Late Medieval Unrest in the Holy Roman Empire

Climate, Institutions, and Documented Unrest

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Abstract

I link a geolocated, event-level catalog of popular unrest to an annual panel of city-council representation across 301 cities of the Holy Roman Empire, 1300–1520, dating unrest against institutional change and climate shocks. Guild representation's apparent associations with unrest come from the years in which representation changed, often in the course of unrest; the same panel, coded at each century's start, makes guild seats appear to lower unrest. Away from those years neither guild nor burgher representation shows a clear association with unrest. Contrary to the closest published study, documented unrest is, if anything, more frequent in wetter years. The divergence lies in the catalogs, which barely overlap; on that study's own data, neither the association of drought with the outbreak of conflict in the Empire nor its muting by elected councils is significant once standard errors are clustered by city and by year. Every association is descriptive.

Keywords: popular unrest, political representation, guilds, drought and conflict, Holy Roman Empire

JEL Codes: N43, N93, D74, Q54

1 Introduction

Uprisings recurred throughout late medieval society, yet why they broke out where and when they did is only partly understood. Popular unrest in the late medieval Holy Roman Empire is well studied at the level of individual cities and regions but rarely at the level of the Empire as a whole. Dozens of well-archived cities have detailed political histories (Bader & Dilcher, 1999; Isenmann, 2014).[1] Above the city, the record thins: pan-European conflict catalogs (Brecke, 1999; Chambru & Maneuvrier-Hervieu, 2024, p. 1088) cover the Empire only in passing. The institutional record has the same gap. Four datasets code institutions for cities in the Empire (Bosker et al., 2013; Bosshart & Dittmar, 2026; Schaff, 2024; Wahl, 2016, 2019), but the released ones date fourteenth- and fifteenth-century representation only by century, and none records the unrest that so often accompanied institutional change.[2] Debates over what drove unrest and how institutions shaped it therefore remain unresolved for want of evidence at the right resolution. This paper builds evidence at that resolution: two new datasets, one of unrest events and one of who governed, linked city by city and year by year, so that unrest can be dated against institutional change and against climate shocks.

The first dataset is a geolocated, event-level catalog of popular unrest from 1300 to 1520 CE, coded by location, timing, participants, apparent grievance, and outcome. Most analyses of medieval unrest rest on a few well-known episodes or treat revolt as a fixed city-level attribute over long periods.[3] Existing discussions of the circumstances of unrest focus on particular regions or settings: Kaser (1902) on urban social movements in northern Germany, Franz (1977) and Blickle (1979) on peasant resistance in central and southern Germany, and Hergemöller (2012) on inner-city unrest. The lack of a comprehensive analysis leaves basic questions open. Did revolts cluster in particular institutional environments? Were they more likely in drought or unusually wet years, or in years of plague? Did guild inclusion in city government precede unrest or follow it? The catalog, linked to the institutional dataset described next, is built to put these questions to the test.

The second dataset is an annual city-level record of political representation that tracks guild seats and broader burgher representation on city councils, built mainly from the regional volumes of the *Deutsches*

[1] Non-exhaustively: Aachen (Meuthen, 1963); Andernach (Tille, 1901); Augsburg (Adrian, 2017; Frensdorff, 1865, p. XXXIV; Rogge, 1996); Basel (Heusler, 1860, p. 194 ff.); Bern (de Capitani, 1982); Braunschweig (Dürre, 1861, p. 151 ff.; Garzmann, 1986; Rotz, 1973, p. 209); Bremen (Schwarzwälder, 1975); Cologne (Bader & Dilcher, 1999, p. 565; Herborn, 1977, 1980; Looz-Corswarem, 1980; Maschke, 1980a, p. 226; Militzer, 1980); Erfurt (Mägdefrau, 1976); Freiburg im Breisgau (Maurer, 1891); Görlitz (Jecht, 1908); Halberstadt (Militzer & Przybilla, 1980); Hamburg (Postel, 1980; Stoob, 1980); Konstanz (Bechtold, 1981; Beyerle, 1898); Lübeck (Bader & Dilcher, 1999, p. 567; Maschke, 1980a, pp. 182–183; Rotz, 1977; Wehrmann, 1879); Magdeburg (Uitz, 1976); Mainz (Hegel, 1882, p. 72 ff.; Fischer, 1958); Marburg (Verscharen, 1985); Osnabrück (Haase, 1952); Regensburg (Ritscher, 1974); Speyer (Maschke, 1980a, pp. 123–124; Voltmer, 1981); Strasbourg (Alioth, 1988; Schmoller, 1875); Ulm (Jäger, 1831; Specker, 1997); Wrocław (Goerlitz, 1962); Würzburg (Willoweit, 2003); Zürich (Bluntschli, 1838; Largiadèr, 1936).

[2] Bosshart & Dittmar (2026, p. 16), for instance, cover cities' disputes with their lords rather than unrest within them.

[3] Wahl (2015, pp. 15, 21), for instance, codes guild revolt as a single indicator per city spanning the fourteenth and fifteenth centuries, and keeps one observation per city: the period just before its revolt, or 1500 for cities that had none.

Städtebuch, a city-level encyclopedia of German urban history (Keyser, 1939, 1941, 1952, 1954, 1956, 1957, 1959, 1962, 1964; Keyser et al., 1995; Keyser et al., 2000; Keyser et al., 2003; Keyser & Stoob, 1971, 1974; Neitmann et al., 2022). Three existing datasets are coarser in time or coverage: one codes council and guild representation only by century (Wahl, 2016, 2019); another, local participative government by century and only for cities that reached 10,000 inhabitants (Bosker et al., 2013); a third, council elections for only 35 cities at 25-year intervals from 1500 onward (Schaff, 2024). A fourth dates councils, mayors, charters, and the rules for selecting councils by year for more than 2,000 cities from 1200 to 1700, but has not yet been released (Bosshart & Dittmar, 2026). No second coder has checked my dataset's levels of representation, so the results that rest on those levels rather than on the dates of change (comparisons across years in which representation did not change, the drought-by-guild pattern, and the council types used in the Wigton-Jones benchmark) are conditional on its coding (Section 3).

I link the two datasets to a common set of 301 cities, those with a known population above 1,000 by or before 1500 that lie roughly within the predominantly German-speaking part of the Empire (Bairoch et al., 1988; Buringh, 2021). I regress unrest on wet-dry severity, institutional representation, markets, population, and plague exposure on an annual panel of cities, and set the results against the closest published studies. I take climate first, because the institutional results reuse its measure and its benchmark.

The closest study on climate finds that severe drought raises the probability of conflict in European cities and, within the Empire, that the effect is muted where city councils are elected (Wigton-Jones, 2025). The drought pattern does not replicate on my narrower 1300–1520 CE panel, where drought never goes with more documented unrest at conventional significance, and the institutional one only in part: burgher representation mutes severe-drought periods in five-year bins but not in annual data (Section 5). In my own regressions wetter years go with more unrest, significant at the 5% level in the main specification but not after correcting for the paper's other headline tests (Section 4). I then rule out estimator choice, nonlinearity, and the casualty threshold as explanations for the reversal: I include peaceful episodes that fall outside the threshold employed by Wigton-Jones (2025), but the violent episodes both datasets would count show the same drought-year deficit. Geographic and temporal scope cannot be ruled out: on Wigton-Jones's own data, restricted to my cities and window, the drought coefficient for the outbreak of conflict is too imprecise to tell his effect from none. What divides the two results is the events each catalog records. They barely overlap, and on Wigton-Jones's own data the association of drought with the outbreak of conflict in the Empire depends on how the many simultaneous starts of a year are weighted, and neither it nor its muting by elected councils is significant once standard errors are clustered by city and by year; the muting disappears once one war, entered for every city on each warring side, is set aside (Section 4).

The paper's central question is how representation in city government relates to unrest. Most work on

institutions asks how they shape the activity of those operating within them; far less is known about what changes them (Callen et al., 2024). Representation and unrest are linked: representation can be the object of unrest, the concession it extracts (Acemoglu & Robinson, 2000; Acemoglu & Robinson, 2005; Aidt & Franck, 2015; Aidt & Leon, 2016), as it often was in late medieval cities (Bader & Dilcher, 1999; Dumolyn & Haemers, 2005; Rotz, 1985); the corporate bodies through which it runs, guilds above all, can be the vehicle that organizes violence (Blickle, 2000a; Lichbach, 1998; Planitz, 1965; Rotz, 1985); it can be a channel that absorbs grievances before they become revolts (Blickle, 2000b; Lantschner, 2015; Ogilvie, 2019; Wigton-Jones, 2025); and the groups that win representation can close access behind them, turning a dispute channel into a source of grievance (North et al., 2009; Ogilvie, 2019; Puga & Trefler, 2014, pp. 753, 784; Scribner, 1994; Stasavage, 2014, p. 340). These roles leave different marks in time: unrest bunched at the moment representation changes if it is the object, more unrest once seats are held if it is a vehicle, and less if it is a channel.

My results show that unrest clusters in the years in which guilds gained representation, which they often won in the course of unrest, and any association a panel regression returns between guild seats and unrest comes from those years. Away from them, representation shows no clear association with unrest, within cities or between them. What differs with it is how a city's crisis years went: where guilds already held seats, severe-drought years were quieter, the opposite of the persistence Wigton-Jones reports for guild cities, though this pattern was found in these data rather than specified in advance, rests on few events, and under randomization inference is significant on its t statistic but not on its coefficient. Burgher representation shows no association with unrest either, and its apparent association with later gains of guild seats disappears once it is measured the year before.

The timing result is the paper's main contribution, and it depends on the design. At the century resolution of the released datasets, a revolt and the seats it won fall in the same period, and a regression cannot tell an association between representation and unrest from the change itself. I show this by collapsing my panel to centuries, with representation coded at each century's start: within cities, guild seats then appear to lower unrest. The annual data show that this association comes mostly from the unrest in years of change. The second contribution is the Wigton-Jones benchmark, which traces the divergence to what each conflict catalog records. The third is the two datasets themselves, the infrastructure for both: they carry detail these tests use only in part, including who led each episode, what it was about, and how it ended, and what could be tested next.

Two caveats condition the patterns in the paper. Larger, imperial, and episcopal cities are better covered by the sources: a city twice as large is about 19 percentage points more likely to have linked unrest and 5 points more likely to have an institutional entry, and an imperial city about 30 and 18 points more likely

(online appendix). City fixed effects absorb the part of that tendency that does not change over time, and region × half-century fixed effects the part that shifts with a region's coverage; the main climate association holds under both (online appendix). Neither removes changes in coverage particular to a city. The same sources also often record both an episode and the change in representation it won, so I check the change-year spike on changes dated from passages that describe no conflict and on episodes that do not cite the city's institutional source.

The paper proceeds as follows. Section 2 describes how late medieval city councils were composed and contested, and what the main accounts of revolt predict for them. Section 3 builds the two datasets and the city backbone that links them. Section 4 relates unrest to climate shocks and sets the result against Wigton-Jones's drought finding; Section 5 dates unrest against changes in representation, tests whether representation muted drought as Wigton-Jones (2025) reports, and sets the result against the account of guild revolts in Wahl (2015). Section 6 discusses what the timing result and the benchmark imply for the climate-conflict and institutions-conflict literatures, and what the datasets could test next.

2 Unrest and Institutional Change

Late medieval popular unrest in the Holy Roman Empire was distributed unevenly in ways that resist simple explanation. In the catalog, a year of severe drought or unusual wetness sees unrest in at most a few of the many cities it touches, and nearly all of their equally affected neighbors stay quiet. Accounts of revolt usually begin with how hardship becomes a complaint (grievance) and then ask how a complaint becomes a revolt (collective action). In the cities this paper studies, the council constitution entered that sequence three times. It was the object of unrest, the arrangement much urban unrest set out to change. Its corporate bodies, the guilds above all, were a vehicle that made collective action possible. And where the community had a recognized voice, the constitution was a channel that could absorb a grievance before it became a revolt.

Cities in the Empire generally governed themselves through councils (*Rat*). The council was the organ of a legally recognized community (*commune*, *Gemeinde*) whose partial autonomy, with officeholders the burghers had some part in appointing, set the self-governing European city apart (Weber, 1978, p. 1226). How officeholders were chosen and which corporate bodies held authority (*Stadtregiment*) differed from city to city and changed over time (Isenmann, 2014, pp. 243–244). Most cities were subject to a territorial lord or a bishop, and the free and imperial cities to the emperor directly. The council governed beside the lord rather than in his place, and in territorial towns held only the authority he delegated (Isenmann, 2014, pp. 327–328).

