Dynamics of Democratization: Evidence from Municipal Suffrage Extensions

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Abstract:

This paper investigates the dynamic consequences of franchise extension on future institutional change and local fiscal policy. Using a historical cross-section of Dutch municipal governments with a franchise threshold partially based on income, we exploit exogenous variation in the income threshold assigned to each municipality under the 1896 Electoral Law to identify the causal effect of suffrage expansion on subsequent voting rights and fiscal decisions. We find that while the initial enfranchisement shock persists, it does not trigger further franchise extensions over a six-year horizon, rejecting a multiplier effect of democratization. We also document a large, negative fiscal response: municipalities with a broader electorate exhibit significantly lower municipal expenditures. Decomposing expenditure categories, we find that this effect is driven entirely by reductions in governance and tax-collection costs, with no significant changes in educational spending or poor relief. These findings suggest that the economic effects of democratization can be multifaceted and depend critically on the identity of the newly enfranchised.

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1 Introduction

Does partial democratization beget further democratization? And when it does, what are its fiscal consequences? These questions have occupied scholars since Tocqueville observed that once set in motion, the expansion of popular sovereignty tends to be self-sustaining (De Tocqueville, 2003). Despite a rich theoretical literature on the dynamics of franchise extension, direct causal evidence on whether an initial enfranchisement shock triggers subsequent institutional change—and on what that change means for public finances—remains scarce. This paper addresses both questions using a novel natural experiment from the Netherlands: the introduction of the 1896 Electoral Law (*Kieswet*).

The extension of voting rights is among the most consequential institutional changes a society can undergo. Theories of political economy disagree about both its causes and its effects. In the Acemoglu and Robinson (2000) framework, elites extend the franchise as a credible commitment device to forestall revolution; once extended, broader voting rights shift the identity of the decisive voter, raising the equilibrium demand for redistribution (Meltzer and Richard, 1981). Jack and Lagunoff (2006) develop the intertemporal dimension of this logic, showing formally that a partial franchise extension is equivalent to delegating future policy authority to a new pivotal voter—and that this delegation is self-reinforcing whenever citizens' private decisions generate positive spillovers on the current median voter's payoff. An initial expansion of the franchise may trigger a *multiplier effect*, in which the newly enfranchised demand further political inclusion, producing sequential waves of reform. Yet this theoretical mechanism has received limited empirical scrutiny. Testing it requires an exogenous shock to the initial level of enfranchisement.

The 1896 Dutch Electoral Law offers an unusually clean natural experiment. The reform extended voting rights to all male residents aged 25 and above who satisfied income or rental thresholds that varied systematically across municipalities. These thresholds were determined by a nine-class typology of municipalities, itself inherited—largely unchanged—from the contemporaneous Personal Taxation Law of 1896. Crucially, the classification of municipalities into classes was not based on a continuous measure of local income; rather, it relied on a preexisting administrative table that contemporaries widely acknowledged to be imprecise. Provincial authorities were explicit: the assignment of municipalities to classes was "strikingly incorrect," grouping economically disparate communities into identical tax brackets. This administrative misclassification generates plausibly exogenous variation in the *degree* of franchise extension across otherwise similar municipalities. A municipality assigned to a lower class faced lower income thresholds on both the primary route to eligibility (assessment under the personal tax) and the secondary route (rent and income tables), thereby enfranchising a larger share of its population than an otherwise identical municipality assigned to a higher class. We exploit this discontinuous, class-induced variation to construct an instrumental variable for the size of the 1896–1897 suffrage extension.

This paper makes three contributions to the literature. First, we test whether the initial enfranchisement shock triggered a *multiplier effect* in subsequent suffrage growth. Instrumenting for the 1896–1897 shock with class rank, we find no significant causal effect on suffrage growth over the subsequent six years: the IV point estimate is close to zero and statistically indistinguishable from it. This null result qualifies the theoretical prediction of Jack and Lagunoff (2006)—that partial extensions are self-reinforcing—suggesting that the conditions for such dynamics either require a longer time horizon than our six-year window captures, or depend on the type of voters enfranchised. Acemoglu et al. (2025) show that democratic self-reinforcement is driven primarily by successful democracies that deliver economic and policy gains; our results are consistent with this conditionality.

Second, we document the fiscal consequences of franchise extension at a level of institutional granularity that cross-national studies cannot achieve. The existing empirical literature relies predominantly on country-level panel data spanning decades (Aidt et al., 2006; Aidt and Jensen, 2013; Lindert, 1994). These studies find broadly positive effects of democratization on social spending and redistribution, consistent with the Meltzer-Richard hypothesis, but cannot identify the specific mechanisms through which voter enfranchisement affects budget outcomes. Our IV estimates show that an exogenous increase in the 1896–1897 suffrage shock substantially reduced municipal expenditures per capita by 1903: the effect is -171 guilders per capita in the full sample and implies a 6.8% reduction in total spending in log terms. Aidt et al. (2022) find that the major British Reform Acts did not causally produce the fiscal shifts the standard redistribution hypothesis predicts; our results reinforce the view that the fiscal consequences of democratization are more contingent than cross-national evidence suggests.

Third, and most substantively, we decompose the aggregate expenditure effect to distinguish between two competing mechanisms. The *retrenchment hypothesis* (Aidt et al., 2010; Chapman, 2018, 2024) holds that middle-class franchise extensions reduce public goods provision, as newly dominant ratepayers demand lower spending to reduce their tax burden. The *efficiency hypothesis* holds that taxpayer-voters, bearing the cost of municipal overhead, demand leaner administration rather than cuts to substantive services. Our disaggregated IV estimates decisively favor the efficiency hypothesis. The total expenditure decline is driven entirely by falls in governance costs (-11.1 guilders per capita) and tax compliance costs (-1.5 guilders per capita), while expenditures on education and poor relief show no statistically significant change. Municipalities with a larger franchise extension became administratively leaner without retrenchment of redistributive programs. This pattern contrasts with the findings of Chapman (2026), who shows that the 1894 democratization of English Poor Law boards—which enfranchised a genuinely poorer electorate—*raised* poor relief spending. The juxtaposition confirms that the fiscal direction of franchise extension is determined by who enters the electorate: middle-class taxpayer enfranchisement generates efficiency gains, while working-class enfranchisement generates redistributive expansion. Aidt and Mooney (2014) document a related mechanism in early twentieth-century London, showing that taxpayer suffrage shapes the composition of public spending; our Dutch evidence generalizes this insight to administrative overhead specifically.

