Late Medieval Unrest in the Holy Roman Empire

Online Appendix

Appendix

A Data Appendix

A.1 Terms for unrest

The German historiography has a wide vocabulary for these events. Riot (*Aufruhr*, *Auflauf*), uprising (*Aufstand*, *Empörung*), revolt (*Revolte*), discord (*Zwietracht*), dispute (*Streit*), conspiracy (*Verschwörung*), civic and guild struggle (*Bürgerkämpfe*, *Zunftkämpfe*), and many others name points on a continuum of social and political conflict, each describing a different degree of intensity or an implicit teleology (Blickle, 2012, p. 5; Isenmann, 2014, p. 254). The clearest case is the East German proto-bourgeois revolution (*frühbürgerliche Revolution*), which read Reformation and Peasants' War together as an early bourgeois revolution at the start of the transition from feudalism to capitalism, prepared by a revolutionary crisis between 1470 and 1517 (Laube et al., 1982, p. 302; Steinmetz, 2021). I follow Blickle (2012) and use the more inclusive label "unrest" (*Unruhen*). Terms like rebellion, riot, or social conflict arrive with an implicit theory attached—a view of what the event *was* and between *whom*—and I want to avoid prejudging episodes that differ widely in context, participation, extremity, and effect. *Bürgerkämpfe* shows the problem: it implies that essentially only citizens took part, which the sources cannot show, since they say little about the lower strata without citizenship and guilds sometimes included members without it (Isenmann, 2014, p. 255); *Bürger* itself can carry Marxist connotations (as in *bourgeois*) in certain contexts, but not in others. Even the sources do not agree on what to call an episode or who stood behind it, and support could shift within a single episode: a Hanseatic chronicle recorded the disturbance at Minden in 1405 as a rising of the citizenry, though the sworn alliance behind it came from councillors and the Forty, not the broader community (Ehbrecht, 1980), and a 1493 conspiracy in Korbach, believed to involve 120 citizens, lost the support of several conspirators when they were asked to swear a general oath, whereas the arrest of the Jauer conspirators in 1516 brought out the guilds and the common people (Johanek, 1997). "Unrest" sidesteps that debate and can accommodate the whole range of socio-political activity.

A.2 Unrest types

Each episode carries one or more of six types, coded from the source text by what the participants attempted and how:

- Revolution, or violent action aimed at a major change of the social or constitutional order, such as the Bundschuh conspiracies of 1502 and 1517
- Rebellion, or open collective action by a large group, including guild or commune risings that depose or enter the council and risings against a lord
- Coup, or a seizure of power, or an attempt at one, by a small group such as a faction or a few families
- Riot, or short-lived crowd violence without organization

- Protest, or nonviolent collective action, including petitions, refusals of dues or homage, and strikes
- Unclear, where the sources do not specify what form the unrest took

An episode takes every type its source rows record, so one whose sources describe both a protest and a rebellion counts under both. Severity is major for revolution, rebellion, and coup, minor for riot and protest, and unknown otherwise: 49.0%, 25.3%, and 25.7% of episodes. An episode's type is a coding of how the sources describe it, not an independent measurement.

A.3 Grievance coding

Each row of the unrest catalog carries six binary grievance flags: economic, fiscal, representation, autonomy, administration, and religious. The flags record why the participants rose, as the source describes it. They do not record what the unrest looked like, who took part, or how it ended. This section sets out the definitions and the rules used to apply them. Every row with a description was coded against these rules.

Six general rules apply to every flag.

1. *Stated causes only.* A flag is set only when the row's description names that grievance as a cause, demand, or trigger of the unrest. Nothing is inferred from the kind of event, its date, or the wider movement it belongs to. A row recording that a town joined the Peasants' War of 1525 is not flagged religious or political unless its description says what the town's participants wanted.
2. *No description, no flag.* Rows whose description says nothing about causes or complaints carry no flags; they are left blank rather than coded zero.
3. *Uncertain causes are not coded.* A cause that the source itself marks as probable, possible, presumed, or uncertain sets no flag.
4. *Flags are independent.* A row may carry several flags or none. A row whose description reports only what happened, and not why, carries none.
5. *One description, one coding.* Rows that share an identical description, such as the villages of a single regional movement, receive identical flags.
6. *Episode level.* An episode carries a flag if any of its source rows carries it.

Several recurring kinds of grievance could fall under more than one flag; they are resolved as follows.

- *Seigneurial burdens* are split by content. Dues, rents, tithes, and labor services are fiscal. Serfdom, loss of liberties, tenure, and jurisdiction are autonomy. A row naming both carries both. A lord's purely administrative duties (protection, defence, justice) are administration, not fiscal.
- *Famine, dearth, and natural shocks* are economic, whether the description names a harvest failure, a weather shock, a price spike, or hunger.
- *Wages and working conditions* are economic. A strike with no stated cause carries no flag.

Table 1: Grievance flag definitions

Flag	Set when the description names...	Not this flag
Economic	Hardship from conditions beyond the opposed authority's control, from markets or from nature: coin debasement or inflation by others, food-price spikes, famine, wages, trade disruption or competition, the decline of an industry, drought, flood, frost, storms, harvest failure, epidemics, and town fires.	Debasement or price-setting by the opposed authority itself (fiscal).
Fiscal	The extraction burden imposed by the opposed authority: taxes, excises, tolls, levies, war contributions, billeting, public debt, embezzlement, demands to audit the accounts, debasement or price-setting by the authority; seigneurial dues, tithes, rents, labor services, and death duties; private debts with harmful public effects.	A single creditor-debtor dispute; a lord's failure to protect or judge (administration).
Representation	Who sits in government: council seats, elections, eligibility for office, oversight or audit committees, and guild constitutions.	The rights of the town or community against its lord (autonomy).
Autonomy	Who rules the place and with what rights: jurisdiction and courts, the liberties and self-government of the town or community against its lord, foreign rule, and personal status including serfdom and tenure.	Who sits on the council (representation).
Administration	How the authority governs: regulation of trade and crafts, the conduct of officials and soldiers, conscription, and a lord's failure to provide protection, defence, or justice.	The tax itself (fiscal).
Religious	Belief, worship, or church authority: Reformation demands, disputes over preachers or the mass, iconoclasm, clerical conduct, and contested church appointments.	Hostility toward Jews (recorded through the target; debts are fiscal under the public-harm rule).

Notes:
Flags are coded at the row level of the unrest catalog from each row's description and aggregated to episodes by an any-flag rule. The boundary rules in the text resolve cases that fit more than one definition.

- *Regulation of trade.* The authority's rules on crafts, prices, weights, market rights, or brewing are administration, since they concern how the authority governs. Competition and trade disruption are economic.
- *War and soldiers.* Foreign rule is autonomy; conscription and plundering or harassment by troops are administration. Billeting and war contributions are fiscal.
- *Debts.* Private debts are fiscal only when the source ties them to a public harm: widespread indebtedness of townspeople or peasants, debts the authority took over, cancelled, or taxed, or debts that strained public finances.
- *The clergy.* A dispute over clerical tax exemptions is fiscal. It is also religious only when the description names doctrine, worship, clerical conduct, or church appointments.
- *Outcomes and targets.* A concession the rebels won counts as evidence of their demand. An outcome on its own, or a bare target ("against the council"), does not establish a cause.

A.4 The backbone and record matching

Three entries of the population dataset that meet the backbone's rule are left out of it: Metz and Thionville, Lorraine cities of the Three Bishoprics that fall inside the region as approximated but outside the Alsatian and German source volumes, and modern Wuppertal, whose constituent towns Barmen and Elberfeld are already in the backbone. The institutional volumes for Alsace and Switzerland number cities differently from the backbone, so those cities are linked by name; 31 of the 33 link to the study population, and the other two are Metz and Thionville.

Population and plague records are matched to the backbone cities (Section 3.3 of the main text) by name, after normalization (lower case, diacritics folded, generic words such as *Neu*, *Alt*, and *Stadt* dropped), in three steps. A record whose name matches a backbone city exactly is linked to the nearest such city within 50 km, which keeps out distant namesakes such as the plague source's "Freiburg", Fribourg in Switzerland. Any other record with coordinates is linked to the backbone city within 10 km with the most similar name, provided the two reach 0.70 on the Jaro–Winkler spelling-similarity score (0–1), which admits variants such as Küstrin and Kostrzyn but not a neighboring place such as Deutz, outside Cologne. Records without coordinates are linked to the most similar name overall, provided it is at least as similar as 95% of exact-name matches. One plague record, for Eibingen across the Rhine from Bingen, is excluded by hand.

An AI-assisted plausibility review of a spot-check sample of 64 records drawn from this matching step—a stratified sample of accepted population and plague matches plus a random draw of unmatched records—flagged 8 of them (12.5%) under the review's stated rules, which apply the matcher's own distance and name-similarity thresholds. All of them were accepted matches with moderate name similarity, and each proved on inspection to be a variant of the same town's name (for example Bad Langensalza and Langensalza, Halle (Saale) and Halle, Iena and Jena); none of the sampled unmatched records had a nearby, similarly named backbone city that the matcher had missed. The review is not a domain historian's confirmation, and it does not substitute for one; it covers every match method plus a random check of non-matches, and illustrates rather than bounds the error rate the backbone linking could carry forward.

Unrest episodes are linked by a similar name rule, applied to each place an episode is recorded at rather than to the episode as a whole. A catalog row links to a backbone city when its place name matches the city's exactly within 50 km, as in the first step above, or when it lies within 7.6 km and its place name reaches 0.70 on the same similarity score, so an abbey, village, or region near a city does not count for it, and an episode recorded in several backbone cities counts for each. A region named after a city links when the city ruled it, as with the risings of 1411 against Zurich, Bern, and Lucerne and the Ulm peasants of 1450. Three regional records that share a city's name but concern another lordship are excluded by hand: the Krempe marshes in 1306, a rising against the counts of Holstein; the prince-abbot of Kempten's lands in 1491; and the resistance to the Lusatian nobility recorded in the Görlitz court books around 1303.

B Additional Robustness Checks

B.1 Standard errors in the main-text regressions

The regression tables in the main text report Conley standard errors with Wigton-Jones's cutoffs: 100 km in space and 50 years in time, with Bartlett weights in both. Table 2 repeats the main text's institutions table (Table 5) with standard errors clustered by city instead, and Table 3 refits the Conley correction at 50, 100, and 250 km and at 10, 25, and 50 years. The upper radius covers the bound set out above, twice the Old World Drought Atlas cell. A further column keeps the 100 km and 50-year windows but weights every pair inside them equally, as Wigton-Jones's estimator does (Colella et al., 2019); the main tables use weights that fall linearly with distance and time, which in time guarantee a non-negative variance estimate (Newey & West, 1987). Equal weights leave wet-dry severity and guild seats in the main specification at the same significance level ($p = 0.026$ and 0.029) but move the market control to the 10% level ($p = 0.076$). Point estimates are the same throughout; only the standard errors change. Across the nine cutoff pairs, no coefficient's significance moves by more than one level. Guild seats are significant at the 5% level in every pair (p from 0.040 to 0.045) and under city clustering ($p = 0.048$), and the same-year guild coefficient quoted in the main text is significant at the 10% level in every pair (p from 0.061 to 0.075). No climate interaction reaches the 10% level under any pair. Clustering by the 24 regions instead gives guild seats a p-value of 0.033, though the region count falls below the usual rule of thumb of 30 clusters.

B.2 Is the drought association nonlinear?

The main text reports the drought association as linear. Table 4 sets the main specification beside three nonlinear forms: a quadratic in wet-dry severity, centered at its panel mean so the linear term is the slope at the mean; an indicator for city-years in the bottom decile of scPDSI; and that indicator together with one for the top decile. The squared term is small and insignificant, and the fitted curve turns at -8.4, deep in the dry tail of the observed range of -10.0 to +7.3, with fewer than 1% of city-years beyond it. The two deciles take opposite signs, bottom-decile years with less unrest and top-decile years with more, but neither is significant.

B.3 Randomization inference for the climate coefficients

Unrest falls in under 1% of city-years, so the Conley standard errors of the climate terms rest on few events. Table 5 checks them by randomization inference. Each draw shifts every city's scPDSI series forward by the same number of years, wrapping the end of the window back to its start, and re-fits the main specification and its decile form. The shift keeps the spatial pattern of climate within a year and its persistence within a city, and breaks only its alignment with unrest. The window allows exactly 220 such shifts, and all of them are used, so the reference distribution is exact rather than sampled. The randomization p-value is the share of fits, the

Table 2: Main-text regressions with city-clustered standard errors

	Any unrest × 100		
	Climate only	Main	Interaction
Model:	(1)	(2)	(3)
Variables			
scPDSI	0.0655**	0.0754**	0.0629*
	(0.0304)	(0.0345)	(0.0362)
Guild seats		-0.4567**	-0.4199*
		(0.2302)	(0.2460)
Burgher seats		-0.0956	-0.0769
		(0.1956)	(0.1988)
Market (0/1)		0.2685	0.2675
		(0.1721)	(0.1722)
Plague indicator		0.6868	0.6892
		(0.4737)	(0.4736)
scPDSI × Guild seats			0.0336
			(0.0429)
scPDSI × Burgher seats			0.0180
			(0.0350)
Log population	0.0565	-0.0022	-0.0021
	(0.1072)	(0.1358)	(0.1360)
City	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	66,521	51,063	51,063
R^2	0.02070	0.02135	0.02137

Clustered (City) standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Same models as Table 3, column 1 (climate only), and columns 1 and 3 of Table 5 of the main text (representation the year before), with standard errors clustered by city instead of the Conley correction. Coefficients in percentage points (outcome 100 if unrest, 0 otherwise). Linear probability models, annual city-year panel, 1300–1520; city and year fixed effects. Sources as in the main table.

actual one included, whose coefficient or Conley t-statistic is at least as large in absolute value as the actual one. For scPDSI it is 0.045 on the coefficient and 0.045 on the t-statistic, against a Conley p-value of 0.027; leaving out the shifts of 10 years or fewer, whose climate series still correlate with the real one, gives 0.050. Neither decile is significant under either method: the bottom decile has a randomization p-value of 0.443 (Conley 0.330) and the top decile 0.344 (Conley 0.328). The randomization test thus places the linear association where the Conley standard errors do, at the 5% level.

The same shifts, applied to the settled-years interaction of bottom-decile years with guild seats the year before (main text, Table 6, column 4), give a randomization p of 0.032 on its Conley t statistic and 0.109 on its coefficient (0.035 and 0.114 without the shifts within 10 years). The

Table 3: Main-text regressions: p-values by standard-error method and Conley cutoffs

		Clustered	Equal weights	Conley (Bartlett), distance (km) / lag (years)								
Coefficient	Estimate	City	Equal 100/50	50/10	100/10	250/10	50/25	100/25	250/25	50/50	100/50	250/50
scPDSI (climate only)	0.0655	0.032	0.028	0.014	0.014	0.012	0.016	0.016	0.014	0.019	0.019	0.018
scPDSI (main)	0.0754	0.030	0.026	0.026	0.026	0.023	0.026	0.026	0.023	0.027	0.027	0.024
Guild seats	-0.4567	0.048	0.029	0.040	0.040	0.040	0.045	0.045	0.045	0.044	0.044	0.044
Burgher seats	-0.0957	0.625	0.607	0.627	0.627	0.621	0.619	0.619	0.613	0.615	0.615	0.609
Market	0.2685	0.120	0.076	0.130	0.130	0.128	0.137	0.137	0.135	0.129	0.129	0.127
Guild seats, same year	0.4003	0.080	0.046	0.072	0.071	0.067	0.075	0.074	0.070	0.067	0.066	0.061
scPDSI x Guild seats	0.0336	0.434	0.415	0.449	0.447	0.446	0.445	0.443	0.442	0.443	0.441	0.439
scPDSI x Burgher seats	0.0180	0.608	0.560	0.604	0.595	0.584	0.606	0.597	0.586	0.602	0.592	0.581
Severe drought	-0.1566	0.314	0.315	0.328	0.328	0.318	0.334	0.333	0.324	0.329	0.329	0.319

Notes:
Each cell is the p-value of the coefficient in the main-text column for that row (climate only: Table 3, column 1; the others: Table 5), under the standard errors named in the column header: City is clustered by city; Equal 100/50 gives every pair within 100 km and 50 years equal weight; the remaining columns are Conley standard errors at the stated distance (km) and lag (years) cutoffs, with Bartlett weights in space and time. Coefficients in percentage points. The main text uses 100 km and 50 years. Estimates are identical across columns. Annual city-year panel, 1300–1520; city and year fixed effects. Sources as in the main table.

shift tests the sharp null that climate is unrelated to unrest, and the coefficient is not pivotal under it, because the number of settled bottom-decile years that fall in cities with guild seats changes from shift to shift; the t statistic is the better guide. With standard errors clustered by city and by year, the interaction has p = 0.017.

B.4 Rye suitability across its range

The rye columns of Table 3 in the main text report each climate term at zero rye suitability and at the median city. Zero suitability lies far outside where the cities are. Among the backbone cities, only 2 score zero (Chur and Montreux), and the next lowest are also Alpine or Jura towns (Innsbruck, 0.39; le Locle, 0.51; Schwaz, 0.60 t/ha). Most cities lie between 1.46 t/ha at the 10th percentile and 1.84 at the 90th, and the maximum is 1.93. Table 6 evaluates each climate term at these points.

Over the range where the cities lie, the scPDSI associations change little; the decile associations shift more but are imprecise. The same-year association with scPDSI is +13% per unit at the 10th percentile and +12% at the maximum, significant throughout. The same-year deciles are larger in cities with poorer rye land, the top decile going from +54% at the 10th percentile to +22% at the maximum; it is significant, at the 5% level, only at the 10th percentile, and the bottom decile is significant at no point. Last year's scPDSI also rises across the range, from +8% to +12%. The steepest gradient is last year's top decile, from +6% at the 10th percentile to +60% at the maximum, but only the maximum reaches even the 10% level, and the slope rests on the few Alpine and Jura cities at the bottom of the range, where the extrapolated association at zero suitability is -163%. None of the rye interactions is significant at the 5% level.

