Online Appendix (not for publication)

OA.1 Market grants and economic activity

To validate the market right as an indicator of economic activity, rather than merely a change in the formality of economic institutions that had already existed, we begin by examining the association between city size in 1500 (and, in other specifications, 1400) and market rights granted up to the year 1500 (or 1400). This analysis requires some background discussion. Typically, *urbanization*, rather than city size, has been used to study economic development prior to the era of modern economic growth. However, urbanization rates are unavailable for units smaller than countries prior to 1700. Because we study market establishment at much more disaggregated levels of analysis—indeed, at the city level—we will use the size of a city as a measure of a city's economic development. As noted in the main text of the paper, the data on city size are very limited in their coverage for the medieval period, but they are still able to provide suggestive evidence on the relationship between market establishment and economic development, with city size acting as a proxy for the latter.

We use data collected by Bairoch et al. (1988), containing city size data for 279 German cities across the period 800–1850. To be included in the dataset, cities needed to have populations of size 5,000 or greater by the year 1800. Of these, 128 were large enough in 1500 to have available city size data; 75 were large enough in 1400 to have city size data. We do not know with certainty that cities without data were small; however, the ability to find city size information for a given century is a strong indicator that the city was, indeed, relatively large.

We begin our analysis by studying the relationship between market grants and a dummy variable indicating whether a city has city size data available in the Bairoch dataset in 1500 (or 1400). This will tell us whether cities that were granted more market rights in the 1100–1500 period were, in fact, "big" cities in 1500 (and whether cities granted more market rights in the 1100–1400 period were "big" in 1400). Of course, cities that did not exist were more likely to have no population data and no market grants; however, only 6 of the 279 cities in the Bairoch dataset had not been mentioned in historical documents prior to 1500, and only 7 had not been mentioned prior to 1400.[1] In addition, we have conducted all of our analyses including only cities that had been *incorporated* prior to 1500 (or 1400), and this does not significantly change our findings.

We thus regress the "city size data available" dummy variable on the number of market rights granted, for cities that had previously been mentioned in documents prior to 1500 (or 1400). As can be seen in Table OA.1, column 1, there is a very strong, positive correlation between market grants and being a big enough city to have population data in 1500. The same holds for the year 1400 (see Table OA.1, column 5). We next run the same regressions, but using an indicator of at least one market right granted as the explanatory variable. This is a useful robustness check given the noisiness of the market grant data, and to ensure that our results were not driven by many grants in a particular city. We again find a statistically significant, positive association between receiving a market grant and being a city large enough to have population data in 1500 (see Table OA.1, column 2) or in 1400 (see Table OA.1, column 6). We have estimated all of these specifications controlling for cities' latitude and longitude, and we continue to find a strong, positive association between market grants and the city having population data. Finally, we have estimated the same relationships using probit and logit specifications, and our results are robust to these alternative models as well.

[1] Information on the date a city was first mentioned comes from the *Deutsches Städtebuch*.

Next, we condition on a city having population data in 1500 (or 1400), and examine the relationship between market grants and city size among the 128 cities in Germany with population data in 1500 (or the 75 cities in Germany with population data in 1400). We regress log city size in 1500 on the number of market grants a city received between 1100 and 1500, and we again find a statistically significant, positive association (see Table OA.1, column 3); when we examine city size in 1400 and market establishment between 1100–1400, the relationship is positive, though it is no longer significant (see Table OA.1, column 7). We then regress city size in 1500 (or 1400) on an indicator that a city received at least one market right between 1100 and 1500 (or 1100 and 1400), and find again a strong, positive relationship between market rights and city size (see Table OA.1, column 4 for the year 1500 results and Table OA.1, column 8 for the year 1400 results). As was the case in our study of cities with and without population data, our results are robust to including latitude and longitude controls.

While the analysis above can only be suggestive, it does indicate a robust association between market rights granted and economic development at the city level in medieval Germany.

Table OA.1: City sizes and market grants

Dependent variable:	City size in 1500				City size in 1400			
	Any size reported		log(size)		Any size reported		log(size)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total markets granted	0.200***		0.176*		0.251***		0.106	
	[0.0449]		[0.106]		[0.0514]		[0.132]	
Any markets granted		0.601***		0.610***		0.514***		0.468**
		[0.0400]		[0.156]		[0.0822]		[0.212]
Observations	273	273	128	128	272	272	75	75
R^2	0.103	0.185	0.045	0.104	0.120	0.141	0.014	0.058

*: Significant at 10%; **: 5%; ***: 1%. Each column shows estimates from a cross-sectional regression of a measure of city population on that city's level of market establishment in the Middle Ages; all regressions include a constant term (not reported). Measures of city size are either a dummy variable indicating whether a city had population data in 1500 (or 1400) available in the Bairoch dataset (columns 1, 2, 5, and 6) or, conditional on having city size data in 1500 (or 1400), the log of the city's population (columns 3, 4, 7, and 8). City size data come from Bairoch et al. (1988) and market grants data were collected by the authors from the *Deutsches Städtebuch*. All cities included in these regressions had populations of size 5,000 or greater by the year 1800, and thus are included in Bairoch dataset. Regression estimates in columns 1–2 and 5–6 are based on the sample of cities that had been mentioned in documents prior to 1500, or 1400, respectively (to ensure that the absence of city size data) is not a mechanical result of no city existing). Regression estimates in columns 3–4, and columns 7–8, are based on the sample of cities with city size data available in the Bairoch dataset in 1500, and in 1400, respectively. Robust standard errors in brackets.

In addition to examining the relationship between market grants and city size, we examined historical sources to determine whether the granting of a market right was associated with other observable indicators of economic activity in the historical record (we discuss this in the main text as well). We identified several useful sources; first, the *Deutsches Städtebuch* and the *Handbuch der historischen Stätten Deutschlands* (Klose, ed, 1958–) provide descriptions of notable new construction in each German city. Some of these are plausibly directly linked to the establishment of a functioning market: for example, construction or fortification of city walls, construction of customs houses, and merchants' halls. Other construction might have been supported by increased trade, and increased tax revenues derived from that trade: for example, the construction of a church, or a city hall.

We examined historical construction records for the cities receiving the 80 market grants and/or city incorporations between 1386 and 1406, and find that 38 of them experienced some notable construction within 20 years of the year of the market grant or city incorporation (in Table OA.2, we list the cities; column 4 indicates with an "X" which of these experienced notable construction). As we discuss in the main text, the city of Bacharach received a market grant in 1402. Around that time, a customs house was built and the city walls were extended to accommodate it. Then, five years after the market grant, a new city hall was built on the market square. Seventeen years after the market right was granted, a new merchants' hall was constructed. The city of Kulmbach received a market grant in 1397; that same year was the first time a moat around the city was mentioned, and in 1398, a city hall and a merchant hall were mentioned for the first time. Petershagen received a market grant in 1399; 20 years later, the nearby bridge over the Weser River needed to be expanded. Overall, it appears that a great deal of new construction activity followed the granting of a medieval market right, suggesting real effects of the grant.

We also randomly selected 80 "comparison" cities that did not receive a market grant or city incorporation between 1386 and 1406, and searched for evidence of construction activity in these cities between 1386 and 1426 (a conservative, 40-year time window). We found evidence of construction in only 23 of these 80 cities—the difference in construction activity between cities receiving market grants within a twenty-year window and control cities within a forty-year window is statistically significant at the 5% level.[2] This suggests that the construction activity occurring in the cities receiving market grants truly was linked to the market establishments themselves.

We also consulted a report on the markets existing in Germany in 1936, *Verzeichnis der Märkte und Messen im Deutschen Reich im Jahre 1936* (Statistisches Reichsamt, ed, 1935) and matched the markets in the report to the 63 market establishments in our dataset between 1386 and 1406. We find that 60 of the cities receiving market grants from 1386–1406 had functioning markets in 1936 (see Table OA.2). Among these 60 cities, the *Deutsches Städtebuch* provided information on the frequency of the market, and/or the goods traded there, for 50 of their medieval market grants. Of these 50 grants, we are able to successfully match 39 of them *across 500 years* on at least one market characteristic (frequency or goods traded), with no mismatch; moreover, in 14 cases, we find that the market existing in 1936 exactly matches the 14th (or early 15th) century market grant in *both* the frequency and type of market (see Table OA.2). These findings indicate that market grants to small towns were not formalities, but rather produced functioning markets. The small towns receiving these grants continued to have functioning markets over 500 years later, and frequently their medieval markets persisted into the 20th century.