Our results on the pre-reform period confirm that the IV strategy is valid: class rank is uncorrelated with pre-reform trends in suffrage, and observable municipal characteristics are balanced across classes conditional on province fixed effects. All results are robust to the inclusion of province fixed effects, controls for municipal population, pre-reform tax revenue, and religious composition, and to sample restriction to municipalities near the top of the class hierarchy.

This paper contributes to several strands of the literature. On the fiscal effects of democratization, it relates to the cross-national evidence in Aidt et al. (2006), Aidt and Jensen (2009), and Persson and Tabellini (2004), and to the British panel evidence in Aidt et al. (2010) and Aidt et al. (2022), while exploiting sharper within-country variation and a spending decomposition unavailable in aggregate data. On the conditionality of fiscal responses, it speaks to the emerging consensus that the type of voters enfranchised matters as much as the breadth of enfranchisement (Aidt and Mooney, 2014; Chapman, 2018, 2024, 2026). On identification, we follow the methodological approach of Naidu (2012) and Bernini et al. (2023), who use administrative discontinuities and boundary designs to isolate the causal effect of changes in franchise eligibility. Finally, the Dutch context adds to a growing literature on the heterogeneity of democratization outcomes across settings (Abou-Chadi and Orlowski, 2015; Marcucci et al., 2023; Larcinese, 2024; Cassan et al., 2025), confirming that the consequences of partial enfranchisement depend on the specific rules through which voter eligibility is determined.

The remainder of the paper is organized as follows. Section 2 describes the historical and institutional context of the 1896 Electoral Law and Personal Taxation Law. Section 3 presents the data sources and develops the empirical strategy. Section 4 presents the main results. Section 5 concludes.

2 Historical Background

2.1 The 1896 Electoral Law

Article 1 of the Electoral Law establishes the criteria for voting. To vote, a male resident aged 25 or older must meet financial requirements. The law provides two main paths to eligibility. The most direct way (Article 1a) to vote is to be assessed for direct taxes. The law explicitly cites the Personal Tax Law of April 16, 1896 (Subsection 2.2), stipulating that "if a man is assessed for the Personal Tax (based on his rent, fireplace, furniture, servants, or horses), and pays at least one guilder in Land Tax or is assessed for Wealth/Business tax, he is automatically eligible to vote." Because the Personal Tax assesses lifestyle assets (size of house, number of servants), it acts as a proxy for wealth. If you were rich enough to be taxed under the Personal Tax Law, the state considered them a stakeholder worthy of a vote. The second main path (Article 1b) provides an alternative route to suffrage. If a man did not pay enough direct tax (perhaps because of exemptions in the Tax Law), he could still vote if he met specific criteria regarding Rent (Huur) or Income (Inkomen).

The Electoral Law recognized that the cost of living varied wildly across the Netherlands. A rental payment of 150 guilders might get you a mansion in a rural village but only a small room in Amsterdam. If the voting requirement were a flat national rate, rural citizens would be disenfranchised because rents there were naturally lower. Conversely, if the bar were set too low to accommodate villages, every urban worker in the cities would be able to vote – something the government at the time wanted to prevent. To solve this, the Electoral Law attached a Table that assigned different financial thresholds to different municipalities. The table lists municipalities and sets specific thresholds for Rent and Minimum Income required to vote. For example, in 's Hertogenbosch (a major provincial city), to vote based on rent, you must pay at least 1.75 guilders per week, and to vote based on income, you must earn at least 450.00 guilders per year. In Besoijen (a smaller, cheaper municipality), to vote based on rent, you only need to pay 1.00 guilder per week, and to vote based on income, you only need to earn 325.00 guilders per year. Hence, someone earning 350 guilders in Besoijen could vote, but a man earning 350 guilders in 's Hertogenbosch could not, even though they earned the exact same amount. The law assumes that the man in Besoijen is relatively wealthier compared to his local peers than the man in the city.

2.2 The 1896 Personal Taxation Law

The Electoral reforms did not only encompass a change in the Electoral Law, but went paired with changes in the Personal Taxation Law to regulate the question of enfranchisement. The 1896 Personal Taxation Law establishes a direct tax system based on external indicators of prosperity and lifestyle rather than direct income. The tax is levied on the head of a household and is calculated based on six distinct "bases" or "groundings" (Grondslagen): Rental Value, Fireplaces, Furniture, Servants, Horses and Bicycles. A crucial administrative feature of this law (Article 5) is the division of all Dutch municipalities into nine classes. The law uses these classes to create a taxation system that accounts for the varying cost of living across the country. This 9-class system to set exemption thresholds and deductions.

Generally, Class 1 represents the most expensive municipalities (likely major cities like Amsterdam or Rotterdam), whereas Class 9 represents the least expensive, rural municipalities. As an example of a deduction, Article 7 of the Law stipulates that taxpayers get a deduction on rental value for children. In a Class 1 municipality, this applies if the rent is under 250 guilders; in a Class 9 municipality, it applies if the rent is under 50 guilders. In addition to deductions, there are also tax-free allowances (Articles 12 & 13): in Class 1, you pay no rental tax if your rent is under 125 guilders. In Class 9, you pay no rental tax if your rent is under 25 guilders. This ensures that a "modest" home is not taxed, but the definition of "modest" changes based on the local economy.

In practice, tax liability was determined by the taxpayer's status on January 15. Taxpayers must then file declarations. Assessments are made by inspectors and a "college of setters" (college van zetters). Based on these assessments, lists of electors were assembled yearly. Disputes were handled by a Council of Appeals (Raad van Beroep).

2.3 Implications for Suffrage

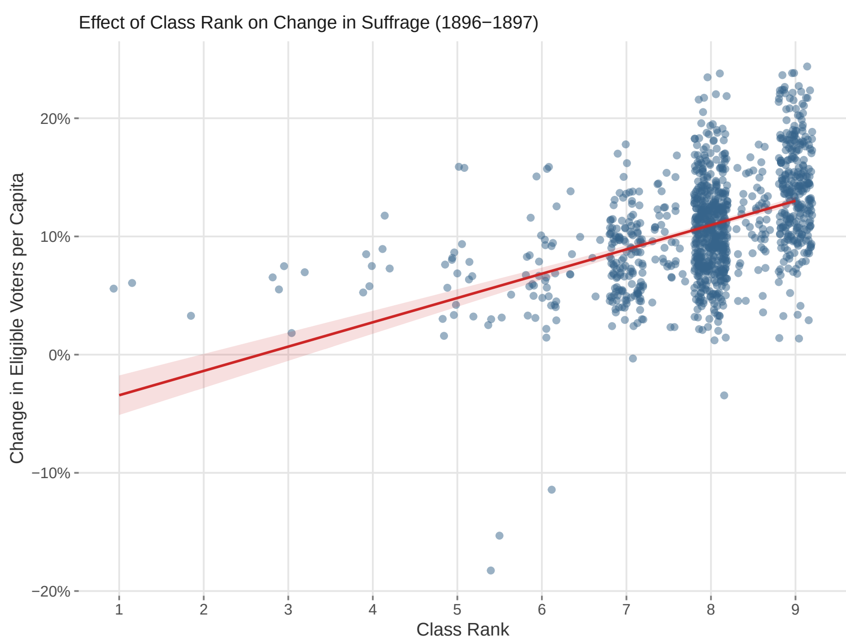
The ostensible goal of modifying these two laws was specifically designed to equalize franchise and to ensure that the top percentage of society voted in every location. However, the system failed at this and likely caused a discontinuous increase in the amount of voters per capita as a function of the class placement regulated in the Personal Taxation Law.

