical and year and then gives every active source-year equal weight, which prevents prolific titles or unusually high-volume years from dominating the aggregate; the resulting index remains positive. The four primary outcomes also survive Holm adjustment across the family. These checks show at the same time that the magnitude is predominantly Protestant and falls when *De School met den Bijbel* is omitted, even though the net contrast remains significantly positive.

Table [A.20](#) reports the distribution and coverage of the deduplicated 1903–1909 outcomes. Zeros in event rates mean that no qualifying signal was observed for a municipality in the processed national source-year coverage; they do not prove that no unadvertised hiring occurred. Conditional shares and offered terms are defined only where a qualifying advertisement supplies the relevant denominator.

¹⁹Table [A.24](#) documents these differences title by title, reporting years of coverage, issues and pages read, records extracted, the share linked to a municipality, the share whose affiliation is stated in the advertisement itself, and the share of each title's records falling in its largest province.

[Table A.20 here]

Affiliation can be explicit in an advertisement or inferred from a periodical whose denominational orientation is known. Table A.21 compares the preferred source-default classification with a text-explicit restriction. Confessional vacancy and opening estimates remain similar. Because shares condition on having classified activity, the source-selection audit instead makes unconditional confessional and public levels and their difference primary. Table A.22 compares the full, specialist, balanced, and text-explicit frames and the source-volume constructions. Table A.23 reallocates unknown and silent mixed-source events. The net-rate confidence interval remains above zero in every construction under HC1 inference.

[Table A.21 here]

[Table A.22 here]

[Table A.23 here]

A complementary specification stacks municipality by source-year cells, absorbs source-year and province fixed effects, and clusters at the municipality. Table A.25 reports it alongside the collapsed cross-section it reproduces. Because the franchise expansion, the class rank, the controls and the province effects are all measured at the municipality, and because each active source-year covers the same municipalities, stacking replicates the cross-sectional comparison rather than adding independent variation. The stacked coefficient multiplied by the number of active source-years returns the collapsed estimate exactly. The specification is therefore informative about inference rather than about magnitude. Treating the stacked cells as independent observations shrinks the standard error by more than half and inflates the conventional first-stage statistic by nearly two orders of magnitude, while clustering at the municipality restores the cross-sectional standard error. Replacing the additive fixed effects with a full province by source-year interaction, which absorbs the regional advertising market of each title in each year, leaves the estimates unchanged.

[Table A.25 here]

Source-specific estimates and a graphical comparison appear in Table A.26 and Figure A.5. Those give a separate column to the nine titles that carry enough records to support one; the two lowest-volume titles, *School en Leven* and *De Volksonderwijzer*, still contribute to every pooled outcome and are examined through the leave-one-source-out diagnostics. Leave-one-source-out and leave-one-province-out estimates appear in Figures A.6 and A.7. Their signs are stable, but the source-specific estimates and the attenuation after

omitting *De School met den Bijbel* show that the economic magnitude is Protestant-led and source-dependent. Table A.24 records coverage, coupling, allegiance provenance, and geographic concentration. Table A.27 varies the post-reform window; the annual movement is treated as a timing diagnostic rather than a dynamic causal design.

[Table A.26 here]

[Figure A.5 here]

[Table A.27 here]

Alternative event-spell deduplication and use of 1892 rather than 1896 population as the rate denominator do not overturn the qualitative confessional level pattern. Table A.28 collects employment composition, offered-salary quantiles, salary-reporting incidence, and housing terms. These outcomes are exploratory. In particular, conditional salary coefficients cannot be interpreted as wages because treatment changes the reporting margin.

[Table A.28 here]

E.5 Unie Collection Construction and Mechanism Results

The Unie material consists of annual Protestant collection reports printed in *De Standaard*. The completed source pipeline re-reads 543 articles from their printed page images rather than relying on the newspaper OCR, classifies each line, retains local committee remittances, and links historical place names to the 1896 municipality spine. It extracts 14,497 committee rows, of which 14,148 (97.6 percent) couple to 626 municipalities. The resulting balanced panel contains 31,360 rows: 1,120 municipalities over 28 collection seasons from 1890 through 1917. A committee covering several villages contributes presence to every named municipality, while its receipts remain credited to the committee seat.

Source completeness varies by season. We therefore weight period means by the share of numbered lists observed and report a restriction to seasons with at least 50 percent coverage. The printed transport arithmetic closes exactly for 74 of 126 testable adjacent-list pairs. Nineteen failures are below one guilder and the median discrepancy is 3.50 guilders; the largest failures occur in near-opposite adjacent pairs characteristic of one misread transport header. The presence outcomes do not depend on those amounts. Receipts enter as the inverse hyperbolic sine of guilders per 1,000 inhabitants, and explicitly unreadable amounts remain missing. Until the flagged headers receive their correction pass, receipt estimates are less secure than committee-presence estimates.

Table A.29 implements the mechanism test. It reports conditional pre-period levels, a coverage-weighted linear pre-trend with municipality and season fixed effects and

province-specific trends, the separate 1895–1896 change, and the coverage-weighted post-minus-pre IV. The change models condition on the paper’s predetermined education controls, province fixed effects, and extensive and intensive Protestant-school strength from the 1896 Unie census. The cross-sectional first-stage F -statistic is 28.7. Neither the pre-period collection levels nor the linear pre-trends vary significantly with predicted suffrage expansion. The separate change between the final two pre-reform seasons is positive for all three measures: a one-percentage-point larger predicted shock is associated with 5.81 percentage points more committee coverage, 4.68 points more seat presence, and 0.230 higher transformed receipts over that pair of seasons. That single-pair movement is reported for completeness and is not used to adjudicate the mechanism, since a two-season difference is a noisy basis for inference relative to the pre-period levels and trends reported alongside it. Predicted expansion is also associated with the 1896 Protestant-school baseline, especially its intensive margin.

Against that background, the post-reform estimates are null. The preferred post-minus-pre estimates are -0.82 percentage points for committee coverage, 0.34 points for seat presence, and 0.014 for transformed receipts. None of the three is statistically distinguishable from zero at conventional levels. The high-coverage restriction is also null. Table A.30 allows the effect to differ with 1896 Protestant-school presence. The between-group differences are marginally significant, but neither within-group estimate supplies a significant positive post effect. Hence heterogeneity does not rescue the preference mechanism.

Table A.31 repeats the post-minus-pre test where the collection was a live local institution. The “A School with the Bible” collection was organized through Protestant congregations, so a reform-induced rise in the relative appeal of confessional schooling should be most visible, and the test correspondingly most powerful, where Protestants were numerous. The second column restricts the sample to municipalities in which Protestants exceeded half of the 1889 population, and the third additionally restricts the season means to well-harvested seasons; the first column reproduces the estimates of Table A.29 for reference. The conclusion is unchanged. In the Protestant-majority subsample, a one-percentage-point larger suffrage expansion moves committee coverage and seat presence by less than one percentage point each and in opposite directions, coverage downward and presence upward, and transformed receipts by about 0.04 . None of the three is statistically distinguishable from zero. The subsample is more relevant to the channel but smaller, so it is less precisely estimated than the full sample. Two of its estimates, seat presence and receipts, move in the direction the preference interpretation requires, but both are imprecise and both reverse sign once attention is limited to the seasons whose lists were substantially recovered. The instrument remains strong in every column. What the exercise delivers is a bound rather than a bare absence: among Protestant-majority

municipalities, the data rule out post-reform increases in committee coverage larger than about two percentage points and increases in seat presence larger than about three and a half percentage points, per one-percentage-point larger suffrage expansion.

We therefore exclude a reform-induced shift in relative confessional preference, θ , as the explanation for the later private expansion. That leaves few candidates. Cheaper provision cannot by itself draw families toward private schools, and in any case the prices of teachers, buildings, and materials were market and technology parameters rather than instruments a council commanded; the salary evidence shows no uniform change in the schedules municipalities paid. The public fee fell by roughly the same proportion as spending per public place, so the share of the cost charged to parents did not detectably change either. What survives is the model's remaining margin, the real resources standing behind each public place, which no count of schools or pupils registers. The administrative panel narrows that margin further by ruling out its one directly measured component: regular public teachers per pupil do not fall, and the interval around that estimate excludes a substantial decline. The reduction therefore fell on what a public place contained besides its regular teaching establishment, and none of the sources assembled here measures those inputs. The inference is that austerity reduced what a public place delivered rather than achieving the same schooling for less; it is not a direct observation of the reduced input.

[Table [A.29](#) here]

[Table [A.30](#) here]

[Table [A.31](#) here]

F Appendix Tables and Figures

The appendix exhibits below appear in their order of citation. Standalone tables superseded by the consolidated exhibits, raw correlation tables and plots, legacy descriptive suffrage figures, the first-stage scatter that duplicates Table [2](#), alternative level transformations, and redundant descriptive tables are deliberately excluded.

