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When Universities Matter: Academic Quality, Market Access, and Urban Growth in Pre-Industrial Europe

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Abstract

We examine universities and urban growth in pre-industrial Europe using a panel of 2,247 cities from 1000 to 1800 and a staggered difference-in-differences design. Under the maintained identifying assumptions, university establishment is associated with approximately 20 percent higher city population, but estimates are highly heterogeneous. Population gains are strongest in earlier centuries and weaken as universities diffuse, while after 1500 they are concentrated among high-quality universities and commercially connected cities, especially near Atlantic and North Sea trade routes. University establishment is also followed by improved local market access. Overall, universities matter most where academic quality and commercial opportunities are greatest.

JEL classification

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Keywords

universities, urban growth, upper-tail human capital, market access, pre-industrial Europe

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1. Introduction

Corporations—voluntary associations of unrelated individuals pursuing common aims—were among the most distinctive institutional innovations of late medieval and early modern Europe (Greif, 2006; Greif et al., 2012). By providing durable forms of collective organization, corporate bodies such as free cities, fraternities, monasteries, guilds, and universities helped shape European institutions and culture and may have contributed to the Great Divergence (Greif et al., 2025). Yet, although universities emerged during the expansion of markets and fairs and the broader cycle of economic growth and urbanization that reshaped medieval Europe (Bosker et al., 2013), their economic role remains comparatively less well understood. This paper studies whether university establishment affected local economic development in medieval and early modern Europe. We use city population, as proxy for economic development because larger population reflected productive capacity as well as long-run prosperity in this era and examine whether universities also shaped local market access. Universities could plausibly affect both margins because, from the twelfth century onward, they created a distinctive model of higher education based on corporate autonomy, formal teaching privileges, degree-granting authority, and a close link between teaching and scholarship (Maierù & Pryds, 1994; Välimaa, 2019; de la Croix et al., 2024). By attracting students and scholars, certifying skills, and producing legal, administrative, and professional human capital, universities may have affected local development through several channels. They directly raised local demand for housing, food, books, lodging, legal services, and other urban goods and services. Moreover, they trained lawyers, notaries, administrators, physicians, and theologians whose skills were relevant for governance, contract enforcement, legal administration and commerce. In particular, the revival and teaching of Roman and canon law may have strengthened the legal foundations of intercity exchange and reduced transaction costs (Cantoni & Yuchtman, 2014; Cox, 2017; Greif et al., 2025). Universities may also have generated local knowledge spillovers, attracted skilled migrants, and raised the returns to labor (Dittmar, 2011). These effects were unlikely to be uniform. Rather, they are expected to be stronger for higher-quality institutions and in cities able to absorb and employ the human capital produced by universities, especially cities connected to trade routes, ports, markets, or autonomous local institutions.

To examine these issues, we construct a city-level panel of 2,247 European cities observed from 1000 to 1800. The dataset combines population data from Buringh (2021) with information on university foundations, university quality, geography, trade connectivity, political institutions, and other city characteristics. University data are based primarily on de Ridder-Symoens (1992) and Rüegg and de Ridder-Symoens (1996), and, following these authors, we define universities as legally recognized corporate bodies endowed with formal teaching privileges and degree-granting authority. We then match these institutions to cities and treatment cohorts according to the first century in which a university had operated long enough to plausibly affect local population dynamics.[3]

The main empirical challenge is that universities were not founded randomly. They were often established in cities that were already politically important, commercially connected, ecclesiastically central, or economically dynamic; in the absence of a natural experiment comparable to Cantoni and Yuchtman (2014), we therefore estimate the effect of university establishment using a staggered difference-in-differences design. Our preferred specification applies Wooldridge's (2023, 2025) lags-only extended two-way fixed effects (ETWFE) estimator in a Poisson framework. The Poisson ETWFE framework accommodates the skewness and zero values in city population data while allowing treatment effects to vary across

[3] We assign a university to a century only if it had been already operating for at least 50 years by that century's population observation. Alternative cohort definitions assign universities to treatment once they had operated for 30, 40, 50, 60, or 70 years before a century's population observation.

cohorts and centuries, thereby avoiding the pitfalls of conventional TWFE estimators in staggered treatment settings. Our preferred specification also includes country-by-century fixed effects, and interactions between predetermined geographic characteristics and century dummies. Identification therefore comes from within-country, within-century comparisons among cities with flexibly controlled differential population trends.

Under the maintained difference-in-differences assumptions, our baseline results indicate that university establishment is associated with an increase in city population of about 20 percent on average. The association is positive and statistically significant across specifications and robust to region-by-century fixed effects, interactions between predetermined city characteristics and century dummies, matched samples, leads-and-lags event study specifications, selection-adjusted placebo assignments and alternative treatment-cohort definitions. In addition, a local HonestDiD sensitivity analysis (Rambachan and Roth, 2023), using the pre-treatment deviations two and three centuries before university establishment as the benchmark, suggests that the main result is robust to moderate deviations from parallel trends. Effects are strongest in the early centuries and weaken over time, consistent with universities being especially valuable during and immediately after the Commercial Revolution, when legal and administrative human capital was scarce, and with their marginal contribution declining as universities became more widespread and the academic labor market changed after 1650 (de la Croix et al., 2024).

At the same time, the weakening of the average effect over time does not imply a uniform decline across all universities and cities, as the estimated association also masks substantial heterogeneity. Population gains are concentrated among higher-quality institutions, as excluding high-quality universities sharply attenuates the estimated effect, while a continuous dose-response analysis using the Quality Index of Universities from de la Croix et al. (2024) reveals a steep positive gradient. The role of universities seems especially important also in the case of cities near the coast, specifically those close to the Atlantic Ocean and the North Sea, particularly after 1500, the period of Atlantic trade expansion (Acemoglu et al., 2005). This result is consistent with a possible complementarity between universities and commercial opportunities. Universities may indeed have supplied legal, administrative, mathematical and organizational skills, while commercially connected cities provided the demand conditions under which local returns to these skills were highest. Consistently, we find that university establishment was followed by improved local market access, as treated cities' average distance to a trade center declined, and their probability of becoming a trade center increased after university foundation. Overall, these findings suggest that university-related demographic gains were larger along two related margins: academic quality on the supply side and commercial opportunities on the demand side. In this sense, these results provide empirical evidence that might be relevant to the debates on the determinants of the Commercial Revolution, the rise of Atlantic trade, and the Little Divergence (Acemoglu et al., 2005; de Pleijt & van Zanden, 2016; Cantoni & Yuchtman, 2014). Finally, we find suggestive evidence that the role of universities was stronger in cities with free communal or participatory local institutions (Bosker et al., 2013), while controlling for scientific academies leaves the baseline estimates largely unchanged, although academies may have reinforced the demographic advantage of university cities in the eighteenth century.

Our paper contributes to three strands of literature. First, it adds to work on upper-tail human capital, universities, and knowledge institutions in pre-industrial Europe. Cantoni and Yuchtman (2014) show that medieval German universities contributed to market formation by supplying legal human capital, while de la Croix et al. (2024) document agglomeration, sorting, and selection in the European academic labor market. More broadly, our research relates to Dittmar (2011) on the printing press; Squicciarini and Voigtländer (2015) on scientific knowledge and industrialization; Serafinelli and Tabellini (2022) on creative elites and city growth; Buhler et al. (2024) on women secondary education and women's entry into Germany's human-capital elite; Dittmar and Meisenzahl (2022) on research universities, invention, and industry; Curtis et al. (2026a) on premodern scholarship and long-run growth; and Zanardello (2024) on universities, scientific academies, and urban development. We complement this literature by showing that the urban returns to universities were conditional rather than automatic, depending on university quality and cities' embeddedness in trade networks. Second, the paper contributes to the historical urban economics literature on pre-industrial city growth, which emphasizes geography, trade, political institutions, market access, agglomeration forces, and historical shocks as determinants of urban development (De Long & Shleifer, 1993; Bosker et al., 2013; Bosker & Buringh, 2017; Jedwab et al., 2022; Serafinelli & Tabellini, 2022; Cox, 2017). We show that universities were an additional institutional determinant of urban growth, especially where they interacted with trade networks, local political institutions, and other knowledge-producing organizations. Third, our study relates to research on the economic effects of universities and research institutions in contemporary settings, which shows that universities can generate local knowledge spillovers, innovation, employment growth, and regional productivity gains, especially when university research capacity is strong and local firms are technologically or industrially connected to university knowledge (Aghion et al., 2010; Kantor and Whalley, 2014; Valero & Van Reenen, 2019; Hausman, 2022; Cottini et al., 2026, among others). Our findings suggest that this local economic role has deep historical roots and was already shaped by university quality and local context even in pre-industrial times.

The rest of the paper is organized as follows. Section 2 presents the historical background and discusses the mechanisms through which universities may have affected urban growth. Section 3 describes the data. Section 4 outlines the empirical strategy. Section 5 presents the baseline results, robustness checks, heterogeneity analyses, and evidence on mechanisms. Section 6 concludes. Details on data and additional results are provided in the Appendix.

2. Historical framework

The medieval city was a complex institutional and economic environment in which commercial activity, political organization, religious authority, and social mobility interacted closely (Braudel, 1982). Urban centers depended on their surrounding countryside for food and labor while attracting migrants seeking markets and economic as well as social opportunities (Verger, 1992; Bairoch, 1988; Chartier, 1989). Over the long run European city growth reflected the interaction of geography, political institutions, commercial networks, and agglomeration forces (Bosker et al., 2013). Within this broader transformation, the rise of universities represented a distinctive European institutional innovation.

The first European universities emerged in the late eleventh and twelfth centuries in cities such as Bologna, Paris, and Oxford, establishing a new organizational form based on corporate autonomy, formal teaching privileges, legal recognition, and degree-granting authority (de Ridder-Symoens, 1992; Rüegg & de Ridder-Symoens, 1996; Haskins, 2017). From the thirteenth century onward, university foundations spread across Europe (Map A1 and Figure A1 in Appendix A), supported by trade expansion, growing demand for trained clerics and administrators, and the development of urban and princely governments. Universities were therefore more than centers of learning; they were durable corporate institutions embedded in the legal, economic, and political structure of medieval and early modern cities.

Universities may have affected urban development through three broad mechanisms. Firstly, they directly increased local demand by attracting students, masters, clerics, booksellers, copyists, lodging providers, and other service workers into their city of establishment. Additionally, and probably more importantly, they produced legal, administrative, and professional upper-tail human capital which could improve governance, contract enforcement, and market exchange. Finally, the local conditions, including trade opportunities and political autonomy as well as the broader institutional environment, may have shaped the extent to which the first two channels mattered. These mechanisms form the historical background for our empirical analysis of university quality, trade connectivity, and complementarities with autonomous cities and other knowledge-producing institutions.

The first and direct channel operated through local demand. Universities attracted students and teachers from different regions, by concentrating on a relatively large and mobile population in a single city, thereby raising demand for housing, food, clothing, books, manuscripts, legal services, lodging, and other urban goods and services (Verger, 1992). These effects may have stimulated local employment and encouraged migration from surrounding areas — a mechanism particularly relevant in the medieval period, when universities were among the few institutions capable of regularly attracting large numbers of non-local individuals. Students and scholars were not only consumers of urban services but also generated demand for specialized activities such as book production, copying, translation, and legal advice, so that universities contributed to urban economic life well beyond instruction itself.

Universities also enhanced the prestige and attractiveness of host cities. By attracting students, scholars, theologians, jurists, and physicians, they enriched local intellectual life and connected university towns to wider networks of study, learned mobility, and professional advancement. They also offered channels of social mobility for groups seeking advancement through education, ecclesiastical office, law, medicine, teaching, or public administration (de Ridder-Symoens, 1992; Grendler, 1999; de la Croix et al., 2024). The strength of this demand channel likely depended on university quality: larger, more prestigious, and academically stronger universities were better able to attract students and the best scholars from further afield and create stronger local multiplier effects, consistent with de la Croix et al. (2024), who document agglomeration, sorting, and selection in the medieval academic labor market. This pattern motivates our analysis of whether university effects were concentrated among institutions with stronger academic capacity.

A second and arguably more important channel operated through the production of legal, administrative, and professional human capital. Medieval and early modern universities trained lawyers, notaries, clerics, physicians, theologians, and administrators — occupations central to urban economies, ecclesiastical institutions, princely states, and commercial networks, possibly playing a role in the building of the bureaucratic state after 1450, as argued in Grzymala-Busse (2020). The study of law was especially important as well. The rediscovery and teaching of Roman law, together with canon law, provided Europe with a common legal framework for contracts, property rights, inheritance, dispute resolution, and corporate organization. By training jurists and notaries, universities supplied the human capital required to operate courts, draft contracts, administer public institutions, and support commercial exchange, plausibly reducing transaction costs and improving the enforcement of agreements, thereby facilitating intercity trade and market development (Cantoni & Yuchtman, 2014; Cox, 2017; Greif et al., 2025). This mechanism links universities directly to the Commercial Revolution. As trade expanded from the twelfth and thirteenth centuries, merchants, city governments, and rulers increasingly required individuals able to manage legal documentation, adjudicate disputes, collect taxes, administer privileges and enforce contracts, and universities met this demand by producing professionals who served in urban governments, ecclesiastical administrations, courts, and commercial organizations.

This channel helps explain why university effects may have been stronger in the medieval and early Renaissance periods, i.e. when the marginal value of trained jurists, notaries, and administrators was high during the early phases of urban and commercial expansion. As universities became more numerous and the academic labor market changed after 1650, the marginal contribution of additional foundations may have declined. Indeed, de la Croix et al. (2024) show that agglomeration and selection forces weakened in the early modern period, while Mokyr (2016) highlights that some university institutions became the bastion of conservatism, while printing and alternative knowledge institutions reshaped the organization of learning (Eisenstein, 1979; Dittmar, 2011). This does not imply that universities ceased to matter after 1500, but rather that their role changed — becoming more dependent on institutional quality, specialization, and local economic conditions — as the rise of nation-states, confessional divisions, religious censorship, and educational diversification altered both the demand for university-trained human capital and the environments in which universities operated (Anderson, 1983; Jardine, 2000).

A third channel concerns complementarities between universities and the local environment. Universities were more likely to generate urban growth where cities could employ and reward the human capital they produced, so their effects may have been amplified by trade connectivity, political autonomy, and the presence of other knowledge-producing institutions. Trade was a key complement since, as already discussed, universities supplied skills especially valuable in commercial cities, where merchants required legal services and governments required trained officials to regulate markets, collect taxes, and administer privileges. University effects may therefore have been stronger in cities integrated into regional or long-distance trade networks, consistent with Cantoni and Yuchtman's (2014) argument that universities contributed to the creation of markets by supplying legal human capital. Maritime trade was extremely important in this respect in the early modern period. Coastal cities and those connected to Atlantic, North Sea, Baltic, or Mediterranean networks gained access to expanding commercial opportunities in which university-trained human capital might have been especially valuable for navigation, trade law, mercantile practice, and administrative coordination. This possible complementarity is consistent with accounts of pre-industrial growth and the Little Divergence emphasizing Atlantic trade, human capital, and commercial institutions discussed in the economics and economic history literatures (Acemoglu et al., 2005; de Pleijt & van Zanden, 2016; Koyama et al., 2019), and with evidence that coastal cities grew faster over this period (Bosker et al., 2013).

Political institutions were another likely complement. Cities with greater autonomy or stronger communal institutions may have been better able to employ university-trained individuals in administration, legal reform, fiscal management, and public-good provision. They may also have offered more attractive career opportunities to jurists, notaries, administrators, and other skilled individuals. Indeed, related work shows that cities with institutions protecting economic and political freedoms were more likely to attract and produce creative talent (Serafinelli & Tabellini, 2022), while stronger public-good institutions supported human capital formation and city growth (Dittmar & Meisenzahl, 2022).

The early modern period also saw scientific academies emerging as complementary centers of knowledge production. Academies differed from universities in organization, membership, and intellectual orientation but often interacted with university environments, and the broader climate of the Scientific Revolution reinforced the role of cities as nodes of knowledge exchange (Bender, 1988; Jardine, 2000; Lindberg & Numbers, 1983; Shapin, 1996). Zanardello (2024) shows that academies generated positive spillovers onto co-located universities, suggesting the two could be complementary rather than competing sources of urban knowledge production.

The central thesis of this paper is therefore not that universities mechanically increased city growth, but that their estimated association with urban growth was strongest where academic quality and commercial demand for skilled human capital were high.

3. Data

We construct a city-level panel for Europe by combining Buringh's (2021) population data with university, geographic, institutional, and conflict-related information for 1000-1800.

Buringh (2021) reports population, longitude, latitude, elevation, and water catchment area for 2,262 European cities from 700 to 2000[4]. We begin in 1000, the onset of the medieval urban revival (Bosker et al., 2013), because the Early Middle Ages reflected a different post-Roman socioeconomic and political environment. We conclude our analysis in 1800 to avoid the fundamental structural changes associated with the Industrial Revolution, in line with Bosker et al. (2013). The dataset focuses on human settlements in Europe spanning from 1000 to 1800, with a population of more than 1,000 inhabitants at any given point in time. Because population is observed at century intervals before 1500 and at 50-year intervals thereafter, for consistency we measure population at 100-year intervals.

University data are drawn from de Ridder-Symoens (1992) and Rüegg and de Ridder-Symoens (1996). Following their standard definition, we treat as universities only legally recognized corporate bodies with formal teaching privileges and degree-granting rights sanctioned by ecclesiastical or secular authorities[5]. The baseline sample contains 164 operating universities between 1200 and 1800, after excluding suppressed institutions (Tables A2a and A2b); academies, technical institutes, municipal colleges and other proto-university forms are not coded as universities. We exclude these alternative intellectual hubs because they lacked the legal privileges and recognized degree-granting authority which define the medieval and early modern university as a corporate body. Including them would blur treatment timing by conflating scholarly activity with formal university status. Nevertheless, because such

[4] Our working sample reduces to 2,247 cities after excluding cities whose universities were suppressed at some point during the study period. See Appendix A for details.

[5] The dates reported in these sources identify the formal foundation or recognition of a legally recognized university, not necessarily the first year of effective or continuous operation. This distinction is important because medieval and early modern universities were sometimes formally founded before they became active, experienced interruptions, or were later re-established. We therefore separate the identification of a university from its use as a treatment. Institutions are retained only when they satisfy the definition of a legally recognised university, while cohort assignment is based on whether the university operated for at least 50 years within the preceding century.

institutions could still matter for knowledge production, we also control for scientific academies in robustness checks.

Treatment cohorts are defined by the first benchmark year in which a university could plausibly affect city population. A city is assigned to a cohort only if its university operated for at least 50 years within the preceding century; otherwise, it is assigned to the next cohort. Thus Cambridge, founded in 1209, belongs to the 1300 cohort, while Vienna, founded in 1365, belongs to the 1500 cohort. We also test alternative thresholds of 30, 40, 60 and 70 years of operation.[6] It is important to note that the first three universities — Bologna (1088), Oxford (1096), and Paris (c. 1150) — are grouped with the 1300 cohort because estimating a separate earlier cohort from only three cities might give these pioneering institutions disproportionate influence. However, we show, as a robustness check, that assigning them to a separate 1200 cohort leaves main results unchanged. Suppressed universities in turn are excluded for sample consistency because the staggered Difference-in-Differences design treats university establishment as an absorbing state.[7]

We acknowledge that city population is an imperfect proxy for local economic development. Indeed, human-capital deepening, proto-industrial activity, or institutional improvements may have raised productivity without immediately increasing population, while population growth may also reflect migration and local demand rather than welfare gains. Our estimates should therefore be interpreted as lower-bound demographic effects rather than as direct estimates of productivity, income, welfare, or the full economic impact of universities (Baudin & Stelter, 2022; Baudin & de la Croix, 2024). Unfortunately, a popular alternative variable capturing

[6] In the case of Vienna, we include it in the 1400 cohort when we consider a 30-year threshold rule.

[7] Nevertheless, as a sensitivity check, we also examine whether this exclusion drives the results by adding the suppressed universities back to the sample. The estimated effects remain broadly similar, suggesting that the baseline findings are not affected by the exclusion of these institutions. Results available from the authors upon request.

economic activity like real wages is not available for most cities in the sample (Serafinelli & Tabellini, 2022). Nevertheless, despite its limitations, population is a widely used measure to assess city growth in medieval times (Johnson & Koyama (2017), among others). Moreover, we do examine whether university establishment was followed also by changes in the creation of trade centers, using data from Wahl (2016)[8]. Finally, to study the role of university heterogeneity, we use the Quality Index of Universities (QIU), scholar mobility and university size from de la Croix et al. (2024), whose construction is described in Appendix A. Beyond the baseline geographic controls, the robustness analyses also account for additional historical shocks and urban characteristics, including bishoprics, archbishoprics, potato suitability, Black Death mortality, market potential, and battles. Further details on all these controls as well as maps and descriptive statistics are reported in Appendix A.