Thus, based on a range of historical evidence, we are confident that the granting of a market right generally indicates increased commercial activity.[3]

[2]When limiting the window of analysis for the comparison cities to the 20 year period 1386–1406, one finds 19 instances of construction activity; the difference between this level of activity and that for the treatment cities is statistically significant at 1%.

[3]As we also note in the main text, we are not the first to treat the granting of a market privilege as an event marking market establishment. The work of historians of medieval Europe studying such grants supports the view that royal charters were often associated with the actual creation of new markets, and were not simply the formal recognition of existing ones. Bindseil and Pfeil (1999, pp. 739-740) write that "The setting up of a marketplace became a legal privilege of the German King in the 9th century, implying the need of a deed of foundation for every market." Britnell (1981, p. 211) and Masschaele (2002) discuss the case of England; Epstein (2000) uses legal documents as indicators of economic activity in Italy.

Table OA.2: Cities incorporated and/or granted markets, 1386–1406

City	State	Year	Incorporation	Market grant	Market in 1936	Markets matched?	Construction
Panel A: Cities incorporated, 1386–1406							
Abensberg	Bayern	1401	X		–	–	
Altdorf	Bayern	1387	X		–	–	X
Bad Liebenzell	Württemberg	1388	X		–	–	
Boxberg	Baden	1388	X		–	–	
Breckerfeld	Westfalen	1396	X		–	–	X
Hammerstein	Pommern	1395	X		–	–	X
Hattingen	Westfalen	1396	X		–	–	X
Hirschberg	Thüringen	1397	X		–	–	
Hirschhorn	Hessen	1391	X		–	–	
Kölleda	Sachsen-Anhalt	1392	X		–	–	X
Otterndorf	Niedersachsen	1400	X		–	–	
Plettenberg	Westfalen	1397	X		–	–	X
Scheinfeld	Bayern	1405	X		–	–	X
Treuen i. V.	Sachsen	1390	X		–	–	
Ummerstadt	Thüringen	1394	X		–	–	
Veringenstadt	Württemberg	1393	X		–	–	
Wächtersbach	Hessen	1404	X		–	–	X
Panel B: Cities incorporated and granted markets, 1386–1406							
Alzenau	Bayern	1401	X	X		no	
Aub	Bayern	1404	X	X	X	yes	
Gaildorf	Württemberg	1404	X	X	X	yes	X
Groß-Gerau	Hessen	1398	X	X	X	likely	
Hadmersleben	Sachsen-Anhalt	1390	X	X	X	.	
Thiersheim	Bayern	1398	X	X	X	no	X
Panel C: Cities granted markets, 1386–1406							
Aalen	Württemberg	1398		X	X	yes	X
Auerbach i.d.Opf.	Bayern	1397		X	X	yes	X
Bühl	Baden	1403		X	X	likely	
Bacharach	Rheinland-Pfalz	1403		X	X	no	X
Besigheim	Württemberg	1405		X	X	likely	X
Bischofswerda	Sachsen	1406		X	X	likely	
Bogen	Bayern	1389		X	X	yes	
Burgsteinfurt	Westfalen	1406		X	X	likely	X
Esslingen/Neckar	Württemberg	1388		X	X	.	X
Fraustadt	Schlesien	1404		X	X	likely	
Freystadt	Bayern	1393		X	X	yes	
Friedberg	Bayern	1404		X	X	yes	X

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Table OA.2: *(continued)*

City	State	Year	Incorporation	Market grant	Market in 1936	Markets matched?	Construction
Gerbstedt	Sachsen-Anhalt	1404		X	X	yes	X
Gunzenhausen	Bayern	1401		X	X	likely	
Hirschhorn	Hessen	1404		X	X	no	X
Ichenhausen	Bayern	1406		X	X	.	
Ingolstadt	Bayern	1395		X	X	yes	
Külsheim	Baden	1405		X	X	likely	
Kelheim	Bayern	1399		X	X	likely	
Kulmbach	Bayern	1398		X	X	likely	X
Lüchow	Niedersachsen	1398		X	X	.	
Lauchheim	Württemberg	1402		X	X	likely	
Liebenau	Hessen	1393		X	X	no	X
Limburg	Hessen	1403		X	X	likely	
Lörrach	Baden	1403		X	X	no	X
Mainburg	Bayern	1397		X	X	likely	
Meppen	Westfalen	1387		X	X	no	
Merkendorf	Bayern	1398		X	X	yes	X
Meschede	Niedersachsen	1399		X	X	no	
Moringen	Niedersachsen	1390		X	X	likely	
Neumarkt	Schlesien	1387		X	X	.	X
Neustadt	Rheinland-Pfalz	1404		X	X	no	
Neustadt a.d.Waldnaab	Bayern	1387		X		no	
Petershagen	Westfalen	1400		X	X	.	X
Philippsburg	Baden	1402		X	X	no	
Pirna	Sachsen	1392		X	X	likely	X
Pleystein	Bayern	1391		X	X	yes	
Pressath	Bayern	1398		X	X	likely	
Radevormwald	Rheinland	1400		X	X	likely	
Rain	Bayern	1397		X	X	no	X
Rastatt	Baden	1404		X	X	.	
Regensburg-Stadtamhof	Bayern	1389		X	X	yes	
Rinteln	Westfalen	1392		X	X	likely	X
Roth b. Nürnberg	Bayern	1392		X	X	likely	X
Rothenburg o.d. Tauber	Bayern	1406		X	X	yes	X
Rottenburg a.d. Laaber	Bayern	1393		X	X	likely	X
Rottweil	Württemberg	1397		X	X	likely	X
Sömmerda	Sachsen-Anhalt	1389		X	X	.	X
Scheßlitz	Bayern	1395		X	X	.	X
Schweinfurt	Bayern	1397		X	X	likely	X
Solingen-Gräfrath	Rheinland	1402		X		no	
Soltau	Niedersachsen	1388		X	X	.	
Thum	Sachsen	1407		X	X	no	
Vilsbiburg	Bayern	1401		X	X	likely	X

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Table OA.2: *(continued)*

City	State	Year	Incorporation	Market grant	Market in 1936	Markets matched?	Construction
Vilseck	Bayern	1396		X	X	likely	X
Volkach	Bayern	1406		X	X	likely	X
Weißenhorn	Bayern	1387		X	X	yes	

Table includes cities incorporated and/or granted market rights between 1386 and 1406 (incorporations and market grants taken from the *Deutsches Städtebuch*). For each city, it is first indicated whether incorporation or the granting of a market right (or both) occurred between 1386 and 1406, along with the date when this event (or these events) occurred. Next, for cities that received a market grant between 1386 and 1406, we indicate whether *Verzeichnis der Märkte und Messen im Deutschen Reich im Jahre 1936* identifies a market in that city in 1936. If information on the timing of the medieval market or the goods traded at the market (or both) is available in the *Deutsches Städtebuch*, the table next shows whether the medieval market and the 1936 market match: "yes" indicates a match on both timing and goods traded; "likely" indicates a match on one characteristic and no mismatch on timing or goods traded; "no" indicates a discrepancy between the medieval market and the 1936 market in timing or goods traded (or the non-existence of a market in 1936); a dot indicates that no information on timing or goods traded is available for the medieval market. Finally, the table indicates whether the city experienced a significant construction event within 20 years of its incorporation or receiving a market grant (information on construction activity comes from the *Deutsches Städtebuch* and the *Handbuch der historischen Stätten Deutschlands*).

OA.2 Universities and urbanization across Europe

We argue in the main text of the paper that our analysis of the impact of German universities on market establishment can speak to the larger question of the causal effect of universities on the Commercial Revolution across Europe. Although the establishment of Europe's universities was generally endogenous with respect to economic activity (as we discuss in the main text), it remains of interest to examine the (non-causal) relationship between universities and economic development across countries in medieval Europe.

