Getting Religion: Identity Formation in the Protestant Reformation

Marcel Caesmann*

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Abstract

This paper examines how new identity markers emerge, spread, and define enduring social boundaries. I study Reformation Germany (1480–1806) using 4.9 million baptismal names from 2,112 towns to trace how families expressed religious affiliation through naming choices. Naming remained nearly identical across Catholics and Protestants for decades after 1517 but diverged sharply after 1570, stabilizing by 1618. Church enforcement strengthened local identities, while social learning and conformity spread them across space. Using a staggered difference-in-differences design around Jesuit and Protestant college foundings—complemented by counterfactual placements and visitation records—I show a strong link between institutional presence and local alignment. Applying a DeGroot model of learning with spatial-lag specifications, I show that towns updated their naming toward locally shared beliefs and prevailing norms, producing lasting cultural and economic divides.

Keywords: Identity, cultural change, religion, first names, Holy Roman Empire

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1 Introduction

> Everywhere one confronts confessional prejudice; everywhere one encounters the fence, indeed the wall of confession.
>
> *Harnack (1907)*

Culture is learned. People learn identities by observing others: which groups matter, what they mean, and how to express belonging (Boyd and Richerson 1988; Henrich 2016). These identities, often built around nation, class, or religion, shape how people interact and behave economically (Tajfel and Turner 1979; Horowitz 1985; Shayo 2009). While cultural identities are often persistent, they can change: People lose their faith, abandon class identity, or redefine national belonging.[1] How do new identity markers emerge, spread, and come to define deep social divides?

I study these dynamics during the Protestant Reformation, Western Europe's largest religious rupture. The Reformation split Christianity into competing confessions, creating new group boundaries that shaped politics, culture, and economic life for centuries (Becker, Pfaff, and Rubin 2016). Three features make this setting ideal. First, religion has long structured social life, shaping communities for millennia and continuing to guide behavior for billions today (Iannaccone 1998; McCleary and Barro 2006; Becker, Rubin, and Woessmann 2024), making these findings broadly relevant. Second, the Holy Roman Empire's fragmented sovereignty generated rich variation in institutional responses: within a compact geographic area, hundreds of sovereign states pursued different enforcement strategies. Third, first names provide a consistent, observable identity marker spanning three centuries, allowing me to trace identity formation continuously from before the shock through its long-run consolidation—avoiding the "compression of history" typical of long-run persistence studies.[2]

I assemble a novel dataset linking 4.9 million baptismal records from 2,112 towns (1480-1806) to detailed measures of church enforcement: 112 Jesuit colleges and Protestant academies, church visitation records in 460 towns, detailed clergy data, religious printing volumes, and conflict exposure during the Thirty Years' War. This granular panel allows me to trace how institutional investments shaped identity formation and diffusion.

Three descriptive facts motivate the analysis. First, names acquired religious meanings gradually and selectively. By 1650, "Matthias" became distinctively Catholic while "Christoph" became distinctively Protestant. Yet, "Jacob" remained denominationally neutral suggesting that religious meaning was not inherent to names but emerged through social processes that selected certain names as identity markers. Second, identity polarization took time: flat from 1520 to 1570, accelerating sharply between 1570 and 1620, then slowing after 1620. This suggests positive feedback mechanisms that amplify polarization

[1] See Bisin and Verdier (2001); Voigtländer and Voth (2012); Alesina, Giuliano, and Nunn (2013) on persistence. On identity change, see Fouka (2020); Fouka and Serlin (2023); Bonomi, Gennaioli, and Tabellini (2021); Gennaioli and Tabellini (2023); Cantoni, Mohr, and Weigand (2025); Fernández (2025).

[2] An established literature uses first names as an expression of identity and links it to behavioral outcomes (Fryer and Levitt 2004; Abramitzky, Boustan, and Eriksson 2020; Fouka 2020; Fouka and Serlin 2023; Bentzen and Andersen 2022; Cantoni, Mohr, and Weigand 2025)

before saturating. Third, doctrinal differentiation preceded identity polarization. Using language in printed books as a proxy for doctrinal partisanship (Gentzkow, Shapiro, and Taddy 2019), I document that doctrinal differentiation precedes identity polarization in names. This lag indicates that doctrinal differences alone were insufficient; something beyond theology was needed to translate doctrine into lived identity distinctions. This suggests three questions: Which mechanisms transformed doctrine into identity markers? How did communities coordinate on shared name meanings? Why was polarization initially slow, accelerate mid-period, then saturate?

The empirical analysis establishes three corresponding mechanisms. First, I show that stronger church enforcement shifted local naming patterns. Using a staggered difference-in-differences approach around the founding of Jesuit colleges and Protestant academies, I find that exposure to a college increased denomination-specific naming by .44 standard deviations for Catholics and .36 standard deviations for Protestants, equivalent to moving from the median to the 31st or 64th percentile in the empirical name score distribution of towns in 1650. Event studies reveal no differential pre-trends, and effects are robust to controlling for expected college placement based on counterfactual scenarios taking church strategy and pre-Reformation town characteristics as given (Borusyak and Hull 2023). I corroborate this using church visitations, inspection tours by central church authorities to local parishes. I instrument visitation intensity with distance to administrative seats. Greater visitation frequency increased confessional naming by around .07 of a standard deviation in the Protestant name score per additional visitation by 1620 (mean number of visitations = 7.8). These effects operated through two channels: academies increased clergy quality and religious printing, while I provide suggestive evidence that visitations operated through monitoring and replacing ineffective pastors. The dissolution of the Jesuit order in 1773 provides further evidence for the importance of sustained institutional enforcement: when direct oversight was weakened, the distinctiveness of Catholic naming declined.

Second, I demonstrate that parents learned name meanings through decentralized social observation. For each name-town-decade, I construct the "gap" between local usage and neighbors' usage of names within a 50km radius. Names underused relative to neighbors subsequently increased in popularity, consistent with DeGroot (1974) learning where parents update beliefs about name meanings by observing neighbors'. Convergence is faster when signals were clearer: for names with high usage, in densely connected networks, and where local naming portfolios are more distinctive.

Third, I establish that towns coordinated aggregate identity through peer conformity. Towns' overall confessional intensity converged toward nearby same-denomination communities, controlling for town and time fixed effects. Critically, conformity operates through portfolio adjustment—communities maintain diverse name menus while shifting their overall positions—rather than mechanical concentration on fewer names. Together, learning at the name level and conformity at the aggregate level create social amplification: enforcement effects diffuse spatially and accelerate through reinforcing feedback.

I develop a dynamic model integrating these three forces. Individuals balance church enforcement, conformity to neighbors, and transmitted preferences when choosing identity positions, then select names to signal these positions. This two-stage structure generates dual-level social forces: preference-based conformity at the aggregate level and information-based learning at the name level. Their interaction creates reinforcing feedback—enforcement seeds variation, conformity spreads it spatially, and learning accelerates diffusion of clear signals—generating substantial polarization from modest institutional interventions.

Finally, I examine whether naming patterns correlate with consequential behaviors. Within town-decade contexts, parents' and children's name scores are strongly correlated. Similarly, spouses' scores are strongly correlated in local marriage markets—suggesting names signal transmitted identity and partner compatibility. Individuals with stronger confessional signals were more likely to pursue church careers. At the group level, towns with clearer confessional signals faced asymmetric conflict exposure during the Thirty Years' War: Catholic towns with strong Catholic signals were 21 percentage points more likely to be attacked by Protestant Union forces. Protestant towns with strong Protestant signals were 9.2 percentage points more likely to be attacked by Catholic League forces. While these correlations cannot be interpreted causally, their consistency across family, marriage, career, and conflict domains suggests that naming differentiation reflects meaningful identity formation.

This paper makes three primary contributions. First, I contribute to the literature on the origins and dynamics of social identity and cultural traits. A foundational literature emphasizes the persistence of cultural traits through intergenerational transmission (Bisin and Verdier 2001; Bénabou and Tirole 2011; Bisin et al. 2011; Voigtländer and Voth 2012). Michalopoulos (2012) highlight geographic factors shaping ethnolinguistic diversity, while institutional origins have received increasing attention (Grosfeld, Rodnyansky, and Zhuravskaya 2013; Alesina and Giuliano 2015; Becker and Pascali 2019; Cantoni, Mohr, and Weigand 2025). Recent work shows that identity salience can shift rapidly within a lifetime due to shocks or changing economic conditions (Shayo 2009; Fernández 2013; Bentzen 2019; Fouka 2020; Fouka and Serlin 2023; Voth and Yanagizawa-Drott 2023; Bonomi, Gennaioli, and Tabellini 2021; Gennaioli and Tabellini 2023; Fernández 2025). I extend this literature in two ways. Empirically, I provide a granular, individual-level measure of religious identity spanning more than 300 years, documenting the full trajectory of differentiation before, during, and after the emergence of a new creed—avoiding the "compression of history" common in long-run persistence studies. Conceptually, I show that identity boundary formation is neither purely top-down (institutional imposition) nor purely bottom-up (spontaneous coordination), but emerges from their interaction. I document three mechanisms—enforcement, learning, and conformity—and demonstrate how their interaction generates self-reinforcing dynamics.

Second, I contribute to the literature on the rise of state capacity in early modern Europe. A long-standing tradition views inter-state conflict as a central driver of effective states (Tilly 1975; Besley and Persson 2008; Besley and Persson 2009; Dincecco and Onorato 2016; Dincecco and Onorato 2017; Cantoni, Mohr, and Weigand 2024; Bosshart and Weigand 2025), with empirical work predominantly focusing on fiscal capacity (Besley and Persson 2010; Gennaioli and Voth 2015). This paper shows that religious competition also drove an expansion of state capacity, particularly in its ideological dimension—the ability of states and religious institutions to build shared belief systems and induce social discipline (Oestreich 1968). Churches built extensive clergy training systems, monitoring infrastructure, and doctrinal production. This dimension of capacity, central to the rise of early modern state-building, complements existing work focusing on fiscal and legal institutions.

Third, I contribute to the literature on the Protestant Reformation (see Becker, Pfaff, and Rubin (2016) for a comprehensive review). Previous work has studied the causes of the Reformation (Cantoni

2012; Rubin 2014; Dittmar and Seabold 2019) and its long-run consequences for economic development (Weber 1905; Becker and Woessmann 2009; Boppart, Falkinger, and Grossmann 2014; Cantoni 2015), institutions (Rubin 2017), human capital (Dittmar and Meisenzahl 2020; Becker and Pascali 2019; De La Croix and Morault 2024), and violence (Leeson and Russ 2018). Recent work adds evidence on the consequences of the Catholic Counter-Reformation (Becker, Pino, and Vidal-Robert 2021; Cabello 2025). I contribute by documenting *when and how* Protestant and Catholic identities diverged, unpacking the black box linking the initial Reformation shock to persistent behavioral differences. By tracking identity formation continuously from 1480 to 1806, I show that confessional boundaries were not immediate consequences of theological disputes but were actively constructed through decades of institutional investment, social learning, and peer conformity. This unpacks the process through which the Reformation's long-run effects were generated and transmitted.

The remainder of the paper proceeds as follows. Section 2 introduces the historical context. Section 3 describes data construction. Section 4 documents the three motivating facts about identity polarization. Section 5 presents the main empirical results on enforcement. Section 6 studies the social transmission of religious identity through learning and conformity. Section 7 provides a conceptual framework integrating the empirical results. Section 8 documents the link between names as identity markers and individual and group level behaviors. Section 9 concludes.

2 Historical Background

In the late medieval period, the Catholic Church was the dominant religious institution in the Holy Roman Empire, exercising wide spiritual, political, and economic authority. By the early sixteenth century, however, critics across the Holy Roman Empire decried perceived abuses and administrative weaknesses: the sale of indulgences, simony, and absenteeism undermined credibility and pastoral care (Ozment, Eire, and Rittgers 2020). Administrative distance—particularly in prince-bishoprics where bishops balanced territorial and spiritual duties—limited consistent enforcement (Whaley 2012). Many clergy were poorly trained, preaching and catechesis varied widely, and lay understanding of doctrine was limited (Zeeden 1965; Strauss 1975). While religion permeated civic and ritual life, these gaps in oversight and instruction left space for persistent elements of pagan and superstitious beliefs (Zeeden 1965; Becker and Voth 2025). Reform attempts such as the conciliar movement failed to deliver lasting change addressing the shortcomings (Tanner and Tanner 2001).

2.1 Protestant Reformation and Church Organization

Against this backdrop, Martin Luther's Ninety-Five Theses (1517) marked the beginning of the Protestant Reformation. Print technology amplified the movement, rapidly circulating pamphlets and vernacular Bibles to urban and rural audiences alike (Eisenstein 1980; Rubin 2014; Dittmar and Seabold 2019). By the mid-sixteenth century, Protestant communities formalized doctrine: the *Confessio Augustana* (1530), drafted by Philipp Melanchthon and presented at the Diet of Augsburg, articulated Lutheran teaching, and the *Konkordienformel* (1577) later sought to unify divergent strands (Kolb and Arand 2005).

Protestant territories invested heavily in enforcement infrastructure that would shape congregants' religious identity. The Reformation embedded belief through a coordinated package of print, catechetical instruction, the training of an effective pastoral cadre, and persistent institutional oversight (Reinhard 1983). Luther's *Small Catechism* (1529) distilled basic doctrine for households, while the *Large Catechism* targeted clergy and schoolmasters (Pettegree 2005). Secular rulers and city councils issued church ordinances regulating liturgy, schooling, and poor relief (Sehling 1902; Dittmar and Meisenzahl 2020). Resources from dissolved monasteries financed the new ecclesiastical infrastructure with buildings and endowments redirected to support clergy and schools (Schmidt 1992). Institutions for training and administration were created: the former Augustinian house in Tübingen became the *Tübinger Stift*, a seminary preparing pastors on generous scholarships; the University of Marburg (1527) trained Protestant clergy and administrators; Saxon *Fürstenschulen* prepared boys for clerical and bureaucratic careers on monastic endowments; and urban centers such as Nuremberg adopted comprehensive ordinances establishing publicly funded schools and regulating civic life along confessional lines (Schmidt 1992). Regular visitations, promoted by Luther and Melanchthon, enforced conformity and revealed substantial gaps in instruction, reinforcing the need for catechesis (Zeeden 1982).

These institutions aimed not merely to train elites but to reshape religious identity among ordinary congregants. Catechetical instruction, whether through Luther's Small Catechism or weekly sermons, taught laypeople not just theological doctrine but how to be Protestant in everyday life. One domain where this identity formation became visible was naming practices. First names had no inherent confessional content—their confessional meanings emerged entirely from social convention. As Protestant and Catholic authorities invested in enforcement infrastructure, parents' naming choices increasingly reflected these emerging confessional identities, transforming names from neutral labels into markers of religious belonging (Zschunke 1984; François 1991).

2.2 Catholic Counter-Reformation

The Catholic response, the Counter-Reformation, was anchored in the Council of Trent (1545–1563). Trent reaffirmed doctrine on sacraments and authority while mandating higher standards for clerical education, residency, and discipline (Ekelund, Hebert, and Tollison 2004; O'Malley 2013). Implementation relied on new and revitalized institutions, above all the Society of Jesus (1540), which became central to Catholic renewal through missions, schools, and pastoral work (Zeeden 1965; Reinhard 1983; Wright 2005; Friedrich 2016).

Jesuit foundations spread widely across the Empire, typically through episcopal, princely, civic, and private patronage. Ingolstadt, though modest in size, received the first Jesuit college (1556) and became a bastion of Catholic scholarship once Jesuits secured professorships (Schmidt 1992). Larger towns, like Cologne (from 1556), Trier (1560), Mainz (1561), and Munich (from 1559), integrated Jesuit teaching into universities and urban institutions; Augsburg's college benefited from a substantial Fugger endowment (Schmidt 1992; Grendler 2019). Although the Jesuit Constitutions favored foundations in populous cities and universities to maximize reach, local actors frequently shaped the map in more idiosyncratic ways (Friedrich 2016). Dillingen, for example, was elevated to a Jesuit university in 1564 through the initiative of Prince-Bishop Otto Truchsess von Waldburg, who secured papal authorization and assigned monastic lands to support the endowment (Grendler 2019). Landsberg am Lech established a college in 1576 owing to the prestige and efforts of the administrator Schweikhard von Helfenstein, former president of the Reichskammergericht (Friedrich 2016). Such cases illustrate how personal networks, officeholders, and patrons could direct foundations to smaller towns as well as major centers. The *Ratio studiorum* (1599) standardized pedagogy across colleges; tuition-free day schools educated both elites and burghers, and post-Tridentine visitations translated conciliar norms into regular oversight and enforcement at the parish level (Zeeden 1982). Taken together, the Tridentine decrees, Jesuit schooling, and systematic visitations constituted sustained investments in personnel training, the articulation and dissemination of doctrine, and parish monitoring across the Empire.

The Counter-Reformation's emphasis on catechesis and standardized instruction similarly aimed to embed Catholic identity in everyday practice. Jesuit schools did not merely educate nobility; they conducted public catechism classes for urban children and coordinated with parish priests to ensure doctrinal conformity among families. This systematic pedagogy extended to the most intimate family decisions, including the choice of children's names. Saints' names, promoted through Jesuit religious instruction and reinforced in baptismal rituals, became vehicles for expressing Catholic identity—a form of everyday confessional signaling that required no theological sophistication but marked one's place in the religious order.

2.3 Confessionalization

By the late sixteenth century, these parallel reforms crystallized into what German historians have termed Confessionalization: the consolidation of distinct confessional churches and cultures through coordinated efforts by territorial states and religious authorities. Pioneering work by Zeeden (1958); Zeeden (1965), and Reinhard (1983) documents how Protestant and Catholic territories alike invested heavily in doctrinal standardization, clerical training, and institutional oversight—transforming abstract theological differences into hardened social boundaries.

A crucial institutional feature shaped this process: the Religious Peace of Augsburg (1555) enshrined "*cuius regio, eius religio*"—whose realm, his religion. The ruler's confession determined the official religion of all subjects within that territory. While subjects formally possessed the right to emigrate (ius emigrandi) rather than convert, substantial barriers limited cross-confessional mobility in practice. Legal restrictions required formal conversion or explicit permission to resettle; economic costs included forfeiting land tenure and trade networks; social costs involved severing kinship ties. Contemporary sources suggest large-scale confessional migration was rare except under direct persecution; most subjects remained in their birth territories regardless of religious changes (Schilling 1981; Schilling 1988; Whaley 2012).

This institutional arrangement meant religious denomination became effectively a location-level characteristic, determined by rulers' decisions rather than individual choice. These decisions reflected dynastic, political, and theological considerations that evolved independently of local customs. A town's confession thus depended largely on which dynasty controlled it and when that dynasty converted—factors external to local populations' preferences or practices. In the case of free imperial cities it was a function of which confessional group gained the upper hand in local city councils during the contested and turbulent early stages of the Reformation.

Yet boundaries remained fluid in practice, and peaceful coexistence was often maintained under the arrangements of the Peace of Augsburg (Schilling 1988). Intermarriage across religious boundaries were still frequent, particularly in bi-confessional cities like Augsburg where Protestants and Catholics lived door at door (François 1991). When Michel de Montaigne visited Augsburg in 1580, he noted:

> *"Marriages between Catholics and Protestants take place daily. The part that has the most desire accepts the faith of the other. There are thousands of such marriages. Our host, for example, was a Catholic, his wife a Protestant."*[3]

At the same time, Protestant and Catholic authorities invested heavily to sharpen doctrine, enforce social norms, develop persuasive media, and build educational systems to foster internalization of the new order (Reinhard 1983).[4] These changes took time to materialize: training a new generation of clergy and promoting them through ecclesiastical hierarchies required decades. By the 1580s, harder-line figures on both sides occupied influential positions and started to propel religious polarization (Reinhard 1983). This joint effort of territorial states and churches formed part of a broader drive toward social control (Oestreich 1968). In 1589, Giovanni Botero captured the appeal of confessional governance for early modern rulers:

> *"Among all laws, there is none more favorable to princes than the Christian one, for it subjects to them not only the bodies and goods of their subjects [...] but also their souls and consciences, and it binds not only the hands but also the feelings and thoughts."*[5]

By targeting minds as well as conduct, confessionalization extended beyond the strictly religious sphere and brought profound social change (Schilling 1988). As confessional boundaries hardened and Protestants and Catholics came to be perceived as mutually exclusive groups (Zeeden 1965; Schmidt 1992), their cultures and ways of life diverged and remained a central social cleavage well into the nineteenth and twentieth centuries (Zschunke 1984; François 1991).[6]

3 Data

To examine how distinct religious identities took shape in early modern Germany, I compile a dataset combining novel information on markers of religious identity, measures of church enforcement capacity, and data on social and economic consequences. The base structure comprises 2,390 towns and their catchment areas within the Holy Roman Empire, as depicted in the *Deutsches Städtebuch* (Keyser et al. 1939).[7]

[3] Quoted according to François (1991).

[4] The centrality of higher education in both confessions' efforts to strengthen identity contributed to a near breakdown of scholarly exchange across confessional lines, producing increasingly separate intellectual worlds. Figure A.12 plots the share of scholar transfers that crossed between Catholic and (eventually) Protestant universities. See also De La Croix and Morault (2024); Curtis et al. (2025).

[5] Quoted according to Reinhard (1983).

[6] François (1991) notes that as late as the 1960s, 31% of Protestant and 36% of Catholic parents expressed serious distaste for their children marrying someone of the opposing confession. This is comparable to the 38% of Republicans and Democrats reporting in 2020 that they would be somewhat or very upset if their child married someone with opposing party preferences (YouGov 2020).

[7] This source covers all settlements within the 1937 German borders that ever obtained town status. I use town borders from Bogucka, Cantoni, and Weigand (2019) to match parishes to towns.

3.1 Religious Identity

To measure religious identity, I compile information on 4.9 million first names of individuals born within the Holy Roman Empire. The data draw on two broad categories of sources.

Church records. First, I use birth and baptism records from Catholic and Protestant churches. In these records, local priests documented the date of birth or baptism (which, for religious reasons, closely coincided), the child's full name, and the names of parents and godparents. Part of these data come from newly collected and digitized baptism records. Additional records are provided by the Church of Jesus Christ of Latter-Day Saints, which collected and digitized baptism records for genealogical purposes, made available on FamilySearch.com.[8] Additionally, I draw on indexed birth and baptism records from Computergenealogie (2025).

Spatial and temporal coverage reflects record availability. Many parishes began systematic baptism records only in the late sixteenth and early seventeenth centuries. Coverage also depends on the current status of indexed records on FamilySearch and Computergenealogie (2025), which varies across regions. I drop entries with missing birth years, remove duplicates, precisely geolocate all parish locations, and assign each place to its nearest town.

Elite sources. Second, I use university matriculation lists to increase geographic and temporal coverage, especially for 1450-1600. These lists record students' names, places of origin, and sometimes subjects of study at enrollment. I collect and digitize edited matriculation lists for most German universities in the early modern period.[9] I supplement this with student information from the *Repertorium Academicum Germanicum* (Hesse and Schwinges 2025).

Additionally, I use information on 730,359 notable individuals from *Deutsche Biographie* (Hockerts and Lanzinner 2022) and 404,954 individuals from the *Oberdeutsche Personendatenbank* (Rupp 2025). These sources provide birth and death dates, professions, and places of residence. For all individuals from elite sources, I remove duplicates across sources, geolocate birthplaces, and assign them to the nearest town. Crucially, the occupational information in *Deutsche Biographie* enables analysis of how confessional identity predicts career choices (church careers versus commercial occupations).

Name processing. I clean and standardize names to ensure comparability across time and space. I harmonize first-name spelling (e.g., Johann to Johannes, Josef to Joseph), exclude names occurring 50 or fewer times over the sample period, and assign confession based on parish of birth. Where the parish is not observed, I use confession of the town of birth as fallback for assigning confessional status. For a subset of the data, where I observe parents, I construct parent-child and spousal pairs.

I also categorize names by identity type. I identify *religious names* relating to Christian traditions using a list of biblical first names[10] and names of saints with major churches in Germany (Buringh et al. 2020), plus names containing "Gott" (God). I identify *Germanic names* using Behind the Name (2025).

[8] I access the data through an agreement with the Church of Jesus Christ of Latter-Day Saints.
[9] Appendix Section C.1 lists the consulted sources.
[10] Source: https://de.wikipedia.org/wiki/Liste deutscher Vornamen aus der Bibel (accessed January 15, 2025).

Appendix subsection C.1 describes the digitization process, provides a detailed description of the data processing, data coverage, and validation exercises testing the internal consistency of the data sources and representativeness of the sample.

Text data on doctrine. To measure doctrinal differentiation, I collect data on all known prints produced in the Holy Roman Empire between 1400 and 1700 from the *Universal Short Title Catalogue* (USTC) (2025). I match authors to *Deutsche Biographie* (Hockerts and Lanzinner 2022) to infer confession and use USTC subject classifications. I match print locations to towns and their religious status.

For the 53,904 prints matched to authors and locations, I use GPT to translate short titles from Latin and early modern German to modern German, remove procedural information (e.g., dissertation metadata), and generate embeddings via OpenAI API.[11] For an alternative interpretable approach, I follow Gentzkow, Shapiro, and Taddy (2019) in processing titles with stopword removal, stemming, and bigram tokenization.

Supplementary institutional data. I complement identity data with information on institutional and religious history. I map towns to religion in a given period using a three step process. First, I code year of adoption of reform and counter-reformation from *Deutsches Städtebuch* (Keyser et al. 1939). Second, if the information is not given, I use religious status in 1546 and 1650 from Becker and Pascali (2019) building on the maps by Zeeden (1986). Third, I assign all towns that are Protestant by 1546 to be part of the eventual Protestant group for all years from 1480 to 1546 to study base. Based on religious status, I code whether towns are at religious borders (neighboring towns with opposing confession). I use Becker and Pascali (2019) for population estimates by century. I add geographic characteristics: agricultural suitability (Fischer et al. 2021), terrain ruggedness, distances to coast and navigable rivers (Cantoni, Mohr, and Weigand 2024), number of markets (Cantoni, Mohr, and Weigand 2020a), construction activity (Cantoni, Mohr, and Weigand 2020b), ruler mappings 1500-1789 (Cantoni, Mohr, and Weigand 2019), fiscal centralization (Cantoni, Mohr, and Weigand 2024), and school establishments from *Deutsches Städtebuch* (Keyser et al. 1939).

3.2 Church Enforcement

I compile novel data on local church enforcement—the institutional infrastructure through which churches monitored congregations, disseminated doctrine, and shaped religious behavior.

Catholic enforcement infrastructure. For Catholic regions, I collect information on Jesuit activity and monastic presence. Data on Jesuit college foundations (and closures) come from Niedersächsische Akademie der Wissenschaften (2025). I match Jesuit presence to each town by year. From the same source, I obtain information on monasteries and their activity periods. Borders and seats of bishoprics in 1500 are from Niedersächsische Akademie der Wissenschaften (2020).

[11] Appendix Section D.1 details data processing.

Protestant enforcement infrastructure. For Protestant regions, I use the *Repertorium Eruditorum Totius Europae* (De La Croix 2021) to identify Protestant universities and academies, including foundation dates, activity periods, associated scholars, and confessional affiliation. I supplement this with information on *Fürstenschulen* and *Gymnasium Illustre* (academic schools in Protestant territories) from Walter (2017). I match the presence of active Protestant academies to each town by year. I add data on church ordinances from *Die evangelischen Kirchenordnungen des XVI. Jahrhunderts* (Sehling 1902), as coded by Dittmar and Meisenzahl (2020). This records which cities adopted new ordinances regulating religious life and public good provision.

Church visitations. I create a novel dataset on church visitations—supervisory inspections by higher church authorities to ensure discipline, correct doctrine, and proper administration. Data come from *Repertorium der Kirchenvisitationsakten* (Zeeden 1982), covering visitations in modern-day states of Baden-Württemberg and Hesse during the sixteenth and seventeenth centuries.[12] I record when each town experienced its first visitation and total visitation frequency to capture monitoring intensity. From the same source, I record seats of Protestant *Superintendenturen* (bodies conducting parish visitations) and compute each Protestant town's distance to the nearest *Superintendentur*.

Enforcement channels. Two variables proxy investments in persuasion and doctrinal dissemination—key mechanisms through which enforcement operates.

First, I gather information on priests. Data on notable clergy comes from *Deutsche Biographie* (Hockerts and Lanzinner 2022), using place of death to link them to the nearest town. Place of death better proxies where clergy were active than birthplace. This constructs a panel of clergy personnel by town and decade, measuring local enforcement capacity through human capital. For the Duchy of Württemberg, the *Pfarrerbuch des Herzogtums Württemberg* (2025) provides detailed biographical information on all priests active in the region from 1500 to 1806. I construct full educational and career trajectories, geolocate places of activity, and match them to towns in my dataset.

Second, I use the universe of religious prints from USTC (2025), matching print locations to nearest towns. Religious printing activity captures doctrinal dissemination intensity—the primary channel through which theological positions reached congregations. I construct town-decade panels of religious print production.

Adverse shocks. To explore alternative explanations, I gather data on shocks that might affect naming patterns. Conflict incidents come from Cantoni and Weigand (2021). Pandemic and fire outbreaks are coded from *Deutsches Städtebuch* (Keyser et al. 1939). Winter temperature data are from Luterbacher et al. (2004); I code winters with average temperatures below 1 standard deviations from the long-run average as shocks. Following Barber, Jetter, and Krieger (2023), I construct local exposure to solar eclipses using data from Jubier (2025), defining treatment when eclipses occurred after sunrise and before sunset with obscuration rates above 80%.

[12] Unfortunately the project to compile a comprehensive overview of church visitation records was never finished with only the two volumes covering modern-day Baden-Württemberg and Hesse published.

3.3 Outcomes

To investigate outcomes of confessional identity formation, I collect additional data at both individual and group levels.

Individual-level outcomes. Two sources provide data on individual level outcomes. First, the church baptism records described in Section 3.1 enable measurement of family transmission patterns. I construct parent-child pairs to measure intergenerational transmission of confessional identity (correlation between parents' and children's name scores) for all records where information on parents is recorded. If information on both parents is available, I construct married couples to measure assortative matching (correlation between husbands' and wives' name scores) removing duplicates from couples with multiple children in my data.

Second, *Deutsche Biographie* (Hockerts and Lanzinner 2022) provides occupational information for 730,359 notable individuals. I use the categorization of occupations into church, administrative, military and business careers. Combined with individuals' name scores from birthplace, this enables analysis of how confessional identity predicts career paths. I use place of birth to link individuals to towns, as birth location captures childhood exposure to enforcement rather than later career location.

Group-level outcomes: conflict. To examine whether confessional polarization affected collective violence, I use conflict data from Cantoni and Weigand (2021) on the Thirty Years' War (1618-1648). Crucially, this dataset records which alliance troops belonged to in each incident—the Imperial-Catholic faction or the Protestant Union. This allows testing whether towns with stronger confessional signals faced differential targeting by opposing alliances. I construct town-level indicators for any troop exposure and separate indicators for exposure to Catholic League versus Protestant Union troops.

4 Descriptive Patterns: The Emergence of Confessional Identity

This section documents the emergence of confessional identity through naming patterns. I begin by defining how names signal religious identity and establishing the empirical measure used throughout the paper. I then present a puzzle: three illustrative names that follow divergent paths after the Reformation, becoming Protestant, Catholic, or remaining neutral. Aggregating across all names reveals systematic polarization beginning in the 1580s—more than six decades after the Reformation's start. Finally, I show that doctrinal differentiation in printed texts follows a similar but earlier pattern, suggesting a top-down force where theological divergence gradually translates into lay identity formation.

4.1 Measuring Confessional Identity Through Names

First names provide a revealed-preference measure of confessional identity. Unlike self-reported religious affiliation or church membership records, naming choices reflect parents' desired identity expression at a critical moment: the child's baptism. Names have no inherent confessional content—their confessional meanings emerge entirely from social convention. This makes them ideal for studying identity formation:

observing how names acquire Protestant or Catholic meanings reveals the process through which religious identity crystallizes.

To measure how strongly a name signals Protestant versus Catholic identity, I construct the Protestant name score for each name in each decade. Following an extensive literature using names to measure identity (Fryer and Levitt 2004; Bazzi, Fiszbein, and Gebresilasse 2020; Abramitzky, Boustan, and Eriksson 2020; Fouka 2020; Bentzen and Andersen 2022), the score captures the posterior probability that an individual is Protestant given their name, assuming a neutral prior:

$$\text{ProtScore}_{name,t} = \frac{\Pr(name \mid \text{Prot})_t}{\Pr(name \mid \text{Prot})_t + \Pr(name \mid \text{Cath})_t} \tag{1}$$

Intuitively, the Protestant name score measures "how Protestant" a name is. If Catholics and Protestants use a name with equal relative frequency, the score equals 0.5 (neutral). If only Protestants use a name, the score equals 1.0 (perfectly Protestant). If only Catholics use a name, the score equals 0 (perfectly Catholic). The Protestant name score has a clear Bayesian interpretation: it represents how much observing a name updates beliefs about confession. This interpretation guides the analysis throughout: names with scores far from 0.5 are "distinctive" or "clear signals," while scores near 0.5 indicate "neutral" or "ambiguous" names.

I compute these scores separately for each decade using observed birth records, allowing me to track how individual names' meanings evolve over time. For the main empirical analysis, I compute this using the full set of birth records in my sample in a given decade making use of all available data. For the name-level learning and group-level conformity analysis, I compute a local Protestant name score using the choice probabilities within a certain radius around towns to construct their respective Protestant name scores based on the usage patterns in their social environment.

I validate the Protestant name score measure in Appendix Section C.6. First, I show that names with extreme scores align with historians' assessments of highly partisan names (François's classification for Augsburg). Second, I demonstrate that confessional differentiation is distinct from purely geographic patterns: the Protestant name score shows low correlation with regional or linguistic cluster scores, confirming it captures confessional rather than geographic variation.

4.2 The Puzzle: Three Names, Three Paths

Before examining aggregate patterns, I illustrate the puzzle with three specific names that follow divergent trajectories after the Reformation. Figure 1 plots the Protestant name score over time for Christoph, Jacob, and Matthias—three of top 10 names in 1500.