4. Empirical strategy

As already discussed, one of the challenges in analyzing the impact of universities on urbanization is their potentially non-random location. Universities may have been more likely to be established in cities with specific characteristics, or which adopted certain policies, that also favored subsequent population growth, independently of university creation. For example, an unobserved local shock might have generated a food surplus, allowing a city to accommodate more migrants while also raising its demand for higher levels of human capital (Cantoni and Yuchtman, 2014).

In the absence of a natural experiment, we employ a staggered Difference-in-Differences (DiD) econometric framework that accounts for the biases that can arise with conventional TWFE

[8] The trade-center measures are constructed at the city-century level from Wahl (2016). We use the distance from each city to the nearest recorded trade center, an indicator for whether the city itself is classified as a trade center, and indicators for whether at least one trade center lies within fixed distance bands around the city. Because these outcomes may also reflect broader commercial dynamics, we use them as mechanism-consistent evidence on market access rather than as predetermined sources of identifying variation.

models in the presence of staggered treatment timing (Callaway and Sant'Anna, 2021; Roth et al., 2023). To address this issue, we adopt the ETWFE estimator proposed by Wooldridge (2023, 2025), which avoids the contamination problem by saturating the model with cohort × century interaction terms and identifies the ATT under the standard DID assumptions of no anticipation and (conditional) parallel trends. Specifically, we use the lags-only version of the ETWFE, in which the control group is composed of never-treated and not-yet-treated cities and ATTs are estimated relative to the whole pre-treatment period. In the linear case, this estimator reproduces the imputation-based estimator of Borusyak et al. (2024).

A further issue concerns the nature of the outcome variable. Population data are highly skewed, with many cities having very small populations and a non-negligible mass of zero observations in the dataset,[9] especially in the early years. This makes the use of linear regression models less suitable in our setting.[10] Since the outcome is non-negative, highly skewed, and unbounded above, a Poisson specification provides a natural alternative (Chen and Roth, 2024). Indeed, Wooldridge (2023) shows that a Poisson model can be used to estimate ATTs in staggered DiD settings such as ours. More specifically, we can represent the ETWFE model by Wooldridge (2023) as follows:

$$E\left(Y_{it} \middle| d_{iq}, x_i\right) = \exp\left(\eta + \sum_{q=1300}^{1800} \sum_{t=q}^{1800} \lambda_{qt}\left(d_{iq} \cdot f_t\right) + \sum_{t=1300}^{1800} [(f_t \cdot \widetilde{x_i})\pi_t + (lon_i \times lat_i \times f_t)\gamma_t] + \alpha_{c_i \times t} + u_i\right) \quad (1)$$

where d_{iq} is a binary variable equal to one if a city i belongs to treatment cohort q, with $q \in \{1300, 1400, 1500, 1600, 1700, 1800\}$, denoting the first century in which a university can

[9] Zeros in the population variable denote zero thousands of inhabitants, not a literal absence of population. They identify settlements that are recorded in the dataset but fall below the 1,000-inhabitant threshold used by Buringh (2021) in that given century.

[10] The common practices of taking the log of 1+Y or using transformations such as the inverse hyperbolic sine to accommodate zero values have been criticized because they are not invariant to the unit of measurement (Chen & Roth, 2024).

affect city population; f_t is a century dummy equal to one in period t; λ_{qt} are the coefficients of interest, representing the cohort-and-century-specific DiD estimates of the average treatment effect of university establishment on the Poisson conditional mean;[11] $\widetilde{x_\iota}$ is a vector of time-invariant demeaned geographic controls, including distance to the coast and navigable rivers (first nature characteristics), Roman roads (second nature characteristics), and the city of Wittenberg (as a proxy for the spread of the Reformation), interacted with century dummies with coefficients π_t; $(lon_i \times lat_i \times f_t)\gamma_t$ represents interactions of city longitude and latitude (and their interactions) with century dummies, with coefficients γ_t, capturing smooth spatial gradients in urban development that are allowed to vary over time; $\alpha_{c_i \times t}$ represents NUTS0 (country) × century fixed effects (or historical macroregion × century fixed effects for models 2a and 2b), while u_i is a set of city- specific fixed effects. The overall ATT and the calendar-time ATT of the establishment of a university on the Poisson index reported in the tables are weighted averages of the $\hat{\lambda}_{qt}$, aggregated across cohorts with weights proportional to cohort size (Chen & Roth, 2024).[12] Because the outcome model is multiplicative, these cohort-and-century coefficients and the aggregates reported in the tables measure proportional effects on the conditional mean of population, that is, average semi-elasticities; the implied percentage effect is therefore obtained as exp(ATT) minus one, and standard errors for these nonlinear aggregates are computed by the delta method. Calendar-time effects average the relevant cohort-specific effects for each century, so later calendar estimates combine effects for early-treated cohorts with effects for more recently treated ones.

[11] One for each (q, t) with $t \geq q$ so that the double summation over q and $t \geq q$ spans all treated cohort-by-post-treatment-century combinations.

[12] It is important to note that, in a Poisson specification, the identification of the treatment effect relies on a specific version of the parallel trend assumption, namely the conditional index parallel trend (CIPT) assumption, which is equivalent to the stability in the proportional difference in growth rates across treatment and control groups (Wooldridge, 2023).

Importantly, the inclusion of the country-by-century fixed effects absorbs country-level unobservables that vary over time, so that identification comes from comparisons within a country and a given period. We acknowledge that NUTS0 (country) units correspond to modern national boundaries and are therefore anachronistic for the pre-industrial period; we use them as a practical and exhaustive partition of space that flexibly absorbs time-varying shocks common to broad geographic regions, and we assess sensitivity to historically grounded units through the macroregion specification described next. Indeed, we also consider macroregion-by-century fixed effects based on the historically informed classification proposed by Curtis et al. (2026a), which delineates geographically and institutionally coherent regions across pre-industrial Europe.

To assess pre-treatment dynamics without contamination bias that can affect conventional event study specifications in the case of staggered treatment (Wooldridge, 2023, 2025), we employ a leads-and-lags version of the ETWFE model, estimating event-study coefficients from a saturated specification that includes the full set of (cohort-specific) leads and lags. The leads-and-lags specification also provides an interesting robustness check to the lags-only estimator. Indeed, the leads-and-lags specification identifies the ATT using only the never treated as the control group and measures the ATT relative to the cohort-specific period immediately before treatment onset. It therefore relies on a different use of pre-treatment information. Unlike the lags-only specification it does not impose parallel trends over the full pre-treatment path, because the lead terms estimate rather than restrict differential pre-treatment dynamics (Baker et al., 2026). However, this does not remove the need for parallel trends in the relevant counterfactual post-treatment evolution: identification still requires that, absent university establishment, treated and control cities would have followed parallel untreated outcome paths from the reference period onward. Wooldridge (2025) shows that, although both strategies yield valid estimates under the DiD assumptions, the bias of the leads-and-lags specification may be larger than that of lags-only if parallel-trend violations extend into the treatment period.

We further probe the credibility of our identification strategy by combining the DiD framework with a pre-treatment matching procedure. In the first stage, we construct matched samples separately for each treatment cohort by pairing newly treated cities with control cities that had not yet hosted a university at that date and similar pre-treatment characteristics. To this end, we implement a k-to-k Coarsened Exact Matching algorithm (Blackwell et al., 2009). Treated cities are those establishing a university in a given century, while controls are cities without a university at that time. Matching is based only on predetermined and historically relevant predictors of university location.[13] After constructing the matched samples, we restrict the analysis to observations that lie within the common support. Balance diagnostics confirm that the matching procedure substantially reduces observable differences between treated and control cities. We then re-estimate the DiD models on the matched samples as an additional robustness exercise.

These exercises should not be understood as proof of the DiD identifying assumptions; however, they seek to assess whether the main estimates are robust to alternative control groups, different uses of pre-treatment information, and improved balance on predetermined covariates.

5. Results

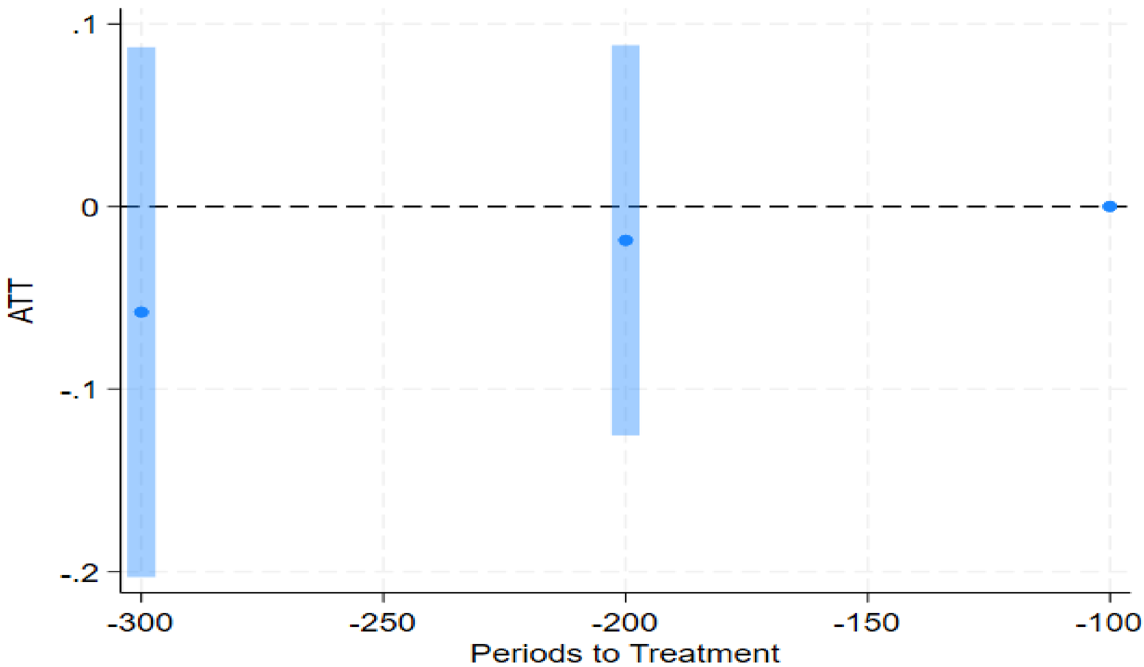
5.1 Pre-trends analysis

Figure 1 presents the pre-treatment event-study coefficients from the leads-and-lags specification of our most complete model (i.e., Model 3b in Table 1), which includes NUTS0

[13] These include city population in the century preceding cohort entry, indicators of trade centrality and long-distance commercial integration, proximity to episcopal and archiepiscopal seats, access to major geographic transport routes such as the coast, navigable rivers, and Roman roads, distance to major historical battle sites, and proximity to former Roman or Greek cities to account for deep rooted administrative and cultural legacies.

(country) × century fixed effects, time-invariant geographic controls interacted with century dummies, and latitude × longitude × century interactions. The leads-and-lags specification is fully saturated, i.e., it contains all leads and lags; however, in assessing pre-treatment dynamics, we focus on the local event-study window from -300 to -100 years. This choice is motivated by two considerations. First, in a setting with century-spaced observations and treatment effects that may persist over very long horizons, testing for non-parallel trends many centuries before treatment onset is of limited economic and historical relevance. In this respect, our approach is similar in spirit to Serafinelli and Tabellini (2022), who also emphasize pre-treatment periods close to treatment onset.[14] Second, the window from -300 to -100 is the only pre-treatment interval for which all treatment cohorts contribute to the estimation, making it the cleanest basis for a comparable assessment of pre-trends across cohorts.

Figure 1. Pre-trends analysis — Model 3b (main sample)



Note: Pre-treatment event-study coefficients (−300 to −100 years before university establishment) from the leads-and-lags variant of Model 3b. Point estimates cluster near zero across the entire pre-treatment window, supporting the conditional parallel trend assumption. The horizontal axis measures event time in 100-year increments relative to the first century of university operation (event time 0, the omitted reference period). Shaded bands are 95%

[14] See also the discussion in Baker et al. (2026) who emphasize that pre-trends measured far away before treatment might be less informative about post-treatment counterfactual trends.

confidence intervals. Standard errors are clustered at the city level. The dashed line marks zero. A joint Wald test of all pre-treatment coefficients fails to reject the null of parallel pre-trends ($p > 0.10$).

The estimated leads over the −300, −100 windows are limited in magnitude (-0.05 three centuries before treatment, with a p-value of 0.433, and -0.02 two centuries before treatment, with a p-value of 0.725), jointly statistically insignificant (the p-value is 0.632). Both the magnitude and the statistical significance are broadly consistent with the absence of important pre-treatment violations. As a robustness check, Figure B1 in Appendix B replicates the same leads-and-lags specification on the CEM-matched subsample. The pre-trends analysis in the matched sample looks even more reassuring, with coefficients lying closer to zero (-0.05 and +0.005, respectively), both individually and jointly statistically insignificant.

As a final sensitivity check, Table B1 in Appendix B reports the results from applying the HonestDiD approach of Rambachan and Roth (2023) to the treatment-onset event-study coefficient. Specifically, we calibrate deviations from parallel trends using the estimated event-study coefficients two and three centuries before treatment, while the century immediately before treatment serves as the reference period. The estimate remains positive and statistically different from zero when allowing violations as large as the largest recent pre-treatment deviation. However, when the allowed violation equals one and a half of the recent pre-treatment deviation, the confidence interval marginally includes zero. We therefore interpret this exercise as indicating that the treatment-onset estimate is robust to moderate, but not large, violations of local pre-treatment trends.

All in all, given the century-spaced nature of the data, we view this set of results on the importance of pre-trends as only supportive, rather than definitive. However, while not definitive, we believe that the local pre-treatment evidence is sufficiently reassuring to justify the use of the lags-only specification as our preferred estimator, which uses the full pre-treatment period rather than a single benchmark year as reference group and assumes (conditional) parallel trends, while the leads-and-lags model is considered only as a robustness check.

5.2 Baseline results

In this section, we present the findings from the analysis conducted on the impact of university establishment on urban growth. Following the methodology outlined in Section 4, the estimates of the first set of six regressions, which use city population (in thousands) as the dependent variable, are displayed in Table 1. The models, labeled Model 1a through Model 3b, are based on different specifications. Models 1a and 1b include city, century, and cohort-by-century fixed effects; Models 2a and 2b additionally include historical macroregion × century fixed effects;[15] and Models 3a and 3b — our preferred specifications — include NUTS0 (country) × century fixed effects, which absorb all country-level time-varying unobservables. Models 1b, 2b, and 3b additionally incorporate interactions of predetermined geographic covariates with century dummies, allowing cities to follow different population growth trajectories depending on their proximity to the coast, rivers, Roman roads, and Wittenberg. In all specifications we also include interactions of city latitude and longitude (and their interaction) with century dummies to capture smooth spatial gradients in urban development over time (Conley and Kelly, 2025). Standard errors are clustered at the city level in all specifications.

[15] Please see Map A3 of Appendix A. As an additional robustness check, we extend the classification by defining two supplementary macroregions — Eastern Europe and the Balkans — to incorporate cities in our sample that fall outside the original Curtis et al. (2026a) boundaries and find that our baseline results hold. Please see Table B2 of Appendix B.

Table 1. Average treatment effects by century — Baseline specifications

	(1) Model 1a	(2) Model 1b	(3) Model 2a	(4) Model 2b	(5) Model 3a	(6) Model 3b
ATT	0.177**	0.207***	0.203***	0.229***	0.159**	0.187***
	(0.080)	(0.079)	(0.064)	(0.066)	(0.064)	(0.068)
By century						
1300	0.298***	0.390***	0.282***	0.374***	0.256***	0.331***
	(0.085)	(0.104)	(0.073)	(0.083)	(0.071)	(0.088)
1400	0.352***	0.408***	0.380***	0.436***	0.344***	0.377***
	(0.095)	(0.100)	(0.071)	(0.076)	(0.077)	(0.084)
1500	0.158*	0.184**	0.187***	0.250***	0.124	0.183**
	(0.085)	(0.090)	(0.072)	(0.074)	(0.076)	(0.081)
1600	0.237***	0.260***	0.236***	0.270***	0.142**	0.187**
	(0.090)	(0.091)	(0.066)	(0.068)	(0.071)	(0.074)
1700	0.208**	0.204*	0.224**	0.230**	0.158*	0.177*
	(0.105)	(0.105)	(0.090)	(0.092)	(0.087)	(0.091)
1800	0.076	0.129	0.129	0.141	0.136	0.145
	(0.106)	(0.099)	(0.088)	(0.088)	(0.089)	(0.089)
Observations	20,223	20,223	16,875	16,875	20,223	20,223
No. of cities	2,247	2,247	1,875	1,875	2,247	2,247
Geo. controls × century	No	Yes	No	Yes	No	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Hist. regions × century FE	No	No	Yes	Yes	No	No
NUTS0 × century FE	No	No	No	No	Yes	Yes

Note: Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. "ATT" is the overall average across all cohorts and post-treatment centuries; "By century" rows are calendar-time ATTs aggregated across cohorts. All specifications include city FE, century FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to coast, river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Estimates are obtained using the Stata command jwdid. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Before discussing our empirical results, we note that in the following tables we do not report the coefficients for the various DiD effects that we have estimated (i.e., one for each cohort in each century of operation), that can however be seen in Figure B2 of the Appendix B for our Model 3b. Instead, we do report an average effect for all cohorts over the sample period, as well as an average effect (across all cohorts) at calendar level, i.e., for each post-treatment century from 1300 onwards, 1300 being the first post-treatment century for the earliest cohort of universities (established in the 13th century).

The first line of results suggests the existence of a significant positive ATT associated with the establishment of a university, robust across specifications, i.e. when we include different region-by-century fixed effects and allow cities to display flexible century-specific effects according to a set of observable predetermined characteristics. Our preferred specification, Model 3b, suggests that, on average, over the sample period the establishment of a university increased population by about 20%, computed as $100 \times (exp(0.187) - 1)$.

Before moving to analyze the ATT per calendar century, it is important to note that the calendar-time ATTs should be interpreted with caution because they average over all cohorts treated by a given century, with weights proportional to cohort size. Later calendar estimates therefore combine “late effects” for early cohorts with “recent effects” for more recently treated cities. As a result, later calendar estimates incorporate a higher number of cohorts. Consistent with this, Figure B2 in Appendix B shows substantial cohort heterogeneity: the 1300 and 1400 cohorts, and to a lesser extent the 1700 and 1800 ones, drive much of the overall positive effect, whereas the 1500 and 1600 cohorts display smaller effects, very close to zero. With this caveat, the calendar estimates in Table 1 show that effects are strongest in 1300 and 1400, remain positive but smaller thereafter, and become statistically insignificant by 1800. This finding might suggest that the establishment of universities had the most substantial impact during the early centuries, broadly corresponding to the culmination of the Commercial Revolution and the period immediately following it (Lopez, 1976). The time profile of the ATT, consistent across specifications, might thus reflect the fact that, in the early centuries, universities may have favored population growth by supplying legal and administrative human capital; however, in the following centuries the enhancing impact of universities might have been less important. Several mechanisms may account for this decline. First, the decline of the university influence over time is consistent with the finding by de la Croix et al. (2024) that the market forces of agglomeration (with scholars concentrating in the best universities) and positive sorting (the best scholars more likely to take a position in the best places) essentially faded out after 1650, when the number of universities increased and the choice set faced by scholars expanded. In addition, as noted by de la Croix et al. (2024), after 1650 the emergence of scientific academies might have attracted some of the best talents outside universities, reducing university output and therefore their economic impact.[16] Finally, in the second half of the 16th century, Catholic censorship of books became important, and this may have contributed to further reducing the influence of intellectuals working in universities, at least in some regions of Europe, as found, for the Italian case, by Blasutto and de la Croix (2023).[17]

5.3 Robustness checks

This section seeks to assess the robustness of the baseline results to alternative specifications, samples, and identifying assumptions. Table 2 presents six alternative specifications. Column (1) reproduces the baseline lags-only Model 3b for comparison. Column (2) reports the leads-and-lags specification. The overall ATT is 0.130, somewhat smaller than the baseline but still positive and statistically significant. We believe this is reassuring, because the leads-and-lags and lags-only estimators rely on different control groups and different pre-treatment benchmarks and therefore would not necessarily be biased in the same way if parallel trends failed in the post-treatment period.[18]

To further assess the credibility of our identifying assumptions, we implement Coarsened Exact Matching (CEM), thereby improving pre-treatment balance between university and non-university cities. We estimate the CEM specification consistently with our baseline approach.

[16] Nevertheless, Zanardello (2024) finds a positive spillover of academies on the quality of pre-existing universities. This is however consistent with the enduring positive effects of early cohorts of universities that we find in this paper. See below for further discussion.

[17] The Thirty Years War might have also disrupted university education in various regions of Continental Europe during the 17th century (Curtis et al., 2026b).

[18] For instance, simulation results discussed in Wooldridge (2025) show that the leads and lags specification could be even more biased if parallel trend violations extend in the post-treatment periods.