Figure 1 draws the same-year and prior-year associations across the whole range of suitability: flat, and distinguishable from zero over the range where nearly all cities lie.

Table 4: Linear and nonlinear forms of the drought association

	Any unrest × 100			
	Linear	Quadratic	Bottom decile	Both deciles
Model:	(1)	(2)	(3)	(4)
Variables				
scPDSI	0.07537**			
	(0.03411)			
scPDSI (centered)		0.07768**		
		(0.03453)		
scPDSI (centered), squared		0.00522		
		(0.00733)		
Bottom-decile scPDSI (0/1)			-0.15659	-0.15622
			(0.16049)	(0.16045)
Top-decile scPDSI (0/1)				0.15306
				(0.15635)
Guild seats	-0.45667**	-0.45568**	-0.45935**	-0.45713**
	(0.22639)	(0.22622)	(0.22658)	(0.22638)
Burgher seats	-0.09565	-0.09582	-0.09970	-0.09921
	(0.19026)	(0.19030)	(0.19040)	(0.19028)
Market (0/1)	0.26847	0.26902	0.26303	0.26357
	(0.17670)	(0.17678)	(0.17657)	(0.17665)
Plague indicator	0.68684	0.68617	0.68782	0.68860
	(0.51849)	(0.51829)	(0.51847)	(0.51851)
Log population	-0.00219	-0.00161	-0.00400	-0.00395
	(0.13427)	(0.13427)	(0.13438)	(0.13438)
City	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	51,063	51,063	51,063	51,063

Conley standard errors (100 km, 50 years) in parentheses
*Signif. codes: ***: 0.01, **: 0.05, *: 0.1*

Linear probability models, annual city-year panel, 1300–1520; same sample and controls as the main specification; outcome 100 if unrest, 0 otherwise, so coefficients are in percentage points. Column 1 is the main specification. Column 2 centers scPDSI at its panel mean and adds its square. Bottom decile: scPDSI at or below its 10th percentile; top decile: at or above its 90th percentile. scPDSI is negative in drought. City and year fixed effects. Sources as in the main table.

B.5 Does drought's association with unrest vary with a city's grain-growing suitability?

A further cut asks whether wet-dry severity's association with unrest depends on a city's agricultural endowment specifically, rather than on its institutions. Table 7 adds a single interaction term, wet-dry severity times a time-invariant rye-suitability index, to the five-year specification's covariate set, holding the fixed effects fixed. Rye suitability is extracted at each backbone city's coordinates from the GAEZ winter-rye, rainfed, low-input yield-potential raster, the same raster family Galor and Özak (2016) draw on to construct an analogous

Table 5: Randomization inference for the climate coefficients: circular shifts of scPDSI

			All shifts		Shifts beyond 10 years		Shifted coefficients
Term	Coefficient	Conley p	Coef.	t	Coef.	t	5th–95th pct.
scPDSI	0.0754	0.027	0.045	0.045	0.050	0.045	[-0.0597, 0.0578]
Bottom-decile scPDSI	-0.1562	0.330	0.443	0.434	0.443	0.433	[-0.2737, 0.3069]
Top-decile scPDSI	0.1531	0.328	0.344	0.312	0.358	0.328	[-0.2418, 0.2943]

Notes:
Coefficients from the main specification (scPDSI) and its decile form, outcome 100 if unrest, 0 otherwise. Each randomization p-value compares the actual coefficient or Conley t-statistic with its values when every city's scPDSI series is shifted by the same number of years, wrapping at the window's end; all 220 shifts are used. The last column gives the range of the middle 90 percent of shifted coefficients.

Table 6: Climate associations across the range of rye suitability

			Zero	10th pct.	Median	90th pct.	Max.
Column	Year	Term	0.00	1.46	1.70	1.84	1.93
(2)	Same year	scPDSI	+14%	+13%**	+12%**	+12%**	+12%**
(4)	Same year	Bottom-decile scPDSI	-31%	-18%	-16%	-14%	-13%
(4)	Same year	Top-decile scPDSI	+153%	+54%**	+37%	+27%	+22%
(6)	Last year	scPDSI	-4%	+8%	+10%*	+11%**	+12%**
(8)	Last year	Bottom-decile scPDSI	+47%	+2%	-6%	-10%	-13%
(8)	Last year	Top-decile scPDSI	-163%	+6%	+33%	+50%	+60%*

Notes:
Each cell is the climate term's combined association, its coefficient plus the rye interaction times the rye suitability (t/ha) in the header, from the rye columns of Table 3, as a share of the outcome's base rate. scPDSI rows are per unit; decile rows compare those city-years with the middle 80 percent. Conley standard errors; *** 0.01, ** 0.05, * 0.1.

caloric-suitability index; because it is fixed within a city across the panel, its own main effect is absorbed by the city fixed effect and only the interaction is estimable. The interaction coefficient is positive and significant at the 5% level (0.0000091, Conley SE 0.0000044, p = 0.037, N = 10,484, same estimation sample as the five-year specification). Its sign would mean the five-year specification's already-reversed drought coefficient is more pronounced in high-rye-suitability cities, the opposite of a storage-and-market-integration buffering story. The annual specifications in Table 3 of the main text show no such interaction, and this one was not set in advance, so I read it as a five-year pattern rather than evidence that agricultural endowment conditions the drought association.

B.6 Does climate matter differently across grievance and conflict type?

The main text's type and perpetrator split (the Climate and Unrest section) and the outcome ladder (Table 17) ask whether the climate association depends on what kind of episode is coded. The same question can be asked here on the five-year panel, and also of the specific grievance

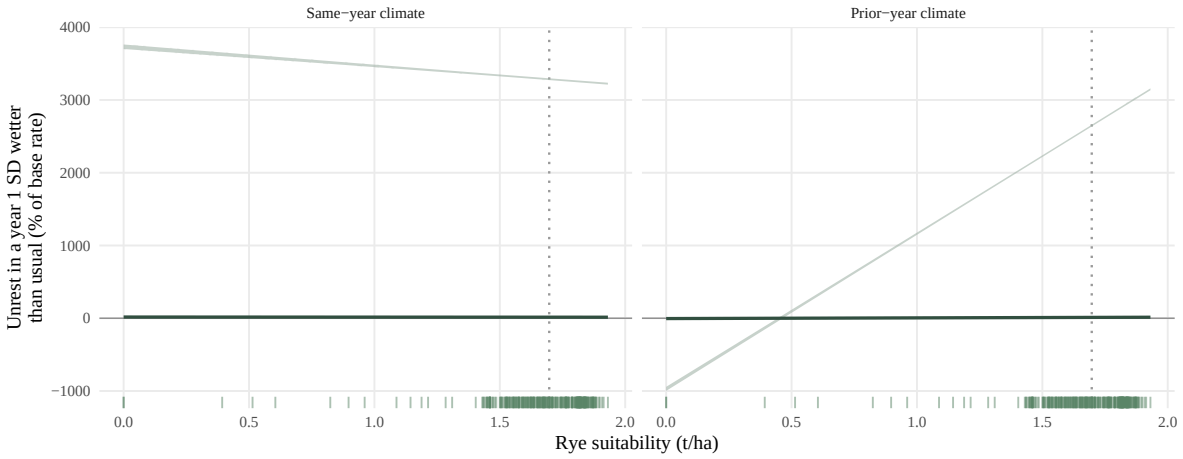


Figure 1: Association of wet-dry severity with unrest across rye suitability, from the rye-interaction columns of Table 3 (same year and year before). The line is the change in unrest in a year one within-city standard deviation wetter than usual, as a percent of the base rate; the band is a 95% Conley confidence interval. Ticks mark the cities; the dotted line marks the median city.

an episode raises rather than only its form. Table 8 reports wet-dry severity's coefficient from the same full-covariate specification across the baseline, severity, and violence-type cuts already reported in the main text, alongside a parallel cut by grievance flag—economic, fiscal, representation, autonomy, administration, and religious. Grievance flags are a non-exclusive coding (see the Data section: a single episode can raise more than one), so they cannot share a single categorical breakdown the way the form-of-unrest classification does; each flag gets its own city-year outcome and its own regression instead.

In this five-year specification climate is not distinguishable from zero for all unrest ($p = 0.204$), is positive only at the 10% level for the major-severity and Revolution/Rebellion episodes, which now coincide ($p = 0.093$), and is not significant for violent episodes ($p = 0.136$). Among the grievance flags, representation returns a significant coefficient (+0.0006, $p = 0.003$); economic ($p = 0.126$), administration ($p = 0.123$), fiscal ($p = 0.237$), autonomy ($p = 0.870$), and religious ($p = 0.572$) grievances show no detectable association. Every grievance coefficient but the religious one is positively signed: wetter rather than drier conditions go with these episodes, the same reversed direction found throughout this paper. Religious grievance, on 17 matched city-years, just above the 15-period floor used throughout this section, has a small negative coefficient. The category most directly implicated by a subsistence-shock mechanism, economic grievance, which includes every named natural shock, is estimable on 27 matched city-years, and its coefficient too is signed toward more unrest in wetter years.

Table 7: Wet-dry severity interacted with city-level rye-growing suitability

	Any unrest (0/1)	
	Five-year	+ Rye interaction
Model:	(1)	(2)
Variables		
scPDSI (higher = wetter)	0.0029	-0.0129*
	(0.0023)	(0.0078)
Guild representation (0–3)	0.0024	0.0024
	(0.0045)	(0.0045)
Burgher representation (0–3)	0.0015	0.0014
	(0.0055)	(0.0055)
Market (share of period)	0.0153**	0.0153**
	(0.0075)	(0.0075)
Plague indicator	0.0024	0.0025
	(0.0104)	(0.0104)
Wet-dry severity × rye suitability		9.13×10^{-6}**
		(4.38×10^{-6})
Log population	0.0012	0.0014
	(0.0064)	(0.0064)
City	Yes	Yes
Period	Yes	Yes
Fit statistics		
Observations	10,484	10,484
R^2	0.08201	0.08225
Within R^2	0.00052	0.00077

Conley standard errors (100 km, 50 years) in parentheses
*Signif. codes: ***: 0.01, **: 0.05, *: 0.1*

Dependent variable: city-period unrest incidence, five-year periods, 1300–1520, on the five-year specification's estimation sample (252 cities, 45 periods, N = 10,484). Rye suitability is the time-invariant GAEZ winter-rye yield potential at each city's coordinates; the city fixed effect absorbs its main effect. City and period fixed effects.

B.7 Multiple testing across the headline results

The paper's headline tests are fixed here as one family of 11, declared after the drought-by-guild interaction had been seen: the linear and decile climate terms and the guild and burgher seats of the main specification (representation the year before), guild and burgher seats on settled years, the exploratory bottom-decile × guild interaction on settled years, the change-year spike of the

Table 8: Climate coefficient across unrest-type and grievance-flag cuts

Category	Climate coefficient	(SE)	p	N
All unrest (baseline)	0.0029	(0.0023)	0.204	10,484
Severity: major only	0.0028	(0.0017)	0.093	10,484
Type: Revolution/Rebellion	0.0028	(0.0017)	0.093	10,484
Violence tier: violent	0.0025	(0.0017)	0.136	10,484
Grievance: Economic	0.0001	(0.0001)	0.126	51,315
Grievance: Fiscal	0.0002	(0.0002)	0.237	51,315
Grievance: Representation	0.0006	(0.0002)	0.003	51,315
Grievance: Autonomy	0.0000	(0.0001)	0.870	51,315
Grievance: Administration	0.0002	(0.0001)	0.123	51,315
Grievance: Religious	-0.0000	(0.0000)	0.572	51,315

Notes:
Each row is the wet-dry severity coefficient (Conley SE, 100 km and 50 years; city and period fixed effects) from a full-covariate LPM (climate + guild representation + burgher representation + log population + plague indicator) fit on the outcome named. The first four rows reuse already-fit models from the main-text regressions and the severity/type/violence cuts above; grievance rows are newly fit, one per non-exclusive grievance flag, restricted to flags clearing the 15-matched-period floor used throughout this section.

event study around changes in representation, and the two government-type interactions of the Wigton-Jones benchmark. Table 9 sets each test's Conley p-value beside three adjustments: Holm's step-down, which holds the family-wise error rate under any dependence between the tests; the Bonferroni bound; and the sharpened q-values of Anderson (2008), which control the false discovery rate. A Romano–Wolf step-down would need all the models resampled jointly under one dependence structure, which Conley standard errors on different samples do not provide, so Holm is the step-down reported. At the 5% level, Holm's adjustment leaves the change-year spike (adjusted p <0.001). With sharpened q-values, the settled-years interaction of bottom-decile years with guild seats ($q = 0.029$) and the five-year benchmark's interaction of severe drought with broader burgher representation ($q = 0.029$) also pass at the 5% level, though not under Holm or Bonferroni. Every other headline test, the climate terms among them, has a sharpened q-value of 0.058 or more.

B.8 Which forms of unrest carry the results?

Coups (Section B.32) were singled out after the exercise reported here. Each headline specification was re-fitted with its outcome restricted to one form of unrest, using the six-term form vocabulary grouped as in the main text (forms overlap, since one episode can carry several). Because the forms differ greatly in frequency, Table 10 reports each coefficient divided by its outcome's base rate, so that a cell reads as the proportional change in the outcome. If an association were spread evenly across forms, the scaled coefficients in a row would be similar; if one form carried it, that form's cell would stand out.

Table 9: The headline family: unadjusted and adjusted p-values

Test	Conley p	Holm	Bonferroni	Sharpened q
scPDSI, main specification	0.027	0.217	0.298	0.058
Bottom-decile scPDSI, main specification	0.330	1.000	1.000	0.261
Top-decile scPDSI, main specification	0.328	1.000	1.000	0.261
Guild seats, prior year	0.044	0.262	0.480	0.062
Burgher seats, prior year	0.615	1.000	1.000	0.462
Guild seats, settled years	0.631	1.000	1.000	0.462
Burgher seats, settled years	0.844	1.000	1.000	0.624
Bottom decile x prior-year guild seats, settled	0.008	0.075	0.092	0.029
Change-year spike, any representation change (0-4)	<0.001	<0.001	<0.001	0.001
Severe drought x broader burgher rep., five-year Wigton-Jones	0.007	0.069	0.076	0.029
Severe drought x burgher/guild, annual Wigton-Jones re-fit	0.036	0.252	0.396	0.062

Notes:
Each row is one of the 11 headline tests, with its p-value from Conley standard errors (100 km, 50 years) as the main text reports it. Holm and Bonferroni adjust for the family-wise error rate across all 11 tests; sharpened q-values (two-stage step-up) control the false discovery rate. The change-year spike is the 0–4 bin of the event study around any change in representation.

Across the 72 form-specific coefficients of the family as first fit, with its five-year climate and lagged rows shown in Table 10, 10 reach $p < 0.05$, against roughly 4 expected by chance, and none survives a Benjamini–Hochberg false-discovery-rate correction (the lowest q-value is 0.18). Four of the ten involve rebellions: the reversed climate association in the bare, additive, and Wahl-style specifications, and the negative association with lagged guild representation. Two more are the lagged guild and bare climate coefficients in the pooled violent forms, which rebellions dominate, so they are not independent signals. Three involve unspecified unrest, the contemporaneous and lagged guild coefficients and the annual-re-fit interaction of severe drought with burgher/guild government, and one involves peaceful action, the negative annual-re-fit severe-drought term; none involves coups. Lagged guild representation is negative for rebellions, riots, peaceful action, and the pooled violent forms, positive for coups and unspecified unrest, and individually significant for rebellions, the pooled violent forms, and, with its positive sign, unspecified unrest; lagged burgher representation is not individually significant in any column and changes sign across them. In this catalog a guild takeover of the council counts as a rebellion, not a coup; Section B.32 looks at coups more closely.