To study the relationship between universities and economic development, we use data from Buringh and van Zanden (2009) on the number of universities and the urbanization rate in each European "country", in each century, from 1200–1500.[4] We first present the scatterplot of urbanization against the number of universities, century by century (see Figure OA.1). In each century, there appears to be a positive association between the number of universities in a country and its urbanization rate. One can see that Belgium stands out as a clear outlier, having a very high urbanization rate, but no universities until the University of Louvain was established in 1425 (the Netherlands are an outlier as well in the later centuries). Without Belgium, the correlation of urbanization and the number of universities is strikingly high, being close to 0.8 in most periods considered. Of course, Belgian (and Dutch) students could – and did – attend universities in nearby France and Germany. Even with these outliers, the general association between universities and economic development during the Commercial Revolution is quite clear.

We then estimate cross-sectional regressions of urbanization on the number of universities, century-by-century, for the 1200–1500 period. In Table OA.3, Panel A, one can see that the relationship is positive in 3 of 4 regressions, and statistically significantly so in 2 of them. When we remove Belgium from the regression, all 4 regressions show positive relationships between universities and urbanization, 3 of them statistically significant (see Table OA.3, Panel B).[5]

It is important to emphasize that these associations should *not* be interpreted as causal – they merely indicate that the European countries that had the most universities were also the most urbanized throughout the Middle Ages. Our goal in studying Germany in the late 14th century is to exploit a case of plausibly exogenous variation in the existence of universities to identify the causal role that universities played in economic development in medieval Europe.

[4]Note that as in the main text, the urbanization rate is calculated as the fraction of the population in the country living in cities with populations 10,000 or larger.

[5]Examining the relationship between universities and urbanization in 1100 (with or without Belgium) yields a highly significant, positive relationship. The only country with a university, Italy, has the highest urbanization rate in the sample.

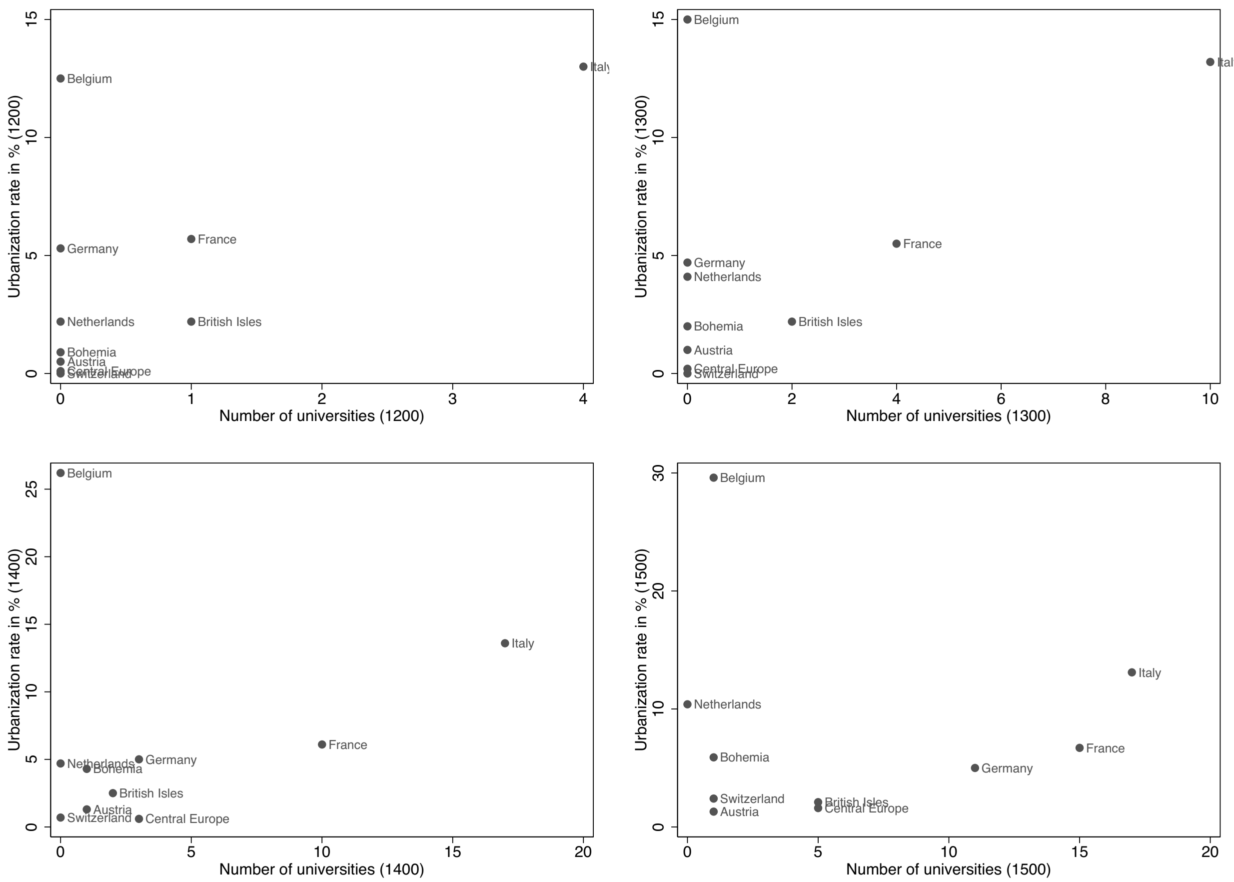

Figure OA.1: Urbanization rates and number of universities, 1200–1500.

Table OA.3: Urbanization rates and number of universities, 1200–1500

Dependent variable:	Urbanization rate (%)			
	1200	1300	1400	1500
	(1)	(2)	(3)	(4)
Panel A: Full sample				
Num. of universities	2.393***	0.866**	0.286	-0.0255
	[0.554]	[0.272]	[0.374]	[0.395]
Observations	10	10	10	10
R^2	0.380	0.285	0.040	0.000
Panel B: Excluding Belgium				
Num. of universities	2.855***	1.091***	0.614***	0.308
	[0.258]	[0.116]	[0.111]	[0.229]
Observations	9	9	9	9
R^2	0.809	0.820	0.760	0.234

*: Significant at 10%; **: 5%; ***: 1%. Each column shows estimates from a cross-sectional regression of the "country"-level urbanization rate on the number of universities. All regressions include a constant term (not reported). Data on the number of universities and the urbanization rate in each European "country" come from Buringh and van Zanden (2009). Robust standard errors in brackets.

OA.3 German University Graduates Before and After 1386: Evidence from the Repertorium Academicum Germanicum

As noted in the main text, we believe that the number university students, rather than graduates, is a better measure of the human capital being produced in the medieval universities. Still, available data on university graduates provide an alternative measure of university training that can complement the matriculation record data that we focus on in the main text. The *Repertorium Academicum Germanicum* (RAG) database includes a great deal of information on German university graduates in the Middle Ages, though the database's search functionality is still being developed.[6] We searched the RAG database for all German university graduates between 1366 and 1385 (all from foreign universities), and find 877 graduates; the same search for graduates between 1387 and 1406 yields 1,623 graduates.[7] The data on German graduates from the RAG database corroborates the evidence on university students from matriculation records.

To provide additional evidence on the change in the number of Germans trained in law following the establishment of Germany's first universities, we can again turn to the RAG database to estimate how many German law *graduates* there were before and after 1386. These individuals were very much the elite—Wieacker (1995, p. 119) notes that, "Imperial law placed the legal doctor on a par with the knight." We searched the RAG database for all German university law graduates between 1366 and 1385 (all of whom attended foreign universities), and find 72 graduates; the same search for graduates between 1387 and 1406 yields 233 graduates.[8] The RAG data show that while the number of German university graduates nearly doubled after 1386, the number of graduates in law more than *tripled*.

[6] The RAG database is online at `http://www.rag-online.org/`, and contains the biographies of scholars from the Holy Roman Empire from 1250 until 1550.

[7] To be precise, to conduct this search, we specified the range of years in which degrees were granted, and included an asterisk (a "wild card") under the graduate's last name. Other search parameters yield different numbers, but the same pattern of a large increase in graduates after 1386. The search engine is still being perfected and currently does not allow for a sharp distinction of Boolean searches of the "and" and the "or" type (personal correspondence with the administrators of the RAG, 2013/05/14).