The political arrangement inherited from the thirteenth century was primarily patrician oligarchy. Councillors came from a circle of long-distance merchants, knightly families (*ministeriales*), or wealthy landowners (Maschke, 1980b, p. 171; Planitz, 1965, pp. 261–263). In most cities craftsmen rarely sat on the council, and from the end of the thirteenth century the sitting council commonly filled its own vacancies by co-optation (Bader & Dilcher, 1999, p. 549; Isenmann, 2014, p. 355). Over the fourteenth and fifteenth centuries, many cities renegotiated that arrangement. Craft guilds (*Zünfte*, *Gilden*) won seats on the council, often as the settlement of an open conflict and in some cities under a full guild constitution (*Zunftverfassung*); in several, the old families later took the seats back (Bader & Dilcher, 1999, pp. 511–512; Isenmann, 2014, p. 234; Maschke, 1980b, pp. 170–171). Cities also varied in how far the wider body of resident burghers held any voice, from complete exclusion, through a supervisory (*Bürgerausschuss*) or minority position in council, to a citizen-chosen majority (Isenmann, 2014, p. 373), which is the range the dataset's 0–3 burgher-representation scale records (Section 3).

These councils governed through the long agrarian depression (*Agrarkrise*) of the fourteenth and fifteenth centuries. From the late fourteenth century grain prices fell while craft wages rose, squeezing farm incomes so that peasants left for the towns (Abel, 2006, pp. 52–53, 91; Campbell, 2016, p. 357; Slicher van Bath, 1963, p. 138). By the end of the Middle Ages about a fourth of settlements in Germany had been abandoned (Abel, 2006, p. 81; Slicher van Bath, 1963, p. 163). In the countryside, the peasant revolts of the period followed the slump (Slicher van Bath, 1963, p. 189), and German historiography reads them as a struggle between lords and peasants over the increasingly scarce resources (Blickle, 1979, pp. 231–232; Blickle, 2012, p. 62). In the towns, the same price movements favored wage earners and craftsmen (Abel, 2006, pp. 53, 59), and the guilds that pressed for council seats were associations that had grown economically stronger as the cities flourished (Maschke, 1980b, p. 171).

Bad harvests recurred: wet summers damaged those of 1401 and 1402, crops failed again in 1408, 1416, and 1426, and in 1432–1438, the fifteenth century's coldest decade, they failed year after year until famine struck regions across Central Europe (Abel, 2006, p. 59; Camenisch et al., 2016, pp. 2108, 2116). Moisture shocks of this kind, drought and wet years, are what this paper measures, and accounts of how such a shock becomes a grievance differ in where they place the trigger. Relative-deprivation theory holds that revolt follows the gap between what people have come to expect and what they receive, so that sustained improvement abruptly reversed is more explosive than persistent misery (Davies, 1962; Gurr, 2010).

Rebellion also competes with production for a household's time and resources, so a shock that lowers income lowers what revolt costs, and a poorer government is weaker at suppressing it (Fearon & Laitin, 2003, pp. 75–76; Grossman, 1991). A bad harvest did not lower every income, however: because food demand is inelastic, a shortfall raised grain prices more than in proportion (Slicher van Bath, 1963, pp. 118–119), so

a bad year cost households that bought grain, while farmers with grain to sell could do better than in an ordinary year.

The moral-economy account places the trigger in the response to the shock. On this account, unrest follows the violation of a normative framework that defines what authorities may legitimately demand (J. C. Scott, 1976; Thompson, 1971), and food riots are better explained by changing grain policy than by a simple sequence of shortage, hunger, and riot (L. A. Tilly, 1971). In German cities, the provisioning duty that the moral economy presumes fell on the council, whose grain policy (including price controls and export bans) was explicitly meant to forestall rioting (Isenmann, 2014, pp. 978–980). In Hamburg's grain crisis of 1483, merchants shipping grain to Iceland while prices rose incensed residents and set off the unrest (Stoob, 1980, p. 365). The comparative record of urban unrest, however, points to politics more than to subsistence: of 1,112 revolts and social movements in Italy, France, and Flanders up to 1425, scarcity sparked about 2 percent (Cohn, 2006, p. 74). The catalog shows the same: disputes over representation and taxes dominate, and economic grievance, which includes every named natural shock, stays marginal in every period (Table 1).

Unrest in the Empire more often set out to change the council itself. Its most general trigger was the patrician councils' refusal to account for the state of the city's finances (Maschke, 1980b, pp. 171–172). The rebels were rarely the poor. They came from the "financially responsible citizenry", whose standing complaints were that taxes were too high, that the council had mismanaged the town's administration, and that it had bungled the town's foreign policy (Rotz, 1985, pp. 68–69, 71). The movements sought a share of the council and were rarely revolutionary in aim. Those that were not put down generally ended in a compromise constitution (Bader & Dilcher, 1999, pp. 556, 562–564). The settlement was narrower than its form suggests. The guild councillors these constitutions installed were usually chosen by the council itself through co-optation, were nearly as wealthy and well connected as the sitting councillors, and remained a minority on most councils (Rotz, 1985, p. 69). For this reason the dataset scores craft seats on a co-opted council as no guild representation (Section 3). Even where guilds chose their own councillors, the men they sent were wealthy masters, so a recorded transition may widen the ruling circle among the propertied rather than open it to ordinary craftsmen. Even a decisive share of the council could leave its policy unchanged: in Augsburg, once the craftsmen entered the council, they continued the armaments policy of the patrician families they had displaced (Maschke, 1980b, p. 173).

The economic theory of democratization gives a reason a council would concede seats at all. Extending political rights commits an elite to future redistribution in a way that a promise made under the old institutions cannot, since the promise lapses once the threat has passed. A temporary threat calls forth institutional change, and the threat is most credible when the opportunity cost of revolt is low, as in recessions (Acemoglu & Robinson, 2001; Acemoglu & Robinson, 2000). Empirical work finds the same link from unrest

to institutional change. The Swing riots moved English voters toward pro-reform candidates and helped give the reformers the Commons majority they needed for the Reform Act of 1832 (Aidt & Franck, 2015), and in sub-Saharan Africa drought-induced riots raised the probability of democratic change, often through concessions by incumbents (Aidt & Leon, 2016). In medieval Flanders, revolt recurred often enough to form a "revolutionary tradition" in collective consciousness (Dumolyn & Haemers, 2005, p. 371). Its aim was access to the polity and control of the city's finances, and craft guilds won permanent representation after 1302 and had it confirmed in 1360 (Dumolyn & Haemers, 2005, pp. 375–376, 391). Because representation was so often the outcome of unrest, it cannot be read only as a background condition that unrest occurred within. A quiet stretch in the unrest record shows only that order held, not that the arrangements holding it were accepted without.

Whatever the grievance, it became unrest only if someone bore the cost of organizing it. The gains from a successful revolt go to participants and bystanders alike, so no one has a reason to take the risk of acting first (Lichbach, 1998; Olson, 2003; C. Tilly, 1978, pp. 62–63). Groups overcome this through existing communities and hierarchies, and through selective incentives that only organizations can supply (Lichbach, 1998, pp. 15, 20; Olson, 2003, p. 51).[4] A shock that absorbs households' time and resources can make that organizing harder even as it lowers the cost of unrest. Late medieval cities had such organizations ready to hand. Guilds were organized militarily as well as economically, which helps explain their fourteenth-century gains (Planitz, 1965, pp. 325–326). Successful movements institutionalized the new balance of power through existing associations built for other purposes (Bader & Dilcher, 1999, p. 563). Organized bodies also shaped the form unrest took: where people acted through recognized corporations such as guilds, contention tended to be local and direct (C. Tilly, 2005, pp. 45, 142), though late medieval revolts were often citywide and, north of the Alps, allied with peasants and other towns (Cohn, 2019, pp. 389, 394–395, 406). Rotz (1985, pp. 72, 84–85) reads the German uprisings as citizen struggles rather than guild struggles: the guilds were an available institution, which the movements used and then brought into government. A shock's effect on unrest may therefore show most clearly among actors already organized to act, which Section 4 examines by separating unrest by who carried it out.

Where a community had a recognized voice, the same organizations could press a grievance without revolt. *Kommunalismus*, Blickle's (2000b, pp. 148, 177) term for a social order built on the self-governing town and village community, supplied the grounds for doing so: a levy or tax could be contested because it endangered household subsistence (*Hausnotdurft*) or did not serve the common benefit (*Gemeiner Nutzen*). The strong,

[4] In the Low Countries, artisans organized in a *takehan*, an alliance with a common fund that financed strikes (Dumolyn & Haemers, 2017, pp. 47–48). In Mamluk Egypt and Syria, where many notables collaborated with the regime, such associations could not form, and resistance depended on the religious scholars (Lapidus, 1984, pp. 190–191); a deviant Nile flood strengthened the bargaining position of the chief judge, who could coordinate a revolt (Chaney, 2013).

corporately constituted communities that carried these norms also supplied the organizational base for unrest, which was largely absent where they had not formed (Blickle, 2000a, pp. 261–262). Organized communities cut both ways, but representation changes the form a grievance takes. Guilds voiced their members' discontent in councils where they were represented, and otherwise through lobbying, petitioning, and litigation, with violence an occasional resort (Dumolyn & Haemers, 2005, p. 379; Ogilvie, 2019, p. 38). Guild or burgher representation can thus substitute for unrest, absorbing a drought as misfortune within a legitimate order and turning a lord's new levy into a petition or a suit; that substitution is the prior Section 5 tests. In a Holy Roman Empire subsample, Wigton-Jones (2025) finds drought-induced conflict muted where city councils were elected but not where they rested on burgher or guild representation. The closest analog to his elected councils in my data is broader burgher representation, and his burgher-or-guild result runs against the guild half of the prior (Section 5).

Kommunalismus has clear limits. Blickle (2000b) first framed the model for Upper Germany alone and stresses the regional limit. Scribner (1994, pp. 200, 202–204) observes that the thesis rests chiefly on south-west German and Swiss evidence, risks a reading in which medieval communes anticipate democracy, and is undercut by case studies in which communes were readily absorbed by hierarchy and oligarchy. Panel evidence on urban construction and political autonomy reaches a similar verdict: across German-speaking Europe, the core institutional gains cities won after the Black Death were non-democratic and served urban oligarchies (Bosshart & Dittmar, 2026, p. 32).

The guilds in particular often narrowed the channel they had opened. Whether guilds solved market failures in quality, training, and innovation or were distributional coalitions serving their members at others' expense is an old debate (Ogilvie, 2019), and the council seats this paper tracks are its political side. In German cities the guild struggles were directed not only against patrician government but also against merchants, Jews, poorer craftsmen, journeymen, and wage-workers, and the guilds used the representation they won to keep such groups out (Ogilvie, 2019, pp. 39–40). In Strasbourg, whose council had seated twenty-five guild representatives beside fourteen burghers and eight nobles since 1333 (Naegle & Solórzano Telechea, 2014), craftsmen assembled in arms before the cathedral in February 1349 after the presiding magistrate refused them a share of the money the Jewish community had paid; they forced him and two other councillors to resign and installed a new council, which four days later resolved to burn the city's Jews (Winkler, 2017). The logic is that of a limited-access order, in which a dominant coalition secures peace by sharing the rents that limited entry creates and incorporates newly powerful groups when the balance of violence shifts (North et al., 2009). Guilds strong enough to threaten the patriciate were incorporated and then defended their new rents by restricting entry, so the organization that once channeled grievances could become a source of them. Whether guild seats dampened unrest or fed it cannot be read from a coding that

places a revolt and the seats it won in the same period; separating the years seats changed from the years they were held, which the annual dataset allows, is the test of Section 5.

3 Data

The paper rests on two new datasets for the Holy Roman Empire, 1300–1520 CE: a catalog of popular unrest and an annual dataset of city-council representation, linked through a common backbone of 301 cities. This section describes each dataset's sources, construction, size, and coverage, and then the backbone that links them (Section 3.3). The climate, population, and plague covariates, matched to the same backbone, are introduced in Section 4, where they are used.

3.1 Unrest

I have compiled a catalog of 490 unrest episodes in the predominantly German-speaking part of the Empire, 1300–1520, built for quantitative analysis of where and when unrest occurred rather than as a narrative history of each episode. To my knowledge it is the most comprehensive compilation for this region and period. The closest existing catalogs cover the region only thinly. Brecke (1999) records mainly large-scale violence such as war,[5] and about 92 percent of the entries in the Historical Social Conflict Database (HiSCoD) come from France and England, whose existing datasets it was built on; its authors note that no comparable survey exists for pre-industrial Germany (Chambru & Maneuvrier-Hervieu, 2024, p. 1088).

The catalog is compiled primarily from secondary regional and urban histories and from encyclopedia descriptions of late medieval German unrest. While I draw on a wide array of sources (online appendix), the most substantive are:

- The regional volumes of the *Deutsches Städtebuch* encyclopedia;
- Turnau (2010), *Städtische Unruhen (Spätmittelalter)*, a *Historisches Lexikon Bayerns* reference entry cataloguing episodes of disorder in late medieval Bavaria;
- The *Historisches Lexikon der Schweiz*, an online encyclopedia which supplies episodes for Swiss cities and rural districts;
- Wikipedia articles in several languages, cited for individual episodes;
- Hergemöller (2012), *Uplop – Seditio: Innerstädtische Unruhen des 14. und 15. Jahrhunderts im engeren Reichsgebiet*, an extensive description of urban movements and riots;
- Franz (1977), *Der deutsche Bauernkrieg*, a systematic survey of rural and some urban unrest in southern

[5] Wigton-Jones's conflict measure, which is built from Brecke (1999), is compared with this catalog in Section 4.