The two five-year rows at the top of Table 10 come from specifications the main text no longer uses. Rebuilt with the annual main specification in their place, with seat indicators from the year before and Conley standard errors, these rows form a family of 18 form-specific tests, corrected separately from the other rows, whose standard errors are city-clustered. Of the 18, 3 reach $p < 0.05$, and 3 have a Benjamini–Hochberg q-value below 0.10: guild seats the year before for rebellions ($q = 0.016$); guild seats the year before for the pooled violent forms ($q = 0.025$); and guild seats the year before for unspecified unrest ($q = 0.049$). The other rows, 42 tests, have no q-value below 0.32, and pooling all 60 tests of the rebuilt family, whose annual rows come from fits not shown in the table, leaves a lowest q of 0.054. The surviving tests are

Table 10: Headline specifications re-fitted by form of unrest: coefficient relative to the outcome's base rate (p-value)

Specification	All unrest	Coup	Rebellion	Riot	Peaceful	Unspecified	Violent (pooled)
Five-year: wet-dry severity	0.09 (0.18)	0.34 (0.28)	0.15 (0.09)	-0.09 (0.70)	0.15 (0.42)	0.10 (0.64)	0.13 (0.10)
Five-year, lagged: guild rep., t-1	-0.26 (0.07)	0.23 (0.57)	-0.66 (0.00)	-0.01 (0.96)	-0.26 (0.36)	0.89 (0.03)	-0.49 (0.01)
Five-year, lagged: burgher rep., t-1	0.02 (0.92)	1.18 (0.07)	0.03 (0.89)	0.29 (0.68)	0.07 (0.87)	-0.51 (0.23)	0.14 (0.49)
Template, bare (annual): wet-dry severity	0.12 (0.03)	0.22 (0.11)	0.19 (0.02)	-0.08 (0.57)	0.19 (0.18)	0.23 (0.23)	0.13 (0.05)
Template, additive (annual): wet-dry severity	0.10 (0.04)	0.27 (0.07)	0.15 (0.05)	-0.07 (0.65)	0.11 (0.37)	0.16 (0.40)	0.11 (0.08)
WJ re-fit (annual): severe drought	-0.01 (0.97)	0.01 (0.99)	0.42 (0.16)	-0.51 (0.71)	-1.24 (0.01)	-0.13 (0.88)	0.41 (0.24)
WJ re-fit (annual): severe drought x burgher/guild	-0.94 (0.05)	-0.65 (0.72)	-0.93 (0.20)	0.48 (0.78)	-0.40 (0.60)	-2.30 (0.02)	-0.98 (0.13)
Drought bins (annual): extreme-drought bin	-0.49 (0.10)	-0.79 (0.50)	-0.59 (0.14)	0.27 (0.83)	-0.99 (0.16)	-0.41 (0.71)	-0.48 (0.18)
Wahl-style (annual): burgher rep.	0.23 (0.30)	1.79 (0.12)	0.05 (0.86)	-0.19 (0.62)	0.17 (0.65)	0.58 (0.24)	0.25 (0.32)
Wahl-style (annual): wet-dry severity	0.11 (0.03)	0.26 (0.08)	0.15 (0.04)	-0.06 (0.66)	0.11 (0.37)	0.21 (0.27)	0.11 (0.07)

Notes:
Each cell re-fits the row's specification with the outcome restricted to city-matched episodes of the column's form and reports the coefficient over that outcome's base rate, with the p-value in parentheses. Forms overlap; Violent (pooled) combines revolution, coup, rebellion and riot. Main-text specifications and fixed effects, with city-clustered standard errors; the Wahl-style rows drop the guild-revolt spillover term. Forms with fewer than 15 events are not fitted.

guild-seat associations in rebellions, in the pooled violent forms that rebellions dominate, and in unspecified unrest. The five-year table is kept for comparison with the family as first fit.

B.9 By form of unrest

Figure 2 re-fits the annual main specification for each form of unrest. The wet-dry association is positive for every form except riots and significant at the 5% level only for rebellions, and at the 10% level for coups and for violent unrest pooled. The guild association is negative for rebellions, riots, and violent unrest pooled, near zero for peaceful action, and positive for coups and unspecified episodes; it is clearest for rebellions, the form that includes the guild and commune risings that deposed or entered a council, and it carries the change-year caveat of the main text. Most form-specific intervals are wide, and the forms overlap, so the figure shows the spread of the estimates rather than separate tests.

B.10 Who carried out the unrest?

Table 11 splits the outcome by who carried out the unrest, as the catalog's perpetrator field records it, and by the catalog's urban and rural flags. An episode counts for a group if any of its source rows names that group, so the groups overlap. Linked episodes are overwhelmingly urban (325 urban against 29 rural), and all 27 peasant-led episodes are among the 29 rural ones.

The wetter-years association is clearest for guild-led and elite-led unrest: about 18% and 21% above their base rates per scPDSI unit in the main specification, against nothing for burgher-led unrest. Rural and peasant-led unrest point the same way, wetter rather than drier, but rest on 29 and 27 episodes and are not significant, so this catalog cannot test whether drought raised peasant unrest as it does elsewhere (Jia, 2014). Guild-led unrest also shows the change-year pattern of the main text: guild seats the year before go with about 105% less

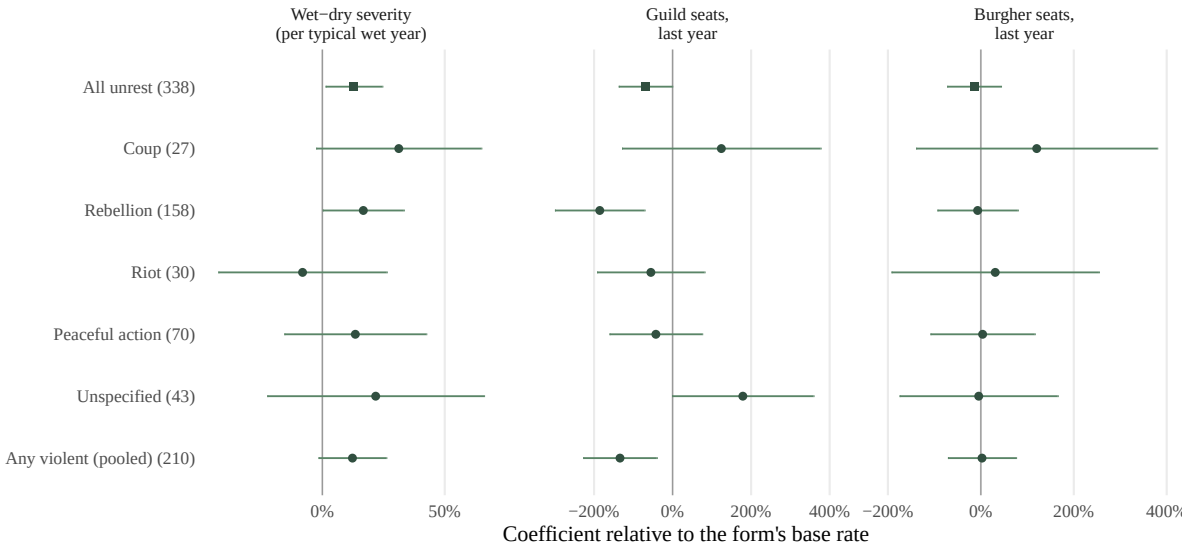


Figure 2: Main-text regressions by form of unrest. Each point is a coefficient from the annual main specification (institutions the year before) re-fit with that form as the outcome, divided by the form's base rate; scPDSI is per standard deviation of its variation within cities and years. Bars are 95% confidence intervals from city-clustered standard errors. Unrest city-years in parentheses. Forms overlap; the pooled violent group contains coups, rebellions, and riots.

Table 11: Climate and institutions by who carried out the unrest

		scPDSI		Main specification		Settled years	
Group	Episodes	Bare	Main	Guild	Burgher	Guild	Burgher
Urban	325	+8%*	+10%*	-71%**	-11%	-14%	+1%
Rural	29	+34%	+10%	+15%	-55%	+2%	-47%
Peasant-led	27	+34%	+8%	-2%	-80%	-15%	-73%
Townspeople (guild or burgher)	281	+9%*	+11%**	-96%**	-28%	-18%	-17%
Guild-led	154	+17%**	+18%**	-105%*	-17%	+45%	+52%
Burgher-led	168	+4%	+7%	-82%**	-71%*	-57%	-59%
Elite-led	60	+17%**	+21%**	-40%	+25%	+32%	+55%

Notes:
Each cell is a coefficient as a share of the group's base rate. Outcome 100 if an episode of the group starts in the city-year, 0 otherwise. Bare: scPDSI and log population, all cities. Main: the main specification (scPDSI, guild and burgher representation the year before, market, log population, plague). Settled: the main specification on city-years at least five years from any change in guild or burgher representation. scPDSI per unit (higher is wetter). City and year fixed effects; Conley standard errors; *** 0.01, ** 0.05, * 0.1.

guild-led unrest, significant only at the 10% level, while on settled years the association is positive and insignificant, about 45%.

Peasant-led unrest, like rural unrest in general, points to a different institutional pattern, though none of its coefficients is significant. It is less frequent where the linked city has burgher seats, in the main specification as on settled years, and shows essentially no association with guild seats. The episodes behind it point to a specific setting: of the 27 peasant-led episodes, 14 were directed at a city government, and most of these were risings of rural subjects against the Swiss city-states that ruled them. Imperial cities such as Zurich and Bern were among the lords against whom late medieval peasants rose, alongside abbots, prince-bishops, and counts (Blickle, 2012, p. 12). Zurich, which in the institutional dataset has full guild representation throughout, faced four risings of its countryside in the catalog (1411, 1489, 1513, 1515). In 1489, after Zurich had built up a large territory from nobles and monasteries, a mandate against large dogs in the countryside set off a rising whose grievances were new taxes, a monopoly on salt purchases, changes to the appointment of local officials, and, above all, curbs on village assemblies and a sharper oath of obedience; two thousand armed peasants stood before the city, and the episode ended with the execution of burgomaster Hans Waldmann (Blickle, 2012, pp. 15–17). Blickle reads the Zurich case as a revolt against a ruler that stripped villages of their rights and extended its claim to legislate without limit (Blickle, 2012, p. 17), which is the kind of territorial government a guild regime set on protecting its members' markets might be expected to run.

The comparison does not yet single out guild government, however. Bern and Lucerne, with no guild representation in the dataset, faced more risings of their own countryside between them, and the 1513 wave, directed in Bern and Lucerne against councillors accused of taking foreign pensions, struck Zurich, with full guild representation, Solothurn, with partial guild representation, and patrician Bern and Lucerne alike. In Zurich, Solothurn, and Bern such

Table 12: Replicated vs. published coefficients, Wigton-Jones (2025) Table 6

Variable	Rep. (Col. 1)	Pub. (Col. 1)	Rep. (Col. 2)	Pub. (Col. 2)	Same sign
Drought severity	0.0329	0.0329	0.1192	0.1190	Yes
Flood severity	0.0248	0.0248	0.0193	0.0193	Yes
Election x Drought	-0.0613	-0.0613	-0.1143	-0.1140	Yes
Election x Flood	-0.0368	-0.0368	-0.0414	-0.0414	Yes
Election	-0.0487	-0.0487	0.0077	0.0077	Yes
Burgher Rep x Drought	0.0211	0.0211	0.1242	0.1240	Yes
Burgher Rep x Flood	0.0202	0.0202	-0.0025	-0.0025	Yes
Burgher rep	-0.0726	-0.0726	-0.1565	-0.1560	Yes
Guild x Drought	0.0472	0.0472	0.0749	0.0749	Yes
Guild x Flood	-0.0221	-0.0221	0.0804	0.0804	Yes
Guild	-0.0371	-0.0371	-0.0250	-0.0250	Yes

Notes:
Column 1: conflict start; column 2: conflict incidence; both on Wigton-Jones's own 324-city, 900–1799 replication data, not this paper's panel. City and year fixed effects; Conley standard errors at 100 km and 50 years, as published. All 22 coefficients match the published values to within 0.0005; N = 291,600 in both columns, as published.

conflicts were often settled by treaties between the city and its rural subjects (Blickle, 1979, p. 238). What these episodes share is a city that ruled a rural territory, whatever its constitution. With city fixed effects the regression coefficient does not compare Zurich with Bern at all; it rests on a handful of other cities whose representation changed, and on fewer than thirty episodes, several of them directed at abbots and lords rather than at the linked city. Testing whether guild regimes governed their hinterlands more harshly would need a record of which cities held rural territories and from when, and I leave it to future work.

B.11 Validation: exact replication of Wigton-Jones (2025) Table 6

The main text's Wigton-Jones benchmark (the Climate and Unrest section) refers to this appendix for its code-validation step: before applying Wigton-Jones's (2025) method to the main paper's own panel (the annual re-fit in the main text), I first confirm my own implementation of his spatial-HAC estimator reproduces his own published numbers on his own data. Having obtained his full ICPSR replication package (study 206441), I reproduce his published Table 6 directly on his own 324-city, 900–1799 panel, using my own implementation of his spatial-HAC standard-error correction (implementing the Conley (1999) estimator, replicating his 100 km / 50-year distance and lag cutoffs) against plain city-and-year fixed effects, with no covariates or fixed effects beyond his own published specification. All 22 reported coefficients across both columns match the published values to within 0.0005, same sign on every row, matching N exactly (291,600). This confirms the replication pipeline is correctly implemented before any of it touches my data—the benchmark results reported in the main text cannot be attributed to an implementation error.

Table 13: Wigton-Jones's Table 6 specification on his Empire panel, with multi-city years removed

Outcome	Conflict start			Conflict incidence		
Sample	Published	No 1503	No multi-city	Published	No 1503	No multi-city
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Drought severity	0.0330**	0.0432***	-0.0055	0.1192***	0.1289***	0.0853***
	(0.0146)	(0.0137)	(0.0069)	(0.0289)	(0.0282)	(0.0251)
Drought severity × Elected council	-0.0613**	-0.0138	-0.0097	-0.1143**	-0.0697*	-0.0676*
	(0.0242)	(0.0135)	(0.0109)	(0.0490)	(0.0379)	(0.0387)
Drought severity × Burgher rep.	0.0211	-0.0100	0.0046	0.1242**	0.0966*	0.1089**
	(0.0236)	(0.0140)	(0.0114)	(0.0628)	(0.0544)	(0.0539)
Drought severity × Guild rep.	0.0472	-0.0136	0.0086	0.0750	0.0176	0.0400
	(0.0389)	(0.0206)	(0.0161)	(0.0711)	(0.0559)	(0.0550)
City	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	291,600	291,276	285,768	291,600	291,276	285,768

Conley standard errors (100 km, 50 years) in parentheses
*Signif. codes: ***: 0.01, **: 0.05, *: 0.1*

His 324-city Empire panel, 900–1799; outcomes 100 if a conflict starts (columns 1–3) or is under way (columns 4–6). No multi-city: drops years in which ten or more cities start a conflict. Flood terms, government main effects and century-geography controls not shown. Stars use Bartlett weights; under his equal-weight windows the published start coefficient is significant only at the 10% level.

B.12 Wigton-Jones's conflict starts and the catalog

The main text traces the divergence from Wigton-Jones to the events the two catalogs record. Table 13 and Table 14 report the regressions behind that reading, all with his Table 6 specification and Conley standard errors at his cutoffs. The first uses his Empire panel, 900–1799: his published conflict-start column, the same without 1503 (the War of the Landshut Succession, entered once for each city on the warring sides), and the same without the 18 years in which his data record conflict starts in ten or more cities at once. Those years hold 372 of his 572 conflict starts in the Empire; 3 of them fall in 1300–1520 (1377, 1503, 1519). The second uses the cities both panels cover, 1300–1520, and swaps the outcome and the government codes between the two datasets: his conflict starts or my episode starts, his century codes or my annual guild and burgher seats.

His conflict-incidence column, which counts every year a conflict is under way rather than its start, behaves differently (Table 13, columns 4–6). Its drought coefficient is 0.119 as published ($p < 0.001$) and 0.085 once the multi-city years are dropped ($p < 0.001$); without 1503 alone it is 0.129. The elected-council × drought term, which carries his muting result, weakens from -0.114 ($p = 0.020$) to -0.068 ($p = 0.081$). On his own data, then, the drought association in conflict incidence does not rest on the years in which many cities start a conflict at once, while the muting term leans on them more.

These standard errors use Bartlett weights, the convention throughout this paper. Wigton-

Table 14: Wigton-Jones's Table 6 specification: his published estimates, and on the cities both panels cover with the two catalogs swapped

Sample	900–1799	Cities both panels cover, 1300–1520				
Outcome	His conflict starts				My episode starts	
Government codes	His		His, no 1503	Mine	His	Mine
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Drought severity	0.0330**	-0.0377	0.0113	-0.0495	-0.0628	-0.0627
	(0.0146)	(0.0470)	(0.0336)	(0.0429)	(0.0505)	(0.0474)
Drought severity × Elected council	-0.0613**	-0.1110*	0.0040		0.0352	
	(0.0242)	(0.0599)	(0.0290)		(0.0520)	
Drought severity × Burgher rep.	0.0211	0.0493	-0.0005		0.0862	
	(0.0236)	(0.0677)	(0.0363)		(0.0653)	
Drought severity × Guild rep.	0.0472	0.1334	0.0012		-0.1503***	
	(0.0389)	(0.0919)	(0.0392)		(0.0549)	
Drought severity × Burgher seats				-0.0539		0.0059
				(0.0574)		(0.0569)
Drought severity × Guild seats				0.2320*		-0.0723
				(0.1240)		(0.0675)
City	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	291,600	43,500	43,288	43,500	43,500	43,500

Conley standard errors (100 km, 50 years) in parentheses
*Signif. codes: ***: 0.01, **: 0.05, *: 0.1*

Column 1: his published conflict-start column, on his 324-city Empire panel, 900–1799; its stars use Bartlett weights, and under his equal-weight windows the drought coefficient is significant at the 10% level, as published. Columns 2–6: cities both panels cover, 1300–1520, city-years with both codings; outcome 100 if a conflict (his) or unrest episode (mine) starts in the city-year. Column 3 also drops 1503. His codes: elected council, burgher and guild representation at century marks; mine: annual guild and burgher seats. Flood terms, government main effects and century-geography controls not shown.

Jones's acreg windows weight every pair inside the cutoffs equally; with uniform weights the drought standard error in his published conflict-start column is 0.0181, against his published 0.0182. The conclusions above hold under uniform weights: drought in the conflict-start column has p 0.069 as published, 0.010 without 1503, and 0.478 without the multi-city years; the election × drought term has p 0.033 as published and 0.326 without 1503.