[8] To be precise, to conduct this search, we specified the range of years in which degrees were granted, required that degrees be in law ("*jur*") and included an asterisk (a "wild card") under the graduate's last name. Other search parameters yield different numbers, but the same pattern of a large increase in graduates in law after 1386.

OA.4 Units of analysis and clustering city-year level regressions at the territory level

As noted in the text, our choice of city-year as the unit of analysis in our panel regressions raises concerns about the statistical inferences we make (i.e., how many independent observations we have) and general equilibrium effects (i.e., the possibility that units' interdependence biases our coefficients). In this section we replicate our panel regressions of Tables IV through VI, but using different units of analysis, or clustering our standard errors at different levels.

We first, in Table OA.4, present results from estimating specifications presented in Tables IV through VI, but using territory-year as the unit of analysis. One can see that our results are very similar to those presented in the main text.

Next, we estimate the same specifications, but using cell-year as the unit of analysis. One can see that our results are again similar to those presented in the main text (see Table OA.5).

Finally, we estimate the specifications presented in Tables IV through VI using city-year data as in the text, but clustering our standard errors at the territory level to account for possibly correlated error terms across observations in an entire territory, across space or time. One can see that the magnitudes of the standard errors and our statistical inferences are largely unchanged using this specification (see Table OA.6).

Table OA.4: Panel regressions, territorial lord-year level

Dependent variable:	Rate of market establishment									
Equivalent to:	TIV, col. 2	TIV, col. 4	TIV, col. 6	TV, col. 2	TV, col. 4	TV, col. 5	TV, col. 6	TVI, col. 2	TVI, col. 4	TVI, col. 6
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Year	0.090	0.135	0.086	0.131	0.034	0.372		0.087	0.087	0.092
	[0.127]	[0.126]	[0.126]	[0.133]	[0.175]	[0.288]		[0.126]	[0.130]	[0.127]
Post1386	-1.077	-1.825	-1.103	-1.347	0.762	0.722		-1.036	-1.046	-1.198
	[2.234]	[2.189]	[2.222]	[2.262]	[3.281]	[7.699]		[2.210]	[2.286]	[2.248]
Year $\times$ Post1386	-0.140	-0.200	-0.129	-0.298	-0.115	-0.489		-0.136	-0.156	-0.129
	[0.207]	[0.214]	[0.206]	[0.201]	[0.333]	[1.291]		[0.205]	[0.212]	[0.208]
ΔDistUniv	-0.811	-1.422	-0.803	-1.535	-0.762	-0.808	-0.125	-0.807	-0.763	-0.844
	[1.048]	[0.954]	[1.035]	[0.965]	[1.428]	[1.030]	[1.828]	[1.031]	[1.101]	[1.069]
ΔDistUniv $\times$ Year	-0.061	-0.092	-0.056	-0.094	-0.037	-0.094	-0.034	-0.058	-0.061	-0.063
	[0.062]	[0.061]	[0.061]	[0.061]	[0.077]	[0.065]	[0.091]	[0.061]	[0.065]	[0.063]
ΔDistUniv $\times$ Post1386	0.004	0.684	0.006	0.440	-0.711	-0.215	-1.597	-0.238	-0.265	0.123
	[1.091]	[1.062]	[1.076]	[1.035]	[1.401]	[1.551]	[2.163]	[1.030]	[1.101]	[1.120]
ΔDistUniv $\times$ Year $\times$ Post1386	0.200*	0.252**	0.191*	0.291**	0.189	0.237	0.349	0.201*	0.210*	0.188*
	[0.111]	[0.127]	[0.109]	[0.119]	[0.146]	[0.228]	[0.230]	[0.106]	[0.114]	[0.113]
Constant	3.279	3.950*	3.275	4.332*	3.287	3.277	0.148	3.28	3.341	3.351
	[2.322]	[2.286]	[2.310]	[2.422]	[3.467]	[2.307]	[2.161]	[2.308]	[2.388]	[2.337]
Window (years)	1386 ± 20									
Observations	20680	19200	20760	18600	17480	20880	20880	20800	18120	19920
Number of cities	2220	2036	2100	2025	1759	2256	2256	2249	2084	2033

*: Significant at 10%; **: 5%; ***: 1%. Robust standard errors in brackets, clustered at the lord level. Cf. notes to the equivalent tables in the main paper. The dependent variable is computed as market establishments per year per 1,000 cities in the region considered.

Table OA.5: Panel regressions, latitude/longitude cells-year level

Dependent variable:	Rate of market establishment									
Equivalent to:	TIV, col. 2	TIV, col. 4	TIV, col. 6	TV, col. 2	TV, col. 4	TV, col. 5	TV, col. 6	TVI, col. 2	TVI, col. 4	TVI, col. 6
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Year	0.020	0.035	0.024	0.026	0.077	-0.063		0.018	0.016	0.027
	[0.051]	[0.057]	[0.051]	[0.053]	[0.136]	[0.259]		[0.051]	[0.050]	[0.051]
Post1386	0.391	0.205	0.781	0.259	-0.108	10.627		0.385	0.430	0.198
	[1.420]	[1.427]	[1.805]	[1.444]	[4.219]	[9.335]		[1.417]	[1.405]	[1.428]
Year × Post1386	-0.071	-0.095	-0.111	-0.102	-0.040	-0.384		-0.070	-0.067	-0.060
	[0.094]	[0.108]	[0.131]	[0.099]	[0.248]	[0.703]		[0.094]	[0.093]	[0.100]
ΔDistUniv	-0.370	-0.612	-0.361	-0.545*	-0.954	-0.364	-0.937*	-0.364	-0.406	-0.466**
	[0.257]	[0.395]	[0.279]	[0.292]	[0.599]	[0.251]	[0.547]	[0.250]	[0.256]	[0.224]
ΔDistUniv × Year	-0.033	-0.057	-0.036	-0.035	-0.051	-0.018	-0.094**	-0.030	-0.031	-0.045**
	[0.023]	[0.048]	[0.027]	[0.030]	[0.051]	[0.048]	[0.045]	[0.022]	[0.022]	[0.022]
ΔDistUniv × Post1386	-0.223	0.030	-0.408	-0.119	-0.112	-1.787	-0.916	-0.298	-0.220	-0.132
	[0.599]	[0.638]	[0.759]	[0.597]	[1.580]	[1.691]	[1.666]	[0.584]	[0.591]	[0.589]
ΔDistUniv × Year × Post1386	0.099*	0.146	0.117*	0.110	0.083	0.142	0.376**	0.095*	0.097*	0.110**
	[0.052]	[0.102]	[0.069]	[0.073]	[0.099]	[0.132]	[0.160]	[0.049]	[0.050]	[0.051]
Constant	1.574***	1.718***	1.595***	1.859***	3.459**	1.572***	0.401*	1.574***	1.508***	1.679***
	[0.566]	[0.597]	[0.588]	[0.620]	[1.564]	[0.564]	[0.211]	[0.564]	[0.558]	[0.560]
Window (years)	1386 ± 20									
Observations	3200	3200	3200	3200	2080	3200	3200	3200	3200	3160
Number of cities	2220	2036	2100	2025	1759	2256	2256	2249	2084	2033

*: Significant at 10%; **: 5%; ***: 1%. Robust standard errors in brackets, clustered at the level of latitude/longitude cells. Cf. notes to the equivalent tables in the main paper. The dependent variable is computed as market establishments per year per 1,000 cities in the region considered.