As reported in Columns (3) and (4), the overall ATT remains positive and significant in both cases, though the calendar-time estimates show some attenuation in the 14th and 15th centuries, likely reflecting the smaller and more homogeneous matched sample. This in turn suggests that, once pre-treatment imbalance is reduced, universities display a discernible impact not only in the core medieval period but also in the early modern one.[19] Column (5) replaces the PPML estimator with OLS after an inverse hyperbolic sine (arcsinh) transformation of population, using the same ETWFE DiD framework. The results are broadly consistent with the baseline, with an overall ATT of 0.165 and a similar calendar time profile. Column (6) in turn includes the full set of triple interactions between cohort dummies, century dummies, and all geographic controls, allowing for treatment effect heterogeneity along all predetermined covariates (Wooldridge, 2023 and Baker et al., 2026). The overall ATT of 0.141 remains significant and the century profile is similar to the baseline.

While in our baseline specifications we do not control for time-varying covariates mainly because of concerns of "bad controls", in Column (7) we augment the baseline specification with a rich set of time-varying covariates including religious affiliation changes, proximity to archbishoprics and bishoprics (as proxies for a city's importance), distance to trade centers, distance to historical battles (which might have pushed people to flee to cities, boosting urbanization), and a measure of a city's market potential (measured as the sum of population of the other cities in the same NUTS-1 region).[20] The overall ATT of 0.168 (statistically

[19] See Tables B3 and B4 of Appendix B for the balance tests on the covariates used for CEM matching, which show a clear improvement in covariate balancing across treated and control cities with respect to the baseline sample.

[20] We also include a variable capturing black death mortality interacted with century dummies, to allow cities to have differential growth trends depending on the severity of the Black death at local level (Jedwab et al., 2022). The reduction in the number of observations in this specification reflects the limited historical coverage of the additional time-varying covariates rather than a change in the definition of the main estimation sample. These variables are only observed for a subset of city-centuries, and requiring them to be jointly non-missing

significant at 5% level) remains very similar to the baseline, confirming that our main results are not driven by omitting these time-varying confounders.

Table 2. Methodological robustness checks

	(1) Baseline (lags only)	(2) + Pre-treatment leads	(3) CEM matched (lags only)	(4) CEM matched (leads & lags)	(5) inverse hyperbolic sine (arcsinh) transformation	(6) Full triple interactions	(7) + Time-varying covariates
ATT	0.187***	0.130**	0.224***	0.169**	0.165***	0.141***	0.168**
	(0.068)	(0.065)	(0.081)	(0.075)	(0.057)	(0.053)	(0.068)
By century							
1300	0.331***	0.201***	0.228**	0.199***	0.354***	0.305***	0.331***
	(0.088)	(0.063)	(0.106)	(0.075)	(0.097)	(0.064)	(0.084)
1400	0.377***	0.252***	0.166	0.023	0.262**	0.273***	0.326***
	(0.084)	(0.077)	(0.152)	(0.160)	(0.104)	(0.070)	(0.076)
1500	0.183**	0.031	0.192*	0.087	0.209***	0.132**	0.154*
	(0.081)	(0.078)	(0.104)	(0.116)	(0.068)	(0.061)	(0.089)
1600	0.187**	0.188**	0.227**	0.200**	0.211***	0.177***	0.219***
	(0.074)	(0.081)	(0.097)	(0.088)	(0.068)	(0.064)	(0.080)
1700	0.177*	0.134	0.202**	0.143	0.167**	0.115*	0.153*
	(0.091)	(0.084)	(0.094)	(0.088)	(0.071)	(0.070)	(0.091)
1800	0.145	0.096	0.259**	0.221**	0.083	0.101	0.103
	(0.089)	(0.081)	(0.106)	(0.095)	(0.073)	(0.068)	(0.087)
Obs.	20,223	19,785	2,079	2,079	20,223	20,223	12,831
No. of cities	2,247	2,204	231	231	2,247	2,247	1,431

Note: All columns use the Model 3b specification. Col. (2) and (4) add event-study pre-treatment indicators; pre-treatment coefficients are statistically zero in both cases (see Figures 1, and B1 of Appendix B). Cols. (3)– (4) restrict to the CEM-matched sample. Col. (5) uses arcsinh(population) estimated via OLS-ETWFE. Col. (6) adds full cohort × century × geographic control triple interactions. Col. (7) augments the baseline with time-varying controls. Standard errors clustered at city level in parentheses. Estimates are obtained using the Stata command jwdid. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

In addition to the robustness checks reported in Table 2, in Appendix C, we present a further set of sensitivity analyses. These examine whether the results are robust to alternative spatial specifications and clustering choices; different thresholds for assigning universities to treatment cohorts; alternative minimum population thresholds for inclusion of a city in the sample to deal with measurement error in the population data; and the inclusion of the first

mechanically reduces the sample from 20,223 to 12,831 observations. Importantly, if we run the baseline model on this restricted sample, i.e., without the time-varying covariates, results (available upon request) are very similar to those we derive from the main sample, despite this substantial loss of observations.

three universities (Oxford, Bologna and Paris) as a separate additional 1200 cohort. Moreover, we also assess the sensitivity of the estimates to additional religious controls, university founder type, regional splits, university size and the exclusion of individual historical macro regions. Across these exercises, the estimated university effect remains positive and generally broadly similar in magnitude, indicating that the baseline findings are not driven by specific modeling choices. Finally, we also implement a selection-adjusted placebo exercise in which placebo treatment cohorts are assigned only to observationally similar never-treated cities within historical macroregions. The baseline ATT lies far in the right tail of the placebo distribution, suggesting that the result is unlikely to arise from the estimation procedure alone. In the remainder of this Section, we investigate possible violations of the SUTVA assumption underlying our DiD identification setting. Indeed, general equilibrium effects associated with the establishment of a university might arise, with untreated cities that could either benefit from the establishment of a university in a nearby town (positive spillover) or lose, with some of their population migrating towards the university one or with the city that fails to attract migrants from nearby rural areas (negative spillover).

We first examine whether the presence of a university affected nearby non-university cities by comparing cities located within 50 km (column 2) or 100 km (column 4) distance from a university city with more distant non-university cities. The estimates presented in Table 3 provide no evidence of systematic net population effects on nearby cities. This exercise should be interpreted cautiously, since a zero net effect cannot rule out the presence of offsetting spillover channels, such as positive agglomeration effects and negative population-pull effects that exactly cancel out. We therefore complement this analysis with a more direct test of whether spillovers bias the baseline ATT. Specifically, we re-estimate the baseline specification after excluding non-university cities located within 50 km (column 3) or 100 km (column 5) of a university city from the control group. If university cities grew mainly by drawing population away from nearby towns, including these towns among the controls would mechanically inflate the estimated treatment effect. The average ATT remains virtually unchanged when these nearby controls are removed, suggesting that the baseline effect is not driven by negative spillovers onto neighboring cities. The effects for the earliest cohorts and centuries are, however, somewhat smaller, which leaves open the possibility of some localized reallocation processes during the early phase of university expansion.[21]

Table 3. Spatial spillover effects

	(1) Baseline	(2) Spillover cities ≤50km	(3) Excl. control cities ≤50km	(4) Spillover cities ≤100km	(5) Excl. control cities ≤100km
ATT	0.187***	0.009	0.194***	-0.022	0.182**
	(0.068)	(0.042)	(0.071)	(0.044)	(0.073)
By century					
1300	0.331***	-0.021	0.304***	-0.037	0.146
	(0.088)	(0.082)	(0.102)	(0.046)	(0.112)
1400	0.377***	-0.151**	0.330***	-0.071	0.176*
	(0.084)	(0.075)	(0.091)	(0.048)	(0.098)
1500	0.183**	0.008	0.188**	0.044	0.134
	(0.081)	(0.055)	(0.083)	(0.046)	(0.090)
1600	0.187**	0.030	0.192**	-0.009	0.111
	(0.074)	(0.050)	(0.078)	(0.050)	(0.089)
1700	0.177*	0.026	0.197**	-0.019	0.271***
	(0.091)	(0.051)	(0.096)	(0.067)	(0.100)
1800	0.145	0.013	0.158*	-0.062	0.171
	(0.089)	(0.050)	(0.094)	(0.064)	(0.112)
Observations	20,223	18,747	14,391	18,747	8,064
No. of cities	2,247	2,083	1,599	2,083	896

Note: Cols. (2) and (4) restrict to never-treated cities within 50/100km of a university, using distance-based cohorts to test for spillover effects. Cols. (3) and (5) keep university cities but drop from the control group any city within 50/100km of a university. Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. "ATT" is the overall average across all cohorts and post-treatment centuries; "By century" rows are calendar-time ATTs aggregated across cohorts. All specifications include city FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to coast, river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Estimates are obtained using the Stata command jwdid. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

[21] For a visual representation of the cities located within a 50km or 100km distance from a university please see Map B1 of Appendix B.

Despite these relatively minor, century-specific spillovers, we nevertheless believe that our results should not be interpreted as a mere spatial reorganization of economic activity (Redding & Turner, 2015).[22]

5.4 When do universities matter? Academic quality and institutional complements

In this section we assess whether the positive ATT identified in the baseline analysis reflects a broad and systematic influence of universities as a whole or is instead concentrated among a narrower group of institutions with specific characteristics. This distinction is particularly relevant in the historical context, as medieval universities differed substantially in their organizational capacity, faculty composition, intellectual reputation and financial resources. Indeed, de la Croix et al. (2023) showed that scholars were attracted by the best universities, giving rise to agglomeration effects, and that the best scholars tended to sort into more prestigious institutions (positive sorting).

By progressively excluding institutions with high scholarly output and documented prominence, we directly test whether the observed demographic gains were generated by the general expansion of higher learning or by a small cluster of elite hubs capable of attracting and retaining significant talent. The findings in Table 4 clearly show that the positive and statistically significant treatment effect identified in the baseline is largely concentrated among a subset of academically strong establishments. In particular, in column (2) we eliminate university cities with a quality index higher than 3 (close to the 75th percentile) for more than 2 centuries, and similarly in the other columns, where we use the lower threshold of QIU higher than 2 (approximately equal to the mean).

[22] Because in the Middle Ages cities tended to grow essentially out of positive net migration (Jedwab et al., 2022), it is possible that either this migration came from very far-away cities or, as it seems more likely, from the rural countryside (Braudel, 1995; Verger, 1992; Clark, 2008), i.e. by means of urbanization, which indeed steadily increased at least since 1100 (Bosker et al., 2013), especially in regions with sustained warfare (Jedwab et al., 2022).

Table 4. Effect heterogeneity by university quality — Dropping high-quality institutions

	(1) Baseline (all universities)	(2) Excl. QIU>3 (≥2 centuries)	(3) Excl. QIU>3 (≥1 century)	(4) Excl. QIU>2 (≥2 centuries)	(5) Excl. QIU>2 (≥1 century)
ATT	0.187***	0.086	0.044	0.036	0.021
	(0.068)	(0.074)	(0.080)	(0.062)	(0.072)
By century					
1300	0.331***	0.250***	0.166*	0.229**	0.177**
	(0.088)	(0.084)	(0.090)	(0.090)	(0.090)
1400	0.377***	0.231	0.141	0.251	0.173
	(0.084)	(0.146)	(0.160)	(0.153)	(0.173)
1500	0.183**	0.215**	0.196**	0.253***	0.241**
	(0.081)	(0.091)	(0.098)	(0.096)	(0.101)
1600	0.187**	0.108	0.064	0.140	0.096
	(0.074)	(0.082)	(0.092)	(0.088)	(0.094)
1700	0.177*	0.065	0.027	-0.045	-0.046
	(0.091)	(0.104)	(0.116)	(0.069)	(0.082)
1800	0.145	0.016	-0.023	-0.068	-0.067
	(0.089)	(0.094)	(0.097)	(0.073)	(0.087)
Observations	20,223	19,764	19,575	19,638	19,386
No. of cities	2,247	2,196	2,175	2,182	2,154

Note: QIU (Quality Index of Universities) is the century-average quality ranking of scholars affiliated with the university based on the notability of affiliated scholars (de la Croix et al., 2024). Each column excludes treated cities whose university exceeded the stated quality threshold for the indicated number of centuries. Model 3b specification throughout. Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. "ATT" is the overall average across all cohorts and post-treatment centuries; "By century" rows are calendar-time ATTs aggregated across cohorts. All specifications include city FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to coast, river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Estimates are obtained using the Stata command jwdid. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

When these high-quality universities are removed from the sample, the ATT estimates decline sharply in magnitude and lose statistical significance, with no remaining evidence of systematic demographic impact at the aggregate level. It is important to acknowledge that the average level of quality increases over time, suggesting that the weakening impact of universities after 1500 in our baseline specifications cannot be attributed to a general decline in academic quality. Rather, this pattern points to a changing marginal contribution of universities as they became more widespread and the agglomeration and selection effects of the first centuries diminished (de la Croix et al., 2024). Indeed, universities founded in later centuries tended to be of systematically lower quality relative to institutions of the same period that had been established in earlier cohorts, even though overall university quality was higher than in earlier periods. This implies that later foundations may have been marginal institutions in an increasingly dense academic landscape, limiting their capacity to generate sizable effects. In fact, when comparing newly founded universities to the contemporaneous population of universities within each century, we find that new foundations typically exhibit lower average and median quality than existing institutions.[23] This pattern indicates that the capacity to contribute to sustained urban expansion was not evenly distributed across the medieval university landscape but was concentrated among a limited group of elite institutions, while lower-tier universities did not measurably influence population dynamics.[24]

To further explore the role of university quality in driving urban population growth, we move beyond the binary treatment framework and exploit variation in the level of university quality across cities and over time. Specifically, we treat the university quality index (QIU) as a continuous dose variable, normalizing it to the unit interval to facilitate comparability across percentiles of the quality distribution. The normalized quality index thus ranges from 0 to 1, where higher values reflect greater academic quality based on the notability of affiliated scholars, where cities with no universities are assigned a quality level equal to 0. We estimate a continuous-treatment variant of the ETWFE specification analogous to that in equation (1), interacting a quadratic polynomial of university-specific normalized quality index $QIU_{norm_{it}}$ with cohort-by-century cells. In other words, the model in equation (1) now has two terms, namely $\lambda_{qt} d_{iq} \cdot f_t * QIU_{norm_{it}}$ and $\lambda^{(2)}{}_{qt} d_{iq} \cdot f_t * QIU^2_{norm_{it}}$, where λ_{qt} and $\lambda^{(2)}{}_{qt}$ are parameters to be estimated. The estimation of quadratic continuous specification allows for nonlinearities, including diminishing returns, increasing returns, or threshold-type patterns in the effects of

[23] Please see Table A4 of Appendix A and Table B5 of Appendix B.

[24] Additional heterogeneity analyses by size and scholars' mobility do not reveal significant differences. This suggests that the baseline effects are not driven by these institutional characteristics.

university quality on city population. In what follows, we recover the average and calendar-time ATTs evaluated at selected percentiles of the quality distribution — specifically the 10th, 25th, 50th, 75th, and 90th percentiles among treated cities.

Before discussing the results, it is important to acknowledge that Wooldridge (2025) notes that, in order to identify the ATT, besides the conventional no anticipation and (conditional) parallel trend assumptions, one needs also to assume, in our ETWFE setting, that, conditional on treatment timing, city fixed effects, region-by-century fixed effects and predetermined city characteristics-by-century controls, cities receiving a higher-quality university should not have systematically different unobserved returns to university quality than cities receiving a lower-quality university.[25] This assumption is not innocuous. Indeed, academic quality may have reflected city-level time-varying factors such as local resources, political or ecclesiastical patronage, pre-existing scholarly networks, and the ability to attract and retain talent in the European academic labor market (de la Croix et al., 2023), all of which could also affect city-specific returns to university quality. We therefore interpret the quality estimates as evidence that the population effects of universities were concentrated among academically stronger institutions, rather than as a fully causal estimate of an exogenous increase in university quality.

The results in Table 5 reveal a striking and consistent dose-response relationship between university quality and urban population growth across all centuries of the sample. At the mean dose, the estimated calendar ATTs are positive and statistically significant in most periods, confirming that the average-quality university exerted a meaningful and persistent demographic effect. The ATTs are, however, highly heterogeneous across the quality distribution. At its bottom, the estimated impacts are negligible, indicating that the lowest-quality universities had no substantial contribution to city growth. Moving up the quality

[25] For other identification assumptions in the case of DiD settings with continuous treatment, see Callaway et al. (2024).

distribution, the estimated impacts expand rapidly, with universities at the top of the quality distribution that are associated with substantially larger population gains than their lower-quality counterparts, across all centuries. This suggests that the capacity of universities to attract students, scholars, and economic activity, and thus to stimulate local population growth, was closely tied to their academic standing, with higher-quality institutions associated with larger and more sustained demographic effects.

Table 5. Continuous dose-response — Calendar ATT by quality percentile (quadratic specification)

	(1) Mean dose	(2) 10th percentile dose	(3) 25th percentile dose	(4) Median dose	(5) 75th percentile dose	(6) 90th percentile dose
ATT	0.154**	0.002	0.014	0.097*	0.291***	0.485***
	(0.075)	(0.001)	(0.011)	(0.058)	(0.089)	(0.103)
By century						
1300	0.204	-0.003	-0.017	0.043	0.851***	2.172***
	(0.159)	(0.004)	(0.028)	(0.124)	(0.328)	(0.840)
1400	0.455***	0.004	0.030	0.259**	1.006***	1.916***
	(0.151)	(0.004)	(0.027)	(0.125)	(0.178)	(0.399)
1500	0.125	0.002	0.015	0.087	0.190*	0.243
	(0.102)	(0.002)	(0.017)	(0.082)	(0.111)	(0.174)
1600	0.215***	0.003**	0.026**	0.152***	0.313***	0.370***
	(0.074)	(0.001)	(0.011)	(0.057)	(0.091)	(0.119)
1700	0.271**	0.004	0.031	0.187*	0.422***	0.557***
	(0.125)	(0.003)	(0.019)	(0.098)	(0.136)	(0.134)
1800	0.056	-0.000	0.000	0.024	0.167*	0.378***
	(0.077)	(0.001)	(0.011)	(0.058)	(0.097)	(0.109)
Observations	19,791	19,791	19,791	19,791	19,791	19,791
No. of cities	2,199	2,199	2,199	2,199	2,199	2,199

Note: Treatment effect is modeled as $\beta_1 \cdot QIU_{norm} + \beta_2 \cdot QIU_{norm}{}^2$, with cohort × century × dose interactions estimated via PPML (city and NUTS0 × century FE). Calendar ATTs recovered using cohort-size weights and evaluated at the stated percentile of QIU_{norm}. "Mean dose" uses the mean QIU_{norm}among treated observations. Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. "ATT" is the overall average across all cohorts and post-treatment centuries; "By century" rows are calendar-time ATTs aggregated across cohorts. All specifications include city FE and latitude × longitude × century interactions. Geographic controls (distance to coast, river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Given our concern about the identification of the effect of university quality, we examine whether high-QIU university cities differed systematically from lower-QIU university cities before treatment. Table B6 splits treated cities at the median of the normalized QIU distribution and compares a set of city characteristics measured one century before university foundation. High-QIU and low-QIU cities do not differ systematically in pre-treatment population, trade-center status, the timing of trade-center emergence, or proximity to bishoprics and archbishoprics. This reduces the concern that the quality gradient simply reflects larger, more commercially central, or more ecclesiastically important cities being also more likely to receive higher-quality universities. However, high-QIU cities differ along other predetermined geographical and historical dimensions. Indeed, the cohort-adjusted comparisons in Table B6 indicate that they score higher on the transport/water catchment location index, lie closer to navigable rivers and to major battle sites, and are located farther from the coast, from Roman roads, and from former Roman or Greek cities. Academic quality was therefore not randomly distributed across cities, which is why the heterogeneity analyses maintain the full set of geographic controls and interacted fixed effects of Model 3b.

We further examine whether future university quality predicts differential pre-treatment population dynamics. Restricting the sample to treated cities, we compare population paths between high- and low-QIU university cities, normalizing outcomes relative to the century immediately before university foundation and focusing on the recent pre-treatment window used in the main event-study analysis. Appendix Table B7 and Figure B3 show no clear evidence of differential pre-treatment growth; indeed, the lead coefficients are both individually and jointly insignificant ($p = 0.42$). This does not imply that university quality was randomly assigned, but it suggests that the estimated quality gradient is not clearly explained by differential population trends immediately preceding treatment.

A potential confounder in the baseline analysis is the role of scientific academies, which emerged as important centers of knowledge production in early modern Europe, especially after 1650 (McClellan, 1985; Mokyr, 2016; Cervellati et al., 2026). Recent work suggests that academies contributed independently to human capital accumulation and urban development and may have generated positive spillovers on nearby or co-located universities (Zanardello, 2024). Curtis et al. (2026a) further show that, after 1500, scholars affiliated exclusively with academies displayed higher average human capital than those affiliated exclusively with universities. This raises the possibility that part of the demographic increase that we associate with the presence of a university may simply reflect the presence of academies in the same cities.