Figure A.1: A Municipal Expenditure Sheet as Printed: Groningen, 1903

1903. 14							15 BIJLAGE M.													
Vulnummer.	GEMEENTEN.	Bestuur der gemeente.		Inning der belastingen.	Politie.	Brandweer.	Inrichtingen van nijverheid. 1)													
		Naleelinge sloten van vorige jaren.	Jaarweden van Burgemeester, Wethouders, Secretaris, Ontvanger, presentiegeld, jaarweden van ambtenaren en bedienden aan Secretarie en Ontvangskantoor, schijfrenten en -schotten, drakkosten, kosten van Gemeentebank, voor en licht, aankoop van boeken, vrachten, zegels, advertentiën, reiskosten.				Uitgaven der gemeenten voor:													
							Gas. Electri. Water. Telo. Tram. Slacht-huis.													
							8	9	10	11	12	13	14	15	16	17				
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18			
1	Groningen . . .	—	70512.165	3113.975	7266.34	87306.73	11423.32	4518.19	8631.40	10519.68	350103.475	6083.33 ^a	46023.50	5589.44	—	—	33744.27	1		
2	Adorp . . .	—	1993.62	197.095	87.85	569.215	101.805	6.545	216.62	830.18	—	—	—	—	—	—	—	2		
3	Adard . . .	—	3013.995	171.745	509.70	104.25	211.11	4.615	42.50	7830.175	—	—	—	—	—	—	—	3		
4	Appingedam . . .	—	4652.015	425.95	325.395	1465.50	534.815	838.145	19.55	209.58	—	—	—	—	—	—	—	4		
5	Bido . . .	—	2544.475	375.135	102.865	435.025	192.915	12.595	25.25	3700.375	—	—	—	—	—	—	—	5		
6	Bedum . . .	—	3710.40	371.05	56.875	1189.95	60.20	487.995	21.345	53.—	—	—	—	—	—	—	—	6		
7	Beerta . . .	—	2978.185	206.75	69.675	1067.—	196.60	—	10.24	18.—	—	—	—	—	—	—	—	7		
8	Bellingwolde . . .	—	3696.065	54.41	73.965	560.50	102.615	—	11.70	129.95	—	—	—	—	250.— ^δ	—	—	8		
9	Bierum . . .	—	2814.325	132.115	230.065	893.275	516.25	—	14.88	23.50	—	—	—	—	—	—	—	9		
10	Delfzijl . . .	—	5563.58	344.675	60.75	1416.52	309.39	898.285	29.85	34.11	—	—	—	—	—	—	—	10		
11	Ennum . . .	—	2721.745	201.55	92.46	585.—	5.—	22.50	13.67	97.55	—	—	—	—	—	—	—	11		
12	Edinge . . .	—	2035.305	2035.305	37.85	385.75	13.35	180.015	4.065	10.—	—	—	—	—	—	—	—	12		
13	Finsterwolde . . .	—	2625.785	89.71	516.635	100.45	—	9.20	—	15499.90	—	—	—	—	—	—	—	13		
14	Grijpskerk . . .	—	2899.73	204.765	160.76	467.25	14.20	622.945	7.595	65.—	—	—	—	—	—	—	—	14		
15	Grootegast . . .	—	3094.695	285.94	115.94	602.74	25.60	55.—	12.30	60.—	—	—	—	—	—	—	—	15		
16	Haren . . .	—	3284.09	315.95	100.505	978.245	486.39	81.155	23.645	20.55	—	—	—	—	500.— ^δ	—	—	16		
17	Hoogezand . . .	—	5693.865	563.37	356.245	1780.20	619.03	1301.945	16.855	679.80	—	—	—	—	—	—	—	17		
18	Hoogkerk . . .	55.465	2121.215	128.83	115.40	460.—	7.95	211.925	4.92	36.—	—	—	—	—	—	—	—	18		
19	Kanamar . . .	—	2050.015	2050.015	430.—	86.64	430.—	147.74	9.44	10.—	—	—	—	—	—	—	—	19		
20	Kloosterburen . . .	—	2380.17	86.48	43.565	455.—	29.—	—	9.725	278.72	—	—	—	—	—	—	—	20		
21	Leek . . .	—	4357.92	230.025	215.69	652.04	9.50	133.735	12.285	120.—	—	—	—	—	—	—	—	21		
22	Leens . . .	—	3328.76	320.73	44.98	621.885	66.—	92.98	19.145	10.—	—	—	—	—	—	—	—	22		
23	Loppersum . . .	—	2454.345	261.915	70.995	351.75	33.295	490.705	11.93	56.40	—	—	—	—	—	—	—	23		
24	Marum . . .	—	3165.21	353.47	114.64	478.55	—	15.—	10.14	10.—	—	—	—	—	—	—	—	24		
25	Meeden . . .	—	2051.97	95.025	85.205	769.—	33.35	312.84	6.295	19.50	—	—	—	—	—	—	—	25		
26	Middelstum . . .	—	2287.29	96.885	56.905	455.—	89.—	179.605	9.98	10.—	—	—	—	—	—	—	—	26		
27	Midwolda . . .	—	3507.375	260.265	96.85	1178.45	189.96	20.25	13.92	358.73	—	—	—	—	—	—	—	27		
28	Muntendam . . .	—	2873.685	156.125	160.88	530.—	69.54	999.06	9.76	1866.265	—	—	—	—	—	—	—	28		
29	Nieuwe-Pekela . . .	—	4208.615	226.58	204.81	1440.20	69.375	539.65	311.29	50.—	—	—	—	—	—	—	—	29		
30	Nieuwe-Schans . . .	—	1954.805	69.475	440.89	25.—	202.32	3.94	57.46	764.81	—	—	—	—	—	—	—	30		
31	Nieuwolda . . .	—	2317.08	201.39	144.16	458.55	144.62	398.43	6.88	40.—	—	—	—	—	—	—	—	31		
32	Noordbroek . . .	—	2584.955	221.355	76.625	1133.14	42.70	383.26	3.07	13.815	—	—	—	—	—	—	—	32		
33	Noorddijk . . .	—	1899.28	79.65	44.865	405.—	25.—	32.00	7.965	25.—	—	—	—	—	—	—	—	33		
34	Oldhove . . .	—	2690.97	113.305	155.37	498.135	15.—	102.46	5.695	21.98	—	—	—	—	—	—	—	34		
35	Oldemark . . .	—	2116.72	189.—	68.67	489.445	—	67.445	3.015	38.97	—	—	—	—	—	—	—	35		
36	Onstwedde . . .	—	5472.35	873.70	298.23	1694.34	1020.73	1540.755	27.98	—	—	—	—	—	—	—	—	36		
37	Oude-Pekela . . .	—	4247.38	243.79	135.11	4357.93	67.77	534.05	440.665	33.11	—	—	—	—	—	—	—	37		
38	Suppemeer . . .	—	5108.505	242.505	351.655	1472.37	335.525	2766.04	6.61	454.55	—	—	—	—	—	—	—	38		
39	Scheemda . . .	—	3407.72	346.21	376.515	1552.865	69.695	848.065	17.865	60.—	—	—	—	—	—	—	—	39		
40	Slochteren . . .	—	5463.04	616.975	446.91	2359.60	17.50	—	18.065	—	—	—	—	—	—	—	—	40		
41	Sterdum . . .	—	2233.805	181.005	77.88	630.61	203.33	197.97	9.11	95.70	—	—	—	—	—	—	—	41		
42	Ten Boer . . .	—	3969.805	216.10	102.12	778.955	1.35	19.80	136.055	4079.135	—	—	—	—	—	—	—	42		
43	Termonden . . .	—	2630.425	145.30	165.67	913.60	218.64	98.245	10.48	16.—	—	—	—	—	—	—	—	43		
44	Uithuizen . . .	—	2958.94	333.30	68.88	563.19	40.—	559.25	16.27	82.10	—	—	—	—	—	—	—	44		
45	Uithuizermeeden . . .	—	2550.67	306.75	113.445	932.885	191.86	180.025	17.735	—	—	—	—	—	—	—	—	45		
46	Utrum . . .	—	2768.195	204.08	46.15	525.—	22.—	347.755	17.46	90.—	—	—	—	—	—	—	—	46		
47	Usqueet . . .	—	2256.53	121.71	41.23	497.40	16.50	120.72	8.58	10.—	—	—	—	—	—	—	—	47		
48	Veendam . . .	—	8108.91	358.91	668.99	5141.895	885.32	2424.77	800.—	35.46	—	—	—	—	—	—	—	48		
49	Vogtwaldd . . .	—	3847.39	339.75	119.065	987.50	25.45	110.—	16.56	1060.65 ^a	—	—	—	—	—	—	—	49		
50	Warffum . . .	—	2504.985	117.37	25.76	505.—	38.64	330.175	9.95	121.65	—	—	—	—	—	—	—	50		
51	Wedde . . .	—	2841.38	65.35	107.695	1093.73	282.55	839.23	7.21	28.02	—	—	—	—	—	—	—	51		
52	Wildervank . . .	—	5477.615	397.21	176.565	2671.265	164.775	1492.74	711.09	31.50	—	—	—	—	—	—	—	52		
53	Wintholten . . .	—	11790.285	226.06	1510.995	4916.91	730.755	5775.265	359.13	185.08	2840.— ^a	—	143051.86 ^a	—	250.— ^δ	—	—	53		
54	Winsum . . .	—	2303.30	221.40	91.50	603.90	73.835	238.48	11.21	24.60	—	—	—	—	—	—	—	54		
55	't Zandt . . .	—	2504.345	316.20	82.465	945.—	9.45	232.95	13.17	217.65	—	—	—	—	—	—	—	55		
56	Zuidbroek . . .	—	2873.845	119.39	26.805	1471.725	269.595	841.10	8.24	12.55	—	—	—	—	—	—	—	56		
57	Zuidhorn . . .	—	2691.475	269.80	91.—	443.—	55.47	353.68	6.71	19.81	—	—	—	—	—	—	—	57		
Totaal		55.465	260092.115	16091.79	16853.73	146969.95	20232.77	74465.275	11880.565	15066.85	657467.65 ^a	8923.33 ^a	46023.50	148241.30	1000.—	33744.27				

1) a. Exploitatie in eigen beheer. δ. Idem bij wijze van concessie.

1) a. Exploitatie in eigen beheer. δ. Iden bij wijze van concessie.

Notes: One opening of part B, *Werkelijke uitgaven*, of the appendix on the municipal accounts in the provincial report of Groningen for 1903. Rows are the fifty-seven municipalities of the province. Columns are numbered spending headings grouped under printed titles: municipal government, tax collection, police, fire service, street lighting, the civic guard, public health, public works, and municipal trading enterprises. Amounts are in guilders and cents, a dash records nothing spent under the heading, and the last row of each column is the provincial total.

Figure A.2: Detail: The Education Columns of the Expenditure Sheet

Volnummer.	GEMEENTEN.	Inrichtingen van nijverheid. 1]			Onderwijs, Kunst en Wetenschap.				
		Uitgaven der gemeenten voor:			Hooger Onderwijs. Jaarwedden, toelagen, vergoeding voor wo- ning, pensioensbijdra- gen, terugbetaling van te veel genoten subsi- die, schoolgebouwen, -meubelen, -behoefte, verwarming en verlich- ting, prijsuitdeelingen, normaallessen, exa- mens, toezicht, sub- sidiën. a] Gymnasium. b] Universiteit.	Middelbaar Onderwijs, als kol. 21.	Lager Onderwijs, als kol. 21 benevens bijdragen aan andere gemeenten.	Andere inrichtingen van onderwijs [bewaar-, brei-, muziek-, zang-, rij- en zwem- scholen, vroedvrou- wenschool].	Verschill. uitgaven te bevordering van kunst en wetenschap als voor musea, bibli- otheken, schouwbur- gen, volksfeesten en muziek- korpsen.
		Vuilnis- verzameling.	Leen- bank.	Gemeente- lijke publiciteit.					
		18	19	20	21	22	23	24	25
1	Groningen .	95906.91 ⁵	—	250.— a	{ 41376.77 a }	57037.11 ⁵	370393.88	48690.84 ⁵	10678.30
2	Adorp . . .	—	—	—	{ 500.— b }	—	2970.63	—	—
3	Aduard . . .	—	—	—	—	—	5772.18 ⁵	—	22.—
4	Appingedam .	1419.72 a	—	—	—	—	15121.37 ⁵	280.—	40.60
5	Baño . . .	—	—	—	—	—	8539.47	—	5.—
6	Bedum . . .	263.80 ⁵ a	—	—	—	—	10602.23 ⁵	—	15.—
7	Beerta . . .	—	—	—	—	50.—	19969.81 ⁵	—	—
8	Bellingwolde .	—	—	—	—	25.—	32104.12	—	—
9	Bierum . . .	—	—	—	—	—	11395.80 ⁵	—	20.—
10	Delfzijl . . .	2083.20 ⁵ a	—	—	—	6785.—	51566.33	150.—	31.02 ⁵
11	Eenrum . . .	—	—	—	—	—	10896.45 ⁵	—	13.—
12	Ezinge . . .	—	—	—	—	—	5909.80 ⁵	—	4.50
13	Finsterwolde .	—	—	—	—	50.—	13409.68 ⁵	—	45.—
14	Grijpskerk . .	—	—	—	—	—	11202.54	—	3.—
15	Grootegast . .	—	—	—	—	—	12779.14 ⁵	—	13.22 ⁵
16	Haren . . .	—	—	—	—	—	16399.15	—	43.16
17	Harsum . . .	—	—	—	—	—	—	—	—

Notes: Enlargement of the education block of the same sheet, from the following opening. The group headed *Onderwijs, Kunst en Wetenschap* separates spending on higher education, on secondary education, on primary education (*Lager Onderwijs*, column 23), on other teaching institutions, and on the promotion of arts and science. Column 23 supplies the municipal primary-education spending used in the paper. The preceding columns record municipal trading enterprises, here refuse collection, a pawnbank, and municipal publicity.

Table A.1: Public and Private Primary Schooling in 1900: Descriptive Statistics

	N	Mean	SD	Min	Median	Max
Public-School Pupils per Capita	1056	0.22	0.11	0.01	0.21	1.34
Public Schools per 1,000	1056	1.98	1.24	0.17	1.72	8.70
Public-School Pupils per Class	1056	22.63	12.16	1.00	20.12	95.33
Public-School Fee (guilders)	1052	6.31	5.71	0.80	4.80	100.00
Public-School Attendance Rate	1035	0.88	0.08	0.47	0.90	1.00
Private-School Pupil Share	1068	0.22	0.27	0.00	0.09	1.00
Any Private School	1068	0.52	0.50	0.00	1.00	1.00
Private-School Pupils per Class	548	23.68	14.40	3.00	20.50	129.33
Private-School Fee (guilders)	538	13.05	15.52	1.20	9.99	150.00
Private-School Attendance Rate	539	0.90	0.10	0.07	0.92	1.00

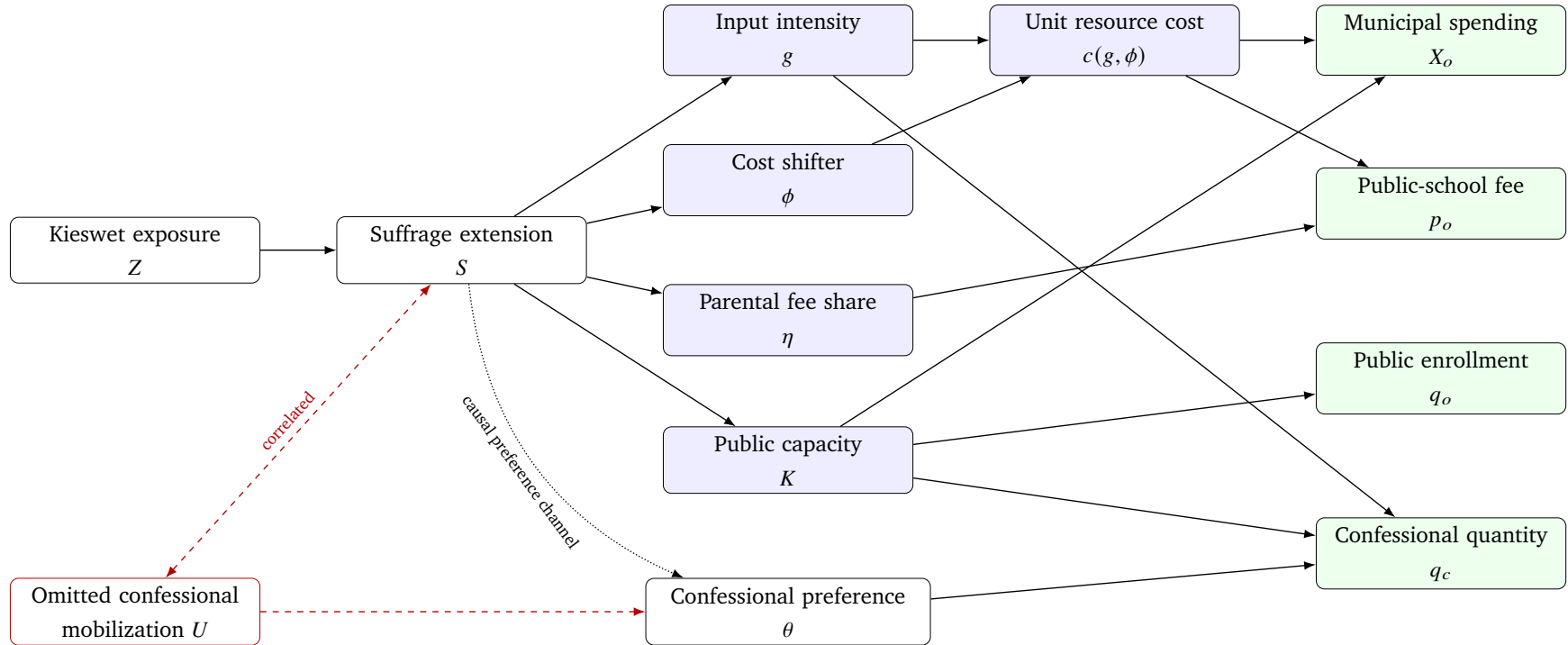
Source: statistical appendix of the national Verslag van den staat van het onderwijs, data year 1900. Per-capita denominators use male population from HDNG.