Christoph begins the period as a neutral name (score ≈ 0.46 in the 1510s), used roughly equally by Catholics and Protestants until the 1580s. After 1580, the score rises steadily, reaching 0.57 by 1600, 0.67 by 1620. Within four decades Christoph had become a distinctively Protestant name, disproportionally used among Protestants for the next 200 years. *Matthias* follows the opposite trajectory. The score begins at 0.51 in the 1510s and declines steadily after 1560, reaching 0.41 in the 15702 and 0.3 by 1650. Matthias transforms from a neutral name into a distinctively Catholic name. *Jacob* exhibits a third pattern: persistent neutrality. The score fluctuates around 0.50 throughout the entire period, never deviating more than 0.05 from neutrality. Despite the religious upheaval, Johann remains a name used equally by both confessions.

This divergence illustrates the guiding puzzle: How do markers of identity emerge? More fundamentally, when and why do names' meanings change? All three names were common before the Reformation. All three remained common afterward. Yet they followed completely different trajectories in the confessional space.

Three core patterns require explanation. First, the association of names with confessional group takes time. Divergence only starts five decades after the Reformation starts, not immediately with Luther's 1517 break. This suggests naming patterns respond not to the theological event itself but to some slower process of identity formation. Second, once names gather meaning, they do so rapidly and persistently. Third, not all names become confessionally loaded. Popular names like Jacob or Johannes, consistently the most frequent name across all decades, remain neutral, i.e., do not become informative of the person's confession.

4.3 Aggregate Differentiation in Naming

To measure aggregate differentiation, I compute the average probability that a neutral observer would correctly infer an individual's confession from their name. Following Gentzkow, Shapiro, and Taddy (2019), average partisanship measure captures how informative names are about group membership:

$$\text{(2)} \qquad \text{Average Partisanship}_t^{\text{name}} = \frac{1}{N_t} \sum_{i \in t} \Big[y_i \cdot \text{ProtScore}_{name_i,t} + (1 - y_i) \cdot (1 - \text{ProtScore}_{name_i,t}) \Big]$$

where $y_i \in \{0, 1\}$ indicates whether individual i is Protestant, and the sum runs over all births in decade t. This equals the expected posterior probability of correct classification. In other words, if I hear someone's name, how confident can I be about their confession? A value of 0.5 indicates names provide no information (random guessing), while values approaching 1.0 indicate perfect segregation.

Figure 2, Panel A, plots Average Partisanship over time. The red line shows estimates using true confessional labels, with 95% confidence intervals from the empirical distribution. The gray dashed line shows a placebo test using randomly shuffled labels. The placebo benchmark hovers around 0.5 (random guessing), indicating that finite-sample bias does not substantively drive the observed patterns (Gentzkow, Shapiro, and Taddy 2019).

The aggregate pattern confirms the illustrative example. First, there is minimal differentiation before 1517. Average Partisanship equals 0.52 in 1510, only slightly above random chance. This indicates that regions that later become Protestant and those that remain Catholic had similar naming practices before the Reformation. Any pre-existing differences were small. Second, differentiation does not gain meaningful traction immediately after 1517. Despite Luther's break, the Confessio Augustana (1530), and the Schmalkaldic War (1546-47), naming patterns remain largely unchanged through to the 1570s. Third, the data shows rapid differentiation between 1570 and 1610, Average partisanship rises from 0.52 to above 0.54, doubling the average predictive power of names over random guessing from 2 percentage points to

4 percentage points. After stabilizing during the Thirty Years' War (1618-1648) and the remainder of the seventeenth century, differentiation resumes its upward trend albeit at a much lower rate during the eighteenth century, reaching 0.57 by 1790. This long-run increase indicates that confessional identity became a permanent and salient social cleavage for the next two hundred years.

Figure 2, Panel B, provides a different way of assessing the magnitude focusing on the idea that pattern recognition typically takes more than just one isolated signal. Recall that average partisanship is the posterior that a neutral observer expects to assign to a person's true confession after learning their name. Figure 2, Panel B, extends this concept to show the expected posterior for groups of various size. In 1550, an observer learning the names of a family of 4, would expect to have a posterior of around .58 on the family's true confession. By 1650, this value would have increased to .63. For a group of 10, it would have gone from .65 to .72 in 1650.

Robustness and alternative measures. These patterns are robust to examining men and women separately, elite versus non-elite samples, and even within the bi-confessional city of Augsburg, ruling out broad differential geographic trends as the primary driver of divergence in naming patterns—even when living in the same place, confessional groups adopted different naming conventions (Appendix Section C.7). Alternative measures yield similar conclusions. Appendix Figure C.15 presents the Theil Index with bootstrap correction (Cantoni, Mohr, and Weigand 2025), which measures distributional divergence without assuming a prediction framework. The Theil Index shows the same timing: minimal divergence before 1570, rapid increase from 1580-1610, continued rise thereafter.

4.4 Differentiation in Doctrine: Evidence from Printed Texts

The delayed emergence of naming differentiation raises a question: did doctrinal differences also emerge gradually, or did theological divergence precede lay identity formation? To address this, I examine confessional differentiation in printed texts as a proxy for elite theological discourse. I represent each title as a text embedding and estimate a penalized logistic regression predicting whether the publisher is Protestant building on the approach by Gentzkow, Shapiro, and Taddy (2019). This yields predicted probabilities $\hat{p}(x_i)$ for each title i. Analogously to the average partisanship in names, I then compute:

$$\text{Average Partisanship}_t^{\text{text}} = \frac{1}{N_t} \sum_{i \in t} \left[y_i \cdot \hat{p}(x_i) + (1 - y_i) \cdot (1 - \hat{p}(x_i)) \right] \tag{3}$$

measuring how accurately language predicts a text's confessional origin. The full data processing pipeline and methodological details are discussed in Appendix D.

Figure D.1 plots Average Partisanship for printed texts over time. Three findings emerge. First, I observe an immediate jump in doctrinal differentiation in the 1520s. This sharp spike reflects Luther's theological writings and the immediate pamphlet wars between reformers and defenders of Catholic doctrine. The early Reformation was primarily a theological conflict, visible immediately in print. Second, differentiation temporarily declines up until the Religious Peace of Augsburg in 1555. Third, sustained differentiation resumes around 1560 and rises steadily through 1620. Importantly, Panels A and B of

Figure D.1 indicate that this later rise extends beyond purely theological texts to other subjects such as legal, historical, and literary works—confessional identity was becoming a broader organizing principle. Appendix Section D.3 employs alternative measurement approaches (bigram-based classification, out-of-sample AUC) and provides further illustrative examples of the type of content that becomes predictive of confession.

The descriptive patterns establish three facts. First, individual names follow divergent paths after the Reformation, with some becoming Protestant, others Catholic, and others remaining neutral. Second, aggregate naming differentiation begins around 1580—more than six decades after Luther's break—and accelerates through the early seventeenth century. Third, doctrinal differentiation in printed texts responds immediately to the Reformation and seems to lead subsequent polarization in naming.

These patterns motivate the analysis in subsequent sections. What forces drive some names to become confessional markers while others remain neutral? Why does differentiation accelerate in the 1580s rather than immediately after 1517? How do naming patterns spread across space and time? The empirical section of the paper investigates these questions by examining enforcement (Section 5) and social transmission (Section 6) as drivers of identity formation.

5 Church Enforcement and Confessional Identity

Empirical setting. I study how local church enforcement shaped religious identity, measured by the average Protestant name score in town c and decade t. The main proxy for local Academy exposure is the presence of institutions of higher learning. For Catholics, I use Jesuit colleges, the monastic order at the forefront of the Counter-Reformation, central to training loyal and effective priests. For Protestants, I use universities and academic gymnasiums, which fulfilled a similar function on the Protestant side.

Figure A.1 plots the raw data. Panel A compares Catholic towns that ever hosted a Jesuit college (red) with those that never did (gray). Both groups had similar name scores until the first Jesuit college opened in Ingolstadt in 1556. Thereafter, Jesuit towns adopted more distinctly Catholic naming patterns, with the gap shrinking again after the Jesuit suppression in 1773. Panel B shows a similar pattern for Protestant towns: those with academies adopted more distinctly Protestant names than those without. These descriptive patterns suggest that higher academy exposure is associated with stronger confessional differentiation. The empirical analysis investigates this link in a more rigorous way.

A central concern is that unobserved variables could affect both academy exposure and naming patterns, confounding causal interpretation. I address this by exploiting geographic and temporal variation in exposure to colleges and academies and by simulating counterfactual college placements to adjust for expected exposure to treatment (Athey and Imbens 2022; Borusyak and Hull 2023).

The choice of location of Jesuit colleges and Protestant academies was shaped by both strategic and logistical considerations. Strategically, rulers chose whether, when, and where to invite the Jesuits or establish Protestant academies. Logistical constraints affected site selection and implementation: colleges required a steady flow of resources to attract scholars, repurposed or newly built facilities to house students, and sustained financial support. However, idiosyncratic preferences and constraints of territorial lords often proved decisive in the final decision where to place colleges.

These factors raise two identification concerns. First, strategic placement may correlate with pre-existing trends in religious loyalty: rulers might have targeted towns experiencing decline in confessional commitment, threatening the parallel-trends assumption in a difference-in-differences design. Second, logistical factors such as town size could confound results: larger towns were more likely to host colleges but might also differ systematically in religious attitudes due to their economic structure. Figures A.3, Panel A, and A.4, Panel A examine balance on baseline town characteristics. Exposure was indeed more likely in larger towns and those with pre-existing educational infrastructure such as universities, schools, or monasteries, highlighting the need to adjust for these confounders in the empirical analysis.

Baseline empirical strategy. I study the relationship between local Academy exposure and confessional identity using variation in the timing and location of Jesuit colleges and Protestant academies across towns. The outcome is the average Protestant name score in town c and decade t.

Institutions of higher education serve as the main proxy for Academy exposure, as they were critical for training clergy. Figure 4 shows the geographic and temporal variation in exposure to Jesuit colleges and Protestant academies. A naive two-way fixed effects (TWFE) specification would estimate

$$ProtScore_{c,t} = \beta Enforcement_{c,t} + \alpha_c + \alpha_t + \epsilon_{c,t}, \tag{4}$$

where $Enforcement_{c,t}$ is an indicator for Jesuit or academy presence, and α_c and α_t are town and decade fixed effects. These absorb time-invariant town characteristics and common naming shocks. However, when effects differ across cohorts or evolve over time, β is a non-transparent weighted average of many group–time comparisons and may even place negative weight on some effects (Goodman-Bacon 2021; De Chaisemartin and D'Haultfœuille 2020; Sun and Abraham 2021).

To address this, I follow Callaway and Sant'Anna (2021) and estimate cohort-by-time average treatment effects:

$$ATT(g,t) = \mathbb{E}[\, ProtScore_{c,t}(1) - ProtScore_{c,t}(0) \mid G_c = g\,], \qquad t \geq g, \tag{5}$$

where G_c is the first decade town c is exposed to an academy ($G_c = \infty$ for never-treated towns). In each period t, towns first treated in decade g are compared to not-yet-treated or never-treated towns, yielding $ATT(g,t)$ for each cohort-time pair. These effects are then aggregated, either across t to form an overall average effect or by event time $e = t - g$ to produce an event-study profile. This approach avoids the problematic weighting when relying on naive TWFE in a staggered adoption setting and makes the timing of effects transparent.

Table 1 reports the aggregated estimates from Equation 5. Column 1 shows that Jesuit exposure is associated with a .022 lower Protestant name score in Catholic towns (39% of a standard deviation in the Protestant name score), indicating stronger adoption of Catholic names. Column 3 shows that Protestant academies are associated with a .017 higher score in Protestant towns (29% of a standard deviation).

Next, I examine whether towns exposed to academies experienced differential time-varying shocks that influenced their naming choice prior to being treated. To do so, I estimate the event study version of equation 5. Results are shown in Figure 5. Panel A indicates no differential pre-trends between Catholic towns that later host Jesuits and those that do not. The difference becomes visible four decades after Jesuit arrival and continues to widen for over a century. Panel B shows similar results for Protestant towns: pre-treatment differences are small and statistically insignificant. Effects start to materialize after three decades and continue to grow for over ten decades. Appendix Figures A.6 and A.7 show results aggregated by calendar time and cohort.

Addressing selection into treatment. A key concern is that the assignment and timing of Jesuit colleges and Protestant academies were not random: they depended on strategic and logistic considerations that may correlate with towns' trajectories of religious identity. Larger towns with universities or monasteries were more attractive sites for early investments, and rulers may have deliberately targeted towns where confessional loyalty was weakening. Failing to account for these factors could bias estimates and threaten a causal interpretation.

To address this, I construct a measure of *expected exposure* that captures the probability that a town would receive a Jesuit college or Protestant academy based solely on its baseline characteristics – city size, monastery, school, and university presence – holding the overall number of colleges or academies active in a decade constant. This simulation follows the logic of Borusyak and Hull (2023): by conditioning on the structural assignment mechanism, I can isolate quasi-random deviations from predicted exposure that are plausibly idiosyncratic. I run 100 counterfactual simulations of where colleges or academies could have been established given baseline characteristics and compute the average predicted exposure by town and decade. This *expected exposure* is then included as a control variable in Equation 5. Appendix Section B.3 describes the implementation algorithm in detail.

The key identifying assumption is that, conditional on expected exposure, residual variation in academy exposure is as good as random, and potential outcomes follow parallel trends in the absence of treatment. In other words, after purging the predictable component of treatment based on town characteristics, remaining variation reflects idiosyncratic institutional decisions rather than systematic differences across towns. This approach downweights evidence from towns that would almost certainly receive an institution under any allocation rule and places greater weight on towns where exposure reflects discretionary, ruler-specific initiatives. Figures A.3 and A.4 show that controlling for expected exposure substantially improves balance on observables. Figures 6 and A.2 plot maps of expected exposure and the resulting residual variation.

An example illustrates the logic. Trier and Landsberg am Lech both received Jesuit colleges – Trier in 1561 and Landsberg in 1576 – but they differed sharply in their baseline characteristics. Trier was a major city with 10,000–15,000 inhabitants around 1500, an active university founded in 1473, multiple schools, and a Benedictine monastery, yielding an expected exposure of roughly 75% by 1750 in the simulations. A town like Trier would almost inevitably attract a Jesuit college given its size, infrastructure, and strategic importance in the Catholic heartland. By contrast, Landsberg am Lech was a much smaller market town with fewer than 5,000 inhabitants and only a parish school in the fifteenth century. Its nearby Benedictine monastery at Sandau, founded in the eighth century, had been destroyed in the mid-tenth century and abandoned ever since, leaving Landsberg without a significant religious institution at baseline. In the counterfactual simulations, Landsberg's probability of receiving a Jesuit college is just 9% by 1750. Its eventual selection was largely due to the personal prestige and initiative of Schweikhard von Helfenstein, president of the Reichskammergericht (1562–1564), who leveraged his influence to secure a Jesuit foundation for the town. Adjusting for expected exposure therefore discounts predictable cases like Trier and exploits quasi-random variation from cases like Landsberg, where exposure arose from idiosyncratic, ruler-specific decisions.

Table 1, Columns 2 (Jesuit exposure) and 4 (Protestant academy exposure) report the results after adjusting for expected exposure. The point estimates are similar in magnitude to the baseline results, providing reassurance that selection on observables is not driving the findings and strengthening the case for a causal interpretation.

In Appendix Section B, I further demonstrate robustness using alternative approaches to account for selection into treatment. First using doubly robust inverse probability weighting (DR-IPW) using baseline city characteristics. Results are reported in Table B.3 following Sant'Anna and Zhao (2020) and Callaway and Sant'Anna (2021). Second, I use probit nearest-neighbor matching on baseline covariates. Results are reported in Table B.2. These approaches yield consistent results, corroborating that the observed effects reflect the causal impact of investments in local Academy exposure on the evolution of confessional identity. Appendix Section B summarizes and discusses a series of additional robustness checks.[13]

5.1 How Does Enforcement Work? The Role of Clergy and Printing

The previous sections established that colleges increased confessional differentiation. This section examines the mechanism: *how did colleges affect what parents named their children?*

I argue colleges operated through two complementary channels. First, they trained clergy who embedded confessional meanings through catechesis, preaching, and pastoral oversight. Second, they produced religious texts that standardized doctrine and disseminated confessional narratives. Together, these channels built "persuasion capital"—the capacity to shape identity systematically.

This parallels findings in political economy showing that state capacity depends on bureaucrat quality: meritocratic and well-trained agents better implement policy and enforce compliance (Weber 1912; Evans and Rauch 1999; Besley and Persson 2009; Dittmar and Meisenzahl 2020). Jesuit colleges and Protestant academies acted as training grounds for such "ideological bureaucrats," equipping them with human capital and standardized narratives to operate the persuasion apparatus.

Channel 1: Clergy training and quality. Illustrative evidence comes from microdata on priests. The *Pfarrerbuch des Herzogtums Württemberg* (*Württemberger Pfarrerbuch* 2025) records full educational histories and careers of all priests active in Württemberg from 1500 to 1806. Figure A.14 shows rapid growth in clergy education after the Reformation. Between 1520 and 1620, the share of parish priests

[13] Church ordinances are another plausible proxy of local church enforcement. Ordinances not only reorganized public good provision more broadly (Dittmar and Meisenzahl 2020) but were primarily concerned with regulating religious life. I provide supportive evidence that this alternative proxy of local church enforcements is consistent with the main result. Figure A.13, Panel A, shows average Protestant name scores by ordinance status; Figure A.13, Panel B, presents event-study estimates relative to the 1540s. As Dittmar and Meisenzahl (2020) document, most adopting towns had ordinances in place by the Schmalkaldic War (1546). I find that ordinances are also associated with stronger confessional differentiation in names, consistent with my main proxy for church enforcement, the presence of confessional academies.

with university education and Magister degrees increased from 12% to 68%. This indicates intentional efforts to raise priestly quality and expand persuasion capacity at the parish level.

To test whether colleges systematically increased clergy presence, I estimate Equation 5 with notable clergy per capita as the outcome. Table 2, Columns 1-2 show that Jesuit colleges increase Catholic clergy. Columns 3-4 show Protestant academies similarly increase Protestant clergy. Effects are statistically significant at the 5 and 10 percent level respectively.

Channel 2: Religious printing. Colleges also served as centers of doctrinal production. Using the universe of religious prints from the Universal Short Title Catalogue (2025), I test whether colleges increased local religious printing activity. Columns 5–8 of Table 2 show that Jesuit and Protestant academy town produce a higher number of religious prints. The estimated coefficients are statistically significant at the 5 percent level.

Religious printing provided the standardized catechisms, sermons, and devotional materials through which clergy transmitted confessional meanings. The combination of trained personnel and doctrinal materials created the infrastructure for systematic indoctrination and enforcement.

5.2 Persistence and Removal of Enforcement

The previous sections documented the link between academies and confessional differentiation, operated through clergy and printing. This section examines persistence: *did effects require sustained oversight, or did they reflect one-time permanent shifts?*

Two pieces of evidence address this question. First, I examine whether repeated church visitations—ongoing monitoring by ecclesiastical authorities—strengthened differentiation. Second, I study whether removing enforcement capacity through the 1773 Jesuit ban reversed differentiation. Together, these tests distinguish between deeply internalized identity (which would fully persist absent enforcement) and identity partially maintained through continued oversight.

5.2.1 Repeated Church Visitations

Church visitations were supervisory inspections by higher church authorities to ensure doctrinal conformity and discipline at the parish level. If sustained monitoring mattered, visitation intensity should predict stronger differentiation.

Data and measurement. I use the *Repertorium der Kirchenvisitationsakten* (Zeeden 1982), which records parishes visited in modern-day Baden-Württemberg and Hesse during the sixteenth and seventeenth centuries. Although the compendium was never completed to cover the remainder of Germany, it provides systematic information on 460 towns, allowing me to measure timing of first visitations and total visitation frequency. Figure A.8 maps these towns and their first visitation dates.

First visitation effects. Applying the staggered difference-in-differences framework from 5, I exploit geographic and temporal variation in when towns experienced their first visitation. Table A.1 reports results. Columns 1–2 present estimates for Catholic towns; Columns 3–4 for Protestant towns.

Catholic towns experienced a .029 lower Protestant name score following their first visitation relative to not-yet-visited towns. This represents .76 standard deviations. Protestant towns experienced a .021 higher score, representing .55 of a standard deviation in the sample restricted to Baden-Württemberg and Hesse. Despite the small sample, effects are statistically significant and consistent with the college results: direct church oversight strengthened confessional differentiation.

Intensive margin: Distance to Superintendentur as instrument. I next examine whether visitation frequency mattered. A key concern is endogeneity: supervision might have targeted areas with weak confessional adherence or avoided hopeless cases. To address this, I instrument visitation intensity with distance to the nearest Superintendentur (regional ecclesiastical administration) for all Protestant towns in the sample covered in Zeeden (1982).

The identifying assumption is that proximity affected visitation frequency for logistical reasons—shorter distances lowered supervision costs—but is otherwise unrelated to unobserved identity trajectories. Figure A.10 shows distance to Superintendentur is balanced with respect to baseline covariates (city size, pre-1500 educational infrastructure, monasteries). Figure 7, Panel B shows distance is unrelated to baseline naming patterns in the 1520s, supporting the exclusion restriction.

Table A.2 presents instrumental variables results. In cross-sections for 1620 (Columns 1–2) and 1720 (Columns 3–4), instrumented visitation frequency positively predicts Protestant name scores: towns visited more frequently adopted more distinctly Protestant names. Including region fixed effects (Columns 2 and 4) strengthens identification by exploiting only within-region variation in distance, thereby purging broad territorial differences.

Complementary evidence: Priest turnover. Table A.3 provides suggestive evidence linking visitations to personnel discipline. Using detailed priest career data from the *Pfarrerbuch des Herzogtums Württemberg* (*Württemberger Pfarrerbuch* 2025), I find priest turnover is significantly higher in decades with at least one visitation. This is consistent with visitations disciplining clergy by credibly threatening replacement for poor performance. Together, these results indicate repeated oversight by church authorities strengthened differentiation on the ground.

5.2.2 Removal of Enforcement: The 1773 Jesuit Ban

As a complementary test, I study consequences of removing enforcement capacity. The Jesuits faced mounting opposition from Bourbon monarchs in the mid-eighteenth century, accused of political meddling and economic subversion. On 21 July 1773, Pope Clement XIV issued the *Dominus ac Redemptor*, formally suppressing the order. Implementation in the Holy Roman Empire was swift.

To estimate the effect of the ban, I estimate:

$$ProtScore_{c,t} = \sum_{\tau=-5}^{5} \beta_\tau \left(EverJesuit_c \times RelativeDecade_{\tau(t)}\right) + \alpha_c + \alpha_t + \epsilon_{c,t}, \tag{6}$$

where $RelativeDecade_{\tau(t)}$ measures decades relative to 1773. Figure A.11 shows that Protestant name scores in previously Jesuit towns rise immediately after the suppression and continue to increase thereafter, indicating a shift toward less distinctively Catholic naming. Crucially, there is no evidence of differential pre-trends at this late stage of Jesuit activity, supporting the parallel-trends assumption. This pattern is consistent with the raw data in Figure A.1, Panel A, where the Jesuit/non-Jesuit gap narrows after 1773.

Taken together, these results demonstrate that church enforcement through educational institutions and sustained monitoring via visitation systems were the primary mechanisms seeding initial variation in confessional identity and maintaining persistent polarization in naming practices.

An alternative explanation is that adverse events—violent conflict, pandemics, environmental shocks—heightened in-group/out-group salience and deepened confessional divides through bottom-up crisis responses rather than top-down institutional enforcement. Appendix Section B.2 tests this hypothesis by examining whether exposure to five types of adverse shocks (military conflict, plague outbreaks, severe winters, solar eclipses, and urban fires) predicts stronger confessional differentiation in naming. I find no evidence that any of these shocks systematically increased naming polarization. This null result contrasts sharply with the robust positive effects of academy exposure, indicating that the documented rise in confessional identity reflects deliberate institutional investments rather than spontaneous responses to existential threats.

6 Social Transmission of Religious Identity

The previous section established that church enforcement increases confessional differentiation in treated locations. However, a key question remains: *how do naming patterns spread beyond directly-treated areas?* Enforcement operates only in specific places at specific times, yet confessional divergence occurs across the entire territory and unfolds gradually over decades. Two patterns suggest social transmission mechanisms at work. First, locations without colleges or printing presses also exhibit increasing differentiation—patterns must diffuse spatially from treated to untreated areas. Second, even in treated locations, naming changes occur gradually rather than instantaneously—consistent with learning or imitation rather than pure enforcement compliance.

I examine whether identity patterns spread through decentralized social transmission: parents observe nearby co-religionists' choices and adjust their own decisions accordingly. This process could operate through two conceptually distinct mechanisms. *Learning* is information-based: parents acquire knowledge about which names signal which confession by observing usage patterns. If most Protestants nearby use "Christoph," this reveals Christoph's Protestant association. *Conformity* is preference-based: parents desire to match the overall confessional intensity of their reference group, becoming more distinctively Protestant when surrounded by Protestants with strong confessional signals.

These mechanisms map naturally to different levels of aggregation. At the *name level*, parents face an information problem: which names signal which confession? Observing that nearby Protestants disproportionately use "Christoph" provides evidence about Christoph's meaning, generating convergence in individual names' usage. At the *group level*, communities face a coordination problem: how Protestant or Catholic should we be? Observing that nearby Protestant communities express strong confessional identity creates pressure to match their intensity, generating convergence in aggregate confessional signals.

While these mechanisms differ conceptually—one updates beliefs about name meanings, the other reflects social preferences about intensity—both generate observationally similar gap-closing dynamics. I cannot cleanly separate information-based learning from preference-based conformity using reduced-form specifications alone. However, examining convergence at both levels of aggregation provides complementary evidence. The name-level analysis tests whether parents update usage of individual names toward local patterns—consistent with learning which names mean what. The group-level analysis tests whether communities coordinate their overall confessional intensity with nearby same-denomination communities—consistent with conforming to how distinctive neighbors are. Additional heterogeneity tests—documenting faster adjustment when signals are more credible—provide suggestive evidence consistent with information processing.

The analysis proceeds in two steps. Section 6.1 examines name-level convergence: individual names' usage adjusts toward spatial network averages, with faster convergence when signals are clearer. Section 6.2 examines group-level convergence: aggregate confessional identity adjusts toward same-denomination neighbors' intensity, with adjustment operating through portfolio reweighting rather than mechanical herding. Together, these patterns demonstrate that social transmission amplified enforcement effects and drove spatial diffusion beyond directly-treated locations.

6.1 Name-Level Learning

I begin by examining social transmission at the disaggregated level: how individual names' usage evolves over time and space. Consider a parent in 1580 choosing a name for her Protestant son. She wants to express Protestant identity but faces uncertainty: which names signal Protestant versus Catholic affiliation? First names carry no intrinsic religious content—their confessional meanings emerge entirely from social convention.

To resolve this uncertainty, she observes naming patterns among nearby Protestants. If most Protestant neighbors name their sons "Christoph," this reveals Christoph's Protestant association. She incorporates this information when making her own choice, potentially adopting Christoph herself. If this process operates systematically, names more popular among nearby co-religionists should exhibit increasing local usage—a testable prediction of information-based learning.

6.1.1 DeGroot Learning Framework

I model this learning process using the DeGroot (1974) framework. DeGroot learning provides a tractable model of belief updating on networks where agents form beliefs as weighted averages of neighbors' beliefs rather than through full Bayesian inference. This bounded rationality approach is well-suited to the historical context: parents lacked centralized information about name meanings and instead relied on decentralized observation of local usage patterns. The framework is tractable and generates testable gap-closing dynamics (Golub and Jackson 2010).

In each period, parents form beliefs about name n's confessional association by observing usage among spatially proximate co-religionists. Let $\text{Network}_{n,g,\ell,t}$ denote the weighted average Protestant score for name n among nearby co-religionists:

$$\text{Network}_{n,g,\ell,t} = \sum_{\ell'} w_{\ell\ell'} \cdot \text{ProtScore}_{n,g,\ell',t} \tag{7}$$

where $\text{ProtScore}_{n,g,\ell,t}$ is the local Protestant score for name n among confession g in location ℓ at time t with the Protestant name score computed as in 4.1, pooling births within 50km of ℓ. $w_{\ell\ell'}$ reflects network connectivity (uniform weights within 50km radius).

Parents then update their own usage as a weighted average of own past usage and neighbors' network signal:

$$\text{ProtScore}_{n,g,\ell,t} = (1-\phi) \cdot \text{ProtScore}_{n,g,\ell,t-1} + \phi \cdot \text{Network}_{n,g,\ell,t-1} \tag{8}$$

Parameter $\phi \in [0,1]$ measures adjustment speed: how much weight parents place on neighbors' patterns versus own past usage. Differencing equation (8) yields the gap-closing form:

$$\Delta\text{ProtScore}_{n,g,\ell,t} = \phi \cdot \underbrace{[\text{Network}_{n,g,\ell,t-1} - \text{ProtScore}_{n,g,\ell,t-1}]}_{\equiv \text{Gap}_{n,g,\ell,t-1}} \tag{9}$$

Locations with below-network usage (negative gap: own score $<$ neighbors' average) increase usage; locations with above-network usage (positive gap: own score $>$ neighbors' average) decrease usage. All locations converge toward neighbors' network average at rate ϕ per period.

Spatial amplification. This framework generates propagation effects: enforcement shocks in one location spread through the network. If a college in location ℓ_0 increases usage of name n, this raises the network average for neighbors, inducing them to increase own usage, which then affects their neighbors' network averages. Effects decay geometrically at rate ϕ^k for k-degree neighbors, creating spatial spillovers beyond directly treated locations.

6.1.2 Empirical Implementation

For each name n, confession g, location ℓ, and decade t, I construct: (i) $\text{ProtScore}_{n,g,\ell,t}$ measuring own usage, (ii) $\text{Network}_{n,g,\ell,t} = \sum_{\ell'} w_{\ell\ell'}\text{ProtScore}_{n,g,\ell',t}$ measuring neighbors' average within 50km, and (iii) $\text{Gap}_{n,g,\ell,t} = \text{Network}_{n,g,\ell,t} - \text{ProtScore}_{n,g,\ell,t}$ measuring the deviation. The baseline specification is:

$$\Delta\text{ProtScore}_{n,g,\ell,t} = \phi \cdot \text{Gap}_{n,g,\ell,t-1} + \gamma_n + \alpha_{g,\ell} + \delta_t + \varepsilon_{n,g,\ell,t} \tag{10}$$

where γ_n are name fixed effects (absorbing aggregate trends in popularity), $\alpha_{g,\ell}$ are location fixed effects (absorbing time-invariant composition), and δ_t are decade fixed effects. Standard errors are clustered at the location level.

Coefficient interpretation. $\hat{\phi}$ measures the fraction of the gap between own usage and neighbors' average that closes per decade. If $\hat{\phi} = 0.3$, then 30% of the distance toward neighbors' network average is traversed each decade, implying convergence half-life of $\ln(2)/\hat{\phi} \approx 2.3$ decades.

6.1.3 Signal Quality and Information Environment

If learning operates through information extraction rather than mechanical imitation, adjustment speed should vary with signal quality. I test three predictions by estimating:

$$\Delta\text{ProtScore}_{n,g,\ell,t} = \phi_0 \cdot \text{Gap}_{n,g,\ell,t-1} + \phi_1 \cdot \text{Gap}_{n,g,\ell,t-1} \times \text{Signal Quality}_{n,g,\ell,t-1} + \gamma_n + \alpha_{g,\ell} + \delta_t + \varepsilon_{n,g,\ell,t} \tag{11}$$

Signal credibility (HighUsage). Names with higher local frequency provide more observations, making the gap between own usage and neighbors' usage more statistically reliable. $\text{HighUsage}_{n,g,\ell,t}$ is an indicator for names in the top quartile of local usage frequency. If $\phi_1 > 0$, frequently-used names exhibit faster convergence—parents trust signals backed by larger samples.

Information reliability (NetworkDensity). Observing many Protestant neighbors provides reliable averages about appropriate naming. $\text{NetworkDensity}_{g,\ell}$ counts the number of towns with confession g within 50km. If $\phi_2 > 0$, locations with denser networks adjust faster—learning is more effective with larger reference groups reducing sampling uncertainty.

Interpretive clarity (LocalClarity). In locations where the overall naming portfolio consists primarily of distinctive names, parents more easily infer the general name-confession mapping. I construct:

$$\text{LocalClarity}_{g,\ell,t} = \sum_n \pi_{n,g,\ell,t} \cdot |\text{ProtScore}_n - 0.5| \tag{12}$$

measuring the average distinctiveness of names used locally. If $\phi_3 > 0$, locations with clearer naming environments exhibit faster adjustment—the local context facilitates interpretation of network signals.

Table 3 presents results. Column (1) shows baseline gap-closing ($\hat{\phi} \approx .35$): locations adjust own usage toward neighbors' naming patterns. Columns (2)-(4) show adjustment accelerates with signal quality—faster when names are frequently observed, networks are dense, and local portfolios are distinctive. This information-dependence is consistent with learning rather than mechanical imitation.

6.2 Group-Level Conformity

The previous subsection examined how individual names diffuse—testing learning about which names signal which confession. This subsection shifts to aggregate confessional identity—testing conformity in overall Protestant or Catholic intensity. The distinction matters: a location could learn name meanings perfectly yet still choose varying confessional intensity. Consider two Protestant communities both understanding that Christoph = Protestant, Matthias = Catholic, Jacob = neutral. One could use 80% Christoph/20% Jacob (strong intensity), the other 40% Christoph/60% Jacob (moderate intensity). Name-level learning explains coordination on meanings; group-level conformity explains coordination on intensity.