To address this concern, we construct an indicator equal to one when a city hosted a scientific academy operating for at least 50 years within a given century, restricting attention to academies first established in 1650 or later, which are more likely to have important effects on human capital accumulation (Zanardello, 2024). We then follow two approaches. First, we add the academy indicator as a time-varying control to the baseline specification. Second, we interact academy presence with cohort, century, and cohort-by-century terms to recover separate university ATTs for cities with and without a contemporaneous post-1650 academy.[26]

The results in Table 6 show that the baseline university effect is not driven by academy presence. Indeed, when the academy indicator is included as a simple control, the overall ATT and the calendar-time estimates remain virtually unchanged; similarly, the university effect remains positive and significant when estimated only for cities without post-1650 academies, with stable estimates through 1700. The only notable change is in 1800, when the ATT falls from 0.145 to 0.027 once academy cities are excluded. Conversely, estimates for cities hosting both a university and an academy are positive but imprecise, reflecting the small number of such cities, even if in 1800 the university ATT is large (0.26): taken together, these results suggest that academies do not confound the baseline university effect, although they may have

[26] These exercises should be interpreted with caution. Indeed, they are strictly valid only assuming exogeneity of the academy dummy, which is clearly questionable.

reinforced the demographic advantage of university cities in the eighteenth century, in line with Zanardello (2024).

Using a similar approach, we conduct additional heterogeneity analysis by considering the role played by political institutions. We do this by building a dummy equal to one for cities with some degree of local autonomy (as coded in Bosker et al., 2013) in a given period, and zero otherwise. Treating local autonomy as a source of heterogeneity rather than as an exogenous population shifter, we estimate university effects separately for cities with and without autonomous institutions (columns 3 and 4 of Table B8 in Appendix B).[27] We find that universities seem to have played a more important role in cities with local autonomy (an increase in city population of 26% versus 12%). This pattern is consistent with a complementarity between universities and local institutions characterized by greater political autonomy and broader civil participation. Indeed, Serafinelli and Tabellini (2022) show that cities with institutions protecting economic and political freedoms and promoting local autonomy produced and attracted more creative talent. In turn, while Cantoni and Yuchtman (2014) emphasize that universities fostered commercial expansion by supplying the legal and administrative human capital needed to support trade, this legal training was also important to enhance the effectiveness of local government. More generally, evidence from German history (Dittmar & Meisenzahl, 2022) indicates that stronger local public-goods institutions increased upper-tail human capital formation and city growth. Overall, these findings suggest that universities may have generated larger urban gains in autonomous cities because local institutions were better able to absorb, employ, and reward skilled human capital. However, it is important to interpret this heterogeneity result cautiously, since political autonomy (but also

[27] We interact the free commune dummy with all centuries, cohort and cohort-century fixed effects; moreover, we include it also as a simple control. Also in this case, we assume that the free commune dummy is exogenous.

the presence of an academy) is time-varying and may itself evolve jointly with other determinants of urban growth.

Table 6. Baseline models controlling for scientific academies.

	(1) Baseline	(2) Cities without academies (universities only)	(3) Cities with both academy and university	(4) Academy as covariate
ATT	0.187^{***} (0.068)	0.159^{**} (0.069)	0.220 (0.152)	0.173^{**} (0.070)
By century				
1300	0.331^{***} (0.088)	0.336^{***} (0.088)	n/a	0.331^{***} (0.088)
1400	0.377^{***} (0.084)	0.381^{***} (0.085)	n/a	0.378^{***} (0.084)
1500	0.183^{**} (0.081)	0.184^{**} (0.081)		0.183^{**} (0.081)
1600	0.187^{**} (0.074)	0.189^{**} (0.074)	n/a	0.188^{**} (0.074)
1700	0.177^{*} (0.091)	0.180^{*} (0.097)	0.108 (0.199)	0.163^{*} (0.096)
1800	0.145 (0.089)	0.027 (0.095)	0.260 (0.167)	0.115 (0.093)
Observations	20,223	20,223	20,223	20,223
No. of cities	2,247	2,247	2,247	2,247

Note: Academy variable equals 1 if the city hosted a scientific academy (≥50 years) founded after 1650; pre-1650 academies are zeroed out across all periods. "Cities without academies" restricts the treatment group to cities that host a university but never hosted an academy. "Academy cities only" reports the isolated effect for cities with a post-1650 academy. "Academy as covariate" adds academy as a time-varying control in Model 3b. Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. "ATT" is the overall average across all cohorts and post-treatment centuries; "By century" rows are calendar-time ATTs aggregated across cohorts. All specifications include city FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to coast, river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Estimates are obtained using the Stata command jwdid. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

The evidence so far shows that academic quality appears to have been central to the demographic impact of universities. We now examine the second part of the complementarity: whether these gains were larger where commercial opportunities created demand for university-trained human capital.

5.5 Universities, trade and market access

Cantoni and Yuchtman (2014) suggest that the opening of the first German universities increased the number of markets, thus contributing to the Commercial Revolution. To assess whether a similar trade-related mechanism is present in our setting, we begin by examining whether university establishment was followed by improvements in local market access. This exercise is motivated by the legal-human-capital channel discussed above: by training jurists, notaries and administrators, universities may have supported the institutional infrastructure required for market exchange, thus promoting the creation of new markets, in university cities or in nearby areas. Table 7 therefore estimates whether university establishment is followed by improved access to trade centers.

Table 7. Universities and local market access

	Full sample			Excluded pre-existing trade centers		
	(1) Distance	(2) City trade center	(3) Within 50 km	(4) Distance	(5) City trade center,	(6) Within 50 km
ATT	-0.065***	0.113***	0.094***	-0.035*	0.065***	0.068***
	(0.024)	(0.024)	(0.024)	(0.020)	(0.024)	(0.026)
By century						
1300	-0.055	0.073	0.103*	-0.005	0.002	0.048
	(0.046)	(0.045)	(0.054)	(0.030)	(0.002)	(0.048)
1400	-0.050	0.155***	0.123**	-0.007	0.078	0.063
	(0.033)	(0.055)	(0.052)	(0.022)	(0.053)	(0.052)
1500	-0.116**	0.192***	0.167***	-0.080*	0.154***	0.164***
	(0.048)	(0.046)	(0.046)	(0.043)	(0.054)	(0.056)
1600	-0.070**	0.127***	0.103***	-0.040	0.083***	0.082**
	(0.030)	(0.029)	(0.029)	(0.025)	(0.030)	(0.032)
1700	-0.061***	0.099***	0.084***	-0.034*	0.053**	0.059**
	(0.022)	(0.021)	(0.023)	(0.019)	(0.020)	(0.024)
1800	-0.050**	0.084***	0.064***	-0.026	0.042**	0.041**
	(0.020)	(0.018)	(0.020)	(0.017)	(0.017)	(0.020)
Observations	20,223	20,223	20,223	19,998	19,998	19,998
No. of cities	2,247	2,247	2,247	2,222	2,222	2,222

Note: Distance to nearest trade center is estimated using PPML. The city trade-center dummy is defined from distance to the nearest trade center, equal to one when the distance is below 0.001 km. The City trade center and the within-50-km outcomes are estimated using linear probability models. Columns (4), (5) and (6) exclude treated cities that already had a trade center before university treatment. All specifications use the Model 3b controls: city FE, NUTS0 (country) × century FE, geographic controls × century, and latitude × longitude × century interactions. Standard errors clustered at city level. Estimates are obtained using the Stata command jwdid. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

We consider three outcomes: the city distance to the nearest trade center; an indicator for whether the city itself is a trade center; and an indicator for whether there is a trade center within 50 km from the university city. Since lower distance implies better market access, negative coefficients in columns (1) and (4) indicate an improvement in the city's commercial position. The results seem to be consistent with the existence of a trade-related channel. Indeed, in the full sample, university establishment is associated with an approximately 6.5% reduction in distance to the nearest trade center, an 11.3 percentage-point increase in the probability that the city itself is a trade center, and a 9.4 percentage-point increase in the probability of having a trade center within 50 km. Interestingly, if we consider the impact of university establishment on the probability of having a trade center within 100 km, the ATT is positive but smaller and statistically insignificant, suggesting that spatial spillovers were delimited in space.

To address the concern that these patterns may simply reflect universities being founded in cities that were already commercial hubs, columns (4)– (6) exclude treated cities that had a trade center before university establishment. The estimates become slightly smaller in absolute value but remain statistically significant at conventional levels.

Having shown that university establishment was followed by both increases in city population and an improvement in local market access, we next examine whether the complementarity between universities and trade networks was especially pronounced in maritime contexts. We do this by estimating our most complete specification accounting for proximity to major seas, which can be considered as largely predetermined with respect to university establishment. Specifically, we include the distance to each water body — alternatively, the coast, the Atlantic Ocean, the Mediterranean Sea, the Baltic Sea, and the North Sea, respectively — as an interaction term with the century and cohort interactions within the ETWFE framework, alongside its interaction with century dummies (in a specification equivalent to Model 3b).[28] A negative gradient across percentiles — that is, a declining ATT as distance increases — would indicate that proximity to a given water body amplified the demographic returns to

[28] For a visual representation of the cities close to the coast, Atlantic Ocean, Baltic and North Sea please see Map B2 of Appendix B.

university presence, consistent with a maritime trade channel linking university cities to broader commercial networks.

Table 8. Spatial heterogeneity — University effect by distance from major water bodies

	(1) Coast	(2) Atlantic Ocean	(3) Mediterranean Sea	(4) Baltic Sea	(5) North Sea
Average	0.177**	0.208***	0.195***	0.204***	0.230***
	(0.069)	(0.068)	(0.069)	(0.061)	(0.068)
10th percentile dist.	0.224**	0.430***	0.211**	0.276***	0.484***
(closest)	(0.102)	(0.097)	(0.096)	(0.085)	(0.116)
25th percentile dist.	0.191***	0.272***	0.201***	0.218***	0.300***
	(0.072)	(0.071)	(0.070)	(0.061)	(0.072)
Median dist.	0.170**	0.161**	0.192**	0.179***	0.153**
	(0.070)	(0.070)	(0.078)	(0.066)	(0.075)
75th percentile dist.	0.153*	0.077	0.190**	0.145*	0.087
	(0.080)	(0.081)	(0.086)	(0.082)	(0.089)
90th percentile dist.	0.147*	0.057	0.188**	0.121	0.063
(farthest)	(0.086)	(0.084)	(0.093)	(0.098)	(0.095)
Observations	20,223	20,223	20,223	20,223	20,223
No. of cities	2,247	2,247	2,247	2,247	2,247

Note: Each cell reports the average treatment effect (ATT) evaluated at the indicated distance percentile. A single PPML-ETWFE model includes log-distance to the water body as a continuous term interacted with century and cohort dummies; ATTs are evaluated at the stated percentiles of the log-distance distribution. Specification identical to Model 3b. Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. All specifications include city FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Estimates are obtained using the Stata command jwdid. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

The results presented in Table 8 reveal a clear and consistent pattern across all water bodies. The average ATT remains positive and statistically significant across all five distance dimensions, confirming that the baseline university effect is robust to the inclusion of geographic interaction terms. However, the heterogeneity across the distance distribution tells a more nuanced story. Indeed, proximity to the sea (column 1) is associated with a slowly declining university effect as we move farther away from the coast, a result mirroring the stronger impact of the printing press in port-cities found by Dittmar (2011). We also find some interesting heterogeneity across specific seas. Indeed, in the case of proximity to the Atlantic Ocean (column 2) and the North Sea (column 5)—the two bodies most closely associated with the commercial expansion of the early modern period—the estimated effects display a sharp and monotonic gradient, declining steeply as cities move further from the coast. This pattern is consistent with the possibility that integration into the Atlantic and North Sea trade networks amplified the demographic returns to university presence, consistent with the argument that universities facilitated commercial activity and legal-institutional development in cities already embedded in expanding maritime trade circuits (Cantoni & Yuchtman, 2014).

One illustrative case is Nantes, which helps clarify the mechanism behind this pattern. The University of Nantes was founded in 1460 and soon received fiscal and judicial privileges, placing higher education within the city's broader institutional development. Nantes was also exceptionally well positioned to benefit from the expansion of Atlantic commerce. From the early modern period, its port developed as a gateway between the Loire valley and the Atlantic, and by the eighteenth century the city had become one of France's leading maritime and colonial commercial centers, including through its central role in the Atlantic slave trade (Michon, 2008; Pétré-Grenouilleau, 2003). Consistently with this broader Atlantic expansion, Nantes grew from about 14,000 inhabitants in 1500 to 25,000 in 1600, 40,000 in 1700, and 77,000 in 1800 — an increase of roughly 450 percent, compared with about 180 percent for the average city in our sample over the same period. While this single case is clearly only illustrative, it is nevertheless consistent with our aggregate results.

The Baltic Sea (column 4) displays a similar but somewhat flatter gradient, with the effect remaining positive and significant up to the median of the distance distribution before fading at the upper percentiles. This pattern is consistent with the role of Hanseatic trade networks in sustaining urban growth in cities with university presence across Northern and Central Europe.

By contrast, when accounting for the proximity to the Mediterranean (column 3) a different pattern emerges, as the ATT is remarkably stable across the entire distance distribution, remaining positive and significant from the 10th through the 90th percentile with little gradient. This suggests that proximity to the Mediterranean did not differentially amplify the university effect, pointing to a more uniform relationship between university presence and urban growth in the Mediterranean world that does not depend on closeness to the sea.

To address the concern that maritime heterogeneity reflects broad differences between Atlantic/North Sea Europe and the rest of the continent, we re-estimate the specification restricting the sample to countries with Atlantic or North Sea coastlines. The results are very similar: the ATT evaluated at the 10th percentile of distance from the coast is 0.40, compared with only 0.13 at the 90th percentile. Taken together, these results seem consistent with the view that coastal universities may have contributed to the creation of human capital useful for maritime trade, including navigation, trade law, and mercantile practices. This interpretation is coherent with evidence on maritime trade and pre-industrial growth (de Pleijt & van Zanden, 2016; Koyama et al., 2019) and with the faster growth of European coastal cities over the sample period (Bosker et al., 2013).

Additional interesting results along these lines emerge when we explore, in Tables B9a–B9e, the ATTs by century for all water bodies. In the case of the North Sea and, especially, the Atlantic Ocean, cities located very close (i.e., the 10^{th} and the 25^{th} percentiles) to the coast experienced a marked increase in the population return to universities from 1600 onward, unlike cities located farther away, which indeed experienced a decline. This pattern suggests that universities might have complemented the development of human capital relevant for navigation, commercial arithmetic, and legal contracting, thereby facilitating participation in the emerging maritime trade circuits associated with the booming Atlantic trade from the sixteenth century, in line with the results in Acemoglu et al. (2005). By contrast, the effect of universities is more uniform with respect to coastal proximity in the Mediterranean, where university-provided human capital appears to have been valuable even beyond coastal cities, although some increase of the university returns for cities near the sea is also visible after 1600.

Importantly, these patterns are estimated while controlling for distance to the Atlantic and to the other water bodies interacted with century dummies, together with country-by-century fixed effects that should absorb at least part of the influence of Western European location, state capacity, and the broad institutional changes that were occurring after 1500 in some Northwestern European countries (Acemoglu et al., 2005).[29]

All in all, the fact that university presence is associated with larger city population growth near the Atlantic and the North Sea and, importantly, that this additional growth accelerates after 1500, unlike in cities located farther from these seas, where the role of universities seems to have declined over time, is therefore consistent with a complementarity between university-trained human capital and the expansion of northwestern maritime trade, rather than with a generic coastal advantage. More broadly, these findings speak to the debate on the Little Divergence. While Acemoglu et al. (2005) emphasize the rise of Atlantic trade combined with institutional change, De Pleijt and Van Zanden (2016) and Curtis et al. (2026b) stress the role of stronger human-capital accumulation in North-Atlantic Europe. Our results are consistent with the view that both mechanisms may have mattered, pointing to a complementarity between upper-tail human capital and the expansion of Atlantic and North Sea trade, consistent also with the trade channel highlighted by Cantoni and Yuchtman (2014) for the German case at the end of the Commercial Revolution.

6. Conclusions

Our study uses a staggered DiD design to examine the relationship between university establishment and city population in a large panel of European cities observed from the eleventh to the end of the eighteenth century. The main result is that, under the maintained DiD assumptions, the establishment of a university is associated with higher city population, with

[29] These results are confirmed if we restrict the sample only to western countries with access to the Atlantic Ocean or the North Sea.

an overall semi-elasticity implying an increase of about 20 percent. At the same time, the impact appears to peak in the early centuries and weaken thereafter, suggesting that the contribution of universities to urban growth might have diminished over time. We interpret the strong early association in light of the role of universities in providing legal and administrative human capital, which likely supported trade and contributed to the expansion and consolidation of the Commercial Revolution (Cantoni and Yuchtman, 2014). The subsequent decline is in turn consistent with de la Croix et al. (2024), who document a weakening of agglomeration and selection forces in the European academic market after 1650.

A central finding, however, is that this decline was not uniform across all universities and cities. Indeed, their demographic impact was concentrated along two related dimensions, namely higher academic quality and stronger commercial opportunities: estimated gains were concentrated among high-quality universities and were especially pronounced in cities close to maritime trade networks, most notably near the Atlantic and North Sea coasts after 1500. These results support a complementarity between upper-tail human capital and trade expansion and may help shed light on the Little Divergence within Europe. In line with this interpretation, treated cities became closer to major trade centers and were more likely to become trade centers themselves. Overall, these findings are consistent with the mechanism identified by Cantoni and Yuchtman (2014) but suggest that it may have operated more broadly across pre-industrial Europe.

We also find suggestive evidence that university effects were stronger in cities with free communal or participatory institutions, while scientific academies may have reinforced the demographic advantage of university cities in the eighteenth century.

These findings should be interpreted as evidence consistent with a university effect on urban growth under the maintained DiD assumptions, rather than as definitive causal evidence. Indeed, the absence of a natural experiment and the long treatment horizon prevent definitive identification. Nevertheless, the results are robust to a rich fixed effects structure, formal pre-trend tests, alternative estimators, and matched-sample analyses. Taken together, the evidence suggests that universities were an important institutional contributor to urban development in pre-industrial Europe, especially where academic quality was high, trade networks were strong, and local institutions could absorb high-skilled human capital. Future research could investigate more closely how university specialization, scale, and regional institutional environments shaped these effects.

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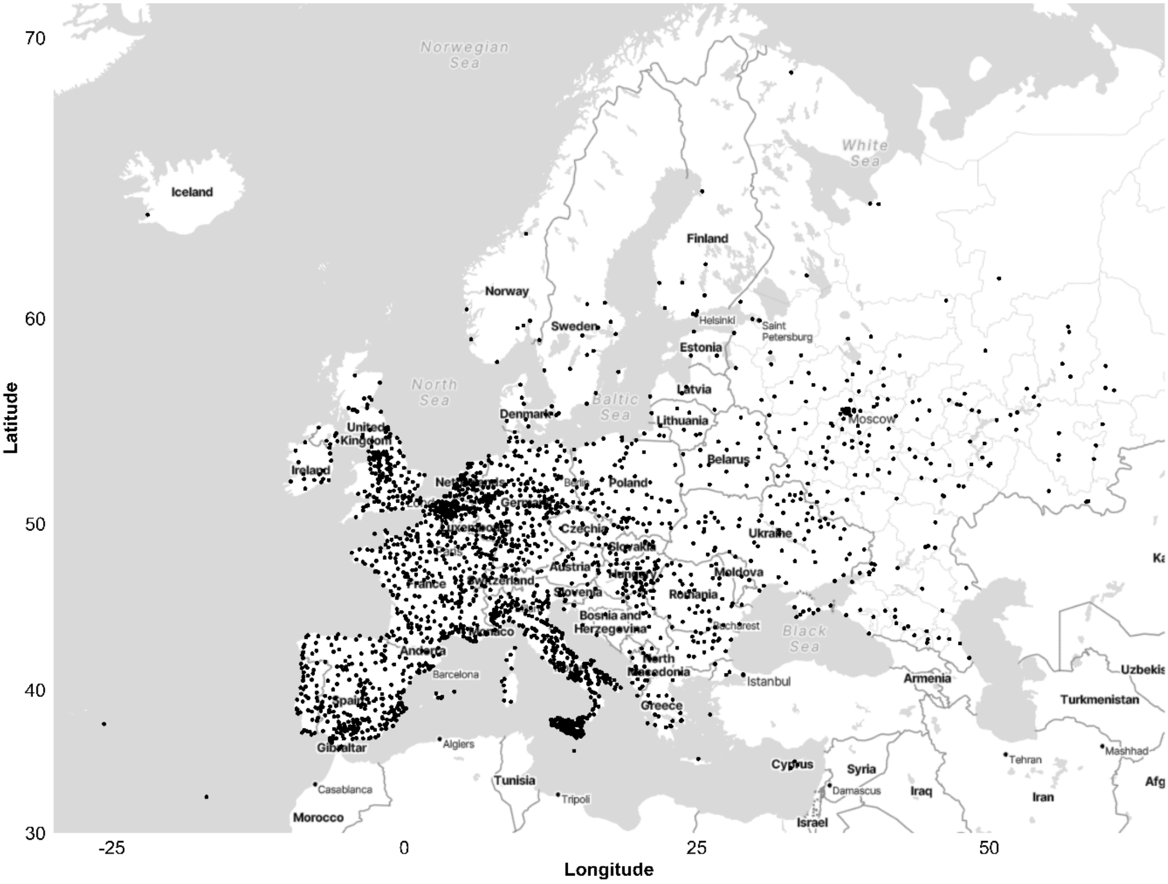
Appendix A. Sample, variables and descriptive evidence

The main text describes the sample definition and treatment-cohort construction. This appendix provides additional details on the database construction, university-quality measures, geographic variables, historical controls, and descriptive evidence used in the analysis.