Table OA.6: Panel regressions, city-year level, standard errors clustered at territorial lord level

Dependent variable:	Rate of market establishment									
Equivalent to:	TIV, col. 2	TIV, col. 4	TIV, col. 6	TV, col. 2	TV, col. 4	TV, col. 5	TV, col. 6	TVI, col. 2	TVI, col. 4	TVI, col. 6
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Year	-0.002	-0.008	0.012	-0.014	-0.021	-0.141		-0.004	-0.002	0.011
	[0.077]	[0.078]	[0.075]	[0.080]	[0.136]	[0.312]		[0.076]	[0.077]	[0.077]
Post1386	1.729	1.400	1.141	1.704	3.608	16.933**		1.659	1.765	1.498
	[1.274]	[1.297]	[1.127]	[1.333]	[2.344]	[6.788]		[1.246]	[1.276]	[1.275]
Year $\times$ Post1386	-0.088	-0.058	-0.080	-0.086	-0.119	-0.673		-0.089	-0.098	-0.099
	[0.139]	[0.142]	[0.144]	[0.143]	[0.238]	[0.586]		[0.137]	[0.139]	[0.140]
ΔDistUniv	-0.167	-0.374	-0.122	-0.300	-0.333	-0.173	-0.231	-0.173	-0.212	-0.400
	[0.422]	[0.461]	[0.459]	[0.466]	[0.588]	[0.406]	[0.827]	[0.406]	[0.420]	[0.367]
ΔDistUniv $\times$ Year	-0.035	-0.025	-0.031	-0.023	-0.027	-0.015	-0.065	-0.033	-0.035	-0.052
	[0.037]	[0.040]	[0.038]	[0.040]	[0.054]	[0.058]	[0.071]	[0.036]	[0.038]	[0.036]
ΔDistUniv $\times$ Post1386	-0.926	-0.594	-0.781	-0.788	-1.589*	-2.896**	-2.325*	-0.989*	-0.904	-0.733
	[0.585]	[0.632]	[0.584]	[0.643]	[0.881]	[1.136]	[1.231]	[0.550]	[0.569]	[0.569]
ΔDistUniv $\times$ Year $\times$ Post1386	0.137**	0.111	0.124*	0.122*	0.149	0.211*	0.322**	0.136**	0.137**	0.158**
	[0.066]	[0.070]	[0.072]	[0.071]	[0.094]	[0.109]	[0.153]	[0.063]	[0.065]	[0.066]
Constant	1.513*	1.687*	1.543*	1.678*	2.062	1.529*	0.269	1.532*	1.545*	1.797**
	[0.850]	[0.868]	[0.885]	[0.884]	[1.453]	[0.840]	[0.962]	[0.841]	[0.847]	[0.833]
Window (years)	1386 ± 20									
Observations	88800	81440	84000	81000	70360	90240	90240	89960	83360	81320
Number of cities	2220	2036	2100	2025	1759	2256	2256	2249	2084	2033

*: Significant at 10%; **: 5%; ***: 1%. Robust standard errors in brackets, clustered at the lord level. Cf. notes to the equivalent tables in the main paper. The dependent variable is computed as market establishments per year per 1,000 cities in the region considered.

OA.5 *Above/below median split sample results for spatial endogeneity, robustness results, and placebo regressions.*

Our baseline results in Table III indicated that Germany experienced a break in the trend rate of market establishment in 1386; that this break was concentrated in cities with a change in distance to a university in 1386 greater than the median; and, we saw that in panel regressions, there was generally a significantly greater trend break in places with larger reductions in distance to a university in 1386.

Because our panel regression contained much of the information presented in the split sample results, we omitted many of the latter from the main text. Here, we present the equivalent split sample regressions for the time series specifications in Tables IV through VI.

One can see that our time series evidence, showing a break in the trend rate of market establishment for all of Germany, is indeed driven by trend breaks specifically in areas with changes in distance to a university in 1386 that were greater than the median (see Table OA.7). This is true across all specifications, providing further evidence in support of our hypothesis that increased access to universities after 1386 significantly affected economic activity.

Moreover, in Figures OA.2 and OA.3 we report the same placebo analysis of Figure IX (examining the effect of varying the year defining the $Post_t$ dummy from 1376 to 1396) separately for the samples of cities above and below median $\Delta DistUniv_i$. Again, one can see that there is a break in trend concentrated on the years around 1386 for the cities with a large change in distance to a university, but no significant break in trend for any of the years 1376–1396 in the sample of cities below median $\Delta DistUniv_i$.

Table OA.7: Aggregate time series regressions, split sample (above and below median $\Delta DistUniv$)

Dependent variable:	Rate of market establishment							
Equivalent to:	TIV, col. 1	TIV, col. 3	TIV, col. 5	TV, col. 1	TV, col. 3	TVI, col. 1	TVI, col. 3	TVI, col. 5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Below median $\Delta DistUniv$								
Year	-0.025	-0.025	-0.007	-0.026	-0.045	-0.025	-0.027	-0.025
	[0.059]	[0.059]	[0.061]	[0.061]	[0.095]	[0.059]	[0.059]	[0.061]
Post1386	0.933	0.937	0.386	1.065	1.938	0.888	1.021	0.962
	[1.152]	[1.158]	[1.119]	[1.209]	[1.899]	[1.152]	[1.157]	[1.188]
Year × Post1386	0.015	0.015	0.017	-0.002	0.034	0.011	0.018	0.016
	[0.097]	[0.098]	[0.097]	[0.104]	[0.155]	[0.097]	[0.099]	[0.100]
Constant	1.470**	1.476**	1.506**	1.525**	1.922*	1.472**	1.436**	1.515**
	[0.684]	[0.687]	[0.702]	[0.710]	[1.105]	[0.685]	[0.663]	[0.705]
Window (years)	1386 ± 20							
Observations	40	40	40	40	40	40	40	40
N. of cities	1128	1123	1104	1087	668	1126	1103	1094
Panel B: Above median $\Delta DistUniv$								
Year	-0.099	-0.076	-0.076	-0.079	-0.100	-0.097	-0.098	-0.131**
	[0.062]	[0.053]	[0.063]	[0.053]	[0.063]	[0.061]	[0.058]	[0.064]
Post1386	-0.689	-0.121	-0.756	-0.275	-0.791	-1.036	-0.633	-0.476
	[0.988]	[0.910]	[1.065]	[0.891]	[1.006]	[0.957]	[1.003]	[0.944]
Year × Post1386	0.286***	0.241***	0.248***	0.236***	0.297***	0.289***	0.261***	0.327***
	[0.084]	[0.077]	[0.089]	[0.080]	[0.087]	[0.080]	[0.081]	[0.095]
Constant	0.974	0.628	1.157	0.819	1.013	0.984	0.907	0.706
	[0.807]	[0.658]	[0.884]	[0.659]	[0.811]	[0.788]	[0.831]	[0.631]
Window (years)	1386 ± 20							
Observations	40	40	40	40	40	40	40	40
N. of cities	1092	913	996	938	1091	1123	981	939

*: Significant at 10%; **: 5%; ***: 1%. Robust standard errors in brackets. Cf. notes to the equivalent tables in the main paper. The dependent variable is computed as market establishments per year per 1,000 cities in the region considered.

Figure OA.2: Changes in the trend rate of market establishment (coefficient on $Year_t \cdot Post_t$) under varying pivot years, 1376–1396, examining only cities with below-median values of $\Delta DistUniv$; for each year between 1376 and 1396, we test for a trend break in that specific year, examining the 20 years before and after that year, as in the specification estimated in Table III, column 2.

Figure OA.3: Changes in the trend rate of market establishment (coefficient on $Year_t \cdot Post_t$) under varying pivot years, 1376–1396, examining only cities with above-median values of $\Delta DistUniv$; for each year between 1376 and 1396, we test for a trend break in that specific year, examining the 20 years before and after that year, as in the specification estimated in Table III, column 3.

OA.6 Robustness of the Empirical Results

We now examine whether our baseline results in Table III are robust to alternative specifications. We first consider changing the window of time around 1386 that we examine. In Table OA.8, columns 1 and 2, we replicate our estimates in Table III, columns 1 and 4, but consider a ±15 year window, rather than a ±20 year window. We find even stronger results using this narrower window than in the baseline specification. If a time window of ±25 years is considered instead, one finds a positive (but statistically insignificant) break in the trend rate of market establishment, and a small, positive (but insignificant) coefficient on $\Delta DistUniv_i \cdot Year_t \cdot Post_t$ (Table OA.8, columns 3 and 4).