Table A.2: The Dutch Municipal Franchise before and under the 1896 Electoral Law

Year	Legal Basis	Voting Age	Principal Eligibility Rule	Important Qualification
1848	Provisional electoral regulation	23	Payment of a municipality-specific amount in direct national taxes	Municipal councils compiled lists for direct elections.
1850	Electoral Law of 1850	23	Assessment in the land, personal, or patent taxes, subject to local thresholds	National tax surcharges counted; municipal and provincial surcharges did not.
1887	Constitutional revision and transitional rules	23	Observable “capability and social well-being,” implemented mainly through tax and housing qualifications	Recent recipients of public relief and residents with unpaid qualifying taxes were excluded.
1896	Van Houten Electoral Law	25	Alternative tax, rent, wage, savings, and examination routes, with income and rent thresholds varying by municipal class	Self-declaration expanded for the new routes; the reform remained short of universal male suffrage.

Notes: The table summarizes the principal municipal-franchise rules relevant to the paper. It omits detailed administrative exceptions. Universal male suffrage followed in 1917.

Figure A.3: Suffrage, Municipal Policy, and Education-Market Outcomes



Notes: Solid arrows are the municipal-policy channels of the baseline model: the suffrage extension moves input intensity, the cost shifter, the parental fee share, and public capacity, and these in turn determine municipal spending, the public-school fee, and enrollment in the two sectors. The dotted arrow allows the suffrage extension to change relative confessional preferences, which is an additional causal channel rather than a threat to identification. The dashed double-headed edge records that omitted confessional mobilization may be correlated with the measured suffrage extension without causing it, and the dashed single-headed arrow is its effect on preferences. The Kieswet exposure identifies the suffrage effect only if it is conditionally independent of that omitted mobilization.

Table A.3: IV Estimates: Effect of 1896–1897 Suffrage Shock on Suffrage Growth 1897–1903

Specification:	Without Controls		With Controls	
	No Province FE	Province FE	No Province FE	Province FE
	(1)	(2)	(3)	(4)
Suffrage Expansion	-0.354*** (0.080)	-0.348*** (0.115)	-0.084 (0.153)	0.225 (0.243)
Log Population (1896)			0.010*** (0.002)	0.011*** (0.002)
Tax Revenue per Capita (1889)			0.002 (0.002)	0.005 (0.004)
Log Surface Area (ha, 1896)			-0.006*** (0.002)	-0.005** (0.002)
Demographic Composition			-0.002 (0.002)	0.000 (0.003)
Religious Composition			0.002* (0.001)	0.004*** (0.002)
Pop. Growth Rate (1880–1889)			0.040** (0.016)	0.051*** (0.019)
R2 Adj.	0.039	0.051	0.076	0.101
R2 Within Adj.		0.019		0.068
Num.Obs.	866	866	835	835
First-Stage F-Stat	207	110	36.9	20.3
Province FE	No	Yes	No	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Balance Regressions: Pre-treatment Predictors of Class Rank

	No controls		Full controls	
	(1)	(2)	(3)	(4)
Pop. growth rate (1880–1889)	-1.042 (0.579)	-0.860 (0.487)	-1.092** (0.424)	-0.886** (0.385)
Eligible Voters per Capita (1896)	-1.795* (0.822)	-0.377 (0.837)	-0.368 (0.781)	-0.140 (1.071)
School truancy rate, ages 6–12 (1889)	-0.008 (0.005)	-0.009* (0.005)	-0.008 (0.005)	-0.007 (0.005)
Petition Signatories per Capita (1878)	0.195 (0.233)	0.237* (0.120)	0.256 (0.209)	0.227 (0.139)
R2 Adj.	0.455	0.540	0.624	0.674
R2 Within Adj.		0.402		0.576
Num.Obs.	1073	1073	1073	1073
Full controls	No	No	Yes	Yes
Province FE	No	Yes	No	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Dependent variable is raw municipal class rank (1–9) under the 1896 Kieswet. Models (1)–(2) include all raw pre-treatment characteristics. Models (3)–(4) add the standard controls (log population, log surface area, tax revenue per capita, demographic composition, religious composition); the raw religion and demography variables are replaced by their PCA components to avoid collinearity. Control coefficients are omitted from display. Province-clustered standard errors in parentheses.

Table A.5: Composition of Municipal Expenditure and Revenue, 1903–1907

Category	Mean (%)	S.D. (%)	Municipalities
<i>Panel A. Expenditure</i>			
Education, arts and science	32.91	11.44	1114
Municipal government and administration	16.90	7.69	1114
Debt service, pensions and other outlays	13.72	8.84	1111
Public works and infrastructure	10.20	6.70	1112
Poor relief	9.89	6.89	1112
Police, fire, lighting and public health	9.24	4.72	1114
Municipal trading enterprises	4.96	8.35	683
Property upkeep, subsidies and church affairs	3.35	3.08	1114
Tax collection	1.01	0.93	1114
<i>Panel B. Revenue</i>			
Local direct taxation	20.04	8.39	1113
Statutory state grant to municipalities	20.01	7.43	1114
Other state and provincial contributions	16.19	7.64	1107
Surcharges on national taxes	10.60	5.75	1108
Loans and proceeds of asset sales	9.22	8.09	970
Carry-over surplus from previous years	8.59	6.34	1111
Receipts from municipal trading enterprises	7.65	11.41	278
Income from municipal property	7.18	9.45	1094
Road, sewer, harbor and similar charges	3.12	4.59	1113
Miscellaneous and incidental receipts	2.66	3.84	1023
School fees	2.24	1.51	1103

Notes: Composition of the municipal accounts over the financial years 1903–1907, for the 1,121 municipalities of the estimation sample. A heading's share is its amount divided by the same municipality-year's total expenditure (Panel A) or total revenue (Panel B), averaged over the five years within a municipality; the table reports the mean and standard deviation of that municipal average across municipalities, in percent, with the number of municipalities behind each row. A heading a municipality never reports counts as missing rather than as zero, so rows average over different sets of municipalities and the mean shares in a panel need not sum to 100 percent. Appendix E.2 describes the construction of the shares, the definition of the individual headings, and how the financing items enter the revenue denominator.

Table A.6: IV Estimates: Effect of 1896–1897 Suffrage Shock on Log Avg. Municipal Expenditure Components per Capita 1903–1907 (Full Sample, Province FE)

	Governance	Tax Compliance	Education	Poor Aid	Infrastructure	Public Services	Utilities	Property Subsidies	Finance
Suffrage Expansion	-5.512*** (1.808)	-15.335*** (4.611)	-8.117*** (3.073)	-4.198 (3.449)	-7.123* (4.234)	-14.648*** (4.100)	-27.177* (13.990)	1.553 (4.097)	-17.363*** (5.855)
Log Population (1896)	-0.227*** (0.024)	0.088 (0.067)	0.012 (0.047)	0.052 (0.041)	0.375*** (0.050)	0.098 (0.060)	0.927*** (0.154)	0.154*** (0.048)	0.427*** (0.076)
Tax Revenue per Capita (1889)	0.003 (0.027)	-0.061 (0.073)	-0.081* (0.048)	-0.011 (0.047)	0.047 (0.059)	-0.024 (0.069)	-0.051 (0.212)	0.116** (0.053)	-0.124 (0.095)
Log Surface Area (ha, 1896)	-0.097*** (0.025)	-0.187** (0.074)	-0.073* (0.042)	-0.091*** (0.032)	-0.219*** (0.047)	-0.191*** (0.068)	-0.565*** (0.163)	-0.201*** (0.039)	-0.268*** (0.079)
Demographic Composition	-0.044 (0.027)	-0.141* (0.077)	-0.088* (0.046)	-0.001 (0.039)	-0.101* (0.054)	-0.150** (0.071)	-0.465** (0.231)	0.028 (0.044)	-0.188** (0.082)
Religious Composition	-0.005 (0.015)	-0.038 (0.041)	-0.030 (0.026)	-0.044 (0.028)	-0.030 (0.038)	-0.006 (0.038)	0.077 (0.170)	-0.027 (0.035)	-0.018 (0.055)
Pop. Growth Rate (1880–1889)	-0.374*** (0.143)	-0.482 (0.349)	-0.742*** (0.240)	-0.643** (0.276)	-0.307 (0.317)	-0.585* (0.315)	-0.415 (1.068)	-0.390 (0.289)	-0.886** (0.449)
R2 Adj.	0.648	0.394	0.191	0.349	0.294	0.397	0.406	0.199	0.203
R2 Within Adj.	0.576	0.184	0.042	0.029	0.159	0.293	0.200	0.055	0.167
Num.Obs.	1067	1067	1067	1065	1065	1067	649	1068	1064
First-Stage F-Stat	17.3	17.3	17.3	17.6	17.2	17.3	9.9	17.6	17.1
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* p < 0.1, ** p < 0.05, *** p < 0.01

Table A.7: IV Estimates: Effect of 1896–1897 Suffrage Shock on Log Avg. Municipal Revenue Components per Capita 1903–1907 (Full Sample, Province FE)

	Balance	Possessions	Fees	School Fees	Utilities	Tax Opcenten	Tax Local	State Grants	Transfers	Incidental	Capital Loans
Suffrage Expansion	-9.317** (4.240)	-9.677 (6.955)	-6.749 (4.900)	-15.603*** (4.715)	16.224 (26.021)	-10.486*** (3.074)	-5.495** (2.712)	-3.453** (1.475)	-6.332** (2.903)	-42.019*** (13.207)	-34.649*** (13.090)
Log Population (1896)	-0.009 (0.054)	0.170** (0.081)	0.532*** (0.052)	0.201*** (0.067)	1.344*** (0.178)	0.056 (0.044)	0.182*** (0.033)	0.021 (0.022)	-0.108*** (0.037)	0.314* (0.187)	0.330** (0.163)
Tax Revenue per Capita (1889)	0.014 (0.059)	0.013 (0.100)	-0.014 (0.069)	-0.143* (0.077)	0.641 (0.518)	0.040 (0.051)	-0.014 (0.039)	0.089*** (0.023)	-0.076* (0.044)	-0.412* (0.239)	-0.236 (0.173)
Log Surface Area (ha, 1896)	-0.157*** (0.053)	-0.416*** (0.075)	-0.450*** (0.053)	-0.199*** (0.072)	-0.701*** (0.183)	-0.056 (0.052)	-0.138*** (0.033)	-0.039* (0.023)	-0.066* (0.034)	-0.293 (0.193)	-0.148 (0.146)
Demographic Composition	-0.062 (0.053)	-0.021 (0.082)	-0.049 (0.052)	-0.160** (0.073)	0.125 (0.389)	-0.122** (0.055)	-0.092** (0.036)	-0.044* (0.025)	-0.061 (0.037)	-0.395* (0.204)	-0.150 (0.098)
Religious Composition	0.057* (0.034)	-0.000 (0.066)	-0.190*** (0.046)	-0.102** (0.043)	-0.202 (0.283)	-0.015 (0.028)	-0.084*** (0.023)	-0.032*** (0.011)	-0.039 (0.026)	0.092 (0.109)	0.149 (0.104)
Pop. Growth Rate (1880–1889)	-0.633* (0.345)	-1.044** (0.519)	-0.458 (0.376)	-0.878** (0.392)	0.819 (1.716)	-0.352 (0.264)	-0.297 (0.244)	-0.314*** (0.116)	-0.645*** (0.248)	-2.689** (1.077)	-1.257 (0.835)
R2 Adj.	0.179	0.161	0.285	0.243	0.447	0.544	0.457	0.646	0.189	0.232	0.116
R2 Within Adj.	0.096	0.082	0.200	0.178	0.375	0.282	0.142	0.360	0.068	0.130	0.102
Num.Obs.	1064	1049	1066	1057	268	1060	1066	1067	1061	980	933
First-Stage F-Stat	17.7	17.5	17.1	16.9	1.3	18.3	17.3	17.3	16.7	12.7	11.3
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* p < 0.1, ** p < 0.05, *** p < 0.01

Table A.8: Ratepayer Enfranchisement and the Absolute Municipal Budget Gap

	Province Fixed Effects (1)	Preferred Specification (2)
Suffrage Expansion	-15.779*** (3.556)	-21.718*** (7.299)
Predetermined Controls	No	Yes
Province Fixed Effects	Yes	Yes
First-Stage F-Statistic	105.2	17.3
Observations	1114	1067