6.2.1 Aggregate Identity Dynamics

I measure aggregate confessional identity as the average Protestant score across all individuals in a confession-location-decade:

$$\bar{a}_{g,\ell,t} = \frac{1}{N_{g,\ell,t}} \sum_{i \in g,\ell,t} \text{ProtScore}_{i,g,\ell,t} \tag{13}$$

capturing overall confessional intensity rather than specific name usage. Following the name-level framework, I test whether aggregate identity exhibits gap-closing toward neighbors' average intensity. For each confession g, location ℓ, and decade t, I construct own aggregate identity $\bar{a}_{g,\ell,t}$, neighbors' average $\text{Network}_{g,\ell,t} = \sum_{\ell'} w_{\ell\ell'} \bar{a}_{g,\ell',t}$, and the gap $\text{Gap}_{g,\ell,t} = \text{Network}_{g,\ell,t} - \bar{a}_{g,\ell,t}$. The baseline specification is:

$$\Delta \bar{a}_{g,\ell,t} = \phi \cdot \text{Gap}_{g,\ell,t-1} + \alpha_{g,\ell} + \delta_t + \varepsilon_{g,\ell,t} \tag{14}$$

where $\alpha_{g,\ell}$ are location$\times$confession fixed effects and δ_t are decade fixed effects. Standard errors are clustered at the location level.

6.2.2 Heterogeneity and Portfolio Adjustment

As with name-level learning, I test whether adjustment varies with information quality:

$$\begin{aligned} \Delta \bar{a}_{g,\ell,t} = \phi_0 \cdot \text{Gap}_{g,\ell,t-1} + \phi_1 \cdot \text{Gap}_{g,\ell,t-1} \times \text{Signal Quality}_{g,\ell} \\ + \alpha_{g,\ell} + \delta_t + \varepsilon_{g,\ell,t} \end{aligned} \tag{15}$$

I test whether convergence accelerates with network density (more same-confession neighbors) and local clarity (more distinctive name portfolio). Additionally, I test whether convergence reflects mechanical concentration on fewer names or sophisticated portfolio adjustment:

$$\Delta \text{Concentration}_{g,\ell,t} = \rho \cdot |\text{Gap}_{g,\ell,t-1}| + \alpha_{g,\ell} + \delta_t + \varepsilon_{g,\ell,t} \tag{16}$$

where $\text{Concentration}_{g,\ell,t}$ is the normalized Herfindahl index of name usage. If $\rho < 0$, larger gaps induce *lower* concentration—communities adjust by reweighting diverse portfolios rather than herding on fewer names.

Table 4 presents results. Column (1) shows baseline gap-closing ($\hat{\phi} \approx .59$): communities adjust aggregate intensity toward neighbors' average. Columns (2)-(3) show adjustment accelerates with network density and signal clarity. Column (4) shows larger gaps induce lower concentration—conformity operates through portfolio reweighting, not mechanical herding. Appendix B.17 shows adjustment strength declines with distance, confirming transmission operates through local networks.

This section provided reduced-form evidence for social transmission at two levels. At the name level, individual names converge toward neighbors' usage patterns, with faster adjustment when signals are credible, reliable, and interpretable—consistent with information-based learning about which names signal which confession. At the group level, aggregate confessional intensity converges toward neighbors' average intensity, with faster adjustment in dense networks and clear environments, operating through portfolio reweighting—consistent with preference-based conformity about how Protestant or Catholic communities should be.

Three features emerge consistently. First, *gap-closing dynamics* indicate systematic adjustment toward neighbors' averages with convergence rates of approximately 30% (name-level) and 59% (group-level) per decade. Second, *information-dependence* shows adjustment accelerating where signals are clearer, whether through name-level characteristics (usage frequency) or location-level characteristics (network density, local clarity). Third, *portfolio sophistication* reveals communities maintain diverse naming menus while shifting aggregate positions, indicating thoughtful selection rather than mechanical herding.

7 Conceptual Framework

The empirical analysis documented three forces driving identity formation: church enforcement shaped local patterns, parents learned name meanings by observing neighbors, and communities conformed to nearby co-religionists' aggregate identity. This section develops a dynamic framework that organizes these findings and clarifies how the three mechanisms interact to generate the observed patterns. The model is deliberately stylized—it formalizes the reduced-form relationships estimated in previous sections rather than providing a fully structural representation.

7.1 Environment and Individual Behavior

A continuum of dynasties inhabit locations $\ell \in \{1, \dots, L\}$ connected by spatial networks (50km proximity weights $w_{\ell\ell'}$). Each location mandates a confession $g(\ell) \in \{C, P\}$ with churches enforcing doctrinal ideals $\theta^C = 0$, $\theta^P = 1$ at varying intensity $\kappa_\ell \in [0, 1]$. Names $n \in \mathcal{N}$ serve as observable identity signals with perceived confessional associations $s_{n,\ell,t} \in [0, 1]$. Time is discrete, with each period representing one generation.

Identity choice. Individual i in location ℓ of confession g chooses identity expression $a_{i,\ell,t} \in [0, 1]$ balancing three forces:

$$u_{i,\ell,t}(a) = -\kappa_\ell(a - \theta^g)^2 - \lambda(a - \bar{a}^{\text{local}}_{g,\ell,t-1})^2 - \gamma(a - \theta_{i,t})^2 \tag{17}$$

where κ_ℓ captures church enforcement pressure toward doctrine θ^g, λ captures conformity pressure toward nearby co-religionists $\bar{a}^{\text{local}}_{g,\ell,t-1} = \sum_{\ell'} w_{\ell\ell'} \bar{a}_{g,\ell',t-1}$, and γ captures attachment to own transmitted preference $\theta_{i,t}$. Optimization yields:

$$a^*_{i,\ell,t} = \frac{\kappa_\ell \theta^g + \lambda \bar{a}^{\text{local}}_{g,\ell,t-1} + \gamma \theta_{i,t}}{\kappa_\ell + \lambda + \gamma} \tag{18}$$

Identity is a weighted average of doctrine, peer identity, and inherited preference, with weights reflecting relative strength of each force.

Name choice. After choosing identity a_i^*, individual i selects a name to signal this position. With idiosyncratic taste shocks, aggregate choice probabilities follow a multinomial logit:

$$\pi_{n|g,\ell,t} = \frac{\exp[-\xi(\bar{a}^*_{g,\ell,t} - s_{n,\ell,t})^2]}{\sum_{n' \in \mathcal{N}} \exp[-\xi(\bar{a}^*_{g,\ell,t} - s_{n',\ell,t})^2]} \tag{19}$$

where $\xi > 0$ measures expressive utility. Names with signals matching group identity ($s_n \approx \bar{a}^*_g$) are chosen more frequently, creating sorting that strengthens confessional associations over time.

7.2 Dynamic Mechanisms

Three forces generate and propagate identity divergence:

1. Intergenerational transmission (Bisin and Verdier 2001). Children's preferences inherit from parental identity:

$$\theta_{i,t+1} = (1-\tau)\theta_{i,t} + \tau \cdot a_{i,t} + \eta_{i,t+1} \tag{20}$$

where $\tau \in [0,1]$ measures socialization effectiveness. This creates path dependence: temporary enforcement shocks have lasting effects through internalized preferences.

2. Social conformity. Equation (17) embeds conformity to spatially proximate co-religionists. Locations adjust identity toward network neighbors, diffusing patterns beyond directly-enforced areas.

3. Cultural learning (DeGroot 1974). Beliefs about name meanings update toward observed usage:

$$s_{n,\ell,t+1} = (1-\beta)s_{n,\ell,t} + \beta \sum_{\ell'} w_{\ell\ell'} \tilde{s}_{n,\ell',t} \tag{21}$$

where $\tilde{s}_{n,\ell,t}$ is the observed Protestant share among users of name n, and $\beta \in [0,1]$ captures learning speed.

Reinforcing feedback. Conformity and learning create amplification: stronger Protestant identity → more Protestant name usage → clearer Protestant signals → accelerated adoption → stronger identity. This feedback generates S-shaped diffusion—slow initially, accelerating as signals clarify, then saturating as polarization completes. Appendix E.2 derives this formally from the coupled dynamics of equations (19) and (21).

7.3 Aggregate Dynamics and Empirical Connection

Aggregating equation (18) over individuals and differencing yields reduced-form gap-closing dynamics (full derivation in Appendix E.1):

$$\Delta\bar{a}_{g,\ell,t} \approx \bar{\phi} \cdot \underbrace{\left[\sum_{\ell'} w_{\ell\ell'}\bar{a}_{g,\ell',t-1} - \bar{a}_{g,\ell,t-1}\right]}_{\text{Conformity gap}} + \bar{\psi} \cdot \underbrace{\kappa_\ell \left[\theta^g - \bar{a}_{g,\ell,t-1}\right]}_{\text{Institutional pull}} \tag{22}$$

where $\bar{\phi} = \lambda/(\bar{\kappa} + \lambda + \gamma)$ captures social force strength and $\bar{\psi} = 1/(\bar{\kappa} + \lambda + \gamma)$ captures responsiveness to enforcement, both evaluated at mean enforcement $\bar{\kappa}$.

This reduced form maps directly to the empirical specifications: the enforcement analysis (Section 5) estimates the effect of varying κ_ℓ on identity divergence, corresponding to the institutional pull term. The group-level conformity analysis (Section 6.2) estimates gap-closing dynamics corresponding to $\bar{\phi}$. The name-level learning analysis (Section 6.1) tests how convergence speed varies with signal quality, corresponding to heterogeneity in β. The framework clarifies that these forces operate jointly—enforcement seeds variation, conformity amplifies it spatially, and learning accelerates diffusion of clear signals through reinforcing feedback.

Appendix E.3 connects this framework to existing theories of cultural transmission (Bisin and Verdier 2001), learning (Fernández 2013), and identity formation (Shayo 2009), highlighting how the model integrates insights from each tradition. The key innovation is the dual-level structure: group-level conformity in aggregate identity combined with name-level learning about specific signals, operating simultaneously through spatial networks. Appendix E.4 sketches how doctrinal positions and enforcement capacity could arise endogenously from church competition, providing potential microfoundations for the institutional parameters I take as given.

8 Names as Identity Signals: Behavioral Validation

The previous sections established that church enforcement generates naming differentiation and that patterns diffuse through social transmission. This section examines whether naming patterns correlate with high-stakes decisions where confessional identity plausibly matters. I organize the analysis by level of aggregation: individual behaviors (family transmission, marriage, careers) and group-level outcomes (conflict exposure). While none of these relationships can be interpreted causally—unobserved factors may drive both naming and outcomes—their consistent pattern suggests naming choices reflect and signal meaningful confessional identity.

8.1 Individual-Level Behavior

Using parent-child pairs from baptism records, I find substantial intergenerational transmission: within city×decade contexts, parents' and children's name scores are strongly correlated, even across gender (fathers to daughters, mothers to sons) (Appendix Table A.6). Marriage records reveal positive assortative matching on confessional intensity—spouses' name scores are correlated within local marriage markets for both Catholics and Protestants (Appendix Table A.5). Finally, linking name scores to occupational data from *Deutsche Biographie*, individuals with stronger confessional signals were more likely to pursue church careers (clergy, theologians) and less likely to become notable in commerce (merchants, bankers) (Appendix Table A.7). While unobserved family characteristics (religiosity, social networks, wealth) contribute to all these patterns, their consistency suggests names signaled orientations relevant for life decisions, echoing findings by Bentzen and Andersen (2022).

8.2 Group-Level Behavior: Conflict Targeting

If naming patterns signal group identity to external observers, towns with clearer confessional signals might face differential treatment during religious warfare. Using conflict data from Cantoni and Weigand (2021) on the Thirty Years' War (1618-1648), I construct measures of Catholic and Protestant naming intensity and examine exposure to troops from Catholic League versus Protestant Union forces.

The patterns reveal striking asymmetries (Table 5). Catholic towns with stronger Catholic naming signals faced 21 percentage points higher probability of attack from Protestant Union forces, while facing marginally lower probability from Catholic League forces. Protestant towns exhibit the mirror pattern: stronger Protestant signals associate with 9.2 percentage points higher probability of Catholic League attacks and 10.2 percentage points lower probability of Protestant Union attacks. Crucially, stronger signals predict attacks from *opposing* rather than same-confession alliances—towns with clearer identity markers do not face uniformly more conflict, but systematically different patterns depending on attackers' confession. While unobserved factors (political allegiances, military resources) may contribute, this asymmetry is consistent with military actors interpreting naming patterns as information about local religious composition when making targeting decisions.

Across these domains—intergenerational transmission, marriage sorting, career paths, and conflict exposure—confessional name scores exhibit systematic correlations with consequential behaviors. While each correlation admits alternative explanations, their consistent pattern validates that naming differentiation represents meaningful identity formation rather than arbitrary fashion, with names functioning as legible markers of confessional affiliation in the social, economic, and political landscape of Reformation Germany.

9 Conclusion

Identity categories that define social boundaries are neither fixed nor natural Throughout history, societies have witnessed rapid realignments in which identities matter and what they mean: ethnic categories that were once salient fade while new ones emerge; class consciousness waxes and wanes; religious affiliations splinter and recombine; partisan divisions deepen or dissolve. Understanding how new identity markers emerge and spread—and when they come to structure deep social cleavages—remains central to explaining conflict, cooperation, and cultural change across contexts.

This paper examines identity formation during the Protestant Reformation using 4.9 million baptismal records to trace how religious boundaries crystallized through naming patterns. Three insights emerge. First, identity boundaries require both institutional investment and decentralized coordination—enforcement alone cannot generate lasting divisions without social transmission mechanisms that spread and internalize new meanings. Second, these mechanisms operate at multiple levels simultaneously: parents learn which markers signal which group (name-level) while communities coordinate on overall group distinctiveness (aggregate-level), and these processes reinforce each other through spatial networks. Third, the process exhibits path dependence with tipping points: initial differences amplify through feedback loops between institutional pressure, social learning, and conformity, eventually locking in divisions that persist for centuries. Future work quantifying the relative contribution of enforcement versus social transmission through structural estimation would enable counterfactual analysis of alternative institutional configurations and network structures.

These findings advance understanding of identity formation, state capacity, and religious change. They demonstrate that cultural boundaries emerge from interactions between top-down institutional investments and bottom-up social coordination, neither alone being sufficient. They reveal a dimension of early modern state-building—ideological capacity to shape beliefs and induce social discipline—that complements existing emphasis on fiscal and legal institutions. They unpack the mechanisms linking theological rupture to enduring behavioral differences, showing confessional identities were actively constructed rather than automatic consequences of doctrinal disputes.

More broadly, the analysis underscores historical contingency (Cantoni and Yuchtman 2025). The Reformation did not automatically produce two coherent confessional blocs. Transforming theological disagreements into a defining cleavage that structured German society for three centuries required deliberate institutional choices and sustained social coordination. Churches invested in enforcement infrastructure; parents learned and transmitted confessional meanings; communities coordinated on shared identities. Each decision reinforced the next, locking in divisions that could have evolved differently under alternative circumstances. Identity categories that appear natural often result from such path-dependent processes, shaped by strategic actors and amplified through social dynamics. Understanding these mechanisms illuminates not only historical transformations but also contemporary questions about polarization, cultural change, and the conditions under which social boundaries harden or dissolve.

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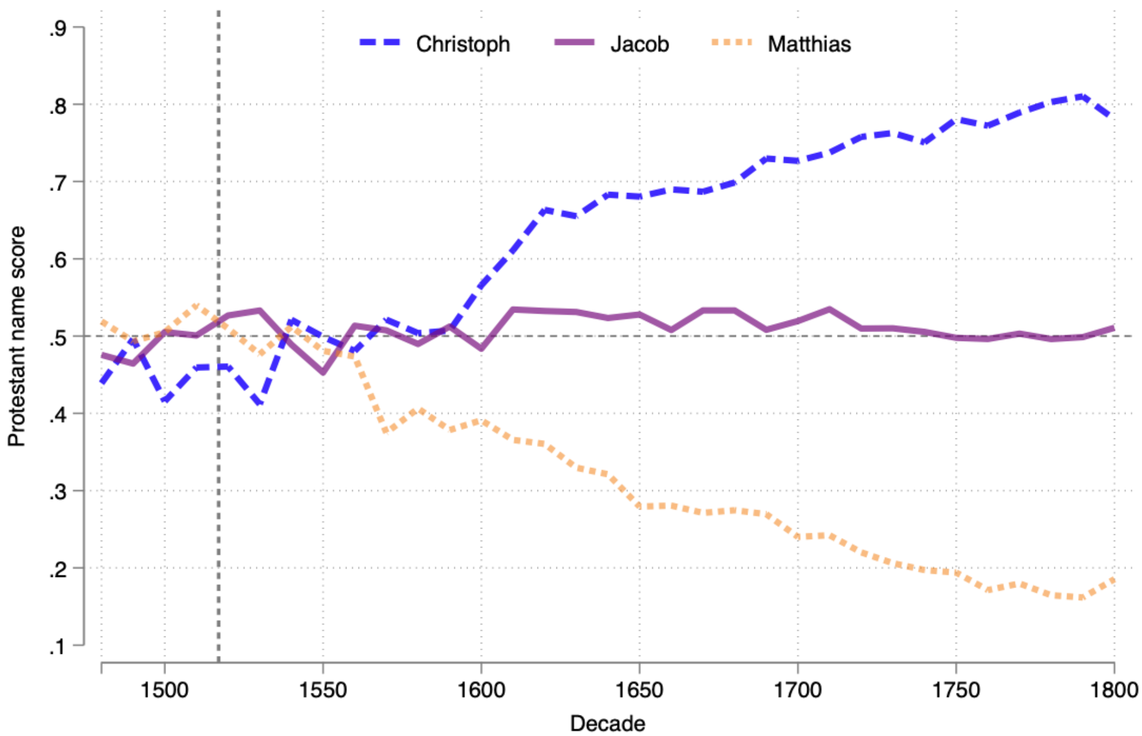
Figures

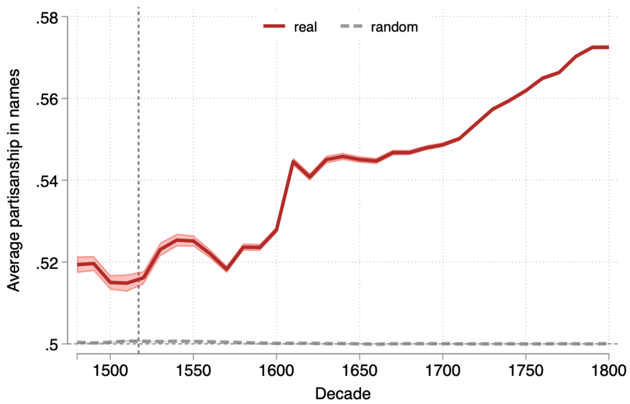
Figure 1: Trajectories of selected name.



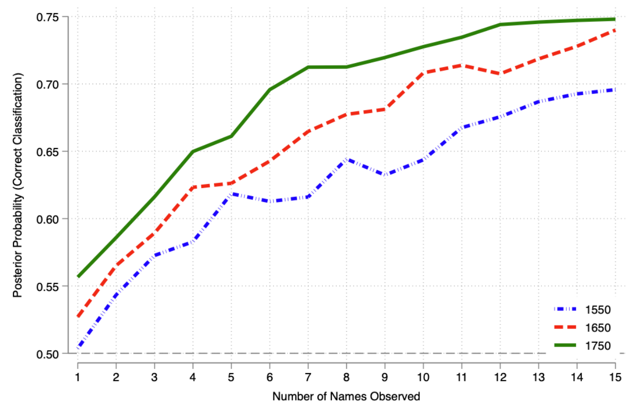
Note: This figure plots the Protestant name score computed as in Equation 1 on the y-axis. A value of 1 indicates a name exclusively used by Protestants, a value of 0 indicates a name exclusively used by Catholics, and a value of .5 indicates a name used in equal probability among both group. I plot the trajectory of the Protestant name score by decade for three selected names for the period between 1480 and 1800: Christoph (dashed blue line), Jacob (solid purple line), and Matthias (dotted orange line) which are among the top 10 names in 1500.

Figure 2: Informativeness of names over time.

(a) Average partisanship in naming over time

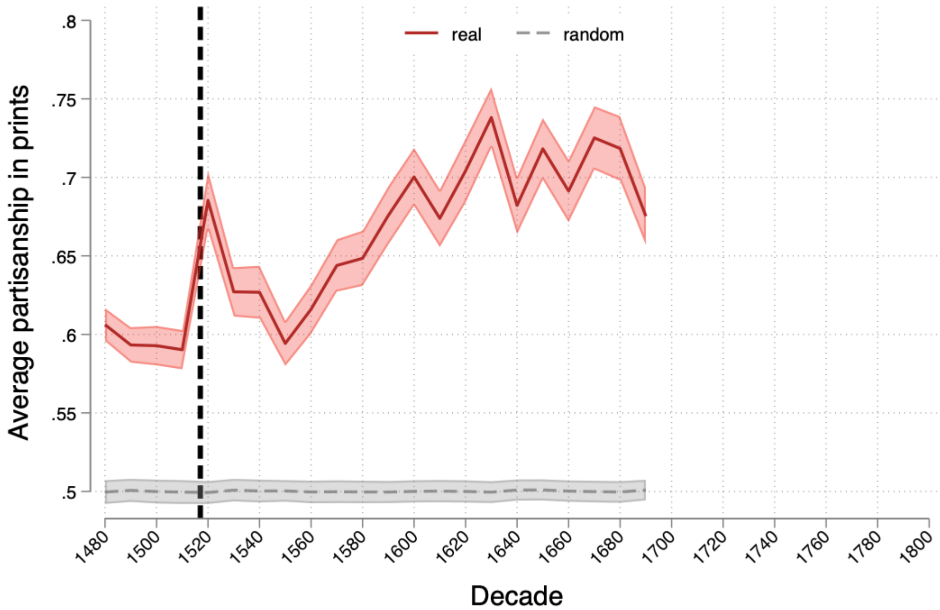


(b) Classification accuracy by number of names over time



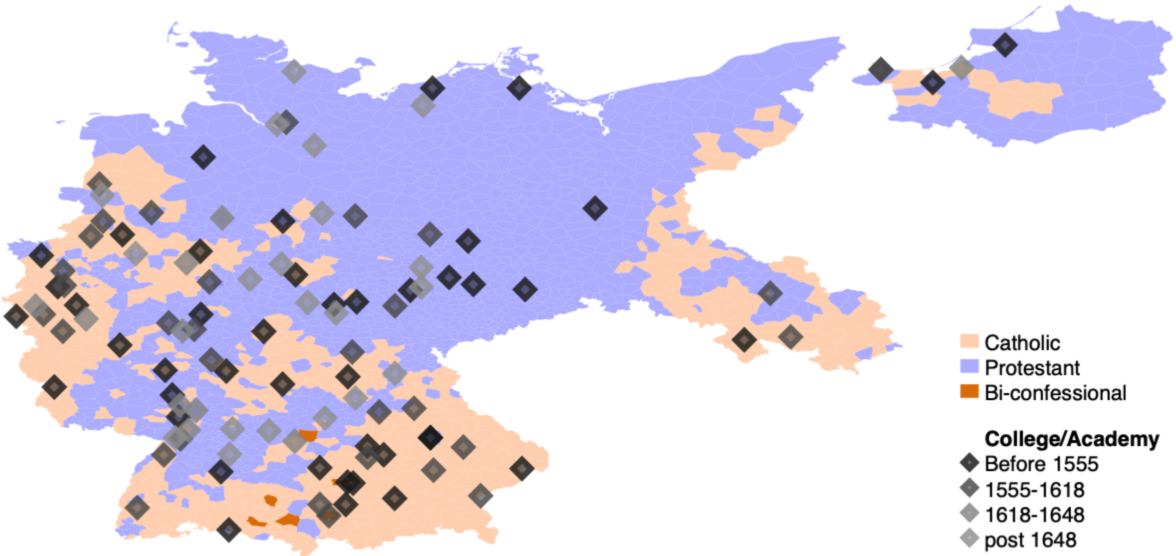
Note: Panel A plots average name-based partisanship, Average Partisanship$_t^{\text{name}}$, by decade from 1480 to 1800, as defined in Equation 2. The solid red line reports the posterior probability that a neutral observer correctly infers confession from a randomly drawn name, with 95% confidence intervals. The dashed gray line shows the average over 500 iterations with randomly reshuffled labels. Names are classified using the parish's (eventual) confessional status. Panel B plots informativeness of names by number of observed names. For each town i and decade t, I calculate the expected posterior an observer with a neutral prior would place on a towns true confessional status after hearing a given number of names. I perform this calculation by Monte Carlo simulation and plot the average across towns for each decade and number of names observed. See Appendix Section C for details on data construction and robustness.

Figure 3: Partisanship in shorttitles over time.



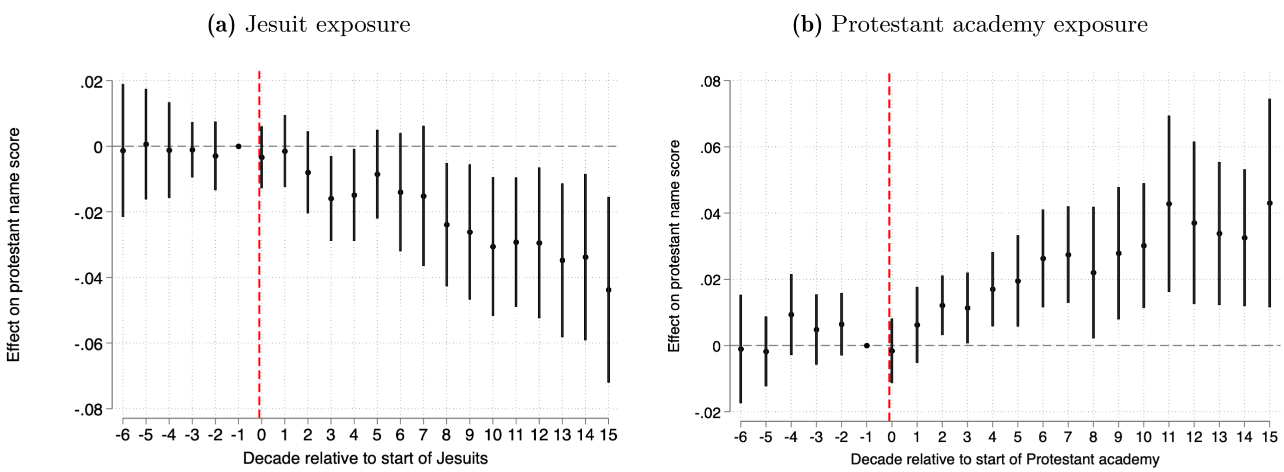
Note: This figure plots the average partisanship in print Average Partisanship$_t^{\text{text}}$ by decade, as defined in Equation 3. The red line shows estimates based on true Protestant and Catholic labels, with 95% confidence intervals from 500 bootstrap iterations. The gray line shows the distribution under randomly reshuffled labels (500 iterations), providing a reference for finite-sample informativeness. Partisanship scores are estimated using a penalized logistic classifier on short-title embeddings, with subject and linguistic cluster fixed effects where indicated. Panel A reports results for all short titles. Panel B restricts to religious subjects; Panel C to all other subjects. Short titles are from the *Universal Short Title Catalogue* (2025) and cover the period 1480–1700. Details on data construction are provided in Appendix Section D.

Figure 4: Jesuit and Protestant Academies.



Note: This map shows the location of Jesuit and Protestant academies. The base map plots towns and their catchment areas using data from Bogucka, Cantoni, and Weigand (2019). Catholic towns are shown in orange, bi-confessional towns in dark orange, and Protestant towns in blue. Jesuit and Protestant academies are marked with gray diamonds, shaded by the period of establishment.

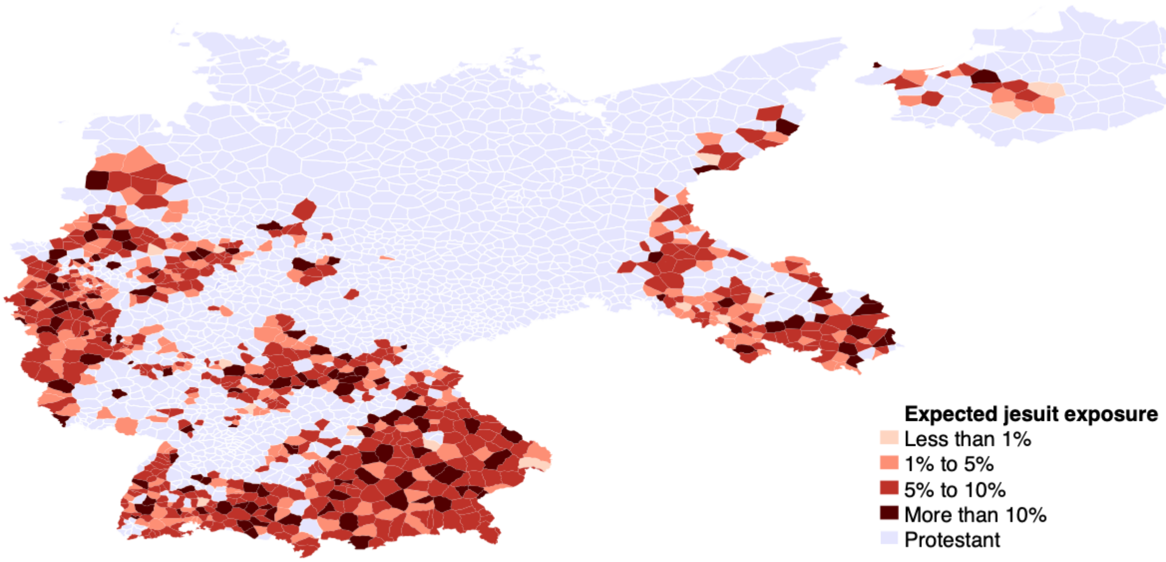
Figure 5: Confessional academies and religious identity (Event studies).



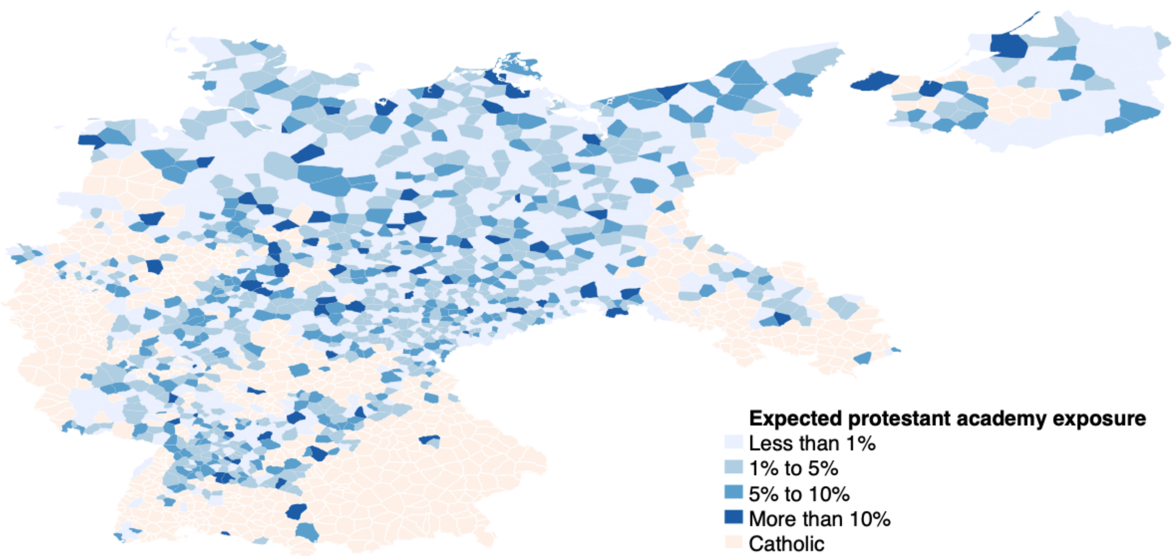
Note: This figure reports event-study estimates from Equation 5 with 95% confidence intervals. Observations are at the town–decade level; sample sizes are reported in Table 1. The sample spans 29 decades (1480–1760) for Catholic towns (ending in the decade before suppression of the Jesuit order in 1773 analyzed separately in 5.2.2) and 33 decades (1480–1800) for Protestant towns. The dependent variable is the average Protestant name score in town i and decade t. Panel A uses Catholic towns and defines treatment by the decade of Jesuit arrival; Panel B uses Protestant towns and defines treatment by the decade of Protestant academy establishment. Control units are never-treated or not-yet-treated towns. Standard errors are clustered at the town level.

Figure 6: Map of expected Jesuit exposure.

(a) Expected Jesuit exposure



(b) Expected Protestant exposure



Note: This map shows expected exposure to confessional academies. Details on the computation of expected exposure are provided in Appendix Section B.3. Panel A shows expected exposure to Jesuit colleges for Catholic towns in red, with darker shading indicating higher probabilities of exposure. Panel B shows expected exposure to Protestant academies for Protestant towns in blue, with darker shading indicating higher probabilities of expected exposure.