Dropping the multi-city years selects on the outcome: a drought that set many cities fighting at once would be removed with them. Table 15 therefore keeps every year and counts each year's conflict starts once, giving each of the cities that start a conflict in a year a share of one unit, so that a war entered for twenty-six cities weighs as much as a single start; his data carry no conflict identifiers, and Brecke's catalog no country codes for European wars, so a conflict cannot be counted once directly. The weighting stands in for that count, but it also merges unrelated starts of the same year into one unit, so in busy years it understates the number of distinct conflicts; it is a conservative stand-in for an exact count, which is why the year-drop

Table 15: Wigton-Jones's Empire conflict-start column with each year's starts counted once

Outcome	Drought, Conley	Drought, by city and year	Election by drought, Conley	Election by drought, by city and year	Base rate	N
Published outcome	0.0329** (0.0146)	0.0329 (0.0262)	-0.0613** (0.0242)	-0.0613 (0.0472)	0.196	291,600
Each year's starts = 1	0.0030 (0.0029)	0.0030 (0.0029)	-0.0085 (0.0056)	-0.0085 (0.0054)	0.040	291,600
Each year's starts = 1, without 1503	0.0034 (0.0029)	0.0034 (0.0029)	-0.0070 (0.0057)	-0.0070 (0.0053)	0.039	291,276

Notes:
His Table 6 column 1 specification on his Empire panel, 900–1799. Rows 2–3: each city's conflict start is 100 divided by the number of city starts in that year. Election by drought: his elected-council indicator times drought severity. Standard errors in parentheses: Conley at his cutoffs, or clustered by city and year, with two-way clustered matrices adjusted to be positive semi-definite; *** 0.01, ** 0.05, * 0.1.

benchmark at the end of this paragraph is reported beside it. As published, drought raises the chance of a conflict start by +17% of the base rate per unit of severity (significant at the 5% level), and elected councils mute it by 31% of the base (significant at the 5% level). Counting each year's starts once, the drought term is +8% of its base (not significant at the 10% level) and the muting 21% (not significant at the 10% level). Clustering by city and year instead, which treats each year's starts as one draw, the published terms themselves are not significant at the 10% level. Dropping random sets of years that hold as many starts as the 18 multi-city years (372; a median of 75 years per draw, 200 draws) leaves a drought coefficient between -0.014 and 0.031, against 0.033 as published and -0.005 without the multi-city years, so losing that many starts in any years weakens his estimate about as much as losing those.

B.13 Wigton-Jones's drought bins, conflict incidence, and effect size

Four further checks bear on how the benchmark is framed. The first takes the annual re-fit of the main text's benchmark and replaces the bottom-decile indicator with Wigton-Jones's own drought bins, severe for scPDSI from -4 to -3 and extreme below -4, each interacted with government type (Table 16, column 2). Neither bin goes with significantly more or less unrest in non-participative city-years, the reference type (severe -0.25 percentage points, $p = 0.110$; extreme -0.11, $p = 0.541$), while extreme drought in burgher/guild city-years goes with -0.64 points less unrest ($p = 0.034$), the same pattern as the decile version. The second builds an incidence outcome like his conflict-incidence column, counting a city-year from an episode's start to its effective end: 691 city-years with unrest under way against 342 with a start, since 34% of linked episodes last more than a year. On incidence none of the drought main effects is significant: the bottom-decile coefficient is +0.07 ($p = 0.779$), and the severe and extreme bins -0.30 and -0.11 ($p = 0.232$ and 0.665). In the main specification scPDSI keeps its wetter-years sign, +0.13 points per unit ($p = 0.045$).

The third check restricts his conflict-start column to the cities both panels cover, 1300–1520. There his drought coefficient is -0.0370, with a 95% interval from -0.118 to 0.044, against 0.0329 (-0.003 to 0.069) on his full Empire panel. The restricted interval contains both zero and his published value, so on the matched cities and years his own data cannot tell his effect from none. For conflict incidence the restricted estimate is 0.273 (0.056 to 0.489), against his 0.119. The fourth asks whether my data rule out an effect of his size, comparing like with like: drought severity in his form, the larger of zero and $-$scPDSI, with flood severity beside it, both interacted with government type, and the main controls. In the reference

category, cities with no elected council and no burgher or guild representation in his data and non-participative city-years in mine, his drought coefficient of 0.0329 on a conflict-start base rate of 0.196 is +16.8% of the base rate per unit of drought severity. My estimate is -6.5% of my base rate per unit, with a 90% interval from -17.5% to +4.5%. The interval excludes his effect: the one-sided test of an effect at least as large as his gives $p < 0.001$. Averaging each side's drought coefficients over its own sample's government types gives +18.3% for his data and -8.9% for mine (90% interval -19.0% to +1.2%, $p < 0.001$). The secondary versions, without the government interactions or with the bottom-decile indicator, for which his per-unit effect times the mean drought severity of my bottom-decile years implies +88% of the base rate against my 90% interval of -63% to +16%, also exclude his effect.

B.14 Drought bins and the outcome ladder

The main text's diagnosis of the Wigton-Jones divergence replaces the continuous measure with his drought and flood bins. The bins sort each city-year by its scPDSI value, on which 0 is normal and negative values are drought: mild drought from -1 to -2, moderate from -2 to -3, severe from -3 to -4, and extreme below -4, with matching flood bins on the wet side, each compared with near-normal years between -1 and $+1$. Severe drought is thus the band just short of the extreme tail; in the annual panel it holds 8% of city-years and the extreme bin 10%. Every drought bin carries a deficit of documented unrest, the severe and extreme bins the largest, the extreme bin's slightly above the severe bin's, although only the severe bin reaches even the 10% level (Table 17, first row). The severe-drought indicator of the main text's two benchmarks is a different cut, the driest tenth of observations. In five-year periods it begins at about -3.0 and spans the severe and extreme bins together; in single years it begins at almost exactly -4 and so coincides with the extreme bin, though there it enters interacted with government type, so its main effect describes non-participative cities only.

Table 17 re-estimates the bins on narrower slices of the catalog. If counting peaceful and minor episodes produced the reversal, the drought deficit should shrink and turn positive as the outcome narrows to the violent episodes both definitions count. It does not. Relative to each outcome's base rate, severe- and extreme-drought city-years carry about 44% and 49% less unrest on the broad measure, the severe bin significant only at the 10% level; for revolution, rebellion, or coup, which in this catalog are exactly the violent episodes of major severity and so the closest analog to a casualty threshold, about 88% and 53% less; and for any violent episode about 78% and 48%; the severe-bin deficits of both violent rungs are significant. Peaceful action alone, with only 2 events in the extreme bin, is too thin to read.

B.15 The severe-drought threshold

The five-year benchmark in the main text marks severe drought as the driest tenth of five-year city-periods. Figure 3 shows where that cutoff falls in the distribution of wet-dry severity.

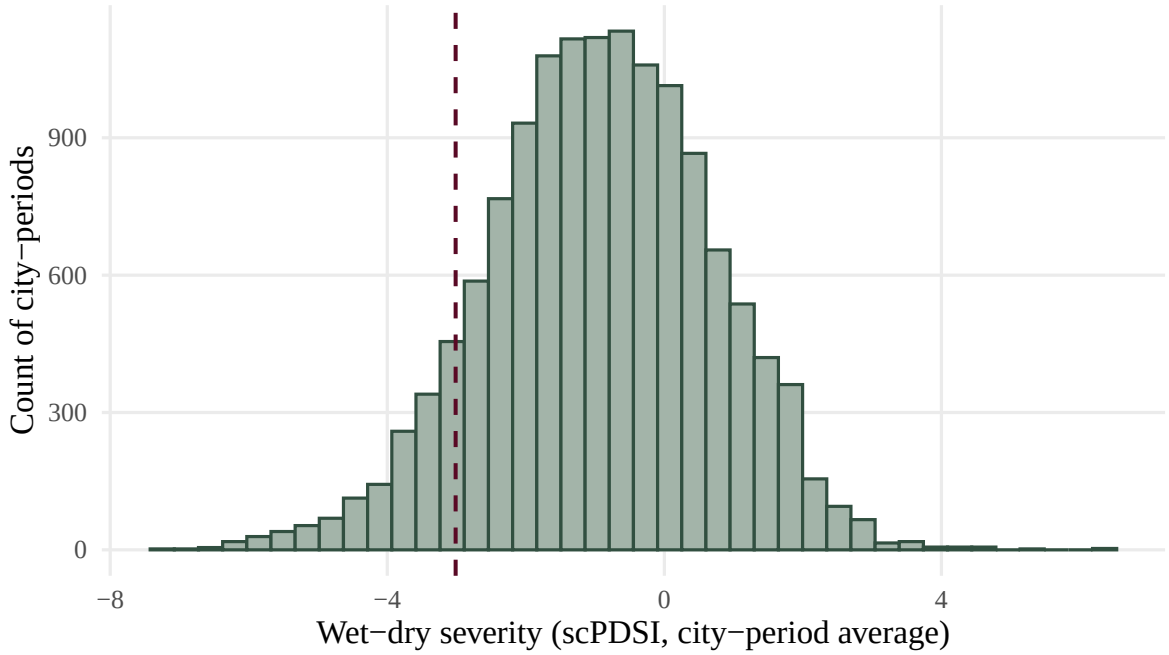


Figure 3: Distribution of wet-dry severity (scPDSI) across the analysis panel's five-year city-periods. Negative values indicate drought; the marked line is the 10th-percentile severe-drought threshold (-3.013).

Table 16: The annual benchmark with Wigton-Jones's drought bins and with unrest under way as the outcome

Outcome	Episode start		Unrest under way		
Drought measure	Decile	His bins	Decile	His bins	scPDSI
Model:	(1)	(2)	(3)	(4)	(5)
Variables					
Bottom-decile scPDSI	-0.005		0.066		
	(0.171)		(0.236)		
Bottom-decile scPDSI × Broader burgher rep.	0.014		-0.551		
	(0.329)		(0.503)		
Bottom-decile scPDSI × Burgher/guild	-0.628**		-0.517		
	(0.300)		(0.596)		
Severe-drought bin		-0.245		-0.296	
		(0.153)		(0.247)	
Severe-drought bin × Broader burgher rep.		0.220		-0.536	
		(0.331)		(0.534)	
Severe-drought bin × Burgher/guild		-0.109		-0.837	
		(0.441)		(0.514)	
Extreme-drought bin		-0.110		-0.112	
		(0.180)		(0.260)	
Extreme-drought bin × Broader burgher rep.		-0.079		-0.725	
		(0.295)		(0.509)	
Extreme-drought bin × Burgher/guild		-0.640**		-0.623	
		(0.301)		(0.608)	
scPDSI					0.129**
					(0.064)
City	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes
Fit statistics					
Observations	51,315	51,315	51,315	51,315	51,063

Conley standard errors (100 km, 50 years) in parentheses
*Signif. codes: ***: 0.01, **: 0.05, *: 0.1*

Annual panel, 1300–1520. Columns 1–2: outcome 100 if a linked episode starts in the city-year; columns 3–5: 100 while one is under way, from its start to its effective end. Severe bin: scPDSI from −4 to −3; extreme: below −4. Columns 1–4 interact the drought measure with the benchmark's government types and control for log population and plague; column 5 is the main specification. City and year fixed effects.

B.16 City-clustered and spatial-HAC standard errors for the annual re-fit

The annual re-fit in the main text reports spatial-HAC (Conley) standard errors. Table 18 sets them beside city-clustered standard errors for the same fit; the point estimates are identical, and the guild interaction is the only term whose significance changes between the two.

Table 17: Outcome ladder: severe- and extreme-drought bins

			Severe drought			Extreme drought		
Outcome	Unrest city-years	Base rate (%)	Events	Coef. (% of base)	p	Events	Coef. (% of base)	p
1. All documented unrest (paper's measure)	342	0.666	24	-44	0.09	29	-49	0.10
2. Organized peaceful action only	56	0.109	8	46	0.55	2	-105	0.10
3. Any violent episode	210	0.409	10	-78	0.01	20	-48	0.18
4. Revolution, rebellion, or coup (= violent, major severity)	188	0.366	9	-88	0.01	17	-53	0.15

Notes:
Rungs narrow the broad documented-unrest measure, to peaceful action, which the comparator excludes, or toward his armed-conflict definition; revolution, rebellion, or coup, which here coincides with violent episodes of major severity, stands in for a casualty threshold the catalog cannot apply. Severe drought is scPDSI from -4 to -3 (4,134 city-years), extreme below -4 (5,180 of 51,315). Coef. is the bin's coefficient from the drought-bin specification as a percentage of the rung's base rate, with its Conley p-value (100 km, 50 years); events are the rung's unrest city-years in the bin.

Table 18: City-clustered vs. spatial-HAC (Conley) standard errors, same specification on the annual panel

Term	Coefficient	(City-clustered SE)	(Spatial-HAC SE)	SE ratio
Severe drought (10th pctile)	-0.0001	(0.0017)	(0.0017)	1.017
Broader burgher rep.	0.0021	(0.0021)	(0.0021)	0.968
Burgher/guild	0.0052	(0.0024)**	(0.0023)**	0.947
Log population	0.0004	(0.0014)	(0.0013)	0.965
Plague indicator	0.0066	(0.0047)	(0.0052)	1.101
Severe drought x Broader burgher rep.	0.0001	(0.0034)	(0.0033)	0.977
Severe drought x Burgher/guild	-0.0063	(0.0032)*	(0.0030)**	0.923

Notes:
The same specification (severe drought x government type, log population, plague, city and year fixed effects) on the annual panel (N = 51,315, 252 cities), with city-clustered and with Conley standard errors (100 km, 50 years). Point estimates are identical; the SE ratio is Conley over city-clustered; stars give significance under each set of standard errors (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). Government types as in the Wigton-Jones benchmark table.

B.17 Spatial-HAC bandwidth sweep for the annual re-fit

The annual re-fit in the main text uses Wigton-Jones's own 100 km distance cutoff so that it changes only the estimator. That cutoff was chosen for his continent-wide panel, and the defensible radius for this one is at least twice the roughly 55 km Old World Drought Atlas cell, and 75 km is included only as a lower-bound check. Table 19 therefore refits that specification at 75, 100, 130, 175, and 200 km, holding the 50-year lag cutoff fixed. Point estimates do not depend on the radius; only the standard errors do. Across the range, no spatial-HAC standard error moves by more than about 1.5% from its 100 km value, and no term changes side of the 5% line: the drought × burgher/guild interaction stays between $p = 0.033$ and 0.037 and the burgher/guild main effect between $p = 0.024$ and 0.025 at every radius. The annual result does not depend on the choice of cutoff.

Table 19: Annual re-fit: spatial-HAC standard errors by distance cutoff

Term	Coefficient	75 km	100 km	130 km	175 km	200 km
Severe drought (10th pctile)	-0.0001	0.0017 [0.975]	0.0017 [0.975]	0.0017 [0.975]	0.0017 [0.975]	0.0017 [0.975]
Broader burgher rep.	0.0021	0.0021 [0.301]	0.0021 [0.303]	0.0021 [0.304]	0.0021 [0.302]	0.0021 [0.301]
Burgher/guild	0.0052	0.0023 [0.024]	0.0023 [0.025]	0.0023 [0.025]	0.0023 [0.024]	0.0023 [0.024]
Log population	0.0004	0.0013 [0.782]	0.0013 [0.782]	0.0013 [0.782]	0.0013 [0.782]	0.0013 [0.782]
Plague indicator	0.0066	0.0052 [0.201]	0.0052 [0.200]	0.0052 [0.201]	0.0052 [0.200]	0.0052 [0.199]
Severe drought x Broader burgher rep.	0.0001	0.0033 [0.967]	0.0033 [0.967]	0.0033 [0.967]	0.0033 [0.967]	0.0033 [0.967]
Severe drought x Burgher/guild	-0.0063	0.0030 [0.037]	0.0030 [0.036]	0.0030 [0.035]	0.0030 [0.034]	0.0030 [0.033]

Notes:
Same specification and annual panel as the main text's annual re-fit table (severe drought x government type, log population, plague indicator, city and year fixed effects; N = 51,315, 252 cities). Each column refits the spatial-HAC (Conley) correction at the stated distance cutoff with a 50-year lag cutoff; 100 km is Wigton-Jones's own cutoff and the main-text value. Cells report the spatial-HAC standard error with its p-value in brackets. The coefficient column is identical at every radius by construction (OLS within the same fixed effects). Burgher/guild is a city-year in which guild representatives hold voting seats; broader burgher rep. is burgher seats without such guild seats.

B.18 Documentation density and the drought deficit

The catalog is a lower bound compiled from secondary sources whose density varies with geography and city size (main text, Data section), so drought might have displaced unrest toward places and episodes less likely to reach the secondary literature. If so, the drought deficit should be *deepest where documentation is thinnest*, since that is where drought-year episodes would go unrecorded. Table 20 re-fits the drought-bin specification of the main text's Wigton-Jones diagnosis on four contrasts: episodes corroborated by two or more named sources against the full catalog; cities above against below the median count of recorded institutional changes, a density measure built from the institutional record rather than from unrest, though partly endogenous to it, since revolts produced many of those changes; the largest against the smallest population tercile; and three latitude bands.

The test does not settle the question. Only the low-documentation cities ($p = 0.086$) and the largest population tercile ($p = 0.085$) reach even the 10% level, so it rests on the pattern of point estimates, each read against its own base rate. One contrast runs the way a documentation artifact predicts: in cities with below-median institutional documentation the deficit is about 83% of the base rate, about 4 times the 23% in better-documented cities, though on only 155 events and with the gap itself untested. The other two run against it. Episodes corroborated by two or more named sources, the subset least exposed to any one compiler's selection, show a deficit as large as the full catalog's or larger (61% against 49%), and the largest and smallest population terciles show about the same (57% and 53%). By region, the deficit is concentrated in the South (73%, on 85 events), weaker in the Center (16%), and absent in the North, where the coefficient is essentially zero; whether that reflects a regional difference in response, the band's composition, or noise cannot be resolved here. Thin documentation therefore remains open as an explanation.