Notes: The outcome is the log absolute difference between annual municipal revenue and expenditure per capita, averaged over 1903–1907. Specifications and inference follow Table 3. A negative coefficient indicates a smaller absolute budget gap. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: Administrative Education Composition across Specifications

Outcome	Specification	Estimate	S.E.	First-stage F	N
Private (Bijzondere) Pupil Share	1896-baseline ANCOVA	1.518	(0.985)	25.9	436
Private (Bijzondere) Pupil Share	1896-to-post change score	0.907	(1.073)	22.9	436
Private (Bijzondere) Pupil Share	Post level	4.071***	(1.546)	26.8	448
Private (Bijzondere) Pupil Share	1894-1895-baseline ANCOVA	1.628*	(0.929)	29.4	447
Private (Bijzondere) Pupil Share	1896 ANCOVA, three-year balanced	1.774*	(1.029)	24.8	423
Private (Bijzondere) School Share	1896-baseline ANCOVA	1.091	(0.991)	26.9	436
Private (Bijzondere) School Share	1896-to-post change score	0.314	(1.156)	22.9	436
Private (Bijzondere) School Share	Post level	3.061**	(1.421)	27.0	449
Private (Bijzondere) School Share	1894-1895-baseline ANCOVA	0.922	(0.925)	30.3	447
Private (Bijzondere) School Share	1896 ANCOVA, three-year balanced	1.308	(1.020)	26.1	425
Private (Bijzondere) Teacher Share	Post level	3.253**	(1.348)	26.1	394
Private (Bijzondere) Teacher Share	Post level, three-year balanced	3.751**	(1.494)	23.2	365

Notes: Estimates are percentage-point effects for a one-percentage-point larger suffrage expansion. The 1896-baseline ANCOVA is preferred where the Onderwijsverslag supplies a comparable pre-reform outcome. The 1894–1895 baseline is the mean of the two strictly pre-law years. Teacher counts have no comparable pre-reform baseline. Every model uses the controls and province fixed effects described in Table 6. HC1 standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: IV Estimates: Effect of the 1896–1897 Suffrage Shock on Openbaar vs. Bijzonder Primary Schooling, 1899 (Province FE, Density, Population, Income, Religion, Demography)										
	Log Public-School Pupils per Capita	Log Public Schools per Capita	Log Public-School Pupils per Class	Log Public-School Fee	Public-School Attendance Rate	Private-School Pupil Share	Any Private School	Log Private-School Pupils per Class	Log Private-School Fee	Private-School Attendance Rate
Suffrage Expansion	-0.204 (1.608)	-1.416 (2.092)	0.781 (1.747)	-7.431*** (2.667)	-0.066 (0.437)	1.645* (0.947)	3.524* (1.906)	-1.024 (2.646)	-2.399 (2.817)	-0.237 (0.295)
Log Population Density (1896)	-0.013 (0.022)	-0.168*** (0.021)	0.125*** (0.021)	0.117*** (0.041)	0.015** (0.006)	0.026** (0.012)	0.005 (0.022)	0.115*** (0.023)	0.055 (0.041)	0.010*** (0.003)
Log Population (1896)	-0.120*** (0.021)	-0.397*** (0.022)	0.227*** (0.020)	0.090*** (0.027)	-0.010* (0.006)	0.083*** (0.011)	0.290*** (0.019)	0.135*** (0.030)	0.235*** (0.041)	0.001 (0.004)
Tax Revenue per Capita (1889)	-0.048** (0.023)	-0.005 (0.027)	-0.058** (0.024)	-0.075* (0.040)	0.011** (0.005)	0.053*** (0.013)	0.083*** (0.025)	-0.048 (0.049)	0.077 (0.053)	-0.002 (0.006)
Religious Composition	-0.055*** (0.018)	-0.046*** (0.015)	-0.005 (0.017)	-0.060** (0.024)	-0.008* (0.005)	0.020** (0.009)	0.020 (0.016)	0.057** (0.027)	-0.111*** (0.035)	0.020*** (0.003)
Demographic Composition	-0.009 (0.021)	-0.024 (0.024)	0.010 (0.021)	-0.042 (0.038)	-0.002 (0.006)	0.035*** (0.012)	0.064*** (0.024)	-0.000 (0.034)	-0.035 (0.043)	-0.004 (0.004)
R2 Adj.	0.173	0.543	0.281	0.280	0.055	0.227	0.333	0.228	0.404	0.146
R2 Within Adj.	0.067	0.532	0.265	0.177	0.017	0.156	0.277	0.165	0.243	0.093
Num.Obs.	1077	1077	1074	1071	1066	1085	1085	558	547	549
First-Stage F-Stat	21.7	21.7	21.9	21.5	21	22.2	22.2	17.6	17.2	16.8
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Reform Control	No	No	No	No	No	No	No	No	No	No

* p < 0.1, ** p < 0.05, *** p < 0.01

Table A.11: IV Estimates: Openbaar vs. Bijzonder Primary Schooling, 1900, Controlling for the Pre-Reform (1894–95) Level of the Same Outcome										
	Log Public-School Pupils per Capita	Log Public Schools per Capita	Log Public-School Pupils per Class	Log Public-School Fee	Public-School Attendance Rate	Private-School Pupil Share	Any Private School	Log Private-School Pupils per Class	Log Private-School Fee	Private-School Attendance Rate
Suffrage Expansion	-1.887 (1.322)	-0.270 (0.862)	-1.039 (1.198)	-2.749 (1.687)	0.020 (0.262)	0.699 (0.689)	0.218 (1.344)	1.868 (2.659)	-4.083 (3.809)	-0.729 (0.591)
Same Outcome, Pre-Reform (1894–95)	0.800*** (0.063)	0.940*** (0.019)	0.789*** (0.037)	0.607*** (0.076)	0.151*** (0.038)	0.846*** (0.031)	0.814*** (0.025)	0.509*** (0.052)	0.448*** (0.075)	0.082 (0.062)
Log Population Density (1896)	-0.035* (0.020)	-0.033*** (0.011)	0.011 (0.019)	0.055** (0.023)	0.004 (0.003)	-0.001 (0.010)	-0.018 (0.014)	0.062** (0.029)	0.072* (0.042)	-0.001 (0.006)
Log Population (1896)	-0.015 (0.018)	-0.026** (0.010)	0.079*** (0.017)	0.028 (0.017)	0.004 (0.004)	0.013* (0.008)	0.050*** (0.013)	0.096*** (0.034)	0.161*** (0.040)	0.002 (0.008)
Tax Revenue per Capita (1889)	-0.036* (0.019)	0.004 (0.012)	-0.037** (0.017)	-0.024 (0.025)	0.006* (0.003)	0.017* (0.009)	0.008 (0.017)	0.013 (0.051)	0.002 (0.071)	-0.007 (0.011)
Religious Composition	-0.001 (0.017)	0.003 (0.008)	0.010 (0.014)	-0.033** (0.016)	0.006** (0.003)	0.004 (0.006)	0.000 (0.009)	0.033 (0.026)	-0.003 (0.037)	0.016*** (0.006)
Demographic Composition	-0.009 (0.018)	0.015 (0.010)	-0.015 (0.015)	-0.026 (0.022)	-0.002 (0.003)	0.004 (0.010)	-0.005 (0.015)	0.026 (0.038)	-0.085 (0.054)	-0.011 (0.008)
R2 Adj.	0.515	0.890	0.588	0.535	0.166	0.666	0.723	0.372	0.530	0.019
R2 Within Adj.	0.440	0.887	0.573	0.460	0.045	0.631	0.699	0.340	0.404	0.021
Num.Obs.	1055	1055	1055	1051	1032	1067	1067	493	481	484
First-Stage F-Stat	22	22	21.6	20.6	21.2	23.2	23.9	9.7	10.6	9.5
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Reform Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* p < 0.1, ** p < 0.05, *** p < 0.01

Table A.12: IV Estimates: Openbaar vs. Bijzonder Primary Schooling, 1899, Controlling for the Pre-Reform (1894–95) Level of the Same Outcome

	Log Public-School Pupils per Capita	Log Public Schools per Capita	Log Public-School Pupils per Class	Log Public-School Fee	Public-School Attendance Rate	Private-School Pupil Share	Any Private School	Log Private-School Pupils per Class	Log Private-School Fee	Private-School Attendance Rate
Suffrage Expansion	-0.008 (1.213)	0.097 (0.805)	-0.073 (1.166)	-2.045 (1.865)	0.139 (0.426)	-0.161 (0.470)	-0.329 (0.953)	-4.681 (2.855)	-0.745 (2.650)	-0.129 (0.387)
Same Outcome, Pre-Reform (1894–95)	0.807*** (0.046)	0.909*** (0.020)	0.827*** (0.030)	0.667*** (0.077)	0.306*** (0.087)	0.848*** (0.025)	0.828*** (0.023)	0.439*** (0.066)	0.461*** (0.066)	0.205*** (0.052)
Log Population Density (1896)	0.000 (0.017)	-0.024** (0.012)	0.033** (0.016)	0.065** (0.025)	0.013** (0.006)	0.000 (0.006)	-0.021** (0.010)	0.074** (0.029)	0.030 (0.034)	0.010*** (0.003)
Log Population (1896)	-0.012 (0.016)	-0.046*** (0.013)	0.067*** (0.015)	0.022 (0.017)	-0.008 (0.006)	0.012** (0.006)	0.062*** (0.011)	0.067** (0.031)	0.146*** (0.036)	0.000 (0.004)
Tax Revenue per Capita (1889)	-0.018 (0.016)	0.010 (0.011)	-0.029* (0.016)	-0.037 (0.026)	0.011** (0.005)	0.006 (0.006)	-0.001 (0.012)	-0.102** (0.051)	0.052 (0.050)	-0.001 (0.007)
Religious Composition	-0.011 (0.015)	-0.007 (0.009)	0.004 (0.013)	-0.023 (0.015)	-0.009** (0.005)	0.011** (0.005)	0.008 (0.009)	0.037 (0.031)	-0.061* (0.033)	0.019*** (0.003)
Demographic Composition	0.025 (0.016)	0.023** (0.010)	0.004 (0.014)	-0.007 (0.021)	0.001 (0.006)	0.003 (0.006)	0.001 (0.011)	-0.054 (0.040)	-0.020 (0.035)	-0.003 (0.005)
R2 Adj.	0.541	0.876	0.662	0.550	0.080	0.742	0.779	0.352	0.539	0.187
R2 Within Adj.	0.483	0.873	0.654	0.486	0.044	0.718	0.761	0.304	0.426	0.140
Num.Obs.	1074	1074	1071	1068	1062	1082	1082	513	501	494
First-Stage F-Stat	22	22.1	21.8	20.2	21.3	23.7	24.3	12.4	12.6	9.9
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Reform Control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

* p < 0.1, ** p < 0.05, *** p < 0.01

Table A.13: Education Results under Heteroskedasticity-Robust and Spatial Inference

Outcome	HC1 Inference		Conley, 25 km		Observations
	Estimate	S.E.	Estimate	S.E.	
Public-School Fee	-8.612***	(2.488)	-8.612***	(2.879)	1074
Private-School Pupil Share	2.106**	(1.067)	2.106	(1.391)	1091
Confessional Vacancy Events	0.887***	(0.301)	0.887*	(0.495)	1120
Public Vacancy Events	-0.049	(0.116)	-0.049	(0.132)	1120
Confessional School Openings	0.055**	(0.025)	0.055*	(0.030)	1120
Confessional Share of Activity	5.451***	(2.090)	5.451	(3.986)	954

Notes: The table reports the preferred specifications from Table 5 for the 1900 school returns and from Table A.19 for the 1903–1909 periodical flows. Effect units follow those tables. The Conley covariance estimator allows spatial dependence within 25 kilometers. The school-census data lack coordinates for a small number of observations; the coefficient sample is unchanged wherever spatial inference is available. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.14: Vacancy-Salary Descriptives, 1903–1909

Sector	Vacancies	Salary offers	Municipalities with salary	Reporting share	Mean offer	Median offer
Private	9446	5628	458	0.596	600.3	550.0
Public	6853	4447	790	0.649	667.0	575.0

Closed specialist-periodical frame: events touching De Tijd are excluded. Resolved affiliations; public means public, private combines Protestant, Catholic, and neutral-private events. Offers are corrected annual amounts between 300 and 2,500 guilders. Unknown affiliation is excluded. Missing salaries are not zeros.