Germany and Switzerland, particularly around the lead-up to the Peasants' War of 1525, and expanded on in Blickle (1979);

- Kaser (1902), *Zur politischen und sozialen Bewegung im deutschen Bürgertum des XV. und XVI. Jahrhunderts*, an extensive enumeration of urban revolts in the fifteenth and early sixteenth centuries; and
- Blickle (2012), *Unruhen in der ständischen Gesellschaft 1300–1800*, a general history of unrest in the period.[6]

The German historiography names these events with a wide vocabulary, from riot (*Aufruhr*) and uprising (*Aufstand*) to civic and guild struggle (*Bürgerkämpfe*, *Zunftkämpfe*), each term implying a degree of intensity or a teleology (Blickle, 2012, p. 5; Isenmann, 2014, p. 254). I follow Blickle (2012) and use the more inclusive "unrest" (*Unruhen*), which does not prejudge what an episode was or who took part in it.

The inclusion criterion is accordingly broad: any collective action, actual or attempted, that asserted grievances or attempted to disrupt existing authority structures. Episodes suppressed before open confrontation are included where there is evidence of coordinated planning or a repressive response. Elite revolts against liege lords or the emperor are excluded. Chronicles do not always make clear whether unrest, rather than an ordinary dispute or settlement, has occurred; where the threshold is ambiguous I follow the interpretation of the source authors rather than impose a criterion of my own. Most episodes were local. The catalog records burghers against a city council (Speyer 1349), peasants against an appointed bailiff (Weingarten 1432), and townspeople against their prince-abbot (Kempten 1363), to name a few, each contesting a specific grievance with the immediate authority rather than the wider order. Not all unrest was violent or popular, and the participants rarely sought to overturn the feudal order, aiming instead at negotiated political or economic concessions. The exceptions are notable for how late in the era they emerge: the Niklashausen episode of 1476, the Bundschuh conspiracies of the upper Rhine (1493–1517), and the Poor Konrad revolt of 1514, all of them eclipsed by the Peasants' War of 1524–25, which falls just after the study period. I end the window in 1520 because the central puzzle of that war, the formation of supraregional, cross-class alliances that turned rebellion into war, is a different question from what explains the scattered local uprisings of the preceding century (T. Scott, 1979, p. 953).

The catalog's sources record 719 rows in the study region and window, one for each place a revolt is recorded. Because a single revolt can unfold in many places, I treat the *episode*, not the raw row, as the primary unit of analysis: rows of the same revolt collapse into one episode, dated by its earliest reported

[6] A further large source is Peter Bierbrauer's research report on peasant revolts in the Empire, in Blickle (1980), which Bosshart & Dittmar (2026, pp. 77–78) describe as the most comprehensive body of evidence on peasant revolts before 1500. I could not consult it, so episodes known only from it are not in the catalog.

year. This yields 490 episodes. Only 5.1% of them span more than one place, but those account for 35.3% of the rows, so counting rows would let a handful of large revolts dominate the record. 31.8% of episodes are recorded as lasting more than one year (where no source reports an end year, it is set equal to the onset year).

Each episode is coded for one or more grievance types, where the source description names the grievance as a cause, demand, or trigger:

- Representation, or who sits in government—council seats, elections, eligibility for office, oversight committees
- Autonomy, or who rules the place and with what rights—jurisdiction, liberties against a lord, foreign rule, serfdom and tenure
- Administration, or how the authority governs—regulation of trade and crafts, the conduct of officials, failure to protect
- Fiscal, or the extraction burden imposed by the opposed authority—taxes, levies, dues, tithes, public debt
- Economic, or hardship from conditions beyond that authority's control—prices, wages, trade, and natural shocks such as drought, flood, harvest failure, epidemic, or fire
- Religious, or belief, worship, or church authority

The online appendix sets out the full definitions and the rules for boundary cases. Table 1 gives the resulting grievance mix by period, for the study region and the city-matched subsample. Disputes over who sat in government are the most common grievance early in the window, and fiscal grievance grows from 19.8% of episodes in the first half-century to 40.0% in the last, alongside a rising rural share. Economic grievance, which includes every named natural shock, by contrast stays marginal in every period (6.1% of episodes overall). Few revolts name a climate shock or hunger as a cause. Subsistence stress either acted through the political and fiscal complaints the sources record, went unrecorded, or mattered little; Section 4 takes up how far this bears on the climate results.

Each episode also carries a type, coded from the source text by what the participants attempted and how: revolution, rebellion, coup, riot, protest, or unclear where the sources do not say (definitions in the online appendix). Pogroms with no other aims are catalogued separately and do not count as unrest here (an episode that also turned on Jewish residents keeps its type and records them as a target), and conflicts between authorities, such as feuds and wars, are excluded. An episode takes every type its source rows record. Table 1 reports the types by period. Rebellion is the most common, at 42.0% of episodes, and revolution in the strict sense appears only in the last two decades of the window. The type field is empty for 6.4%

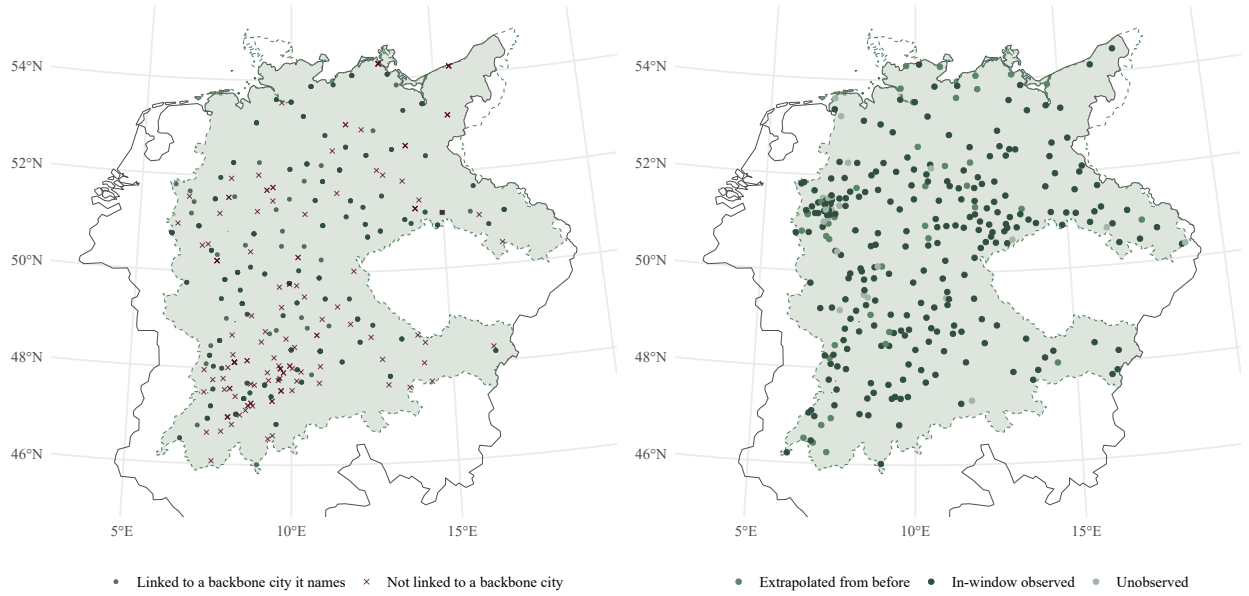
Table 1: Unrest episodes by period: grievance mix, setting, type, and violence, 1300–1520

	1300–49		1350–99		1400–49		1450–99		1500–20		All	
	SR	CM	SR	CM	SR	CM	SR	CM	SR	CM	SR	CM
Episodes												
Episodes (N)	96	79	105	76	115	84	99	60	75	47	490	346
Grievance type (percent of episodes)												
Representation	42.7	46.8	33.3	38.2	36.5	50.0	24.2	30.0	25.3	27.7	32.9	40.2
Autonomy	20.8	17.7	20.0	18.4	24.3	15.5	28.3	25.0	20.0	17.0	22.9	18.5
Administration	9.4	10.1	20.0	22.4	19.1	20.2	24.2	30.0	24.0	21.3	19.2	20.2
Fiscal	19.8	20.3	28.6	28.9	32.2	36.9	37.4	45.0	40.0	42.6	31.2	33.5
Economic	7.3	8.9	5.7	6.6	1.7	1.2	8.1	10.0	9.3	14.9	6.1	7.5
Religious	4.2	5.1	—	—	3.5	3.6	8.1	11.7	2.7	4.3	3.7	4.6
None	26.0	24.1	33.3	30.3	25.2	22.6	25.3	20.0	36.0	40.4	28.8	26.6
Setting (percent of episodes)												
Urban	89.6	96.2	91.4	98.7	83.5	95.2	68.7	88.3	74.7	87.2	82.0	93.9
Rural	5.2	1.3	7.6	—	20.0	10.7	30.3	15.0	33.3	21.3	18.6	8.4
Type (percent of episodes)												
Revolution	—	—	—	—	—	—	—	—	2.7	4.3	0.4	0.6
Rebellion	46.9	48.1	44.8	46.1	42.6	47.6	40.4	46.7	33.3	36.2	42.0	45.7
Coup	11.5	13.9	7.6	5.3	4.3	4.8	9.1	13.3	1.3	2.1	6.9	8.1
Riot	7.3	8.9	6.7	9.2	11.3	13.1	5.1	3.3	6.7	8.5	7.6	9.0
Protest	16.7	12.7	21.0	19.7	32.2	28.6	26.3	28.3	20.0	12.8	23.7	20.8
Unclear	10.4	8.9	12.4	10.5	17.4	15.5	20.2	16.7	22.7	17.0	16.3	13.3
Not recorded	12.5	12.7	12.4	14.5	4.3	3.6	6.1	3.3	13.3	19.1	9.4	10.1
Violence tier (percent of episodes)												
Violent	62.5	67.1	58.1	59.2	54.8	60.7	52.5	60.0	44.0	51.1	54.9	60.4
Peaceful	14.6	11.4	17.1	15.8	23.5	20.2	21.2	20.0	20.0	12.8	19.4	16.2
Ambiguous	10.4	8.9	12.4	10.5	17.4	15.5	20.2	16.7	22.7	17.0	16.3	13.3
Unknown	12.5	12.7	12.4	14.5	4.3	3.6	6.1	3.3	13.3	19.1	9.4	10.1

Notes:
SR = study region: the 490 episodes geocoded inside the predominantly German-speaking part of the Empire (the shaded area of the map), plus non-geocoded episodes the source places in Germany. CM = city-matched: the 346 episodes linked to a study-population city. Grievance types, unrest types, and the urban and rural flags are non-exclusive, so they can sum to more than 100; None is the share with no grievance flag. Violence rows sum to 100. A dash marks exactly zero.

of source rows, and I leave those episodes as not recorded rather than impute a type. Two coarser codings derive from the type. Severity is major for revolution, rebellion, and coup and minor for riot and protest; the violence tier, also in Table 1, is violent for revolution, rebellion, coup, and riot, peaceful for protest, and ambiguous for unclear.

Two source-level concerns condition every pattern drawn from this catalog. The first is the ordinary survivorship problem: no systematic treatment of medieval revolt is ever complete, and even for intensively studied regions, archival work on unrelated topics keeps surfacing episodes absent from the canonical secondary literature (Blickle, 1979, p. 232). The second is that nothing can benchmark it. Earlier work has charted urban conflict over time (Ehbrecht, 1980, p. 136), but no earlier compilation is georeferenced, dated to the year across the Empire, and covers rural as well as urban episodes, so none can test this catalog's coverage at the level of the city-year. The catalog should therefore be read as a lower bound on unrest



(a) Unrest episodes, linked and not linked

(b) Institutional coverage of study-population cities

Figure 1: Unrest episodes and institutional coverage in the study region, 1300–1520. The shaded area is the study region, the intersection of the Empire's c. 1378 border (solid gray) and the German-speaking region (dashed green). Panel (a): unrest episodes, linked to a backbone city or not; a linked episode appears at each linked place. Panel (b): the 301 study-population cities by institutional record: entries inside the window (79.1%), entries only before 1300, carried forward (13%), or none (8%).

incidence, weighted toward episodes prominent enough to draw chronicle notice or secondary commentary. Because each source has its own regional, thematic, or temporal focus, documentation may also vary across region and time: a region with fewer recorded episodes may have had fewer episodes or thinner coverage, and these data alone cannot tell the two apart.