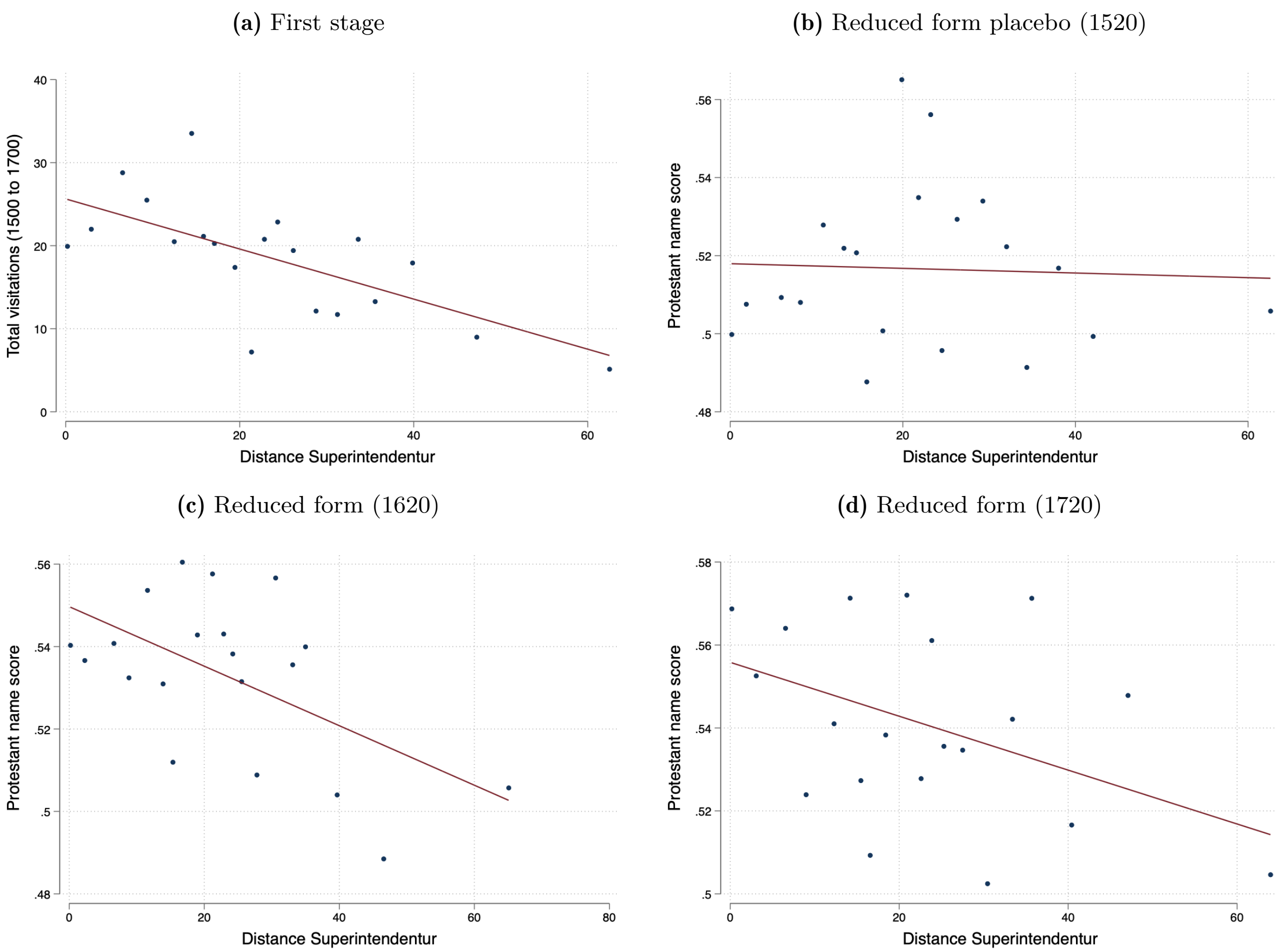
Figure 7: Binscatter plots of instrumental variable.

(a) First stage

(b) Reduced form placebo (1520)

(c) Reduced form (1620)

(d) Reduced form (1720)



Note: This figure shows binscatter plots illustrating the instrumental variables strategy for the sample of Protestant towns in the modern-day states of Baden-Württemberg and Hesse, where systematic visitation records exist for 1500–1700. The unit of observation is a town. Panel A plots the first stage, with the total number of visitations (1500–1700) on the y-axis and distance to the nearest *Superintendentur* on the x-axis. Panel B provides a placebo test, plotting the average Protestant name score in 1520 against distance to the nearest *Superintendentur*. Panels C and D show the reduced form, plotting the average Protestant name score in 1620 and 1720, respectively, against distance to the nearest *Superintendentur*.

Tables

Table 1: Confessional academies and religious identity.

	Protestant name score			
	(1)	(2)	(3)	(4)
	Catholic towns		Protestant towns	
Jesuit exposure	-0.022**	-0.025**		
	(0.009)	(0.010)		
Protestant academy exposure			0.017**	0.020***
			(0.007)	(0.008)
Observations	14122	14122	33319	33319
Number of Towns	699	699	1440	1440
Outcome mean	0.485	0.485	0.547	0.547
Town FEs	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓
Expected Exposure		✓		✓
Cluster	Town	Town	Town	Town

Notes: This table reports estimates from Equation 5. Observations are at the town–decade level; the number of towns is reported in the table. The sample spans 29 decades (1480–1760) for Catholic towns (ending in the decade before suppression of the Jesuit order in 1773 analyzed separately in 5.2.2) and 33 decades (1480–1800) for Protestant towns. The dependent variable is the average Protestant name score in town i and decade t. Columns 1–2 use Catholic towns and define treatment by the decade of Jesuit arrival; Columns 3–4 use Protestant towns and define treatment by the decade of Protestant academy establishment. The control group consists of never-treated and not-yet-treated towns. Columns 2 and 4 include expected exposure as an additional control variable; details on the construction of expected exposure are provided in Section B.3. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table 2: Confessional academies and religious activity.

	Notable clergy (ihs)				Religious prints (ihs)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Catholic towns		Protestant towns		Catholic towns		Protestant towns	
Jesuit exposure	0.149**	0.151**			0.565**	0.585**		
	(0.062)	(0.064)			(0.260)	(0.263)		
Protestant academy exposure			0.171**	0.136*			1.713***	1.545***
			(0.075)	(0.073)			(0.358)	(0.340)
Observations	27060	27060	51810	51810	17578	17578	33924	33924
Number of Towns	820	820	1570	1570	799	799	1542	1542
Outcome mean	0.012	0.012	0.031	0.031	0.066	0.066	0.100	0.100
Town FEs	✓	✓	✓	✓	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓	✓	✓	✓	✓
Expected Exposure		✓		✓		✓		✓
Cluster	Town	Town	Town	Town	Town	Town	Town	Town

Notes: This table reports estimates from Equation 5. Observations are at the town–decade level; the number of towns is reported in the table. olumns 1–4 use the inverse hyperbolic sine of the number of notable clergy dying in town i and decade t as the dependent variable (33 decades, 1480–1800). Columns 5–8 use the inverse hyperbolic sine of the number of religious prints produced in town i and decade t (23 decades, 1480–1700). Columns 1, 2, 5, and 6 use Catholic towns and define treatment by the decade of Jesuit arrival; Columns 3, 4, 7, and 8 use Protestant towns and define treatment by the decade of Protestant academy establishment. The control group consists of never-treated and not-yet-treated towns. Columns 2, 4, 6, and 8 additionally control for expected exposure; details on its construction are provided in Section B.3. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table 3: Name-level learning.

	Δ Name score			
	(1)	(2)	(3)	(4)
Gap (t-1)	0.3082***	0.2918***	0.2928***	0.3000***
	(0.0058)	(0.0068)	(0.0079)	(0.0075)
Gap (t-1) × High name usage		0.0467***		
		(0.0082)		
Gap (t-1) × High network density			0.0342***	
			(0.0110)	
Gap (t-1) × High signal clarity				0.0180**
				(0.0091)
Observations	122079	122079	122079	122079
Number of towns	767	767	767	767
Town FEs	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓
Name FEs	✓	✓	✓	✓
Cluster	Town	Town	Town	Town

Notes: This table examines whether individual names converge toward spatial network patterns. The dependent variable is the change in Protestant score for name n among confession g in location ℓ from decade $t-1$ to t. Gap$(t-1)$ measures the difference between the weighted average Protestant score for name n among nearby co-religionists (within 50km, uniform weights) and the location's own Protestant score for that name. Updating and gap are reverse coded for Catholics, so positive gaps indicate stronger learning pressure and updating indicates updating towards the own confessional extreme consistently. High name usage is an indicator for names above median of local usage frequency. High network density is an indicator for locations above median of same-confession neighbors within 50km. High signal clarity is an indicator for locations above median of local clarity, measured as the average distinctiveness of locally-used names ($\sum_n \pi_{n,g,\ell,t} \cdot |\text{ProtScore}_n - 0.5|$). All specifications include name fixed effects (absorbing aggregate trends in popularity), town fixed effects (absorbing time-invariant composition), and decade fixed effects. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table 4: Group-level conformity.

	Δ Avg. name Score			Δ Name Concentration
	(1)	(2)	(3)	(4)
Gap (t-1)	0.5965***	0.5249***	0.4044***	-0.2117***
	(0.0182)	(0.0252)	(0.0313)	(0.0331)
Gap (t-1) × High network density		0.1427***		
		(0.0359)		
Gap (t-1) × High signal clarity			0.2312***	
			(0.0406)	
Observations	9286	9286	9286	9286
Number of towns	723	723	723	723
Town FEs	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓
Cluster	Town	Town	Town	Town

Notes: This table examines whether aggregate confessional identity converges toward spatial network patterns. The dependent variable in columns (1)-(3) is the change in average Protestant score for confession g in location ℓ from decade $t-1$ to t, aggregating across all names chosen by that confession. Gap($t-1$) measures the difference between the weighted average aggregate identity among nearby same-confession communities (within 50km, uniform weights) and the location's own aggregate identity. Updating and gap are reverse coded for Catholics, so positive gaps indicate stronger learning pressure and updating indicates updating towards the own confessional extreme consistently. High network density is an indicator for locations above median of same-confession neighbors within 50km. High signal clarity is an indicator for locations above median of local clarity, measured as the average distinctiveness of locally-used names. Column (4) tests whether name concentration responds to gaps: the dependent variable is the change in the normalized Herfindahl index of name usage, and the independent variable is the absolute value of Gap($t-1$). The negative coefficient indicates that larger gaps induce portfolio diversification rather than concentration. All specifications include location×confession fixed effects and decade fixed effects. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table 5: Religious identity and conflict.

	Any	w/ Prot Union	w/ Cath League	Any	w/ Prot Union	w/ Cath League
	(1)	(2)	(3)	(4)	(5)	(6)
	Catholic towns			Protestant towns		
Catholic signal	0.165	0.210***	-0.048			
	(0.125)	(0.049)	(0.030)			
Protestant signal				-0.054	-0.102***	0.092**
				(0.058)	(0.032)	(0.039)
R^2	0.171	0.218	0.227	0.228	0.298	0.294
Observations	277	277	277	614	614	614
Outcome mean	0.547	0.259	0.090	0.431	0.148	0.158
Town controls	✓	✓	✓	✓	✓	✓
Region FEs	✓	✓	✓	✓	✓	✓
Cluster	Region	Region	Region	Region	Region	Region

Notes: This table reports estimates from regressing an indicator of conflict on an indicator of confessional conflict. Observations are at the town level. Columns 1–3 use Catholic towns; Columns 4–6 use Protestant towns. *Catholic Signal* equals one if a town's average Protestant name score is below 0.5 during the Thirty Years' War (1618–1648); *Protestant Signal* equals one if the score is above 0.5. Columns 1 and 4 use as dependent variable an indicator equal to one if the town was exposed to any troop presence during the war. Columns 2 and 5 use an indicator for exposure to troops of the Protestant Union; Columns 3 and 6 use an indicator for exposure to troops of the Catholic League. All regressions include the full set of pre-war town-level covariates and region fixed effects. Standard errors are clustered at the region level. Results should be interpreted as conditional associations rather than causal effects on war incidence. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Getting Religion: Identity Formation in the Protestant Reformation

Marcel Caesmann*

Appendix for Online Publication

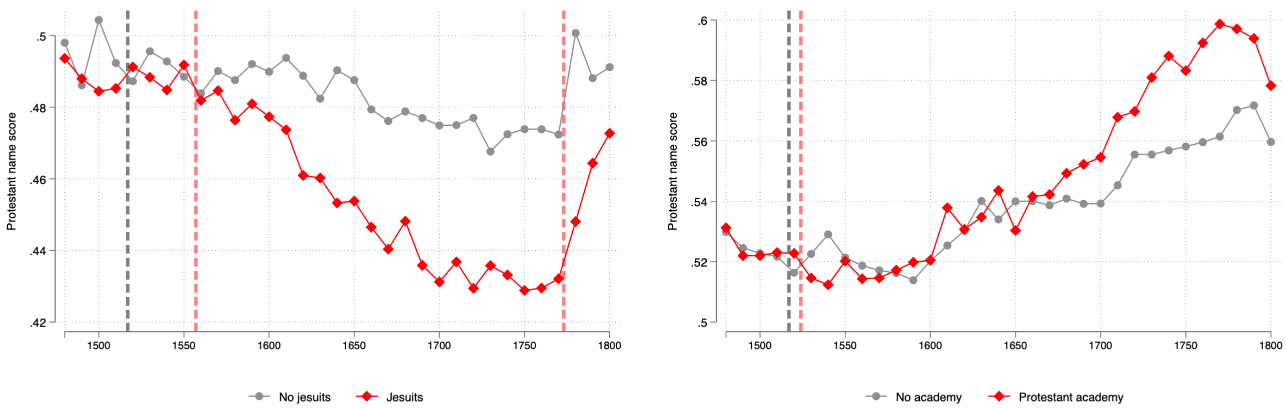
*University of Zurich, E-Mail: marcel.caesmann@econ.uzh.ch

Appendix

Table of Contents

A Tables and Figures

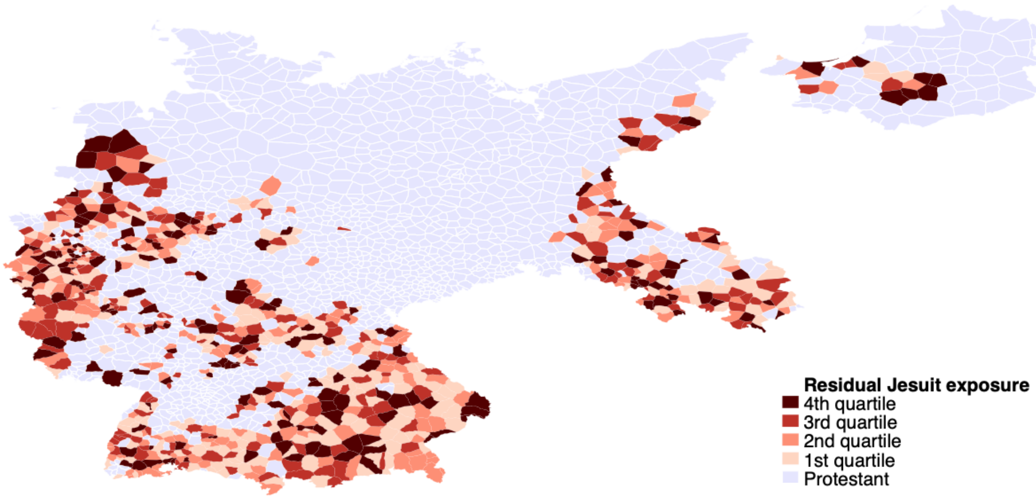
Figure A.1: Academy exposure and religious identity (raw data).



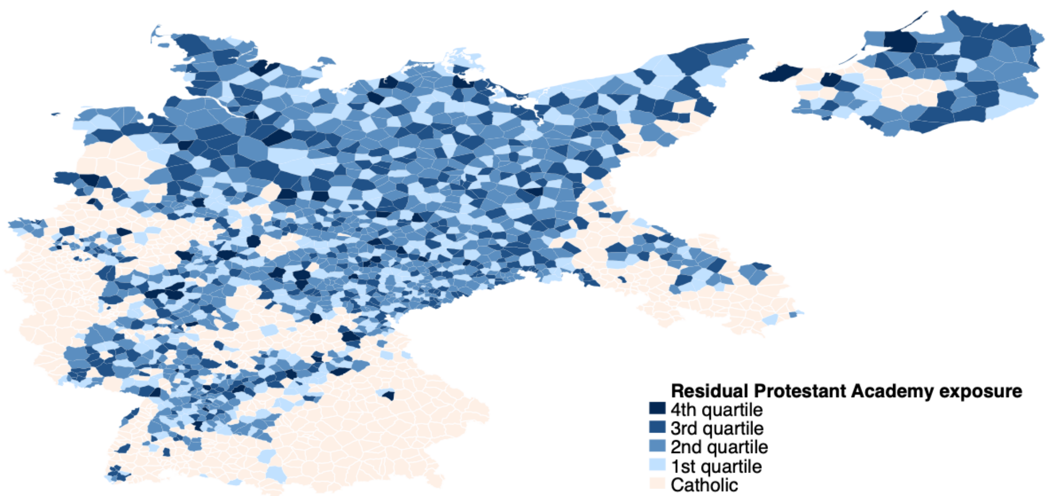
Note: This figure plots the average Protestant name score across towns by decade(1480–1800), as defined in Equation 1. Panel A uses Catholic towns and compares those that ever hosted a Jesuit college (red) to never-exposed Catholic towns (gray). Vertical markers: dashed gray = 1517 (start of the Reformation); solid red = 1556 (first Jesuit college in Ingolstadt); solid red = 1773 (suppression of the Jesuit order). Panel B uses Protestant towns and compares those that ever hosted a Protestant academy (red) to never-exposed Protestant towns (gray). Vertical markers: dashed gray = 1517; solid red = 1524 (first Protestant academy).

Figure A.2: Map of residual Jesuit exposure.

(a) Expected Jesuit exposure



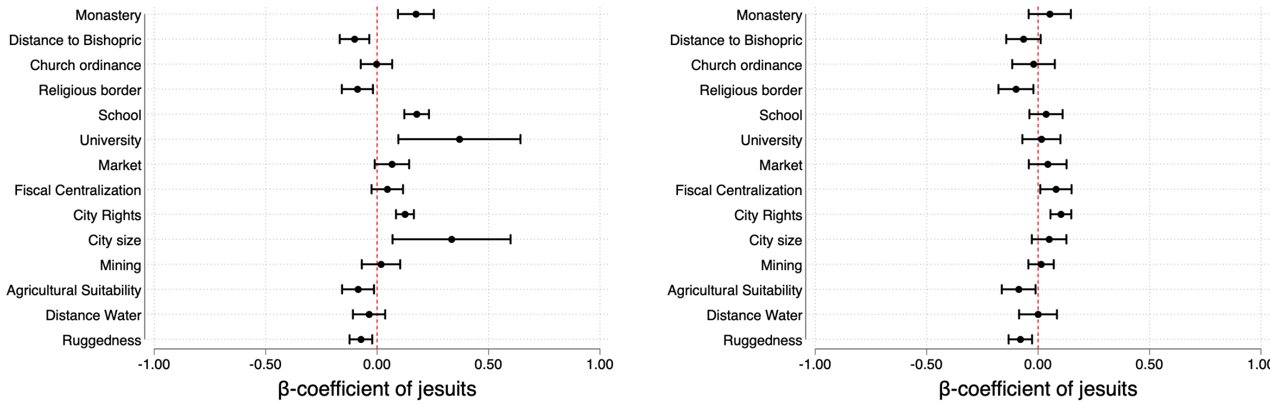
(b) Expected Protestant exposure



Note: This map shows residual exposure to confessional academies. Residual exposure is obtained by regressing actual confessional academy exposure on expected exposure and plotting the resulting residuals. Details on the computation of expected exposure are provided in Appendix Section B.3. Panel A depicts residual exposure to Jesuit colleges. Catholic and bi-confessional towns are shown in red, with darker shading indicating higher quartiles of residual exposure. Protestant towns are shown in light blue. Panel B depicts residual exposure to Protestant academies. Protestant towns are in shown in blue, with darker shading indicating higher quartile of residual exposure. Catholic and bi-confessional are shown in light orange.

Figure A.3: Balance Jesuits.

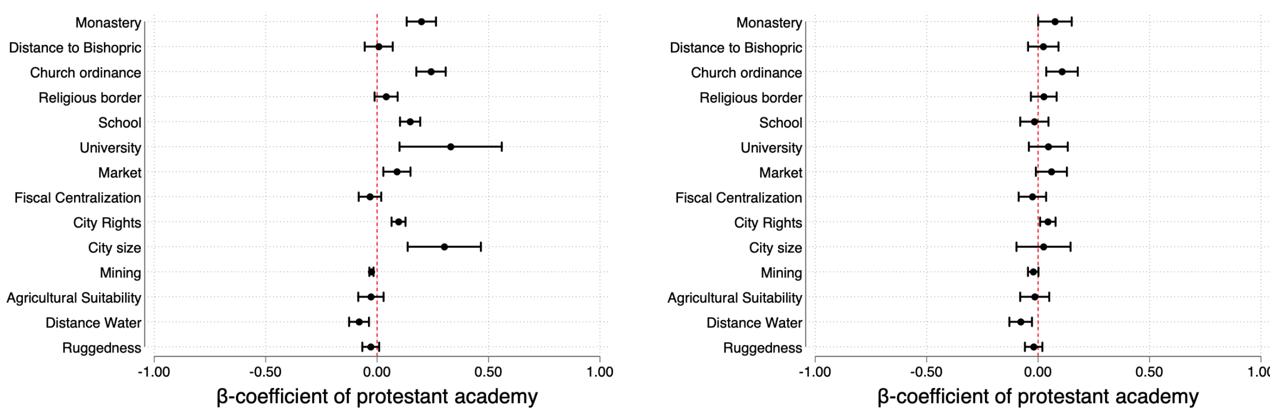
(a) Raw **(b)** Adjusted for expected exposure



Note: This figure shows estimated coefficients and 95% confidence intervals from regressions of Jesuit exposure on observable town characteristics, using Catholic towns. Panel A reports results using unadjusted exposure; Panel B uses exposure residualized for expected exposure. Town characteristics include distance to bishopric, town size, and indicators for market, monastery, school, university, city rights, and mining activity (all measured in 1500). Balance is also shown with respect to time-invariant characteristics: distance to water (river or sea), agricultural suitability, and average ruggedness of the town territory. Additional covariates include indicators for issuing a church ordinance, belonging to a fiscally centralized territory, and location on a religious border (all measured in 1650) show the correlation between exposure and later institutional developments.

Figure A.4: Balance Protestant academies.

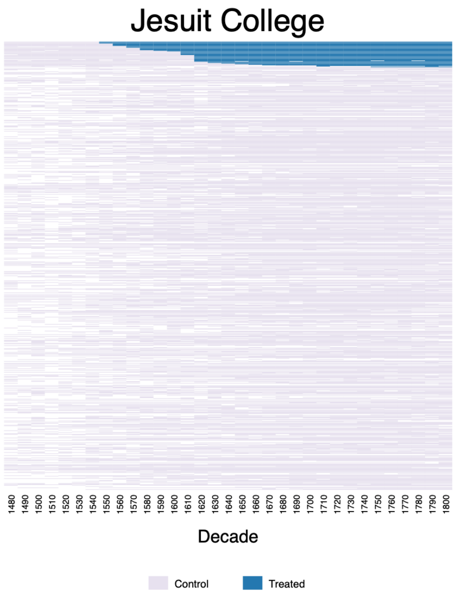
(a) Raw **(b)** Adjusted for expected exposure



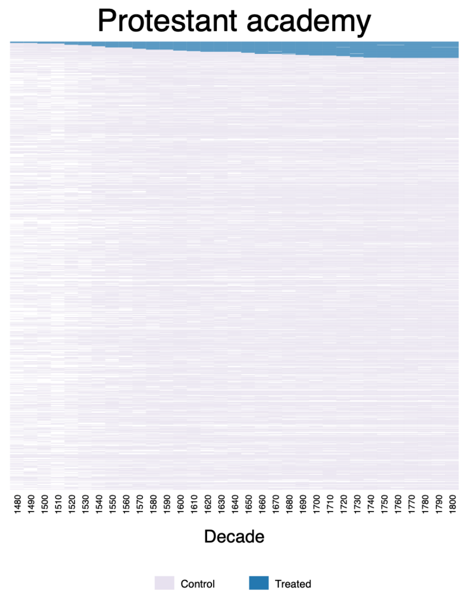
Note: This figure shows estimated coefficients and 95% confidence intervals from regressions of Protestant academy exposure on observable town characteristics, using Protestant towns. Panel A reports results using unadjusted exposure; Panel B uses exposure residualized for expected exposure. Town characteristics include distance to bishopric, town size, and indicators for market, monastery, school, university, city rights, and mining activity (all measured in 1500). Additional covariates include indicators for issuing a church ordinance, belonging to a fiscally centralized territory, and location on a religious border (all measured in 1650) show the correlation between exposure and later institutional developments.

Figure A.5: Treatment status and data coverage.

(a) Catholic towns



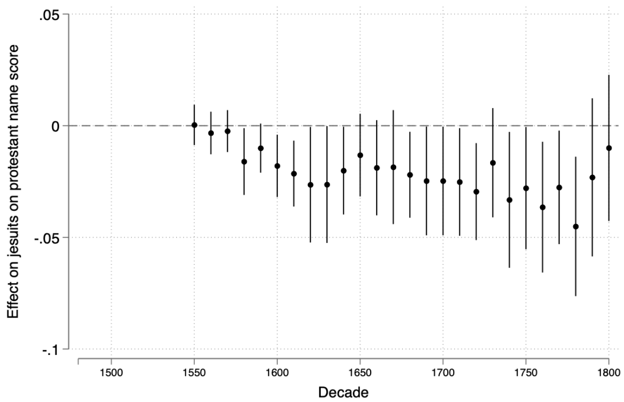
(b) Protestant towns



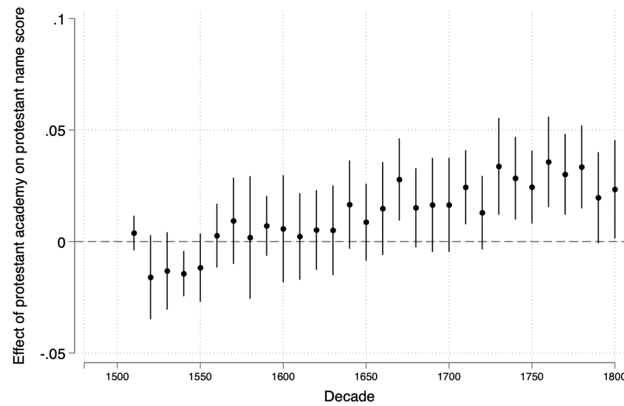
Note: This figure shows treatment status and data availability. Columns represent decades from 1480 to 1800; rows represent towns. Panel A displays Catholic towns, with those exposed to a Jesuit college (an absorbing state) highlighted in blue. Panel B displays Protestant towns, with those exposed to a Protestant academy (also an absorbing state) highlighted in blue. Gray rectangles denote the control group (never-treated or not-yet-treated town–decades). White rectangles indicate town–decade cells without baptism records in the data.

Figure A.6: Effect of academies by calendar date.

(a) Catholic towns



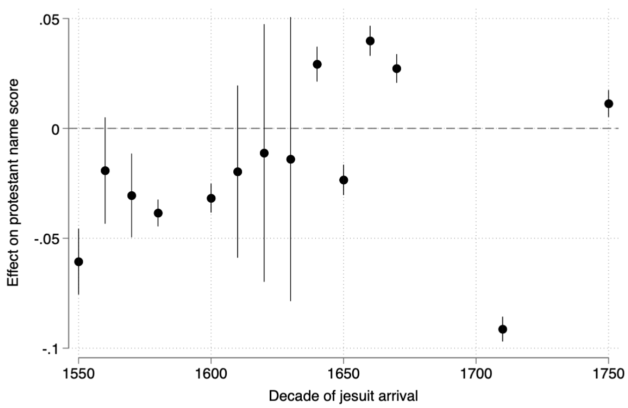
(b) Protestant towns



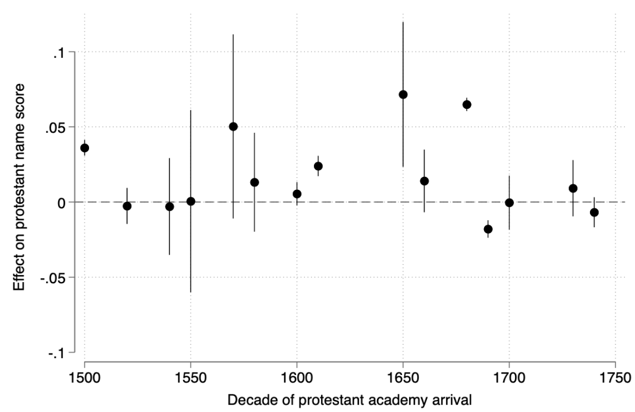
Note: This figure shows treatment effects to confessional academies from Equation 5, aggregated by calendar date following Callaway and Sant'Anna (2021). Estimates include 95% confidence intervals. Observations are at the town–decade level; the number of towns is reported in Table 1. Panel A shows results for Jesuit colleges in the sample of Catholic towns. The sample spans 33 decades (1480–1800). The first Jesuit college was established in Ingolstadt in 1556. Panel B shows results for Protestant academies in the sample of Protestant towns. The sample spans 33 decades (1480–1800). The first post-Reformation Protestant academy was established in 1524. Standard errors are clustered at the town level.

Figure A.7: Effect of academies by treatment cohort.

(a) Catholic towns



(b) Protestant towns



Note: This figure shows treatment effects to confessional academies from Equation 5, aggregated by treatment cohort following Callaway and Sant'Anna (2021). Estimates include 95% confidence intervals. Observations are at the town–decade level. The number of towns is reported in Table 1. Panel A shows results for Jesuit colleges in the sample of Catholic towns. The sample spans 33 decades (1480–1800). The first Jesuit college was established in Ingolstadt in 1556. Panel B shows results for Protestant academies in the sample of Protestant towns. The sample spans 33 decades (1480–1800). The first post-Reformation Protestant academy was established in 1524. Standard errors are clustered at the town level.

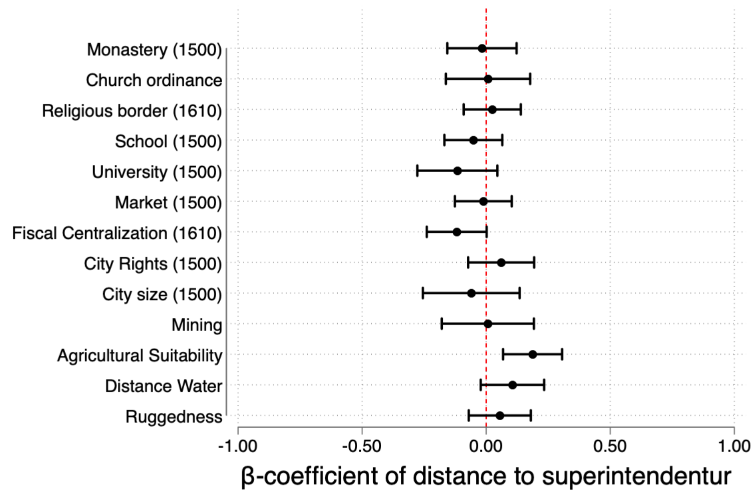
Figure A.8: Map of start of visitations.

Note: This map shows the year of first visitation for towns covered in Zeeden (1982). Catholic towns are shown in orange, with darker shading indicating earlier visitations. Protestant towns are shown in blue, with darker shading indicating earlier visitations.

Figure A.9: Map of distance to Superintendentur.

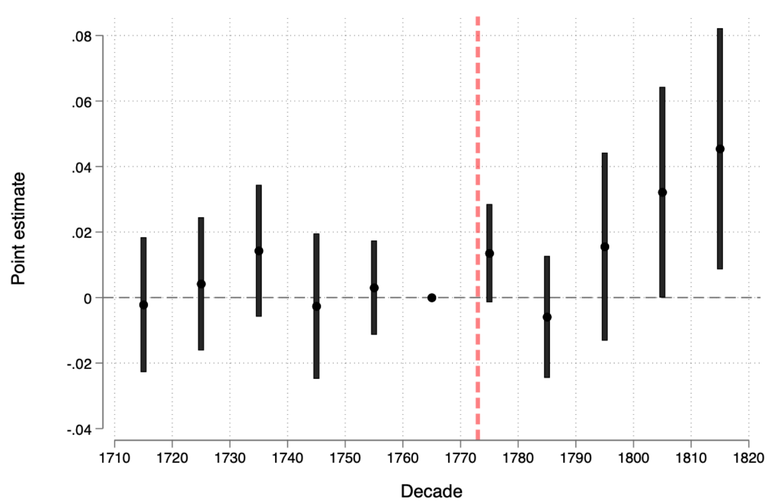
Note: This map shows the distance to the nearest *Superintendentur* for Protestant towns covered in Zeeden (1982). Seats of *Superintendenturen* are marked with black crosses. Darker shading indicates greater distance to the nearest *Superintendentur*.

Figure A.10: Balance of distance to Superintendentur.



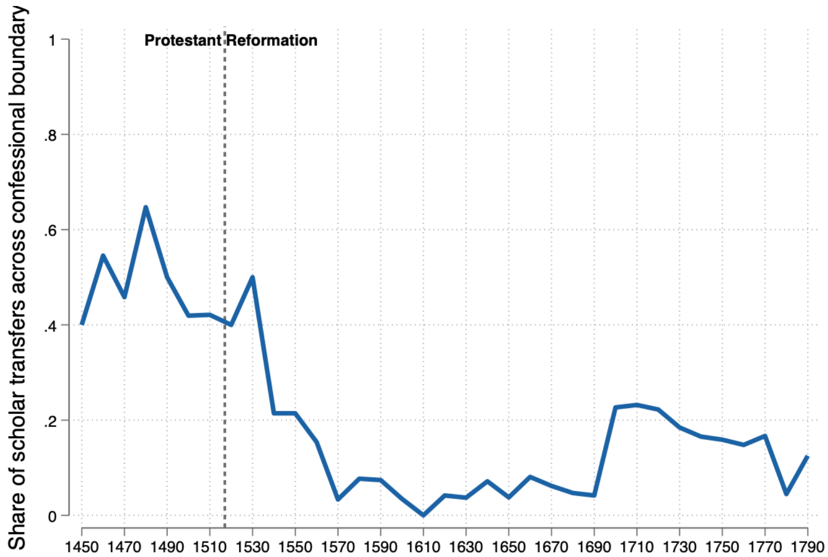
Note: This figure shows estimated coefficients and 95% confidence intervals from regressions of distance to the nearest *Superintendentur* on observable town characteristics, using Protestant towns covered in Zeeden (1982). Town characteristics include town size and indicators for market, monastery, school, university, city rights, and mining activity (measured in 1500). Balance is also reported for time-invariant characteristics: distance to water (river or sea), agricultural suitability, and average ruggedness of the town territory. Additional covariates include indicators for issuing a church ordinance, belonging to a fiscally centralized territory, and location on a religious border (measured in 1610) showing the correlation of treatment with later institutional developments.

Figure A.11: Effect of ban of Jesuits (1773).