Under the assumption of *ceteris paribus* income distributions, being assigned to e.g. Class 9 (the "poorest" class) acts as a powerful institutional lever that increases enfranchisement relative to Class 8. This occurs first through the interaction of Article 1a of the Electoral Law and Article 12 of the Personal Tax Law. Article 1a grants the vote to anyone assessed for the personal tax, but Article 12 sets the tax-exemption threshold lower for Class 9 (25.00 guilders rental value) than for Class 8 (37.50 guilders). Consequently, in two municipalities with identical wealth, citizens with rental values between 25.00 and 37.50 guilders become tax-assessed voters in the Class 9 town, while they remain tax-exempt non-voters in the Class 8 town.

This advantage is further reinforced by Article 1b and the accompanying Table in the Electoral Law, which establishes the safety net requirements for those not fully assessed under the tax law. This table scales the required minimum rent and income according to the municipality's economic standing; just as with the tax thresholds, the requirements for poorer municipalities are set lower. Therefore, a citizen with a marginal income (e.g., earning just enough to qualify in a cheap village but not an expensive one) would satisfy the Electoral Law table's criteria in the Class 9 municipality but fail them in the Class 8 municipality. By lowering the bar on both the primary "taxpayer" route and the secondary "table" route, the Class 9 designation captures a larger slice of the population distribution, resulting in higher enfranchisement per capita.

Under these specific conditions, being assigned to Class 9 (the "poorer" class) acts as an institutional advantage for enfranchisement. Empirically, this is shown in Figure 2.1. Municipalities allocated to higher classes have a higher increase in suffrage per capita in 1897 relative to their level in 1896.

Figure 2.1: Change in suffrage according to class rank



Furthermore, these imbalances were also noticed by lower level authorities, e.g. by the Provincial Estates.

> In establishing the table referred to in Articles 1 and 2 of the Electoral Law, the table for the division of municipalities into classes for the levying of the National Personal Tax was followed.

> We point out that this classification scheme, as far as the province of North Brabant is concerned, is strikingly incorrect.
>
> This is all the more regrettable because these inaccuracies have now already been adopted in three different laws, namely: in the Electoral Law, in the Personal Tax Act, and in the law regulating the financial relationship between the State and the municipalities.
>
> For example, it is inexplicable why the municipality of 's-Hertogenbosch is in the 4th class and the municipalities of Breda and Tilburg (center) are in the 6th class, nor why the municipality of Waalwijk is placed on equal footing with Breda and Tilburg and is even classified higher than Roosendaal and Eindhoven; why these latter two municipalities are again placed on equal footing with a remote corner of the municipality of Mierlo, [specifically] 't Hout; why municipalities such as Zevenbergen, the center of Veghel, and others rank equally with municipalities such as Cromvoirt, Engelen, and Esch; [and] why, for example, some municipalities (..) are in a lower class than [others].

In sum, this provides evidence that the arbitrary division of municipalities into classes caused random and exogenous variation in the amount of voters per capita.

3 Data and Method

3.1 Data

We draw on four data sources, which we link at the municipality level using the *amco* identifier from the Historische Database van Nederlandse Gemeenten (HDNG).

Kieswet classification table. The 1896 Personal Taxation Law assigned each Dutch municipality to one of nine income classes that determined the income threshold for voter eligibility under the concurrently enacted Kieswet. We digitize the classification table appended to this law—a multi-page printed table—using a vision-language model (Gemini), which transcribes each page into a structured format via a predefined schema. This yields a municipality-level dataset recording each gemeente's class rank (1–9). We merge these records with the HDNG using a hand-curated linkage key that reconciles historical name variants.

Suffrage counts, 1896–1897. Data on the number of eligible voters for municipal council elections (*Gemeenteraad*) in 1896 and 1897 come from the *Provinciale Verslagen*—annual administrative reports published by each of the eleven Dutch provinces. We locate the relevant voter tables in scanned JPEG and PDF images and transcribe them using Gemini vision, applying province-specific extraction schemas to accommodate variation in table layout across provinces. Where municipality names differ between the Kieswet table and the provincial reports, we construct a linkage key using a fuzzy string-matching algorithm (similarity threshold 0.85), with ambiguous cases resolved manually. Four municipalities with duplicate names across provinces (Bergen, Laren, Rijswijk, Sloten) are excluded. Dividing eligible voter counts by male population (from the HDNG) yields our per-capita suffrage measures.

HDNG (Historische Database van Nederlandse Gemeenten). The HDNG v4 dataset, obtained from IISG Amsterdam, provides municipality-level statistics compiled from Dutch administrative censuses. We draw on total male population, total personal tax revenues (*totaal personele belastingen*) for various years from 1859 to 1889, and religious denomination counts from the 1889 population census. These series form the basis of our control variables (Section 3.4).

Provinciale Verslagen. To construct medium-run outcome variables, we additionally extract data from the 1903 provincial reports using an agentic multi-page extraction pipeline. For each municipality we recover: (i) the number of eligible voters under the Kieswet (*kiesrecht*); (ii) the number of votes cast in municipal elections (*gemeentelijkeverkiezingen*); and (iii) total municipal expenditures (*gemeentelijkeuitgaven*). These series are matched to the 1896–1897 analysis dataset by province and municipality name.

The final cross-sectional dataset covers all Dutch municipalities for which data are available from all four sources. All regressions include province fixed effects, absorbing province-level heterogeneity in administrative practices, reporting conventions, and pre-existing institutional conditions.