Table 20: Extreme-drought coefficient across documentation and regional cuts

Cut	City-years	Events	Extreme drought	(SE)	p
Baseline (all episodes, all cities)	51,315	342	-0.0032	(0.0020)	0.102
(A) Multi-source episodes only	51,315	150	-0.0018	(0.0012)	0.125
(B) High-documentation cities	21,745	187	-0.0019	(0.0032)	0.547
(B-alt) Low-documentation cities	29,570	155	-0.0044	(0.0025)	0.086
(C) Largest population tercile	18,160	240	-0.0075	(0.0044)	0.085
(C-alt) Smallest population tercile	15,513	23	-0.0008	(0.0015)	0.598
(D) Center (48.5-51.5 N)	25,792	156	-0.0010	(0.0026)	0.713
(D) North (>= 51.5 N)	17,272	101	-0.0000	(0.0033)	0.999
(D) South (< 48.5 N)	8,251	85	-0.0075	(0.0058)	0.200

Notes:
Each row re-fits the main text's drought/flood-bin specification (Wigton-Jones's cutpoints, alongside government type; city and year fixed effects; Conley standard errors, 100 km and 50 years) on the stated sample or outcome and reports the extreme-drought bin. Documentation density counts recorded institutional change points per city. Multi-source episodes name two or more sources. Latitude cutpoints: 48.5 and 51.5 degrees North. Cuts with fewer than 15 unrest city-years are not fitted.

B.19 Representation changes around unrest onsets

The main text's event study puts a change in representation at time zero and asks how unrest moves around it. The account of how representation changed runs the other way, so Table 21 turns the design around. The event is the onset of a linked unrest episode; the outcome is 100 if the city's representation changes in the year; each coefficient is the change in that chance, in percentage points, per onset at that distance, against the decade 21 to 30 years before an onset. Bins, controls, fixed effects and Conley standard errors are those of the main text's figure, and cities that never revolt, and years far from any onset, form the comparison. In the five years from an onset, the chance of any change in representation rises by 4.19 points per onset, against a base rate of 0.54; the chance of a guild gain rises by 1.99 and of a burgher gain by 1.86. The pre-onset bins are jointly indistinguishable from zero ($p = 0.650$ for any change, 0.187 for guild gains and 0.239 for burgher gains), and the joint test on the bins ten to twenty-nine years after gives $p = 0.310$, 0.972 and 0.063. With single-year bins from five years before an onset to five after, the rise sits in the onset year itself: 13.15 points for any change, 8.61 for a guild gain and 4.88 for a burgher gain. The chance of any change stays above zero for the next three years (3.92, 2.79 and 1.86 points), as does the chance of a burgher gain (1.68, 1.62 and 1.30). In this finer version the pre-onset bins are jointly significant ($p < 0.001$), but because changes are less likely three and four years before an onset (-1.02 and -0.82 points), as they are four years after it (-1.10), not because they lead it. The regressions use the 282 changes that fall in their estimation sample, which needs the guild scale and every control observed in the year; the main text's count of 285 includes changes in years the regression cannot use.

Table 21: Changes in representation around unrest onsets

	Any change		Guild gain		Burgher gain	
Years from onset	Coef.	(SE)	Coef.	(SE)	Coef.	(SE)
31 or more before	0.04	(0.17)	-0.07	(0.14)	0.15*	(0.08)
16 to 20 before	0.13	(0.26)	-0.13	(0.20)	0.34*	(0.19)
11 to 15 before	0.12	(0.30)	0.05	(0.26)	0.35*	(0.19)
6 to 10 before	-0.01	(0.25)	-0.25	(0.18)	0.14	(0.16)
1 to 5 before	-0.28	(0.26)	-0.37*	(0.19)	0.11	(0.16)
0 to 4 after	4.19***	(0.55)	1.99***	(0.37)	1.86***	(0.33)
5 to 9 after	0.28	(0.29)	-0.16	(0.19)	0.10	(0.15)
10 to 14 after	0.36	(0.29)	-0.02	(0.19)	0.47**	(0.20)
15 to 19 after	0.54	(0.37)	0.14	(0.27)	0.22	(0.19)
20 to 24 after	0.06	(0.28)	0.02	(0.21)	0.14	(0.17)
25 to 29 after	0.40	(0.28)	-0.06	(0.22)	0.29	(0.21)
30 or more after	0.01	(0.16)	-0.10	(0.13)	0.13*	(0.08)
Base rate	0.54		0.27		0.25	
Changes	282		140		134	
City-years	52,209		52,209		52,573	

Notes:
Each coefficient is the change in the chance of the named change in representation (outcome 100 if it occurs in the city-year), in percentage points, per linked unrest onset at that distance, relative to 21 to 30 years before an onset; 352 onsets. Controls: scPDSI, market, log population, plague. City and year fixed effects; Conley standard errors; *** 0.01, ** 0.05, * 0.1.

B.20 Change-year estimates from the de Chaisemartin–d'Haultfœuille estimator

The main text checks the event study with the estimator of de Chaisemartin & d'Haultfœuille (2026), which compares each city's first change in representation with cities at the same level that have not yet changed. Table 22 reports the estimates with their standard errors and placebo tests. In the year of a city's first change in guild representation, unrest is 17.9 percentage points higher (standard error 4.16, $p < 0.001$), about 27 times the base rate; for burgher representation the estimate is 6.5 (3.82, $p = 0.091$). The ten placebo estimates before a first change are jointly indistinguishable from zero ($p = 0.321$ for guild and 0.439 for burgher representation), and none of them is individually significant at the 5% level. Restricting the switchers to gains gives 18.0 and 6.9 points. The estimator follows first changes only, which are almost all gains, so it says nothing about losses.

The event study cannot rule out a lasting change in unrest after a change in representation. In the main text's version, the four five-year bins from 10 to 29 years after a change and the bin of 30 years or more are jointly indistinguishable from zero for every kind of change (χ^2 with 5 degrees of freedom from 1.31 to 7.86, p from 0.164 to 0.933). Averaged over the bins from 10 to 29 years, unrest per gain in either institution is -0.08 points, with a 95% interval from -0.58 to 0.43, or from -88% to +65% of the base rate. The interval is -96% to +36% for

Table 22: Unrest in the year of a city's first change in representation: de Chaisemartin–d'Haultfœuille estimates

Representation	Changes	Estimate	SE	Placebos p<.05	Joint placebo p	Average effect	Switchers	Base rate
Guild	All first changes	17.86***	(4.16)	0 of 10	0.321	0.45 (0.84)	111	0.66
Guild	Gains only	18.03***	(4.20)	0 of 10	0.321	0.45 (0.85)	110	0.66
Burgher	All first changes	6.46*	(3.82)	0 of 10	0.439	-1.36 (1.66)	122	0.65
Burgher	Gains only	6.90*	(3.89)	2 of 10	0.299	-2.10 (1.65)	114	0.65

Notes:
Effects of a city's first change on the 0–3 scale, against cities at the same level that have not yet changed; outcome 100 if unrest, 0 otherwise. Estimate: the year of the change. Placebos: the ten years before it. Average effect: over the ten years from the change. No covariates; standard errors clustered by city, the estimator's own, which has no spatial-HAC form; *** 0.01, ** 0.05, * 0.1. Base rate on city-years with the scale coded.

any change, -88% to +115% for guild gains, and -156% to +57% for burgher gains. The data therefore cannot rule out lasting shifts of roughly the size of the base rate in either direction. The ten-year average effect of Table 22, change year included, is wider still, from -183% to +320% of the base rate for guild and -708% to +292% for burgher representation, and since it includes the change year it is not a measure of a lasting shift.

B.21 How far the event study survives drift in unrest

The main text's event study reads the windows just before a change as part of the pattern, since unrest builds up to the changes it wins, so they cannot serve as a test of parallel trends. Table 23 applies the sensitivity analysis of Rambachan & Roth (2023) with the roles set accordingly. The windows 30 to 11 years before a change show how much unrest drifts with no change near; the run-up, the window from the change, the years after it, and the fall from the change window to the next are bounded against that drift. The reference window is split into two five-year windows so that all windows are equally spaced, with 15 to 11 years before a change as the reference. The breakdown value is the largest multiple of the largest drift between the far windows that the drift near a change can reach with the 95% interval still excluding zero. For all changes, the spike survives drift up to 0.5 times that size and the fall after it up to 1.5 times; the years after a change stay at the baseline. Burgher gains, with guild changes held fixed, show no significant spike of their own.

B.22 How the changes in representation are dated

The spike in unrest that the main text's event study finds around changes in representation could come from the sources rather than the cities if the year of a change were read off the passage that narrates the revolt behind it. Each of the 285 changes is matched to the passage from which its year was coded and classified by rule in two ways. By precision, 186 are exact: the year stands in the passage, without "um", "vor", "nach", "erst", or "erstmals", outside a passage on first attestation ("urk. erw.", "bezeugt"), and with no range or uncertainty coded. 55 are approximate, 43 are set from a period ("Mitte 14. Jh.", "1430er") or inferred, and 1

Table 23: Event study around changes in representation: bounds under drift in unrest

Panel	Quantity	Estimate (pp)	Multiple of base	95% CI	Breakdown
All changes	Run-up, −10 to −1	1.14	1.7	[0.39, 1.88]	0.25
	Change window, 0 to 4	2.72	4.1	[1.69, 3.75]	0.5
	After, 5 to 29	0.05	0.1	[-0.51, 0.61]	–
	Fall after the change window	-2.82	-4.3	[-3.88, -1.77]	1.5
Guild gains	Run-up, −10 to −1	0.07	0.1	[-0.94, 1.09]	–
	Change window, 0 to 4	3.09	4.7	[1.53, 4.65]	0.5
	After, 5 to 29	0.18	0.3	[-0.64, 0.99]	–
	Fall after the change window	-3.36	-5.1	[-4.84, -1.89]	1.5
Burgher gains	Run-up, −10 to −1	0.81	1.2	[-0.26, 1.87]	–
	Change window, 0 to 4	1.02	1.6	[-0.54, 2.57]	–
	After, 5 to 29	-0.23	-0.3	[-1.05, 0.60]	–
	Fall after the change window	-1.46	-2.2	[-2.90, -0.02]	0

Notes:
Main specification of the event study with the other kinds of change as controls, in equally spaced five-year windows; estimates per change against the window 15 to 11 years before. Breakdown: the largest multiple of the largest drift between the windows 30 to 11 years before a change (grid 0.25 to 2) at which the 95

has no passage. By wording, 89 passages use the vocabulary of conflict (Aufstand, Unruhen, Zunftkampf, Streit, Zwiespalt, Beschwerden, Revolution, and the like), 54 of them for a change that falls within an episode linked to the city; the others describe the constitution alone.

I checked every seventh change, 41 in all, against its passage by hand. The rule errs only toward caution. All 27 changes it classed as exact are exact on reading, while 6 of the 11 it classed as approximate are exact too, held back by a range or uncertainty coded in the dataset, by an attestation word about another year, or by a second mention of the year in a range. None of the 14 passages it read as conflict-worded was misread; 2 of the 27 it read as constitutional describe a conflict in words the rule lacks (“Eindringen der Zünfte” in Kassel, and a French passage on the Strasbourg Geschölle). The exact cut is therefore clean but smaller than it could be, and the cut without conflict vocabulary keeps a few changes that a passage narrates with their conflict, which can only enlarge the spike in that cut. On reading, every year classed as exact is given in its passage as the date of the change itself; whether the compiler took that year from a chronicle’s date for the revolt cannot be told from the passage.

Table 24 refits that event study with the bins of the kept changes as the terms of interest and the bins of the other changes, and of other kinds of change, as controls, so every change stays in the model. Exactly dated changes reproduce the full pattern: for gains in either institution, unrest runs 2.9 points above the baseline per gain in the five years from it and 0.9 in the five years before, against 2.6 and 0.7 for all changes. Exact dating addresses imprecise dates, not the narrative. Changes dated from a passage without conflict vocabulary keep a smaller spike, 0.9 points (significant at the 5% level), and lose the rise before it (0.3, not significant at the 10% level); for guild gains alone the spike is 0.9 points (not significant at the 10% level). In the de Chaisemartin–d’Haultfœuille estimator, dropping the cities whose first guild change is

Table 24: Unrest around changes in representation, by how the change is dated

Changes	Gains kept	Gains, -10 to -6	Gains, -5 to -1	Gains, 0 to 4	Guild gains, 0 to 4	dCDH, guild (year of change)	dCDH, burgher (year of change)
All (main text)	234	0.21 (0.41)	0.70 (0.43)	2.61*** (0.53)	2.90*** (0.73)	17.86*** (4.16)	6.46* (3.82)
Exactly dated	159	0.11 (0.52)	0.88 (0.57)	2.91*** (0.68)	2.77*** (0.89)	15.46*** (4.83)	7.69 (5.46)
No conflict wording	167	0.30 (0.44)	0.25 (0.43)	0.87** (0.43)	0.85 (0.65)	8.22*** (3.13)	2.18 (3.23)
Exact, no conflict wording	108	0.20 (0.57)	0.40 (0.60)	0.91* (0.53)	0.74 (0.80)	6.99** (3.51)	3.68 (4.57)
Linked episodes dropped	234	0.43 (0.37)	-0.61*** (0.22)	-0.61*** (0.22)	-0.43 (0.29)	–	–

Notes:
Columns 3–6: event-study coefficients (percentage points per change) for the named window; changes not kept are controls. Columns 7–8: estimates for the year of a city's first change, without cities whose first change fails the cut; not estimated for the last row, whose outcome omits episodes tied to a change. Standard errors in parentheses, Conley (3–6) or by city (7–8); *** 0.01, ** 0.05, * 0.1.

dated from a conflict passage lowers the change-year estimate from 17.9 to 8.2 points, about 16 times the base rate (significant at the 1% level), and requiring an exact date as well gives 7.0 (significant at the 5% level); for burgher representation the estimate is not significant at the 10% level (all changes: significant at the 10% level). Part of the spike, then, is passages that narrate a revolt and its settlement together, but changes the sources date on their own still come with unrest in the same years.

The rise before a change could mean that cities without seats were primed for unrest, or that the change comes a few years into the episode that won it. Dropping from the outcome the 80 episode links tied to a change (an episode is tied to a change that falls from one year before it begins to two years after it ends) and keeping every change as the event separates the two. In the window 6 to 10 years before a change, which loses only 9 dropped onsets (against 37 in the five years before), the remaining unrest is at the baseline: 0.4 points for gains in either institution (not significant at the 10% level), 0.5 for guild and -0.3 for burgher gains. In the five years before a gain it is below the baseline (-0.6), partly by construction, since every onset from two years before a change to one year after it is linked to the change and so dropped; the window from the change is depressed the same way. Nothing rises before a gain except the episode tied to it: the build-up, the longer one before burgher gains included, is that episode, and the sources show no wider proneness to unrest in the cities that later won seats.

B.23 The spike on independently sourced unrest

The changes in representation are coded mostly from the *Deutsches Städtebuch* (the institutional record of 219 of the 252 cities in the main regression cites it), and many unrest episodes cite the same volumes, so the coincidence of change and unrest could be one source dating both. Table 25 rebuilds the unrest outcome from the 160 of the 352 episode links whose episode cites none of the sources the city's institutional record was coded from, and feeds it to the event study of the main text, the de Chaisemartin–d'Haultfœuille (dCDH) estimate for the year of a city's first change, and the reverse event study. A second subset was to add the episodes that cite the institutional source but are dated within a year by a separate catalog row from another source; only 2 are, so it adds nothing to the first, and the record cannot show whether the *Städtebuch* dated a change apart from the revolt it describes. The reading fixed before the estimates was that the account of change following unrest stands if the spike for gains on corroborated episodes is at least the 0.87 points left by changes dated without conflict

Table 25: Unrest around changes in representation, by the source of the unrest record

Episodes	Links	Gains, 0 to 4	Guild gains, 0 to 4	dCDH, guild	dCDH, burgher	Change in onset year (multiple of base)
All	352	2.61*** (0.53)	2.90*** (0.73)	17.86*** (4.16)	6.46* (3.82)	24*** (3.5)
Not citing the institutional source	160	0.69** (0.30)	0.66 (0.43)	5.17** (2.19)	2.41 (2.15)	15*** (4.1)
Random drop, all episodes	160	[0.67, 1.78]	[0.49, 2.14]	–	–	–
Random drop, same region and half-century	160	[0.36, 1.41]	[0.30, 1.68]	–	–	–

Notes:
Each row rebuilds the unrest outcome from the episode links shown. Columns 3–4: event-study coefficients per gain, as in the main text; 5–6: de Chaisemartin–d'Haultfœuille (dCDH) estimates for the year of a city's first change; 7: change in representation in the year an episode begins, as a multiple of its base rate. Last rows: 95% range over 500 random drops of as many links. Standard errors in parentheses, Conley (3–4, 7) or by city (5–6); *** 0.01, ** 0.05, * 0.1.

vocabulary (Section B.22) and significant, and goes if it disappears. With so few corroborated episodes that test cannot be run, and the subset not citing the institutional source is reported instead.

On episodes not citing the institutional source, unrest runs 0.69 points above the baseline per gain in the five years from it (significant at the 5% level), 26% of the full figure, and 0.66 per guild gain (not significant at the 10% level). A city's first guild change goes with 5.2 points more unrest in its year (significant at the 5% level), about 18 times the base rate, and in the reverse event study a change in representation is about 15 times as likely as usual in the year such an episode begins, against 24 times for all episodes. The shrinkage is consistent with where and when the dropped episodes lie. Dropping 192 links at random from all episodes leaves a spike between 0.67 and 1.78 points in 95% of 500 draws, above the subset's; dropping them within the same regions and half-centuries leaves 0.36 to 1.41, a range that includes it. In 30% of the dropped links the subset leaves out every link of that region and half-century, so the matched draws vary only in the remaining strata.

B.24 Pre-existing institutions: further results

The main text's settled columns drop every city-year within five years of a change in guild or burgher representation. Table 26 shows that the window length does not matter: with two or ten years instead of five, guild seats stay small, negative, and insignificant within cities, burgher seats stay near zero, and wet-dry severity stays positive, significant at the 5% level in every window.