Table A.15: Source-Frame, Annual, and Salary-Bound Robustness

Setting	Private	Stacked private minus public	Matched direct gap
Specialist periodicals; no De Tijd	1.510** (0.588)	2.122** (0.907)	2.143* (1.245)
Stricter homogeneous sources	1.532** (0.612)	2.270** (0.916)	2.394* (1.301)
Annual full corpus; year FE	0.901** (0.416)	1.784* (0.981)	1.738 (1.425)
Full corpus; 350–2,000 guilder bounds	1.535** (0.688)	1.893* (0.968)	1.651 (1.242)
Full corpus baseline	1.173* (0.601)	1.740 (1.195)	1.740 (1.194)

The specialist frame is primary. The stricter homogeneous frame additionally excludes Het Nieuwe Schoolblad, School en Leven, and De Volksonderwijzer. Annual models add year effects and municipality-clustered inference. Stacked interactions retain all available sector rows except the legacy full-corpus comparison, which preserves the original matched-sector verification. Matched gaps require both sectors in the same municipality (and year for the annual model). Standard errors are in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.16: Selection Margins in Vacancy-Salary Reporting

Sector	Margin	Estimate
Public	Any observed vacancy	-0.310 (1.384)
Public	Any quoted salary	1.373 (1.600)
Public	Salary-reporting share	2.190 (1.674)
Private	Any observed vacancy	5.063** (2.176)
Private	Any quoted salary	3.633* (2.007)
Private	Salary-reporting share	0.615 (1.790)

Full-corpus diagnostic. Separate IVs by sector. Any vacancy and any salary use the full municipality universe; reporting share is defined only among municipalities with observed vacancies. Resolved affiliations, 1903–1909, predetermined controls, province fixed effects, and HC1 inference. Standard errors are in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.17: Independent NOG and Education-Budget Checks

Comparison	Estimate
Matched vacancy/NOG municipalities	593
Offer vs. NOG minimum correlation	0.192
Offer vs. NOG maximum correlation	0.243
Budget association	0.003** (0.002)
Budget IV	0.011 (0.013)

Salary outcomes use the specialist-periodical frame without De Tijd. NOG absences are missing, not zero. The budget association uses 1903–1907 primary-education expenditure per capita. The secondary spending IV requires the stronger exclusion restriction that class rank affects offered salaries only through education spending; it is not a mediation estimate. Standard errors for the budget estimates are in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.18: Robustness of the Efficiency-versus-Quality Tests: Compensation, Affiliation, Source Frame, Weighting, and Schedule Reporting

	(1)	(2)	(3)
Panel A. Housing and total compensation			
Share of advertisements mentioning housing	-0.007 (0.011)	-0.016 (0.011)	-0.012 (0.017)
Mean standardised log salary, all advertisements	-0.006 (0.006)	0.015** (0.006)	0.021* (0.012)
Mean standardised log salary, housing-free advertisements	-0.009 (0.006)	0.015*** (0.005)	0.023* (0.012)
Mean standardised log salary, housing rate controlled	-0.006 (0.006)	0.015** (0.006)	0.020* (0.012)
Sample	public	private	private minus public
Municipalities	715–893	427–530	350–484
First-stage F	13.6–18.2	11.8–16.6	8.7–14.0
Panel B. Advertisements that state their school type			
Repeated-listing share	-0.161 (0.186)	0.020 (0.014)	0.100 (0.085)
Log advertising duration	-0.406 (0.504)	0.146** (0.063)	0.424 (0.296)
Vacancy-to-appointment match rate	0.069 (0.078)	-0.031** (0.015)	-0.051 (0.039)
Sample	public	private	private minus public
Municipalities	349	521	238
First-stage F	1.8	16.0	5.8
Panel C. Homogeneous specialist source frame			
Repeated-listing share	-0.003 (0.008)	0.019 (0.014)	0.026 (0.017)
Log advertising duration	-0.014 (0.035)	0.160** (0.067)	0.210** (0.094)
Vacancy-to-appointment match rate	0.003 (0.007)	-0.031** (0.016)	-0.030* (0.017)
Sample	public	private	private minus public
Municipalities	864	528	473
First-stage F	18.3	15.6	14.5
Panel D. Weighting by advertisement and teacher counts			
Repeated-listing share, weighted by advertisements	-0.020 (0.023)	0.029* (0.015)	0.058* (0.031)
Log advertising duration, weighted by advertisements	-0.032 (0.099)	0.157** (0.066)	0.305** (0.152)
Vacancy-to-appointment match rate, weighted	0.004 (0.015)	-0.002 (0.015)	-0.016 (0.021)
Vacancies per 100 teachers, weighted by teacher stock	5.156 (4.501)	-33.448 (39.917)	82.848 (345.527)
Sample	public	private	private minus public
Municipalities	397–893	257–530	254–484
First-stage F	6.9–7.7	1.7–13.0	0.2–12.4
Panel E. Public salary schedules: what is printed, and for whom			
Prints a salary figure at all	0.052** (0.021)	0.003 (0.020)	
Log entry salary, schedules printing entry and maximum	-0.071* (0.040)	0.045* (0.024)	
Log implied maximum, same schedules	-0.011 (0.023)	-0.011 (0.014)	
Sample	ordinary teacher	school head	
Municipalities	378–1,036	455–1,036	
First-stage F	3.4–24.5	6.3–24.5	

Checks on the four tests reported in the preceding table. The design is identical: the change in eligible voters per capita between 1896 and 1897, instrumented by the municipality's class rank under the 1896 Kieswet and scaled to a one percentage point expansion, with log population density, log population, tax revenue per capita in 1889, and the first principal components of religious and demographic composition, province fixed effects, and heteroskedasticity-robust standard errors. Column (1) is public schools, column (2) private schools, and column (3) the private-minus-public difference within municipalities observed in both sectors. Standard errors in parentheses; * p < 0.1, ** p < 0.05, *** p < 0.01.

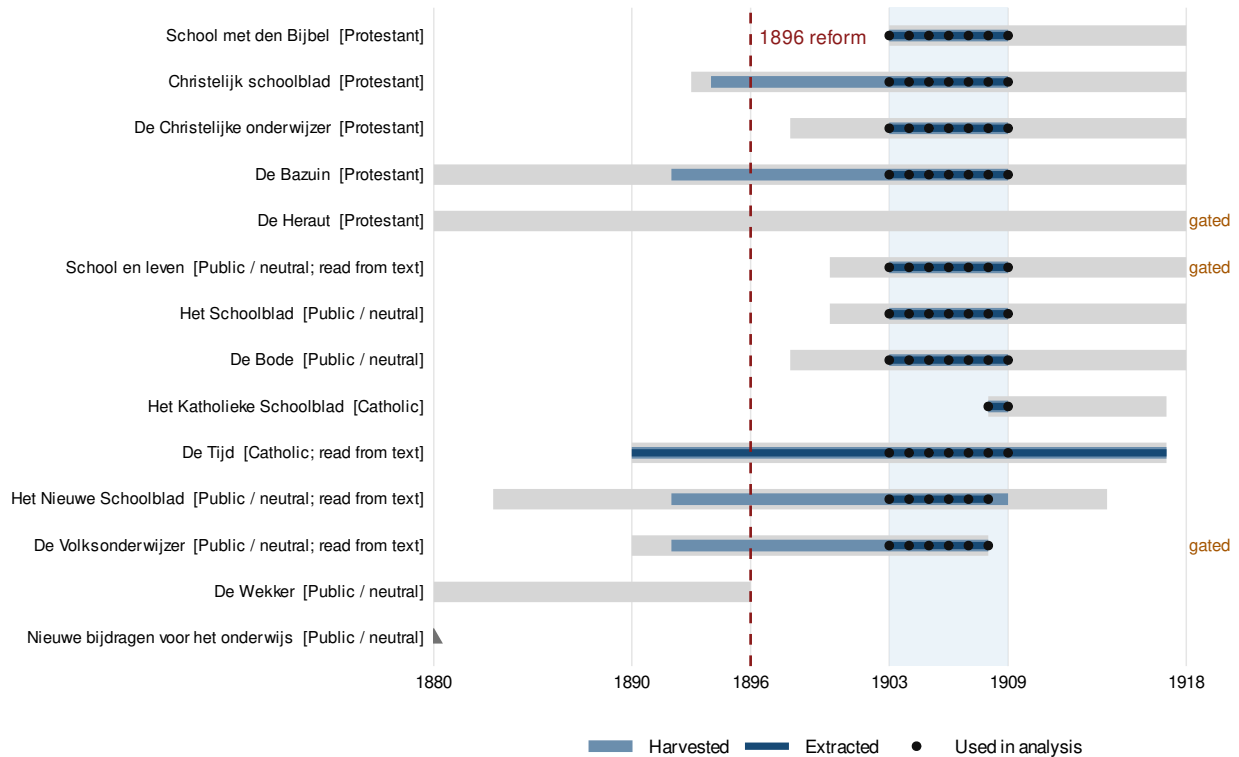
A leave-one-source-out sweep, dropping each periodical in turn, is recorded in the accompanying machine-readable output rather than shown here. The number of municipalities and the first-stage F statistic are reported once per panel, as a range where the panel's outcomes rest on different samples.

Table A.19: Education-Periodical Evidence on Subsequent Educational Activity, 1903–1909

Outcome	Estimate	S.E.	Effect Unit	First-stage F	N
All Vacancy Events	0.853***	(0.312)	Events per 1,000	23.0	1120
Confessional Vacancy Events	0.887***	(0.301)	Events per 1,000	23.0	1120
Public Vacancy Events	-0.049	(0.116)	Events per 1,000	23.0	1120
Confessional School Openings	0.055**	(0.025)	Events per 1,000	23.0	1120
Confessional Share of Activity	5.451***	(2.090)	Percentage points	16.2	954
Appointments	0.247	(0.200)	Events per 1,000	23.0	1120

Notes: Two-stage least-squares estimates for a one-percentage-point larger 1896–1897 suffrage expansion. The outcomes are cross-source-deduplicated flows from education periodicals. Vacancy events measure advertised staffing demand, openings measure school entry, and appointments measure hiring; they are not school or teacher stocks. Rate effects are events per 1,000 inhabitants using 1896 population. The confessional share is defined among municipalities with observed educational activity. All models include predetermined density, population, tax, religion, and demography controls and province fixed effects. HC1 standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.4: Publication and Processing Coverage of the Education-Periodical Corpus



Notes: Gray lines show years in which each title was published within the displayed window. Blue lines show years actually harvested and extracted; dots show the source-years that enter the pooled outcomes. The shaded band is the 1903–1909 outcome window. Triangles denote publications that ended before 1880. “Gated” sources require further verification and contribute no records. Availability is not interpreted as processed coverage.

Table A.20: Teacher-Vacancy, Appointment and School-Opening Outcomes, 1903–1909: Coverage and Distribution

Outcome	Margin	N	Coverage	Zeros	Positive	Mean	SD	Max
Vacancy Events p. 1,000	Extensive	1120	1.000	166	954	7.389	7.856	67.485
Confessional Vacancies p. 1,000	Extensive	1120	1.000	582	538	3.947	6.852	58.282
Confessional Share of Activity	Conditional	954	0.852	416	538	0.350	0.366	1.000
Confessional Openings p. 1,000	Extensive	1120	1.000	879	241	0.235	0.668	6.536
Appointments p. 1,000	Extensive	1120	1.000	290	830	3.792	5.283	76.190
Public Vacancies p. 1,000	Extensive	1120	1.000	218	902	3.268	3.304	22.383
All Openings p. 1,000	Extensive	1120	1.000	832	288	0.296	0.770	6.803
Public Openings p. 1,000	Extensive	1120	1.000	1030	90	0.056	0.331	6.515
Confessional Appointments p. 1,000	Extensive	1120	1.000	601	519	2.144	4.239	66.667
Public Appointments p. 1,000	Extensive	1120	1.000	427	693	1.416	1.966	20.772
Log Median Offered Salary	Conditional	870	0.777	0	870	6.343	0.318	7.378
Salary Quotes p. 1,000	Extensive	1120	1.000	250	870	5.155	6.120	55.215

Source: eleven digitized Dutch titles on Delpher, ten education periodicals and one general newspaper, 1903–1909. Extensive-margin outcomes carry genuine zeros for every municipality in the estimation sample: the corpus is national in every source-year, so no observed signal is an observed absence. Conditional outcomes (shares, salary quantiles, free-residence incidence) are undefined without a qualifying advertisement and remain missing; coverage is the share of the estimation sample on which the outcome is defined. Counts are cross-source deduplicated events; per-capita denominators use the HDNG male population in 1896. All observations postdate the reform by at least six years.