3.2 Method

Our empirical strategy exploits cross-sectional variation in the suffrage shock induced by the 1896 Electoral Law. The key endogenous variable is $\Delta\text{Suffrage}_{i,\text{pc}} \equiv (\text{Voters}_{i,1897} - \text{Voters}_{i,1896})/\text{Population}_{i,1896}$, the change in eligible voters per capita between 1896 and 1897. We instrument for this shock using the municipality's class rank under the 1896 Personal Taxation Law, which determines the income threshold for voter eligibility. The first-stage regression for municipality i in province j is:

$$\Delta\text{Suffrage}_{i,j,\text{pc}} = \alpha_j + \delta_1 \text{Class Rank}_i + \delta_2 \mathbf{X}_{i,j} + \nu_{i,j},$$

where α_j are province fixed effects, $\mathbf{X}_{i,j}$ is a vector of controls (Section 3.4), and $\text{Class Rank}_i \in \{1, \ldots, 9\}$ is the rank assigned to municipality i in the table appended to the 1896 Personal Taxation Law. Since a higher class rank raises the income threshold, it mechanically enfranchises a larger share of the male population, so we expect $\delta_1 > 0$.

Using the class rank as an instrument, we estimate the following second-stage specification for outcome $Y_{i,j}$:

$$Y_{i,j} = \alpha_j + \beta_1 \Delta\widehat{\text{Suffrage}}_{i,j,\text{pc}} + \beta_2 \mathbf{X}_{i,j} + \epsilon_{i,j},$$

where $\Delta\widehat{\text{Suffrage}}_{i,j,\text{pc}}$ is the fitted value from the first stage. We study three sets of outcomes. First, to test whether the initial franchise shock propagated over time, we use the medium-run change in suffrage per capita, $\Delta\text{Suffrage}_{i,\text{pc},1897\text{–}1903}$. Second, to estimate fiscal consequences, we use total municipal expenditures per capita in 1903 (in both levels and logs). Third, to disentangle the mechanism, we use disaggregated per-capita expenditures across four categories: governance, tax compliance, education, and poor relief. All regressions include province fixed effects and are estimated with heteroskedasticity-robust standard errors. We also report OLS benchmarks alongside the IV results to assess the direction of any endogeneity bias.

3.3 Outcome Variables

We study three sets of outcomes, all measured at the municipality level.

Suffrage. To study whether the initial franchise shock propagated over subsequent years, we construct the medium-run change $\Delta\text{Suffrage}_{i,\text{pc},1897\text{–}1903} \equiv \text{Suffrage}_{i,\text{pc},1903} - \text{Suffrage}_{i,\text{pc},1897}$ using the *Provinciale Verslagen*.

Voter turnout. As a measure of democratic engagement, we construct $\text{Turnout}_{i,1903} \equiv \text{Votes cast}_{i,1903}/\text{Eligible voters}_{i,1903}$, the share of eligible voters who participated in the 1903 municipal elections. We trim this variable at the 98th percentile to address implausible values arising from transcription or aggregation errors in the original records.

Municipal expenditures. The fiscal outcomes are expenditures per capita, $\text{Expenditures}_{i,\text{pc},1903} \equiv \text{Total expenditures}_{i,1903}/\text{Population}_{i,1896}$, and its natural logarithm $\log(\text{Total expenditures}_{i,1903})$. Together these allow us to quantify both the level and proportional fiscal consequences of suffrage expansion six years after the initial reform.

3.4 Control Variables

Municipal income. Central to our identification strategy is the argument that class assignment was administratively arbitrary conditional on a municipality's income level. To ensure comparability along this dimension, we include $\log(\text{Tax revenue}_{i,1889})$, the log of total personal tax revenues (*totaal personele belastingen*) assessed in 1889, as a proxy for the municipal income distribution. This variable predates the 1896 reform and is thus predetermined with respect to the treatment.

Population and population density. We additionally control for $\log(\text{Population}_{i,1896})$ to absorb scale differences across municipalities. Using yearly population counts from the HDNG, and shapefiles of the Dutch municipal landscape (NLGIS), we also construct a measure of population density for each municipality. Population density might be correlated with both suffrage and municipal spending decisions, e.g. due to economies of scale — denser areas require less linear infrastructure per resident, reducing maintenance costs in aspects like infrastructure.

Religious affiliation. From the 1889 Dutch population census (via the HDNG), we construct the share of the population affiliated with the Roman Catholic Church and an aggregated Protestant share, combining the Netherlands Reformed Church (*Nederlands Hervormd*), the Reformed Churches (*Gereformeerde Kerken*), and several smaller Protestant denominations. Both shares are expressed as fractions of the total enumerated believers. Religious composition may correlate with political economy outcomes independently of income, and Catholic communities in particular were geographically concentrated in the southern provinces where income distributions differed systematically from the predominantly Protestant north.

4 Results

4.1 First Stage

Table 5.1 shows the first-stage results, confirming a statistically significant relationship between a municipality's class rank and its change in suffrage per capita between 1896 and 1897. Across all specifications, an increase of one step in class rank is associated with an increase of approximately 1.5 percentage points in Δ suffrage per capita (columns 1–3), or equivalently in the level of suffrage per capita in 1897 conditional on the 1896 baseline (columns 4–6). The estimates are precise: the coefficient ranges from 0.015 to 0.018 and carries a t-statistic well above conventional instrument-strength thresholds in every specification. The stability of the estimate across specifications that progressively add logged population, pre-reform tax revenue, and religious composition as controls is reassuring—it suggests that the administrative class assignment carries information about the income threshold for voter eligibility that is orthogonal to the observable municipal characteristics that determine local demand for redistribution.

The restricted sample in columns 3 and 6, which retains only municipalities in the upper portion of the class hierarchy (class rank > 7), yields coefficients of identical magnitude (0.015). This bandwidth restriction trades some precision for a sharper comparison of municipalities that are most similar in terms of their position in the class hierarchy, and confirms that the first-stage relationship is not driven by extreme urban observations.

4.2 Dynamic Effects on Suffrage

Table 5.2 presents OLS and IV estimates of the effect of the 1896–1897 suffrage shock on subsequent suffrage growth between 1897 and 1903. The OLS estimates in columns 1–3 show a large, precisely estimated *negative* association: municipalities that experienced a larger expansion of the franchise in 1897 had, on average, slower subsequent growth in suffrage per capita by 1903. The coefficient ranges from -0.293 to -0.462, depending on the inclusion of controls and the sample restriction. This pattern is consistent with mechanical mean reversion—municipalities that expanded suffrage most sharply in 1897 were closer to their saturation level within the income-and-rent franchise, leaving less room for further expansion—but could also reflect endogeneity in the assignment of the initial shock itself.[1]

When we instrument for the 1896–1897 suffrage shock using class rank, the estimated effect changes dramatically. The IV estimate in the fully specified model (column 5) is -0.017 (s.e. 0.156), indistinguishable from zero. In the restricted sample (column 6), the point estimate turns positive at 0.378, though it remains statistically insignificant (s.e. 0.463). The sharp attenuation once we move to IV estimates confirms that the negative OLS relationship was driven by endogeneity or mean reversion rather than a genuine causal channel from the initial shock to subsequent democratization.