The exploratory interaction of bottom-decile years with guild seats the year before, on settled city-years, keeps its sign and size across the three windows. With a window of two years it is -0.49 points ($p = 0.018$, $N = 49{,}708$), with five -0.57 ($p = 0.008$, $N = 48{,}203$), and with ten -0.63 ($p = 0.001$, $N = 45{,}889$), or -92%, -107%, and -124% of the base rate, all with Conley standard errors. A linear model can put a thin cell beyond the base rate, and the Poisson companion of the five-year cell (Section B.38) is smaller, -66% ($p = 0.051$). The interaction was found rather than set in advance, so a stable estimate across windows does not lift it out of the exploratory tier.

The priming tests rest on the following counts. Of the 346 linked episodes, 77 coincide with a change in guild or burgher representation in every city they are linked to, between the year

Table 26: Settled city-years with windows of two, five, and ten years around changes in representation

	Any unrest × 100		
Window (years)	2	5	10
Model:	(1)	(2)	(3)
Variables			
scPDSI	0.0691**	0.0662**	0.0772**
	(0.0307)	(0.0315)	(0.0335)
Guild seats	-0.1439	-0.0937	-0.0139
	(0.1825)	(0.1952)	(0.1951)
Burgher seats	-0.0730	-0.0331	-0.0640
	(0.1583)	(0.1690)	(0.1776)
Market (0/1)	0.1678	0.1723	0.2150
	(0.1580)	(0.1609)	(0.1647)
Plague indicator	0.3601	0.2837	0.4157
	(0.4373)	(0.4511)	(0.4749)
Log population	-0.0083	-0.0281	-0.0455
	(0.1183)	(0.1213)	(0.1226)
City	Yes	Yes	Yes
Year	Yes	Yes	Yes
Fit statistics			
Observations	49,708	48,203	45,889

Conley standard errors (100 km, 50 years) in parentheses
*Signif. codes: ***: 0.01, **: 0.05, *: 0.1*

Main specification (representation the year before) on city-years at least the stated number of years from any change in guild or burgher representation; five years is the main text's choice. Outcome 100 if unrest, 0 otherwise. City and year fixed effects.

before they began and two years after they ended (80 in at least one of their cities). Of the 91 the catalog records as successful, 40 coincide with such a change in at least one of their cities; the rest won something other than representation, or at least something the institutional record does not show. The result is recorded for every linked episode. 218 episodes carry a political grievance, 60 of them alongside a change in representation in every linked city.

The demand-for-representation flag marks 138 episodes whose description mentions seats, access to or membership of the council, participation in government, a guild constitution, or council elections. It comes from a keyword search of the episode descriptions, so it can catch descriptions that mention representatives without any demand for seats. It differs from the catalog's representation grievance flag used in Section B.26, which is coded from each episode's stated grievances and marks unrest in 140 city-years. For the narrowest outcome, demands that did not win representation, the within-city estimate for guild seats is about 114% below the base rate with seats the year before, 69% with seats five years before, and 102% on five-year periods with seats at the start of the period; the first is significant at the 5% level, the last at the 10% level, and the five-year lag is not significant. A Poisson model gives about 73% fewer

such demands, significant at the 5% level, and it keeps only the 3,085 city-years of cities that ever made one. As the main text notes, a demand for seats is only possible where seats are lacking, so this outcome is biased toward a negative estimate.

Figure 4 reports the estimates behind the main text's figure on representation and unrest that did or did not change it, within cities and between them. Between cities, with region in place of city fixed effects and rye suitability as a control, none of the estimates is significant.

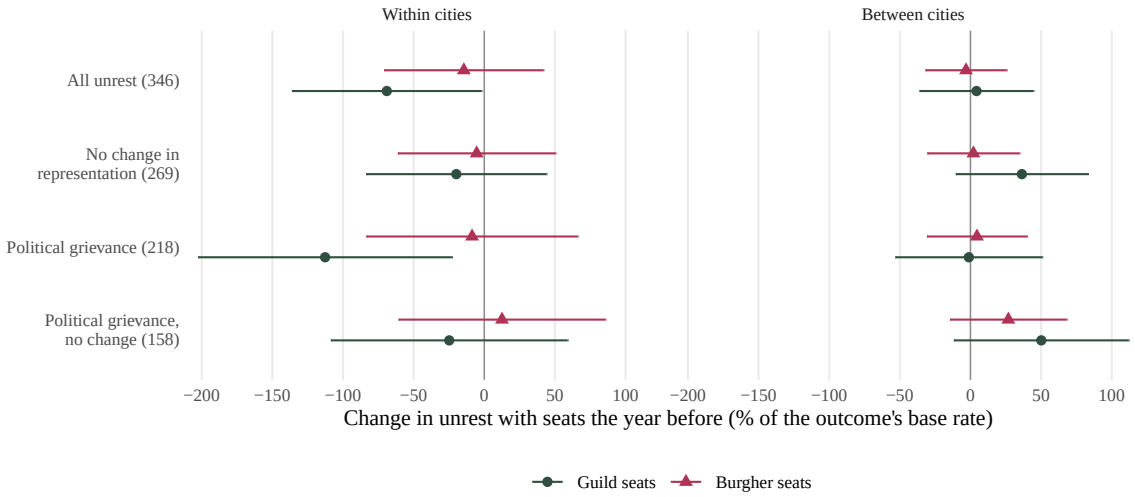


Figure 4: Representation the year before and unrest, within and between cities. Each point is the change in unrest with guild or burgher seats the year before, as a percent of the outcome's base rate; episodes in parentheses. Within cities: city and year fixed effects. Between cities: region and year fixed effects and rye suitability. Lines: 95% Conley confidence intervals.

B.25 Minimum detectable effects in the settled years

With city fixed effects, the settled-years columns of the main text compare a city's representation in spells away from any change with its representation in other such spells, a long-difference comparison that rests on the few cities whose settled spells differ in representation. Their institution coefficients are nulls, and the useful question is how large an association they could have detected. At 80% power in a two-sided test at the 5% level, the minimum detectable effect is 2.8 Conley standard errors. For guild seats it is 0.55 points, 104% of the base rate, and for burgher seats 0.47 points, 90% of the base rate. The 95% intervals run from -90% to +55% of the base rate for guild seats and from -69% to +57% for burgher seats. Between cities on settled years, where representation varies far more, the minimum detectable effects are 0.37 points, 70% of the base rate for guild seats and 0.25 points, 48% of the base rate for burgher seats. Within cities, then, the settled years can reliably detect only associations about as large

as the base rate itself; a lasting effect of standing representation half that size would usually go undetected.

B.26 Representation and unrest that is not about representation

A revolt for council seats can happen only where a group lacks them, so in regressions on all unrest, representation mixes how revolt-prone a city was with which cities were open to one kind of revolt. Table 27 splits the linked episodes by the catalog's representation grievance flag, which marks disputes over council seats, elections, eligibility for office, and guild constitutions. Unrest not about representation counts a city-year in which a linked episode without that flag begins, whatever its stated grievance or none (212 of the 352 unrest city-years). A stricter version keeps only such episodes that state another grievance, fiscal, autonomy, administration, economic, or religious (120). Unrest about representation is the mirror check, where eligibility should show (140); a city-year with both kinds of episode counts in both. Each outcome is regressed on guild and burgher seats the year before with the main controls, within cities and between them, and columns 5 and 6 also drop the city-years within two years of a recorded change in representation.

Within cities, guild seats the year before go with -1% of the base rate in unrest not about representation ($p = 0.982$) and burgher seats with -10% ($p = 0.766$); between cities the figures are +29% ($p = 0.260$) and +8% ($p = 0.669$). In the stricter outcome and in the years away from change, the smallest p-value of either seat term is 0.055. The within-city guild association sits in unrest about representation instead: -168% of its base rate ($p = 0.007$), and -32% between cities ($p = 0.329$). A city whose guilds already held seats had no seats left for them to fight over, and beyond such fights, cities with representation were at most modestly more prone to unrest, and only between cities.

B.27 Unrest by grievance in cities with and without seats

If lacking seats primed a city for unrest in general, cities without guild or burgher seats would see more unrest of every kind, not only revolts for seats. Table 28 makes each grievance of the linked episodes its own outcome on the settled city-years, none within five years of a recorded change in representation (48,203 city-years), and compares seats the year before within cities and between them, with the main controls. Within cities no seat term is significant at the 10% level for any grievance. Between cities 6 of the 14 terms are, all of them positive: cities with seats saw, if anything, more unrest over autonomy, administration, fiscal, and religious grievances. Within cities these comparisons have little power, since few cities' settled spells differ in representation (Section B.25), so their nulls rule out only large differences. Between cities, 6 significant terms are more than the 1.4 that chance alone would give at that level, and all point the same way, against more unrest where seats were lacking.

Table 27: Prior-year representation and unrest split by the representation grievance

Outcome	Not about rep.		Other grievance stated		Not about rep., no change		About rep.	
Comparison	Within	Between	Within	Between	Within	Between	Within	Between
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Variables								
Guild seats (t-1)	-0.003	0.114	0.057	0.155*	-0.060	0.147	-0.454***	-0.086
	(0.138)	(0.102)	(0.109)	(0.080)	(0.138)	(0.101)	(0.168)	(0.088)
Burgher seats (t-1)	-0.041	0.032	0.084	0.075	-0.109	-0.015	-0.055	-0.053
	(0.137)	(0.075)	(0.109)	(0.056)	(0.110)	(0.071)	(0.128)	(0.061)
Base rate	0.392	0.392	0.229	0.229	0.352	0.352	0.270	0.270
City	Yes		Yes		Yes		Yes	
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region		Yes		Yes		Yes		Yes
Fit statistics								
Observations	51,063	51,063	51,063	51,063	49,708	49,708	51,063	51,063

Conley standard errors (100 km, 50 years) in parentheses
*Signif. codes: ***: 0.01, **: 0.05, *: 0.1*

Annual panel; outcome 100 if a linked episode of the named kind begins in the city-year. About rep.: episodes with the representation grievance flag; the other outcomes use the remaining episodes, columns 3–4 only those stating another grievance. Columns 5–6 drop city-years within two years of a change in representation. Controls: scPDSI, market, log population, plague. Within: city and year fixed effects; between: region and year. Base rate in percent of city-years.

Table 28: Prior-year seats and unrest by grievance, settled city-years

Grievance	Unrest city-years	Base rate	Guild, within	Burgher, within	Guild, between	Burgher, between
Representation	85	0.176	-46%	+54%	+22%	+23%
Autonomy	57	0.118	-55%	-70%	+82%*	+45%
Administration	56	0.116	+69%	+22%	+100%*	+60%*
Fiscal	83	0.172	+52%	+2%	+95%**	+20%
Economic	21	0.044	+110%	+56%	+77%	+27%
Religious	12	0.025	+129%	-151%	+272%**	+159%**
None stated	70	0.145	-62%	-72%	-22%	-39%

Notes:
Annual panel, city-years with no change in representation within five years. Each row is its own outcome: 100 if a linked episode with that grievance begins in the city-year; an episode with two grievances counts in both. Cells: guild or burgher seats the year before, as a share of the outcome's base rate (in percent of city-years). Within: city and year fixed effects; between: region and year, plus rye suitability. Conley standard errors; *** 0.01, ** 0.05, * 0.1.

B.28 The annual panel at century resolution

Table 29 collapses the annual panel to 679 city-centuries in 252 cities and codes each century by guild and burgher seats at its start (1300, 1400, 1500), the convention of a century-mark panel. The outcome is 100 if any unrest is recorded in the city that century. Columns 2 and 4 drop the unrest that falls within 2 years of a recorded change in either kind of representation (60 of 352 unrest city-years). Within cities, the negative guild coefficient comes from those years; between cities, guild seats go with more unrest with or without them.

Table 29 codes each century at its start, and the choice matters. Table 30 repeats its within-city columns with each century coded instead at its midpoint (1350, 1450, 1510) or its end (1399,

Table 29: Representation and unrest at century resolution

	Any unrest × 100			
	All unrest	No change years	All unrest	No change years
Model:	(1)	(2)	(3)	(4)
Variables				
Guild seats at century start	-11.47**	-2.355	15.44***	15.96***
	(5.337)	(5.354)	(4.493)	(4.406)
Burgher seats at century start	-9.437*	-8.580*	0.8704	1.889
	(5.211)	(5.123)	(3.595)	(3.398)
City	Yes	Yes		
Century	Yes	Yes	Yes	Yes
Fit statistics				
Observations	664	664	679	679

Conley standard errors (100 km) in parentheses
*Signif. codes: ***: 0.01, **: 0.05, *: 0.1*

City-centuries, 1300–1520 (the last century covers 1500–1520). Representation is coded at each century's start and held for the century. Columns 1–2 compare centuries within cities, columns 3–4 cities within centuries. "No change years" drops unrest within two years of a recorded change in guild or burgher representation. Conley standard errors, 100 km.

1499, 1520). Coded at the start, the guild coefficient is -11.5 points (p = 0.032); coded at the midpoint the coefficient is -6.6 (p = 0.325), and coded at the end 0.2 (p = 0.982). For all unrest the coefficient is negative under start and midpoint coding and near zero under end coding, so the convention changes its size and significance. The start-of-century result is the one the change years account for: with them dropped it falls to -2.4 (p = 0.660), and at the midpoint it is -3.9 (p = 0.552). With end-of-century coding and the change years dropped, a negative association appears instead, -13.8 points (p = 0.046, significant at the 5% level), which the change years do not explain. The sign and significance of the within-city guild coefficient at century resolution therefore depend on which year codes the century. With start coding, a change made during a century, and the unrest around it, falls in a century coded at the level before the change; with end coding both fall in a century coded at the level after it.

B.29 Replicating Wahl's account of guild revolts

Wahl's dataset, compared with this one in the main text's Data section, also supports a directional test. Wahl (2015) argues that the late medieval guild revolts had structural rather than exogenous origins: centers of proto-industry saw more successful revolts and cities living mainly from trade fewer, with the agrarian crisis and the Black Death mattering only in a wider sample of smaller cities, while pre-existing burgher representation and elections, a city's autonomy from its lord, its urban hinterland, and guild participation in neighboring cities enter as controls and spillovers. Without his replication package I replicate the argument approximately, with one reversal of framing relative to the main text's regressions: here guild representation is the *outcome*. I use two outcomes, a gain of guild seats and a guild-led unrest

Table 30: Guild seats and unrest at century resolution, by the year that codes the century

	All unrest		No change years		
Century coded at	Guild seats	Base rate	Guild seats	Base rate	N
Start (1300, 1400, 1500)	-11.47** (5.34)	27.2	-2.36 (5.35)	24.2	664
Midpoint (1350, 1450, 1510)	-6.59 (6.69)	26.5	-3.92 (6.59)	23.6	705
End (1399, 1499, 1520)	0.18 (7.83)	25.6	-13.83** (6.91)	22.8	734

Notes:
Within-city columns of the century table, with guild and burgher seats taken from the stated year and held for the century; the burgher coefficient is not shown. Outcome 100 if any unrest is recorded in the city-century. No change years drops unrest within two years of a change in representation. City and century fixed effects; Conley standard errors (100 km) in parentheses; *** 0.01, ** 0.05, * 0.1.

episode, and ask whether pre-existing institutions predict them. The test covers his controls and his crisis variable rather than his headline, the proto-industry and trade contrast, which no linked source records. The spillover term counts neighboring cities within 100 km with a prior guild revolt, a proxy for his share of neighbors with guild participation within 150 km.

Table 31 reports the replication. In Wahl's specification, with city fixed effects, burgher representation in the same year goes with a gain of guild seats: such gains are rare (77 across 40,951 city-years), and each step up the burgher-representation scale adds about 3 times that base rate to the yearly chance (column 1). The association comes from the years of the gains themselves: 25 of the 77 gains came in the same year as a change in burgher representation, and gains placed at random in each city's own spell produce nothing like it (Section B.30). With burgher representation from the year before and region in place of city fixed effects, the association disappears, with or without those joint reforms (columns 2 and 3). With a later guild-led unrest episode as the outcome (156 across 52,540 city-years), burgher representation shows no significant association either (column 4). Wahl carries pre-existing burgher representation only as an unreported control, so this tests a mechanism he holds fixed rather than his headline. Wet-dry severity shows nothing in the transition equation and is significant and positive, meaning wetter, in the guild-led unrest equation, the opposite of Wahl's agricultural-crisis mechanism and the same reversed sign as the main text's climate tables. The autonomy proxy is null in both equations, plausibly because it is coarse. The spillover term is null for both outcomes, so his neighborhood mechanism does not reappear at this resolution.

B.30 Guild gains: hazard specification and placebo

Columns 2 and 3 of Table 31 re-fit the guild-gain equation as a hazard, with the previous year's burgher representation and region in place of city fixed effects. A complementary log-log hazard with region and quarter-century fixed effects gives the same answer: its burgher coefficient is -0.12 with a standard error of 0.17. As a placebo, each city that gains guild seats has its gain moved to a random year of its own at-risk spell and both specifications re-fit, 500 times.

Table 31: Wahl (2015) guild-revolt replication: institutions and climate as predictors, with a hazard specification

	Gain of guild seats (0/1)			Guild-led unrest (0/1)
	As Wahl	Lagged, region FE	No joint reforms	Guild-led unrest
Model:	(1)	(2)	(3)	(4)
Variables				
Burgher rep. (0–3)	0.0064***	-0.0002	0.0002	0.0010
	(0.0018)	(0.0003)	(0.0002)	(0.0009)
Autonomy proxy	0.0012	0.0007	0.0002	-1.74×10^{-5}
	(0.0022)	(0.0006)	(0.0005)	(0.0013)
scPDSI	1.48×10^{-5}	-2.67×10^{-5}	0.0001	0.0005**
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Log population	0.0001	0.0018***	0.0008***	-0.0006
	(0.0006)	(0.0004)	(0.0003)	(0.0009)
Spatial lag (100km)	0.0012	0.0016	0.0005	0.0016
	(0.0011)	(0.0012)	(0.0009)	(0.0012)
City	Yes			Yes
Year	Yes	Yes	Yes	Yes
Region		Yes	Yes	
Fit statistics				
Observations	40,951	40,291	40,159	52,540
R^2	0.02944	0.00955	0.00856	0.01572
Within R^2	0.00266	0.00113	0.00035	0.00025

Conley standard errors (100 km, 50 years) in parentheses
*Signif. codes: ***: 0.01, **: 0.05, *: 0.1*

Columns 1–3: a first gain of guild seats, after which the city leaves the risk set (77 gains). Column 1 follows Wahl (same-year burgher representation, city fixed effects); columns 2–3 lag it and use region fixed effects, column 3 without gains that coincide with a burgher change. Column 4: guild-led unrest (156 events). Autonomy proxy: 1 where the ruler holds no governing role. Conley standard errors.