These results, especially the strong results for the narrower time window, are reassuring in their qualitative similarity to our findings in Table III. In interpreting the weaker results in columns 3 and 4, it is worth keeping in mind that we model market establishment across time using *linear* trends (and breaks in trends). As the time window under consideration becomes wider, it is more likely that other economic shocks are captured in the data, and that our linear approximation of trend rates of economic activity (and breaks thereof) is less appropriate.[9]

Still we can estimate a version of our time series model across a longer time period, controlling for smooth changes in underlying economic activity using higher-order polynomials. Our regression equation will be equivalent to the simple time-series setup of equation (4), with the addition of higher-order terms in $Year_t$ and higher-order interaction terms in $Year_t \cdot Post_t$:

$$markets_t = \beta_0 + \sum_{k=1}^{K} \beta_{1,k} \cdot Year_t^k + \beta_2 \cdot Post_t + \sum_{k=1}^{K} \beta_{3,k} \cdot Year_t^k \cdot Post_t + \eta_t, \qquad \text{(OA.1)}$$

where $K = 1$ is equivalent to the linear approximation setup of equation (4), $K = 2$ is equivalent to an approximation of time trends with a quadratic polynomial, $K = 3$ to a cubic polynomial, etc. Additionally, with the variable $Year_t$ normalized to equal 0 in 1386, the (local) trend break in 1386 can be easily represented by the coefficient on the interaction term $Year_t \cdot Post_t$, $\beta_{3,1}$.[10] Our model of the impact of new universities on human capital and market establishment will still predict a sharp, *local* change in the trend rate of market establishment in 1386; thus, we expect a significant, positive coefficient on $Year_t \cdot Post_t$.[11]

We first estimate the (time series) model in Table OA.8, column 3, which examines a ±25 year window around 1386, but now we control for economic activity using a quadratic time trend (which may change post-1386). In Table OA.9, column 1, one can see that controlling for a quadratic trend over the longer window, there is a statistically significant, positive (local) trend break in market establishment in 1386. In column 2, we increase the size of the window to ±50 years, and control for underlying activity using a cubic polynomial (again, allowing the coefficients on the polynomial to change post-1386). Again, we find a significant, positive (local) trend break in market establishment in 1386. Finally, in columns 3 and 4, we increase the size of the window to ±75 and ±100 years respectively, and again control for underlying activity using a cubic polynomial; again we find significant,

[9] In fact, the R-squared is decreasing in the size of the window considered: in Table OA.8, column 1 (±15 year window), it is 0.167; in Table III, column 1 (the ±20 year window), it is 0.098; in Table OA.8, column 3 (±25 year window), it is only 0.034.

[10] Formally, $\beta_{3,1}$ is the difference in slopes between the polynomial on the left side of 1386 and the polynomial on the right side of 1386, evaluated at $Year_t = 0$ (i.e., 1386).

[11] Admittedly, this exercise can only be suggestive, as the coefficient on $Year_t \cdot Post_t$ is being estimated using variation that is not just local variation around 1386.

positive trend breaks in 1386.

We next consider the robustness of our baseline results to varying the definition of our outcome variable. In many cases, the incorporation of a city was explicitly linked to the creation of a market; if evidence of both could be found in the *Städtebuch*, this will be reflected both in our city incorporations and our market grants data. But in some cases, no explicit mention of markets is made in the *Städtebuch* when a city incorporation is reported; to the extent that the city charters implicitly included the rights to hold a certain number of markets or fairs, therefore, our market grant data may underestimate the actual number of new markets.

To check whether this ambiguity affects our results, we estimate our baseline specifications (Table III, columns 1 and 4), but use the sum of the market establishments and city incorporations in a given year (or city-year) as the outcome variable. The results in Table OA.10, columns 1 and 2, again show a significant break in the trend rate of market establishment in 1386, and a greater positive trend break in areas with greater reductions in distance to a university. At the same time, there is now a marginally significant negative pre-1386 trend associated with distance to a university. This could raise concerns to the extent that one would like areas with greater reductions in distance to a university to be identical to areas with less. In practice, the negative trend in city incorporation (recall there was no significant trend in market establishment itself) likely biases results against our hypothesis.[12]

The baseline results might also be biased by a few instances of multiple market grants to a city (perhaps with high values of $\Delta DistUniv$) in a single year. In Table OA.10, columns 3 and 4, we thus estimate our baseline specifications using an indicator of *any* market establishment (computed as a rate per 1,000 cities) as our outcome, rather than the total number of markets established in a city-year. Our estimated coefficients are slightly smaller, but we continue to see a positive, highly statistically significant break in trend that is greatest in areas with large reductions in distance to a university.

[12] If areas with high $\Delta DistUniv$ had fewer cities established just before 1386, this may have meant fewer places where markets would then be granted after 1386.

Table OA.8: Different windows of analysis (linear time trends)

Dependent variable:	Rate of market establishment			
	Time series	Panel, city level	Time series	Panel, city level
	(1)	(2)	(3)	(4)
Year	-0.192*	-0.047	-0.035	-0.025
	[0.095]	[0.092]	[0.035]	[0.055]
Post1386	0.933	2.51	0.368	1.335
	[0.843]	[1.629]	[0.761]	[1.359]
Year × Post1386	0.274**	-0.106	0.064	0.006
	[0.122]	[0.165]	[0.050]	[0.095]
ΔDistUniv		-0.454		0.04
		[0.426]		[0.384]
ΔDistUniv × Year		-0.082		-0.006
		[0.057]		[0.023]
ΔDistUniv × Post1386		-0.9		-0.552
		[0.729]		[0.547]
ΔDistUniv × Year × Post1386		0.216**		0.033
		[0.096]		[0.036]
Constant	0.418	1.214	1.410**	1.339*
	[0.635]	[0.815]	[0.589]	[0.789]
Window (years)	1386 ± 15		1386 ± 25	
Observations	30	67680	50	112800
Number of cities/cross sectional units	2256	2256	2256	2256

*: Significant at 10%; **: 5%; ***: 1%. The outcome variable in all regressions is the number of market establishments per 1,000 cities in the region examined (see footnote 42 for additional details). In time series specifications (columns 1 and 3), the unit of observation is the year. In the panel data specifications (columns 2 and 4), the unit of observation is the city×year. Robust standard errors in brackets. Standard errors in the panel data specifications are clustered at the city level.

Table OA.9: Expanded windows of analysis (polynomial time trends)

Dependent variable:	Rate of market establishment			
Time trend approximation:	Quadratic Polynomial	Cubic Polynomial	Cubic Polynomial	Cubic Polynomial
	(1)	(2)	(3)	(4)
Year $\times$ Post1386 (=*difference in slopes around 1386*)	0.493**	0.449***	0.173*	0.177***
	[0.214]	[0.168]	[0.089]	[0.060]
Window (years)	1386 ± 25	1386 ± 50	1386 ± 75	1386 ± 100
Observations	50	100	150	200
Number of cities	2256	2256	2256	2256

*: Significant at 10%; **: 5%; ***: 1%. The outcome variable in all regressions is the number of markets established per 1,000 cities in the region examined (see footnote 42 for additional details). The unit of observation in all regressions is the year. Coefficient estimates for the other explanatory variables (cf. equation (OA.1)) omitted. Robust standard errors in brackets.

Table OA.10: Robustness to definition of the outcome variable

Dependent variable:	Rate of market establishment and city incorporation		Rate of market establishment (indicator)	
	Time series	Panel, city level	Time series	Panel, city level
	(1)	(2)	(3)	(4)
Year	-0.117*	0.015	-0.035	0.026
	[0.062]	[0.081]	[0.024]	[0.046]
Post1386	0.084	1.211	-0.061	0.3
	[0.904]	[1.672]	[0.466]	[0.810]
Year $\times$ Post1386	0.201**	-0.11	0.093**	-0.083
	[0.084]	[0.138]	[0.039]	[0.069]
ΔDistUniv		-0.336		-0.412
		[0.479]		[0.262]
ΔDistUniv $\times$ Year		-0.075*		-0.035
		[0.043]		[0.022]
ΔDistUniv $\times$ Post1386		-0.643		-0.206
		[0.700]		[0.355]
ΔDistUniv $\times$ Year $\times$ Post1386		0.177***		0.100***
		[0.064]		[0.033]
Constant	1.808**	2.396***	0.852**	1.574**
	[0.750]	[0.919]	[0.345]	[0.616]
Window (years)	1386 ± 20			
Observations	40	90240	40	90240
Number of cities/cross sectional units	2256	2256	2256	2256

*: Significant at 10%; **: 5%; ***: 1%. The outcome variable in all regressions is the number of economic "events" per 1,000 cities in the region examined (equivalently to the normalization of the number of markets; see footnote 42 for additional details). "Events" are the sum of market establishments and city incorporations (columns 1–2), or indicators of at least one market being established in a city (so any city receiving multiple market grants in a given year is coded as experiencing a single "event"; columns 3–4). In time series specifications (columns 1 and 3), the unit of observation is the year. In the panel data specifications (columns 2 and 4), the unit of observation is the city$\times$year. Robust standard errors in brackets. Standard errors in the panel data specifications are clustered at the city level.

