

# Dynamics of Democratization

## Evidence from Municipal Suffrage Extensions

Bas Machielsen   Bram van Besouw

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# Introduction

# Why Study Franchise Extension?

- Democratization is one of the most consequential institutional changes a society can undergo
- A longstanding question: does partial enfranchisement create pressure for *further* democratization?
  - Newly enfranchised voters may demand broader inclusion over time
  - Or: elites may arrest further extension after conceding limited reforms
- Separately: what are the *economic* consequences of franchise extension?
  - Meltzer–Richard (1981): more voters → median voter poorer → more redistribution
  - But this logic hinges on *who* gets enfranchised

**This paper provides causal evidence on both questions using a novel natural experiment from the Netherlands**

# This Paper

**Setting:** The Netherlands, 1896 Electoral Law (*Kieswet*) reform

- Extended male voting rights to those meeting income/rental thresholds
- Thresholds differed by municipality class (9 classes, 1–9)
- Administrative errors in class assignment generate quasi-random variation

**Two research questions:**

1. Does an initial suffrage expansion trigger *further* franchise extensions within 6 years?
2. What are the fiscal consequences of franchise extension for municipal expenditures?

**Preview of findings:**

- No evidence of a franchise multiplier effect (1897 → 1903)
- Larger suffrage expansions → *lower* municipal expenditures: composition matters
- Spending decline is **efficiency-driven**: administrative overhead falls; education and poor relief are unaffected

## Historical Background

# The 1896 Electoral Law Reform

- Before 1896: franchise based on direct tax payments; very limited (12% of adult males)
- *Kieswet* 1896: extended eligibility to males 25+ who met **income or rental thresholds**
  - Option 1: assessed under Personal Tax Law (*Wet op de Personeele Belasting*)
  - Option 2: renting above a class-specific weekly threshold, or earning above annual income threshold
- Example: in 's-Hertogenbosch (Class 4), threshold = 1.75 guilders/week rent or 450 guilders/year income
- Example: in Besoijen (Class 9), threshold = 1.00 guilder/week rent or 325 guilders/year income
- Reform substantially expanded the electorate, but unevenly across municipalities

# The Municipality Classification System

- Municipalities assigned to **9 classes** based on wealth/size; Class 1 = largest, Class 9 = smallest
- Higher class rank → higher income threshold → *larger* marginal expansion of the electorate
- **Key source of variation:** misclassification was widespread and noted by contemporaries
  - Wealthy 's-Hertogenbosch placed in Class 4; poorer Breda and Tilburg in Class 6
  - Provincial Estates described classifications as “strikingly incorrect”
- Misclassification plausibly uncorrelated with underlying economic conditions conditional on controls
- **Instrument:** class rank → magnitude of suffrage expansion in 1896–1897

Data

# Data Pipeline

Building this dataset required digitizing multiple historical sources:

1. **Kieswet class table**: multi-page PDF of all municipality class assignments
  - Digitized using Gemini vision model with structured output schema
  - Merged with HDNG municipality codes (*amco*) via hand-curated key
2. **Suffrage counts 1896/1897** (scanned provincial report images):
  - 9 provinces, 4 different table structures → province-specific transcription schemas
  - Voter counts for *Gemeenteraad* elections before and after the reform
3. **Municipal outcomes 1903** (*Provinciale Verslagen*):
  - Multi-page PDF extraction using agentic two-step pipeline (layout map + row extraction)
  - Municipal expenditures, voter turnout, electorate size
4. **HDNG v4** (Historical Database of Dutch Municipalities): population, pre-reform tax revenues, religious composition