3.2 Institutions

The institutional dataset gives each city's constitution year by year. It is built from a record of changes, descriptions, impositions, and reforms—one row per documented entry, not an annual observation—that holds 998 dated changes in 301 of the 301 cities of the common backbone (Section 3.3), the same cities to which the unrest catalog and the other sources are matched. It codes three variables: guild representation and burgher representation, each on an ordinal 0–3 scale, and imperial status; 0/1 versions of the two scales, derived from them, are used in the main tests and the comparisons described below. It is more detailed than the other institutional datasets available for the Empire. The Participative Political Institutions Database (Wahl, 2016, 2019) codes representation at century marks, Bosker et al. (2013) code local participative government by century and only for cities that reached 10,000 inhabitants, and the city-level data in Schaff

(2024) record whether a city held council elections at 25-year intervals, rather than annually, coarsening the same kind of transition record that this paper converts directly into an annual series. The city-institution and political-fragmentation dataset in Bosshart & Dittmar (2026, p. 16) is closer in resolution: it dates its variables directly at the city level, typically by year, and likewise draws its council, charter, and collective-action variables directly from the *Städtebuch*. It is not publicly released at the time of writing, however, so no direct comparison of the two datasets' coded values is possible yet. This dataset scores the shares of the council held by guilds and by the citizenry, and links them to unrest within cities, whereas their conflict data cover cities' disputes with their lords (Bosshart & Dittmar, 2026, p. 16).

The constitutional record is derived primarily from three encyclopedic sources. For German cities it draws on the *Deutsches Städtebuch*'s regional volumes (Keyser, 1939, 1941, 1952, 1954, 1956, 1957, 1959, 1962, 1964; Keyser et al., 1995; Keyser et al., 2000; Keyser et al., 2003; Keyser & Stoob, 1971, 1974; Neitmann et al., 2022), for Austrian cities it draws on the *Österreichisches Städtebuch* (Baltzarek & Pradel, 1973; Csendes et al., 1999; Goldmann et al., 1976; Goldmann, 1982, 1988; Hoffmann, 1968; Hye, 1980, 2001; Knittler, 1970), and for Swiss cities on the *Historisches Lexikon der Schweiz* (HLS). Economists have used the *Städtebuch* as a town-level source before, to date construction by building type (Cantoni et al., 2018, p. 2064; Dittmar & Meisenzahl, 2019, p. 965). Supplementary material, chiefly an Alsatian historical dictionary and Wikipedia, fills in any gaps.

Guild representation measures how far guilds took part in governing the city, and burgher representation measures how far the citizenry as a whole chose its government. The two are scored separately, so that guild delegates count only toward the first. Each runs from 0 to 3, and Table 2 gives the criteria for every score, the treatment of compulsory guild-type electoral units, and the derived indicator for guild seats. A second derived indicator, burgher seats, is 1 where burgher representation is 2 or higher and 0 otherwise. The third variable, imperial status, is taken from the territorial histories of *Princes and Townspeople* (Cantoni et al., 2019) and cross-checked against the *Städtebuch* and Wikipedia; a loss of status is recorded as an entry in the record.

The tests use these variables in different forms. The regressions of Section 4 and Section 5 enter guild seats and burgher seats on the annual panel as they stood the year before and, as a robustness check, as the share of each five-year period in which they are held; the 0–3 scales and imperial status enter only as robustness variants. The comparisons with Wahl and Wigton-Jones also rely on the 0/1 versions. The Wahl cross-walk below compares guild seats and burgher seats with Wahl's coded guild participation and burgher representation. The guild-revolt replication takes a gain of guild seats as its outcome. The Wigton-Jones benchmark builds its three-tier government type from the guild-seats and burgher-seats indicators.

Each entry is scored from the source text against the rules set out in Table 2. No second coder has

Table 2: Coding of the institutional variables

Variable	Score	Criteria
Guild representation (0–3)	0	No formal guild role, including a stated absence, abolition by the lord, craft seats reserved on a council filled by co-optation or by the lord, and a merchant guild that is itself the co-opting ruling elite.
	1	An advisory or consultative channel only, or guilds that elect, or dominate the college that elects, the council without holding council seats.
	2	Guild representatives hold voting seats on the council, half or fewer of its members (exactly half scores 2).
	3	A guild majority on the council, or a formal guild constitution (*Zunftverfassung*).
Burgher representation (0–3)	0	Rule by a lord or retainers, or a hereditary mayor.
	1	A patrician or co-opted council, including one elected by the council itself.
	2	Limited representation: a citizen committee beside a co-opted council, a single delegate, or half or fewer of the council chosen by the citizenry.
	3	A majority of the governing council, or of the executive, chosen by the citizenry.
Guild seats (0/1)	0	Guild representation of 0 or 1: guilds hold no voting seats on the council.
	1	Guild representation of 2 or 3: guild representatives hold voting seats on the council.
Burgher seats (0/1)	0	Burgher representation of 0 or 1: a lord, his retainers, or a patrician or co-opted council governs.
	1	Burgher representation of 2 or 3: the citizenry chooses at least part of the council or its executive.
Imperial status (0/1)	0	A city under a territorial lord, including a city whose imperial standing was claimed but never conceded or was pledged away and not redeemed.
	1	A free, imperial, or free imperial city, immediate to the Empire, from the year the status is documented until it is lost, for example by mediatization.

Notes:
Scores are assigned to each documented entry and carried forward to the next; a cell the sources cannot settle inherits the previous value. Compulsory guild-type electoral units covering the whole citizenry (the Cologne Gaffeln, the Strasbourg tribus) score as guild representation by their share of council seats (3 for a majority, 2 for half or fewer), and election through them counts as election by the citizenry on the same rule. Guild delegates do not count toward burgher representation.

checked the scores. I then convert the resulting record to annual data by carrying values forward for each city under the assumption that a constitutional change persists until the record states otherwise. This assumes no unobserved reversal between recorded changes, and mirrors the convention in other institutional datasets (Schaff, 2024; Wahl, 2016, 2019). The scores still rest on judgment calls, most visibly the treatment of reserved seats, electoral colleges, and merchant elites. Where the source does not settle a cell, I leave it blank and the cell inherits the previous entry's value. The years before a city's first recorded entry are left missing rather than backward-filled, because the early record is thin.[7]

Governance before the Late Middle Ages (except for a few large cities) is not well recorded. Before about 1200, towns were governed mainly through the town lord's officials, such as a *Vogt* or *Schultheiß*, working

[7] A sensitivity check on this choice for guild and burgher representation is in the online appendix.

with citizen judges and advisers; councils first appear in the sources from the end of the 12th century and spread to some 150 towns by 1250 and about 400 by 1300 (Isenmann, 2014, pp. 216–217, 220; Planitz, 1965, p. 297). In this dataset, 167 of the 301 cities (55.5%) have a documented council by 1300, and 235 by 1400. At a city's first recorded entry, burgher representation is already nonzero in 67.8% of cities (185), while guild representation is absent in 95.8% (250). This is consistent with the record usually beginning once a council is already in place, and with guild participation arriving later as a reform, mostly in the fourteenth century: 49 of the 86 cities that reach guild seats do so between 1300 and 1399, with a median year of 1382. Over 1300–1520, the resulting annual dataset covers 66,521 city-years across 301 of the 301 cities. By city, 79.1% have at least one documented entry inside the window, 13% rest on entries from before 1300 carried forward, and 8% have no entry in or before it. By city-year, counting all 301 cities, 84% carry a coded value, but only 55.7% rest directly on an in-window entry; another 27.9% are carried forward from an earlier one. Figure 1b shows the city-level classification geographically.

The new institutional dataset shares much of its archival lineage with Wahl's dataset, so its novelty lies in resolution and construction. At his century marks inside the window (1300, 1400, 1500), the two can be compared on 242 city-years in 134 cities; 219 of the 301 backbone cities appear in his database, and the unmatched cities include the Alsatian and Swiss ones, which his Germany-only database leaves out. Net of the agreement expected by chance, Cohen's kappa is 0.45 for guild and 0.18 for burgher representation, moderate and slight agreement (online appendix). The guild disagreement runs almost entirely one way: of 43 disagreements, 38 are city-years this dataset codes as represented and Wahl does not. Symmetric coding error cannot produce that asymmetry, but a difference in threshold can: this dataset counts guild voting seats short of a full guild constitution, which a century-snapshot panel keyed to a formal guild constitution codes as absent. The guild coefficients here are therefore not directly comparable in level to results estimated on his coding, and the burgher measure is the noisier of the two. I treat neither dataset as ground truth.

3.3 City backbone

The backbone is every city in the European urban population dataset (Buringh, 2021) that lies in the predominantly German-speaking part of the Empire, within its c. 1378 boundary,[8] and records 1,000 or more inhabitants in 1300, 1400, or 1500. The dataset lists every city that reached that size by 2000, so the population condition removes later foundations such as Karlsruhe and Wolfsburg. Most of these cities also

[8]The boundary is held fixed over the whole window. Silesia lies inside it, although its duchies became Bohemian fiefs only from 1327, Schweidnitz-Jauer passed to the Bohemian crown only in 1392, and the incorporation of the duchies into that crown in 1348 and 1355 tied Silesia to the Empire only indirectly (Keyser et al., 1995, p. XXXV); its cities are kept for all years. Restricting the approximated German-speaking region to the Empire's c. 1378 border removed five towns that lay outside the Empire. An exception list then adds two border towns that the map's edge leaves just outside; it changes nothing, since on the map's boundary alone the headline estimates are the same and the climate association is significant at the same level, the 5% level (online appendix).

appear in the *Princes and Townspeople* dataset (Bogucka et al., 2019), whose identifiers link them to its data (also drawn from the *Deutsches Städtebuch*). Three further entries are left out (online appendix). The population data share their lineage with the Participative Political Institutions database (Wahl, 2016, 2019) and the Wigton-Jones benchmark (Wigton-Jones, 2025) in Section 4, which keeps the backbone comparable to both. The institutional volumes for Alsace and Switzerland number cities differently from the backbone, so those cities are linked by name.

4 Climate and Unrest

I first ask whether documented unrest in the backbone cities rises or falls with climate shocks, and whether that association differs with a city's institutions. Weather shocks have been tied to conflict in many settings (see the reviews in Hsiang et al., 2013; Dell et al., 2014, pp. 766–767). Colder growing seasons raised the persecution of Jews in pre-industrial Europe (Anderson et al., 2017), and consecutive periods of cooling went with more armed conflict in Europe, the Near East, and North Africa between 1400 and 1900, though an isolated period of cooling did not (Iyigun et al., 2024, pp. 81–82). Droughts and floods increased peasant revolts in imperial China (Jia, 2014), drought did the same in revolutionary France (Waldinger, 2024) and drove nomadic attacks on China (Bai & Kung, 2011), and negative rainfall-driven growth shocks raised the risk of civil conflict in modern Africa (Miguel et al., 2004). Institutions shape how such shocks play out. Nile floods shifted political power in Egypt toward religious authorities (Chaney, 2013), the Black Death pogroms in the Holy Roman Empire were less intense and violent where the emperor governed directly (Finley & Koyama, 2018), and in an Empire subsample drought-induced conflict was muted where city councils were elected (Wigton-Jones, 2025). This section takes up the climate question and the published result that bears on it; Section 5 takes up the institutional one.

The regressions bring the catalog and the institutional dataset together on the 301-city backbone. An episode is linked to a backbone city when a place it is recorded at lies near the city and bears its name or a close variant (online appendix). Of the 490 study-region episodes, 346 link to a backbone city (Figure 1a); these city-matched episodes are the outcome in every regression that follows. The linked subsample is more urban than the full catalog (Table 1), so the results speak to urban unrest more than to rural. Wet-dry severity comes from a historical climate reconstruction of the self-calibrating Palmer Drought Severity Index (scPDSI) (Cook et al., 2015); negative values indicate drought, so a positive coefficient means wetter conditions. Population comes from the dataset that defines the backbone (Section 3.3), which records towns of 1,000 or more at century marks; each year takes the city's most recent snapshot (1300, 1400, or 1500), with years before a city's first snapshot set to 500 and gaps between snapshots interpolated linearly.

Plague exposure data come from a geocoded dataset of European plague outbreaks (Schmid et al., 2015) supplemented with the plague entries of the *Deutsches Städtebuch*. Rye suitability is the potential yield of rain-fed winter rye under low inputs from the Global Agro-Ecological Zones (GAEZ) project of the Food and Agriculture Organization, the crop-yield data behind the Caloric Suitability Index, extracted at each backbone city's coordinates (Galor & Özak, 2015b, 2015a, 2016); it does not vary over time within a city, so it enters only through its interaction with wet-dry severity. Market rights come from the markets series of the *Princes and Townspeople* collection (Cantoni et al., 2020), which is supplemented for Swiss, Alsatian, and Austrian cities using the *Historisches Lexikon der Schweiz*, the *Dictionnaire historique des institutions de l'Alsace*, and the *Österreichisches Städtebuch*. A city counts as holding a market from its first documented grant or mention, and loses it when a recorded withdrawal cancels its grants.