The university-quality variables are constructed from de la Croix et al. (2024), who digitize the careers of European scholars from 1000 to 1800. Following their replication files, we reconstruct each professor's active period, assign scholars to cities through a consistent institution-city mapping, and aggregate the information at the university-century level. This yields four measures: the Quality Index of Universities (QIU), scholar mobility inflows and outflows and institutional size measured by affiliated professors in each university-century.

We also collect additional variables used in robustness checks and mechanisms: distances to bishoprics and archbishoprics from the Menestrel database; distance to trade centers from Wahl (2016); potato suitability from Bairoch et al. (1988); Black Death mortality from Bairoch et al. (1988); and distance to battles from the World Historical Battles Database (Kitamura, 2022). Summary statistics for these variables are reported in Table A3.

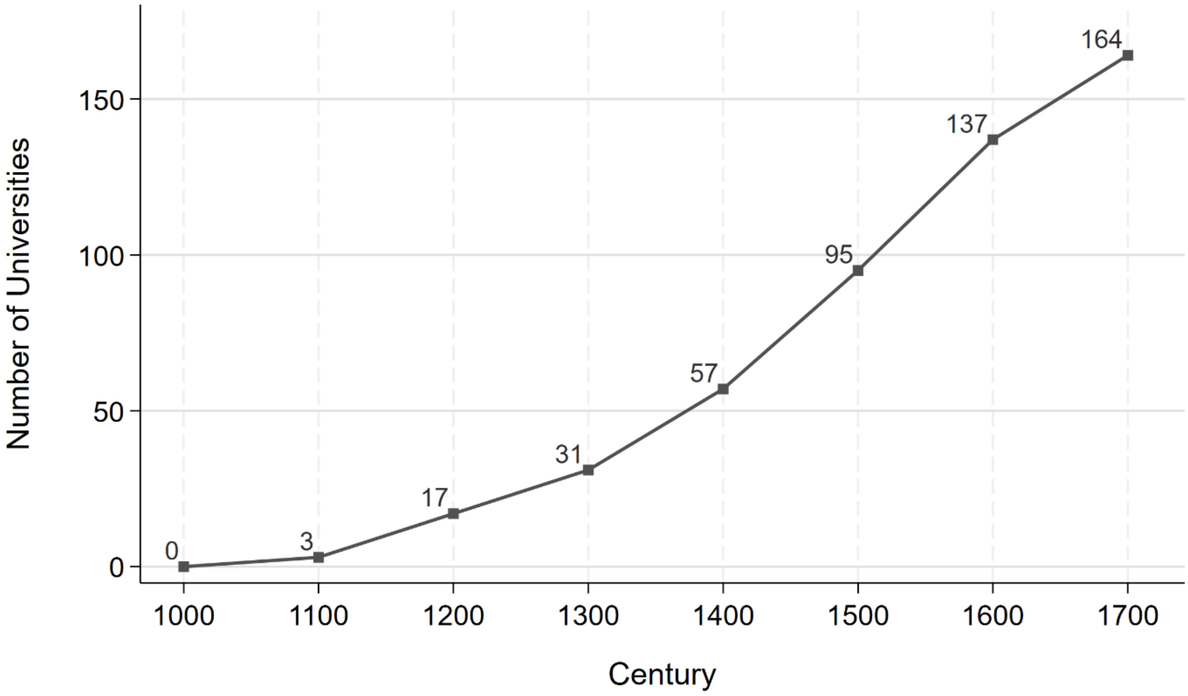
Map A1. European cities from 1000 to 1800



Note: Each dot represents a city included in the analysis panel. City locations and population data are drawn from Buringh (2021), covering 2,262 European cities over the period 700–2000 CE. The analysis sample restricts to 1000–1800 CE and comprises 2,247 unique cities.

Figure A1 presents a chronological distribution of universities across Europe, from 11th to 18th century, highlighting the gradual increase in the number of universities from the 12th century onwards. For clarity, each century label corresponds to the entirety of that century (i.e. 1000 refers to 1000-1099 etc.).

Figure A1. Cumulative number of universities over time



Note: The figure plots the cumulative number of universities operating in each century across the sample period. A university is counted as operating in a given century if it had been active for at least 50 years within that century. Century labels refer to the corresponding calendar century; for example, 1100 refers to 1100–1199. The three earliest universities (Bologna, Paris, and Oxford)— founded in 1088, 1096, and 1150 respectively— are plotted according to the first century in which they satisfy the 50-year operationalization rule for visual accuracy only. In the empirical analysis, however, they are grouped with the 1300 treatment cohort, since their foundation predates the start of our active treatment panel and a separate pre-1200 cohort would rest on only three cities. Table C4 in Appendix C shows that assigning these universities to a separate cohort leaves the results essentially unchanged. University data are drawn from de Ridder-Symoens (1992) and Rüegg and de Ridder-Symoens (1996). Suppressed universities are excluded.

To further enrich the analysis, we have created several indicators using geographical information system (GIS) shapefiles, sourced from the Menestrel database[30] and Harvard Dataverse.[31] Throughout this process, we have created variables to measure proximity to the Atlantic Ocean in order to assess the impact of transatlantic trade routes; coastal and river proximity as well as proximity to the Mediterranean Sea, the Baltic Sea and the North Sea, so as to capture the economic and strategic advantages of coastal access for cities and measure the economic benefits of navigable waters, as well as to reflect the importance of river

[30] https://www.menestrel.fr/?-Ressources-&lang=en

[31] DARMC Scholarly Data Series Citation: McCormick, M. et al. 2013. "Roman Road Network (version 2008)", DARMC Scholarly Data Series, Data Contribution Series #2013-5. DARMC, Center for Geographic Analysis, Harvard University, Cambridge MA 02138.

transportation and water resources for urban centers; distance to Wittenberg, famously associated with Martin Luther and the genesis of the Reformation, to evaluate the impact of spread of Reformation ideas and the development of educational institutions in the vicinity; and proximity to Roman roads, to explore the historical infrastructure's role in urban development. All distance measures enter the regressions in natural logarithm form and interacted with century dummies.

The construction of these variables was manually validated, ensuring that they are not only contextually relevant but also robust in capturing the essence of urban expansion and educational influence on socioeconomic development. The resulting dataset, a synthesis of Buringh's (2021) population data and the newly created variables from shapefiles, forms the informational foundation upon which this research is based.

Table A1 provides descriptive statistics for all the aforementioned variables, for different groups of cities, distinguished by the presence or absence of a university. It is evident that the cities with a university exhibit a significantly higher average population size with a greater standard deviation, while the distances to various geographical features are generally shorter on average. This indicates that cities in which universities were established are more likely to be located in geographically advantageous areas while cities without a university (never treated) tend to have smaller population on average compared to cities with universities.

Table A1. Summary statistics for treated and non-treated cities

Panel A. Treated cities							
Variables	Obs	Mean	Std.Dev.	Min	Max	Skew.	Kurt.
Population	1,476	18.00	46.11	0.00	700.00	8.81	104.75
Distance from:							
Coast	1,476	150.25	153.06	0.04	636.05	1.03	3.28
Atlantic Ocean	1,476	450.39	394.20	0.39	1,853.61	0.98	3.41
Baltic sea	1,476	931.13	619.02	0.01	3,568.20	0.77	3.85
Black sea	1,476	1,722.48	616.12	26.20	4,220.40	0.51	3.85
Mediterranean Sea	1,476	474.03	433.53	0.07	1,984.76	1.03	3.81
North Sea	1,476	716.45	508.71	0.39	2,918.19	0.71	3.83
River	1,476	46.97	110.85	0.02	1,294.60	9.08	99.17
Wittenberg	1,476	937.65	565.86	0.00	3,534.83	0.96	4.64
Roman roads	1,476	109.68	264.35	0.00	1,515.58	3.41	15.18
QIU	1,476	0.81	1.62	0.00	8.16	2.15	6.79
Inflows	1,476	0.80	7.26	0.00	140.00	15.92	279.27
Outflows	1,476	3.80	10.67	0.00	144.00	5.00	38.71
Size	1,476	25.96	71.62	0.00	651.16	4.32	25.55
No. of faculties	1,476	1.35	2.02	0.00	5.00	0.99	2.16
Panel B. Non-treated cities							
Variables	Obs	Mean	Std.Dev.	Min	Max	Skew.	Kurt.
Population	18,747	3.67	10.57	0.00	948.00	49.99	3910.84
Distance from:							
Coast	18,747	174.76	213.23	0.00	1,244.61	1.76	6.39
Atlantic Ocean	18,747	687.97	690.01	0.00	3,166.88	0.98	3.23
Baltic sea	18,747	1,013.32	587.36	0.00	3,566.82	0.45	2.69
Black sea	18,747	1,587.69	735.09	0.00	4,485.00	0.04	2.54
Mediterranean Sea	18,747	623.98	585.03	0.00	2,991.89	1.18	4.41
North Sea	18,747	921.95	695.06	0.00	3,229.42	0.49	2.65
River	18,747	46.69	70.21	0.03	1,463.48	10.96	190.42
Wittenberg	18,747	1,142.05	602.41	28.51	3,532.78	0.54	3.01
Roman roads	18,747	201.85	423.32	0.00	2,513.95	2.59	9.37

Table A2a. List of universities by cohort (1300-1599)

cohort 1300[a]	cohort 1400	cohort 1500
Bologna (IT)	Avignon (FR)	Aix (FR)
Oxford (UK)	Cahors (FR)	Bordeaux (FR)
Paris (FR)	Camerino (IT)	Caen (FR)
Angers (FR)	Coimbra (PT)	Catania (IT)
Arezzo (IT)	Firenze (IT)	Durres (AL)
Cambridge (UK)	Lisboa (PT)	Erfurt (DE)
Modena (IT)	Macerata (IT)	Ferrara (IT)
Montpellier (FR)	Murcia (ES)	Heidelberg (DE)
Napoli (IT)	Northampton (UK)	Huesca (ES)
Orleans (FR)	Perpignan (FR)	Koln (DE)
Padova (IT)	Perugia (IT)	Krakow (PL)
Salamanca (ES)	Pisa (IT)	Leipzig (DE)
Salerno (IT)	Praha (CZ)	Leuven (BE)
Siena (IT)	Roma (IT)	Lucca (IT)
Toulouse (FR)		Marseille (FR)
Valladolid (ES)		Orange (FR)
Vicenza (IT)		Pavia (IT)
		Pecs (HU)
		Parma (IT)
		Poitiers (FR)
		Rostock (DE)
		St Andrews (UK)
		Torino (IT)
		Wien (AT)
		Wuerzburg (DE)
		Zadar (HR)

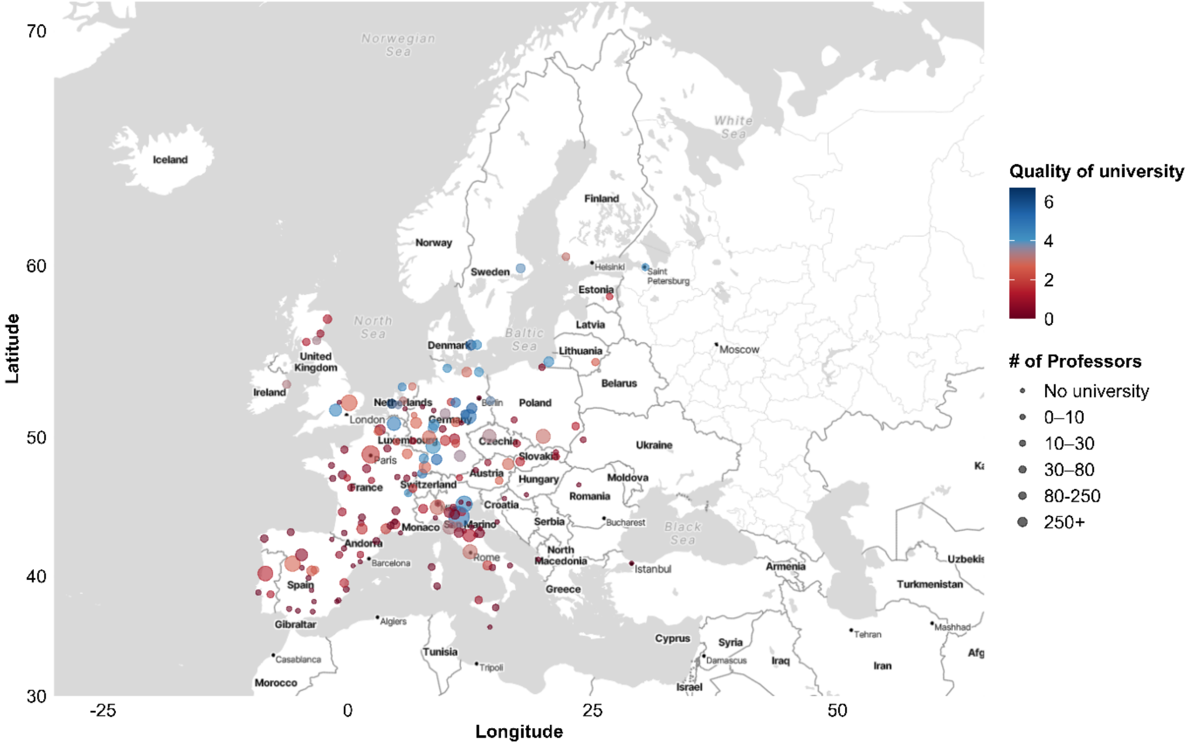
[a] The Universities of Bologna (1088), Oxford (1096), and Paris (c. 1150) are assigned to the 1300 cohort.

Table A2b. List of universities by cohort (1600-1899)

cohort 1600	cohort 1600	cohort 1700	cohort 1700	cohort 1800	cohort 1800
Aberdeen (UK)	Copenhagen (DK)	Ancona (IT)	Milano (IT)	Altamura (IT)	Linz (AT)
Alcala de Henares (ES)	Lausanne (CH)	Avila (ES)	Montauban (FR)	Berlin (DE)	Lund (SE)
Almagro (ES)	Mainz (DE)	Bamberg (DE)	Muenster (DE)	Besancon (FR)	Lvov (UA)
Baeza (ES)	Marburg (DE)	Braniewo (PL)	Olomouc (CZ)	Braunschweig (DE)	Montbeliard (FR)
Basel (CH)	Nantes (FR)	Cagliari (IT)	Oviedo (ES)	Cervera (ES)	Nijmegen (NL)
Bourges (FR)	Nimes (FR)	Douai (FR)	Paderborn (DE)	Dijon (FR)	Pau (FR)
Cluj (RO)	Orihuela (ES)	Dublin (IE)	Palermo (IT)	Duisburg (DE)	Presov (SK)
Frankfurt am Oder (DE)	Osuna (ES)	Edinburgh (UK)	Pamplona (ES)	Erlangen (DE)	Rennes (FR)
Freiburg (DE)	Reims (FR)	Evora (PT)	Pont a Mousson (FR)	Fulda (DE)	San Cristobal de la Laguna (ES)
Gandia (ES)	Santiago (ES)	Franeker (NL)	Salzburg (AT)	Goettingen (DE)	St Petersburg (RU)
Genova (IT)	Sevilla (ES)	Geneve (CH)	Sassari (IT)	Halle (DE)	Urbino (IT)
Glasgow (UK)	Strasbourg (FR)	Giessen (DE)	Saumur (FR)	Innsbruck (AT)	Wroclaw (PL)
Granada (ES)	Toledo (ES)	Graz (AT)	Sedan (FR)	Kiel (DE)	Zagreb (HR)
Greifswald (DE)	Trier (DE)	Groningen (NL)	Tarragona (ES)	Kosice (SK)	
Ingolstadt (DE)	Tuebingen (DE)	Harderwijk (NL)	Tartu (EE)		
Istanbul (TR)	Uppsala (SE)	Helmstedt (DE)	Tortosa (ES)		
Kaliningrad (RU)	Valence (FR)	Iena (DE)	Trnava (SK)		
	Valencia (ES)	Leiden (NL)	Turku (FI)		
	Venezia (IT)	Madrid (ES)	Utrecht (NL)		
	Wittenberg (DE)	Mantua (IT)	Valletta (MT)		
	Zaragoza (ES)		Vilnius (LT)		
			Zamosc (PL)		

Map A2 illustrates the geographic concentration of universities and highlights cross-regional variation in institutional quality and professorial capacity.

Map A2. Geographical Distribution of European Universities by quality and size (1300-1800)

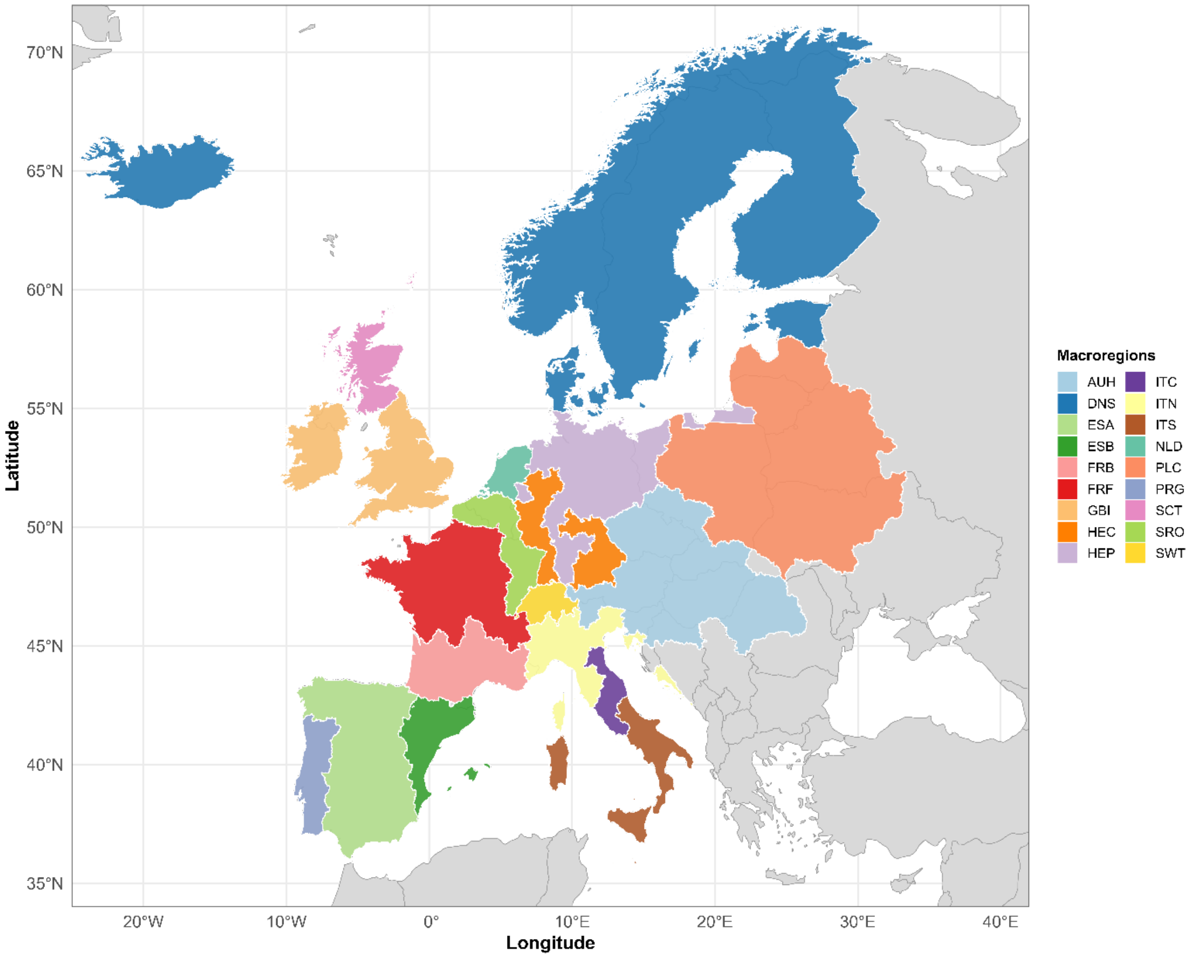


Note: Each dot represents one university city. Dot size reflects the total number of professors across all centuries the city hosted a university. Color reflects the median Quality Index of Universities (QIU) across those centuries (based on de la Croix et al., 2024); red indicates lower quality and dark blue indicates higher quality. Suppressed universities are excluded.