These results suggest that over the six-year window to 1903, an exogenous expansion of the initial franchise did not trigger a statistically detectable further expansion of voting rights. This null result on the multiplier within this horizon contrasts with longer-run dynamic effects of the kind theorized by Jack and Lagunoff (2006), who show formally that partial franchise extension can be self-reinforcing when newly enfranchised citizens' private decisions generate positive spillovers on the current median voter's payoff. On the other hand, the broader literature on democratic self-reinforcement (Acemoglu et al., 2025) also suggests that the conditions for self-sustaining democratization can require time to materialize.[2] Hence, it is possible that the political dynamics emanating from the

[1] There is also a purely mechanical source of negative bias in this regression. Because 1897 serves simultaneously as the endpoint of the independent variable ($\Delta S_{1897\text{–}1896}$) and the starting point of the dependent variable ($\Delta S_{1903\text{–}1897}$), any transitory shock or measurement error in the 1897 observation enters both differences with opposite signs. This mechanically depresses the OLS coefficient even when the true causal effect is zero. See Appendix B for a formal derivation.

[2] The next major franchise reform in the Netherlands—universal male suffrage—did not arrive until 1917, more than two decades after the Kieswet.

1896-1897 reform likely operated on a timescale that our six-year window cannot capture.

[Table 5.2 here]

4.3 Municipal Expenditures

Tables 5.3 and 5.4 turn to the fiscal consequences of the 1896–1897 suffrage shock. The dependent variable is municipal expenditures per capita in 1903 (Table 5.3) and log total municipal expenditures (Table 5.4), both measured approximately six years after the initial enfranchisement.

The OLS estimates reveal a familiar pattern of endogeneity. In the bivariate specification (column 1 of Table 5.3), a larger 1897 suffrage shock is negatively associated with later expenditures per capita (-24.192), but this coefficient collapses to a statistically insignificant 3.212 once observable municipal characteristics are controlled for (column 2). A similar pattern holds in the log specification: the raw OLS coefficient of -3.397 turns small and insignificant (0.289) with controls. The sign reversal and loss of precision in the controlled OLS are consistent with a scenario in which municipalities with higher pre-reform incomes both expanded suffrage less—because their income thresholds were harder to meet—and simultaneously maintained higher expenditure levels, creating a spurious positive correlation between suffrage expansion and spending that masks the underlying causal relationship.

The IV estimates tell a starkly different story. Instrumenting for the suffrage shock with class rank, the effect on municipal expenditures per capita is large and highly significant: -171.450 with controls (column 5 of Table 5.3), and -94.252 in the restricted sample (column 6). The log specification yields an IV coefficient of -6.775 with controls (column 5 of Table 5.4), implying that a one-percentage-point exogenous increase in the suffrage shock is associated with approximately a 6.8 % reduction in municipal expenditures by 1903.

[Table 5.3 here]

[Table 5.4 here]

The negative causal effect of franchise expansion on expenditures is, at first glance, at odds with the canonical Meltzer–Richard prediction that a broader electorate shifts the decisive voter toward higher redistribution and public spending (Meltzer and Richard, 1981; Aidt et al., 2006; Lindert, 1994). However, as Aidt and Mooney (2014) argue, the fiscal direction of franchise extension depends critically on the *type* of voters enfranchised. When suffrage is extended to taxpaying ratepayers rather than to the unpropertied poor, newly eligible voters may prefer lower spending and tighter fiscal discipline—since they bear the tax burden of additional expenditure without deriving disproportionate benefit from redistributive programs. The 1896 Kieswet fits this description precisely: the reform admitted men who satisfied income, rent, or savings thresholds, admitting essentially middle-income ratepayers rather than the unpropertied working class.

This interpretation connects directly to earlier work by Chapman (2018, 2024), who document in the English local government context that franchise extensions targeting middle-class ratepayers were followed by reductions in public goods expenditure, as newly dominant taxpayers demanded fiscal restraint. Our Dutch results confirm this pattern in a different national setting. Importantly, they contrast with the findings of Chapman (2026), who shows that the 1894 democratization of English Poor Law boards—a reform that extended the franchise to a genuinely poorer and more equal electorate—led to *increases* in poor relief spending, consistent with the Meltzer–Richard mechanism, and that these increases were larger in areas of higher income inequality. This suggests that the fiscal consequences of democratization are conditional on who enters the electorate: middle-class enfranchisement compresses public expenditure, while working-class enfranchisement expands redistribution. This conditionality of fiscal responses echoes the broader heterogeneity documented in cross-national work by Aidt et al. (2006) and Aidt and Jensen (2013).

4.4 Efficiency vs. Redistribution

The analysis in Subsection 4.3 aggregates all municipal spending. An alternative explanation focuses on an efficiency motive rather than on preferences for redistribution. Newly enfranchised voters could serve as an impetus to increase efficiency and cut municipal spending. If this is true, we should see a significant drop in administrative overhead, salaries, or general waste, while substantive spending on the poor or public goods remains constant.

Table 5.5 tests this by decomposing municipal expenditures into four categories: governance (personnel and office costs), tax compliance (collection costs), education (primary, secondary, higher, arts and sciences), and poor aid (medical care, asylum costs, charitable subsidies). For each category we estimate the same IV specification as before—instrumenting the 1896–1897 suffrage shock with class rank, controlling for province fixed effects, population density, log tax revenue, and religious composition—separately on the full sample and on municipalities with class rank above 7.

The results reveal a clear pattern. IV estimates for *governance* expenditure per capita are large and precisely estimated: -11.144 in the full sample and -13.171 in the restricted sample. *Tax compliance* costs show a similarly robust negative effect: -1.508 and -0.715

respectively. By contrast, the IV estimates for *education* and *poor aid* are imprecise and statistically indistinguishable from zero across both samples. For education, the point estimates are large in absolute value (-43.002 and 22.018) but carry standard errors of 27.430 and 72.105, reflecting the high variance in this spending category. For poor aid, the point estimates are small (-3.146 and -2.181) and statistically insignificant in both samples.