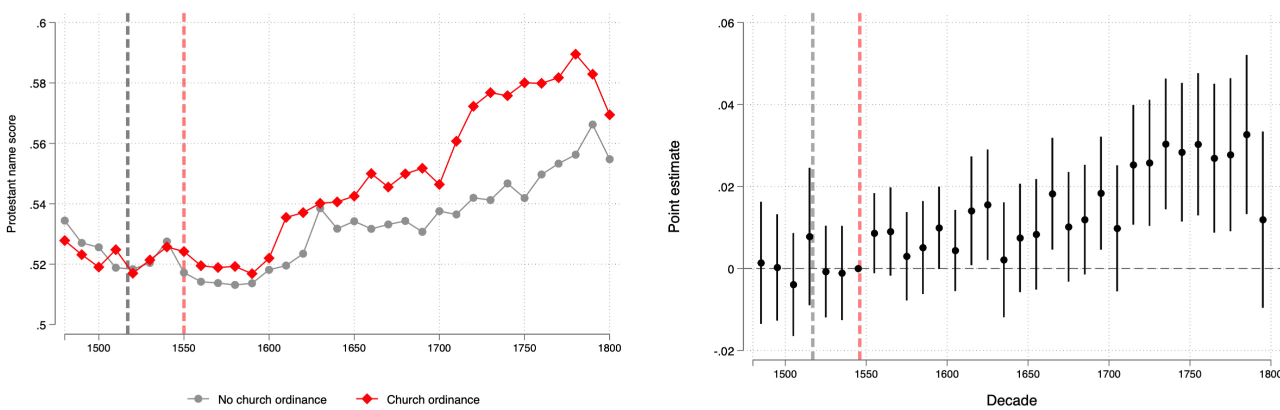
Note: This figure shows estimates from Equation 6 with 95% confidence intervals. Observations are at the town–decade level; the sample includes only Catholic towns. The dependent variable is the average Protestant name score in town c and decade t. Coefficients are plotted relative to the decade before the 1773 suppression of the Jesuit order. Standard errors are clustered at the town level.

Figure A.12: Scholar movement across confessional boundaries.



Note: This figure plots the share of scholar transfers across confessional boundaries—specifically from Catholic to (eventual) Protestant universities—by decade of transfer. Data are from De La Croix (2021). The vertical gray line marks 1517, the start of the Protestant Reformation.

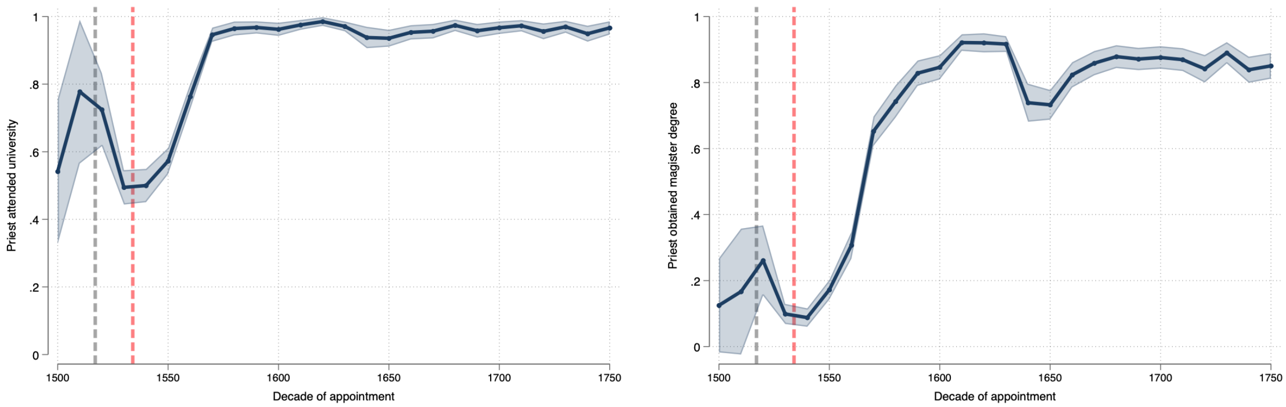
Figure A.13: Church ordinances and Protestant name score.



Note: Panel A plots the average Protestant name score across towns by decade (1480–1700) for the sample of Protestant towns. Towns that issued a church ordinance are shown in red; towns without an ordinance are shown in gray. The vertical dashed gray line marks 1517, the start of the Protestant Reformation. Panel B shows estimates from an adapted version of Equation 6 with 95% confidence intervals. Treatment is defined as whether a town ever passed a church ordinance. Observations are at the town–decade level; the sample includes only Protestant towns. The dependent variable is the average Protestant name score in town c and decade t. Coefficients are plotted relative to the 1540 decade (the omitted category). Standard errors are clustered at the town level.

Figure A.14: Priest education over time - Württembergisches Pfarrerbuch.

(a) Attended university **(b)** Obtained Magister degree



Note: This figure plots the share of priests in the Duchy of Württemberg who attended university (Panel A) or obtained a Magister degree (Panel B), by decade of appointment to a parish. Data are from the *Pfarrerbuch des Herzogtums Württemberg* (2025). The vertical gray line marks 1517, the start of the Reformation; the vertical red line marks 1534, when Protestantism was adopted in the Duchy of Württemberg.

Table A.1: Visitations and religious identity.

	Protestant name score			
	(1)	(2)	(3)	(4)
	Catholic towns		Protestant towns	
Post visitation	-0.033**	-0.029***	0.016*	0.021**
	(0.015)	(0.010)	(0.009)	(0.008)
Observations	763	763	1490	1490
Number of Towns	35	35	69	69
Outcome mean	0.493	0.493	0.522	0.522
Town FEs	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓
Town controls		✓		✓
Cluster	Town	Town	Town	Town

Notes: This table reports estimates from Equation 5. Observations are at the town–decade level; the number of towns is reported in the table. The sample covers 23 decades (1480–1700) and includes towns in the modern-day states of Baden-Württemberg and Hesse, for which systematic visitation records exist for 1500–1700. The dependent variable is the average Protestant name score in town i and decade t. Treatment is defined by the decade of the first visitation of town i. Columns 1–2 use Catholic towns; Columns 3–4 use Protestant towns. The control group consists of never-treated and not-yet-treated towns. Columns 2 and 4 include town-level controls measured at baseline (1500): town size, monastery, school, and university presence. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table A.2: Visitations and religious identity (instrumental variable).

	Protestant name score			
	(1)	(2)	(3)	(4)
Cumulated visitations	0.0042**	0.0043**	0.0021*	0.0032*
	(0.0019)	(0.0021)	(0.0011)	(0.0018)
Observations	215	215	253	253
Outcome mean	0.533	0.533	0.541	0.541
Region FEs		✓		✓
Sample	1620	1620	1720	1720
Cluster	Town	Town	Town	Town
First stage F-stat	15.990	14.513	13.906	7.061

Notes: This table reports two-stage least squares estimates using distance to the nearest *Superintendentur* as an instrument for the cumulative number of church visitations received by each town up to a given decade. The sample consists of Protestant towns in the modern-day states of Baden-Württemberg and Hesse for which systematic visitation records exist. The dependent variable is the average Protestant name score in town i for the given decade. Columns 1–2 use the cross-section in 1620 instrumenting for the cumulated number of visitations by 1620; Columns 3–4 use the cross-section in 1720 instrumenting for the cumulated number of visitations by 1720. Columns 2 and 4 additionally include region fixed effects, exploiting only within-region variation in distance to a *Superintendentur*. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table A.3: Visitations and priest turnover.

	Priest turnover per year			
	(1)	(2)	(3)	(4)
Visitation	0.050***	0.046***	0.048***	0.016*
	(0.014)	(0.013)	(0.015)	(0.008)
Observations	2646	2100	2058	2646
Number of Towns	126	100	98	126
Outcome mean	0.191	0.211	0.214	0.191
Town FEs				✓
Decade FEs	✓	✓		✓
Superintendentur FEs		✓		
Superintendentur × Decade FEs			✓	
Cluster	Town	Town	Town	Town

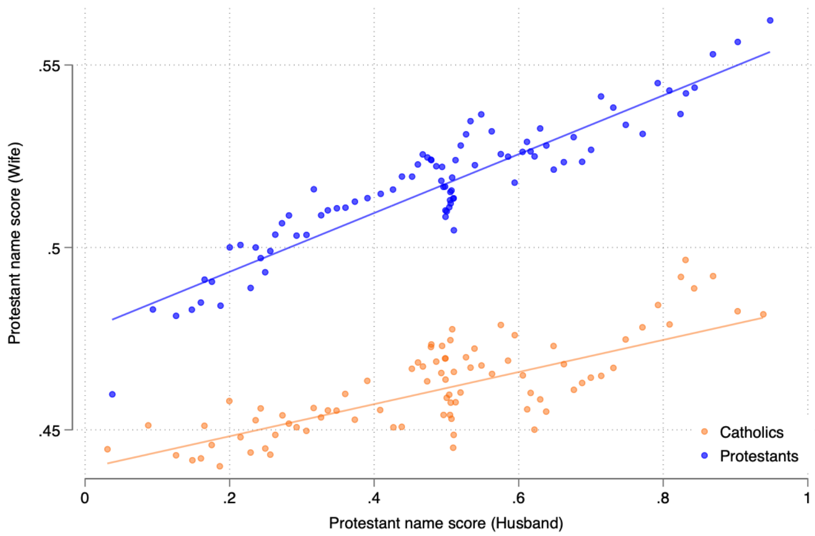
Notes: This table reports estimates from a specification adapted from Equation 4. Observations are at the town–decade level. The sample includes 126 Protestant towns covered in the *Pfarrerbuch des Herzogtums Württemberg* (2025) and Zeeden (1982), spanning 20 decades (1500–1690) for which visitation data are available. The dependent variable is the annual number of priest turnovers in town c and decade t. The treatment indicator equals one if town c was visited in decade t. Column 1 includes only decade fixed effects; Column 2 adds Superintendentur fixed effects; Column 3 includes Superintendentur-by-decade fixed effects; Column 4 includes town and decade fixed effects. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table A.4: Academy exposure and human capital spillovers.

	Notable non-clergy (ihs)				Non-religious prints (ihs)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Catholic towns		Protestant towns		Catholic towns		Protestant towns	
Jesuit exposure	0.195*	0.191*			0.476***	0.465***		
	(0.109)	(0.110)			(0.149)	(0.146)		
Protestant academy exposure			0.535***	0.413***			2.526***	2.233***
			(0.103)	(0.108)			(0.448)	(0.427)
Observations	27060	27060	51810	51810	17578	17578	34540	34540
Number of Towns	820	820	1570	1570	799	799	1570	1570
Outcome mean	0.105	0.105	0.207	0.207	0.072	0.072	0.182	0.182
Town FEs	✓	✓	✓	✓	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓	✓	✓	✓	✓
Expected Exposure		✓		✓		✓		✓
Cluster	Town	Town	Town	Town	Town	Town	Town	Town

Notes: This table reports estimates from Equation 5. Observations are at the town–decade level; the number of towns is reported in the table. Columns 1–4 use the inverse hyperbolic sine of the number of notable non-clergy individuals born in town i and decade t as the dependent variable (33 decades, 1480–1800). Columns 5–8 use the inverse hyperbolic sine of the number of non-religious prints produced in town i and decade t (23 decades, 1480–1700). Columns 1, 2, 5, and 6 use Catholic towns and define treatment by the decade of Jesuit arrival; Columns 3, 4, 7, and 8 use Protestant towns and define treatment by the decade of Protestant academy establishment. The control group consists of never-treated and not-yet-treated towns. Columns 2, 4, 6, and 8 additionally control for expected exposure; details on its construction are provided in Section B.3. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Figure A.15: Assortative matching.



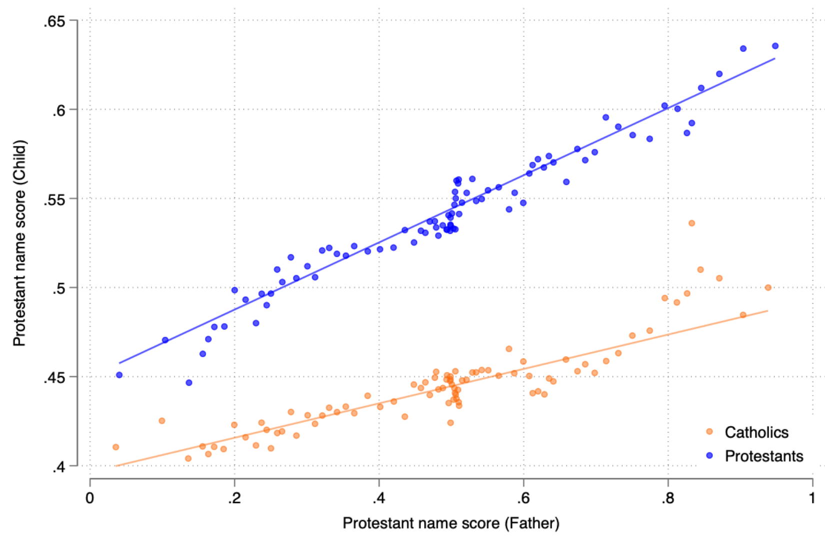
Note: The figure shows a binscatter plot split by Catholics (orange) and Protestants (blue). The Protestant name score of the wife is on the y-axis. The Protestant name score of the husband is on the x-axis.

Table A.5: Assortative matching.

	Protestant name score (wife)			
	(1)	(2)	(3)	(4)
	Catholic towns		Protestant towns	
Protestant name score (husband)	0.013***	0.007***	0.021***	0.015***
	(0.002)	(0.001)	(0.002)	(0.002)
R^2	0.054	0.073	0.069	0.094
Observations	886418	884494	883133	879156
Number of Towns	794	691	1357	1247
Town FEs	✓		✓	
Decade FEs	✓		✓	
Town × Decade FEs		✓		✓
Cluster	Town	Town	Town	Town

Notes: This table reports estimates from regressing the Protestant name score of the wife on the Protestant name score of the husband. Fixed effects used are indicated in each column. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Figure A.16: Inter-generational transmission.



Note: The figure shows a binscatter plot split by Catholics (orange) and Protestants (blue). The Protestant name score of the child is on the y-axis. The Protestant name score of the father is on the x-axis.

Table A.6: Inter-generational transmission.

	Boys		Girls		Boys		Girls	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Catholic towns				Protestant towns			
Protestant name score (mother)	0.022***	0.011***	0.036***	0.024***	0.028***	0.021***	0.093***	0.079***
	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
Protestant name score (father)	0.059***	0.048***	0.015***	0.008***	0.128***	0.117***	0.026***	0.020***
	(0.004)	(0.004)	(0.002)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)
R^2	0.107	0.129	0.088	0.113	0.239	0.271	0.148	0.187
Observations	858901	857388	798133	796731	843064	839861	832973	829991
Number of Towns	741	667	731	657	1302	1217	1273	1196
Town FEs	✓		✓		✓		✓	
Decade FEs	✓		✓		✓		✓	
Town × Decade FEs		✓		✓		✓		✓
Cluster	Town	Town	Town	Town	Town	Town	Town	Town

Notes: This table reports estimates from regressing the Protestant name score of children on the Protestant name score of their father and mother. Fixed effects used are indicated in each column. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table A.7: Career choice.

	Church	Government	Military	Business	Church	Government	Military	Business
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Catholics				Protestants			
Protestant name score	-0.087***	-0.010	0.011*	0.033***	0.020*	0.001	0.004	-0.012**
	(0.017)	(0.013)	(0.006)	(0.010)	(0.012)	(0.007)	(0.004)	(0.006)
Religious name	0.005	-0.014***	-0.009***	0.008*	0.047***	-0.021***	-0.013***	0.003
	(0.007)	(0.005)	(0.002)	(0.004)	(0.004)	(0.002)	(0.001)	(0.002)
R^2	0.106	0.029	0.014	0.018	0.093	0.008	0.010	0.012
Observations	13591	13591	13591	13591	52905	52905	52905	52905
Region FEs	✓	✓	✓	✓	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓	✓	✓	✓	✓
Mean dependent variable	0.171	0.074	0.017	0.047	0.181	0.060	0.017	0.033
Cluster	Region	Region	Region	Region	Region	Region	Region	Region

Notes: This table reports estimates from regressing an indicator of whether the individual chose a career in church (Columns 1 and 5), government (Columns 2 and 6), military (Columns 3 and 7), or business (Columns 4 and 8) on the Protestant name score of the individual covered in *Deutsche Biographie*. All specifications include region and decade fixed effects. Standard errors are clustered at the region level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Figure A.17: Example academies.

(a) Jesuit college in Ingolstadt **(b)** Fürstenschule in Grimma

Note: The figure provides illustrations of academies used to proxy for local church enforcement. Panel A depicts the Jesuit college in Ingolstadt. Panel B depicts the *Fürstenschule* in Grimma, one of the Protestant academies in the sample.

B Robustness

B.1 Robustness of Main Empirical Results

In Section 5, I present evidence linking local academy exposure to the formation of religious identity. This section demonstrates that the results are robust to alternative specifications, sample restrictions, and inference procedures through five complementary exercises.

Controlling for confounds. First, I address concerns about omitted variable bias by flexibly controlling for town characteristics. Appendix Table B.1 includes interactions of baseline covariates (town size, monastery, school, and university presence in 1500) with decade fixed effects, allowing each town characteristic to have time-varying effects.[14] The most demanding specification additionally includes territory fixed effects, absorbing all time-invariant differences across political jurisdictions. The results remain stable across all specifications, suggesting that unobserved time-varying heterogeneity at the territory level does not drive the findings.

Addressing selection into treatment. Second, I construct more comparable control groups to mitigate selection bias. Appendix Table B.2 uses probit nearest-neighbor matching (3 neighbors) on baseline observables to create matched samples. Appendix Figures B.1 and B.2 confirm that covariate balance improves substantially after matching, with standardized mean differences falling below conventional thresholds. Despite this restriction to observably similar towns, main effects remain virtually unchanged. As an additional check, Appendix Table B.3 implements the doubly robust inverse-probability weighting (DR-IPW) estimator of Sant'Anna and Zhao (2020), which combines outcome regression with inverse-probability weighting. Results remain consistent, confirming that selection on observables does not confound the estimates.

Alternative samples and measurement. Third, I test robustness to sample composition and alternative constructions of the religious identity measure. Appendix Table B.4 restricts analysis to a balanced panel of towns with at least ten baptisms in every decade, ensuring that compositional changes in the sample do not drive temporal patterns. Appendix Tables B.5 and B.6 employ alternative aggregations: computing name scores using a 20-year rolling window to smooth short-run fluctuations, and averaging scores across linguistic clusters (High, Middle, and Low German) to account for regional naming traditions. Appendix Table B.7 caps the number of baptisms per town-decade cell at 100 to prevent large urban centers from dominating the measure.[15] Appendix Table B.8 restricts analysis to prime (first-listed) baptismal names only, eliminating potential noise from secondary names. Across all alternative samples and measurement approaches, estimated effects remain statistically and economically significant.

Accounting for spillovers. Fourth, I examine whether spatial spillovers into nearby untreated towns could bias estimates. If academy presence generates diffuse effects on neighboring towns, including proximate control towns would attenuate estimated treatment effects. Appendix Tables B.9 and B.10 re-estimate the baseline specification while excluding control towns within 20 km and 30 km of treated towns, respectively. Results remain robust, suggesting that spillover bias is minimal. Appendix Table

[14] Each covariate is interacted with decade fixed effects, permitting differential trends by baseline town type.

[15] For each capped draw, I recompute the name score, repeat 100 times, and average the resulting estimates to account for sampling variability.

B.11 further excludes all university towns—the most likely locations for both Jesuit colleges and Protestant academies, and therefore the most selected sample. Dropping these towns does not materially change the findings, confirming that results extend beyond the most educationally developed locations.

Robustness of inference. Finally, I assess sensitivity to alternative inference procedures. Appendix Table B.12 reports results using four alternative clustering levels (town, region, territory, and Imperial Circle) as well as spatially corrected Conley standard errors at three distance cutoffs. Statistical significance is maintained across all specifications. Appendix Figure B.3 implements a leave-one-out analysis, sequentially dropping each region from the sample and re-estimating treatment effects. Estimates remain tightly centered on the baseline, demonstrating that no single region drives the results.

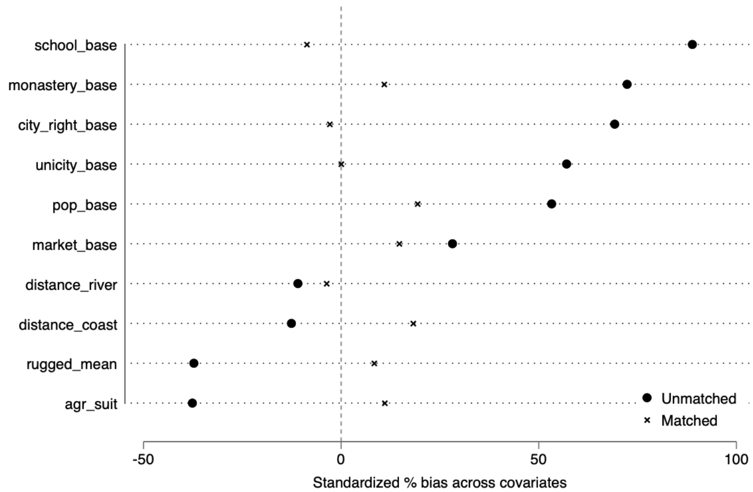
The combined evidence from these five exercises indicates that the estimated effects of academy exposure on religious identity formation are not sensitive to model specification, sample composition, measurement approach, or inference procedure.

Table B.1: Academy exposure and religious identity (TWFE, Controls, and Territory Fixed Effects).

	Protestant name score					
	(1)	(2)	(3)	(4)	(5)	(6)
	Catholic towns			Protestant towns		
Jesuit exposure	-0.022***	-0.023***	-0.023***			
	(0.006)	(0.007)	(0.007)			
Protestant academy exposure				0.009**	0.012**	0.010**
				(0.005)	(0.005)	(0.004)
R^2	0.302	0.311	0.321	0.301	0.300	0.313
Observations	14122	14048	14093	32071	30796	30856
Number of Towns	699	696	699	1436	1431	1433
Outcome mean	0.485	0.485	0.485	0.546	0.546	0.546
Town FEs	✓	✓	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓	✓	✓
Controls		✓			✓	
Territory FEs			✓			✓
Cluster	Town	Town	Town	Town	Town	Town

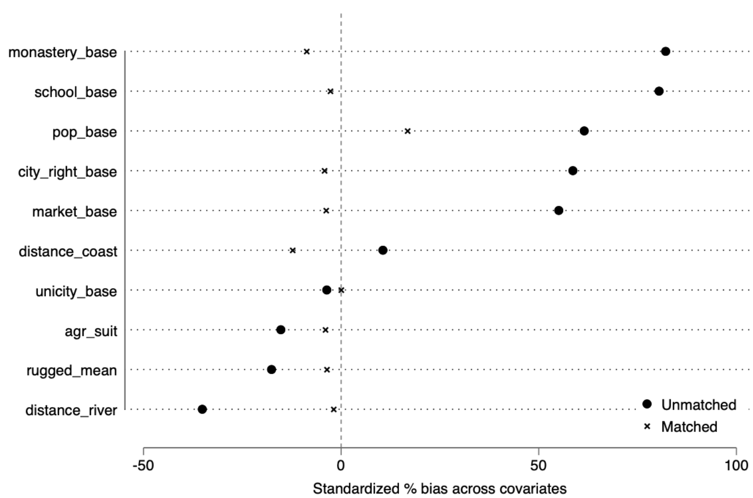
Notes: This table reports estimates from estimating Equation 4. Observations are at the town–decade level; the number of towns is reported in the table. The sample spans 29 decades (1480–1760) for Catholic towns (ending in the decade before suppression of the Jesuit order in 1773 analyzed separately in 5.2.2) and 33 decades (1480–1800) for Protestant towns. The dependent variable is the average Protestant name score in town i and decade t. Columns 1–3 use Catholic towns, with treatment defined as an indicator equal to one if Jesuits were active in town i in decade t. Columns 4–6 use Protestant towns, with treatment defined as an indicator equal to one if a Protestant academy was active in town i in decade t. All specifications include town and decade fixed effects. Columns 2 and 5 additionally include the full set of town characteristics interacted with decade fixed effects. Columns 3 and 6 additionally include territory fixed effects. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Figure B.1: Probit nearest neighbor matching balance: Jesuits.



Note: This figure shows standardized mean differences (%) across covariates before matching (solid black dots) and after matching (black crosses) with respect to Jesuit exposure in the sample of Catholic towns. The matched sample is obtained using probit nearest-neighbor matching (3 nearest neighbors) on pre-1500 observables: indicators for the presence of a monastery, school, university, market, established city rights, and population category, as well as distance to river, distance to coast, average ruggedness, and agricultural suitability.

Figure B.2: Probit nearest neighbor matching balance: Protestant academies.



Note: This figure shows standardized mean differences (%) across covariates before matching (solid black dots) and after matching (black crosses) with respect to Protestant academy exposure in the sample of Protestant towns. The matched sample is obtained using probit nearest-neighbor matching (3 nearest neighbors) on pre-1500 observables: indicators for the presence of a monastery, school, university, market, established city rights, and population category, as well as distance to river, distance to coast, average ruggedness, and agricultural suitability.

Table B.2: Academy exposure and religious identity (nearest-neighbor matching).

	Protestant name score			
	(1)	(2)	(3)	(4)
	Catholic towns		Protestant towns	
Jesuit exposure	-0.023***	-0.024**		
	(0.008)	(0.011)		
Protestant academy exposure			0.013**	0.020**
			(0.006)	(0.008)
R^2	0.316		0.297	
Observations	3678	3678	5778	5778
Number of Towns	157	157	212	212
Outcome mean	0.478	0.478	0.539	0.539
Town FEs	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓
Cluster	Town	Town	Town	Town
Estimator	TWFE	CSDID	TWFE	CSDID

Notes: This table reports estimates from Equation 5 using a sample of towns matched via probit nearest-neighbor matching (3 nearest neighbors) on pre-1500 observables. Observations are at the town–decade level; the number of towns is reported in the table. The sample spans 29 decades (1480–1760) for Catholic towns (ending in the decade before suppression of the Jesuit order in 1773 analyzed separately in 5.2.2) and 33 decades (1480–1800) for Protestant towns. The dependent variable is the average Protestant name score in town i and decade t. Columns 1–2 use Catholic towns, with treatment defined by the decade of Jesuit arrival; Columns 3–4 use Protestant towns, with treatment defined by the decade of Protestant academy establishment. Columns 1 and 3 report two-way fixed effects (TWFE) estimates; Columns 2 and 4 report estimates using the cohort-by-time difference-in-differences estimator of Callaway and Sant'Anna (2021). The control group consists of never-treated and not-yet-treated towns. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table B.3: Academy exposure and religious identity (Doubly-robust Inverse Probability Weighting).

	Protestant name score			
	(1)	(2)	(3)	(4)
	Catholic towns		Protestant towns	
Jesuit exposure	-0.024*	-0.028***		
	(0.013)	(0.009)		
Protestant academy exposure			0.026***	0.017**
			(0.008)	(0.008)
Observations	14122	14122	33319	33319
Number of Towns	699	699	1440	1440
Outcome mean	0.485	0.485	0.547	0.547
Town FEs	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓
Town controls	✓		✓	
Expected Exposure		✓		✓
Cluster	Town	Town	Town	Town
Doubly Robust IPW	✓	✓	✓	✓

Notes: This table reports estimates from Equation 5 using doubly robust inverse probability weighting (DR-IPW) as proposed by Sant'Anna and Zhao (2020). Observations are at the town–decade level; the number of towns is reported in the table. The sample spans 29 decades (1480–1760) for Catholic towns (ending in the decade before suppression of the Jesuit order in 1773 analyzed separately in 5.2.2) and 33 decades (1480–1800) for Protestant towns. The dependent variable is the average Protestant name score in town i and decade t. Columns 1–2 use Catholic towns, with treatment defined by the decade of Jesuit arrival; Columns 3–4 use Protestant towns, with treatment defined by the decade of Protestant academy establishment. Columns 1 and 3 compute inverse probability weights using baseline town characteristics measured in 1500: town size, monastery, school, and university presence. Columns 2 and 4 compute weights based on expected exposure as described in Section B.3. The control group consists of never-treated and not-yet-treated towns. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table B.4: Academy exposure and religious identity (Balanced Panel).

	Protestant name score			
	(1)	(2)	(3)	(4)
	Catholic towns		Protestant towns	
Jesuit exposure	-0.021**	-0.040***		
	(0.009)	(0.012)		
Protestant academy exposure			0.018***	0.019***
			(0.007)	(0.007)
Observations	1617	1617	5285	5285
Number of Towns	56	56	161	161
Outcome mean	0.475	0.475	0.539	0.539
Town FEs	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓
Expected Exposure		✓		✓
Cluster	Town	Town	Town	Town

Notes: This table reports estimates from Equation 5 using a balanced sample of towns with at least 10 baptisms observed in every decade between 1480 and 1800. Observations are at the town–decade level; the number of towns is reported in the table. The sample spans 29 decades (1480–1760) for Catholic towns (ending in the decade before suppression of the Jesuit order in 1773 analyzed separately in 5.2.2) and 33 decades (1480–1800) for Protestant towns. The dependent variable is the average Protestant name score in town i and decade t. Columns 1–2 use Catholic towns, with treatment defined by the decade of Jesuit arrival; Columns 3–4 use Protestant towns, with treatment defined by the decade of Protestant academy establishment. The control group consists of never-treated and not-yet-treated towns. Columns 2 and 4 additionally control for expected exposure, as described in Section B.3, to adjust for selection into treatment based on baseline town characteristics. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table B.5: Academy exposure and religious identity (20 year rolling name score).

	Protestant name score (rolling)			
	(1)	(2)	(3)	(4)
	Catholic towns		Protestant towns	
Jesuit exposure	-0.022***	-0.024***		
	(0.007)	(0.008)		
Protestant academy exposure			0.012*	0.015**
			(0.006)	(0.007)
Observations	14168	14168	33400	33400
Number of Towns	700	700	1440	1440
Outcome mean	0.484	0.484	0.546	0.546
Town FEs	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓
Expected Exposure		✓		✓
Cluster	Town	Town	Town	Town

Notes: This table reports estimates from Equation 5 using the 20-year rolling version of the average Protestant name score as the dependent variable. Observations are at the town–decade level; the number of towns is reported in the table. The sample spans 29 decades (1480–1760) for Catholic towns (ending in the decade before suppression of the Jesuit order in 1773 analyzed separately in 5.2.2) and 33 decades (1480–1800) for Protestant towns. The dependent variable is the rolling average Protestant name score in town i and decade t. Columns 1–2 use Catholic towns, with treatment defined by the decade of Jesuit arrival; Columns 3–4 use Protestant towns, with treatment defined by the decade of Protestant academy establishment. The control group consists of never-treated and not-yet-treated towns. Columns 2 and 4 additionally control for expected exposure, as described in Section B.3, to adjust for selection into treatment based on baseline town characteristics. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table B.6: Academy exposure and religious identity (cluster average name score).

	Protestant name score (cluster average)			
	(1)	(2)	(3)	(4)
	Catholic towns		Protestant towns	
Jesuit exposure	-0.021	-0.023*		
	(0.012)	(0.013)		
Protestant academy exposure			0.013*	0.013*
			(0.007)	(0.007)
Observations	14065	14065	33172	33172
Number of Towns	699	699	1439	1439
Outcome mean	0.497	0.497	0.549	0.549
Town FEs	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓
Expected Exposure		✓		✓
Cluster	Town	Town	Town	Town

Notes: This table reports estimates from Equation 5 using the cluster-averaged version of the Protestant name score as the dependent variable. Observations are at the town–decade level; the number of towns is reported in the table. The sample spans 29 decades (1480–1760) for Catholic towns (ending in the decade before suppression of the Jesuit order in 1773 analyzed separately in 5.2.2) and 33 decades (1480–1800) for Protestant towns. Columns 1–2 use Catholic towns, with treatment defined by the decade of Jesuit arrival; Columns 3–4 use Protestant towns, with treatment defined by the decade of Protestant academy establishment. The control group consists of never-treated and not-yet-treated towns. Columns 2 and 4 additionally control for expected exposure, as described in Section B.3, to adjust for selection into treatment based on baseline town characteristics. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table B.7: Academy exposure and religious identity (capped city influence).

	Protestant name score (rolling)			
	(1)	(2)	(3)	(4)
	Catholic towns		Protestant towns	
Jesuit exposure	-0.024***	-0.022***		
	(0.007)	(0.008)		
Protestant academy exposure			0.018***	0.019***
			(0.006)	(0.006)
Observations	14093	14093	33273	33273
Number of Towns	699	699	1440	1440
Outcome mean	0.476	0.476	0.537	0.537
Town FEs	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓
Expected Exposure		✓		✓
Cluster	Town	Town	Town	Town

Notes: This table reports estimates from Equation 5 using a capped sample, where the number of baptisms per town–decade cell is limited to a maximum of 100 to reduce the influence of large towns. Observations are at the town–decade level; the number of towns is reported in the table. The sample spans 29 decades (1480–1760) for Catholic towns (ending in the decade before suppression of the Jesuit order in 1773 analyzed separately in 5.2.2) and 33 decades (1480–1800) for Protestant towns. The dependent variable is the average Protestant name score in town i and decade t. Columns 1–2 use Catholic towns, with treatment defined by the decade of Jesuit arrival; Columns 3–4 use Protestant towns, with treatment defined by the decade of Protestant academy establishment. The control group consists of never-treated and not-yet-treated towns. Columns 2 and 4 additionally control for expected exposure, as described in Section B.3, to adjust for selection into treatment based on baseline town characteristics. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table B.8: Academy exposure and religious identity (only prime name).

	Protestant name score (rolling)			
	(1)	(2)	(3)	(4)
	Catholic towns		Protestant towns	
Jesuit exposure	-0.022**	-0.024**		
	(0.009)	(0.010)		
Protestant academy exposure			0.016**	0.019**
			(0.007)	(0.008)
Observations	14096	14096	33270	33270
Number of Towns	699	699	1440	1440
Outcome mean	0.487	0.487	0.546	0.546
Town FEs	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓
Expected Exposure		✓		✓
Cluster	Town	Town	Town	Town

Notes: This table reports estimates from Equation 5 using only the prime (first) name to construct the average Protestant name score. Observations are at the town–decade level; the number of towns is reported in the table. The sample spans 29 decades (1480–1760) for Catholic towns (ending in the decade before suppression of the Jesuit order in 1773 analyzed separately in 5.2.2) and 33 decades (1480–1800) for Protestant towns. Columns 1–2 use Catholic towns, with treatment defined by the decade of Jesuit arrival; Columns 3–4 use Protestant towns, with treatment defined by the decade of Protestant academy establishment. The control group consists of never-treated and not-yet-treated towns. Columns 2 and 4 additionally control for expected exposure, as described in Section B.3, to adjust for selection into treatment based on baseline town characteristics. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table B.9: Academy exposure and religious identity (Spillovers: 20km).