Under Wahl's specification (column 1), no placebo coefficient reaches the actual one (0.0064, against placebo coefficients that average about zero with a standard deviation of 0.0005), so the column-1 association is not produced by burgher representation drifting upward within a spell. It comes from the gain years themselves: 25 of the 77 gains came in the same year as a change in burgher representation, in joint reforms that moved both scales at once. Under the lagged specification of column 2, the actual estimate sits well inside the placebo distribution: 83% of the placebo coefficients are at least as large.

B.31 The 0–3 representation scales

The main text enters guild and burgher representation as seat indicators, 1 where a scale reaches 2 or more. Table 32 repeats Table 5 of the main text with the two 0–3 scales, and Table 33 repeats its settled and between-city columns. The pattern is the same: the climate association is unchanged, guild representation goes with less unrest, as the change years would

Table 32: Climate and institutions with the 0–3 scales

	Any unrest × 100			
	Main		Interaction	
	scPDSI	Deciles	scPDSI	Deciles
Model:	(1)	(2)	(3)	(4)
Variables				
scPDSI	0.0750**		0.0524	
	(0.0341)		(0.0416)	
Bottom-decile scPDSI		-0.1551		0.0536
		(0.1603)		(0.2355)
Top-decile scPDSI		0.1514		0.3858*
		(0.1562)		(0.2233)
Guild rep.	-0.2025**	-0.2036**	-0.1923*	-0.2027*
	(0.1024)	(0.1024)	(0.1077)	(0.1054)
scPDSI × Guild rep.			0.0091	
			(0.0177)	
Bottom-decile scPDSI × Guild rep.				-0.1566
				(0.1134)
Top-decile scPDSI × Guild rep.				0.1921
				(0.1771)
Burgher rep.	0.0335	0.0319	0.0465	0.0705
	(0.1247)	(0.1249)	(0.1261)	(0.1302)
scPDSI × Burgher rep.			0.0136	
			(0.0195)	
Bottom-decile scPDSI × Burgher rep.				-0.0942
				(0.1542)
Top-decile scPDSI × Burgher rep.				-0.2768*
				(0.1467)
Market (0/1)	0.2548	0.2497	0.2547	0.2492
	(0.1769)	(0.1769)	(0.1768)	(0.1768)
Plague indicator	0.6838	0.6856	0.6862	0.6880
	(0.5185)	(0.5185)	(0.5185)	(0.5187)
Log population	0.0029	0.0012	0.0025	0.0021
	(0.1349)	(0.1350)	(0.1348)	(0.1350)
City	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	51,063	51,063	51,063	51,063
R^2	0.02133	0.02125	0.02134	0.02137

Conley standard errors (100 km, 50 years) in parentheses
*Signif. codes: ***: 0.01, **: 0.05, *: 0.1*

As the main text's institutions table, with guild and burgher representation on their 0–3 scales in place of the seat indicators. Representation is measured the year before; even-numbered columns use the bottom- and top-decile scPDSI indicators. Outcome 100 if unrest, 0 otherwise. City and year fixed effects.

produce, burgher representation shows no association of its own, and on settled years neither scale shows a significant association within cities. No climate interaction reaches the 5% level in either form, and burgher representation in top-decile years reaches the 10% level in both. Between cities the guild scale is positive and significant at the 5% level, and guild seats are positive and significant at the 10% level. The scales give smaller coefficients because each is per step on a four-level scale rather than for the whole step to seats.

Table 33: Settled city-years and comparisons between cities with the 0–3 scales

	Any unrest × 100	
	Within	Between
Model:	(1)	(2)
Variables		
scPDSI	0.0663**	0.0626**
	(0.0315)	(0.0295)
Guild rep.	-0.0250	0.1132**
	(0.0923)	(0.0542)
Burgher rep.	0.0166	0.0639
	(0.1171)	(0.0506)
Market (0/1)	0.1679	0.2221***
	(0.1619)	(0.0751)
Plague indicator	0.2816	0.4196
	(0.4510)	(0.4423)
Rye suitability		0.1004
		(0.2601)
Log population	-0.0262	0.3991***
	(0.1216)	(0.0634)
City	Yes	
Year	Yes	Yes
Region		Yes
Fit statistics		
Observations	48,203	48,203
R^2	0.02022	0.01222

Conley standard errors (100 km, 50 years) in parentheses
*Signif. codes: ***: 0.01, **: 0.05, *: 0.1*

As the main text's settled and between-city table, with guild and burgher representation on their 0–3 scales. Every column drops the city-years within five years of a change in guild or burgher representation. Outcome 100 if unrest, 0 otherwise.

B.32 Coups

A coup, a seizure of power, or an attempt at one, by a small group such as a faction or a few families, is the form of unrest closest to a change in who sits on the city council. Coups are one of the forms of unrest re-fitted separately in Section B.8, and I look at them more closely here because they are the form closest to a change in who sits on the council. The catalog classes takeovers by large groups, such as a guild or the commune, as rebellions, so coups are thin, 28 city-matched episodes. The look is exploratory, not set in advance, and carries the multiple-testing caveat of the form-specific results. Table 34 reports the results. Coups are about 124% more frequent where guilds held seats the year before, but not significantly, and on settled city-years the guild coefficient is small and insignificant. Wetter years go with more coups in both columns, at the 10% level, and markets show no association. The form-specific association with guild representation sits in rebellions instead.

Table 34: Coups as the outcome: all years and settled city-years

	Coup × 100	
	All years	Settled
Model:	(1)	(2)
Variables		
scPDSI	0.0145*	0.0142*
	(0.0082)	(0.0083)
Guild seats	0.0657	-0.0131
	(0.0558)	(0.0440)
Burgher seats	0.0636	0.1010
	(0.0650)	(0.0628)
Market (0/1)	0.0120	0.0133
	(0.0373)	(0.0168)
Plague indicator	-0.0398	0.0112
	(0.1354)	(0.1479)
Log population	0.0494*	0.0545**
	(0.0264)	(0.0277)
City	Yes	Yes
Year	Yes	Yes
Fit statistics		
Observations	51,063	48,203

Conley standard errors (100 km, 50 years) in parentheses
*Signif. codes: ***: 0.01, **: 0.05, *: 0.1*

Linear probability models, annual panel, 1300–1520; outcome 100 if a city-matched coup falls in the city-year, 0 otherwise. Representation is measured the year before. The settled column drops the city-years within five years of a change in guild or burgher representation. City and year fixed effects.

B.33 The market control

The city fixed effects absorb any market a city holds throughout the window, so the market coefficient rests on the cities that gain or lose one, and adding it could in principle shift the other estimates. Table 35 shows that it does not: without the control, wet-dry severity and guild and burgher representation keep essentially the same coefficients, for unrest and for coups alike.

Holding a market itself goes with more unrest, as the coordination account predicts. Within cities the association is not significant at the 10% level; between cities, on settled city-years, a market goes with about 46% more unrest than the base rate, significant at the 1% level. The between-city estimate carries the usual caveat: market towns were larger, better connected, and better documented, and region fixed effects and log population absorb only part of that, so it likely overstates the association. It also sits beside the finding of Wahl (2015) that cities living mainly from trade saw fewer successful guild revolts, though that concerns one kind of unrest and whether it succeeded, rather than how often any unrest was recorded.

Table 35: Main specification with and without the market control

	Any unrest × 100 Unrest		Coup × 100 Coup	
Model:	(1)	(2)	(3)	(4)
Variables				
scPDSI	0.0748**	0.0754**	0.0145*	0.0145*
	(0.0341)	(0.0341)	(0.0082)	(0.0082)
Guild seats	-0.4703**	-0.4567**	0.0651	0.0657
	(0.2266)	(0.2264)	(0.0562)	(0.0558)
Burgher seats	-0.0776	-0.0957	0.0644	0.0636
	(0.1887)	(0.1903)	(0.0652)	(0.0650)
Plague indicator	0.6858	0.6868	-0.0398	-0.0398
	(0.5185)	(0.5185)	(0.1354)	(0.1354)
Market (0/1)		0.2685		0.0120
		(0.1767)		(0.0373)
Log population	0.0045	-0.0022	0.0497*	0.0494*
	(0.1341)	(0.1343)	(0.0264)	(0.0264)
Market control	No	Yes	No	Yes
City	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	51,063	51,063	51,063	51,063

Conley standard errors (100 km, 50 years) in parentheses
*Signif. codes: ***: 0.01, **: 0.05, *: 0.1*

Annual panel, 1300–1520. Columns 1–2 repeat the main specification of the main-text institutions table without and with the market control, and columns 3–4 the coup specification; representation the year before. Outcomes 100 if the event occurs, 0 otherwise. Market is 1 where the city holds a recorded market right; its coefficient is identified only from cities that gain or lose one. City and year fixed effects.

B.34 By half-century

Guild constitutions spread unevenly over the two centuries: of the 86 cities that ever reach guild seats, 49 do so between 1300 and 1399. Table 36 re-fits the main specification within each half-century. Wet-dry severity is positive in all four, though significant only in 1350–1399, and there only at the 10% level. The guild coefficients follow the gains in representation: negative in the fourteenth century, when gains are most frequent, and close to zero after it. They carry the same change-year caveat as the main table and say nothing about representation a city already had.

Table 36: Main specification by half-century

	Any unrest × 100			
Period	1300–1349	1350–1399	1400–1449	1450–1499
Model:	(1)	(2)	(3)	(4)
Variables				
scPDSI	0.1011	0.1518*	0.0499	0.0209
	(0.0858)	(0.0898)	(0.0625)	(0.0648)
Guild seats	-2.3997**	-2.3192***	-0.1646	0.2831
	(1.1310)	(0.7821)	(1.1171)	(1.2526)
Burgher seats	-2.2315*	0.4524	1.5447	-0.2050
	(1.2279)	(1.1120)	(1.3259)	(0.7012)
Market (0/1)	-0.6858***	0.6017	-0.3279	-0.1191
	(0.2480)	(0.4902)	(0.5150)	(0.5261)
Plague indicator	2.0788	-0.8707	2.5828	0.4955
	(1.9878)	(0.8845)	(1.9179)	(0.7656)
City	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	10,089	11,373	12,024	12,288

Conley standard errors (100 km, 50 years) in parentheses
*Signif. codes: ***: 0.01, **: 0.05, *: 0.1*

Annual panel. Each column re-fits the main specification (representation the year before) within one half-century; 1500–1520 is left out. Outcome 100 if unrest, 0 otherwise. Log population drops out within a half-century because it changes only at the century snapshots. Market and plague controls, city and year fixed effects.

B.35 Does the guild-representation baseline assumption matter?

The institutional dataset leaves city-years before a city's first recorded change point missing rather than assuming a configuration held into an unknown past. Because most cities record guild representation absent at their first observed row (95.8%, 250 of 261 cities, consistent with guild seats typically being *granted* at a documented moment rather than present by default), a baseline-zero sensitivity variant sets pre-first-record guild representation to zero instead. This expands guild-representation coverage substantially (78.5% to 86.7% non-missing across the 66,521 city-years of the 1300–1520 skeleton, +5,472 city-years), but it barely enlarges the regression sample, because the regression also needs burgher representation and almost all of the newly recovered guild city-years lack it: the main specification grows from 51,063 to 51,311 city-years. Guild seats the year before are essentially unchanged under the alternative construction (-0.457, p = 0.044, against -0.463, p = 0.040, with Conley standard errors), and so are burgher seats and wet-dry severity. The Wigton-Jones qualitative benchmark, refit on five-year periods under the same baseline-zero assumption, shows the same pattern: no coefficient changes sign or crosses a significance threshold. Burgher representation shows the opposite pattern: 67.8% of cities (185 of 273) are already nonzero at their first entry, which suggests documentation lag, with the record beginning after a council was already in place,

rather than genuine absence, so no such variant is built for it.

B.36 Imperial status

This variant is fit on the five-year periods used in the robustness checks. Table 37 adds imperial status to the five-year specification, additively and interacted with drought. The city fixed effects absorb the level of imperial status, so its additive coefficient (-0.0297, p = 0.050) is identified only from the 18 cities whose status changes within the window; it is a descriptive association within those cities, not an estimate of the effect of status. The interaction with drought (+0.0057, p = 0.063) indicates a stronger wetter-years association in periods of imperial status, against a drought coefficient of +0.0010 (p = 0.680) when a city is not imperial. These specifications were not set in advance.

One coding rule can count a single reform twice. Election through compulsory guild-type electoral units covering the whole citizenry, such as the Cologne Gaffeln, scores as guild representation and also as election by the citizenry, so a reform that introduces it can raise both scales in the same year and enter the main text's event study as a guild gain and a burgher gain. Of the 54 changes that move both scales in the same year, 3 are coded from passages that describe such units: Aachen 1513, Cologne 1396, and Strasbourg 1482. Refitting the event study with the burgher half of those changes removed, so that the burgher level holds at its earlier value until the city's next burgher change, puts unrest in the five years from a gain at 2.61 points for gains in either institution (2.61 in the main text) and 0.82 for burgher gains (0.87), not significant at the 10% level. The rule touches too few changes to move the spike.

B.37 Five-year periods and a count model

The main text's annual linear models are re-fitted in Table 38 on five-year periods and as a Poisson model of the annual unrest count, each on all years and on settled years. The climate association keeps its sign under both changes, though on five-year periods it is no longer significant: a period one standard deviation wetter than usual goes with about 7% more unrest, and in the count model a year one standard deviation wetter with about 13% more, significant at the 5% level. On settled years guild and burgher seats show nothing in either model, and markets go with more unrest in every column.

B.38 Poisson companions for the thin cells

Several cells of the main-text tables imply changes larger than the outcome's base rate, which a linear probability model can produce in thin cells and a count model cannot. Table 39 fits fixed-effects Poisson models beside them, with the same controls, fixed effects and Conley standard errors, and reports each coefficient as a rate ratio minus one. In the main specification the two models agree in sign and significance: a unit of scPDSI goes with +11.1% more unrest

Table 37: Imperial status in the five-year specification

	Any unrest (0/1)	
	Imperial status	Drought x imperial
Model:	(1)	(2)
Variables		
scPDSI (higher = wetter)	0.0027	0.0010
	(0.0023)	(0.0023)
Imperial status (period share)	-0.0297*	-0.0242
	(0.0152)	(0.0155)
Market (share of period)	0.0164**	0.0163**
	(0.0075)	(0.0075)
Plague indicator	0.0041	0.0042
	(0.0104)	(0.0104)
Wet-dry severity × imperial status		0.0057*
		(0.0031)
Log population	0.0008	0.0009
	(0.0065)	(0.0064)
City	Yes	Yes
Period	Yes	Yes
Fit statistics		
Observations	10,340	10,340
R^2	0.08309	0.08350
Within R^2	0.00088	0.00133

Conley standard errors (100 km, 50 years) in parentheses
*Signif. codes: ***: 0.01, **: 0.05, *: 0.1*

Dependent variable: city-period unrest incidence, five-year periods, 1300–1520; 248 cities, N = 10,340. Imperial status: share of the period with free or imperial status; city fixed effects absorb its level, so it is identified from the 18 cities whose status changes. Guild and burgher representation (0–3) are included but not shown. City and period fixed effects.

in the Poisson model (p = 0.012) against +11.4% in the linear one, and guild seats the year before with -39% (p = 0.039). In the thin cells the Poisson estimates are smaller than the linear ones but keep their sign. In the decile interaction model with institutions the year before, guild seats in bottom-decile years go with -118% of the base rate in the linear model and -62% in the Poisson model (p = 0.063), and on settled years with -122% and -72% (p = 0.032); on settled years the bottom decile in cities where guilds held seats goes with -70% (p = 0.028). Protest in bottom-decile years falls by 83% (p = 0.007), and political-grievance unrest with guild seats the year before by 55% (p = 0.015). The Poisson model drops cities that never

Table 38: Robustness of the main-text regressions: five-year periods and a count model

	Any unrest × 100		Unrest count	
	5-yr	5-yr, settled	Poisson	Poisson, settled
Model:	(1)	(2)	(3)	(4)
	OLS	OLS	Poisson	Poisson
Variables				
scPDSI	0.2860	0.3044	0.1054**	0.1124**
	(0.2273)	(0.2190)	(0.0417)	(0.0466)
Guild seats	0.4880	-0.0127	-0.4969**	-0.1682
	(0.9854)	(0.9140)	(0.2409)	(0.2715)
Burgher seats	-0.2690	-0.7153	-0.1126	-0.0443
	(0.8444)	(0.8085)	(0.2578)	(0.3004)
Market (0/1)	1.5869**	1.0097	0.5657*	0.5758
	(0.7455)	(0.6486)	(0.3422)	(0.4432)
Plague indicator	0.2516	0.3659	0.3379	0.1515
	(1.0359)	(0.9371)	(0.2698)	(0.3444)
Log population	0.1133	-0.0286	-0.0027	-0.0589
	(0.6403)	(0.5803)	(0.1743)	(0.2011)
City	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	10,484	9,556	18,004	12,562

Conley standard errors (100 km, 50 years) in parentheses
*Signif. codes: ***: 0.01, **: 0.05, *: 0.1*

Columns 1–2: linear probability models on five-year periods, outcome 100 if any unrest in the period, 0 otherwise; guild and burgher seats are the share of the period in which they are held, and column 2 drops the period in which guild or burgher representation changes and the periods either side of it. Columns 3–4: Poisson pseudo-likelihood models of the annual unrest count, which drop cities that never record unrest (N = 18,004 in column 3); column 4 drops the city-years within five years of a change. City and year (or period) fixed effects.

record the outcome, so its samples are smaller.