The outcome is one when a city-matched unrest episode falls in a city in a given year and zero otherwise. The main specification is a linear probability model with wet-dry severity, guild and burgher representation, a market indicator, log population, and a plague indicator as covariates, and city and year fixed effects. Log population and the plague indicator enter as the covariates most likely to drive both unrest and its survival in the record. Representation enters as it stood the year before, since it often changed in the course of unrest, and its value in the same year would partly record that unrest (Section 5).

The accounts in Section 2 give the climate shock no single predicted sign. Deprivation and opportunity-cost accounts predict more unrest after a bad year, the moral-economy account predicts it only where the response breaches customary entitlement, and the collective-action account predicts less, since a shock that absorbs households' time and resources makes organizing harder. Scarcity also cut both ways for councils: they fixed maximum grain prices, sold from communal stores below the market price, and banned exports in times of dearth to forestall unrest (Isenmann, 2014, pp. 978–980; see also Dameron, 2017),[9] but the cost could itself breed fiscal grievance, as when Basel borrowed the equivalent of a full year's budget in 1438/39 to buy grain and build a granary (Isenmann, 2014, p. 980). Because these accounts speak to bad years, I also split the distribution into its tails, with indicators for a city's driest and wettest tenth of years against the middle 80 percent; a linear term alone cannot show whether an association sits in drought years, in wet years, or in both.

The models are estimated on the annual panel, the resolution of the climate-conflict literature and of the benchmark below. Unrest is rare at that resolution, recorded in 0.66% of city-years in the main sample, the 252 cities with a recorded constitution, so I read every coefficient against that base rate. A logit or probit with city fixed effects, estimated directly, is exposed to the incidental-parameters problem;[10] a conditional

[9] Nuremberg did all three in 1437, under popular pressure (Isenmann, 2014, p. 980).

[10] Fixed-effects logit and probit estimators can suffer from the incidental-parameters problem, the bias that arises when many fixed effects are each estimated from few observations; it shrinks as the number of observations per city grows (Greene, 2004).

Table 3: Climate timing: wet-dry severity this year and last

	Any unrest × 100			
	Same-year climate		Prior-year climate	
Model:	(1)	(2)	(3)	(4)
Variables				
scPDSI	0.065**	0.074	0.052*	-0.019
	(0.028)	(0.071)	(0.027)	(0.063)
scPDSI × Rye suitability		-0.005		0.042
		(0.039)		(0.036)
Log population	0.056	0.056	0.056	0.056
	(0.104)	(0.104)	(0.104)	(0.104)
At median rye suit. (1.70 t/ha)				
scPDSI		0.065**		0.053*
		(0.028)		(0.027)
City	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes
Fit statistics				
Observations	66,521	66,521	66,521	66,521
R^2	0.02070	0.02070	0.02066	0.02067

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

Linear probability models on the annual city-year panel, 1300–1520, all 301 backbone cities; outcome 100 if unrest, 0 otherwise, so coefficients are in percentage points. Columns 1–2 enter the same year's scPDSI (negative in drought) and 3–4 the year before's. Even columns add the rye interaction and report the climate term at zero suitability; the rows below the line give it at the median city.

logit avoids it but drops every city that never records unrest, since its outcome never varies (Timoneda, 2021), and reports no change in probability. Those cities carry no within-city variation in a linear model either, so the choice is one of modeling rather than of sample: the linear model handles the fixed effects directly and keeps every coefficient readable against the base rate. Standard errors allow for correlation across space and over time (Colella et al., 2019; Conley, 1999): between cities within 100 km of each other in the same year, and within a city across years up to 50 years apart. The weights fall linearly with distance and with time, which keeps the estimated variance from turning negative (Newey & West, 1987). The online appendix reports equal weights, city-clustered standard errors, and other cutoffs.

Table 3 starts with climate alone, in the same year and the year before, and asks whether the association depends on a city's agricultural endowment, through an interaction with rye suitability. The association is clearest for the same year's conditions. Across all 301 cities, including those without an institutional record, unrest breaks out in just 0.53% of city-years, and a year one standard deviation wetter than usual sees roughly 15% more of it (column 1). That standard deviation, 1.2 scPDSI units, is how far a year typically departs from its city's average and from that year's average across cities, so the comparison is with a city's own usual conditions. Because the preindustrial past was drier than the twentieth-century calibration period (Cook et al., 2015, p. 5), with an average city-year scPDSI of -0.9 here, much of that variation runs from more drought to less. Neither tail carries the association on its own: with the controls of Section 5, bottom-

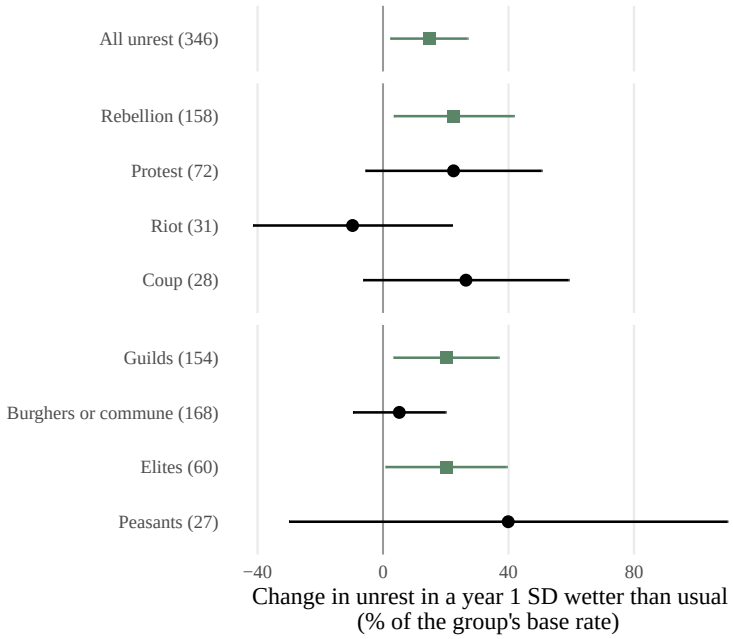


Figure 2: Same-year wet-dry severity and unrest by type and perpetrator. Each point is the change in unrest in a year one within-city standard deviation wetter than usual, as a percent of the group's own base rate, from column 1 of Table 3 with the outcome restricted to that group; episodes in parentheses. Lines are 95% Conley confidence intervals; green squares are significant at the 5% level, black dots are not. Groups overlap, since an episode counts for every type and perpetrator its sources name.

and top-decile years do not differ significantly from the middle 80 percent (Table 5, column 2). The bottom decile (median −5.0) is extreme drought, while the top decile (median 2.7) is mostly only moderately wet. Last year's climate points the same way but more weakly (column 3).

Rye suitability does not condition the association in either year: neither interaction is significant, and since nine in ten cities lie between 1.46 and 1.93 t/ha, the association at the median city, about 15% of the base rate, restates column 1's for the cities in the sample (online appendix). The association holds beyond this table. Its sign is the same on five-year periods and in a Poisson model of the unrest count, where it is significant. In the main specification of Section 5, which adds institutions and the other controls, it is significant at the 5% level with Conley standard errors, and at the same level under randomization inference and with region × half-century fixed effects, which absorb regional differences in coverage that change over time; dropping every episode that cites the residual group of other literature leaves it not significant (online appendix). The consistency of the sign is weaker than it looks, since most of these specifications are fit on close variants of the same panel. A wetter-than-usual year going with more unrest runs against the work in which drought or income loss raises conflict (Miguel et al., 2004) and against deprivation accounts that expect more unrest after a bad year (Gurr, 2010); it is the direction the collective-action account allows.

Figure 2 splits the outcome by what the unrest was and by who carried it out. In a year one standard

deviation wetter than usual, rebellions rise by about 23%, unrest led by guilds by about 20%, and unrest led by urban elites by about 20%, each against its own base rate and each significant. The association is not universal. Unrest led by burghers or the commune, the largest group, shows almost none (about 5%), and riots show none. Across these subsets the wet-year association sits with organized actors rather than with unrest in general, but that is an exploratory pattern found among many forms and groups, and none of the subset coefficients survives correction for the number of forms examined (online appendix). Protest, coups, and peasant risings point the same way but rest on too few episodes to read. The pattern fits the collective-action account: guilds and elites are small, organized groups that can act at low cost and press their claims when conditions are good. The subsets overlap and several are small, so the consistency of the pattern carries more weight than any one coefficient.

Wigton-Jones (2025) studies the drought-conflict relationship, moderated by local government type, across 673 European cities from 900 to 1800, and finds that more severe droughts raise the probability of conflict. On a 324-city Holy Roman Empire subsample built on Wahl's dataset, the effect is muted where councils are elected and persists where representation runs through burghers or guilds. A single comparison on my own data could not tell a failure of his pattern from an error in my implementation, so I first reproduce his published Table 6 exactly on his own data (online appendix); Table 4 then moves from his estimates to my own specification one change at a time. A diagnosis of the remaining divergence follows.

His pattern does not survive the move to my data, and the columns show where it breaks. On the 212 cities that both panels cover and both codings reach, in the years 1300–1520, his own data no longer tie drought to the outbreak of conflict, though elected councils still mute it at the 10% level (columns 1 and 2). Replacing his catalog with mine, his codes and cities held fixed, turns the drought-by-guild term negative and significant (column 3). Columns 4 and 5 keep his functional form on all 252 backbone cities with a recorded constitution, the institutional sample that Section 5 uses throughout, with government in three tiers: burgher/guild, where guild representatives hold voting seats; broader burgher representation, with burgher seats chosen at least in part by the citizenry but no guild seats, which stands in for his elected councils; and non-participative. Year by year nothing is significant. In five-year periods broader burgher representation mutes drought, as his elected councils do, and mutes wet periods as well. Columns 6 and 7 replace drought severity with the driest tenth of years or periods, the bottom-decile cut used above, and add population and plague. In five-year periods both participative tiers carry significantly less unrest in the driest periods; year by year only the guild tier does. Both signs fit the substitution prior of Section 2. The guild sign runs against his finding that the drought effect persists where guilds are represented, and matches what Section 5 finds once the years of institutional change are set aside.

The annual guild interaction does not hinge on the driest-tenth cut or the standard errors. With his

Table 4: Wigton-Jones's drought specification, from his panel to my data

	His specification			His form, my data		My form	
	Published	1300–1520	My catalog	Annual	5-year	Annual	5-year
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variables							
Drought	0.033**	-0.038	-0.063	-0.043	-0.205	-0.005	0.733
	(0.015)	(0.047)	(0.050)	(0.045)	(0.292)	(0.171)	(0.803)
Flood	0.025*	0.076	0.111	0.045	0.193		
	(0.015)	(0.075)	(0.086)	(0.068)	(0.523)		
Elected / broader burgher	-0.049	0.137	0.223	0.306	0.131	0.213	-0.892
	(0.052)	(0.149)	(0.272)	(0.233)	(1.078)	(0.207)	(0.889)
Burgher rep.	-0.073	-0.287*	-0.073				
	(0.047)	(0.151)	(0.269)				
Guild rep. / burgher-guild	-0.037	-0.278	-0.007	0.427	1.021	0.517**	0.565
	(0.062)	(0.174)	(0.227)	(0.275)	(1.400)	(0.230)	(1.072)
Drought × Elected / broader burgher	-0.061**	-0.111*	0.035	-0.030	-0.666**	0.014	-2.609***
	(0.024)	(0.060)	(0.052)	(0.061)	(0.316)	(0.329)	(0.966)
Drought × Burgher rep.	0.021	0.049	0.086				
	(0.024)	(0.068)	(0.065)				
Drought × Guild rep. / burgher-guild	0.047	0.133	-0.150***	-0.057	-0.534	-0.628**	-2.766**
	(0.039)	(0.092)	(0.055)	(0.063)	(0.445)	(0.300)	(1.317)
Flood × Elected / broader burgher	-0.037**	-0.065	-0.062	-0.090	-1.906***		
	(0.018)	(0.051)	(0.156)	(0.101)	(0.707)		
Flood × Burgher rep.	0.020	-0.010	-0.025				
	(0.018)	(0.065)	(0.155)				
Flood × Guild rep. / burgher-guild	-0.022	-0.038	-0.183	0.231	-0.267		
	(0.023)	(0.072)	(0.116)	(0.167)	(0.965)		
Log population						0.037	0.207
						(0.133)	(0.642)
Plague						0.664	0.263
						(0.518)	(1.036)
City and year or period fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Geography × century controls	Yes	Yes	Yes	No	No	No	No
Fit statistics							
Observations	291,600	43,500	43,500	51,315	10,484	51,315	10,484

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

Outcome 100 if a conflict (columns 1–2) or an unrest episode (3–7) starts. Column 1: his 324-city panel, 900–1799. Columns 2–3: the 212 cities both codings reach, 1300–1520, with his codes. Columns 4–7: all 252 cities, my three tiers, same-period (broader burgher representation in the elected-council row, burgher/guild in the guild row). Drought is severity on his PDSI (1–3) or scPDSI (4–5), and a driest-tenth indicator (6–7).

severe and extreme bins in place of the driest tenth, the extreme-drought interaction with burgher/guild representation is again negative and significant; city-clustered standard errors leave it significant at the 10% level; and distance cutoffs from 75 to 200 km barely move any standard error (online appendix).