[Table 5.5 here]

This decomposition resolves the interpretation of the aggregate fiscal effect. The total expenditure decline documented in Table 5.3 is driven by reductions in administrative overhead and tax collection costs, not by cuts to substantive public services. Municipalities where the franchise extended more broadly—as induced by their class assignment—became administratively leaner: they spent less on running the municipal apparatus and on collecting their revenues, while maintaining the same level of educational provision and poor relief. This pattern is precisely what one would expect if newly enfranchised middle-class ratepayers demanded efficient, low-overhead government—bearing the cost of municipal taxation, they had strong incentives to reduce waste—rather than demanding cuts to programs that aided the poor or supported public education.

These results speak directly to the debate between the *retrenchment hypothesis* and the *efficiency hypothesis* in the franchise extension literature. Aidt et al. (2010) and Chapman (2018, 2024) document that middle-class franchise extensions in nineteenth-century England reduced local public goods expenditure, consistent with retrenchment: newly dominant ratepayers cut spending on sanitation and infrastructure because they bore the tax cost without proportionate benefit. Our Dutch results identify a more nuanced channel. The aggregate spending decline does not reflect retrenchment of redistributive programs—poor aid is unaffected—but rather a compression of the overhead costs of municipal government. The distinction matters because it shifts the welfare interpretation: an efficiency-driven contraction implies that the same level of public services is delivered at lower cost, whereas a retrenchment story implies that public goods provision itself falls.

Aidt and Mooney (2014) offer a related insight from the London Metropolitan Boroughs: taxpayer suffrage shifts the composition of public spending rather than simply reducing it. Our results are consistent with this framing. The franchise character in the 1896 Kieswet—extended to taxpaying ratepayers satisfying income, rent, or savings thresholds—generated political pressure for fiscal discipline in overhead categories, while leaving intact the substantive services that even middle-class voters valued. This stands in contrast to the findings of Chapman (2026), who shows that the 1894 democratization of English Poor Law boards—a reform that enfranchised a genuinely poorer electorate—led to *increases* in poor relief, consistent with a Meltzer-Richard redistribution mechanism.

Taken together, these findings confirm that the fiscal direction of franchise extension is determined by the composition of the newly enfranchised group: middle-class taxpayer enfranchisement generates efficiency gains without retrenchment; working-class enfranchisement generates redistributive expansion.

5 Conclusion

This paper exploits the administrative misclassification of Dutch municipalities under the 1896 Kieswet to identify the causal effects of franchise extension on subsequent democratization and local fiscal policy. The 1896 Electoral Law assigned municipalities to nine classes that determined the income threshold for voter eligibility; because this classification was widely acknowledged to be imprecise, class assignment generates plausibly exogenous variation in the size of the 1896–1897 suffrage shock. Using class rank as an instrument, we establish three results. First, the first stage is strong: a one-step increase in class rank raises the change in suffrage per capita by approximately 1.5 percentage points, with t-statistics well above conventional instrument-strength thresholds. Second, this exogenous suffrage shock did not trigger further franchise expansion over the subsequent six years—the IV estimate of the multiplier effect is close to zero and statistically insignificant, in sharp contrast to the large negative OLS coefficient, which reflects mean reversion and endogeneity in the initial reform. Third, the suffrage shock had a large negative causal effect on total municipal expenditures per capita by 1903: -171 guilders in levels and approximately -6.8% in logs, both precisely estimated.

Decomposing this aggregate effect reveals the mechanism. The entire expenditure decline is driven by falls in governance costs—personnel, office expenses, and general administrative overhead—and in tax compliance costs, the cost of collecting municipal revenues. Expenditures on education and poor relief, by contrast, show no statistically significant response in either the full sample or the restricted sample of municipalities with higher class ranks. This pattern is precisely consistent with an efficiency hypothesis: newly enfranchised middle-class ratepayers, who bore the direct cost of municipal taxation, demanded leaner government and lower collection overhead, but did not seek to dismantle the substantive services they and their communities used. The result stands in contrast to the retrenchment found in certain nineteenth-century English contexts (Aidt et al., 2010; Chapman, 2018, 2024), where franchise extension reduced public goods provision more broadly. It also contrasts with the findings of Chapman (2026), who shows that the 1894 democratization of English Poor Law boards—a reform that enfranchised a genuinely poorer electorate—raised poor relief spending, consistent with the Meltzer-Richard redistribution mechanism. The juxtaposition underscores a lesson increasingly documented across settings: the fiscal direction of franchise extension depends critically on who enters the electorate.

These findings speak to three broader debates. On democratic self-reinforcement, the null multiplier result suggests that the conditions under which partial franchise extension is self-sustaining (Jack and Lagunoff, 2006) either require longer time horizons than six years, or depend on the type of voters enfranchised and the policy gains they are able to secure—consistent with the evidence in Acemoglu et al. (2025) that democratic consolidation is driven by democracies that deliver. On the conditionality of fiscal responses to franchise extension, our evidence confirms and deepens the view in Aidt and Mooney (2014) that the character of the suffrage—taxpayer versus universal—shapes the composition, not just the level, of public spending: middle-class enfranchisement in the Netherlands produced efficiency without retrenchment. On identification, our use of an administrative classification discontinuity contributes to a methodological literature alongside Naidu (2012), Bernini et al. (2023), and Larcinese (2024) that exploits institutional rules to isolate the causal consequences of changes in franchise eligibility. Together, the results underscore that democratization is neither uniformly redistributive nor uniformly retrenchment-producing: its economic consequences are conditional on the institutional rules and social composition that define who counts as a citizen.

Table 5.1: Influence of Rank on Suffrage per Capita

	Delta Suffrage per Capita			Suffrage per Capita 1897		
Sample:	All Municipalities		Class Rank > 7	All Municipalities		Class Rank > 7
	(1)	(2)	(3)	(4)	(5)	(6)
Class Rank	0.018***	0.015***	0.015***	0.018***	0.015***	0.015***
	(0.002)	(0.004)	(0.004)	(0.001)	(0.003)	(0.004)
Log Population		0.028***	0.016***		-0.006	0.010
		(0.006)	(0.005)		(0.011)	(0.006)
Log Tax Revenue 1889		-0.029***	-0.020***		-0.001	-0.014**
		(0.005)	(0.004)		(0.009)	(0.006)
Share Catholics 1889		-0.303*	-0.071		-0.116	-0.053
		(0.176)	(0.180)		(0.126)	(0.182)
Share Protestants 1889		-0.291*	-0.061		-0.101	-0.043
		(0.175)	(0.180)		(0.126)	(0.181)
Suffrage per Capita 1896				0.591***	0.555***	0.903***
				(0.028)	(0.174)	(0.050)
R2 Adj.	0.281	0.322	0.278	0.446	0.455	0.506
R2 Within Adj.	0.103	0.154	0.063	0.365	0.376	0.417
Num.Obs.	1112	1112	888	1112	1112	888
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes

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

Table 5.2: IV Estimates: Effect of 1896-1897 Suffrage Shock on Suffrage Growth 1897-1903

	OLS			IV (Instrument: Class Rank)		
Sample:	All Municipalities		Class Rank > 7	All Municipalities		Class Rank > 7
	(1)	(2)	(3)	(4)	(5)	(6)
Delta Suffrage p.c. 1896-1897	-0.293***	-0.300***	-0.462***	-0.170*	-0.017	0.378
	(0.027)	(0.093)	(0.049)	(0.088)	(0.156)	(0.463)
Log Population		0.010**	0.018***		0.001	-0.002
		(0.004)	(0.004)		(0.007)	(0.013)
Log Tax Revenue 1889		-0.006	-0.011**		0.005	0.012
		(0.004)	(0.004)		(0.007)	(0.015)
Share Catholics 1889		0.309	-0.038		0.338	0.185
		(0.221)	(0.145)		(0.227)	(0.250)
Share Protestants 1889		0.295	-0.046		0.320	0.170
		(0.221)	(0.145)		(0.226)	(0.247)
R2 Adj.	0.138	0.157	0.228	0.037	0.061	0.057
R2 Within Adj.	0.107	0.127	0.206	0.003	0.027	0.029
Num.Obs.	952	952	749	952	952	749
Province FE	Yes	Yes	Yes	Yes	Yes	Yes

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

Table 5.3: IV Estimates: Effect of 1896-1897 Suffrage Shock on Municipal Expenditures per Capita 1903

	OLS			IV (Instrument: Class Rank)		
Sample:	All Municipalities		Class Rank > 7	All Municipalities		Class Rank > 7
	(1)	(2)	(3)	(4)	(5)	(6)
Delta Suffrage p.c. 1896-1897	-41.837***	-0.710	6.433	-407.311***	-317.174***	-116.207*
	(10.110)	(9.462)	(8.249)	(39.981)	(104.539)	(63.354)
Log Population		-2.186**	-2.526***		7.655**	0.342
		(1.094)	(0.924)		(3.008)	(1.805)
Log Tax Revenue 1889		5.991***	1.211		-6.984**	-2.324
		(1.181)	(0.934)		(3.346)	(2.249)
Share Catholics 1889		-495.429***	-242.514***		-539.338***	-275.271***
		(61.013)	(74.404)		(65.320)	(77.061)
Share Protestants 1889		-492.611***	-238.529***		-533.992***	-270.841***
		(61.051)	(74.149)		(65.366)	(76.672)
R2 Adj.	0.074	0.296	0.113	0.297	0.359	0.119
R2 Within Adj.	0.017	0.253	0.050	0.254	0.320	0.057
Num.Obs.	961	961	758	961	961	758
Province FE	Yes	Yes	Yes	Yes	Yes	Yes

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

Table 5.4: IV Estimates: Effect of 1896-1897 Suffrage Shock on Log Municipal Expenditures 1903

	OLS			IV (Instrument: Class Rank)		
Sample:	All Municipalities		Class Rank > 7	All Municipalities		Class Rank > 7
	(1)	(2)	(3)	(4)	(5)	(6)
Delta Suffrage p.c. 1896-1897	-4.309***	0.191	0.753*	-37.359***	-9.248***	-4.396*
	(0.749)	(0.303)	(0.440)	(3.356)	(2.933)	(2.581)
Log Population		0.801***	0.794***		1.095***	0.914***
		(0.040)	(0.044)		(0.085)	(0.079)
Log Tax Revenue 1889		0.294***	0.146***		-0.093	-0.003
		(0.040)	(0.045)		(0.097)	(0.093)
Share Catholics 1889		-14.454***	-10.774***		-15.763***	-12.150***
		(2.106)	(2.457)		(2.171)	(2.675)
Share Protestants 1889		-14.307***	-10.582***		-15.542***	-11.938***
		(2.105)	(2.453)		(2.171)	(2.666)
R2 Adj.	0.208	0.867	0.834	0.496	0.875	0.834
R2 Within Adj.	0.033	0.838	0.759	0.384	0.848	0.759
Num.Obs.	961	961	758	961	961	758
Province FE	Yes	Yes	Yes	Yes	Yes	Yes

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

Table 5.5: IV Estimates: Effect of 1896-1897 Suffrage Shock on Municipal Expenditures per Capita 1903

Sample:	Governance		Tax Compliance		Education		Poor Aid	
	All	Class Rank > 7	All	Class Rank > 7	All	Class Rank > 7	All	Class Rank > 7
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Delta Suffrage p.c. 1896-1897	-11.144***	-13.171**	-1.508***	-0.715*	-43.002	22.018	-3.146	-2.181
	(3.620)	(5.134)	(0.561)	(0.433)	(27.430)	(72.105)	(4.744)	(8.091)
Population Density	0.000	-0.001	0.000	0.000	0.001	0.001	-0.000	0.000
	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.005)	(0.000)	(0.001)
Log Tax Revenue 1889	-0.600***	-0.755***	0.001	-0.005	-1.939**	-2.499***	0.077	0.018
	(0.064)	(0.076)	(0.007)	(0.006)	(0.860)	(0.936)	(0.096)	(0.122)
Share Catholics 1889	-7.901	-3.883	-0.895	-0.271	-32.757	35.279	3.951	5.783
	(4.943)	(6.817)	(0.669)	(0.464)	(32.339)	(51.698)	(5.696)	(9.739)
Share Protestants 1889	-7.825	-3.767	-0.885	-0.261	-28.655	40.150	4.434	6.144
	(4.940)	(6.801)	(0.671)	(0.465)	(32.778)	(52.929)	(5.687)	(9.700)
R2 Adj.	0.458	0.529	0.202	0.207	0.064	0.081	0.305	0.350
R2 Within Adj.	0.146	0.202	0.062	0.020	0.016	0.034	0.005	-0.001
Num.Obs.	1112	888	1112	888	1112	888	1112	888
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

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A Background of Suffrage in the Netherlands

Table A.1: Status Quo of Suffrage Criteria in the Netherlands (1848, 1850, 1887, 1896)