# Key Variables

Table 1: Key Variables

Variable	Description	Source
Suffrage expansion 1896–1897	$\Delta$ eligible voters 1896 $\rightarrow$ 1897 / male pop.	Prov. Verslagen
Municipality class rank	Municipality class (1–9; higher = larger)	Kieswet 1896
Suffrage expansion 1897–1903	$\Delta$ eligible voters 1897 $\rightarrow$ 1903 / pop.	Prov. Verslagen
Municipal expenditures p.c. 1903	Municipal expenditures per capita, 1903	Prov. Verslagen
Log tax revenue 1889	Log pre-reform tax revenue (control)	HDNG
Pop. 1889	Municipal population in 1889 (control)	HDNG
Catholic/Prot share 1889	Cath./Prot. share of population (control)	HDNG

# Identification

## Identification Strategy

**First stage:** class rank  $\rightarrow$  suffrage expansion

$$\Delta \text{Suffrage}_{i, 1896 \rightarrow 97} = \alpha_j + \delta_1 \cdot \text{ClassRank}_i + X_i' \delta_2 + u_i$$

**Second stage:** suffrage expansion  $\rightarrow$  outcomes

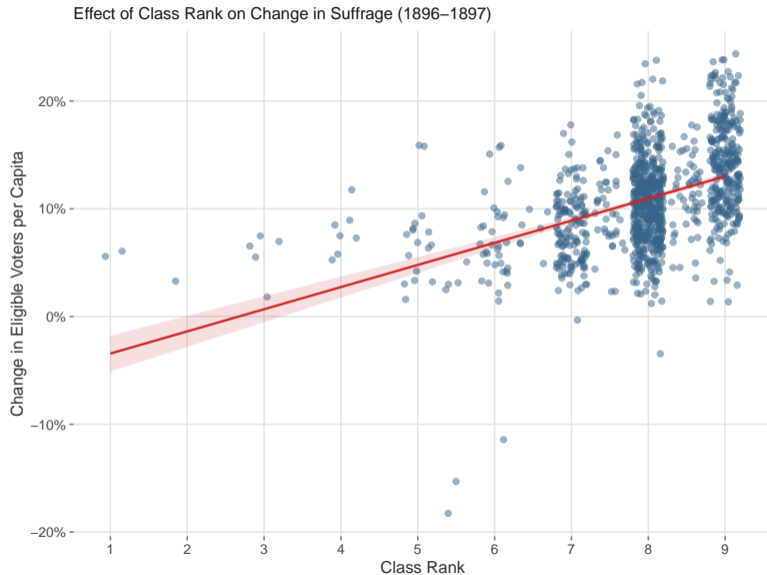
$$Y_{i, 1903} = \alpha_j + \beta_1 \cdot \widehat{\Delta \text{Suffrage}}_{i, 1896 \rightarrow 97} + X_i' \beta_2 + \varepsilon_i$$

where  $j$  indexes provinces (province fixed effects) and  $X_i$  includes log population, log pre-reform tax revenue, and religious composition shares.

**Exclusion restriction:** class rank affects 1903 outcomes *only* through the 1897 suffrage expansion

- Plausible given administrative arbitrariness of classification
- Robustness: results stable across control sets; restricted sample (Class Rank  $> 7$ ) yields similar estimates

# First Stage: Graphical Evidence



# Results

## First Stage: Table

Table 2: Dep. Var.:  $\Delta$  Suffrage per Capita 1896–1897

	All Municipalities		Class Rank > 7
	(1)	(2)	(3)
Class Rank	0.018*** (0.002)	0.015*** (0.004)	0.015*** (0.004)
Log Population		0.028*** (0.006)	0.016*** (0.005)
Log Tax Revenue 1889		-0.029*** (0.005)	-0.020*** (0.004)
Share Catholics 1889		-0.303* (0.176)	-0.071 (0.180)
Share Protestants 1889		-0.291* (0.175)	-0.061 (0.180)
R2 Adj.	0.281	0.322	0.278
Num.Obs.	1112	1112	888
Province FE	Yes	Yes	Yes

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

Class rank has a strong, stable positive effect on the 1897 suffrage expansion.