Table A3. Descriptive statistics for additional variables used in the analysis

	N	Mean	SD	Min	Max	Skewness	Kurtosis
Dist. Roman/Greek city (km)	20,223	223.17	426.17	0.02	2,476.14	2.66	10.01
Is archbishopric (0/1)	20,223	0.02	0.14	0.00	1.00	7.08	51.10
Dist. nearest archbishopric (km)	20,223	409.53	597.99	0.02	3,313.63	2.28	7.76
Is bishopric (0/1)	20,223	0.12	0.32	0.00	1.00	2.39	6.72
Dist. nearest bishopric (km)	20,223	262.68	488.94	0.02	3,238.75	2.63	9.89
Transport/water catchment location	20,223	16.07	5.66	1.00	26.00	-0.31	2.20
Is medieval trade center (0/1)	2,565	0.18	0.38	0.00	1.00	1.70	3.89
Centuries as trade center	2,565	0.85	2.16	0.00	11.00	2.77	10.12
Dist. nearest trade center (km)	20,223	426.70	482.19	0.00	2,755.10	1.85	6.73
Black Death mortality (%)	12,978	41.56	13.27	4.54	96.09	-0.10	2.54
Potato suitability (log)	12,978	1.65	0.17	0.99	2.07	-1.06	4.11
Dist. nearest battle (km)	20,223	184.22	300.60	0.00	3,321.83	4.59	30.54
Ever changed religion (0/1)	20,223	0.02	0.13	0.00	1.00	7.71	60.43
Current affiliation: Catholic (0/1)	20,223	0.06	0.24	0.00	1.00	3.63	14.17
No. of religious affil. Changes	20,223	0.00	0.07	0.00	3.00	22.51	634.75
University founded by Church (0/1)	20,223	0.06	0.24	0.00	1.00	3.62	14.12
Academy present in century (0/1)	20,223	0.01	0.11	0.00	1.00	9.17	85.18

Map A3. Historical Macroregions of Europe



Note: Colors denote the 18 historical macroregions defined by Curtis et al. (2026a). Areas outside any macroregion boundary are shown in grey. AUH Austrian Habsburg Lands, DNS Scandinavia, ESA Levante, ESB Castilla, FRB Occitania, FRF Francia, GBI English Realms, HEC Tridentine Germania, HEP Evangelical Germania, ITC Central Italy, ITN Northern Italy, ITS Southern Italy, NLD Netherlands, PLC Poland-Lithuania, PRG Portugal, SCT Scotland, SRO Lotharingian corridor, and SWT Switzerland.

Table A4. University quality over time

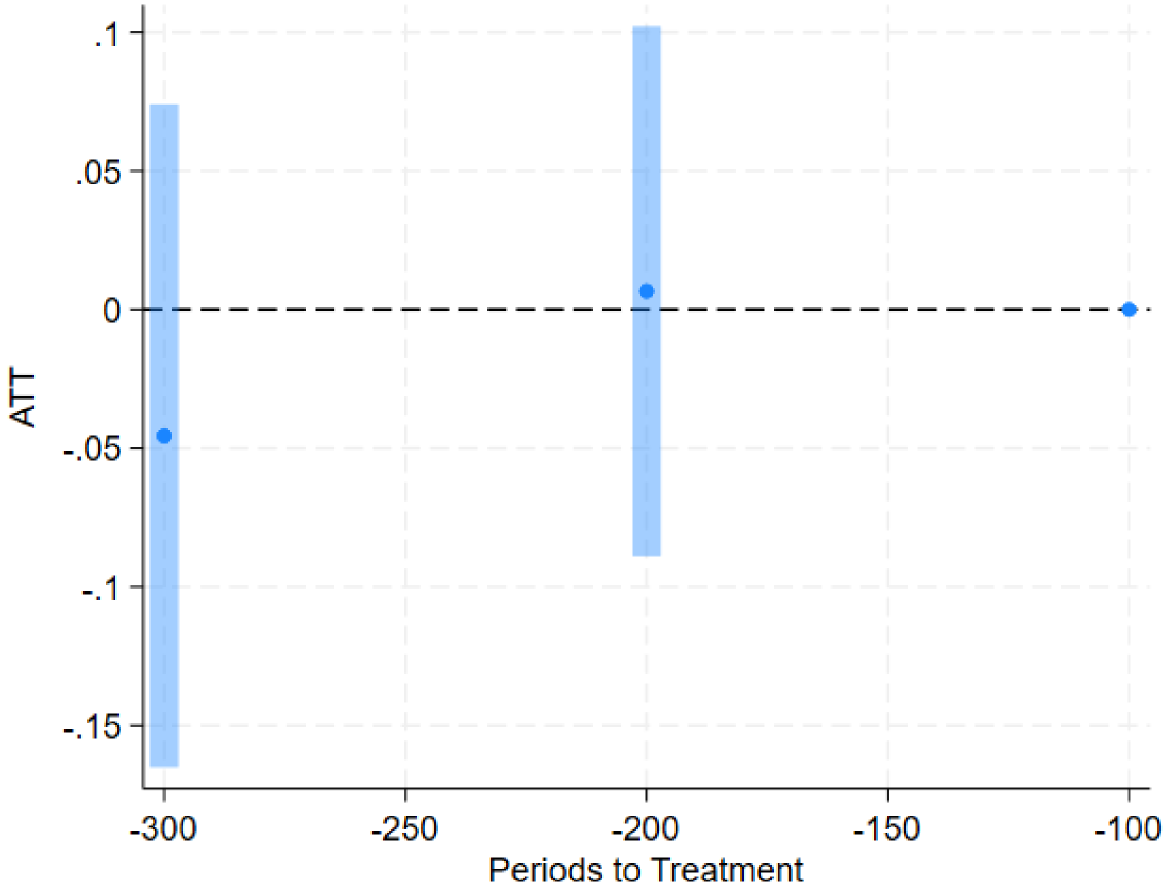
	Panel A. All universities			Panel B. New universities		
Century	Mean QIU	Median QIU	N	Mean QIU	Median QIU	N
1300	0.76	0.25	17	0.76	0.25	17
1400	0.83	0.13	31	0.18	0.01	14
1500	0.83	0.13	57	0.69	0.08	26
1600	1.66	1.02	95	1.39	0.18	38
1700	1.90	1.33	137	1.66	1.14	42
1800	1.74	0.96	164	1.31	0.54	27

Table A5. Regional Classification of countries

Western Europe		Eastern Europe	
Andorra	Italy	Albania	Moldova
Austria	Luxembourg	Belarus	Montenegro
Belgium	Malta	Bosnia & Herzegovina	North Macedonia
Cyprus	Netherlands	Bulgaria	Poland
Denmark	Norway	Croatia	Romania
Finland	Portugal	Czech Republic	Russia
France	Spain	Estonia	Serbia
Germany	Sweden	Hungary	Slovakia
Greece	Switzerland	Kosovo	Slovenia
Iceland	United Kingdom	Latvia	Turkey
Ireland		Lithuania	Ukraine

Appendix B. Additional figures and diagnostic tests

Figure B1. Pre-treatment parallel trends test — CEM-matched sample



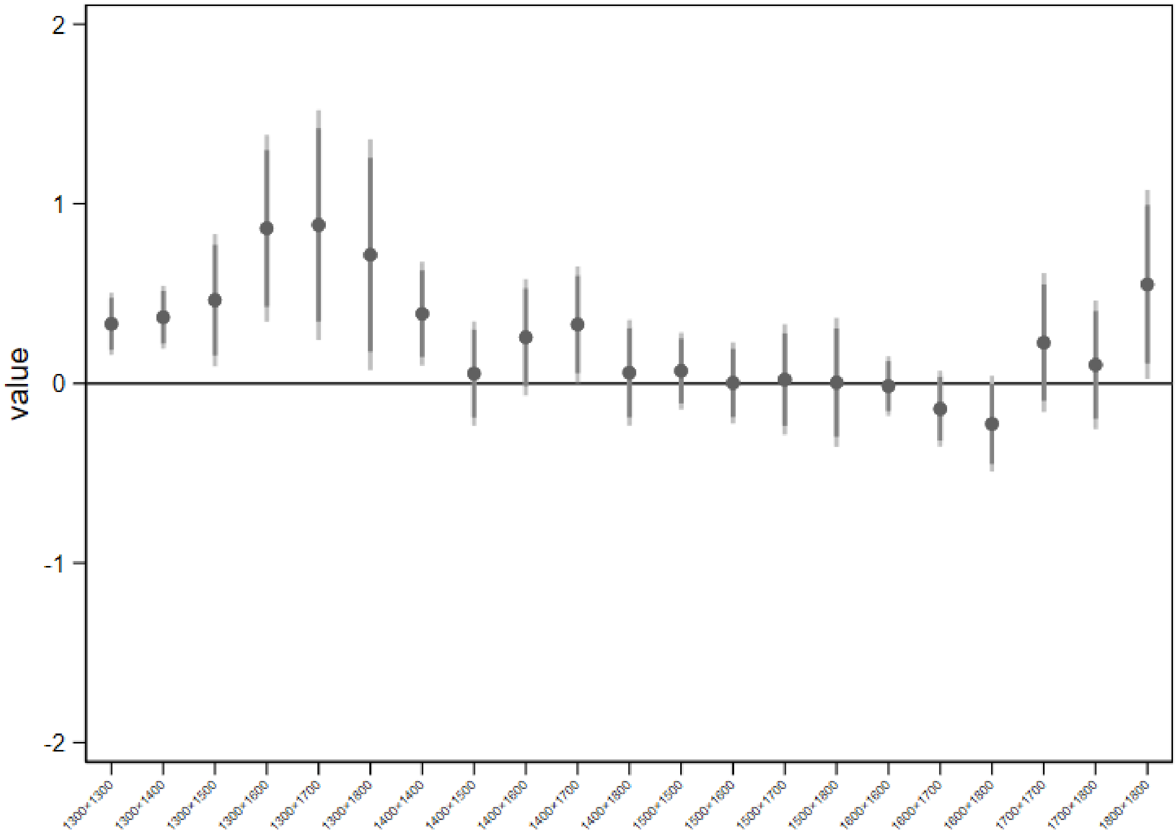
Note: Same specification as in Figure 1, restricted to the CEM-matched sample (231 treated cities matched on pre-treatment population, trade centrality, ecclesiastical status, geography, and battle proximity).

Table B1. HonestDiD sensitivity analysis using the recent pre-treatment window

(M)	95% CI lower bound	95% CI upper bound	90% CI lower bound	90% CI upper bound
Original	0.062	0.298	0.081	0.279
0.0	0.063	0.296	0.082	0.277
0.5	0.039	0.318	0.058	0.298
1.0	0.007	0.347	0.029	0.325
1.5	-0.034	0.383	-0.004	0.358
2.0	-0.076	0.427	-0.040	0.391
2.5	-0.124	0.473	-0.080	0.431
3.0	-0.173	0.522	-0.121	0.472

Note: The table reports HonestDiD sensitivity bounds for the treatment-onset event-study coefficient. The calibration of deviations from parallel trends uses only the recent pre-treatment window, namely the estimated event-study coefficients two and three centuries before treatment. The century immediately before treatment is the omitted reference period. M denotes the maximum allowed post-treatment violation of parallel trends relative to the largest violation observed in this recent pre-treatment window. Bounds are computed using the relative-magnitude restriction with 95 and 90 percent confidence intervals. The result remains statistically different from zero when the allowed violation is as large as the largest recent pre-treatment deviation (M = 1.0), while the 95 percent confidence interval marginally includes zero when the allowed violation equals one and a half times that deviation (M = 1.5).

Figure B2. Cohort-by-century treatment effect coefficients



Note: Raw cohort × post-treatment century DiD coefficients from Model 3b. Dark and light whiskers are 90% and 95% confidence intervals respectively. Standard errors clustered at city level.

Table B2. Average treatment effects by century — extended macroregion fixed effects

	(1) Model 2a	(2) Model 2b	(3) Model 2a (extended Hist. macroregions)	(4) Model 2b (extended Hist. macroregions)
ATT	0.203***	0.229***	0.208***	0.233***
	(0.064)	(0.066)	(0.063)	(0.067)
By century				
1300	0.282***	0.374***	0.289***	0.373***
	(0.073)	(0.083)	(0.071)	(0.086)
1400	0.380***	0.436***	0.382***	0.436***
	(0.071)	(0.076)	(0.071)	(0.079)
1500	0.187***	0.250***	0.170**	0.233***
	(0.072)	(0.074)	(0.071)	(0.078)
1600	0.236***	0.270***	0.202***	0.243***
	(0.066)	(0.068)	(0.066)	(0.069)
1700	0.224**	0.230**	0.225***	0.230**
	(0.090)	(0.092)	(0.087)	(0.091)
1800	0.129	0.141	0.170*	0.176*
	(0.088)	(0.088)	(0.091)	(0.092)
Observations	16,875	16,875	20,223	20,223
No. of cities	1,875	1,875	2,247	2,247
Geo. controls × century	No	Yes	No	Yes
City FE	Yes	Yes	Yes	Yes
Hist. regions × century FE as per Curtis et. al (2026a)	Yes	Yes	No	No
Extended Hist. regions × century FE	No	No	Yes	Yes

Note: Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. “ATT” is the overall average across all cohorts and post-treatment centuries; “By century” rows are calendar-time ATTs aggregated across cohorts. All specifications include city FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to coast, river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table B3. Balance test on the CEM covariates

Variable	Type	Mean treated	Mean control	% Bias	% Bias reduction	t-stat	p-value
Population (lag)	U	18.00	3.67	42.82		-32.94	0.00
	M	8.25	6.38	25.58	40.26	-1.97	0.05
Transport location	U	17.83	15.93	33.08		-12.45	0.00
	M	17.83	16.86	16.69	49.55	-1.28	0.20
Trade center dummy	U	0.37	0.12	60.28		-14.17	0.00
	M	0.18	0.14	11.55	80.84	-0.48	0.63
Centuries of trade	U	1.68	0.62	45.26		-10.48	0.00
	M	0.45	0.39	4.18	90.76	-0.17	0.86
Proximity to archbishoprics	U	220.82	424.39	-42.77		12.64	0.00
	M	207.49	191.51	6.79	84.12	-0.52	0.60
Proximity to bishoprics	U	93.45	276.01	-48.07		13.88	0.00
	M	75.56	78.39	-1.89	96.07	0.15	0.88
Proximity to coast	U	150.25	174.76	-13.21		4.33	0.00
	M	147.23	144.73	1.71	87.03	-0.13	0.90
Proximity to river	U	46.97	46.69	0.30		-0.14	0.89
	M	49.62	48.88	0.59		-0.05	0.96
Proximity to Roman roads	U	109.68	201.85	-26.12		8.24	0.00
	M	102.22	90.54	4.82	81.53	-0.37	0.71
Proximity to battles	U	124.17	188.95	-26.21		7.98	0.00
	M	71.58	71.42	0.17	99.36	-0.01	0.99
Proximity to Roman/Greek cities	U	125.54	230.85	-29.39		9.16	0.00
	M	122.03	114.31	3.27	88.88	-0.25	0.80

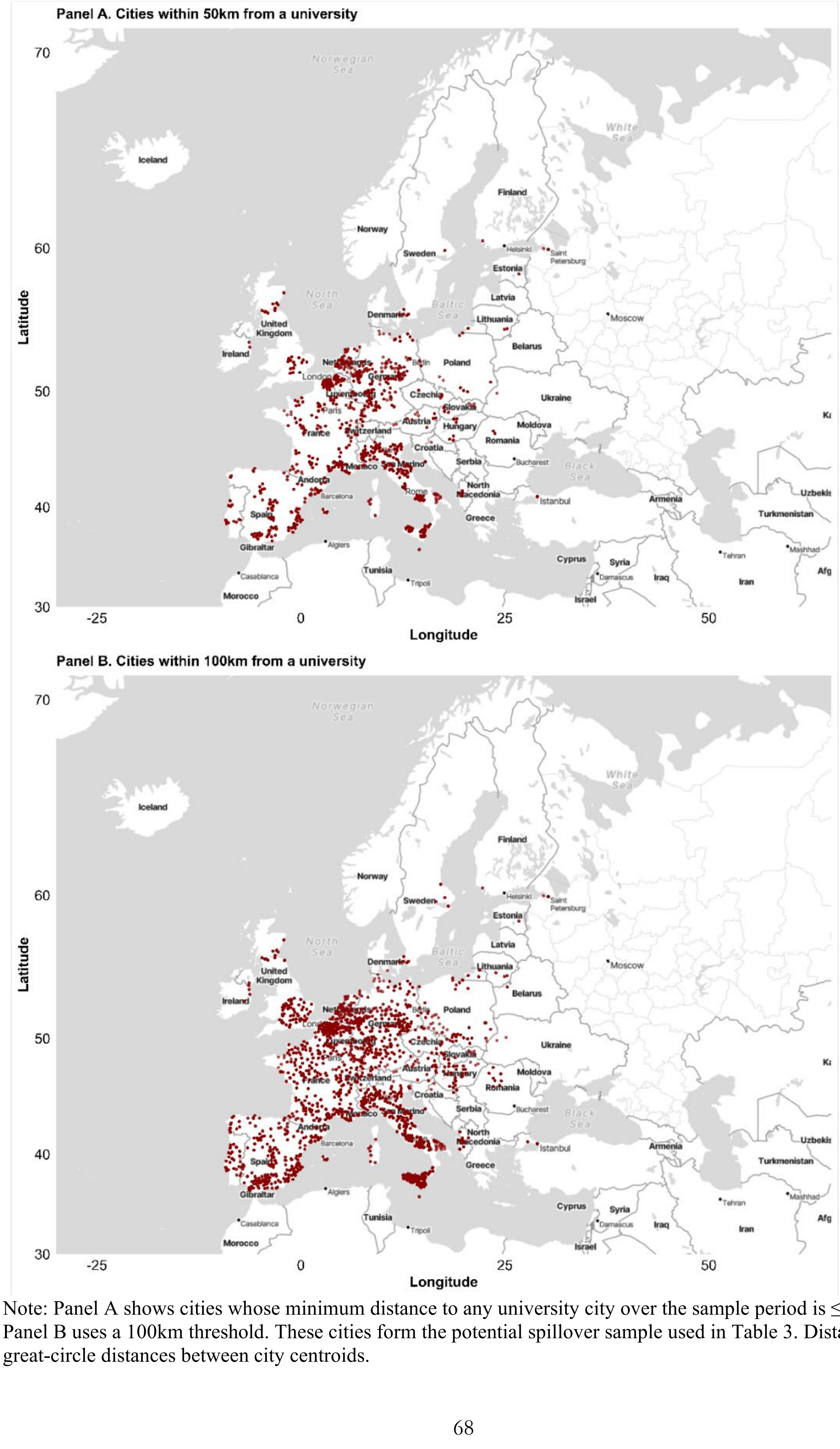
Note: U = unmatched sample; M = CEM-matched sample. % Bias is the standardized percentage bias. t-statistics and p-values are from two-sample t-tests with equal variances testing equality of means between treated and control cities. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Table B4. Pre-treatment balance: population at cohort entry

Cohort	Treated	Control	Difference	t-stat	p-value
1200	7.10	8.36	-1.26	0.58	0.568
1300	5.33	3.80	1.53	-0.86	0.400
1400	8.07	5.69	2.38	-1.24	0.226
1500	4.68	3.23	1.45*	-1.87	0.067
1600	7.23	5.46	1.77	-1.26	0.214
1700	8.52	5.71	2.81	-1.10	0.280

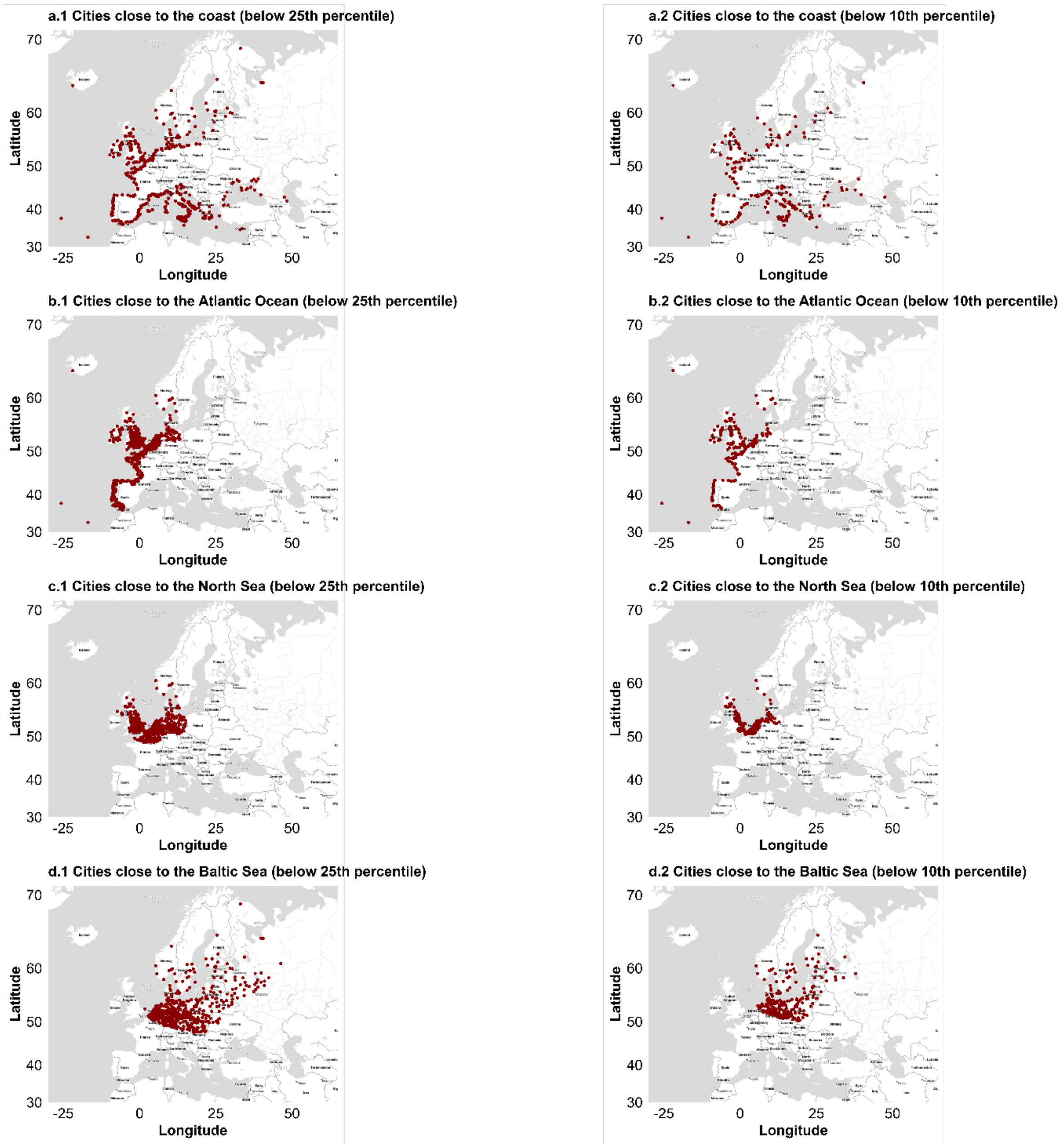
Note: Population measured in thousands in the period immediately preceding each cohort's treatment date. Treated = CEM-matched university cities; Control = matched never-treated cities. Two-sample t-test assuming equal variances. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.