I then diagnose the remaining divergence one dimension at a time: geographic and temporal scope, functional form, the definition of an event, and the events each catalog records. Restricted to the 237 backbone cities his panel also covers and to the 1300–1520 window, his data still tie drought to more years of ongoing conflict: in his conflict-incidence column the drought coefficient stays positive and significant, and larger than published. They do not tie it to the outbreak of conflict, the measure that corresponds to my episode starts. In his conflict-start column the drought coefficient turns slightly negative, the direction of my own estimates, though too imprecisely to tell it from either zero or his published value. The rest of his

pattern largely holds: 8 of 11 coefficients keep the published sign in the conflict-start column and 9 of 11 in the incidence column, and the sign changes are the kind of instability a much smaller sample produces. Scope cannot therefore be ruled out for outbreaks; the restriction leaves too little precision to decide.

Functional form does not explain the reversal either. With his drought and flood bins in place of my continuous measure, every drought bin carries a deficit of documented unrest, the severe and extreme bins the largest, though only the severe bin reaches even the 10% level; a quadratic in wet-dry severity adds nothing, and the bins show no interior turning point (online appendix).

The two conflict measures are built differently, and the difference runs deeper than a casualty threshold. Wigton-Jones draws his events from Brecke's general historical conflict catalog, supplemented by Bradbury, and keeps an event only if he can attribute it to a specific originating city. His main source admits only conflicts with 30 or more casualties, a floor that excludes exactly the peaceful protests and unarmed refusals my catalog is built to admit. His civil category also pools two kinds of conflict, a city's population rising against its own government and a city fighting a higher authority whose power over it was only nominal, as the emperor's was over the Free Imperial Cities, and he counts city-level risings within interstate wars (Wigton-Jones, 2025, pp. 1110, 1112). My catalog's closest analog to the second kind is a refusal of homage; internal unrest against a city's own council or patriciate, the more common case in both catalogs, is closer to a like-for-like comparison. On the same 237 cities, unrest breaks out in my catalog in 0.561% of city-years, about three times his conflict-start rate of 0.185%, as a broader inclusion rule would produce, though the rates alone cannot prove it.

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. Re-estimated on violent episodes alone, severe-drought city-years carry about 78% less unrest than near-normal years, and on revolutions, rebellions, and coups, the closest analog to a casualty threshold, about 88% less, both significant (online appendix). The casualty threshold therefore does not explain the divergence.

What does is the set of events each catalog records. On the 237 cities both panels cover, only 9 of his 97 conflict starts fall within two years of an episode start in my catalog, and only 9 of my 294 episode starts within two years of one of his. His starts also fall in drier years: 38% of them in years of severe or extreme drought on his own measure, against 16% of mine and 18% of all city-years. With his specification and his government codes held fixed on those cities and years, his catalog gives a positive drought-by-guild term and mine a negative one, significant at the 1% level (Table 4, columns 2 and 3); switching to my annual codes leaves both signs in place (online appendix). The divergence therefore sits in what the two catalogs record more than in the estimator or the government codes.

Many of his starts come in clusters. In 18 years his data record conflict starts in ten or more cities of the

Empire at once, and those years hold 65% of all his conflict starts there; in 1503, a year of severe drought on his measure, they record starts in 26 cities, one war entered once for each city on the warring sides: the War of the Landshut Succession, fought by the Palatinate, Bavaria, the Swabian League, and the emperor, which Brecke's catalog dates from December 1503 (Brecke, 1999) and historians to 1504–05 (Heil, 2010). Whether his results for the outbreak of conflict in the Empire hold depends on letting these simultaneous starts share a shock. With standard errors clustered by city and by year, which allows it, neither his published drought term nor its muting by elected councils is significant. His data carry no conflict identifiers, so a war entered for many cities cannot be counted once; counting each year's starts once stands in for that. Because it also merges unrelated starts of the same year, it understates the number of separate conflicts, which works against finding an association. Counted that way, so that a war entered for 26 cities weighs as much as a single start, neither the drought association in his conflict-start column nor the muting by elected councils remains significant. The weighted drought estimate, about 8% of the base rate with a 95% interval from -7% to 22%, cannot be told apart from zero or from his 17%. The years with many starts are not special for the drought term: dropping random sets of years that hold as many starts as the 18 multi-city years leaves it between -7% and +16% of the base rate in 95% of draws, and dropping those years leaves -3% (online appendix). The muting is different. It disappears once 1503 is set aside, falling from about 31% of the base rate to about 7% (online appendix).

On Wigton-Jones's own data drought goes with more conflict, as published, though for the outbreak of conflict not once the many starts of a single year are counted once (Table 4, column 1; online appendix). On mine it never goes with significantly more documented unrest, and the data rule out an effect as large as his: in cities without participative government, a drought one unit more severe on his measure raises the chance of an outbreak by about 17% of the base rate in his data, against at most about 5% in mine, the upper end of a 90% interval. The regressions above and the Wahl guild-led unrest specification (online appendix) find the reverse, wetter years with more unrest, and the drought bins point the same way without reaching the 5% level; the severe-drought indicators of the two benchmarks are null. The diagnosis locates the divergence in the catalogs, which barely overlap. Neither the estimator nor the functional form explains it, and neither does the casualty threshold, since the violent episodes both catalogs count show the deficit too. Whether thin documentation also suppresses drought-year unrest in my catalog remains open (online appendix). The grievance record bears on the question from outside the regressions: climate is a marginal presence in what the sources say the unrest was about (Section 3), consistent with the argument that Flemish urban rebellion in the same broad period was the work of politicized burghers pursuing fiscal and constitutional access more than an undifferentiated hunger revolt, even where food prices were rising (Dumolyn & Haemers, 2005).

5 Institutions and Unrest

Representation enters the regressions next, as a covariate in its own right and as a moderator of the climate association. The question here is what a city's representation went with, and when. For representation, the *Kommunalismus* channel predicts that guild and burgher seats each substitute for unrest. For guilds the effect may fade: once in power a guild could turn its organizing capacity to defending its position and close membership and citizenship to outsiders, who then had a new grievance (Ogilvie, 2004; Olson, 2003), so the guild prior is ambiguous and may change over time. Markets make coordination cheaper (Geloso & Kufenko, 2019) and anchor the prosperity that, on one reading, enriched the craftsmen who then sought power (Wahl, 2015), so their prior is positive.

Table 5 adds institutions, measured as they stood the year before, and interacts wet-dry severity with each to test whether representation mutes the climate association. Guild seats go with about 69% less unrest, significant at the 5% level, a result taken up below. Burgher seats, which the *Kommunalismus* channel predicts would quiet unrest, show nothing. Holding a market goes with more unrest, as the coordination account predicts (Geloso & Kufenko, 2019), though not significantly within cities; between cities, on the settled years of Table 6, it is significant at the 1% level but likely overstated, since market towns were larger, better connected, and better documented (online appendix). A plague year goes with more recorded unrest in every specification, roughly doubling the usual rate, but the estimate is never significant.

The climate association keeps its sign across the table, though not its precision. The scPDSI coefficient is positive in every column. Section 4 reports how the main estimate of column 1 holds up. In the interaction columns it measures the association in cities without guild or burgher seats, and where both are held it is larger and significant (rows below the line). The deciles keep their opposite signs too, about 24% less unrest in bottom-decile years and 23% more in top-decile years, though neither is significant (column 2). With institutions and the other controls in, the exploratory split of Section 4 looks the same: unrest led by guilds is about 20% and unrest led by elites about 24% more frequent in a year one standard deviation wetter than usual, while burgher-led unrest shows none (online appendix). The pattern is the one Section 4 read as collective action, and with the guild result taken up next it would fit a reading in which better years let the best-organized actors press their claims, and sometimes win them.

The climate association differs little with representation. No interaction term reaches the 5% level, and the readings of the deciles that follow are exploratory: the decile interactions are among many interactions examined and do not survive correction for their number (online appendix), and each tail with seats holds only 6 to 13 unrest city-years (table notes). The scPDSI interactions are small, so a wetter year goes with about as much more unrest where guilds or burghers sit on the council as where they do not (column 3); in

Table 5: Climate and institutions: representation the year before

	Any unrest × 100			
	Main		Interaction	
	scPDSI	Deciles	scPDSI	Deciles
Model:	(1)	(2)	(3)	(4)
Variables				
scPDSI	0.075**		0.063*	
	(0.034)		(0.036)	
Bottom-decile scPDSI		-0.156		-0.013
		(0.160)		(0.178)
Top-decile scPDSI		0.153		0.170
		(0.156)		(0.164)
Guild seats	-0.457**	-0.457**	-0.420*	-0.474**
	(0.226)	(0.226)	(0.240)	(0.232)
scPDSI × Guild seats			0.034	
			(0.044)	
Bottom-decile scPDSI × Guild seats				-0.309
				(0.285)
Top-decile scPDSI × Guild seats				0.595
				(0.444)
Burgher seats	-0.096	-0.099	-0.077	-0.021
	(0.190)	(0.190)	(0.193)	(0.195)
scPDSI × Burgher seats			0.018	
			(0.034)	
Bottom-decile scPDSI × Burgher seats				-0.280
				(0.257)
Top-decile scPDSI × Burgher seats				-0.533*
				(0.280)
Market (0/1)	0.268	0.264	0.268	0.263
	(0.177)	(0.177)	(0.177)	(0.176)
Plague and log population	Yes	Yes	Yes	Yes
With guild and burgher seats				
scPDSI			0.114**	
			(0.053)	
Bottom-decile scPDSI				-0.602**
				(0.299)
Top-decile scPDSI				0.233
				(0.435)
Institution by climate variable				
Guild seats, bottom-decile scPDSI				-0.783**
				(0.336)
Guild seats, top-decile scPDSI				0.121
				(0.468)
Burgher seats, bottom-decile scPDSI				-0.300
				(0.311)
Burgher seats, top-decile scPDSI				-0.553*
				(0.299)
City and year fixed effects	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 on the annual city-year panel, 1300–1520, the 252 cities with institutional coverage; outcome 100 if unrest, 0 otherwise, so coefficients are in percentage points. Representation is measured the year before. Columns 2 and 4 replace scPDSI (negative in drought) with its bottom and top deciles. In the interaction columns the climate rows refer to cities without seats, and the first block below the line gives the association where both are held; each tail-by-seats cell in column 4 holds between 6 and 13 unrest city-years.

a city with both, a year one standard deviation wetter than usual goes with about 20% more unrest. The decile form shows where the tails differ (column 4). The worst years stand out. In the driest years, cities where guild government was already in place recorded far less unrest than cities without it: in bottom-decile years, guild seats go with about 118% less unrest as a share of the base rate, significant at the 5% level (column 4), with no significant difference in wet years; the gap exceeds the base rate, which a linear model allows and the data cannot, so I read it as a sign rather than a size. At the wet end, top-decile years are

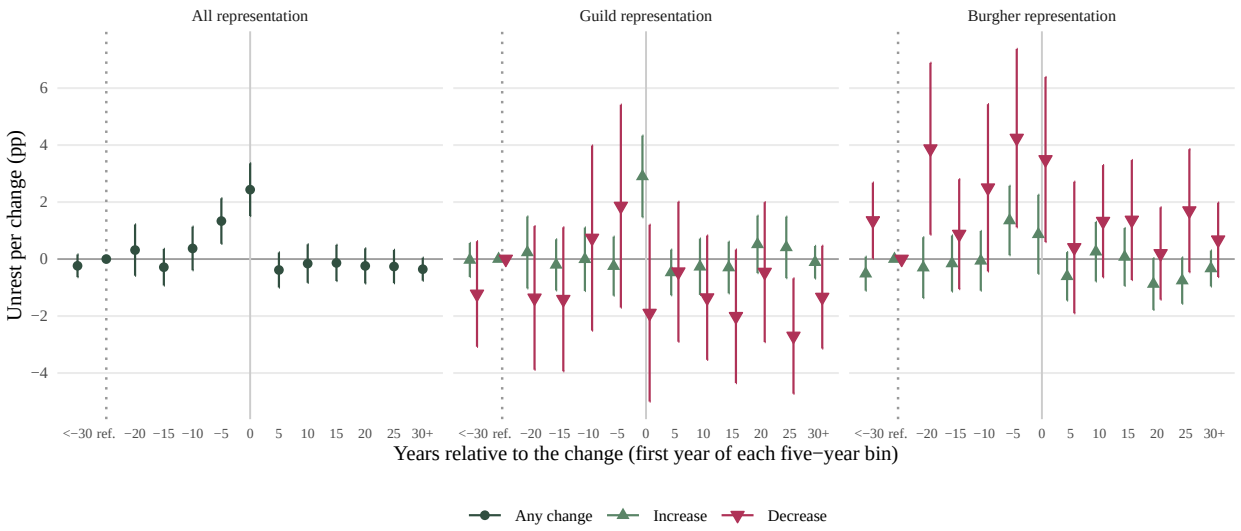


Figure 3: Recorded unrest around every change in representation. Each point is the change in unrest (percentage points) per change of that kind, other kinds held fixed, in the five-year window starting at the year shown, against the decade 21 to 30 years before; the outer windows pool everything earlier or later. Changes are moves on the 0–3 scales of Table 2. The left panel pools guild and burgher changes in either direction; the others split each into gains and losses. Lines: 95% Conley confidence intervals.

quieter where burghers hold seats, but only at the 10% level.