OA.7 Full-sample tests of heterogeneous trend breaks across German cities

In the main text, we examine a range of explanations for the positive trend breaks in market establishment that we find, other than the increased human capital following the establishment of universities in 1386. We consider a range of alternatives:

- universities may have been established in cities that were expected to experience greater economic activity
- territorial lords establishing universities may have administered their regions differently, or may have been differentially affected by the Schism
- our results may have been driven by regional differences: areas near the Rhine may have driven our results, and areas east of the Elbe may have done so as well
- changes in city jurisdiction might have produced market grants that were indicative of political changes, rather than economic change
- cross-city conflicts may have produced trade-diverting market establishments
- finally, cities loyal to the Avignon Pope may have received market grants for political reasons

To determine whether these alternative explanations for variation in economic activity were likely drivers of our findings, in the main text we dropped particular subsets of cities from our analysis, and found that no set of potentially "problematic" cities seemed to drive our findings (see Tables IV through VI).[13] As a check of those findings, we now test whether allowing these various sub-groups of cities to experience their own trend rates of economic activity (and their own trend breaks in 1386) affects our findings in our standard panel data analysis. Rather than dropping these groups of cities from the analysis all together, we use our entire sample of cities and test whether our findings in the main text are preserved when accounting for these various subgroups of cities' possibly divergent economic paths.

To be precise, we estimate the model in Table III, column 4, but include the interaction of $Year_t \cdot Post_t$ with an indicator that a city belongs to a particular subgroup (we examine one subgroup at a time, as in the main text, and we include all lower-order interactions). As in the main text, our model predicts that the coefficient on $\Delta DistUniv_i \cdot Year_t \cdot Post_t$ will be positive and statistically significant. In Table OA.11, columns 1–8, we show the results of estimating our panel model allowing the 8 different groups of cities dropped in the text to have their own trend rates of economic activity. In every case, we continue to find a positive, statistically significant coefficient on $\Delta DistUniv_i \cdot Year_t \cdot Post_t$ (except for the marginally insignificant case of column (2)), providing further evidence that the human capital produced in universities drove the trend breaks we identified, rather than the alternatives proposed.

[13]Note that in addition to merely dropping cities to test among hypotheses, we also examined pre-1386 trend rates of market establishment in cities with different $\Delta DistUniv_i$; we allowed trend breaks in market establishment to vary with longitude, and exploited only within-state variation; and, we examined market establishment in Italy and England around the time of the Papal Schism, to rule out alternative explanations of our findings.

Table OA.11: Full sample analysis of problematic regions

Dependent variable:	Rate of market establishment							
Equivalent to:	TIV, col. 2	TIV, col. 4	TIV, col. 6	TV, col. 2	TV, col. 4	TVI, col. 2	TVI, col. 4	TVI, col. 6
Subgroup:	< 20 km from a univ.	< 50 km from a univ.	university territories	< 20 km from Rhine	east of Elbe	jurisdictional changes	Württem-berg	Avignonese obedience
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Year	-0.000	-0.018	0.011	-0.018	-0.004	-0.005	-0.004	-0.003
	[0.075]	[0.076]	[0.075]	[0.075]	[0.076]	[0.075]	[0.075]	[0.075]
Post1386	1.692	1.615	1.495	1.705	1.710	1.684	1.725	1.757
	[1.562]	[1.584]	[1.542]	[1.582]	[1.607]	[1.553]	[1.553]	[1.555]
Year $\times$ Post1386	-0.089	-0.063	-0.099	-0.064	-0.080	-0.086	-0.086	-0.088
	[0.139]	[0.139]	[0.141]	[0.139]	[0.143]	[0.138]	[0.138]	[0.138]
ΔDistUniv	-0.193	-0.210	-0.432	-0.114	-0.578	-0.179	-0.219	-0.118
	[0.426]	[0.452]	[0.455]	[0.433]	[0.445]	[0.413]	[0.422]	[0.401]
ΔDistUniv $\times$ Year	-0.036	-0.013	-0.052	-0.012	-0.032	-0.033	-0.035	-0.033
	[0.036]	[0.038]	[0.038]	[0.037]	[0.035]	[0.035]	[0.036]	[0.038]
ΔDistUniv $\times$ Post1386	-0.898	-0.788	-0.630	-0.907	-0.935	-0.984	-0.872	-1.011
	[0.647]	[0.695]	[0.653]	[0.692]	[0.636]	[0.627]	[0.638]	[0.639]
ΔDistUniv $\times$ Year $\times$ Post1386	0.139**	0.104	0.152**	0.104*	0.135**	0.136**	0.134**	0.140**
	[0.061]	[0.063]	[0.066]	[0.063]	[0.059]	[0.059]	[0.060]	[0.063]
Constant	1.545*	1.552*	1.613*	1.503*	2.795***	1.509*	1.512*	1.526*
	[0.836]	[0.843]	[0.833]	[0.832]	[0.955]	[0.830]	[0.828]	[0.829]
Window (years)	1386 ± 20							
Observations	90240	90240	90240	90240	90240	90240	90240	90240
Number of cities	2256	2256	2256	2256	2256	2256	2256	2256

*: Significant at 10%; **: 5%; ***: 1%. Robust standard errors in brackets, clustered at the city level. Cf. notes to the equivalent tables in the main paper. The dependent variable is computed as market establishments per year per 1,000 cities in the region considered. The subgroup indicated in the respective header of each column is characterized by an indicator variable in each regression, and this indicator variable is fully interacted with the $Post_t$ and $Year_t$ variables, and their interaction term. Coefficients relating to the subgroup indicators and their corresponding interaction terms not reported.

OA.8 Using within-state variation

In the main text, we consider a specification that adds to our baseline specification state-specific time trends, as well as state-specific breaks of the trend in 1386 (results are presented in Table V, column 6). We adopt the division of Germany into 18 states, as in the volumes of the *Deutsches Städtebuch*: these states are Bavaria, Baden, Württemberg, Hesse, Rhineland-Palatinate, Saarland, Rhineland, Westphalia, Lower Saxony (including Bremen), Schleswig-Holstein (including Hamburg), Saxony, Thuringia, Saxony-Anhalt, Brandenburg (including Berlin), Mecklenburg, Silesia, and Pomerania (for the vast majority of cases, these states correspond to present-day *Länder* in the Federal Republic of Germany). Here, we provide a map showing these 18 states, and the locations of cities within them, see Figure OA.4.