B.39 Estimator form and fit

Two checks bear on the linear form, both on the five-year panel. A Firth bias-reduced logit keeps the sign of the climate association: a period one scPDSI unit wetter goes with about 9% higher odds of unrest with no fixed effects ($p = 0.012$) and about 11% with period fixed effects only ($p = 0.079$). These fits omit city fixed effects, since adding some 250 city dummies to a penalized fit would bring back the incidental-parameters problem the LPM avoids, so their estimand differs from the LPM's within-city one and they are not a like-for-like comparison. And 31.3% of the five-year LPM's fitted values (3,277 of 10,484) fall below zero, none above one (range -0.040 to 0.373), the usual cost of a linear form for a rare outcome.

Table 39: Linear and Poisson estimates for the cells beyond the base rate

		Poisson			
Cell	LPM	Rate ratio - 1	95% CI	p	N
scPDSI, per unit (main)	+11%	+11%	[+2%, +21%]	0.012	18,004
Guild seats, year before	-69%	-39%	[-62%, -2%]	0.039	18,004
Guild seats, year before, in bottom-decile years	-118%	-62%	[-86%, +5%]	0.063	18,004
Same, settled years	-122%	-72%	[-91%, -10%]	0.032	12,562
Bottom decile where guilds held seats, settled years	-112%	-70%	[-90%, -12%]	0.028	12,562
Protest, bottom decile	-122%	-83%	[-95%, -38%]	0.007	3,233
Political grievance, guild seats year before	-113%	-55%	[-76%, -15%]	0.015	11,668
Bottom decile x guild seats year before (interaction), settled years	-107%	-66%	[-88%, +1%]	0.051	12,562

Notes:
LPM: the linear coefficient as a share of its base rate. Rows on guild seats in bottom-decile years add the guild-seat term and its interaction with the bottom decile; the row on the bottom decile where guilds held seats adds the decile term and the interaction. The last row is the interaction term alone. Poisson: fixed-effects Poisson pseudo-likelihood with the controls and fixed effects of the linear model in that row, as a rate ratio minus one, with Conley 95% intervals and p-values. N is the Poisson sample, which drops cities that never record the outcome.

B.40 Outcome cuts for the five-year regression

Wet-dry severity is clearer in the severe and violent cuts than for all unrest. Restricting to episodes classified "major" severity (190 matched city-periods, against 343 for the all-severity outcome), it is +0.0028 ($p = 0.050$, $N = 10{,}484$). Under the current form coding the major tier and the Revolution/Rebellion group hold the same episodes, so the two cuts give identical estimates, and the coarser "violent" pooling of Revolution/Rebellion and Riot (212) gives +0.0025 ($p = 0.087$). These are not independent checks: they are one pattern seen through overlapping outcome definitions.

A further variation uses the standardized form field directly: re-estimating the five-year specification with the outcome restricted to one form at a time (coup, rebellion, riot, peaceful action, unspecified unrest) finds the wet-dry association only in rebellions, and there only at the 10% level (+0.0023, $p = 0.092$), and revolution in the strict sense has only one city-matched episode in the window and cannot be estimated. Table 10 reports the full set of form-specific re-estimates, for this specification and for each benchmark and replication specification in Sections 4 and 5 of the main text.

B.41 Which cities the two sources cover

Whether coverage varies with the covariates can be measured at the city level. Table 40 regresses two coverage measures for the backbone cities on log population at the 1400 snapshot (with an indicator for cities at the 500 floor), imperial status, and region fixed effects: whether any linked unrest episode begins in the city in 1300–1520, and whether the institutional source records any entry for it in the window, each beside its count. Of the 301 backbone cities, 299 enter the regressions, since 2 are alone in their region and drop out with its fixed effect; of these, 115 have linked unrest and 236 an institutional entry. Larger and imperial cities are

Table 40: Which backbone cities the unrest catalog and the institutional source cover

	Any unrest	Unrest episodes	Any inst. entry	Inst. entries	Any unrest	Any inst. entry
			All			Matched
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Log population, 1400	0.270***	1.235***	0.075***	0.517***	0.298***	0.078**
	(0.026)	(0.139)	(0.024)	(0.101)	(0.051)	(0.034)
At population floor, 1400	0.124**	0.896***	-0.528***	-0.517**	0.462**	-0.505***
	(0.053)	(0.196)	(0.078)	(0.200)	(0.190)	(0.121)
Imperial city	0.300***	1.214***	0.176***	0.468*	0.229**	0.185***
	(0.070)	(0.377)	(0.044)	(0.258)	(0.104)	(0.068)
Episcopal seat, 1400					0.182**	0.035
					(0.083)	(0.045)
Region	Yes	Yes	Yes	Yes	Yes	Yes
Fit statistics						
Observations	299	299	299	299	63	63
Dependent variable mean	0.38462	1.1605	0.78930	1.6957	0.65079	0.84127

Conley standard errors (100 km) in parentheses
*Signif. codes: ***: 0.01, **: 0.05, *: 0.1*

City-level linear models, one row per backbone city. Any unrest: a linked episode begins in the city in 1300–1520; unrest episodes: their number. Any inst. entry: an institutional change point dated in the window; inst. entries: their number. Columns 5–6 use the cities matched to the episcopal-seat data. Region fixed effects; cities without a region take that of their nearest neighbor.

better covered by both sources. A city one log point larger is 27 percentage points more likely to have linked unrest and 8 points more likely to have an institutional entry; an imperial city 30 and 18 points more likely. The imperial association with institutional coverage is partly mechanical, since imperial status is itself coded from the institutional source. An episcopal seat in 1400, from the bishop and archbishop indicators distributed with Wigton-Jones's replication files, can be matched for only 67 cities (20 seats), 63 of them in the regressions (19 seats); on that subsample it goes with 18 points more unrest coverage ($p = 0.035$) and 4 points more institutional coverage ($p = 0.430$). No variable for Hanse membership exists in the project data, and the decade of each *Städtebuch* volume is not recorded, so neither enters. Because the regressions in the main text use city fixed effects, these level differences in coverage are absorbed there; they matter for the between-city comparisons, which control for log population and region.

B.42 Catalog sources

Each catalog row records up to three sources, and an episode is assigned every source family its rows cite: the larger compilations each form their own family, beside the two *Städtebücher*, the *Historisches Lexikon der Schweiz*, Wikipedia, other literature, and other web pages. Of the 346 linked episodes, 63 cite Wikipedia and 2 rest on it alone. Table 42 re-fits the main specification

Table 41: Linked episode links by source family, half-century, and region

	Episode links by half-century							
Source family	1300-49	1350-99	1400-49	1450-99	1500-20	Total	Regions	Largest region
Deutsches Städtebuch	35	37	46	29	18	165	21	14%
Hergemöller	38	30	39	22	0	129	20	16%
Other literature	9	20	21	23	11	84	19	18%
Wikipedia	6	19	12	15	12	64	16	20%
HLB (Turnau)	20	23	9	7	3	62	3	63%
Kaser	0	0	17	16	26	59	17	17%
Ehbrecht	13	12	16	7	1	49	14	18%
Franz	0	0	4	8	34	46	13	37%
Blickle	3	5	6	8	12	34	10	44%
HLS	5	6	4	4	5	24	2	96%
Other web	6	5	2	4	5	22	12	23%
Lorenz (web)	0	0	4	2	10	16	5	75%
Österreichisches Städtebuch	0	0	1	1	1	3	1	100%
Cohn / HISCOD	0	0	0	1	1	2	2	50%

Notes:
Episode-city links of episodes linked to a study-population city, by every source family the episode's rows cite, so an episode citing several families counts once in each. Regions are the panel's regions, the German Städtebuch regions with Alsace and Switzerland together and a group of cities without institutional coverage; largest region gives the share of a family's links in its most frequent region.

leaving out one family at a time, for each family cited by at least ten linked episodes, in two ways: dropping the episodes that rest on that family alone, and dropping every episode that cites it. Dropping single-source episodes moves the scPDSI coefficient only between 0.056 and 0.077, against 0.075 in the full catalog. The stricter cut moves it more, from 0.033 (without every episode citing other literature, p = 0.262) to 0.083, and guild seats from -0.564 to -0.190, against -0.457. Adding region × half-century fixed effects, which absorb regional differences in coverage that change over time, leaves scPDSI at 0.070 (p = 0.047) and guild seats at -0.362 (p = 0.130).

Table 41 shows where and when each family's linked episodes fall. The leave-one-out results below should be read against it: a family concentrated in a few regions or half-centuries, such as the *Städtebuch* in the German regions, cannot be dropped without also changing the panel's regional and temporal composition.

B.43 The population floor

Buringh records towns of 1,000 or more, and a city absent from his 1300 or 1400 snapshot is given a population of 500 for that century, which puts a jump in log population inside the city at its first recorded snapshot. The floor applies to 7 cities and 1,100 city-years, 497 of them in the main estimation sample, which hold 1 unrest city-year. Dropping log population leaves the scPDSI coefficient at 0.0754 (0.0754 with it) and guild seats the year before at -0.4566

Table 42: The main specification leaving out one source family at a time

		scPDSI		Guild seats	
Source family left out	Linked episodes	Alone	Citing	Alone	Citing
None (full catalog)		0.075**		-0.457**	
Deutsches Städtebuch	162	0.069**	0.054*	-0.421*	-0.190
Hergemöller	128	0.070**	0.042	-0.369*	-0.312**
Other literature	84	0.072**	0.033	-0.443**	-0.354*
Wikipedia	63	0.075**	0.063**	-0.453**	-0.301
HLB (Turnau)	61	0.056*	0.053*	-0.418**	-0.273
Kaser	56	0.072**	0.070**	-0.457**	-0.564***
Ehbrecht	49	0.071**	0.065**	-0.447**	-0.309
Franz	43	0.071**	0.072**	-0.464**	-0.495**
Blickle	31	0.077**	0.083**	-0.462**	-0.463**
HLS	24	0.075**	0.080**	-0.474**	-0.479**
Other web	20	0.075**	0.069**	-0.457**	-0.347
Lorenz (web)	13	0.075**	0.081**	-0.460**	-0.463**

Notes:
Each row re-fits the main specification without the linked episodes that rest on the named source family alone (Alone) or without every episode that cites it (Citing), and reports the scPDSI and guild-seat coefficients in percentage points. Linked episodes: those citing the family. Families cited by at least ten linked episodes. City and year fixed effects; Conley standard errors; *** 0.01, ** 0.05, * 0.1.

(-0.4567); dropping the floor years instead gives 0.0762 and -0.4624. The results do not depend on the floor.

B.44 The study region

The study region is a rough approximation of the predominantly German-speaking part of the Empire within its c. 1378 boundary: a backbone city is in it if its coordinates fall inside both the Empire's boundary and the edge of the German-speaking area, each a map layer drawn for this study. The Empire's boundary is traced by hand from the map of Germany and northern Italy in 1378 in Putzger's historical school atlas (Putzger, 1900, plate 18). The German-speaking area joins Germany within its 1919–37 borders, without East Prussia, and the Alsace of the 1871–1918 imperial province (MPIDR [Max Planck Institute for Demographic Research] & CGG [Chair for Geodesy and Geoinformatics, University of Rostock], 2011), all of Switzerland (Bundesamt für Landestopografie swisstopo, n.d.), and Austria without Styria, Carinthia, and Burgenland (STATISTIK AUSTRIA, n.d.). These are modern and imperial-era borders, not a historical language line, so the approximation is rough at its edges. It takes in ten backbone cities in French- or Italian-speaking areas (Geneva, Lausanne, Vevey, Montreux, Neuchâtel, Le Locle, La Chaux-de-Fonds, Fribourg, Sion, and Lugano), and it leaves out German-speaking Styria, Carinthia, and South Tyrol and the German towns of Bohemia. Cities near the Empire's edge are decided by a city-level exception list rather than by the layer alone, and Table 43 gives

Table 43: The study region's exception list

City	Outside (km)	Decision	Reason
Rendsburg	0.3	Include	Holstein border town on the Eider; error at the boundary's edge
Wolin	0.1	Include	Town of the Duchy of Pomerania, an imperial fief; error in the boundary's coastline
Flensburg	50.0	Exclude	Duchy of Schleswig, north of the Eider: a Danish fief, outside the Empire 1300–1520
Schleswig	23.0	Exclude	Duchy of Schleswig
Husum	10.0	Exclude	Duchy of Schleswig
Wschowa	5.7	Exclude	Fraustadt: Duchy of Glogow until 1343, then Kingdom of Poland
Toenning	0.6	Exclude	Eiderstedt, north bank of the Eider, Duchy of Schleswig; fixed here rather than left to the edge

Notes:
Backbone cities whose place in the study region is set by hand. Outside: the distance by which the Empire's c. 1378 boundary layer places the city outside it. Included cities enter the study region although the layer places them just outside; excluded cities lie outside it already and are listed for transparency. Reasons are shortened from the exception list.

each exception, the distance by which the layer places it outside the boundary, the decision, and its reason. The five excluded towns, Flensburg, Schleswig, Husum, Toenning, and Wschowa, all lie outside the boundary already, so excluding them changes nothing; they are listed for transparency. The two included cities, Rendsburg and Wolin, lie within 0.3 km of its edge: Rendsburg was a Holstein town on the Eider and Wolin a town of the Duchy of Pomerania, an imperial fief, so the layer's edge rather than their status puts them outside.

Without the exception list the headline annual specification barely moves. Dropping the two included cities leaves 251 cities and 50,843 city-years in the estimation sample, against 252 and 51,063 in the study region. Wet-dry severity is then 0.0759 ($p = 0.027$) against 0.0754 ($p = 0.027$), guild seats -0.4572 ($p = 0.044$) against -0.4567 ($p = 0.044$), and burgher seats -0.0960 ($p = 0.614$) against -0.0957 ($p = 0.615$), all with Conley standard errors.

B.45 Sample flow

Table 44 reconciles the city and episode counts quoted in the paper. Of the 301 backbone cities, 277 have an institutional entry in or before the window and 265 a coded value inside it; 252 enter the main annual regression, which needs both representation scales coded in the same year and every covariate observed. Of the 490 study-region episodes, 346 link to a backbone city, through 352 episode-city links, and 338 unrest city-years fall in the main estimation sample.

Table 44: Sample flow: cities and unrest episodes

Step	N
Cities	
Backbone cities	301
Any institutional entry (in or before the window)	277
Any in-window coded value (after forward fill)	265
Guild and burgher scales both coded the year before	252
All main-specification variables observed in some year	252
Main annual regression sample	252
Unrest episodes	
Rows in the study region and window	719
Episodes in the study region and window	490
Episodes linked to a backbone city	346
Episode-city links	352
Unrest city-years (linked starts)	352
Unrest city-years in the main estimation sample	338

Notes:
Counts at each step of the sample construction, 1300–1520. Cities: the main regression needs both representation scales coded the year before and every covariate observed. Episodes: catalog rows in the study region, collapsed to episodes and linked to backbone cities; an episode recorded in several cities counts once per city, and the resulting city-years with a linked start are the outcome.

B.46 Agreement with Wahl's dataset

Raw agreement with Wahl's codes overstates how far the two datasets agree when one value dominates both codings, and Cohen's κ corrects for the agreement expected by chance from each coding's shares. For guild representation, the two codings agree on 81% of the 229 city-years both code, against 66% expected by chance, so $\kappa = 0.45$ (standard error 0.075). For burgher representation, Wahl codes 27% of city-years as represented and this dataset 31%, chance agreement is 59%, and $\kappa = 0.18$ (0.075); his coding counts 35 city-years as represented that mine does not, and mine 43 that his does not. On the conventional reading of κ, agreement is moderate for guild representation and slight for burgher representation. Low chance-corrected agreement on burgher representation means that the burgher measure is the noisier of the two, and associations that run through it are estimated less precisely than those through guild representation.

An annual dataset built from a record of changes and a century-snapshot panel can legitimately disagree about a given city-year, so the question is whether they disagree at random or at a threshold. Raw agreement is lower for burgher than for guild representation: the burgher-seats indicator matches Wahl's coded burgher representation in 66.4% of 232 pairs, and guild seats match his coded guild participation in 81.2% of 229. The datasets differ in source lineage, in granularity, and plausibly in where they draw the line on what counts as institutionalized

representation, so moderate agreement is informative rather than alarming; near-perfect agreement would have raised the opposite concern, that the new dataset merely re-derives the old one.

For burgher representation, unlike guild representation, the disagreement splits close to evenly, 43 against 35 of 78. The base rates say the same: on the compared city-years (229 for guild, 232 for burgher representation), Wahl codes guild participation present in 14.0% of city-years and this dataset in 28.4%, while for burgher representation the two sit close together, 27.2% against 30.6%. The two guild measures capture adjacent constructs: voting seats on the council against a formal guild constitution.

Carrying values forward also shapes the agreement. It is *higher* where a city-year's value is carried forward from before 1300: 100.0% for guild seats (82 pairs) and 89.4% for burgher representation (85), against 70.7% and 53.1% where a change is documented inside the window. Carried-forward city-years mostly belong to cities whose arrangements were settled well before 1300, which both datasets pick up the same way; the disagreement concentrates where the record documents new changes, which the two datasets register differently. The burgher-representation measure is therefore least certain around newly documented changes inside the window, which is also where the *Kommunalismus* prior draws its variation.

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