	Protestant name score			
	(1)	(2)	(3)	(4)
	Catholic towns		Protestant towns	
Jesuit exposure	-0.024***	-0.024**		
	(0.009)	(0.011)		
Protestant academy exposure			0.013*	0.015**
			(0.007)	(0.007)
Observations	11542	11542	28927	28927
Number of Towns	660	660	1363	1363
Outcome mean	0.487	0.487	0.548	0.548
Town FEs	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓
Expected Exposure		✓		✓
Cluster	Town	Town	Town	Town

Notes: This table reports estimates from Equation 5 excluding all untreated towns located within 20 km of a treated town, in order to limit potential spillover effects. Observations are at the town–decade level; the number of towns is reported in the table. The sample spans 29 decades (1480–1760) for Catholic towns (ending in the decade before suppression of the Jesuit order in 1773 analyzed separately in 5.2.2) and 33 decades (1480–1800) for Protestant towns. The dependent variable is the average Protestant name score in town i and decade t. Columns 1–2 use Catholic towns, with treatment defined by the decade of Jesuit arrival; Columns 3–4 use Protestant towns, with treatment defined by the decade of Protestant academy establishment. The control group consists of never-treated and not-yet-treated towns outside the 20 km spillover radius. Columns 2 and 4 additionally control for expected exposure, as described in Section B.3, to adjust for selection into treatment based on baseline town characteristics. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table B.10: Academy exposure and religious identity (Spillovers: 30km).

	Protestant name score			
	(1)	(2)	(3)	(4)
	Catholic towns		Protestant towns	
Jesuit exposure	-0.024**	-0.021		
	(0.010)	(0.013)		
Protestant academy exposure			0.013	0.015*
			(0.008)	(0.009)
Observations	9100	9100	23897	23897
Number of Towns	619	619	1264	1264
Outcome mean	0.487	0.487	0.547	0.547
Town FEs	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓
Expected Exposure		✓		✓
Cluster	Town	Town	Town	Town

Notes: This table reports estimates from Equation 5 excluding all untreated towns located within 30 km of a treated town, in order to limit potential spillover effects. Observations are at the town–decade level; the number of towns is reported in the table. The sample spans 29 decades (1480–1760) for Catholic towns (ending in the decade before suppression of the Jesuit order in 1773 analyzed separately in 5.2.2) and 33 decades (1480–1800) for Protestant towns. The dependent variable is the average Protestant name score in town i and decade t. Columns 1–2 use Catholic towns, with treatment defined by the decade of Jesuit arrival; Columns 3–4 use Protestant towns, with treatment defined by the decade of Protestant academy establishment. The control group consists of never-treated and not-yet-treated towns outside the 30 km spillover radius. Columns 2 and 4 additionally control for expected exposure, as described in Section B.3, to adjust for selection into treatment based on baseline town characteristics. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table B.11: Academy exposure and religious identity (No university towns).

	Protestant name score			
	(1)	(2)	(3)	(4)
	Catholic towns		Protestant towns	
Jesuit exposure	-0.021	-0.023*		
	(0.012)	(0.013)		
Protestant academy exposure			0.013*	0.013*
			(0.007)	(0.007)
Observations	13740	13740	32580	32580
Number of Towns	685	685	1417	1417
Outcome mean	0.485	0.485	0.547	0.547
Town FEs	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓
Expected Exposure		✓		✓
Cluster	Town	Town	Town	Town

Notes: This table reports estimates from Equation 5 excluding all towns that ever hosted a university from the sample. Observations are at the town–decade level; the number of towns is reported in the table. The sample spans 29 decades (1480–1760) for Catholic towns (ending in the decade before suppression of the Jesuit order in 1773 analyzed separately in 5.2.2) and 33 decades (1480–1800) for Protestant towns. The dependent variable is the average Protestant name score in town i and decade t. Columns 1–2 use Catholic towns, with treatment defined by the decade of Jesuit arrival; Columns 3–4 use Protestant towns, with treatment defined by the decade of Protestant academy establishment. The control group consists of never-treated and not-yet-treated towns. Columns 2 and 4 additionally control for expected exposure, as described in Section B.3, to adjust for selection into treatment based on baseline town characteristics. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table B.12: Panel A. Jesuits and religious identity (Standard Errors).

	Protestant name score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Catholic towns			
Jesuit exposure	-0.022***	-0.022***	-0.022***	-0.022***	-0.022***	-0.022***	-0.022***
	(0.006)	(0.004)	(0.004)	(0.006)	(0.003)	(0.003)	(0.003)
R^2	0.302	0.302	0.302	0.302	0.002	0.002	0.002
Observations	14122	14122	14122	14122	14142	14142	14142
Number of Towns	699	699	699	699	699	699	699
Outcome mean	0.485	0.485	0.485	0.485	0.485	0.485	0.485
Town FEs	✓	✓	✓	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓	✓	✓	✓
Cluster	Town	Region	Territory	Imperical Circle	50km	100km	200km

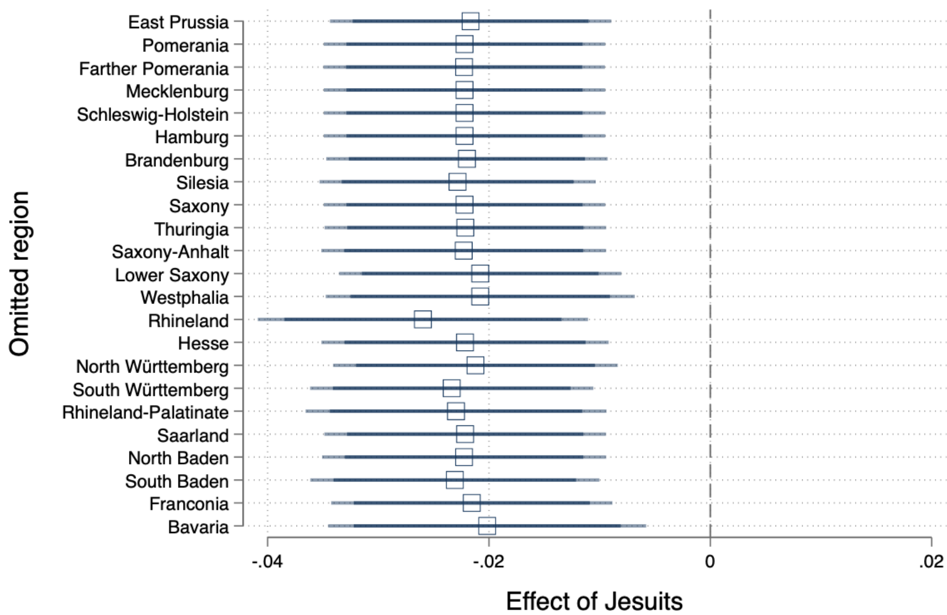
Table B.12: Panel B. Protestant academies and religious identity (Standard Errors).

	Protestant name score						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Protestant towns			
Protestant academy exposure	0.009*	0.009**	0.009**	0.009*	0.009***	0.009***	0.009***
	(0.005)	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)	(0.002)
R^2	0.301	0.301	0.294	0.294	0.000	0.000	0.000
Observations	32071	32071	30889	30889	32098	32098	32098
Number of Towns	1436	1436	1436	1434	1436	1436	1436
Outcome mean	0.546	0.546	0.546	0.546	0.546	0.546	0.546
Town FEs	✓	✓	✓	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓	✓	✓	✓
Cluster	Town	Region	Territory	Imperical Circle	50km	100km	200km

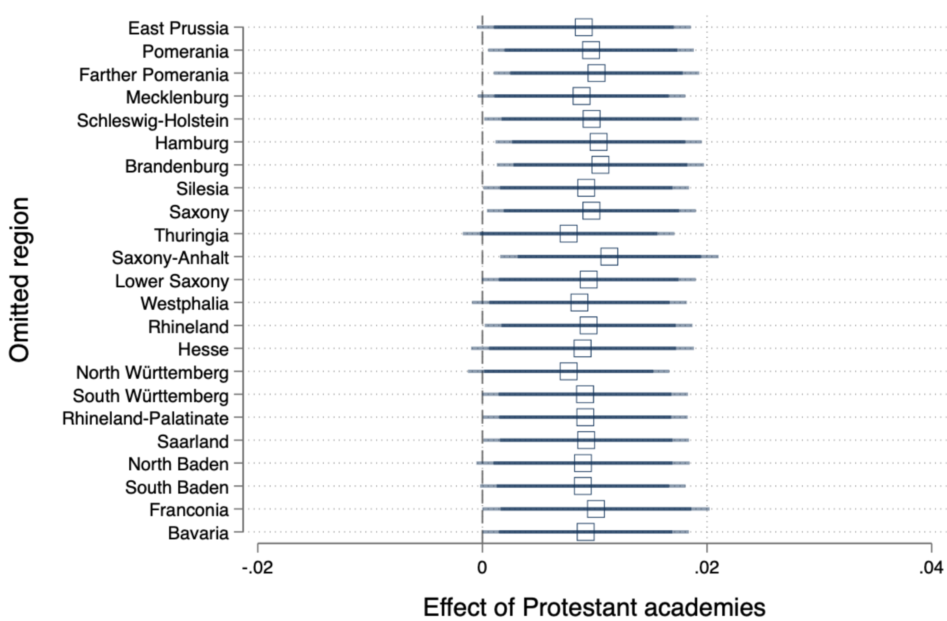
Notes: This table reports results from estimating Equation 4. Observations are at the town–decade level; the number of towns is reported in the table. The sample spans 29 decades (1480–1760) for Catholic towns (ending in the decade before suppression of the Jesuit order in 1773 analyzed separately in 5.2.2) and 33 decades (1480–1800) for Protestant towns. The dependent variable is the average Protestant name score in town i and decade t. Columns differ by the level of clustering used to compute standard errors: town (Column 1), region (Column 2), territory (Column 3), and Imperial Circle (Column 4). Columns 5–7 report Conley (spatially corrected) standard errors using the distance cutoffs indicated in the table. Panel A uses the sample of Catholic towns with Jesuit exposure as treatment; Panel B uses the sample of Protestant towns with Protestant academy exposure as treatment. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Figure B.3: Leave-out plots.

(a) Effect of Jesuit colleges



(b) Effect of Protestant academies



Note: This figure shows results from estimating Equation 4 while sequentially leaving out one region (based on the *Deutsches Städtebuch* classification) at a time. Observations are at the town–decade level; the number of towns is reported in Table 1. The dependent variable is the average Protestant name score in town c and decade t. Panel A reports results for the sample of Catholic towns with Jesuit college exposure as treatment. The sample spans 29 decades (1480–1760) (ending in the decade before suppression of the Jesuit order in 1773 analyzed separately in 5.2.2). Panel B reports results for Protestant towns with Protestant academy exposure as treatment (33 decades, 1480–1800). All models include town and decade fixed effects. Standard errors are clustered at the town level, and 95 percent confidence intervals are shown.

B.2 Alternative Explanation: Adverse Shocks

The robustness exercises above confirm that academy exposure systematically increases religious identity differentiation. However, a conceptually distinct question remains: could bottom-up responses to adverse events, rather than top-down church enforcement, drive the observed polarization? If crises heightened existential anxieties or group boundaries, they might trigger stronger adoption of confessional markers independent of institutional efforts.

Hypothesis. Under this alternative mechanism, adverse shocks should coincide with sharper confessional differentiation in naming. Crises that threaten community survival—violent conflict, disease outbreaks, environmental disasters—may intensify in-group solidarity and out-group distinction. If parents use names to signal group membership more clearly during uncertain times, Protestant name scores should rise among Protestants and fall among Catholics following local shocks.

Empirical strategy. I test this hypothesis by replacing academy exposure with indicators for five shock types, using the baseline difference-in-differences specification from Equation 4. The shocks are: (i) violent conflict (battles, sieges, military occupations), (ii) pandemic outbreaks (plague, typhus, dysentery), (iii) severe winters (mean temperature $>$ 1 standard deviation below the long-run mean), (iv) solar eclipses (linked to heightened ingroup-outgroup salience by Barber, Jetter, and Krieger (2023)), and (v) major urban fires. Each shock indicator equals one in the decade of occurrence. If adverse events heighten confessional salience, I should observe significant coefficients on these indicators.

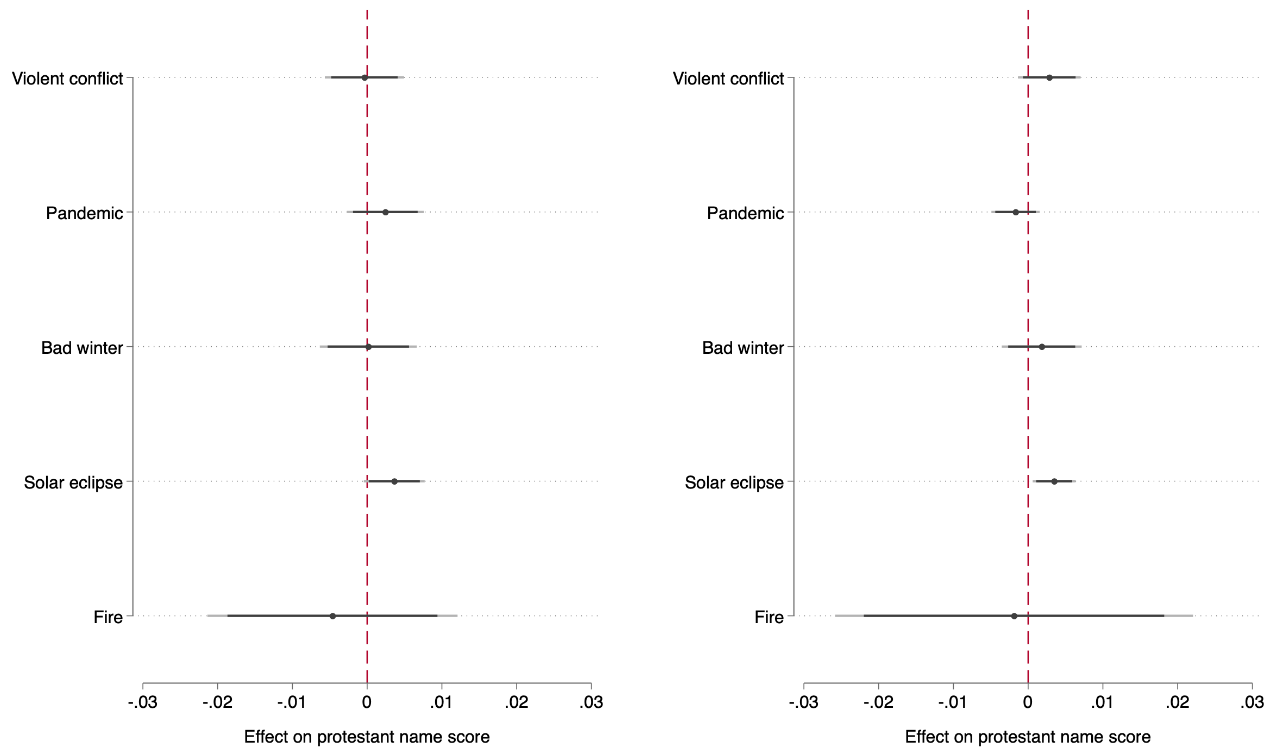
Results: Confessional differentiation. Appendix Figure B.4 presents estimates separately for Catholic towns (Panel A) and Protestant towns (Panel B), with the Protestant name score as the outcome. None of the five shock types associates with meaningful changes in confessional differentiation. Point estimates cluster near zero, and 95% confidence intervals include zero for all shocks in both panels. Violent conflict—the shock most likely to intensify group boundaries—shows no systematic effect. Solar eclipses, widely interpreted as divine warnings in this period, similarly fail to predict naming polarization.

Results: Religious intensity. As an alternative test, Appendix Figure B.5 examines whether shocks increase the overall prevalence of religious names (biblical names, saint names, names containing ”Gott”), which might rise with heightened religiosity (see Bentzen (2019)). Some shocks modestly increase religious name usage among Catholics—consistent with pre-existing Catholic emphasis on saints—but these increases do not translate into greater Catholic-Protestant differentiation in naming patterns. Among Protestants, coefficients remain close to zero.

Interpretation. The null findings for adverse shocks contrast sharply with the robust effects of academy exposure documented in Section 5. This pattern is inconsistent with a crisis-driven, bottom-up theory of confessional identity formation. Instead, the results suggest that the documented rise in naming differentiation reflects deliberate, top-down enforcement investments by churches rather than spontaneous responses to existential threats. Churches actively constructed and maintained confessional boundaries through sustained institutional efforts—educational infrastructure, clergy training, visitation systems—not merely reactive intensification during crises.

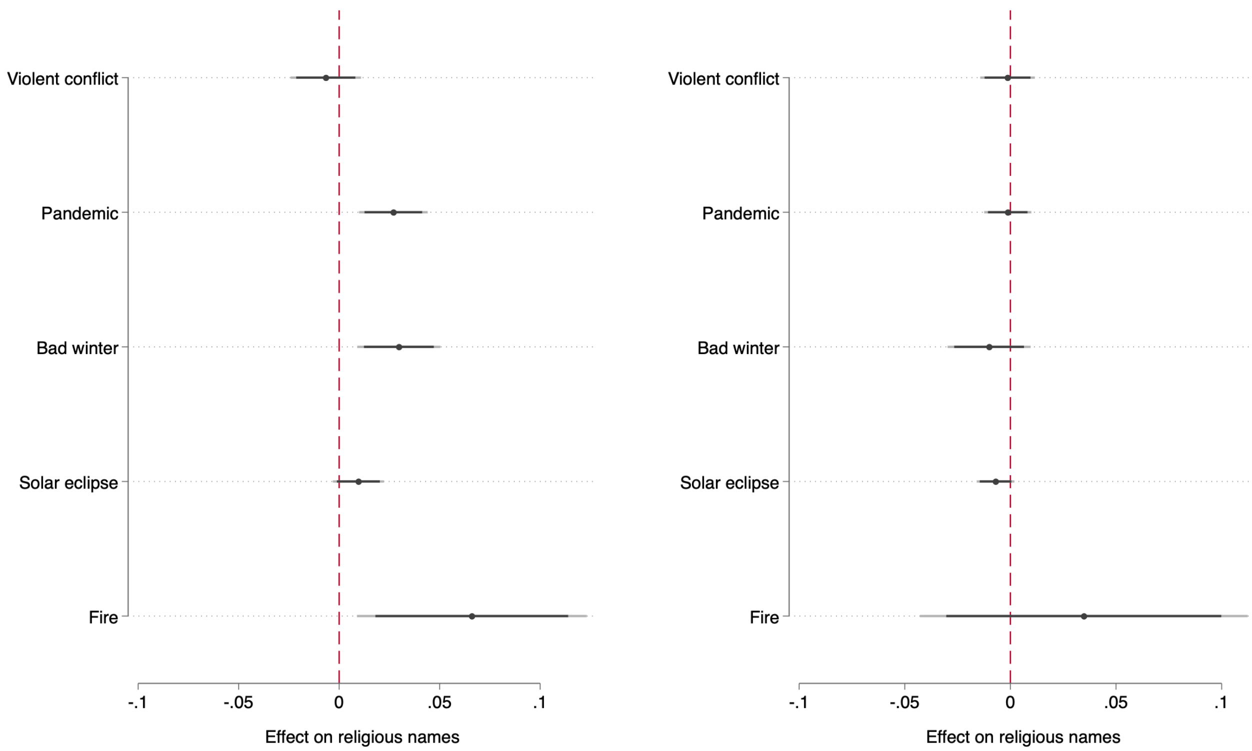
This exercise complements the main robustness checks by ruling out an alternative causal mechanism. Together, the evidence points to church capacity, not environmental shocks, as the primary driver of confessional identity formation during the Reformation era.

Figure B.4: Adverse shocks and confessional differentiation.



Note: This figure shows estimates from estimating Equation 4 with 95% confidence intervals using an indicator of exposure to adverse shocks as treatment. Observations are at the town–decade level; the number of towns is reported in Table 1. The sample spans 33 decades (1480–1800). The dependent variable is the average Protestant name score in town c and decade t. Coefficients are reported for exposure to violent conflict, pandemic, cold winter (temperature $>$ 1 SD below the long-run mean), solar eclipse, and fire outbreak. Panel A uses Catholic towns; Panel B uses Protestant towns. Standard errors are clustered at the town level.

Figure B.5: Adverse shocks and religious names.



Note: This figure shows estimates from estimating Equation 4 with 95% confidence intervals, using the share of baptisms with religious names as the dependent variable and an indicator of exposure to adverse shocks as treatment. Observations are at the town–decade level; the number of towns is reported in Table 1. The sample spans 33 decades (1480–1800). Coefficients are reported for exposure to violent conflict, pandemic, cold winter (temperature > 1 SD below the long-run mean), solar eclipse, and fire outbreak. Panel A uses Catholic towns; Panel B uses Protestant towns. Standard errors are clustered at the town level.

B.3 Expected Exposure

Motivation and Identification Challenge

The main empirical analysis faces a fundamental identification challenge: church colleges were not randomly assigned across towns. Jesuit colleges and Protestant academies were systematically placed in towns with universities, larger populations, and existing educational infrastructure—precisely the characteristics that might independently predict stronger confessional identity formation. This non-random selection threatens causal inference.

Consider a simplified, time-invariant version of the main regression:

$$y_i = \beta x_i + \varepsilon_i, \tag{23}$$

where $i = 1, \dots, N$ indexes towns, $x_i = \text{Jesuit}_i$ (or Academy_i) is an indicator for exposure, and y_i is the outcome of interest (e.g., the Protestant name score). The parameter of interest β measures the effect of exposure on y_i. The concern is that x_i is endogenous: $\mathbb{E}[\varepsilon_i|x_i] \neq 0$ because towns selected for colleges systematically differ from untreated towns in ways correlated with y_i.

To address this, I construct an *expected exposure* measure following Borusyak and Hull (2023). The intuition is to separate college placement into two components: (i) a predictable component based on observable town characteristics that influenced historical site selection, and (ii) quasi-random residual variation arising from idiosyncratic factors (e.g., local political circumstances, availability of suitable buildings, benefactor preferences). By controlling for the predictable component, I isolate variation in exposure that is plausibly exogenous conditional on observables.

Formal Framework

I model the historical assignment process as $G(g \mid w)$, which provides the probability that each town would have been treated given its baseline characteristics w_i (university presence, population size, educational infrastructure). Formally, let $g = (\text{Realized}_k)_{k=1}^{K}$ denote one possible realization of foundation sites, and define a deterministic mapping

$$x_i = \tau(g; w_i), \tag{24}$$

where $\tau(\cdot)$ encodes (i) the eligible set of towns, (ii) the priority-based selection mechanism, (iii) the assignment of start years, and (iv) the construction of the exposure indicator.

Expected exposure for town i at time t is then:

$$\widehat{x}_{it} = \mathbb{E}[x_{it} \mid w_i]\,, \tag{25}$$

where the expectation is taken over $S = 100$ simulation draws of g from $G(g \mid w)$. The resulting $\widehat{x}_{it}$ is the probability that town i would have hosted a Jesuit college (or academy) by year t given w_i.

In the main specification, $\widehat{x}_{it}$ enters directly as a control:

$$y_{it} = \beta x_{it} + \gamma \widehat{x}_{it} + \alpha_i + \alpha_t + \varepsilon_{it}, \tag{26}$$

where γ absorbs the predictable component of treatment status. Identification of β comes from quasi-random deviations: towns that received colleges earlier or later than predicted by their baseline characteristics, or towns observably similar to treated towns that were never treated.

Identifying Assumptions. This approach requires three key assumptions (Borusyak and Hull 2023):

- **Shock Exogeneity:** Conditional on w_i, the residual randomness in the selection of foundation sites is as-good-as-random. Deviations from expected exposure arise from factors uncorrelated with potential outcomes.
- **Known Assignment Process:** The number of colleges (N_g) and the priority rule are correctly specified based on historical sources. Misspecification would leave predictable variation in the residual.
- **Relevance for Selection:** The simulated assignment process captures the main sources of predictable selection into treatment. Observables w_i span the characteristics that actually determined site selection.

The validity of these assumptions can be partially assessed through specification tests, reported below.

Construction of Expected Exposure: Simulation Procedure

For each institution type $g \in \{\text{Jesuit}, \text{Academy}\}$, I generate expected exposure $\widehat{x}^g_{it}$ through repeated simulation of the assignment process. Define x^g_{its} as an indicator that town i hosts an institution of type g in year t in simulation s. Expected exposure is then:

$$\widehat{x}^g_{it} = \frac{1}{S}\sum_{s=1}^{S} x^g_{its}, \tag{27}$$

where $S = 100$ is the number of simulations. Each simulation draw proceeds in four steps:

1. **Eligible Set:** Identify towns eligible to host an institution at baseline (1516). For Jesuit colleges, this includes Catholic or mixed-confession towns. For Protestant academies, this includes towns that eventually became Protestant.
2. **Priority-Based Selection:** Randomly select N_g towns using a priority rule that mimics historical site selection:
   - Draw randomly within priority tiers (universities first, then large towns, then towns with schools/monasteries)
   - Randomness within tiers ensures that observably similar towns have positive probability of (non-)selection

- Aggregate across all tiers to reach the historical total N_g

3. **Timing Assignment:** Randomly order the N_g selected towns and assign historically observed foundation years to generate simulated start dates. This preserves the temporal distribution of foundations while randomizing which specific towns receive each date.

4. **Exposure Construction:** For each town-year pair, set $x^g_{its} = 1$ if $t \geq \text{start}_{is}$ (treating exposure as an absorbing state), and $x^g_{its} = 0$ otherwise.

Repeating this procedure $S = 100$ times and averaging yields the expected exposure measure $\widehat{x}^g_{it}$ for each town-year. Towns with characteristics that strongly predict selection (e.g., university presence) receive high expected exposure; towns with few predictive characteristics receive low expected exposure.

Implementation: Jesuit Colleges

The Jesuit simulation uses parameters calibrated to match the historical record:

- **Eligible Towns:** 820 Catholic or mixed-confession towns at baseline (1516)
- **Number of Sites:** $N_{\text{Jesuit}} = 58$, matching the historical number of Jesuit colleges established in the sample region
- **Priority Rule:**
  - Tier 1: Randomly select 3 towns with universities (reflecting Jesuit strategy of establishing colleges in university towns)
  - Tier 2: Randomly select 5 towns with >5,000 inhabitants (large urban centers)
  - Tier 3: Randomly select 15 towns with schools or monasteries (existing educational infrastructure)
  - Tier 4: Randomly select remaining 35 sites from all eligible towns
- **Timing:** Assign historical foundation years (1556–1753) in randomized order to selected towns

Implementation: Protestant Academies

The Protestant academy simulation follows an analogous structure:

- **Eligible Towns:** 1,570 (eventually) Protestant towns
- **Number of Sites:** $N_{\text{Academy}} = 51$, matching the historical number
- **Priority Rule:**
  - Tier 1: Randomly select 5 towns with universities
  - Tier 2: Randomly select 10 large towns (>5,000 inhabitants)
  - Tier 3: Randomly select 25 towns with schools or monasteries

  - Tier 4: Randomly select remaining 11 sites from all eligible towns

- **Timing:** Assign historical foundation years (1386–1760) in randomized order

The priority tier sizes reflect the historical importance of each characteristic in actual site selection, estimated from descriptive regressions of realized exposure on baseline observables.

Specification Tests and Validation

I conduct three tests to validate the expected exposure approach:

(1) Covariate balance. Appendix Figures A.3 and A.4 regress baseline covariates on (i) raw exposure and (ii) residual exposure (actual minus expected). Panel A of each figure shows that raw exposure strongly predicts baseline characteristics—confirmation of non-random selection. Panel B shows that residual exposure is approximately orthogonal to observables: after conditioning on $\widehat{x}_{it}$, treatment and control towns are balanced on all measured characteristics. Standardized differences fall below 0.1 in absolute value for nearly all covariates. This supports the assumption that remaining variation is plausibly exogenous.

(2) Geographic distribution. Figure A.2, Panel A, maps residual Jesuit exposure; Figure A.2, Panel B, maps residual Protestant academy exposure. Positive residuals (darker shading) indicate towns that received colleges earlier or more intensively than predicted; negative residuals indicate towns that were never treated despite favorable characteristics. The maps show that residual variation spans the full geographic area rather than clustering in specific regions, supporting the plausibility of quasi-random assignment conditional on observables.

Together, these tests support the validity of the expected exposure approach. By conditioning on $\widehat{x}_{it}$, the main analysis isolates plausibly exogenous variation in academy exposure, strengthening causal interpretation of the estimated effects.

B.4 Robustness of name-level learning analysis

Section 6 presents evidence that individuals learn name-confession associations by observing usage patterns among geographically proximate co-religionists. This subsection assesses robustness of the name-level learning results to alternative specifications and network definitions.

Appendix Table B.13 replicates the analysis using the change in choice probability—the share of births receiving name n—as the dependent variable, rather than changes in the Protestant score of name n. Results remain consistent, confirming that convergence occurs in actual naming behavior, not just in the measured confessional association.

Appendix Table B.14 tests sensitivity to the geographic scope of the learning network by varying the distance threshold from 25km to 75km. Convergence coefficients remain stable across specifications, with point estimates slightly larger at intermediate distances (50km), suggesting that learning operates primarily through nearby rather than distant locations.

Appendix Table B.15 decomposes the network effect by distance bands, constructing separate gap measures for concentric rings (0-50km, 50-100km, 100-150km, 150-200km). The coefficient on the 0-50km gap dominates, with effects attenuating sharply for more distant rings. This spatial decay pattern confirms that name-level learning primarily operates through local observation rather than diffuse regional trends.

Table B.13: Name-level updating: change in choice probabilities.

	Δ Choice Probability			
	(1)	(2)	(3)	(4)
Gap (t-1)	0.0519***	0.0351***	0.0524***	0.0371***
	(0.0017)	(0.0015)	(0.0023)	(0.0015)
Gap (t-1) × High name usage		0.0373***		
		(0.0021)		
Gap (t-1) × High network density			-0.0012	
			(0.0030)	
Gap (t-1) × High signal clarity				0.0324***
				(0.0025)
Observations	122079	122079	122079	122079
Number of towns	767	767	767	767
Town FEs	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓
Name FEs	✓	✓	✓	✓
Cluster	Town	Town	Town	Town

Notes: This table replicates the name-level learning analysis using an alternative dependent variable. The dependent variable is the change in the probability that name n is chosen by confession g in location ℓ from decade $t-1$ to t, measured as the change in the share of births to confession g receiving name n. Gap($t-1$) measures the difference between the weighted average choice probability for name n among nearby co-religionists (within 50km, uniform weights) and the location's own choice probability for that name. High name usage, high network density, and high signal clarity are indicators for being above the median of local usage frequency, number of same-confession neighbors within 50km, and local clarity ($\sum_n \pi_{n,g,\ell,t} \cdot |\text{ProtScore}_n - 0.5|$), respectively. All specifications include name fixed effects, town fixed effects, and decade fixed effects. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table B.14: Name-level updating: variation of neighborhood radius.

	Δ Choice Probability			Δ Name Score		
	(1)	(2)	(3)	(4)	(5)	(6)
Gap (t-1)	0.0652***	0.0519***	0.0458***	0.3031***	0.3082***	0.3096***
	(0.0030)	(0.0017)	(0.0014)	(0.0094)	(0.0058)	(0.0054)
Observations	41526	122079	169356	41526	122079	169356
Number of towns	468	767	870	468	767	870
Town FEs	✓	✓	✓	✓	✓	✓
Decade FEs	✓	✓	✓	✓	✓	✓
Name FEs	✓	✓	✓	✓	✓	✓
Cluster	Town	Town	Town	Town	Town	Town
Radius	25km	50km	75km	25km	50km	75km

Notes: This table examines name-level learning using different distance thresholds for defining spatial networks. The dependent variable is the change in choice probability (columns 1-3) or Protestant score (columns 4-6) for name n among confession g in location ℓ from decade $t-1$ to t. Gap$(t-1)$ is constructed using uniform weights within three alternative radii: 25km (columns 1 and 4), 50km (columns 2 and 5), and 75km (columns 3 and 6). All specifications include name fixed effects, town fixed effects, and decade fixed effects. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table B.15: Name-level updating: ring specifications.

	Δ Choice Probability	Δ Name Score
	(1)	(2)
Gap to 0-50km (t-1)	0.0447***	0.1335***
	(0.0015)	(0.0067)
Gap to 50-100km (t-1)	0.0025***	0.0714***
	(0.0005)	(0.0043)
Gap to 100-150km (t-1)	0.0030***	0.0732***
	(0.0007)	(0.0054)
Gap to 150-200km (t-1)	0.0045***	0.0782***
	(0.0007)	(0.0052)
Observations	111510	111510
Number of towns	745	745
Town FEs	✓	✓
Decade FEs	✓	✓
Name FEs	✓	✓
Cluster	Town	Town

Notes:Notes: This table examines whether name-level convergence operates through proximate versus distant locations by constructing separate gaps for concentric distance rings. The dependent variable is the change in choice probability (column 1) or Protestant score (column 2) for name n among confession g in location ℓ from decade $t-1$ to t. Gap to 0-50km measures the difference between the average among neighbors within 50km and own usage. Gap to 50-100km, Gap to 100-150km, and Gap to 150-200km measure the difference between the average among neighbors in each respective distance ring and own usage. All specifications include name fixed effects, town fixed effects, and decade fixed effects. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

B.5 Robustness of group-level conformity analysis

Section 6 demonstrates that aggregate religious identity converges toward the local mean through preference-based conformity. This subsection verifies that the group-level conformity results are robust to alternative network specifications.

Appendix Table B.16 varies the distance threshold used to define spatial networks from 25km to 75km. Conformity coefficients remain positive and statistically significant across all specifications, with magnitudes relatively stable. This insensitivity to distance cutoffs suggests that conformity pressures operate consistently within local geographic clusters.