Map B1. Cities within 50km (Panel A) or 100km (Panel B) from a university



Note: Panel A shows cities whose minimum distance to any university city over the sample period is ≤ 50km; Panel B uses a 100km threshold. These cities form the potential spillover sample used in Table 3. Distances are great-circle distances between city centroids.

Map B2. Cities close to a) coast b) Atlantic Ocean c) North Sea d) Baltic Sea



Note: Each panel shows cities below the 25th percentile (panels x.1) and 10th percentile (panels x.2) of log-distance to the respective coastline or sea boundary. These distance measures correspond to the spatial heterogeneity terms interacted with century and cohort dummies analyzed in Table 8. Distances are computed as great-circle distances from city centroids to the nearest point on the respective coastline.

Table B5. University quality over time — Cross-sectional evidence

	(1) 1800 cross-section	(2) 1800 + city controls	(3) 1700 cross-section	(4) 1700 + city controls
1400 cohort	-1.790*	-0.868	-1.183	-0.352
	(0.912)	(1.224)	(0.779)	(0.761)
1500 cohort	-1.647**	-1.314	-1.686***	-1.122
	(0.810)	(1.000)	(0.585)	(0.699)
1600 cohort	-2.085***	-1.609	-1.746***	-1.328*
	(0.750)	(1.005)	(0.576)	(0.690)
1700 cohort	-2.660***	-2.116**	-2.163***	-1.232*
	(0.752)	(1.059)	(0.569)	(0.724)
1800 cohort	-2.393***	-1.712		
	(0.821)	(1.138)		
N	152	127	122	102
R-squared	0.400	0.563	0.475	0.695

Note: OLS regressions where the outcome is QIU in the reference century (1800 or 1700), among ever-treated cities with an active university. Cohort dummies (base: 1300-cohort) capture quality differentials relative to the earliest treated group. Cols. (1) and (3) absorb NUTS0 (country) fixed effects. Cols. (2) and (4) add lagged population and geographic controls and NUTS1 FE. Robust standard errors. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Figure B3. Pre-treatment population paths of very high-quality university cities relative to other treated university cities

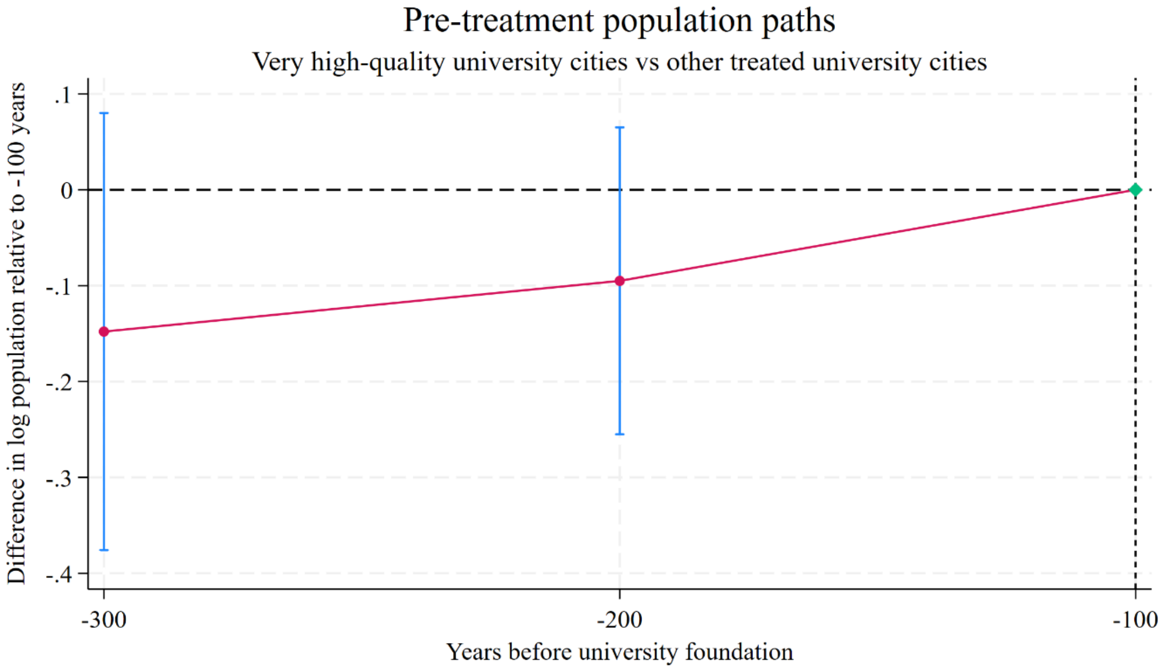


Table B6. Pre-treatment characteristics of high- and low-QIU university cities

Variable	Low-QIU mean	High-QIU mean	Raw diff.	Cohort-adjusted diff.
Lagged population	12.683	10.427	-2.256	-2.190
Transport location	16.988	18.671	1.683	1.882**
Trade center	0.414	0.394	-0.020	-0.048
Century became trade center	1.207	2.000	0.793	0.486
Distance to archbishopric	170.494	247.891	77.397	47.792
Distance to bishopric	74.716	86.289	11.573	-2.146
Distance to coast	97.884	202.615	104.730	95.598***
Distance to navigable river	63.485	30.451	-33.034	-44.589*
Distance to Roman road	53.863	165.490	111.628	82.402**
Distance to battle site	95.171	54.151	-41.020	-42.056**
Distance to Roman or Greek city	64.919	186.163	121.244	90.654**

Table B7. Differential pre-treatment population trends among treated university cities, by university quality

Event time	Coefficient	Standard error	p-value
−300 years	−0.148	0.115	0.202
−200 years	−0.095	0.081	0.243
Joint test		**F-statistic**	**p-value**
−300 years = −200 years = 0		0.852	0.429

Table B8. Additional robustness: The role of Free communes

	(1) Baseline	(2) Full: Non-commune	(3) Full: Commune	(4) Commune control
ATT	0.187***	0.120*	0.259***	0.189***
	(0.068)	(0.066)	(0.078)	(0.067)
By century				
1300	0.331***	0.341***	0.409***	0.340***
	(0.088)	(0.087)	(0.094)	(0.087)
1400	0.377***	0.363***	0.400***	0.381***
	(0.084)	(0.099)	(0.081)	(0.084)
1500	0.183**	0.148*	0.222***	0.186**
	(0.081)	(0.078)	(0.084)	(0.081)
1600	0.187**	0.085	0.274***	0.191***
	(0.074)	(0.065)	(0.088)	(0.073)
1700	0.177*	0.076	0.252***	0.181**
	(0.091)	(0.088)	(0.097)	(0.091)
1800	0.145	0.132	0.210**	0.144
	(0.089)	(0.097)	(0.101)	(0.089)
Observations	20,223	20,223	20,223	20,223
No. of cities	2,247	2,247	2,247	2,247

Note: Col 1 = baseline (full sample). Cols 2–3 = full sample, commune interacted with century and cohort; ATTs split by commune status. Col 4 = commune entered as time-varying covariate (additive control only). Standard errors clustered at city level. * p<0.10, ** p<0.05, *** p<0.01.

Table B9a. Spatial heterogeneity — University effect by distance from the coast

	(1) Average	(2) 10th percentile dist. (closest)	(3) 25th percentile dist.	(4) Median dist.	(5) 75th percentile dist.	(6) 90th percentile dist. (farthest)
ATT	0.177**	0.224**	0.191***	0.170**	0.153*	0.147*
	(0.069)	(0.102)	(0.072)	(0.070)	(0.080)	(0.086)
By century						
1300	0.320***	0.321***	0.320***	0.319***	0.319***	0.318***
	(0.086)	(0.110)	(0.088)	(0.087)	(0.095)	(0.100)
1400	0.358***	0.347***	0.355***	0.360***	0.365***	0.366***
	(0.084)	(0.112)	(0.087)	(0.086)	(0.094)	(0.100)
1500	0.171**	0.064	0.147**	0.202***	0.243***	0.260***
	(0.070)	(0.114)	(0.074)	(0.073)	(0.087)	(0.096)
1600	0.179**	0.129	0.167**	0.192***	0.210***	0.218***
	(0.074)	(0.112)	(0.080)	(0.073)	(0.079)	(0.083)
1700	0.155*	0.243**	0.178*	0.135	0.103	0.090
	(0.091)	(0.120)	(0.093)	(0.093)	(0.104)	(0.110)
1800	0.146	0.287**	0.188**	0.122	0.074	0.054
	(0.093)	(0.124)	(0.095)	(0.094)	(0.104)	(0.110)
Observations	20,223	20,223	20,223	20,223	20,223	20,223
No. of cities	2,247	2,247	2,247	2,247	2,247	2,247

Note: A single PPML-ETWFE model includes log-distance to the water body as a continuous term interacted with century and cohort dummies; ATTs are evaluated at the stated percentiles of the log-distance distribution. Specification identical to Model 3b. Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. All specifications include city FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table B9b. Spatial heterogeneity — University effect by distance from the Atlantic Ocean

	(1) Average	(2) 10th percentile dist. (closest)	(3) 25th percentile dist.	(4) Median dist.	(5) 75th percentile dist.	(6) 90th percentile dist. (farthest)
ATT	0.208***	0.430***	0.272***	0.161**	0.077	0.057
	(0.068)	(0.097)	(0.071)	(0.070)	(0.081)	(0.084)
By century						
1300	0.338***	0.381***	0.355***	0.336***	0.322***	0.319***
	(0.086)	(0.109)	(0.089)	(0.086)	(0.090)	(0.092)
1400	0.373***	0.372***	0.373***	0.373***	0.373***	0.373***
	(0.077)	(0.102)	(0.081)	(0.078)	(0.083)	(0.086)
1500	0.159**	0.218**	0.178**	0.150*	0.128	0.123
	(0.078)	(0.106)	(0.082)	(0.078)	(0.084)	(0.086)
1600	0.189**	0.362***	0.242***	0.157**	0.093	0.078
	(0.074)	(0.103)	(0.079)	(0.074)	(0.079)	(0.081)
1700	0.208**	0.517***	0.295***	0.139	0.021	-0.007
	(0.090)	(0.113)	(0.090)	(0.094)	(0.107)	(0.112)
1800	0.190**	0.487***	0.276***	0.127	0.015	-0.011
	(0.091)	(0.124)	(0.093)	(0.094)	(0.107)	(0.112)
Observations	20,223	20,223	20,223	20,223	20,223	20,223
No. of cities	2,247	2,247	2,247	2,247	2,247	2,247

Note: A single PPML-ETWFE model includes log-distance to the water body as a continuous term interacted with century and cohort dummies; ATTs are evaluated at the stated percentiles of the log-distance distribution. Specification identical to Model 3b. Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. All specifications include city FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table B9c. Spatial heterogeneity — University effect by distance from the North Sea

	(1) Average	(2) 10th percentile dist. (closest)	(3) 25th percentile dist.	(4) Median dist.	(5) 75th percentile dist.	(6) 90th percentile dist. (farthest)
ATT	0.230***	0.484***	0.300***	0.153**	0.087	0.063
	(0.068)	(0.116)	(0.072)	(0.075)	(0.089)	(0.095)
By century						
1300	0.336***	0.415***	0.365***	0.325***	0.307***	0.301***
	(0.089)	(0.120)	(0.092)	(0.092)	(0.099)	(0.102)
1400	0.392***	0.399***	0.395***	0.392***	0.390***	0.390***
	(0.084)	(0.126)	(0.090)	(0.085)	(0.092)	(0.095)
1500	0.175**	0.357***	0.232***	0.132	0.088	0.071
	(0.078)	(0.120)	(0.082)	(0.082)	(0.092)	(0.097)
1600	0.234***	0.518***	0.322***	0.165**	0.094	0.068
	(0.075)	(0.122)	(0.080)	(0.080)	(0.091)	(0.097)
1700	0.228***	0.577***	0.320***	0.114	0.022	-0.012
	(0.088)	(0.131)	(0.090)	(0.098)	(0.112)	(0.119)
1800	0.206**	0.453***	0.269***	0.122	0.056	0.032
	(0.090)	(0.138)	(0.093)	(0.098)	(0.112)	(0.119)
Observations	20,223	20,223	20,223	20,223	20,223	20,223
No. of cities	2,247	2,247	2,247	2,247	2,247	2,247

Note: A single PPML-ETWFE model includes log-distance to the water body as a continuous term interacted with century and cohort dummies; ATTs are evaluated at the stated percentiles of the log-distance distribution. Specification identical to Model 3b. Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. All specifications include city FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table B9d. Spatial heterogeneity — University effect by distance from the Baltic Sea

	(1) Average	(2) 10th percentile dist. (closest)	(3) 25th percentile dist.	(4) Median dist.	(5) 75th percentile dist.	(6) 90th percentile dist. (farthest)
ATT	0.204***	0.276***	0.218***	0.179***	0.145*	0.121
	(0.061)	(0.085)	(0.061)	(0.066)	(0.082)	(0.098)
By century						
1300	0.337***	0.413***	0.374***	0.346***	0.323***	0.306***
	(0.090)	(0.127)	(0.099)	(0.090)	(0.092)	(0.099)
1400	0.373***	0.524***	0.448***	0.396***	0.352***	0.319***
	(0.085)	(0.131)	(0.098)	(0.086)	(0.087)	(0.094)
1500	0.172**	0.278**	0.209**	0.161**	0.120	0.091
	(0.079)	(0.110)	(0.083)	(0.080)	(0.089)	(0.101)
1600	0.240***	0.299***	0.253***	0.222***	0.195**	0.176
	(0.075)	(0.097)	(0.075)	(0.079)	(0.093)	(0.108)
1700	0.199***	0.260***	0.208***	0.173**	0.143	0.121
	(0.072)	(0.093)	(0.072)	(0.078)	(0.093)	(0.108)
1800	0.153**	0.214**	0.150**	0.107	0.070	0.043
	(0.071)	(0.086)	(0.071)	(0.082)	(0.102)	(0.119)
Observations	20,223	20,223	20,223	20,223	20,223	20,223
No. of cities	2,247	2,247	2,247	2,247	2,247	2,247

Note: A single PPML-ETWFE model includes log-distance to the water body as a continuous term interacted with century and cohort dummies; ATTs are evaluated at the stated percentiles of the log-distance distribution. Specification identical to Model 3b. Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. All specifications include city FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table B9e. Spatial heterogeneity — University effect by distance from the Mediterranean Sea

	(1) Average	(2) 10th percentile dist. (closest)	(3) 25th percentile dist.	(4) Median dist.	(5) 75th percentile dist.	(6) 90th percentile dist. (farthest)
ATT	0.195***	0.211**	0.201***	0.192**	0.190**	0.188**
	(0.069)	(0.096)	(0.070)	(0.078)	(0.086)	(0.093)
By century						
1300	0.325***	0.301***	0.326***	0.347***	0.354***	0.359***
	(0.088)	(0.103)	(0.088)	(0.100)	(0.107)	(0.114)
1400	0.380***	0.347***	0.383***	0.414***	0.423***	0.431***
	(0.078)	(0.092)	(0.078)	(0.093)	(0.101)	(0.108)
1500	0.171**	0.149	0.171**	0.190**	0.196*	0.200*
	(0.075)	(0.094)	(0.075)	(0.093)	(0.103)	(0.112)
1600	0.190**	0.139	0.183**	0.221***	0.233**	0.242**
	(0.078)	(0.106)	(0.079)	(0.086)	(0.093)	(0.100)
1700	0.210**	0.238**	0.215**	0.195**	0.189*	0.184
	(0.092)	(0.118)	(0.093)	(0.099)	(0.106)	(0.112)
1800	0.147*	0.217*	0.163*	0.116	0.102	0.091
	(0.088)	(0.116)	(0.089)	(0.096)	(0.104)	(0.111)
Observations	20,223	20,223	20,223	20,223	20,223	20,223
No. of cities	2,247	2,247	2,247	2,247	2,247	2,247

Note: A single PPML-ETWFE model includes log-distance to the water body as a continuous term interacted with century and cohort dummies; ATTs are evaluated at the stated percentiles of the log-distance distribution. Specification identical to Model 3b. Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. All specifications include city FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Appendix C. Additional robustness analysis

Table C1 shows that the baseline results are robust to spatial correlation concerns: they are unchanged when allowing for more flexible local spatial trends through higher-order latitude–longitude interactions and when clustering standard errors at the NUTS1 or NUTS2 level rather than at the city level.

Table C1. Spatial robustness — Polynomial trends and alternative clustering

	(1) Baseline (city-clustered)	(2) + Quadratic lat/lon interactions	(3) NUTS1-clustered SE	(4) NUTS1-clustered + quadratic	(5) NUTS2-clustered SE	(6) NUTS2-clustered + quadratic
ATT	0.187***	0.195***	0.187***	0.195***	0.187***	0.195***
	(0.068)	(0.067)	(0.069)	(0.068)	(0.065)	(0.064)
By century						
1300	0.331***	0.335***	0.331***	0.335***	0.331***	0.335***
	(0.088)	(0.094)	(0.092)	(0.096)	(0.087)	(0.093)
1400	0.377***	0.380***	0.377***	0.380***	0.377***	0.380***
	(0.084)	(0.085)	(0.071)	(0.072)	(0.076)	(0.077)
1500	0.183**	0.195**	0.183**	0.195**	0.183**	0.195**
	(0.081)	(0.081)	(0.079)	(0.079)	(0.085)	(0.085)
1600	0.187**	0.202***	0.187***	0.202***	0.187***	0.202***
	(0.074)	(0.073)	(0.068)	(0.066)	(0.066)	(0.066)
1700	0.177*	0.184**	0.177*	0.184**	0.177**	0.184**
	(0.091)	(0.091)	(0.093)	(0.093)	(0.089)	(0.089)
1800	0.145	0.150*	0.145	0.150*	0.145*	0.150*
	(0.089)	(0.088)	(0.091)	(0.090)	(0.086)	(0.084)
Observations	20,223	20,223	20,223	20,223	20,223	20,223
No. of cities	2,247	2,247	2,247	2,247	2,247	2,247

Note: "Quadratic" adds $lat^2 \times lon^2$ terms interacted with century dummies. Remaining columns replace city-level clustering with NUTS1 or NUTS2 clustering, with and without the quadratic trend. All other choices as in Model 3b. Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. "ATT" is the overall average across all cohorts and post-treatment centuries; "By century" rows are calendar-time ATTs aggregated across cohorts. All specifications include city FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to coast, river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

In Table C2 we examine the sensitivity of the baseline results to the definition of university operation. The baseline treatment indicator classifies a university as belonging to a particular cohort if it has been in operation for at least 50 years in the previous century, a threshold intended to allow the university to have enough time to influence city growth by the beginning of the following century. Columns 1, 2, 4, and 5 re-estimate the preferred Model 3b specification using alternative thresholds of 30, 40, 60, and 70 years, respectively. The results are remarkably stable across all five thresholds. The overall ATT ranges narrowly between 0.151 and 0.187, remaining positive and statistically significant at conventional levels in all cases.