Year	Legal Basis	Age	Sex	Financial & Capability Criteria (Census)	Procedural & Exclusionary Notes
1848	*Voorloopig Kiesreglement* (Provisional Regulation)	23	M	**Direct Taxation Census:** Payment of direct taxes between **20 and 160 guilders**. **Urban:** Based on existing city regulations for voting eligibility. **Rural:** Based on regulations for Provincial States voting rights (with adjusted table).	**Procedure:** Lists created by municipal councils; public inspection for 8 days. **Context:** First direct elections; single-member districts introduced.
1850	*Kieswet 1850* (Electoral Law)	23	M	**Direct Taxation Census:** Assessment (*aangeslagen zijn*) in: • Land Tax (*Grondbelasting*) • Personnel Tax (*Personele belasting*) • Patent Tax (*Patentbelasting*) Includes State Surcharges (*Rijksopcenten*). Excludes provincial/municipal surcharges. Per municipality table (max 160 fl, Amsterdam 116 fl).	Married women explicitly excluded (husband deemed payer). Widows/unmarried women practically excluded. Automatic placement on list by municipality based on tax collector data. Limited objection rights.
1887	Constitution Revision & Additional Articles	23	M	**Criteria:** "Signs of capability and social well-being." **Taxes:** • Patent tax removed. • *Land Tax:* ≥ 10 guilders. • *Personnel Tax:* Assessed for rental value > threshold for reduction. **Housing:** Sub-tenants (*onderhuurders*) eligible if rented part value > reduction threshold and occupied for **9 months** prior to Feb 15.	Explicit exclusion of those receiving public charity/welfare (*onderstand*) in the past year, and those with unpaid taxes. Introduction of self-declaration (*aangifte*) for lodgers; tax voters still largely automatic.
1896	*Kieswet 1896* (Van Houten)	25	M	**Expansion of signs of capability:** • **Tax Voters:** Assessment ≥ 1 fl in Land, Wealth, Business, or specific Personnel tax bases. • **Rent Voters:** Tenants of houses/apartments. • **Wage Voters:** Based on annual wage earnings. • **Savings Voters:** Based on savings balance. • **Exam Voters:** Based on academic degrees.	Age raised from 23 to 25. Generalized self-declaration (*aangifte*) available for all; mandatory for new categories. Introduction of formal candidate lists; voting by marking a box rather than writing a name.

B OLS Bias in the Regression of Consecutive Suffrage Changes

This appendix formalizes why a regression of $\Delta S_{i,1903-1897}$ on $\Delta S_{i,1897-1896}$ yields a mechanically negative OLS coefficient, and why instrumenting with class rank corrects this bias.

Setup. Let $S_{i,t}$ denote observed suffrage per capita in municipality i at time t. Decompose observed suffrage into a structural component and a transitory shock:

$$S_{i,t} = S^*_{i,t} + u_{i,t}, \tag{1}$$

where $S^*_{i,t}$ is the true, persistent suffrage level and $u_{i,t}$ is a mean-zero, serially uncorrelated idiosyncratic shock with variance σ_u^2 (arising from measurement error, temporary administrative irregularities, or transient local political events). Define the independent variable as the first difference and the dependent variable as the second:

$$X_i = \Delta S_{i,1} = S_{i,1897} - S_{i,1896} = (S^*_{i,1897} - S^*_{i,1896}) + (u_{i,1897} - u_{i,1896}), \tag{2}$$

$$Y_i = \Delta S_{i,2} = S_{i,1903} - S_{i,1897} = (S^*_{i,1903} - S^*_{i,1897}) + (u_{i,1903} - u_{i,1897}). \tag{3}$$

The structural relationship of interest is

$$(S^*_{i,1903} - S^*_{i,1897}) = \beta\,(S^*_{i,1897} - S^*_{i,1896}) + \varepsilon_i. \tag{4}$$

OLS bias. Because 1897 is both the endpoint of X_i and the starting point of Y_i, the 1897 shock $u_{i,1897}$ enters both differences with *opposite* signs. Consequently,

$$\mathrm{Cov}(X_i,\, Y_i) = \beta\,\mathrm{Var}(\Delta S^*_{i,1}) - \sigma_u^2, \tag{5}$$

while $\mathrm{Var}(X_i) = \mathrm{Var}(\Delta S^*_{i,1}) + 2\sigma_u^2$. The probability limit of the OLS estimator is therefore

$$\mathrm{plim}\,\hat{\beta}_{\mathrm{OLS}} = \frac{\beta\,\mathrm{Var}(\Delta S^*_{i,1}) - \sigma_u^2}{\mathrm{Var}(\Delta S^*_{i,1}) + 2\sigma_u^2}. \tag{6}$$

Even when the true causal effect is zero ($\beta = 0$), OLS converges to $-\sigma_u^2/[\mathrm{Var}(\Delta S^*_{i,1})+2\sigma_u^2] < 0$. The bias is therefore structural: any positive variance in the 1897 shock is sufficient to produce a spurious negative OLS estimate. This is the classic regression-to-the-mean problem—municipalities that experienced an unusually large measured expansion in 1897 (a positive $u_{i,1897}$) mechanically appear to grow more slowly thereafter, as the transitory component reverts.

IV correction. Let Z_i denote a municipality's class rank under the 1896 Kieswet. Because class rank is a rigid, administratively determined threshold rule, it predicts structural suffrage changes ($\mathrm{Cov}(Z_i, \Delta S^*_{i,1}) \neq 0$) while being uncorrelated with transitory shocks

$(\mathrm{Cov}(Z_i, u_{i,t}) = 0)$. The numerator of the IV estimator is

$$\mathrm{Cov}(Z_i,\, Y_i) = \mathrm{Cov}\Big(Z_i,\ \beta\, \Delta S^*_{i,1} + \varepsilon_i + u_{i,1903} - u_{i,1897}\Big) = \beta\, \mathrm{Cov}(Z_i,\, \Delta S^*_{i,1}), \tag{7}$$

since the shock terms are orthogonal to Z_i. It follows that

$$\operatorname{plim} \hat{\beta}_{\mathrm{IV}} = \frac{\beta\, \mathrm{Cov}(Z_i,\, \Delta S^*_{i,1})}{\mathrm{Cov}(Z_i,\, \Delta S^*_{i,1})} = \beta. \tag{8}$$

The IV estimator purges the transitory 1897 shock from the first-stage variation, recovering the true structural effect. The attenuation of the negative OLS coefficient to near-zero in the IV estimates reported in Table 5.2 is therefore consistent with $\beta \approx 0$: there is no structural causal relationship between the initial suffrage shock and subsequent suffrage growth over this horizon.