Table A.21: Vacancy Results and the Measurement of School Affiliation

Outcome	Source Default		Text Explicit		Observations
	Estimate	S.E.	Estimate	S.E.	
Confessional Vacancy Events	0.887***	(0.301)	0.870***	(0.290)	1120
Public Vacancy Events	-0.049	(0.116)	-0.002	(0.043)	1120
Confessional School Openings	0.055**	(0.025)	0.047**	(0.024)	1120
Confessional Share of Activity	5.451***	(2.090)	4.355*	(2.598)	643

Notes: Preferred two-stage least-squares specifications for 1903–1909. Source Default classifies records by the orientation of the periodical when the advertisement does not state an affiliation. Text Explicit instead uses only records whose school affiliation is stated in the text. Effect units follow Table A.19. The explicit confessional-share estimate is similar in magnitude and direction to the preferred estimate but is less precise. Heteroskedasticity-robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.22: Vacancy IV Estimates across Pre-Specified Source Frames and Volume Constructions

Frame / construction	Outcome	Estimate	S.E.	<i>p</i>	Mean	Zero frac.	<i>N</i>	First-stage <i>F</i>
Full current corpus	Total events per 1,000	0.913***	(0.348)	0.009	7.353	0.15	1113	27.0
Full current corpus	Confessional events per 1,000	0.978***	(0.338)	0.004	3.941	0.52	1113	27.0
Full current corpus	Public events per 1,000	-0.062	(0.128)	0.630	3.255	0.20	1113	27.0
Full current corpus	Confessional minus public per 1,000	1.040***	(0.371)	0.005	0.686	0.19	1113	27.0
Closed specialist-periodical frame	Total events per 1,000	0.904***	(0.347)	0.009	7.326	0.15	1113	27.0
Closed specialist-periodical frame	Confessional events per 1,000	0.971***	(0.336)	0.004	3.929	0.53	1113	27.0
Closed specialist-periodical frame	Public events per 1,000	-0.065	(0.128)	0.613	3.242	0.20	1113	27.0
Closed specialist-periodical frame	Confessional minus public per 1,000	1.036***	(0.370)	0.005	0.687	0.19	1113	27.0
Existing balanced family	Total events per 1,000	0.223	(0.144)	0.121	1.990	0.52	1113	27.0
Existing balanced family	Confessional events per 1,000	0.307**	(0.137)	0.025	1.552	0.62	1113	27.0
Existing balanced family	Public events per 1,000	-0.098*	(0.058)	0.092	0.409	0.74	1113	27.0
Existing balanced family	Confessional minus public per 1,000	0.405***	(0.155)	0.009	1.144	0.54	1113	27.0
Text-explicit allegiance frame	Total events per 1,000	0.960***	(0.330)	0.004	4.146	0.43	1113	27.0
Text-explicit allegiance frame	Confessional events per 1,000	0.960***	(0.326)	0.003	3.668	0.53	1113	27.0
Text-explicit allegiance frame	Public events per 1,000	-0.002	(0.047)	0.961	0.462	0.68	1113	27.0
Text-explicit allegiance frame	Confessional minus public per 1,000	0.962***	(0.329)	0.004	3.206	0.45	1113	27.0
Source-volume constructions	Total events per 1,000	0.913***	(0.348)	0.009	7.353	0.15	1113	27.0
Source-volume constructions	Raw within-source spells per 1,000	1.322***	(0.491)	0.007	9.381	0.15	1113	27.0
Source-volume constructions	Equal-source-year standardized index	0.016*	(0.008)	0.056	-0.000	0.00	1113	27.0
Source-volume constructions	Balanced-family spells per 1,000	0.295*	(0.164)	0.072	2.242	0.52	1113	27.0

Notes: All specifications use the distinct-municipality IV spine, the same predetermined controls and province fixed effects, and HC1 standard errors. Estimates are effects of a one-percentage-point larger suffrage expansion. Event outcomes are cross-source deduplicated. The equal-source-year index standardizes municipal within-source spell rates within every active source-year and weights active source-years equally. Inactive source-years remain missing.

Table A.23: Allegiance-Classification Bounds for Vacancy Levels and Net Composition

Frame / construction	Outcome	Estimate	S.E.	<i>p</i>	Mean	Zero frac.	<i>N</i>	First-stage <i>F</i>
Registered source defaults	Confessional events per 1,000	0.978***	(0.338)	0.004	3.941	0.52	1113	27.0
Registered source defaults	Public events per 1,000	-0.062	(0.128)	0.630	3.255	0.20	1113	27.0
Registered source defaults	Confessional minus public per 1,000	1.040***	(0.371)	0.005	0.686	0.19	1113	27.0
Registered source defaults	Protestant minus public per 1,000	0.998***	(0.364)	0.006	0.561	0.18	1113	27.0
Text-explicit events only	Confessional events per 1,000	0.960***	(0.326)	0.003	3.668	0.53	1113	27.0
Text-explicit events only	Public events per 1,000	-0.002	(0.047)	0.961	0.462	0.68	1113	27.0
Text-explicit events only	Confessional minus public per 1,000	0.962***	(0.329)	0.004	3.206	0.45	1113	27.0
Text-explicit events only	Protestant minus public per 1,000	0.920***	(0.322)	0.004	3.086	0.46	1113	27.0
Unknown assigned to public	Confessional events per 1,000	0.978***	(0.338)	0.004	3.941	0.52	1113	27.0
Unknown assigned to public	Public events per 1,000	-0.068	(0.132)	0.606	3.390	0.19	1113	27.0
Unknown assigned to public	Confessional minus public per 1,000	1.046***	(0.376)	0.005	0.551	0.18	1113	27.0
Unknown assigned to public	Protestant minus public per 1,000	1.004***	(0.369)	0.007	0.426	0.18	1113	27.0
Unknown assigned to confessional	Confessional events per 1,000	0.972***	(0.334)	0.004	4.075	0.47	1113	27.0
Unknown assigned to confessional	Public events per 1,000	-0.062	(0.128)	0.630	3.255	0.20	1113	27.0
Unknown assigned to confessional	Confessional minus public per 1,000	1.034***	(0.367)	0.005	0.820	0.19	1113	27.0
Unknown assigned to confessional	Protestant minus public per 1,000	0.998***	(0.364)	0.006	0.561	0.18	1113	27.0
Strict mixed-source bound: public	Confessional events per 1,000	0.978***	(0.338)	0.004	3.941	0.52	1113	27.0
Strict mixed-source bound: public	Public events per 1,000	-0.064	(0.132)	0.629	3.393	0.19	1113	27.0
Strict mixed-source bound: public	Confessional minus public per 1,000	1.042***	(0.376)	0.006	0.548	0.18	1113	27.0
Strict mixed-source bound: public	Protestant minus public per 1,000	1.000***	(0.369)	0.007	0.423	0.18	1113	27.0
Strict mixed-source bound: confessional	Confessional events per 1,000	0.943***	(0.327)	0.004	4.340	0.41	1113	27.0
Strict mixed-source bound: confessional	Public events per 1,000	-0.029	(0.120)	0.812	2.994	0.21	1113	27.0
Strict mixed-source bound: confessional	Confessional minus public per 1,000	0.972***	(0.349)	0.005	1.346	0.20	1113	27.0
Strict mixed-source bound: confessional	Protestant minus public per 1,000	0.965***	(0.353)	0.006	0.822	0.19	1113	27.0

Notes: Outcomes are unconditional rates on the full municipal spine. Strict bounds also assign every non-explicit event touching a mixed/general source (De Tijd, Het Nieuwe Schoolblad, School en Leven, or De Volksonderwijzer) to the indicated side. Estimates are effects of a one-percentage-point larger suffrage expansion; standard errors are HC1.

Table A.24: Vacancy-Source Coverage, Matching, Allegiance, and Geographic Concentration

Source	Active years	Issues	Pages	Extracted	Coupled share	Spells	Explicit share	Largest province share
De Bazuin	7	365	1302	361	0.884	192	0.673	Groningen (24.5%)
De Bode	7	380	2074	7082	0.856	5039	0.063	Zuid-Holland (19.3%)
Christelijk Schoolblad	7	366	1415	10597	0.866	4587	0.728	Zuid-Holland (31.9%)
De Christelijke Onderwijzer	7	223	680	2238	0.942	949	0.306	Zuid-Holland (31.4%)
De Tijd	7	616	740	91	0.769	53	0.868	Noord-Holland (35.8%)
Het Katholieke Schoolblad	2	85	685	696	0.976	345	0.930	Zuid-Holland (23.3%)
Het Nieuwe Schoolblad	7	244	958	2347	0.956	1529	0.205	Zuid-Holland (48.3%)
School en Leven	7	1147	4731	236	0.894	161	0.326	Zuid-Holland (37.3%)
De School met den Bijbel	7	430	1898	16731	0.937	7142	0.819	Zuid-Holland (28.3%)
Het Schoolblad	7	363	4265	7288	0.911	1667	0.286	Noord-Holland (19.1%)
De Volksonderwijzer	6	137	520	88	0.739	41	0.477	Zuid-Holland (29.3%)

Notes: Issues and candidate pages are taken from the frozen coverage audit. Coupling success is national by source because an unmatched extracted place has no defensible province assignment. The final column identifies the province with the largest share of each source's matched spells; detailed source-province rates and shares are retained in the machine-readable run summary.

Table A.25: Stacked Municipality by Source-Year Vacancy Estimates

Specification	Outcome	Estimate	S.E.	Implied 1903–09 total	<i>N</i>	Municipalities	First-stage Wald
Stacked, source-year FE, clustered by municipality	Within-source spells per 1,000	0.0186***	(0.0069)	1.322	79023	1113	23.4
Stacked, source-year FE, clustered by municipality	Confessional spells per 1,000	0.0202***	(0.0068)	1.433	79023	1113	23.4
Stacked, source-year FE, clustered by municipality	Public spells per 1,000	-0.0016	(0.0022)	-0.111	79023	1113	23.4
Stacked, source-year FE, clustered by municipality	Confessional minus public spells per 1,000	0.0217***	(0.0075)	1.544	79023	1113	23.4
Stacked, source-year FE, clustered by municipality	Standardized within-source-year spell rate	0.0158*	(0.0082)	–	77910	1113	23.4
Stacked, source-year FE, rows treated as independent	Within-source spells per 1,000	0.0186***	(0.0029)	1.322	79023	1113	1660.5
Stacked, source-year FE, rows treated as independent	Confessional spells per 1,000	0.0202***	(0.0024)	1.433	79023	1113	1660.5
Stacked, source-year FE, rows treated as independent	Public spells per 1,000	-0.0016	(0.0016)	-0.111	79023	1113	1660.5
Stacked, source-year FE, rows treated as independent	Confessional minus public spells per 1,000	0.0217***	(0.0029)	1.544	79023	1113	1660.5
Stacked, source-year FE, rows treated as independent	Standardized within-source-year spell rate	0.0158***	(0.0049)	–	77910	1113	1637.1
Stacked, province x source-year FE, clustered by municipality	Within-source spells per 1,000	0.0186***	(0.0069)	1.322	79023	1113	23.2
Stacked, province x source-year FE, clustered by municipality	Confessional spells per 1,000	0.0202***	(0.0069)	1.433	79023	1113	23.2
Stacked, province x source-year FE, clustered by municipality	Public spells per 1,000	-0.0016	(0.0022)	-0.111	79023	1113	23.2
Stacked, province x source-year FE, clustered by municipality	Confessional minus public spells per 1,000	0.0217***	(0.0075)	1.544	79023	1113	23.2
Stacked, province x source-year FE, clustered by municipality	Standardized within-source-year spell rate	0.0158*	(0.0082)	–	77910	1113	23.2
Collapsed municipality cross-section	Within-source spells per 1,000	1.3221***	(0.4911)	1.322	1113	1113	23.1
Collapsed municipality cross-section	Confessional spells per 1,000	1.4327***	(0.4889)	1.433	1113	1113	23.1
Collapsed municipality cross-section	Public spells per 1,000	-0.1112	(0.1582)	-0.111	1113	1113	23.1
Collapsed municipality cross-section	Confessional minus public spells per 1,000	1.5439***	(0.5327)	1.544	1113	1113	23.1
Collapsed municipality cross-section	Standardized within-source-year spell rate	0.0158*	(0.0083)	–	1113	1113	23.1

Notes: Observations are municipality by active source-year cells; source-years without issues are excluded rather than set to zero. Outcomes are within-source vacancy spells, which unlike the cross-source deduplicated events can be attributed to a single source-year. Estimates are effects of a one-percentage-point larger suffrage expansion. In the stacked rows that effect is measured in a single source-year, and the implied total multiplies it by the 71 active source-years; the collapsed row is already a 1903–1909 total, so its two estimate columns coincide. The stacked totals and the collapsed estimates agree exactly, by construction. The standardized outcome is measured in within-source-year standard deviations and has no meaningful total. Treatment, instrument, controls and province fixed effects are municipality-level, so stacking replicates rather than adds information: the second row shows the precision obtained by treating stacked rows as independent, and the clustered standard error is roughly 2.4 times larger. The reported first-stage statistic is the Wald test under each estimation’s own variance matrix; the conventional unclustered first-stage statistic on the stacked panel exceeds 1945 and reflects replicated rows rather than instrument strength.