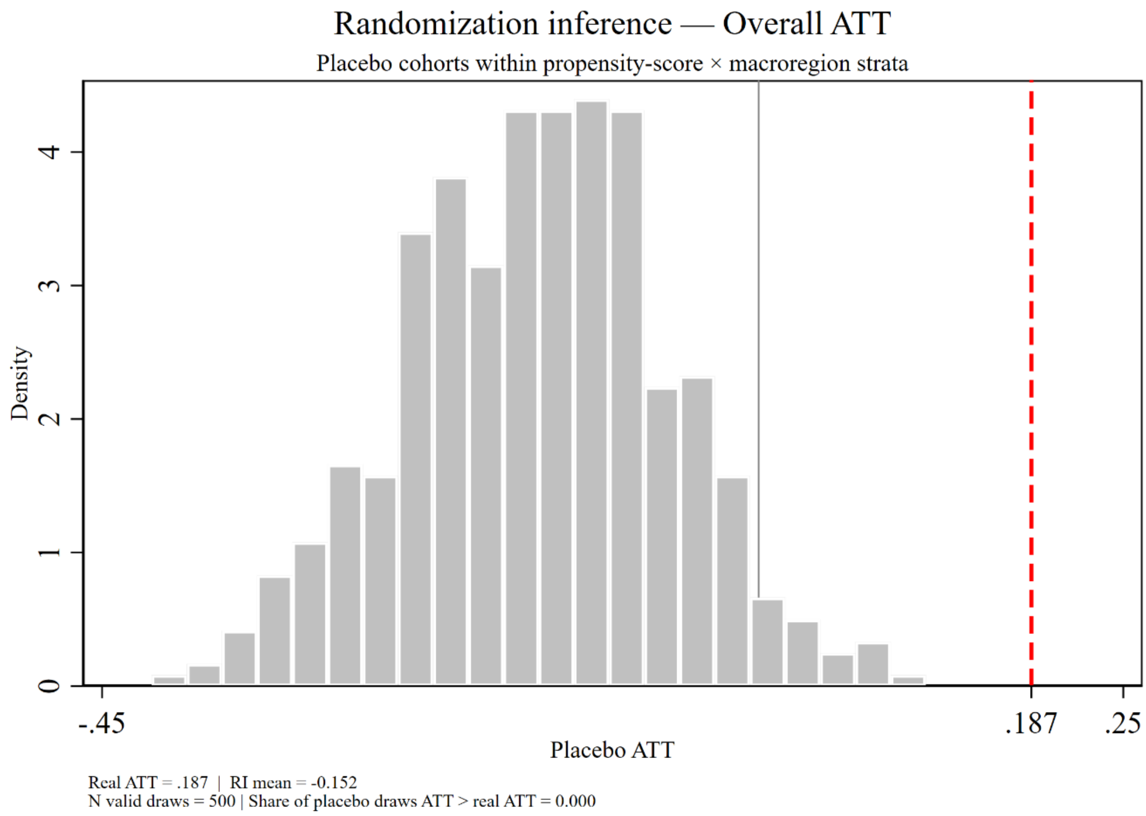
Table C2. Sensitivity to the university operation threshold

	(1) ≥30 years active	(2) ≥40 years active	(3) ≥50 years active (baseline)	(4) ≥60 years active	(5) ≥70 years active
ATT	0.162** (0.072)	0.151** (0.073)	0.187*** (0.068)	0.168** (0.067)	0.163** (0.066)
By century					
1300	0.325*** (0.088)	0.331*** (0.088)	0.331*** (0.088)	0.319*** (0.097)	0.338*** (0.101)
1400	0.330*** (0.083)	0.370*** (0.084)	0.377*** (0.084)	0.343*** (0.087)	0.351*** (0.088)
1500	0.074 (0.114)	0.021 (0.125)	0.183** (0.081)	0.124 (0.082)	0.097 (0.082)
1600	0.169** (0.077)	0.150** (0.075)	0.187** (0.074)	0.162** (0.073)	0.166** (0.070)
1700	0.172* (0.097)	0.164* (0.098)	0.177* (0.091)	0.173* (0.090)	0.165* (0.089)
1800	0.128 (0.091)	0.130 (0.092)	0.145 (0.089)	0.139 (0.088)	0.139 (0.089)
Observations	20,223	20,223	20,223	20,223	20,223
No. of cities	2,247	2,247	2,247	2,247	2,247

Note: Each column re-estimates Model 3b varying the minimum years of operation required to classify a city as treated within a given century. Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 30/40/50/60/70 years within a given century. "ATT" is the overall average across all cohorts and post-treatment centuries; "By century" rows are calendar-time ATTs aggregated across cohorts. All specifications include city FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to coast, river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

In Figure C1, we report the result of a randomization-inference placebo exercise. The aim is to assess whether an effect of similar magnitude could be obtained when university timing is assigned to comparable cities that never actually hosted a university. To do so, we first estimate a city-level propensity score for ever hosting a university using baseline population, predetermined geographic characteristics and macroregion fixed effects. We then split cities into propensity-score quintiles and combine these with historical macroregions, so that placebo cohorts are assigned within relatively comparable propensity-score × macroregion strata. Within these strata, we randomly assign placebo treatment cohorts to never-treated cities while preserving the actual number of treated cities in each cohort, and we re-estimate Model 3b for each of the 500 placebo assignments. The resulting placebo ATT distribution is centered well below the actual estimate. The mean placebo ATT is -0.152, while the baseline ATT is 0.187. None of the 500 valid placebo draws produces an ATT at least as large as the one estimated from the actual university allocation. Hence, the empirical one-sided randomization-inference share is 0.000. The observed effect therefore lies far in the right tail of the placebo distribution, suggesting that it is very unlikely to be generated by chance alone under random assignment. We view this as supportive, though not conclusive, evidence that the baseline result is not an artifact of the estimation procedure.

Figure C1. Randomization Inference - Placebo ATT Distribution



Note: The histogram displays the distribution of placebo ATTs from 500 randomized draws in which university locations are reassigned across cities while preserving the observed treatment timing structure. The red dashed line marks the estimated ATT of 0.187 from the baseline specification (model 3b).

As an additional robustness check, we restrict the sample exclusively to cities that reached higher population thresholds. This exercise is motivated by the concern that population figures may be measured less precisely for very small cities; however, we also note that conditioning on city size may itself induce sample selection. Reassuringly, the estimates presented in Table C3 remain broadly stable across these alternative samples, although the ATT is somewhat smaller when attention is restricted to larger cities. In particular, the overall ATT loses significance only when the sample is confined to cities exceeding 10,000 residents, which however features only 392 units. Indeed, the progressive attenuation of the ATT as the threshold rises while the standard errors remain roughly stable, likely reflects, at least in part, the sharp reduction in sample size rather than a genuine absence of effects among larger cities. In fact, restricting attention to progressively larger cities removes the smaller urban centers, for which the proportional contribution of a university was largest, and retains cities that were already sizable commercial or administrative centers, where the marginal effect of a university was plausibly smaller. The estimates remain positive throughout, which suggests attenuated rather than absent effects among larger cities.

Table C3. Sensitivity to minimum city population threshold

	(1) Baseline (Full sample)	(2) Pop. > 5,000	(3) Pop. > 8,000	(4) Pop. > 10,000
ATT	0.187***	0.170**	0.143*	0.117
	(0.068)	(0.070)	(0.076)	(0.077)
By century				
1300	0.331***	0.305***	0.244**	0.238**
	(0.088)	(0.091)	(0.098)	(0.102)
1400	0.377***	0.341***	0.288***	0.262***
	(0.084)	(0.084)	(0.087)	(0.088)
1500	0.183**	0.146*	0.088	0.075
	(0.081)	(0.084)	(0.091)	(0.088)
1600	0.187**	0.165**	0.126	0.105
	(0.074)	(0.077)	(0.084)	(0.084)
1700	0.177*	0.154	0.121	0.093
	(0.091)	(0.096)	(0.102)	(0.106)
1800	0.145	0.144	0.147	0.115
	(0.089)	(0.091)	(0.100)	(0.106)
Observations	20,223	9,396	4,608	3,528
No. of cities	2,247	1,044	512	392

Note: Each column restricts to cities whose population exceeded the stated threshold in at least two observation periods. The two-period requirement avoids classifying cities on the basis of a single, possibly transitory, population observation and ensures that the restricted samples consist of cities consistently above the threshold. All other choices as in Model 3b. Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. "ATT" is the overall average across all cohorts and post-treatment centuries; "By century" rows are calendar-time ATTs aggregated across cohorts. All specifications include city FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to coast, river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

In our baseline specification we consider Oxford, Bologna and Paris as belonging to the 1300 cohort, to avoid having a separate 1200 cohort with just three treated cities. In Table C4 we assign the three earliest universities, i.e., Bologna, Oxford and Paris, to a separate 1200 cohort instead of grouping them with the 1300 cohort. The overall ATT remains very close to the baseline estimate, falling only from 0.187 to 0.177, and remains statistically significant at the

1 percent level.[32] The calendar-time effects are also very similar to the baseline estimates. The newly estimated 1200 effect is positive and statistically significant, while the effects from 1300 onwards remain positive and follow the same declining pattern over time. This in turn suggests that the baseline results are not mechanically driven by the decision to group the three earliest universities with the 1300 cohort.

Table C4. Alternative cohort assignment for Bologna, Oxford and Paris

	(1) Baseline	(2) Separate 1200 cohort
ATT	0.187***	0.177***
	(0.068)	(0.066)
By century		
1200		0.318***
		(0.058)
1300	0.331***	0.376***
	(0.088)	(0.088)
1400	0.377***	0.384***
	(0.084)	(0.089)
1500	0.183**	0.194**
	(0.081)	(0.087)
1600	0.187**	0.177**
	(0.074)	(0.072)
1700	0.177*	0.151*
	(0.091)	(0.087)
1800	0.145	0.130
	(0.089)	(0.087)
Observations	20,223	20,223
No. of cities	2,247	2,247

Note: Model 3b throughout. Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. "ATT" is the overall average across all cohorts and post-treatment centuries; "By century" rows are calendar-time ATTs aggregated across cohorts. All specifications include city FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to coast, river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

In our baseline specification we consider Oxford, Bologna and Paris as belonging to the 1300 cohort, while in table C4 we assign them in a separate 1200 cohort with just these three treated cities. In Table C5 we further explore the robustness of this choice by excluding the first three

32 The effect remains positive and statistically significant if we eliminate these three cities from the sample. Please see Table C5 of Appendix C.

universities from our sample. Reassuringly, both the average ATT as well as the calendar profile are barely changed.

Table C5. Robustness to excluding the three earliest universities

	(1) Full sample (incl. first 3)	(2) Model 1a, excl. first 3	(3) Model 1b, excl. first 3	(4) Model 2a, excl. first 3	(5) Model 2b, excl. first 3	(6) Model 3a, excl. first 3	(7) Model 3b, excl. first 3
ATT	0.187***	0.157*	0.173**	0.163**	0.180***	0.125*	0.145**
	(0.068)	(0.085)	(0.079)	(0.065)	(0.065)	(0.066)	(0.067)
By century							
1300	0.331***	0.375***	0.463***	0.280***	0.369***	0.290***	0.367***
	(0.088)	(0.097)	(0.128)	(0.078)	(0.089)	(0.074)	(0.094)
1400	0.377***	0.378***	0.429***	0.360***	0.410***	0.336***	0.364***
	(0.084)	(0.107)	(0.116)	(0.078)	(0.085)	(0.088)	(0.097)
1500	0.183**	0.164*	0.182*	0.169**	0.230***	0.111	0.170*
	(0.081)	(0.094)	(0.099)	(0.077)	(0.080)	(0.082)	(0.090)
1600	0.187**	0.208**	0.217**	0.191***	0.218***	0.101	0.140*
	(0.074)	(0.093)	(0.089)	(0.067)	(0.067)	(0.072)	(0.071)
1700	0.177*	0.169	0.145	0.170*	0.164*	0.109	0.117
	(0.091)	(0.105)	(0.099)	(0.090)	(0.090)	(0.087)	(0.088)
1800	0.145	0.058	0.097	0.094	0.096	0.107	0.105
	(0.089)	(0.110)	(0.099)	(0.090)	(0.088)	(0.091)	(0.088)
Observations	20,223	20,196	20,196	16,848	16,848	20,196	20,196
No. of cities	2,247	2,244	2,244	1,872	1,872	2,244	2,244
Geo. controls × century	Yes	No	Yes	No	Yes	No	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Hist. regions × century FE	No	No	No	Yes	Yes	No	No
NUTS0 ×century FE	Yes	No	No	No	No	Yes	Yes

Note: Bologna, Oxford, and Paris are dropped entirely. All data preparation and specifications are otherwise identical to Table 2. Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. "ATT" is the overall average across all cohorts and post-treatment centuries; "By century" rows are calendar-time ATTs aggregated across cohorts. All specifications include city FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to coast, river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

We also examined potential confounding effects associated with founder type and religious affiliation of universities. If we include variables capturing these dimensions as additional controls, results appear rather stable, as reported in Table C6. Likewise, in results available upon request, we investigated whether variation in scholar mobility, captured by high or low inflows and outflows of scholars, systematically shaped the estimated effects; again, we find no evidence that these dynamics materially affect our results.

Table C6. Robustness to religious controls and university founder type

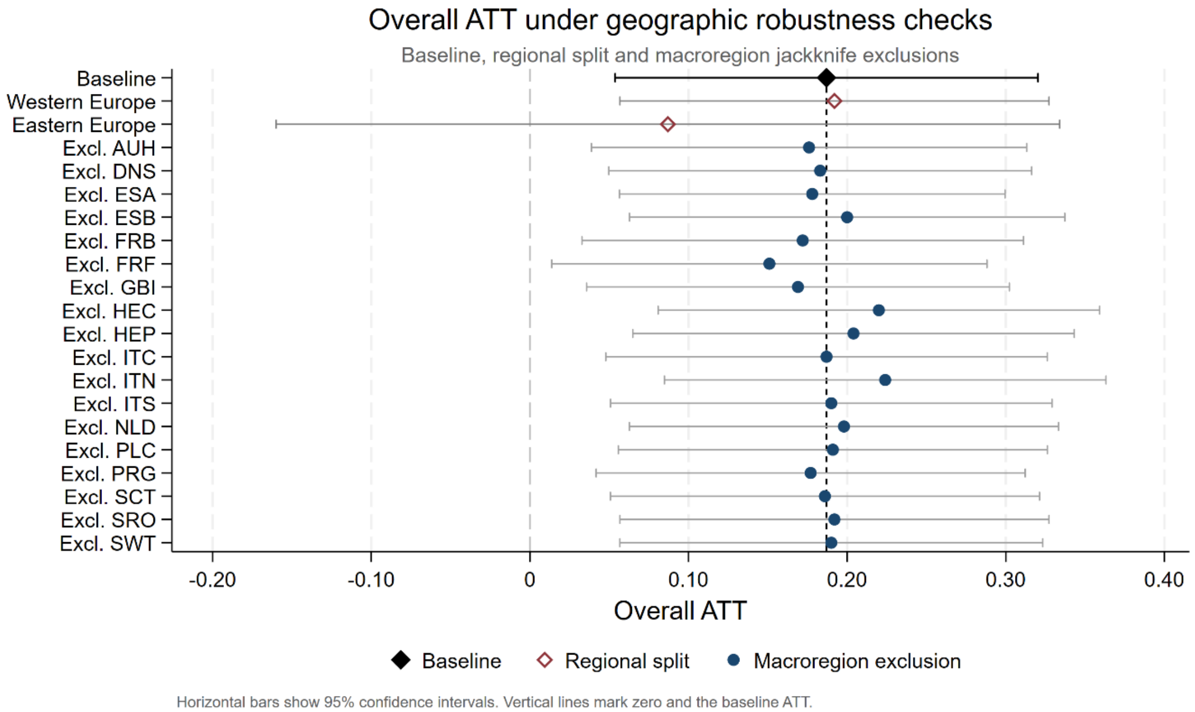
	(1) Baseline	(2) + Religious controls	(3) + Founder type
ATT	0.187***	0.143**	0.209*
	(0.068)	(0.072)	(0.116)
By century			
1300	0.331***	0.296***	0.203*
	(0.088)	(0.084)	(0.104)
1400	0.377***	0.294***	0.256**
	(0.084)	(0.092)	(0.103)
1500	0.183**	0.186**	0.264*
	(0.081)	(0.094)	(0.139)
1600	0.187**	0.154*	0.033
	(0.074)	(0.080)	(0.124)
1700	0.177*	0.116	-0.034
	(0.091)	(0.093)	(0.119)
1800	0.145	0.099	0.488*
	(0.089)	(0.095)	(0.254)
Observations	20,223	20,223	20,223
No. of cities	2,247	2,247	2,247

Note: "Religious controls" adds current religious affiliation, denomination change count, and a change indicator interacted with century dummies. "Founder type" adds a dummy for ecclesiastical vs. secular founder. Model 3b otherwise. Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. 'ATT' is the overall average across all cohorts and post-treatment centuries; "By century" rows are calendar-time ATTs aggregated across cohorts. All specifications include city FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to coast, river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

A further source of heterogeneity concerns the geographical distribution of university effects across Europe. While the aggregate estimates suggest a sizeable impact of university foundations, disaggregating the sample into Eastern and Western European cities reveals that this pattern is not spatially uniform. For clarity, the regional classification employed in the Eastern – Western Europe split follows the grouping reported in Table A5 of the Appendix A. As reported in Figure C2, the baseline pattern is indeed entirely driven by Western Europe, where the ATT remains positive and statistically significant throughout the period and closely mirrors the full-sample estimates. On the contrary, Eastern Europe exhibits no detectable university effects, and the coefficients are small, statistically insignificant. This divergence suggests that the demographic contribution of universities was not uniformly diffused across the continent but was instead embedded in the institutional, commercial, and political environment of Western Europe, where universities developed earlier, operated under stronger legal and fiscal privileges, and were more closely integrated into long-distance trade and ecclesiastical-administrative networks. In Eastern Europe, where foundations were fewer, emerged later, and were frequently more constrained by state or religious authorities, universities do not appear to have generated measurable urban expansion. Moreover, if the most important intellectual product of European universities was the rediscovery of Roman law and the development of an entirely new European civil law system, the result of the fusion of Roman and Canon law which created a systematic framework that integrated the technical sophistication of the Roman tradition with the pragmatic, equity-based solutions of ecclesiastical law (Greif et al., 2025), then this creation was largely confined to Western Europe (Giaro, 2011), with limited diffusion in the East.

In Figure C2 we also check the sensitivity of our results to dropping, one at a time, each historical macro-region. The results are stable. The overall ATT remains positive and statistically significant in all specifications, ranging from 0.151 to 0.224. The calendar-time estimates also remain close to the baseline pattern, with positive and generally precisely estimated effects from the earlier post-treatment centuries, while the estimates for 1800 are smaller and less precise in several specifications. Overall, this suggests that the baseline result is not driven by any single historical macro-region.

Figure C2. Overall ATT under geographic robustness checks



Note: Western/Eastern Europe split follows the regional classification presented in Table A5 of Appendix A. Macroregion exclusions sequentially drop all cities from the named historical macroregion. Macroregion codes refer to the 18 historical macroregions defined by Curtis et al. (2026a), namely AUH Austrian Habsburg Lands, DNS Scandinavia, ESA Levante, ESB Castilla, FRB Occitania, FRF Francia, GBI English Realms, HEC Tridentine Germania, HEP Evangelical Germania, ITC Central Italy, ITN Northern Italy, ITS Southern Italy, NLD Netherlands, PLC Poland-Lithuania, PRG Portugal, SCT Scotland, SRO Lotharingian corridor, and SWT Switzerland. Model 3b throughout. Outcome is city population measured in thousands. Treatment is defined as hosting a university that operated for at least 50 years within a given century. “ATT” is the overall average across all cohorts and post-treatment centuries, while “By century” rows are calendar-time ATTs aggregated across cohorts. All specifications include city fixed effects, country-by-century fixed effects, and latitude × longitude × century interactions, estimated using PPML with the Wooldridge (2023) ETWFE approach. Geographic controls are interacted with century dummies where indicated. Standard errors are clustered at city level and reported in parentheses. Significance levels are * p<0.10, ** p<0.05, *** p<0.01. Model 3b throughout. Outcome is city population (thousands). Treatment is defined as hosting a university that operated for at least 50 years within a given century. “ATT” is the overall average across all cohorts and post-treatment centuries; “By century” rows are calendar-time ATTs aggregated across cohorts. All specifications include city FE and latitude × longitude × century interactions estimated via PPML with the Wooldridge (2023) ETWFE approach. Geographic controls (distance to coast, river, Roman roads, Wittenberg) are interacted with century dummies where indicated. Standard errors clustered at city level in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.