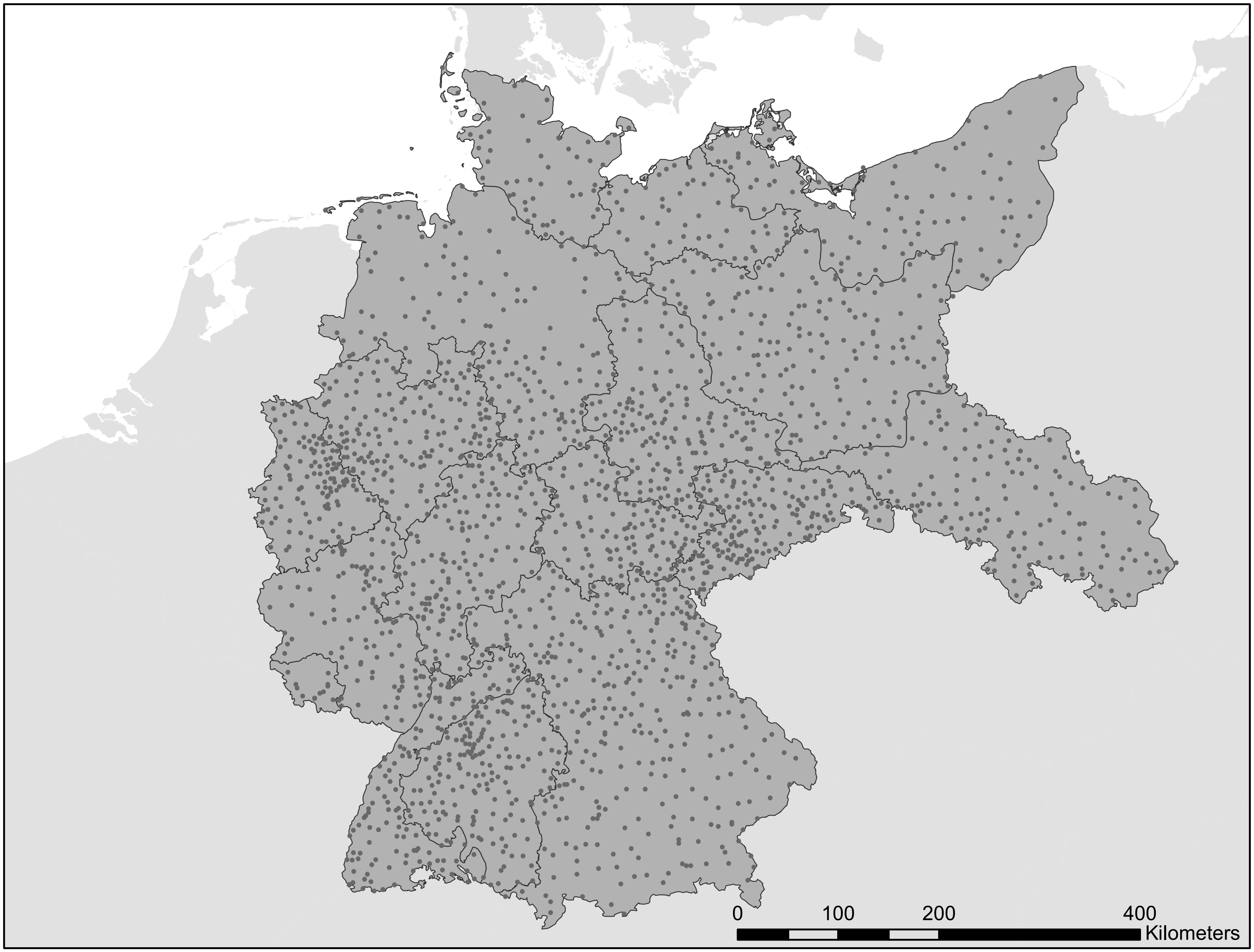

Figure OA.4: This map divides the German territory analyzed in the paper into 18 states, as in the volumes of the *Deutsches Städtebuch*. Each dot indicates a city included in the dataset.

OA.9 The impact of political shocks outside of Germany

In the main text, we discuss the impact of political changes within Germany (the impact of the Papal Schism, of jurisdictional changes, and of inter-city conflict) on patterns of market establishment. Of course, political shocks *outside* of Germany may have had spillover effects, affecting patterns of German trade, and thus market establishment. One important event overlapping with the Papal Schism was the political rise of Burgundy following the establishment of Philip II as Duke in 1363. One might worry that political change in Burgundy could have affected the German lands we study: perhaps trade increased in German cities near Burgundy around this time; because the cities in the western part of Germany had larger values of $\Delta DistUniv_i$, this could have generated the trend break we find.

To determine whether this was likely to have driven our results, we estimate our specifications from Table III, columns 1 and 4, but drop all cities in the state of Baden, which was the German territory closest to Burgundy, and the most likely to have been affected by political shifts there. We find that excluding the cities in Baden has very little effect on the coefficients of interest; we continue to observe a positive trend break in 1386, concentrated in cities experiencing a large reduction in distance to a university (see Table OA.12, columns 1 and 2).

Another important political event of the late 14th century was the revolt of Flanders (see, for example, Cohn (2006, pp. 225-227)). One might wonder if conflict in Flanders redirected trade toward the adjacent, western parts of Germany, thus generating the pattern of market establishment we observe. To address this concern, we estimate the specifications in Table III, columns 1 and 4, but drop all cities west of Düsseldorf (6 degrees, 47 minutes, east longitude). We find that our main results are practically unaffected (see Table OA.12, columns 3 and 4).

The reign of Jogaila (Wladyslaw II Jagiello of Poland), beginning in the late 14th century and marking the beginning of Poland's "Golden Age," roughly coincided with the Papal Schism, and might have affected economic activity in Germany. However, we do not believe it drives our results: when we dropped the regions of Germany east of the Elbe (in Table V, columns 3 and 4), which were most likely to have been affected by political change in Poland, we find that our results are unchanged.

Finally, the Hundred Years' War, waged between France and England throughout the period we study, was also unlikely to have generated our results: by the 1380s, the War was focused on Calais, quite far from the territories we study. To the extent that the War affected German trade, it was most likely to do so in the western part of Germany; yet, as noted above, dropping cities west of Düsseldorf does not affect our results. Moreover, as also noted above, the most important political changes in France at the time occurred in Burgundy, and dropping German cities closest to Burgundy does not change our results.

Table OA.12: Accounting for external political shocks

Dependent variable:	Rate of market establishment			
Sample:	excluding Baden		excluding "close" to Flanders	
		Panel, city level		Panel, city level
	(1)	(2)	(3)	(4)
Year	-0.058	-0.011	-0.060	-0.004
	[0.059]	[0.077]	[0.054]	[0.075]
Post1386	0.205	1.879	0.084	1.752
	[0.873]	[1.596]	[0.832]	[1.561]
Year × Post1386	0.125	-0.090	0.154**	-0.090
	[0.082]	[0.140]	[0.075]	[0.138]
ΔDistUniv		-0.097		-0.147
		[0.433]		[0.431]
ΔDistUniv × Year		-0.027		-0.033
		[0.036]		[0.037]
ΔDistUniv × Post1386		-0.975		-0.977
		[0.655]		[0.650]
ΔDistUniv × Year × Post1386		0.125**		0.143**
		[0.061]		[0.061]
Constant	1.345*	1.511*	1.266*	1.517*
	[0.670]	[0.853]	[0.633]	[0.836]
Window (years)	1386 ± 20			
Observations	40	85160	40	87440
Number of cities/cross sectional units	2129	2129	2186	2186

*: Significant at 10%; **: 5%; ***: 1%. The outcome variable in all regressions is the number of market establishments per 1,000 cities in the region examined (see footnote 42 for additional details). In time series specifications (columns 1 and 3), the unit of observation is the year. In the panel data specifications (columns 2 and 4), the unit of observation is the city×year. Robust standard errors in brackets. Standard errors in the panel data specifications are clustered at the city level.

OA.10 England and Italy falsification exercises, using 1378 as the pivot year

Our test for a break in the trend rate of market establishment in Italy or England and Wales in 1386 was intended to check whether places experiencing the Schism, but not experiencing university foundations as a result, also experienced changed rates of market establishment.

Of course, the pivot year we used was chosen to fit specifically German circumstances (the slight delay between the year of the Schism and the foundation of Germany's first universities). If the Papal Schism affected market establishment in England and Wales or in Italy, it might have done so immediately. We thus estimate the specifications in Table VII, but use 1378 as the pivot year used to define $Post_t$. We again find no effect of the Papal Schism on market establishment in England and Wales or in Italy, providing further evidence that without university establishment as a consequence, the Papal Schism did not significantly affect economic activity (see Table OA.13).

Table OA.13: Robustness of placebo analyses

Dependent variable:	Rate of market establishment	
Sample:	Italy	England and Wales
	(1)	(2)
Year	-0.087	-0.020
	[0.064]	[0.034]
Post1378	2.161	1.742*
	[1.373]	[0.954]
Year × Post1378	0.119	-0.079
	[0.145]	[0.072]
Window (years)	1378 ± 20	
Observations	40	40
Number of cities	190	2254

*: Significant at 10%; **: 5%; ***: 1%. Both regressions estimate the baseline time series specification (Table III, column 1), except that 1378 is used as the "pivot year" defining the $Post_t$ dummy variable. The outcome variable in the regressions is the number of markets established per 1,000 cities (see footnote 42 for additional details). The unit of observation is the year. Regions examined in the table are: Italy in column 1 (i.e., Naples, Sicily, and Lombardy); England and Wales in column 2. Data on market establishments in Italy come from Mira (1955); Grohmann (1969); Epstein (1992); data on market establishments in England and Wales come from Keene and Letters (2004). Robust standard errors in brackets.