Appendix Table B.17 tests whether conformity operates through proximate versus distant neighbors by including separate gaps for concentric distance rings. The 0-50km gap drives most of the effect, with coefficients on more distant rings are lower. This spatial decay mirrors the learning results, indicating that social influence in identity formation operates primarily through geographically proximate communities rather than long-distance diffusion.

Together, these robustness checks confirm that both learning and conformity mechanisms exhibit strong spatial localization, consistent with the theoretical framework's emphasis on network-mediated social influence.

Table B.16: Group-level updating: variation of neighborhood radius.

	Δ Avg. name Score		
	(1)	(2)	(3)
Gap (t-1)	0.4903***	0.5965***	0.6124***
	(0.0237)	(0.0182)	(0.0182)
Observations	4380	9286	10943
Number of towns	429	723	822
Town FEs	✓	✓	✓
Decade FEs	✓	✓	✓
Cluster	Town	Town	Town
Radius	25km	50km	75km

Notes: This table examines group-level conformity using different distance thresholds for defining spatial networks. The dependent variable is the change in average Protestant score for confession g in location ℓ from decade $t-1$ to t. Gap$(t-1)$ is constructed using three alternative radii: 25km (column 1), 50km (column 2), and 75km (column 3). All specifications include location×confession fixed effects and decade fixed effects. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

Table B.17: Group-level updating: ring specifications.

	Δ Name Score
	(1)
Gap to 0-50km (t-1)	0.2080***
	(0.0297)
Gap to 50-100km (t-1)	0.0843***
	(0.0252)
Gap to 100-150km (t-1)	0.1330***
	(0.0417)
Gap to 150-200km (t-1)	0.0772**
	(0.0346)
Observations	9023
Number of towns	699
Town FEs	✓
Decade FEs	✓
Cluster	Town

Notes: This table examines whether group-level convergence operates through proximate versus distant locations by constructing separate gaps for concentric distance rings. The dependent variable is the change in average Protestant score for confession g in location ℓ from decade $t-1$ to t. Gap to 0-50km measures the difference between the aggregate identity among neighbors within 50km and own aggregate identity. Gap to 50-100km, Gap to 100-150km, and Gap to 150-200km measure the difference between the average among neighbors in each respective distance ring and own aggregate identity. All specifications include location×confession fixed effects and decade fixed effects. Standard errors are clustered at the town level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

C Data and Measurement

This section documents the construction, processing, and validation of the name dataset used in the analysis. The dataset combines three sources: parish baptism registers, university matriculation lists, and biographical databases, spanning 1480–1800 across more than 2,000 German towns. I first describe data sources and digitization procedures (Section C.1), then assess sample coverage and representativeness (Section C.2). I next detail data processing and harmonization (Section C.3), followed by the construction of the Protestant name score and aggregate partisanship measures (Section C.4). Finally, I address concerns about finite-sample bias (Section C.5), validate the measure against historical evidence and alternative constructions (Section C.6), and present illustrative examples (Section C.7).

C.1 Data Sources and Digitization

Baptism Records

The baptismal data originate from sixteenth- and seventeenth-century parish registers, preserved as handwritten archival documents in municipal and church archives across Germany. Systematic baptism registration began gradually after the Reformation, with Protestant territories typically adopting record-keeping earlier than Catholic regions following Luther's emphasis on parish administration. Catholic territories expanded systematic registration after the Council of Trent (1563) formalized record-keeping requirements.

To transform these manuscripts into a structured dataset, I implement a three-step digitization pipeline:

Step 1: Layout detection. I use *Transkribus* (https://www.transkribus.org), a platform for automated handwritten text recognition, to detect page layout and segment tables. The algorithm identifies text lines, columns, and table structures, isolating individual baptismal entries for subsequent processing (Appendix Figure C.1, Panel B).

Step 2: Optical character recognition (OCR). I apply state-of-the-art handwriting recognition models in *Transkribus* to perform OCR on the segmented lines. These models, trained on historical German scripts, extract text from each entry. For registers with tabular structure, the algorithm parses entries into fields (date, child's name, parents' names, godparents, residence). Appendix Figure C.1, Panel C shows sample output, with the child's first name appearing in the second column.

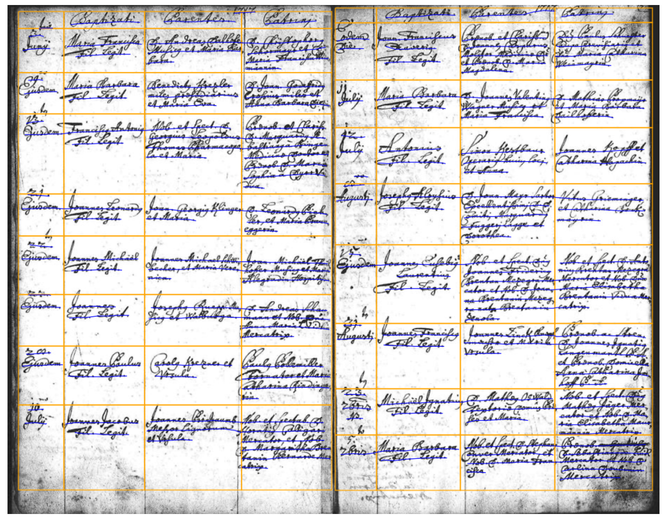
Step 3: Name standardization. I match the extracted name strings to a standardized list of first names compiled from the *Behind the Name* database (https://www.behindthename.com), which provides historical variants and etymologies. Fuzzy string matching (Jaro-Winkler distance with 0.85 threshold) reconciles spelling variations across time and region. Names that cannot be matched with sufficient confidence are dropped from the sample.

This process yields a harmonized dataset of given names suitable for computing the Protestant name score and other measures of naming practices.

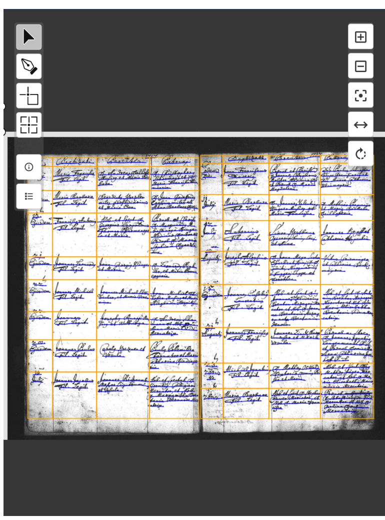
Figure C.1: Digitization of baptism records.

(a) Raw archival page

(b) Layout recognition



(c) OCR output



	Bantitate		hatten
oder de.	Joan. Franciscus Raverig. Fil. Legit.	Reprob. et Christ. P. Joanne parus Molitor Medicin et Cronob. c. Maria Magdalena	zu Baron Feuchter Coupo revisiare et zu Maria Catharina Weinwagen.
Juli	Maria Barbara Fil. Legit.	8. Joannes Valentin Wagner Music et Maria Fratris.	8. Mathias Gropmayr 24 Maria Barbara Bailhoferin
12. sul so	Antonius Fil. Legit.	Simon Kerbau Operario kein loci et Anna	Joanner Kopffel Catharina Hapachin
. Augusti	Joseph Klapsius Fl. Legit.	8. Jan. May Pastor Excellentissimi D. C. mit Marquard, Fugger Lage et Dorothea	Vitus Grieminger et Catharina Bock magen.
5. Ejusdem	Joannes Eusebig Laurentius Fil. Legit.	Nob. et spect. g. Joannes Frideri Erenten Mezegra Mei¬ cator et Nob. 8. Jan. na Brentaria Mere re natu Brentarin genola.	Nob. et Pact. p. Anto. ein Krenten Mezegra Mercator et Nob. Qua Maria Elisabetha Brentani Senate catris.
31. August	poannes Francise Fil. Legit.	Joannes Lincken, macher et Maril Anla¬	Pronob. a. Stren¬ Joannes Ignatio Langemant et Pronob. Domicella¬ Anna Catharina In¬ hoff P. A.

Note: This figure illustrates the digitization process used to extract first names from original church registers. Panel A displays a sample page of the raw archival record from a Catholic parish register. Panel B shows the output of the layout recognition algorithm, which detects text lines and segments the page into a table structure with columns for date, name, parents, and witnesses. Panel C presents the OCR output, where the second column lists the baptized child's first name; these names are then cleaned and matched to a standardized list using fuzzy string matching.

University Matriculation Lists

The dataset also incorporates university matriculation lists from 19 German universities, covering the period 1386–1800. Unlike baptism registers, which survive inconsistently, matriculation lists were systematically preserved and have been transcribed and published in edited volumes by historical societies during the late 19th and early 20th centuries. These sources provide complementary coverage, particularly for the pre-1550 period when systematic baptism registration was rare.

Each matriculation entry typically records the student's name, place of origin, and often the subject of study or degree sought. I digitize these printed volumes using OCR (for typeset text) and manual transcription (for complex tables), then parse entries into standardized fields. Appendix Figure C.2 illustrates the process. Place names are geocoded to town coordinates, and names are standardized using the same procedures applied to baptism records. Students are assigned to their town of origin rather than the university town, ensuring geographic correspondence with baptism data.

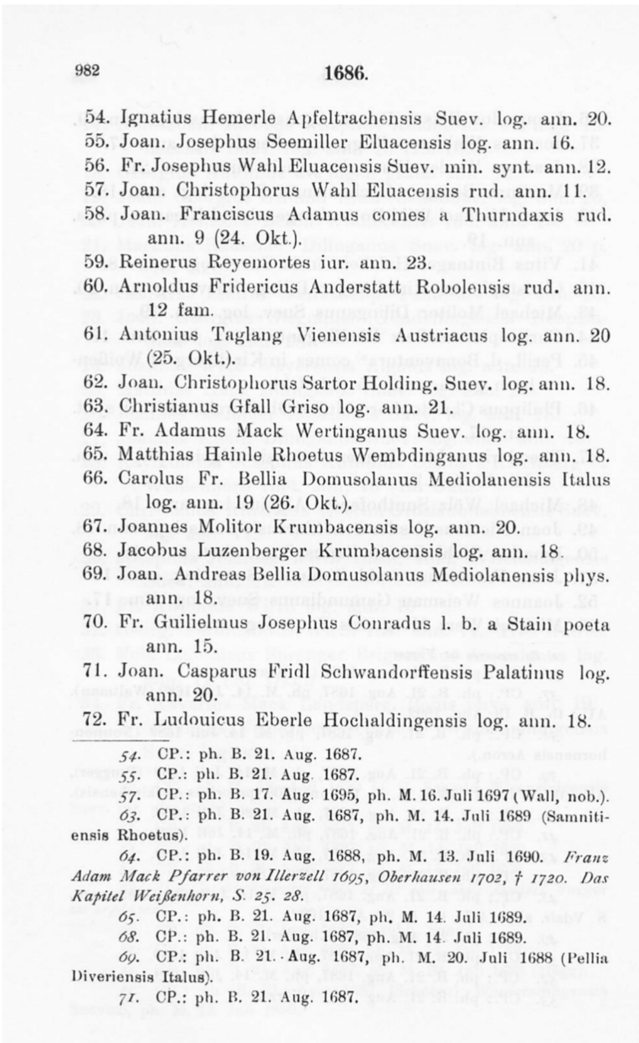
The matriculation lists draw from the following published sources:

- Ernst Friedländer, ed. (1887). *Aeltere Universitäts-Matrikeln der Universitaet Greifswald (1506 - 1648)*. ger. Vol. 1, Band 1. Place: Leipzig
- Fritz Juntke and Bernhard Weissenborn (1894). *Album Academiae Vitebergensis*. ger. Ed. by Karl Eduard Förstemann
- *Catalogus studiosorum Marpurgensis* (1872). lat. Marburg: Elwert
- Franz Gundlach (1915). *Das Album der Christian-Albrechts-Universität zu Kiel 1665-1865*. ger. Kiel: Lipsius & Tischer
- Franz Xaver Freninger (1872). *Das Matrikelbuch der Universität Ingolstadt-Landshut-München.*
- Georg Erler (1895). *Die Matrikel der Universität Leipzig, 1409-1559*. ger. Codex diplomaticus Saxoniae regiae Tl. 2, Bd. 16-18. Leipzig: :
- Georg Erler (1909). *Die jüngere Matrikel der Universität Leipzig 1559-1809 als Personen- und Ortsregister bearbeitet und durch Nachträge aus den Promotionslisten ergänzt*. ger. Leipzig: Giesecke & Devrient
- Bernhard Spörlein, ed. (2014). *Die Matrikel der Akademie und Universität Bamberg 1648-1803*. ger. Veröffentlichungen der Gesellschaft für fränkische Geschichte. 4. Reihe, Matrikeln fränkischer Schulen 12. Band. Würzburg: Gesellschaft für fränkische Geschichte. ISBN: 978-3-86652-413-2
- Elias von Steinmeyer, ed. (1912). *Die Matrikel der Universität Altdorf*. Vol. 1.
- Hans Rindlisbacher et al. (1951). *Die Matrikel der Universität Basel*. ger. Basel: Verl. der Universitätsbibliothek
- Alfred Schröder (1909). *Die Matrikel der Universität Dillingen*. ger. Archiv für die Geschichte des Hochstifts Augsburg Bd. 2-3. Dillingen a. D: Selbstverl. Alfr. Schröder

- Wilhelm Rotscheidt (1938). *Die Matrikel der Universität Duisburg, 1652 - 1818.* en.
- Hermann Mayer, ed. (1907). *Die Matrikel der Universität Freiburg im Breisgau von 1460 - 1656.* de. Vol. 1. Freiburg im Breisgau: Herder.
- Hermann Mayer (1910). *Die Matrikel der Universität Freiburg im Breisgau von 1460 - 1656.* de. Vol. 2. Freiburg im Breisgau: Herder.
- Paul Hintzelmann and Gustav Toepke (1884). *Die Matrikel der Universität Heidelberg.* ger. Heidelberg: C. Winter
- *Die Matrikel der Universität Helmstedt* (1926). Hildesheim: Komm. August Lax.
- Georg Mentz (1944). *Die Matrikel der Universität Jena 1. 1548 bis 1652.* ger. Veröffentlichungen der Thüringischen Historischen Kommission Bd. 1. Jena: G. Fischer
- Reinhold Jauernig and Marga Steiger (1961). *Die Matrikel der Universität Jena 2. 1652 bis 1723.* ger. Veröffentlichungen des Historischen Instituts der Friedrich-Schiller-Universität Jena. Weimar: H. Böhlau
- Otto Köhler (1969). *Die Matrikel der Universität Jena 3. 1723 bis 1764.* ger. Veröffentlichungen der Universitätsbibliothek Jena. Halle/Saale: M. Niemeyer
- Hermann Keussen, Ulrike Nyassi, and Manfred Groten (1892). *Die Matrikel der Universität Köln.* ger. Publikationen der Gesellschaft für Rheinische Geschichtskunde 8. Bonn: Behrendt
- Adolf Hofmeister (1889). *Die Matrikel der Universität Rostock.* ger. Rostock: Schwerin
- Heinrich Hermelink et al. (1906). *Die Matrikeln der Universität Tübingen.* ger. Verschiedene Aufl. Stuttgart: Kohlmmer
- Gregor Richter (1936). *Die Studentenmatrikel der Adolphsuniversität zu Fulda (1734 - 1805).* en.

Figure C.2: Digitization of university matriculation lists.

(a) Printed matriculation volume

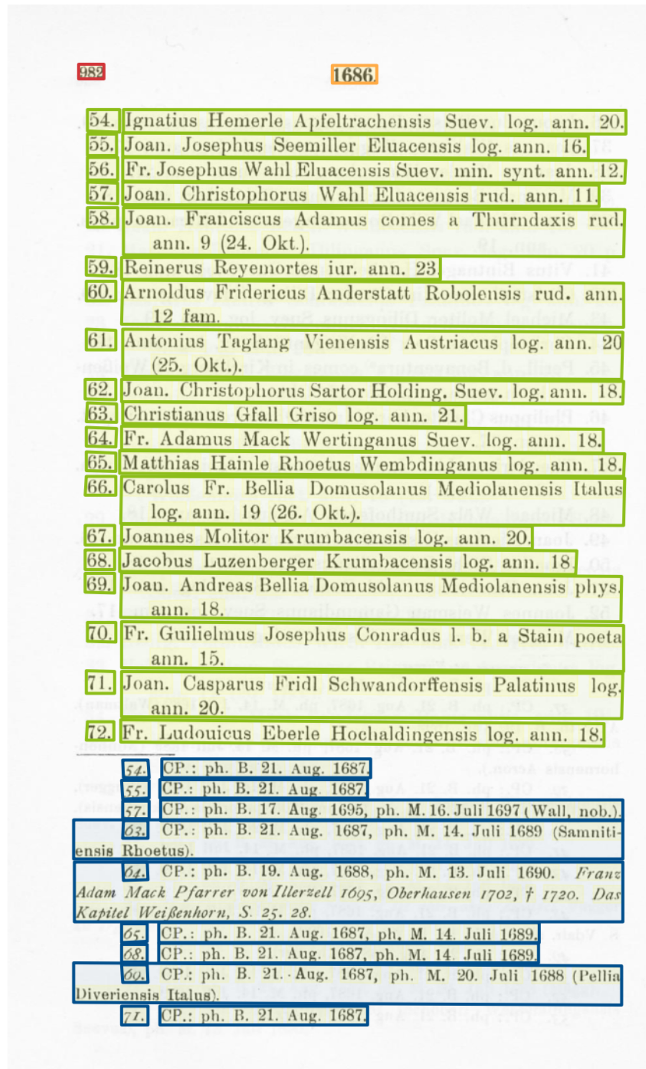


982 1686.

54. Ignatius Hemerle Apfeltrachensis Suev. log. ann. 20.
55. Joan. Josephus Seemiller Eluacensis log. ann. 16.
56. Fr. Josephus Wahl Eluacensis Suev. min. synt. ann. 12.
57. Joan. Christophorus Wahl Eluacensis rud. ann. 11.
58. Joan. Franciscus Adamus comes a Thurndaxis rud. ann. 9 (24. Okt.).
59. Reinerus Reyemortes iur. ann. 23.
60. Arnoldus Fridericus Anderstatt Robolensis rud. ann. 12 fam.
61. Antonius Taglang Vienensis Austriacus log. ann. 20 (25. Okt.).
62. Joan. Christophorus Sartor Holding. Suev. log. ann. 18.
63. Christianus Gfall Griso log. ann. 21.
64. Fr. Adamus Mack Wertinganus Suev. log. ann. 18.
65. Matthias Hainle Rhoetus Wembdinganus log. ann. 18.
66. Carolus Fr. Bellia Domusolanus Mediolanensis Italus log. ann. 19 (26. Okt.).
67. Joannes Molitor Krumbacensis log. ann. 20.
68. Jacobus Luzenberger Krumbacensis log. ann. 18.
69. Joan. Andreas Bellia Domusolanus Mediolanensis phys. ann. 18.
70. Fr. Guilielmus Josephus Conradus l. b. a Stain poeta ann. 15.
71. Joan. Casparus Fridl Schwandorffensis Palatinus log. ann. 20.
72. Fr. Ludouicus Eberle Hochaldingensis log. ann. 18.

54. CP.: ph. B. 21. Aug. 1687.
55. CP.: ph. B. 21. Aug. 1687.
57. CP.: ph. B. 17. Aug. 1695, ph. M. 16. Juli 1697 (Wall, nob.).
63. CP.: ph. B. 21. Aug. 1687, ph. M. 14. Juli 1689 (Samnitiensis Rhoetus).
64. CP.: ph. B. 19. Aug. 1688, ph. M. 13. Juli 1690. *Franz Adam Mack Pfarrer von Illerzell 1695, Oberhausen 1702, † 1720. Das Kapitel Weißenhorn, S. 25. 28.*
65. CP.: ph. B. 21. Aug. 1687, ph. M. 14. Juli 1689.
68. CP.: ph. B. 21. Aug. 1687, ph. M. 14. Juli 1689.
69. CP.: ph. B. 21. Aug. 1687, ph. M. 20. Juli 1688 (Pellia Diveriensis Italus).
71. CP.: ph. B. 21. Aug. 1687.

(b) Data extraction



982 1686.

54. Ignatius Hemerle Apfeltrachensis Suev. log. ann. 20.
55. Joan. Josephus Seemiller Eluacensis log. ann. 16.
56. Fr. Josephus Wahl Eluacensis Suev. min. synt. ann. 12.
57. Joan. Christophorus Wahl Eluacensis rud. ann. 11.
58. Joan. Franciscus Adamus comes a Thurndaxis rud. ann. 9 (24. Okt.).
59. Reinerus Reyemortes iur. ann. 23.
60. Arnoldus Fridericus Anderstatt Robolensis rud. ann. 12 fam.
61. Antonius Taglang Vienensis Austriacus log. ann. 20 (25. Okt.).
62. Joan. Christophorus Sartor Holding. Suev. log. ann. 18.
63. Christianus Gfall Griso log. ann. 21.
64. Fr. Adamus Mack Wertinganus Suev. log. ann. 18.
65. Matthias Hainle Rhoetus Wembdinganus log. ann. 18.
66. Carolus Fr. Bellia Domusolanus Mediolanensis Italus log. ann. 19 (26. Okt.).
67. Joannes Molitor Krumbacensis log. ann. 20.
68. Jacobus Luzenberger Krumbacensis log. ann. 18.
69. Joan. Andreas Bellia Domusolanus Mediolanensis phys. ann. 18.
70. Fr. Guilielmus Josephus Conradus l. b. a Stain poeta ann. 15.
71. Joan. Casparus Fridl Schwandorffensis Palatinus log. ann. 20.
72. Fr. Ludouicus Eberle Hochaldingensis log. ann. 18.

54. CP.: ph. B. 21. Aug. 1687.
55. CP.: ph. B. 21. Aug. 1687.
57. CP.: ph. B. 17. Aug. 1695, ph. M. 16. Juli 1697 (Wall, nob.).
63. CP.: ph. B. 21. Aug. 1687, ph. M. 14. Juli 1689 (Samnitiensis Rhoetus).
64. CP.: ph. B. 19. Aug. 1688, ph. M. 13. Juli 1690. *Franz Adam Mack Pfarrer von Illerzell 1695, Oberhausen 1702, † 1720. Das Kapitel Weißenhorn, S. 25. 28.*
65. CP.: ph. B. 21. Aug. 1687, ph. M. 14. Juli 1689.
68. CP.: ph. B. 21. Aug. 1687, ph. M. 14. Juli 1689.
69. CP.: ph. B. 21. Aug. 1687, ph. M. 20. Juli 1688 (Pellia Diveriensis Italus).
71. CP.: ph. B. 21. Aug. 1687.

Note: This figure illustrates the digitization process used to extract information from university matriculation lists. Panel A displays a sample page from an edited volume (Erler's Leipzig matriculation records). Panel B depicts the same page overlaid with the table recognition algorithm result used to extract each matriculation entry. Each entry is then parsed into name, place of origin, and subject of study using OCR and regular expressions.

C.2 Sample Coverage and Representativeness

A central concern for any historical dataset is whether the observed sample accurately reflects the underlying population. The combined baptism-matriculation-biography dataset covers a non-random subset of German towns and time periods, raising three questions: (1) What is the spatial and temporal coverage? (2) Do observed towns differ systematically from unobserved towns? (3) Do different data sources (elite vs. non-elite; male vs. female) produce consistent measures of religious identity?

Spatial and Temporal Coverage

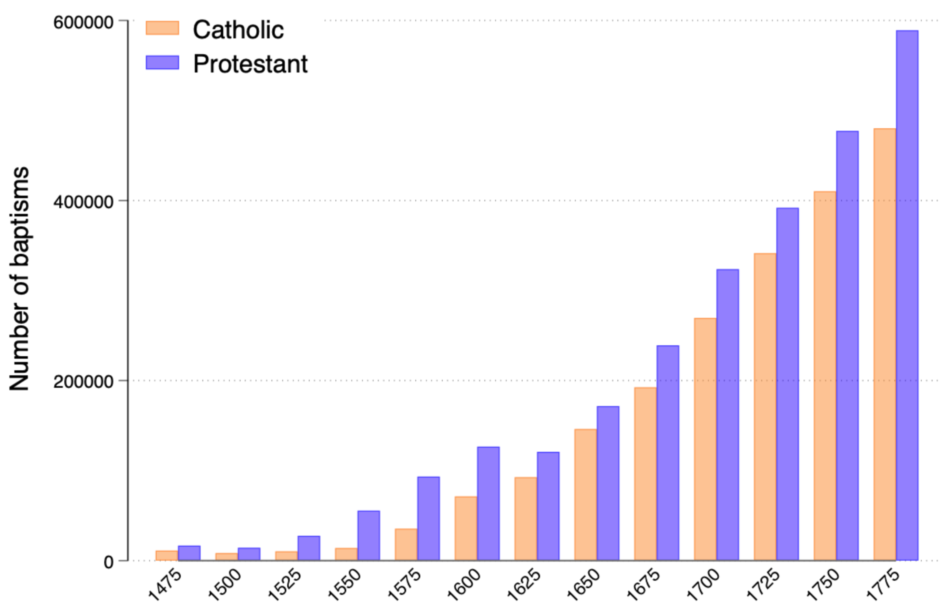
Appendix Figure C.3 maps the spatial coverage of the baptism data. Each town is shaded according to the number of decades in which it appears with at least 10 baptisms. Coverage is densest in southern and central Germany (Bavaria, Württemberg, Hesse, Saxony), reflecting both archival survival and the geographic distribution of towns. Northern and eastern regions are sparsely covered due to World War II destruction of church archives and lower urbanization rates.

Appendix Figure C.4 plots the number of baptisms observed by 25-year intervals, separately for Catholics (orange) and Protestants (blue). The sample grows sharply after 1550, reflecting the post-Reformation adoption of systematic record-keeping. Protestant coverage begins earlier, consistent with Lutheran church ordinances mandating baptism registration from the 1530s onward. Catholic coverage expands after the Council of Trent (1563), which formalized record-keeping requirements. The sample remains substantial through 1800.

Figure C.3: Spatial coverage of baptism sample.

Note: This map depicts the spatial coverage of the first name data used in the analysis. Each town is shaded according to the number of decades in which it is observed with at least 10 baptisms. Darker shading indicates more complete temporal coverage. The map includes 2,047 towns from the *Deutsches Städtebuch* with geocoded coordinates. Coverage is densest in southern and central Germany.

Figure C.4: Temporal coverage of baptism sample.



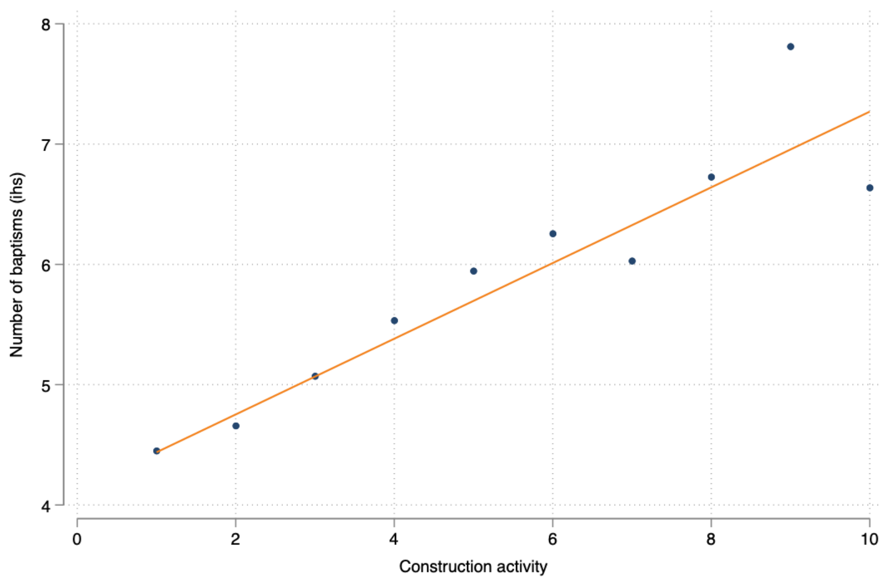
Note: This figure plots the number of baptisms observed in the dataset for Catholics (orange) and Protestants (blue) by 25-year intervals between 1480 and 1800. The sharp increase after 1550 reflects the post-Reformation adoption of systematic parish record-keeping, with Protestant territories typically implementing registration earlier than Catholic regions.

Representativeness Across Towns

Appendix Figure C.5 assesses whether baptism coverage correlates with urban growth. The figure plots the inverse hyperbolic sine of baptisms observed (y-axis) against the number of construction events in that city-decade (x-axis), a proxy for economic activity and population growth provided by Cantoni, Mohr, and Weigand (2020). The strong positive correlation indicates that observation intensity tracks town size, as expected—larger, more economically active towns have both better-preserved archives and more baptisms to record. This suggests the sample captures variation in urbanization, though it may under-represent small, economically stagnant villages.

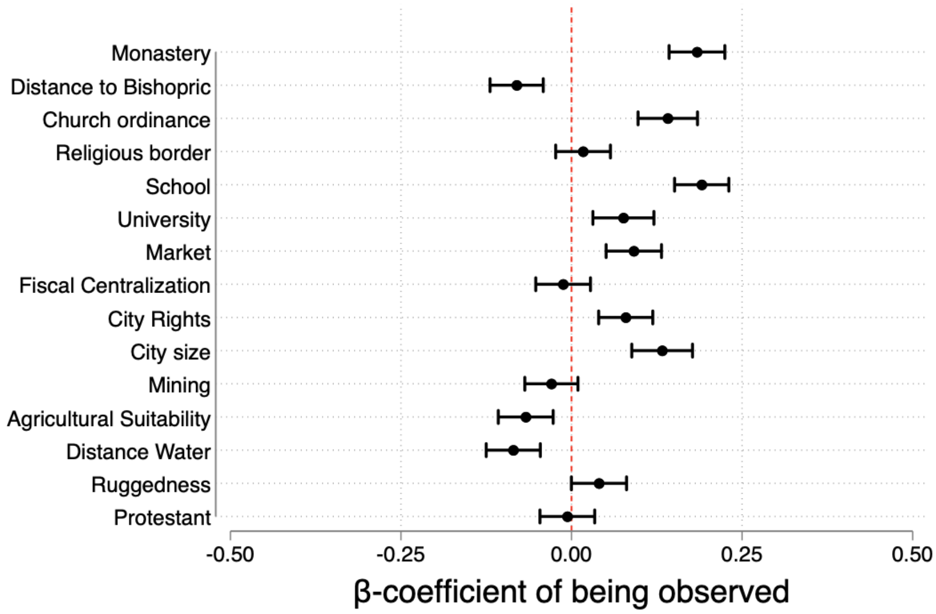
Appendix Figure C.6 tests whether observed and unobserved towns differ systematically on baseline (1500) observables. The figure plots coefficients from regressing an indicator for whether a town appears in the 1650 sample on geographic characteristics (distance to water, ruggedness, agricultural suitability), demographic characteristics (population, market rights, university presence), and an indicator for Protestant affiliation. Most geographic coefficients are small and statistically insignificant, suggesting that archival survival is not strongly selected on these dimensions. Factors that correlate with city size and professional administrative infrastructure such as monasteries, schools, universities, city rights positively predicts observation—university towns have better-preserved records.

Figure C.5: Baptism coverage and urban economic activity.



Note: This figure shows a binscatter plot of the inverse hyperbolic sine of the number of baptisms observed in a town-decade (y-axis) against the number of construction events recorded in that town-decade (x-axis). Construction events—drawn from historical building inventories—serve as a proxy for urban economic activity and population growth. The positive correlation indicates that baptism coverage is higher in economically dynamic towns.

Figure C.6: Baseline characteristics of observed versus unobserved towns.



Note: This figure plots coefficients from regressing an indicator for whether the town appears in the 1650 sample on baseline (1500) geographic and demographic town characteristics. Error bars show 95% confidence intervals. Towns with universities and Protestant towns are slightly more likely to be observed, consistent with better archival preservation and earlier adoption of systematic record-keeping in Protestant territories. Other characteristics are not strongly predictive of sample inclusion.

Representativeness Across Data Sources

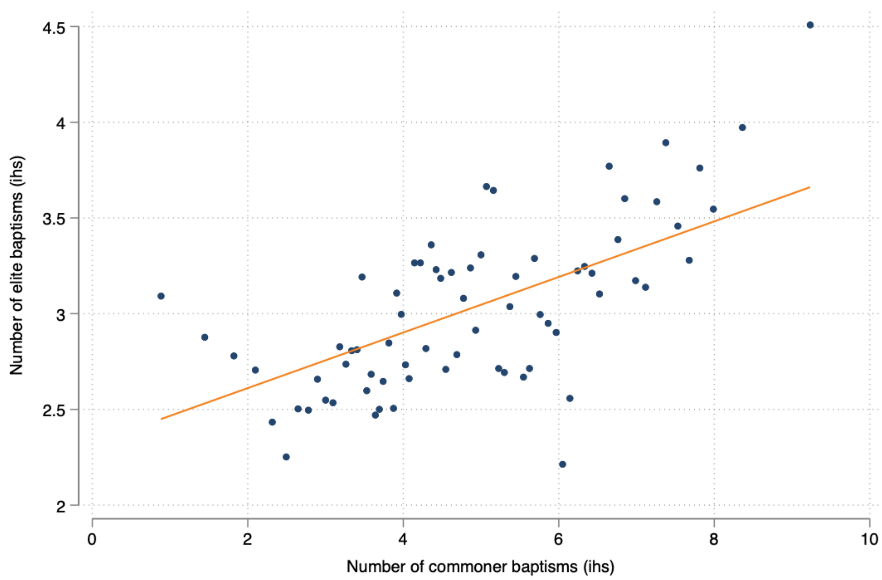
The dataset combines observations from different sources (parish registers, university matriculation lists, biographical databases) and social strata (elite students vs. general population). If measurement of religious identity differs systematically across sources, the Protestant name score might reflect sample composition rather than underlying identity patterns.

Appendix Figure C.7 plots the correlation between elite-source observations (matriculation lists and biographies) and general baptism records within town-decades. The strong positive correlation indicates that towns with many elite observations also have many baptism records, suggesting these sources are complementary rather than substitutes. Elite sources dominate the pre-1550 sample, while baptism records dominate post-1550.