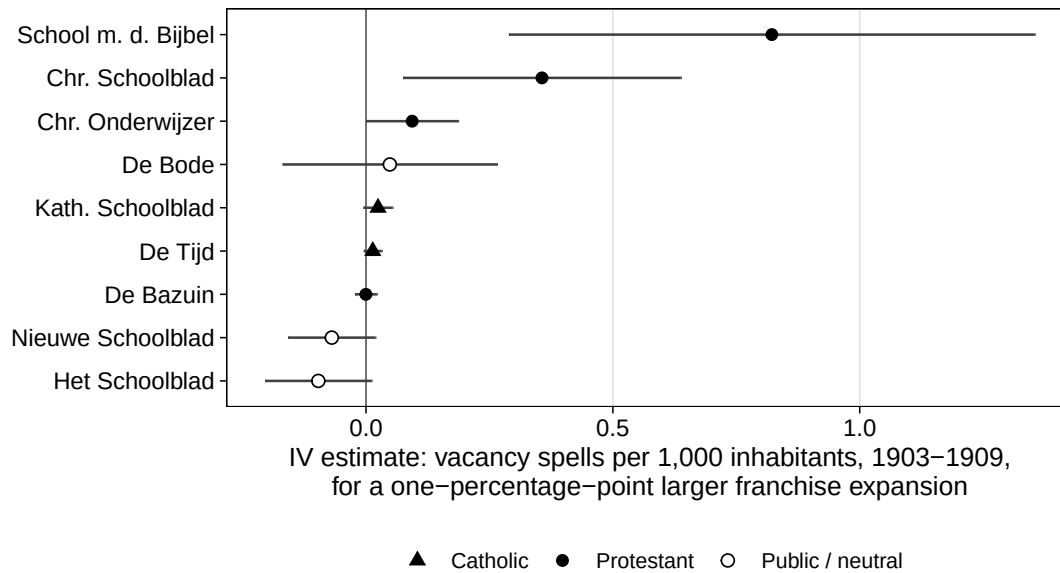
Table A.26: IV Estimates by Periodical: Source-Specific Vacancy Spells per 1,000 Inhabitants, 1903–1909. Het Katholieke Schoolblad Covers 1908–1909 Only; De Tijd Is a Catholic Daily Whose Vacancy Volume Had Already Thinned by 1903

	School m. d. Bijbel	Chr. Schoolblad	De Bode	Het Schoolblad	Nieuwe Schoolblad	Chr. Onderwijzer	Kath. Schoolblad	De Bazuin	De Tijd
Suffrage Expansion	0.822*** (0.272)	0.356** (0.144)	0.048 (0.111)	-0.097* (0.055)	-0.069 (0.045)	0.093** (0.048)	0.024 (0.015)	-0.000 (0.011)	0.014 (0.009)
Log Population Density (1896)	0.346 (0.437)	0.179 (0.193)	0.197 (0.139)	0.063 (0.091)	-0.104** (0.050)	0.131** (0.065)	-0.002 (0.020)	-0.011 (0.019)	0.012 (0.011)
Log Population (1896)	0.389 (0.310)	0.402** (0.170)	-0.654*** (0.145)	0.041 (0.064)	-0.129** (0.056)	-0.003 (0.078)	0.056*** (0.014)	0.025 (0.016)	-0.009 (0.011)
Tax Revenue per Capita (1889)	0.523 (0.375)	0.385* (0.199)	-0.053 (0.149)	-0.032 (0.069)	-0.070 (0.067)	0.143** (0.072)	0.054** (0.025)	-0.009 (0.017)	0.029* (0.018)
Religious Composition	-2.095*** (0.266)	-0.950*** (0.122)	-0.251** (0.113)	-0.014 (0.039)	-0.026 (0.037)	-0.239*** (0.041)	0.069*** (0.021)	-0.025* (0.013)	0.052*** (0.016)
Demographic Composition	1.038** (0.411)	0.409* (0.209)	-0.079 (0.137)	-0.121 (0.074)	-0.082 (0.058)	0.088 (0.068)	0.050** (0.022)	0.002 (0.014)	0.020* (0.012)
R2 Adj.	0.199	0.163	0.083	0.034	0.048	0.072	0.039	0.003	0.033
R2 Within Adj.	0.102	0.065	0.035	0.016	0.014	0.040	0.027	-0.004	0.019
Num.Obs.	1120	1120	1120	1120	1120	1120	1120	1120	1120
First-Stage F-Stat	23	23	23	23	23	23	23	23	23
Dep. Var. Mean	3.171	1.707	2.867	0.444	0.551	0.391	0.122	0.066	0.040
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Treatment Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Window	1903–09	1903–09	1903–09	1903–09	1903–09	1903–09	1908–09	1903–09	1903–09
Sample	Full (zeros)	Full (zeros)	Full (zeros)	Full (zeros)	Full (zeros)	Full (zeros)	Full (zeros)	Full (zeros)	Full (zeros)

* p < 0.1, ** p < 0.05, *** p < 0.01

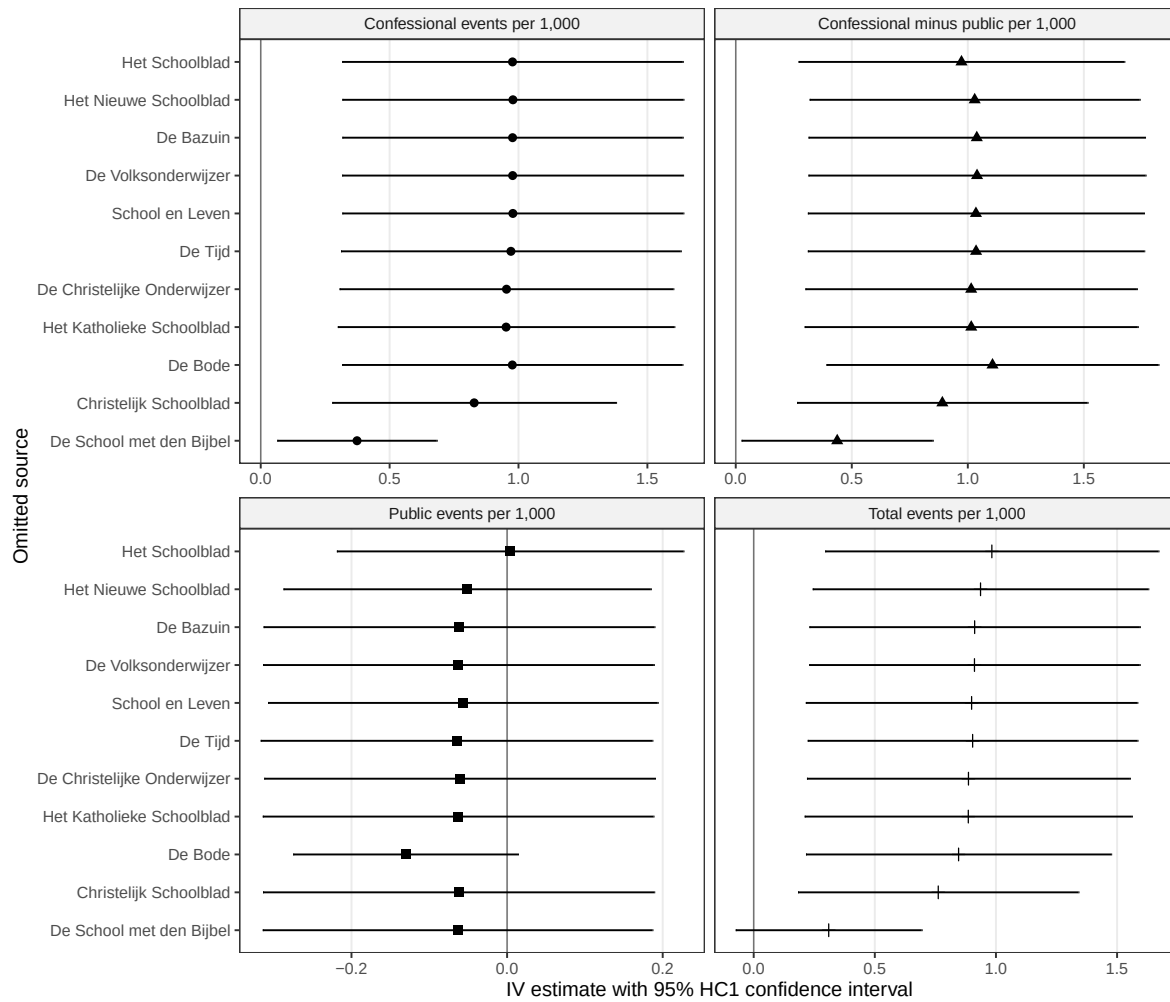
Estimates are for a one-percentage-point larger franchise expansion. Rate outcomes are events per 1,000 inhabitants, share and indicator outcomes are in percentage points, and logged outcomes are in percent.

Figure A.5: Source-Specific Vacancy Estimates



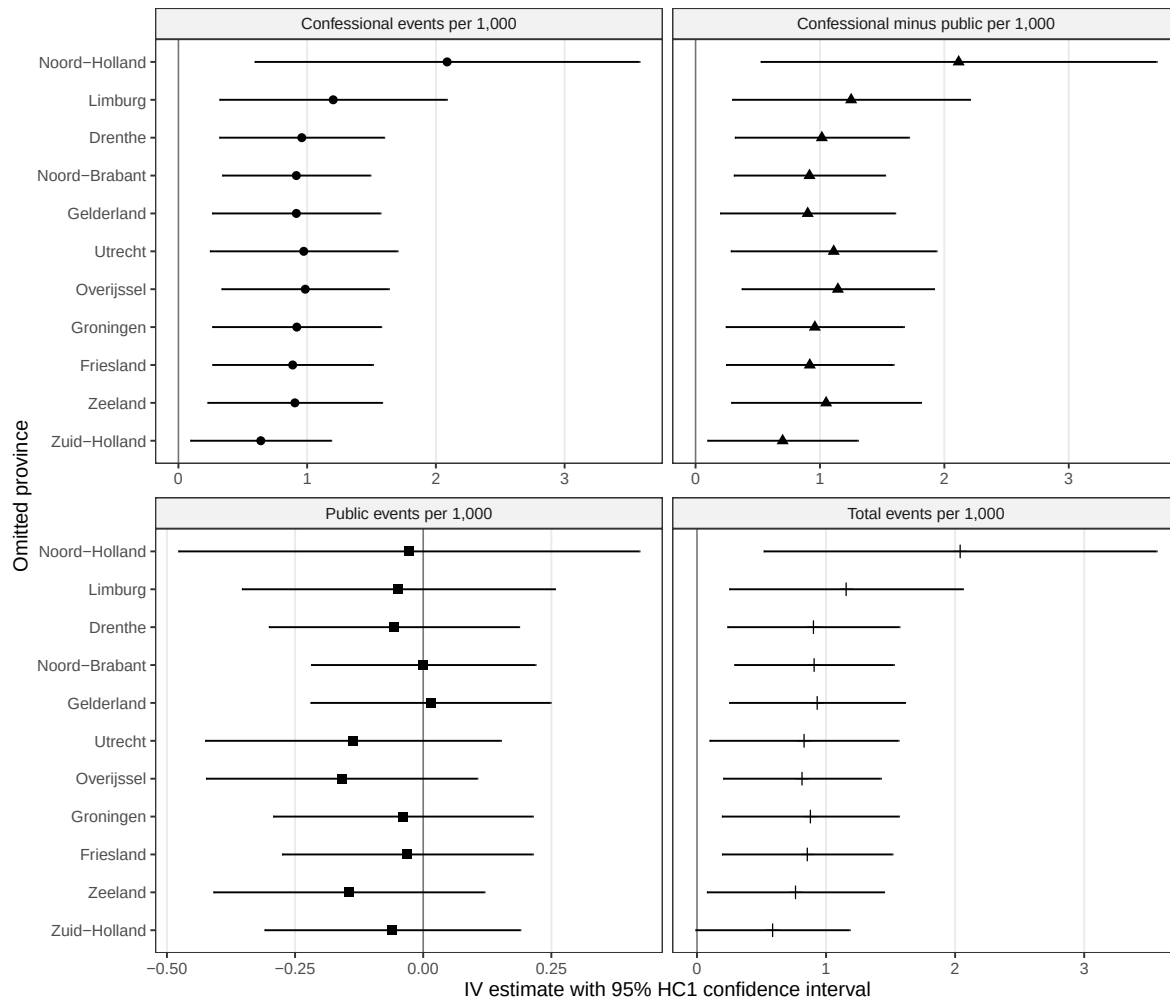
Notes: Points report source-specific two-stage least-squares estimates for vacancy-event rates during 1903–1909. Lines are heteroskedasticity-robust confidence intervals. Sources differ in orientation and years of usable coverage; the figure is a diagnostic rather than a set of independent replications.