## Does Initial Enfranchisement Trigger More?

**Hypothesis:** newly enfranchised voters demand further franchise extension → multiplier effect

**Challenge:** OLS is biased — municipalities that expanded more in 1897 are closer to saturation

- OLS shows a large *negative* relationship (mean reversion)
- IV corrects for this by using only class-rank-driven variation

**IV result:** no significant effect of the 1897 suffrage shock on subsequent expansion 1897–1903

- Point estimate close to zero; confidence interval rules out large positive multipliers
- Universal male suffrage came only in 1917 — any multiplier may operate on a longer horizon

## IV Results: Subsequent Suffrage Expansion

Table 3: Dep. Var.:  $\Delta$  Suffrage per Capita 1897–1903 (IV)

	All Municipalities		Class Rank > 7
	(1)	(2)	(3)
$\Delta$ Suffrage p.c. 1896–1897 (IV)	-0.170*	-0.017	0.378
	(0.088)	(0.156)	(0.463)
Log Population		0.001	-0.002
		(0.007)	(0.013)
Log Tax Revenue 1889		0.005	0.012
		(0.007)	(0.015)
Share Catholics 1889		0.338	0.185
		(0.227)	(0.250)
Share Protestants 1889		0.320	0.170
		(0.226)	(0.247)
R2 Adj.	0.037	0.061	0.057
Num.Obs.	952	952	749
Province FE	Yes	Yes	Yes

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

No significant multiplier effect within 6 years of the initial reform.

# Fiscal Effects: Theory

## Standard prediction (Meltzer–Richard 1981):

- Franchise extension → poorer median voter → demand for redistribution → higher spending

## Why we might see the opposite:

- The 1896 reform enfranchised **income taxpayers and ratepayers** above class-specific thresholds
- These are **middle-class** households, not the working poor
- Middle-class voters typically prefer fiscal restraint and lower municipal expenditures
- Contrast: English boards of guardians (1894) enfranchised working-class voters → higher poor relief (Chapman 2018)

**Testable implication:** IV estimate of suffrage expansion on municipal expenditures should be *negative*

## IV Results: Municipal Expenditures (Per Capita)

Table 4: Dep. Var.: Municipal Expenditures per Capita, 1903 (IV)

	All Municipalities		Class Rank > 7
	(1)	(2)	(3)
$\Delta$ Suffrage p.c. 1896–1897 (IV)	-407.311*** (39.981)	-317.174*** (104.539)	-116.207* (63.354)
Log Population		7.655** (3.008)	0.342 (1.805)
Log Tax Revenue 1889		-6.984** (3.346)	-2.324 (2.249)
Share Catholics 1889		-539.338*** (65.320)	-275.271*** (77.061)
Share Protestants 1889		-533.992*** (65.366)	-270.841*** (76.672)
R2 Adj.	0.297	0.359	0.119
Num.Obs.	961	961	758
Province FE	Yes	Yes	Yes

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

## IV Results: Municipal Expenditures (Log)

Table 5: Dep. Var.: Log Municipal Expenditures, 1903 (IV)

	All Municipalities		Class Rank > 7
	(1)	(2)	(3)
$\Delta$ Suffrage p.c. 1896–1897 (IV)	-37.359*** (3.356)	-9.248*** (2.933)	-4.396* (2.581)
Log Population		1.095*** (0.085)	0.914*** (0.079)
Log Tax Revenue 1889		-0.093 (0.097)	-0.003 (0.093)
Share Catholics 1889		-15.763*** (2.171)	-12.150*** (2.675)
Share Protestants 1889		-15.542*** (2.171)	-11.938*** (2.666)
R2 Adj.	0.496	0.875	0.834
Num.Obs.	961	961	758
Province FE	Yes	Yes	Yes

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

A 1 pp increase in the suffrage shock  $\rightarrow$  5-10% *reduction* in total municipal expenditures.