Those are the years of the council provisioning described in Section 4, which the record cannot separate from the organizing capacity discussed there; both fit an established guild council whose worst years were quiet. They also fit the channel reading: a guild with seats could press its members' grievances inside the council, as Flemish guilds could once they sat in city government, so the actor best placed to turn a bad harvest into unrest already belonged to the order it would have challenged (Dumolyn & Haemers, 2005, p. 379; North et al., 2009; Ogilvie, 2019, p. 38). These terms still rest on every city-year, change years included; Table 6 re-estimates them with the change years removed.

The guild main effect does not hold up. Measured in the same year instead, guild seats go with about 60% *more* unrest, significant at the 10% level (online appendix). A one-year shift is enough to turn the sign, which points to the years in which representation changes rather than to the institution itself. Figure 3 shows why the guild coefficients flip. It compares recorded unrest in five-year windows around each recorded change in representation with the decade 21 to 30 years before (Schmidheiny & Siegloch, 2023). In the five years from a gain in guild representation, unrest runs about 2.9 percentage points above that baseline per gain, several times the base rate; in the five years before it is no higher, and afterwards the data cannot tell it from the baseline. Turned around, with an unrest onset as the event and a change in representation as the outcome, the event study shows the same order: in the year an episode begins, the chance of a change

in representation rises by about 24 times its base rate, and of a gain of guild seats by about 32 times, while the five-year windows before an onset show no rise (online appendix).

On the sources' account, representation was often won through unrest, the concession to a credible threat that Acemoglu & Robinson (2000) model: in Dortmund the twenty-month siege of 1388–89 left the city deeply in debt, the council met it with new war taxes on sales and on wealth, and the guild rising of 1400 ended in a constitution that gave the six guilds six of the council's eighteen seats (Keyser, 1954, pp. 112–114).[11] When a revolt wins seats, its unrest falls in the first year of the new, higher representation, while the year before still records the old, lower level, so a single event yields a positive same-year coefficient and a negative lagged one. With city fixed effects the institution coefficients rest on exactly these changes, because representation is otherwise constant within a city: it changes in 285 of the 52,209 city-years with guild seats coded. Neither the same-year coefficient nor the prior-year one in Table 5 therefore measures how unrest went with the institutions a city already had.

Across all 285 recorded changes, in either direction and for both institutions, the guild pattern is not particular to guild seats or to gains. In the five years from a change in either institution, unrest runs about 2.4 percentage points above the baseline per change, and in the five years before it about 1.3; five years on it can no longer be told apart from its earlier level: the windows from 10 to 30 years after a change are jointly indistinguishable from the baseline, and a lasting shift anywhere from about 96% below to 36% above the base rate cannot be ruled out. The spike and the fall after it would survive drift in unrest near a change of up to 0.5 and 1.5 times the largest drift far from one (online appendix). Burgher gains differ: their unrest comes in the five years before the gain, with no rise of its own from it once guild changes are held fixed, and that build-up is the episode tied to the change (online appendix). Losses differ in timing: unrest comes in the five years before a loss of burgher representation, about 4.2 points per loss, with little after it, as if representation was taken back once a rising had run its course; before a loss of guild seats, as in the restorations of patrician rule that ended some guild struggles (Bader & Dilcher, 1999, pp. 511–512), the rise is 1.9 points (n.s.). Losses are few (30 guild and 35 burgher), so that is the least certain part of the figure.

A difference-in-differences estimator robust to staggered and heterogeneous effects gives the same answer for gains (de Chaisemartin & d'Haultfœuille, 2026). Comparing each city's first change with cities at the same level that had not yet changed, a change in guild representation goes with about 18 percentage points more unrest in the year it happens, with no significant difference in the ten years before, jointly or year

[11] In the sources for the episodes that fall in the year a city gains guild seats, the unrest comes first wherever it is described apart from the change itself. The one exception is Göttingen in 1513/14, where the council, seeking the guild masters' consent to tax increases to pay down the city's debts, first withdrew from the guilds and gave them a share in administering the city's finances; their criticism of its past conduct then turned into popular outrage that drove several councillors from the city (Postel, 1975, p. 85).

by year; averaged over the change year and the nine years after it, the effect is too imprecise to read, with a 95% interval from about 183% below to 320% above the base rate; a change in burgher representation points the same way and is significant only at the 10% level.[12] On the sources' account, then, unrest is how representation changed; once it had changed, the data cannot rule out a lasting shift as large as about the whole base rate downward, though they detect none.

The sources may explain part of that spike. Changes in representation are coded mostly from the *Deutsches Städtebuch*, which also records many of the episodes. Changes dated from a passage that describes no conflict still come with unrest in the five years from them, about 0.9 points per gain, 33% of the full figure and 1.3 times the base rate, significant at the 5% level; on the same cut a city's first guild change goes with about 8 points more unrest in its year, significant at the 1% level. Leaving out every episode that cites the city's institutional source, 192 of the 352 episode links, shrinks the spike to about 0.7 points, significant at the 5% level and 2.4 times that sample's lower base rate, but below the 0.87 points for changes dated from passages that describe no conflict; for guild gains alone it is 0.66 points (n.s.), against 0.85 for those changes (online appendix). Not citing the *Städtebuch* does not make an episode independent of it: the syntheses that take its place draw on the same chronicle tradition, so the cut removes shared citation but leaves shared provenance. On the remaining episodes a city's first guild change still goes with about 5 points more unrest in its year, about 18 times the base rate, significant at the 5% level. The episodes the *Städtebuch* records are almost never dated by a second source as well (2 of 192), so corroboration cannot be tested, and the order of revolt and concession rests on the sources' own narration (online appendix).

A panel coded by century cannot separate the years of change from the representation that followed them. Collapsing the annual panel to centuries and coding each by its representation at the century's start, as a century-mark panel does, a within-city comparison reads guild seats as lowering the chance of any recorded unrest in the century by about 11 percentage points, against a base of 27%, significant at the 5% level. Leaving out the unrest within 2 years of a recorded change shrinks that to about 2 points, no longer significant (online appendix). The large negative coefficient belongs to start-of-century coding: coded by the century's midpoint, guild seats show no significant association with any recorded unrest (about 7 points lower, against a base of 27%), and coded by its end essentially none. Coded by the century's start, the revolts that won guild seats count against the regime they overturned, and the result looks like the substitution the *Kommunalismus* channel predicts. With end-of-century coding and the change years left out, a negative association of about 14 points against a base of 23% appears instead, significant at the 5% level, which this

[12] The estimator follows each city's first change only, so it cannot estimate losses: a city's first move is almost always a gain, and its losses come later. Re-estimating a year-by-year event study around each city's first gain of guild seats with an interaction-weighted estimator for staggered adoption (Sun & Abraham, 2021) moves no window by more than 0.2 percentage points.

Table 6: Pre-existing institutions: settled city-years and comparisons between cities

	Any unrest × 100			
	Main		Interaction	
Model:	Within (1)	Between (2)	scPDSI (3)	Deciles (4)
Variables				
scPDSI	0.066**	0.062**	0.045	
	(0.031)	(0.029)	(0.032)	
Bottom-decile scPDSI				-0.025
				(0.161)
Top-decile scPDSI				0.250
				(0.157)
Guild seats	-0.094	0.232*	-0.022	-0.077
	(0.195)	(0.131)	(0.211)	(0.202)
scPDSI × Guild seats			0.063	
			(0.043)	
Bottom-decile scPDSI × Guild seats				-0.566***
				(0.214)
Top-decile scPDSI × Guild seats				0.611
				(0.478)
Burgher seats	-0.033	0.026	-0.004	0.020
	(0.169)	(0.090)	(0.175)	(0.176)
scPDSI × Burgher seats			0.028	
			(0.031)	
Bottom-decile scPDSI × Burgher seats				-0.224
				(0.192)
Top-decile scPDSI × Burgher seats				-0.372
				(0.296)
Market (0/1)	0.172	0.240***	0.172	0.171
	(0.161)	(0.074)	(0.161)	(0.161)
Plague indicator	0.284	0.420	0.287	0.289
	(0.451)	(0.444)	(0.452)	(0.452)
Log population	-0.028	0.412***	-0.028	-0.030
	(0.121)	(0.063)	(0.121)	(0.121)
Rye suitability		0.119		
		(0.261)		
Institution by climate variable				
Guild seats, median scPDSI			-0.078	
			(0.198)	
Guild seats, bottom-decile scPDSI				-0.642**
				(0.255)
Guild seats, top-decile scPDSI				0.534
				(0.492)
Burgher seats, median scPDSI			-0.029	
			(0.170)	
Burgher seats, bottom-decile scPDSI				-0.204
				(0.232)
Burgher seats, top-decile scPDSI				-0.353
				(0.311)
City and year fixed effects	Yes	No	Yes	Yes
Region and year fixed effects	No	Yes	No	No
Fit statistics				
Observations	48,203	48,203	48,203	48,203

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

Linear probability models on the annual city-year panel, 1300–1520, with representation the year before; outcome 100 if unrest, 0 otherwise, so coefficients are in percentage points. Every column drops city-years within 5 years of a change in representation (49,345 of 52,209 city-years with guild seats coded remain). Column 2 replaces city with region fixed effects and adds rye suitability; columns 3–4 repeat the interactions of the main institutions table, with each institution's association in a median, bottom-decile, or top-decile year below the line. Each tail-by-seats cell in column 4 holds between 3 and 12 unrest city-years.

account does not explain.

Table 6 asks how unrest went with the representation a city already had, by setting the changes aside. Its first column repeats the main specification on settled city-years, dropping every year within five years of a change in guild or burgher representation, a window that also takes in the unrest that builds before a change and any error in when it was recorded. The second compares cities with one another on the same years, with

region in place of city fixed effects and rye suitability as a control, and the last two repeat the interactions of Table 5. Dropping the change years is conservative: if weak representation provoked revolts that won seats, those revolts are among the years removed, which pulls the settled coefficients toward zero. Within cities, what remains compares years well before a change with years well after it, so the settled columns can detect a lasting shift in unrest but not a passing one; the later windows of Figure 3 are the more direct check, and they detect no lasting shift after a change.

Outside the years of change, representation shows no clear association with unrest. Guild seats lean negative within cities, about 18% below the base rate, and positive between cities, about 44% above it; neither reaches the 5% level. Burgher seats show nothing in any column, so the data do not support the negative association that *Kommunalismus* predicts (Blickle, 2000b), and wet-dry severity stays positive throughout. The online appendix shows the same picture with windows of two and ten years and with the 0–3 scales. These nulls bound the association rather than rule it out. Within cities, the 95% interval for guild seats runs from about 90% below the base rate to 55% above it, and for burgher seats from about 69% below to 57% above, so the data exclude only an association close to the whole base rate; between cities the burgher interval is narrower, from about 28% below to 38% above.