Figure A.6: Leave-One-Source-Out Vacancy Estimates



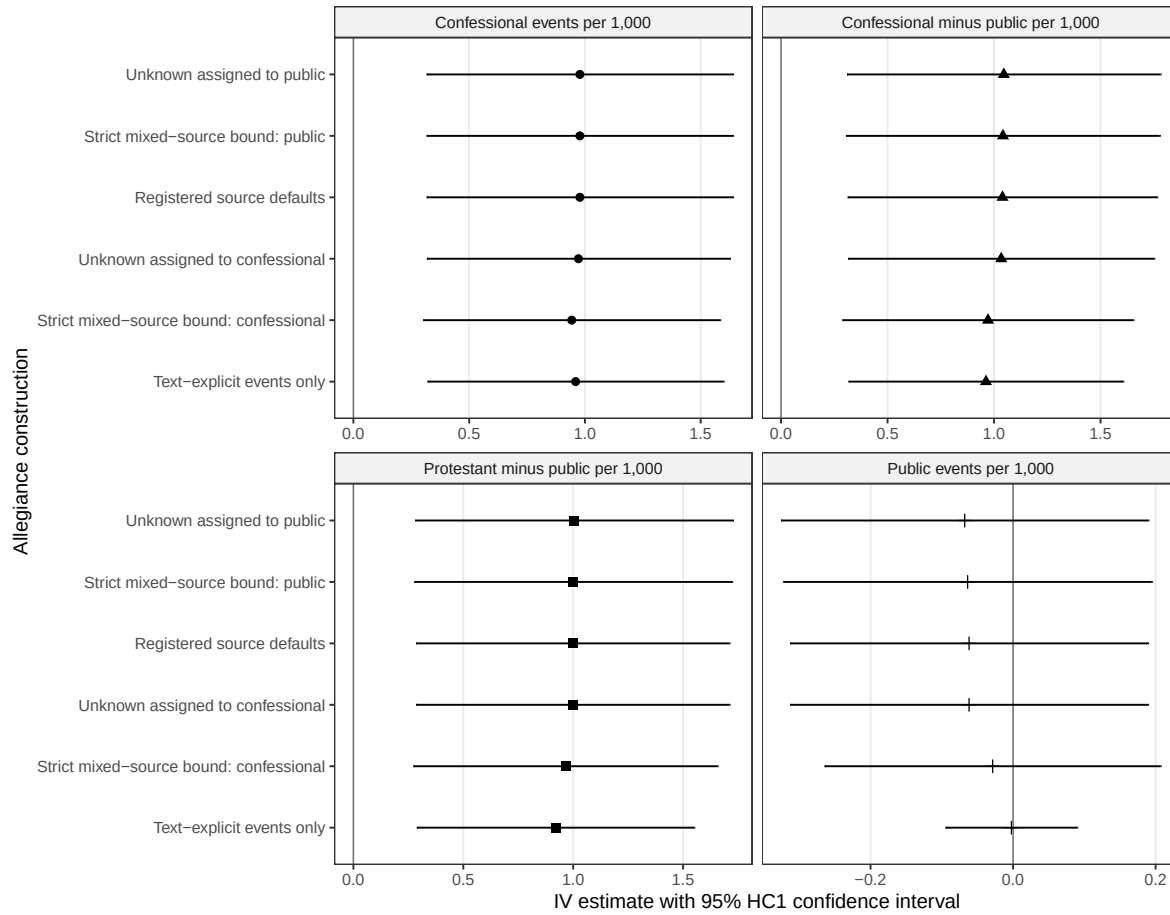
Notes: Points report two-stage least-squares estimates for a one-percentage-point larger suffrage expansion after omitting the named source. Lines are 95 percent HC1 confidence intervals. Event counts remain cross-source deduplicated within each frame.

Figure A.7: Leave-One-Province-Out Vacancy Estimates



Notes: Points report the same specification after omitting the named province. Lines are 95 percent HC1 confidence intervals.

Figure A.8: Allegiance-Classification Bounds



Notes: Outcomes are unconditional rates on the full municipal spine. Strict bounds also assign every non-explicit event touching a mixed or general source to the indicated side. Lines are 95 percent HC1 confidence intervals.

Table A.27: Timing Diagnostics: the Same Outcomes over 1903-05, 1906-07 and 1908-09. All Observations Are Post-Treatment; These Are Not Event-Study Estimates									
	Vacancy Events p. 1,000 (1903-05)	Vacancy Events p. 1,000 (1906-07)	Vacancy Events p. 1,000 (1908-09)	Confessional Vacancies p. 1,000 (1903-05)	Confessional Vacancies p. 1,000 (1906-07)	Confessional Vacancies p. 1,000 (1908-09)	Balanced-Family Spells p. 1,000 (1903-05)	Balanced-Family Spells p. 1,000 (1906-07)	Balanced-Family Spells p. 1,000 (1908-09)
Suffrage Expansion	0.122 (0.116)	0.341** (0.140)	0.390*** (0.121)	0.206* (0.107)	0.384*** (0.122)	0.298*** (0.102)	0.139* (0.085)	0.103 (0.069)	0.048 (0.031)
Log Population Density (1896)	0.146 (0.149)	0.217 (0.234)	0.350* (0.202)	0.087 (0.143)	0.145 (0.218)	0.285* (0.168)	0.063 (0.110)	0.111 (0.094)	0.089** (0.040)
Log Population (1896)	-0.147 (0.168)	-0.090 (0.182)	0.023 (0.143)	0.083 (0.136)	0.278* (0.157)	0.117 (0.115)	0.249** (0.100)	0.164* (0.086)	0.074** (0.037)
Tax Revenue per Capita (1889)	0.109 (0.150)	0.271 (0.190)	0.310* (0.185)	0.233* (0.140)	0.346* (0.182)	0.191 (0.147)	0.178* (0.107)	0.132 (0.084)	0.038 (0.041)
Religious Composition	-0.780*** (0.090)	-0.869*** (0.116)	-0.669*** (0.122)	-0.673*** (0.080)	-0.896*** (0.118)	-0.683*** (0.111)	-0.484*** (0.062)	-0.270*** (0.048)	-0.117*** (0.044)
Demographic Composition	0.174 (0.135)	0.364* (0.197)	0.441** (0.193)	0.256* (0.138)	0.438** (0.196)	0.374** (0.165)	0.181* (0.101)	0.127 (0.088)	0.062 (0.050)
R2 Adj.	0.164	0.118	0.132	0.132	0.150	0.139	0.132	0.080	0.051
R2 Within Adj.	0.048	0.046	0.046	0.048	0.070	0.067	0.047	0.024	0.018
Num.Obs.	1120	1120	1120	1120	1120	1120	1120	1120	1120
First-Stage F-Stat	23	23	23	23	23	23	23	23	23
Dep. Var. Mean	2.459	2.690	2.240	1.278	1.453	1.216	1.096	0.816	0.351
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Treatment Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Window	1903-05	1906-07	1908-09	1903-05	1906-07	1908-09	1903-05	1906-07	1908-09
Sample	Full (zeros)	Full (zeros)	Full (zeros)	Full (zeros)	Full (zeros)	Full (zeros)	Full (zeros)	Full (zeros)	Full (zeros)

* p < 0.1, ** p < 0.05, *** p < 0.01
Estimates are for a one-percentage-point larger franchise expansion. Rate outcomes are events per 1,000 inhabitants, share and indicator outcomes are in percentage points, and logged outcomes are in percent.

Table A.28: IV Estimates: Composition of Advertised Posts and Offered Terms, 1903–1909. Salary and Share Outcomes Are Conditional on the Municipality Having a Qualifying Advertisement

	Public Share of Activity	Head-Post Share	Log Median Offered Salary	Log p25 Offered Salary	Log p75 Offered Salary	Salary Quotes p. 1,000	Free-Residence Share
Suffrage Expansion	-5.246*** (2.018)	-2.048* (1.102)	-0.812 (1.179)	-3.401** (1.662)	-1.721 (1.408)	0.672*** (0.259)	-1.550 (2.103)
Log Population Density (1896)	-2.809 (2.547)	-3.013** (1.240)	0.008 (0.011)	-0.003 (0.021)	0.027* (0.014)	0.554 (0.411)	-2.536** (1.105)
Log Population (1896)	-9.653*** (1.951)	-4.727*** (1.305)	0.009 (0.018)	-0.006 (0.024)	0.042** (0.019)	-0.130 (0.307)	-2.006* (1.171)
Tax Revenue per Capita (1889)	-6.835** (2.943)	-1.070 (1.550)	0.006 (0.016)	-0.024 (0.024)	0.007 (0.019)	0.568 (0.357)	-3.137 (3.627)
Religious Composition	11.296*** (1.565)	1.059 (0.892)	0.019* (0.011)	0.035* (0.018)	0.020* (0.012)	-1.775*** (0.211)	0.681 (0.927)
Demographic Composition	-7.545*** (2.854)	-2.261 (1.508)	-0.014 (0.015)	-0.051** (0.026)	-0.022 (0.019)	0.755** (0.355)	-1.201 (2.670)
R2 Adj.	0.273	0.050	0.012	0.016	0.067	0.214	0.027
R2 Within Adj.	0.144	0.044	0.002	0.003	0.051	0.074	0.030
Num.Obs.	954	954	870	870	870	1120	562
First-Stage F-Stat	16.2	16.2	14.9	14.9	14.9	23	4.9
Dep. Var. Mean	62.038	19.026	6.343	6.219	6.458	5.155	91.757
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-Treatment Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Outcome Window	1903–09	1903–09	1903–09	1903–09	1903–09	1903–09	1903–09
Sample	Conditional	Conditional	Conditional	Conditional	Conditional	Full (zeros)	Conditional

* p < 0.1, ** p < 0.05, *** p < 0.01

Estimates are for a one-percentage-point larger franchise expansion. Rate outcomes are events per 1,000 inhabitants, share and indicator outcomes are in percentage points, and logged outcomes are in percent.

Table A.29: Kieswet Suffrage Expansion and Protestant Unie Mobilization

	Pre level	Linear pre-trend	1895–96	Post – pre	High coverage
Committee Presence (Covering)	0.727 (1.033)	0.194 (0.322)	5.805** (2.596)	-0.824 (0.988)	-1.149 (1.110)
Committee Presence (Seat)	0.593 (0.976)	0.341 (0.304)	4.676** (2.211)	0.337 (1.083)	-0.511 (1.197)
IHS Receipts per 1,000	0.049 (0.045)	0.010 (0.013)	0.230** (0.102)	0.014 (0.052)	-0.017 (0.057)
<i>Predetermined 1896 Unie-census balance</i>					
Any Protestant School, 1896	2.757* (1.648)	–	–	–	–
IHS Protestant Schools per 1,000, 1896	0.050*** (0.018)	–	–	–	–
Observations	1112	7772–7784	1112	1112	1112

Notes: Entries are effects of a one-percentage-point larger 1896–97 suffrage shock. The pre-trend is the annual differential slope over 1890–96. The 1895–96 column isolates the final pre-1897 change as an anticipation/placebo test. Post-minus-pre compares coverage-weighted means for 1897–1917 and 1890–96. High coverage retains seasons with at least half of the numbered lists observed. All models use the standard predetermined controls and province adjustments; change models also condition on 1896 Protestant-school strength. The census-balance rows use the standard controls but do not condition on the census outcome itself. HC1 standard errors for changes and municipality-clustered standard errors for pre-trends. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A.30: Unie Mobilization Effects by Baseline Protestant-School Presence

	No school	School present	Difference
Committee Presence (Covering)	-1.614* (0.951)	-0.229 (1.158)	1.385* (0.812)
Committee Presence (Seat)	-0.526 (1.063)	0.986 (1.185)	1.512* (0.828)
IHS Receipts per 1,000	-0.025 (0.052)	0.043 (0.055)	0.067* (0.034)

Notes: The first two columns are linear combinations from one interacted IV model; the third tests their difference. Outcomes are coverage-weighted post-minus-pre changes. The remaining specification matches Table [A.29](#).

Table A.31: Protestant Collection Activity in Protestant-Majority Municipalities

	All municipalities	Protestant majority	Protestant majority, high-coverage seasons
<i>Panel A. Committee coverage (percentage points)</i>			
Suffrage expansion, 1896–97	-0.824 (0.988)	-0.589 (1.234)	-0.838 (1.430)
First-stage F	28.7	24.1	24.1
Municipalities	1,112	672	672
<i>Panel B. Committee seat presence (percentage points)</i>			
Suffrage expansion, 1896–97	0.337 (1.083)	0.637 (1.470)	-0.328 (1.624)
First-stage F	28.7	24.1	24.1
Municipalities	1,112	672	672
<i>Panel C. Receipts per 1,000 inhabitants (transformed)</i>			
Suffrage expansion, 1896–97	0.014 (0.052)	0.037 (0.072)	-0.002 (0.079)
First-stage F	28.7	24.1	24.1
Municipalities	1,112	672	672

Notes: Two-stage least-squares estimates of the effect of the 1896–97 expansion in eligible voters per capita on the change in Protestant “A School with the Bible” collection activity between the pre-reform seasons of 1890–1896 and the post-reform seasons of 1897–1917. The expansion is instrumented by the municipality’s class rank under the 1896 Kieswet. Each municipality’s pre-period and post-period outcome is a mean over seasons, weighted by the share of that season’s numbered subscription lists that the digitization recovered; the outcome is the post-period mean minus the pre-period mean. Committee coverage records a municipality named anywhere in a collection committee, seat presence only a municipality hosting the committee itself, and receipts are the inverse hyperbolic sine of guilders collected per 1,000 inhabitants. The first two are measured in percentage points and the third in transformed units, each per one-percentage-point larger suffrage expansion. The Protestant-majority subsample retains municipalities in which Protestants exceeded half of the 1889 population, the population for which the collection is a live institution and the test therefore has most power. Of the 673 such municipalities in the estimation sample, one is the sole Protestant-majority municipality in its province and is absorbed by the province fixed effect. The third column further restricts the season means to seasons in which at least half of the numbered lists were observed. All specifications include province fixed effects and the paper’s predetermined controls, namely log population density, log population, tax revenue per capita in 1889, and the first principal components of the religious and demographic composition of the municipality, and additionally condition on the presence and per-capita intensity of Protestant schools recorded in the 1896 Unie census. Heteroskedasticity-robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.