# Mechanism: Efficiency vs. Redistribution

## Who was enfranchised by the 1896 reform?

- Income taxpayers above class-specific thresholds (roughly the **upper middle class**)
- Property owners whose rental values exceeded class-specific minimum thresholds
- NOT the working poor (who lacked assessed income or held no property)

## Two candidate mechanisms:

Mechanism	Prediction	Evidence
Redistribution	New voters demand transfers	Inconsistent (sign is negative)
Efficiency	Ratepayers constrain wasteful spending	→ tested below

- Ratepayers who pay local taxes have a direct interest in fiscal restraint
- If efficiency: administrative overhead should fall, but education and poor relief should be unaffected
- If retrenchment: public service spending (education, poor aid) should fall too

## IV Results: Disaggregated Expenditures

Table 7: Dep. Var.: Expenditure per Capita by Category, 1903 (IV)

Sample:	Governance		Tax Compliance		Education		Poor Aid	
	All	Rank > 7	All	Rank > 7	All	Rank > 7	All	Rank > 7
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$\Delta$ Suffrage p.c. 1896–1897	-11.144*** (3.620)	-13.171** (5.134)	-1.508*** (0.561)	-0.715* (0.433)	-43.002 (27.430)	22.018 (72.105)	-3.146 (4.744)	-2.181 (8.091)
R2 Adj.	0.458	0.529	0.202	0.207	0.064	0.081	0.305	0.350
Num.Obs.	1112	888	1112	888	1112	888	1112	888
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

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

# Efficiency, Not Retrenchment

## What the decomposition shows:

Category	IV Estimate (All)	IV Estimate (Rank > 7)	Significant?
Governance	-11.1	-13.2	Yes
Tax compliance	-1.5	-0.7	Yes
Education	-43.0	+22.0	No
Poor aid	-3.1	-2.2	No

## Interpretation:

- Administrative overhead and tax collection costs fall sharply
- Education and poor relief are statistically indistinguishable from zero
- ⇒ *The same level of public services delivered at lower overhead cost*
- Consistent with middle-class ratepayers demanding lean government, not service cuts
- Contrast: English Poor Law reform (1894) enfranchised the poor → higher poor relief (Chapman 2026)

# Robustness

## Specification robustness:

- Results stable adding controls progressively (log population, log tax revenue, religious composition)
- Province fixed effects included in all specifications
- Restricted sample (Class Rank  $> 7$ ) yields qualitatively similar results

## Instrument validity:

- Class rank captures only the income-threshold component of the reform
- Misclassification documented in primary sources; plausibly orthogonal to economic outcomes conditional on controls
- No obvious channel from class rank to 1903 outcomes other than the suffrage expansion

## Robustness (cont.)

### **Outcome robustness:**

- Per capita and log expenditure specifications agree on sign and significance
- Turnout specification confirms broader electorate is associated with participation changes
- Suffrage persistence result robust: IV null effect confirmed across all control specifications

## Conclusion

# Conclusion

## Main findings:

1. **No franchise multiplier within 6 years:** the 1897 suffrage expansion did not generate further democratization by 1903 — partial enfranchisement is not self-amplifying in the short run
2. **Middle-class enfranchisement reduces expenditures:** a 1 percentage-point larger suffrage expansion → \$ \$9–10% lower municipal expenditures in 1903
3. **Administrative efficiency, not retrenchment:** governance overhead and tax compliance costs fall; education and poor relief are unaffected — same services at lower cost

# Implications

## Why composition matters:

- The 1896 reform extended rights to income taxpayers and ratepayers (upper middle class), not the working poor
- These new voters had direct fiscal stakes in constraining overhead, not in cutting public services
- The fiscal direction of democratization depends on *who* enters the electorate

## Implications:

- The economic consequences of democratization depend critically on *who* is enfranchised
- Partial democratization may not be a stepping stone — institutional complementarities matter
- Universal male suffrage (1917 in the Netherlands) was a distinct political transition

## References I

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- . 2026. "Democracy, Redistribution, and Inequality: Evidence from the English Poor Law." *Journal of the European Economic Association*.