In the interaction columns the guild main effect is gone, which confirms that the lagged coefficient in Table 5 came from the change years, but the drought interaction is not: where guilds already held seats, recorded unrest in bottom-decile years falls to almost nothing, 3 of 1,013 such city-years, and the interaction itself is significant at the 1% level, while the burgher interaction in very wet years does not survive the cut. The guild interaction holds whichever window defines the settled years: with two, five, or ten years around each change set aside, it is negative and comes to about 92%, 107%, and 124% of the base rate, significant at the 5% level, 1% level, and 1% level respectively. Estimates beyond the whole base rate, which a linear model allows and the data cannot, I read as a sign rather than a size; a Poisson model, which cannot overshoot, puts the interaction, with five years around each change set aside, at about 66% below the base rate, significant at the 10% level (online appendix). Held to the standards applied to the climate terms and to Wigton-Jones's estimates, it is significant at the 5% level with standard errors clustered by city and by year, and under randomization inference that shifts every city's climate series, which tests whether climate is unrelated to unrest altogether, it is significant at the 5% level on its t statistic ($p = 0.03$) but not on the raw coefficient ($p = 0.11$), whose spread across shifts depends on how many settled drought years fall in cities with seats (online appendix). As one of the paper's 11 headline tests it survives a false-discovery-rate correction at the 5% level, but the stricter Holm correction only at the 10% level (online appendix). Representation, then, may have gone with how a city met its worst years, and only guild representation shows it; on average neither guild nor burgher seats show an association with unrest that these data can tell apart from zero, within the

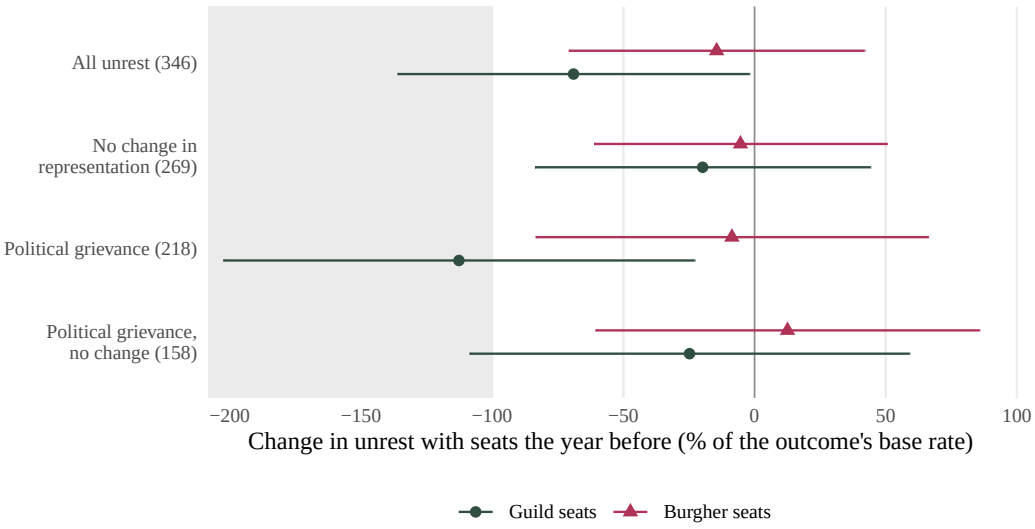


Figure 4: Representation the year before and unrest, by whether the episode changed representation. Each point is the change in unrest with guild or burgher seats the year before, within cities, as a percent of the outcome's base rate, with the controls and fixed effects of Table 5; episodes in parentheses. No change: representation unchanged from the year before the episode to two years after it. Shaded: less than no unrest, which a linear model allows. Lines: 95% Conley confidence intervals. Between-city estimates (n.s.) are in the online appendix.

intervals given above.

Dropping the years of change, however, also drops the revolts that low representation may have provoked. If a recognized voice in government lowers the pressure for unrest, low representation should also raise unrest that did not itself change representation: riots, protests, failed revolts, and revolts that won something else, such as a tax concession. Figure 4 tests that directly, keeping every year and changing the outcome instead. It classifies each of the 346 linked episodes by whether guild or burgher representation changed between the year before it began and two years after it ended, and by whether the catalog records a political grievance.

The within-city association runs through the episodes that changed representation. For all unrest, guild seats the year before go with about 69% less, significant; leave those episodes out and the gap shrinks to about 20%. For unrest with a political grievance it shrinks from a drop larger than its base rate (about 55% in a Poisson model), which is significant, to about 25%, which is not. Between cities none of the estimates is significant, with or without those episodes (online appendix). The data therefore do not show that standing representation lowers unrest: the guild association in Table 5 comes from the episodes that changed representation, the pattern Figure 3 traced. Setting aside unrest over representation itself, seats the year before show no association with the remaining unrest within cities, and at most a weak positive one between them (online appendix).

The one institutional claim Wigton-Jones tests, that representation mutes the drought-conflict link, can

be read off his benchmark in Section 4 (Table 4, columns 4–7), and the answer splits by type. Broader burgher representation mutes the severe-drought association in five-year periods, as his elected councils do, but not on the annual panel. For guild representation the sign is the opposite of his: where guilds held seats, the driest years carried the least recorded unrest, in the annual re-fit as on the settled city-years of Table 6.

The Wahl dataset compared in Section 3 supports a directional test as well (online appendix). Wahl (2015) argues that the late medieval guild revolts had structural rather than exogenous origins, in proto-industry and trade, with the agrarian crisis and the Black Death mattering only in a wider sample of smaller cities. Turning guild representation into the outcome, burgher representation predicts a gain of guild seats in his specification, which enters it in the same year, but only through the years of the gains themselves; entered from the year before, it predicts nothing. Wet-dry severity is positive and significant for guild-led unrest, the opposite of his agricultural-crisis mechanism and the same reversed sign as in Section 4. The test covers a mechanism he holds fixed rather than his headline, the proto-industry and trade contrast, which no linked source records.

Across Section 4 and this section, three patterns stand out. Wetter years go with more recorded unrest, most visibly in unrest led by organized actors, though that split is exploratory and does not survive correction for the number of forms examined; changes in representation coincide with unrest, so the representation coefficients a panel regression returns reflect the change years; and where guilds already held seats, the driest years were the quietest. The comparisons with Wigton-Jones and Wahl leave each of them in place. Treated as one family of 11 headline tests, only the change-year spike survives the strict correction for the whole family; the five-year muting by broader burgher representation and the drought-by-guild interaction on settled years survive a false-discovery-rate correction; the wet-year association survives neither (online appendix). The institutional coefficients also describe only part of the backbone. The regression sample (51,063 of 66,521 city-years, 252 cities) is smaller than the full panel because representation is missing wherever no in-window or carried-forward entry exists, following the documentation-survival pattern described in Section 3, so the institutional results speak only to cities with a recorded constitutional history. The online appendix reports the same specifications with coups as the outcome, without the market control, by half-century, and by form of unrest.

6 Conclusion

This paper builds a link the literature on the Holy Roman Empire lacked: a geolocated, event-level record of popular unrest matched, city by city and year by year, to a comparably resolved record of who governed, across 301 cities from 1300 to 1520. Its main finding concerns timing. Unrest clusters in the years in which

guilds gained representation, and those years produce the guild associations a standard panel regression returns, positive in the same year and negative the year after; away from them, neither guild nor burgher representation shows a clear association with unrest, though a moderate within-city association cannot be ruled out (Section 5). Coded by each century's start, the same panel places each of those revolts under the regime it overturned and reads guild seats as lowering unrest (Section 5). Where guilds already sat on the council, the driest years carry the least recorded unrest, a pattern that survives setting the years of institutional change aside but rests on few events, and under randomization inference is significant on its t statistic but not on its coefficient (Section 5). If it holds, representation went with how a city met its worst years.

The second finding concerns the closest published comparator, which finds that drought raises conflict and that the effect is muted where councils are elected but persists where representation runs through burghers or guilds (Wigton-Jones, 2025). In a panel built for the Empire the drought result does not reproduce, the muting reproduces only in part, for burgher representation in five-year periods, and for guild cities the sign is the opposite of his. The diagnosis built to explain the divergence rules out the estimator, the functional form, and the casualty threshold, since the violent episodes both definitions admit show the deficit too, and traces it to the two catalogs. They barely overlap, and on his own data the association of drought with the outbreak of conflict in the Empire depends on how the many simultaneous starts of a year are weighted, and neither it nor its muting by elected councils is significant once standard errors are clustered by city and by year; the muting disappears once the War of the Landshut Succession, counted once for each city on the warring sides in 1503, is set aside (Section 4).

For the climate-conflict literature, the implication is narrower than a claim that drought was unrelated to unrest in the pre-industrial Empire. The drought-year deficit is estimated on documented episodes in 301 cities. A casualty-thresholded conflict measure and a broader measure that includes peaceful protest and refusals of homage alongside armed revolt are related constructs, and a finding that holds for one need not hold for the other even within the same population and period. Whether thin documentation also suppresses drought-year unrest in the catalog remains open (online appendix). The breakdown by grievance and conflict type (online appendix) sharpens the question without settling it: economic grievance, which includes every named natural shock and is the category a subsistence-shock account would most directly implicate, is rare and, where it can be tested, also goes with wetter rather than drier years, and the grievance record is thin on subsistence language generally (Section 3). Climate may have mattered through channels the grievance record does not name, such as the room a good year gives organized actors to press their claims, or the association may be too weak to carry much weight.

Three features of the construction bound what the paper can claim. The first is the possible selectivity

of Franz (1977), one of the catalog's main sources: a survey organized around the Peasants' War is likely to understate the smaller, locally resolved disputes that never fed into its story. The second is the study population, restricted to cities large enough to enter the population data by 1500, a criterion plausibly correlated with the institutional stability the representation measures capture. If institutionally fragile cities were also more likely to decline, miss the population threshold, or lose documentary continuity, the panel underrepresents the least robust cities, so its estimates describe the more durable ones and may understate a stabilizing association. The third is that no second coder has checked the dataset's levels of representation. The timing result rests on when representation changed, which the sources date; the settled-years comparisons, the drought-by-guild pattern, and the government types used against Wigton-Jones rest on the levels, and are conditional on how I coded them.

Together, the reversed drought association, the dominance of political and fiscal grievances in the complaint record (Section 3), and the deliberately broad event criterion fit a reading in which documented urban unrest in this setting more often worked as an instrument of institutional bargaining than as a subsistence-driven eruption, closer, in the terms of Section 2, to unrest with the council as its object and the guilds as its vehicle, and to the moral-economy account than to a deprivation account keyed to the shock itself. The grievance record cannot adjudicate that reading against a competing one. Chroniclers who recorded political and fiscal complaints may simply have been less likely to record a subsistence one, so the thin subsistence record (Section 3) is consistent with either reading: bargaining dominated, or subsistence grievance went unrecorded.

If the bargaining reading holds, two things should follow. First, because urban communes had standing councils and guild structures that could absorb a grievance inside an existing forum, while rural populations more often had to build the vehicle for collective action from scratch, extending the catalog to rural episodes through a grid-cell design should show a weaker institutional association and a stronger climate association in the rural subsample; a rural extension that reproduced the urban pattern would undercut that account. Second, if unrest functioned as political negotiation under weak institutionalized voice, the extension of judicial access after 1495 through the *Reichskammergericht*, and above all the 1526 settlement that referred disputes between lords and subjects to the courts, should displace it, with the documented-unrest rate falling in a series continued past 1520 as litigation substitutes for revolt. Schulze (1980, pp. 76–77, 141) finds that shift for the early modern Empire: after the Peasants' War, peasant conflict was increasingly channeled into litigation before the territorial courts and the *Reichskammergericht*, a "tendential juridification of social conflict" that he dates from the 1526 Reichsabschied and that subjects pursued so persistently that it set the German lands apart from France, England, and Hungary.

The drought-by-guild interaction and the organized-actor pattern were found in these data rather than

specified in advance, so they should be checked on data not used to find them: in a catalog extended beyond this window or region, cities whose guilds already held seats should again record quieter severe-drought years, and any wet-year association should again be carried by rebellions and by guild- and elite-led unrest rather than by the riots and peasant risings a subsistence account would implicate. The closure account of Section 2 needs a longer catalog too. It predicts unrest returning decades after guilds gained their seats, as outsiders shut out of membership and citizenship acquired a grievance of their own; the windows 10 to 30 years after a gain in Figure 3 detect no such rise, but cannot rule out one of up to about 115% of the base rate. The data also bear on several questions beyond this paper.[13]

The debates this paper started from, over why late medieval communities revolted and whether admitting guilds and burghers to government calmed or fed that unrest, have long rested on a few well-archived cities. Linking an event-level record of unrest to an annual record of who governed does not settle them. It does move them onto evidence that can be counted, dated, and checked city by city. Dating alone shows that much of the apparent relationship between representation and unrest belongs to the years in which representation changed, and counting shows that at least one published pattern for the Empire does not reproduce on a catalog built for it.

[13]Three stand out. First, set against matched English and French series over 1350–1400, where a tight post-plague cluster of wage, tax, and war-finance grievances produced compressed revolt waves, the German series should lack a synchronized spike and remain dominated by political grievances, consistent with an agrarian depression that deepened over a century and struck regions very unevenly (Abel, 2006, pp. 66, 81, 86). Second, finer institutional coding, with seat counts in place of the 0–3 bands and the seats indicator and with the formal procedures for removing officeholders, would sharpen tests of mechanism, and the guild results should be re-run under alternative rulings on reserved seats, electoral colleges, and merchant elites. Third, the catalog identifies eleven geocoded episodes of unrest directed at least in part at Jewish communities (online appendix), too few to test formally whether weather shocks predicted such violence, as Anderson et al. (2017) ask at pan-European scale, or whether institutions shaped it at the city-year resolution the matched data allow, as Finley & Koyama (2018) ask across the Empire's territories and Jedwab et al. (2019) across Western Europe.

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