Appendix Figure C.8 tests whether the Protestant name score is consistent across elite and non-elite sources. The figure plots average name scores for elite observations (y-axis) against scores for baptism-record observations (x-axis) within town-decades where both are available. The slope is close to one and the correlation is 0.87, indicating that elite and non-elite naming patterns move together. This alleviates concerns that the measure reflects social stratification rather than confessional identity.

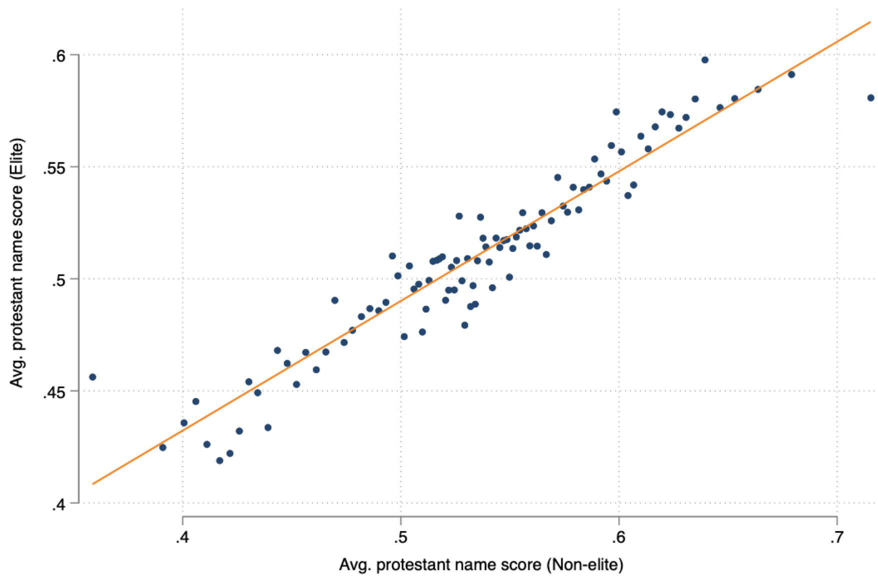
Appendix Figure C.9 assesses whether male and female naming patterns produce consistent measures of religious identity. The figure plots average Protestant name scores for females (y-axis) against scores for males (x-axis) within town-decades. The correlation is 0.92, indicating that male and female names polarize in parallel. This validates the decision to pool genders in the main analysis while computing separate name scores by sex.

Figure C.7: Correlation between elite and general population observations.



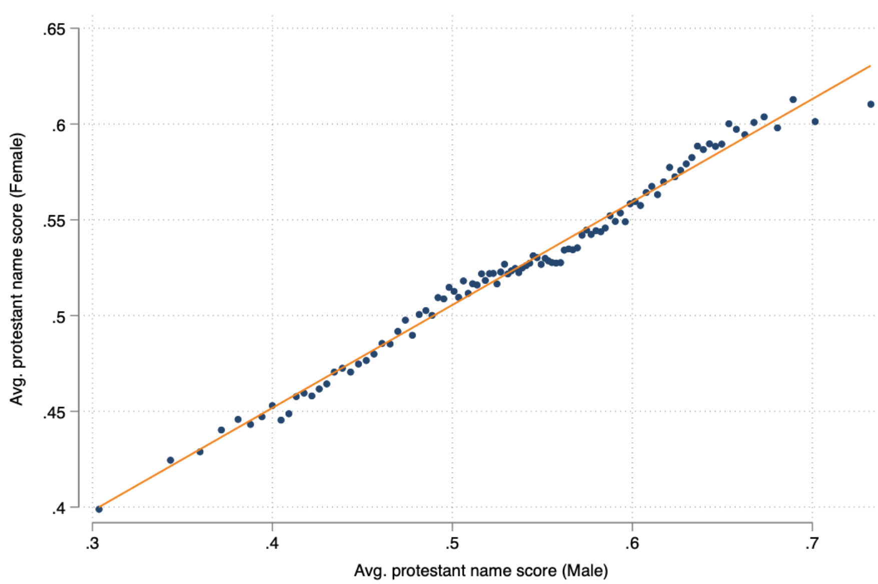
Note: This figure shows a binscatter plot of the inverse hyperbolic sine of the number of general baptism records observed in a town-decade (y-axis) against the inverse hyperbolic sine of the number of elite-source observations (university matriculations and biographies) in that town-decade (x-axis). The positive correlation indicates that these sources are complementary: towns with many elite observations also tend to have many baptism records.

Figure C.8: Consistency of name scores: elite versus non-elite sources.



Note: This figure shows a binscatter plot of average Protestant name score among elite-source observations (y-axis) against the average Protestant name score among baptism-record observations (x-axis) in town-decades where both sources are available. The slope near one and correlation of 0.87 indicate that elite and non-elite naming patterns produce consistent measures of confessional identity.

Figure C.9: Consistency of name scores: male versus female.



Note: This figure shows a binscatter plot of average Protestant name score among female observations (y-axis) against the average Protestant name score among male observations (x-axis) within town-decades. The correlation of 0.92 indicates that male and female naming patterns polarize in parallel, validating the pooling of genders in the main analysis.

C.3 Data Processing and Harmonization

After digitization, I harmonize the raw data through several processing steps to create an analysis-ready dataset linking individuals to towns, confessions, and standardized first names.

Step 1: Deduplication and cleaning. I drop entries with missing birth years and remove duplicate records both within and across sources. Duplicates are identified using combinations of name, birth year, and birthplace, with manual review of ambiguous cases.

Step 2: Geocoding and confession assignment. I precisely geolocate all parishes and birthplaces using historical GIS data and assign each to the nearest town in the *Deutsches Städtebuch*. For observations where the parish is known, I use the confession of the parish in which the baptism occurred to determine the individual's confession, this is in particular relevant for the few bi-confessional towns (most importantly Augsburg) where the town's confessional status is not a one-to-one mapping to the parish confession. For cases where the parish is unknown, in particular for the matriculation records, I assign confession based on the town of origin's eventual confessional status.

Step 3: Name standardization. I standardize first-name spelling variants to ensure comparability over time and across regions (e.g., Johann → Johannes, Josef → Joseph). To focus on commonly used names, I exclude names that occur fewer than 50 times over the entire sample period. This threshold balances the desire for comprehensive coverage against the need to avoid noisy estimates from extremely rare names.

Step 4: Name classification. I classify names into conceptually relevant categories for heterogeneity analysis. I construct a set of religious names based on (i) a list of biblical names from the Old and New Testament, (ii) names of saints with major churches in Germany (Buringh et al. 2020), and (iii) names containing the element "Gott" (e.g., Gottfried, Gottlieb). I also classify Germanic revival names using etymological information in Behind the Name (2025), identifying names with pre-Christian Germanic roots that were revived during the early modern period.

The result is a harmonized, geolocated, confession-linked dataset of 4.9 million first names by town and year, which serves as the basis for computing the Protestant name score and aggregate partisanship measures.

C.4 Computation of Name-Based Partisanship

After harmonizing and geolocating names, I compute a measure of the "Protestant-ness" of each name following the logic of Fryer and Levitt (2004). The procedure proceeds in several steps.

Cell Definition

I define a *cell* as the combination of decade, sex, and—where relevant—geographic grouping (cluster, region, or city). For each cell, I tabulate the number of Catholic and Protestant births for each first name, keeping only names with at least 20 occurrences within the cell to ensure stable probabilities.

Protestant name score

For each name–cell combination, I compute the posterior probability that a newborn with name n is Protestant:

$$\text{ProtScore}_{n,c} = \frac{\Pr(\text{name} = n \mid \text{Prot}, c)}{\Pr(\text{name} = n \mid \text{Prot}, c) + \Pr(\text{name} = n \mid \text{Cath}, c)}. \tag{28}$$

This score equals 0.5 if a name is used equally often by both confessions, 1 if used exclusively by Protestants, and 0 if used exclusively by Catholics. Intuitively, it is the Bayesian posterior probability of Protestant affiliation given the observed name, under a neutral prior.

Temporal Aggregation and Smoothing

I compute scores at multiple levels of temporal granularity:

- Decade-level scores, pooling across the entire sample
- Decade $\times$ linguistic cluster scores (High, Middle, Low German), then take simple averages across clusters to measure the ”typical” Protestantness of a name
- Rolling 20-year windows, both within clusters and pooled across clusters, to smooth short-term fluctuations

Random Benchmark

To gauge how much separation could arise by chance, I recompute $\text{ProtScore}_{n,c}$ after randomly permuting confessional labels (preserving the overall Protestant share within each cell) and average over 100 iterations. This yields a null distribution of name scores that serves as a finite-sample benchmark.

Capped Implementation

To prevent towns with very large numbers of observations from dominating the results, I implement a capped version of the procedure: in each city–decade, I randomly draw at most 100 births, repeat the sampling 100 times, and report the mean $\text{ProtScore}_{n,c}$ with its 5th–95th percentile range as a precision measure. I also compute a capped random benchmark using the same procedure with shuffled labels.

Average Partisanship

Finally, I aggregate the name-level scores to construct the main measure of confessional differentiation in naming practices over time. For each individual observation i in decade t with true confessional label $y_i \in \{0, 1\}$, I compute the posterior probability of correct classification:

$$\text{CorrectPost}_i = y_i \, \text{ProtScore}_{name_i,t} + (1 - y_i)(1 - \text{ProtScore}_{name_i,t}). \tag{29}$$

The decade-level measure is then

$$\text{Average Partisanship}_t^{\text{name}} = \frac{1}{N_t} \sum_{i \in t} \text{CorrectPost}_i, \tag{30}$$

the mean posterior probability that a neutral observer would correctly infer confession after observing a randomly drawn name in decade t. By construction, Average Partisanship$_t^{\text{name}} = 0.5$ indicates that names carry no information about confession, while values approaching 1 indicate near-complete segregation of naming practices. I compute 95% confidence intervals using the empirical distribution of Average Partisanship$_t^{\text{name}}$ from the capped resampling procedure. For the random benchmark, I replace ProtScore$_{name_i,t}$ with the score from permuted labels to obtain a null distribution for comparison.

Regional and Multi-Group Extensions

As a robustness and validation exercise, I generalize the procedure to other group partitions beyond the Catholic–Protestant divide—e.g., linguistic clusters or historical regions. For a given partition, I compute for each name its within-group usage probability relative to the outside-group probability, using the same posterior-probability formula as above. This allows me to validate that the measure recovers historically documented regional or linguistic naming patterns and is not driven by a single group boundary.

C.5 Addressing Sparsity

A core challenge in comparing naming distributions across time and space is that observed names represent only a finite sample from the underlying cultural choice set available to parents. The number of children born in a town–decade is limited, while the potential choice set of names is large. This raises two concerns: (i) sampling noise may produce spurious differences in the observed distribution of names across confessions, and (ii) name-based measures of partisanship may mechanically vary with the number of births rather than reflecting true cultural divergence.

I address these concerns in several ways. First, I require that each name–cell combination be observed at least 20 times to enter the analysis, reducing the influence of extremely rare names. Second, I implement a capped sampling procedure that limits the number of births per city–decade to 100. This ensures that large urban centers with complete parish registers do not dominate the estimated distribution. I repeat the capped sampling 100 times and report the mean and 5th–95th percentile range of the estimated Protestant-name score to account for resampling variability.

Third, I benchmark my results against a random-assignment null distribution. I randomly permute confessional labels within each decade 100 times and recompute the Protestant-name score, thereby capturing the amount of "partisanship" that would arise from chance alone. These placebo series are flat and close to 0.5 (random guessing) for all periods, suggesting that finite sample bias does not drive the observed patterns.

Finally, as a complementary measure of distributional separation, I follow Cantoni, Mohr, and Weigand (2025) and compute the normalized Theil index, a special case of the generalized entropy index frequently used to measure segregation. Let c_{ng} be the number of children given name n in group

$g \in \{\text{Catholic}, \text{Protestant}\}$ in a given decade, and let $p_{ng} = c_{ng}/c$ be the joint probability of observing name n in group g, where c is the total number of baptisms in that decade. The mutual information between names and confessions is

$$M = \sum_{n=1}^{N} \sum_{g=1}^{G} p_{ng} \log \frac{p_{ng}}{p_{n\cdot} p_{\cdot g}}, \tag{31}$$

where $p_{n\cdot}$ and $p_{\cdot g}$ denote marginal probabilities of names and groups. Normalizing by the entropy of the group distribution yields the Theil index

$$T = \frac{M}{-\sum_{g=1}^{G} p_{\cdot g} \log p_{\cdot g}}, \tag{32}$$

which ranges from 0 (identical name distributions across confessions) to 1 (completely disjoint sets of names). In robustness checks, I show that the time path of the Theil index closely mirrors the Protestant-name score, confirming that my results are not an artifact of sparsity or model specification.

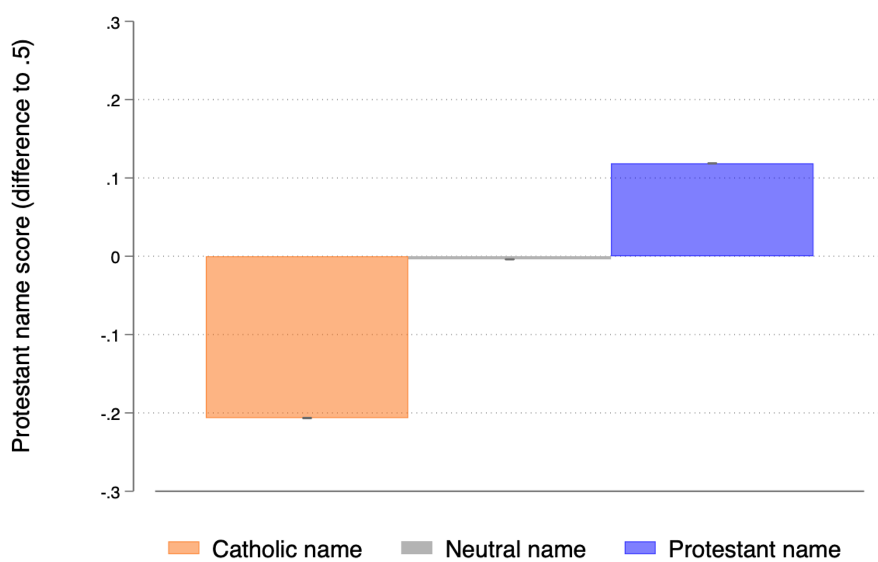
C.6 Validation

To validate the Protestant-name score as a meaningful measure of religious identity, I conduct a series of exercises that examine its predictive power, persistence, and internal consistency.

Comparison with Historical Evidence

First, I benchmark the measure against historical qualitative evidence. Appendix Figure C.10 compares the data-driven Protestant name score to a list of typical Catholic and Protestant names compiled by historian François (1991) in his study of Augsburg. François identified these names through extensive archival research on confessional conflict in the bi-confessional city. Reassuringly, names François classified as typically Catholic have very low Protestant name scores (averaging 0.2), while names he classified as typically Protestant have high scores (averaging 0.8). Names not specifically mentioned by François cluster near 0.5. This correspondence validates that the quantitative measure captures the same confessional associations documented in detailed historical case studies.

Figure C.10: Validation against historical evidence.



Note: This figure shows average Protestant name score deviation from 0.5 for names classified by François (1991) as typical Catholic (orange), typical Protestant (blue), or not specifically mentioned (gray) in his study of confessional identity in Augsburg. Catholic names have scores well below 0.5; Protestant names have scores well above 0.5, validating the measure against historical evidence.

Internal Consistency Across Constructions

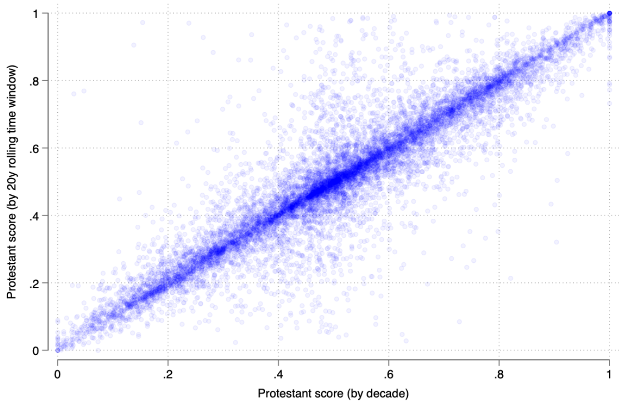
Second, I assess the internal consistency of the measure across alternative constructions. Appendix Figure C.11 plots correlations between the decade-level Protestant name score and three alternatives: the 20-year rolling window score (Panel A), the cluster average score (Panel B), the regional name score (Panel C), and the cluster-specific name score (Panel D).

Panels A and B show high correlations between the baseline decade-level measure and temporally or geographically smoothed variants. This indicates that the measure is not sensitive to the specific aggregation window or geographic unit.

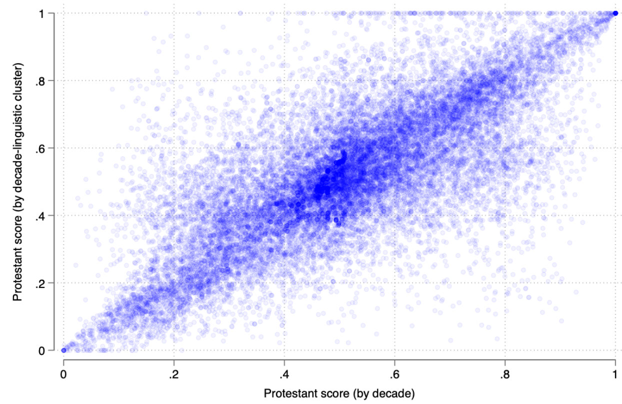
Panels C and D show low correlations between the confessional Protestant name score and purely geographic scores (computed by treating regions or linguistic clusters as ”groups” rather than confessions). This demonstrates that confessional differentiation in naming is distinct from geographic differentiation. The Protestant name score does not merely reflect naming differences between northern (Protestant) and southern (Catholic) Germany, but captures a confessional dimension orthogonal to geography.

Figure C.11: Internal consistency across alternative score constructions.

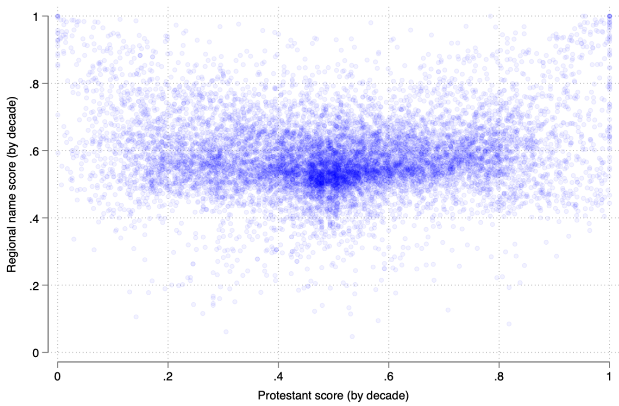
(a) Decade vs. rolling window (20 years)



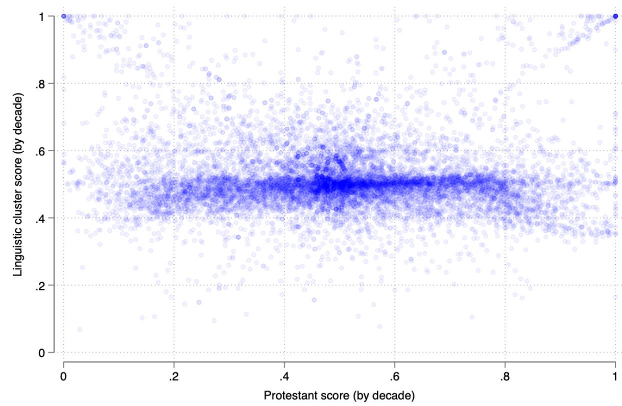
(b) Decade vs. cluster average



(c) Confessional vs. regional score



(d) Confessional vs. cluster score



Note: This figure plots correlations between alternative constructions of name-based identity scores. Panels A and B show high correlations between the baseline decade-level Protestant name score and temporally/geographically smoothed variants, indicating robustness to aggregation choices. Panels C and D show low correlations between confessional scores and purely geographic scores (treating regions or linguistic clusters as groups), demonstrating that confessional differentiation is distinct from geographic variation.

C.7 Examples and Robustness

To illustrate the workings of the Protestant name score and build intuition for the results, I show the evolution of individual name choices over time. Appendix Figures C.12 and C.13 display choice probabilities of selected male and female names separately for Catholics and Protestants between 1450 and 1850. These examples make clear that certain names are strongly associated with one confession and display sharp shifts in relative popularity following the Reformation. For instance, names such as *Joseph* and *Maria* remain predominantly Catholic, while *Christian* and *Friedrich* become more common among Protestants. This provides a transparent illustration of the confessional content captured by the measure. Appendix Tables C.1 and C.1 complement these examples by systematically reporting the most partisan names by decade, ranked according to their Protestant name score.

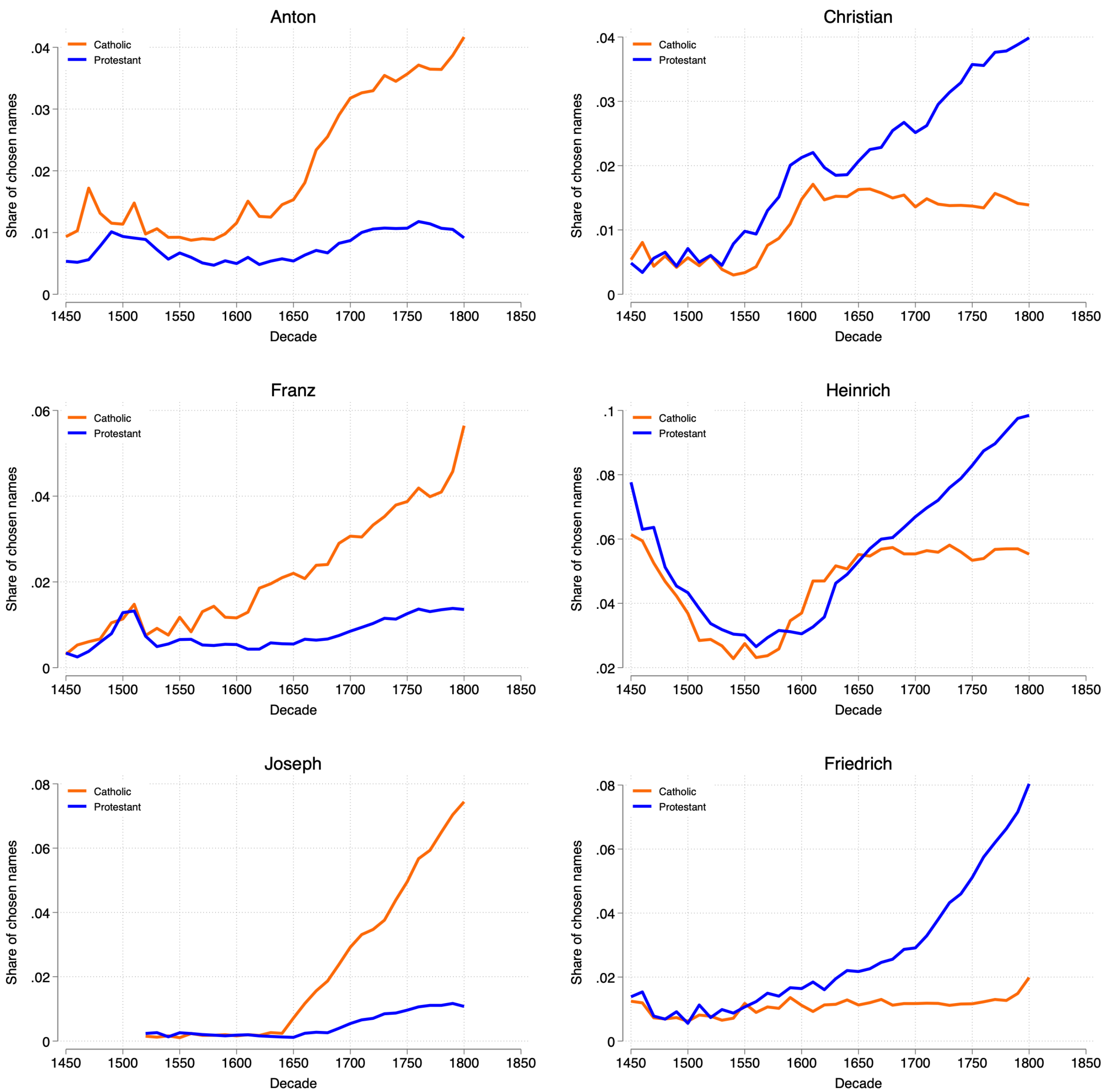
I next assess the robustness of the aggregate partisanship patterns to alternative sample restrictions and constructions of the measure. Appendix Figure C.15 documents the evolution of naming polarization using the Theil index following Cantoni, Mohr, and Weigand (2025). The trajectory closely mirrors the main results, with rapid increase in segregation from 1580 to 1610 and remaining elevated thereafter, consistent with persistent confessional differentiation.

Appendix Figures C.16 and C.17 show that the patterns hold when restricting the sample to male and female names separately. I further explore heterogeneity by social status: Appendix Figure C.18 focuses on elites (university students and notable individuals), whereas Appendix Figure C.19 considers non-elites (baptism records). Both groups exhibit similar dynamics. This is particularly important because the sample for early periods (pre-1550) primarily relies on university matriculation lists, while systematic baptism records are only introduced in the aftermath of the Reformation in most places with different timing in adoption of reliable record keeping.

Finally, Appendix Figure C.20 zooms in on Augsburg, a bi-confessional city, which allows me to study differentiation in a geographically contained unit that rules out any concern of differentiation between regions driving the results. Reassuringly, even in this micro setting the differentiation pattern follows the general trends observed in the full sample of German towns.

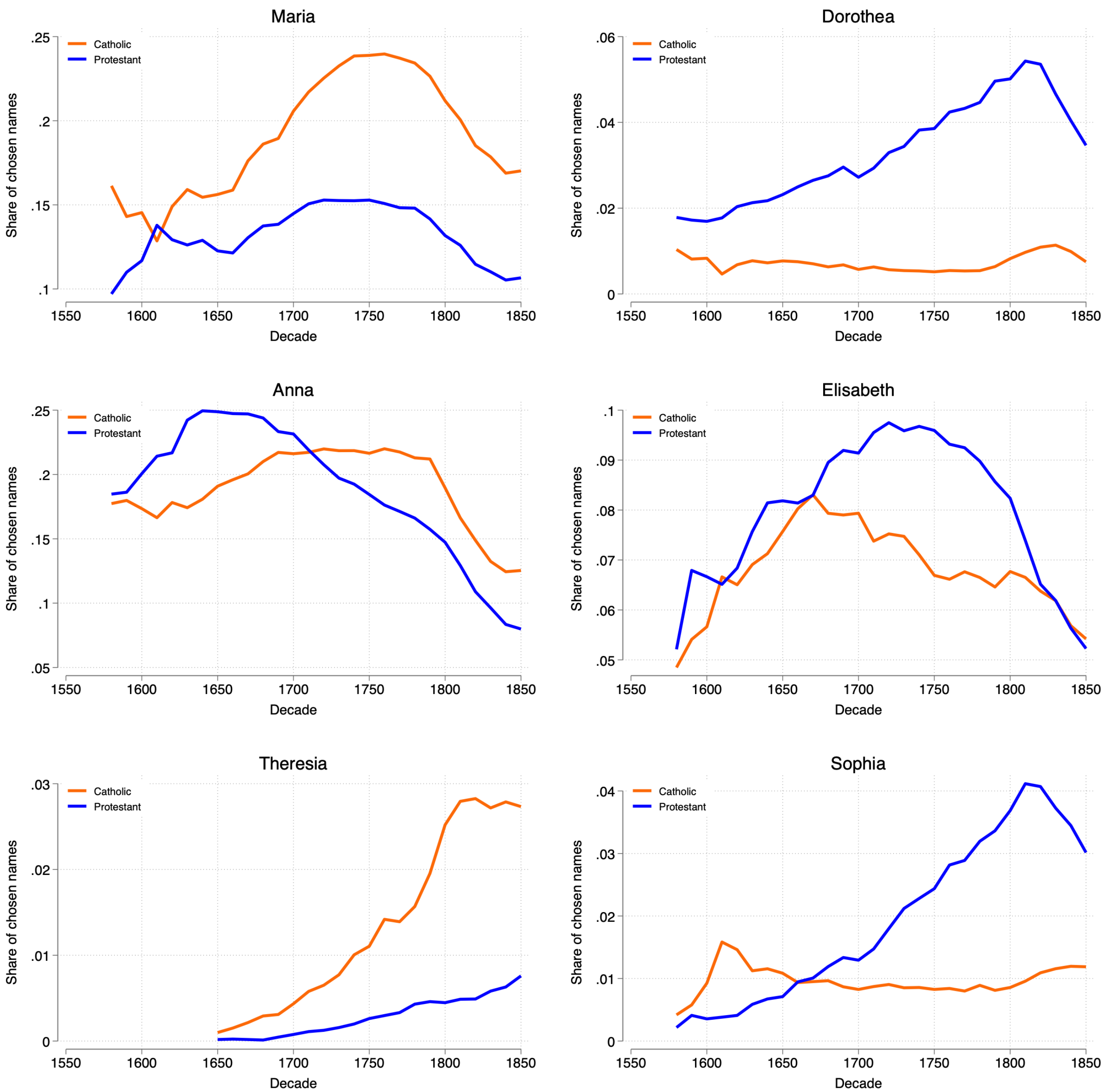
Appendix Figure C.14 displays the distribution of Protestant name scores by confession for four selected decades (1510, 1560, 1610, 1760). In 1510, before the Reformation, both Catholic and Protestant distributions are centered near 0.5, indicating minimal differentiation. By 1560, the distributions have begun to separate, with Protestant names clustering above 0.5 and Catholic names below. By 1760, the separation is pronounced, with distinct modes for each confession. The vertical dashed lines mark mean scores by confession, illustrating the widening gap over time.

Figure C.12: Choice probabilities of selected male names by confession over time.



Note: This plot shows choice probabilities by religious groups for selected male names between 1450 and 1850. The orange line shows the share of Catholics given name X in decade t. The blue line shows the share of Protestants given name X in decade t. Details on the construction of the data are given in Appendix Section C.1.

Figure C.13: Choice probabilities of selected female names by confession over time.



Note: This plot shows choice probabilities by religious groups for selected female names between 1550 and 1850. The orange line shows the share of Catholics given name X in decade t. The blue line shows the share of Protestants given name X in decade t. Details on the construction of the data are given in Appendix Section C.1.

Table C.1: Panel A. Most partisan male names by decade.

Catholic	Pr(C)	Pr(P)	Protscore	Protestant	Pr(C)	Pr(P)	Protscore
			1550s				
Sebastian	1.6	0.8	.32	Joachim	0.5	2.3	.80
Jacob	4.5	3.7	.45	Andreas	1.9	3.3	.63
Conrad	1.6	1.4	.46	Martin	1.8	2.6	.58
Johannes	17.8	15.5	.46	Hans	1.9	2.6	.57
Georg	6.9	6.1	.47	Peter	1.8	2.2	.55
			1650s				
Franz	2.2	0.6	.20	Hans	1.5	7.3	.83
Anton	1.5	0.5	.26	Christoph	1.5	3.2	.68
Matthias	3.4	1.3	.28	Friedrich	1.1	2.1	.66
Wilhelm	2.9	1.3	.29	Andreas	1.4	2.5	.64
Peter	4.8	2.3	.32	Conrad	1.2	1.9	.61
			1750s				
Joseph	4.9	0.9	.16	Friedrich	1.1	5.1	.81
Matthias	3.9	0.9	.19	Christoph	0.8	2.8	.78
Anton	3.6	1.1	.23	Christian	1.3	3.5	.72
Franz	3.9	1.3	.25	Conrad	0.7	1.7	.68
Peter	6.4	2.4	.28	Georg	2.2	4.4	.67

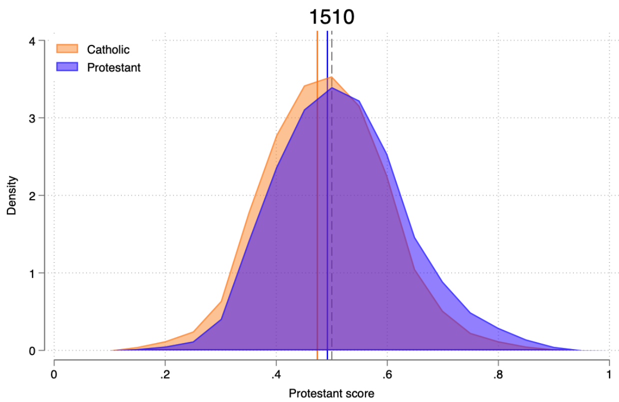
Table C.1: Panel B. Most partisan female names by decade.

Catholic	Pr(C)	Pr(P)	Protscore	Protestant	Pr(C)	Pr(P)	Protscore
			1550s				
Rosina	1.6	0.6	.28	Catharina	5.8	10.7	.65
Regina	2.0	0.9	.30	Dorothea	1.0	1.7	.64
Susanna	2.0	1.0	.34	Margaretha	7.2	12.3	.63
Gertrud	1.6	0.8	.34	Magdalena	2.6	4.2	.62
Maria	10.7	7.3	.40	Ursula	2.6	4.0	.61
			1650s				
Sybilla	1.6	0.3	.16	Dorothea	0.7	2.3	.75
Gertrud	5.3	1.9	.27	Barbara	2.8	4.9	.63
Agnes	2.1	1.1	.35	Anna	19.1	24.9	.57
Christina	2.8	2.1	.43	Magdalena	2.1	2.5	.55
Maria	15.6	12.2	.44	Margaretha	9.1	10.1	.53
			1750s				
Gertrud	4.6	1.6	.25	Dorothea	0.5	3.9	.88
Maria	23.8	15.3	.39	Sophia	0.8	2.4	.75
Anna	21.6	18.4	.46	Elisabeth	6.6	9.6	.59
Magdalena	2.1	2.2	.51	Christina	2.2	3.1	.58
Catharina	10.9	11.6	.51	Margaretha	6.3	8.4	.57

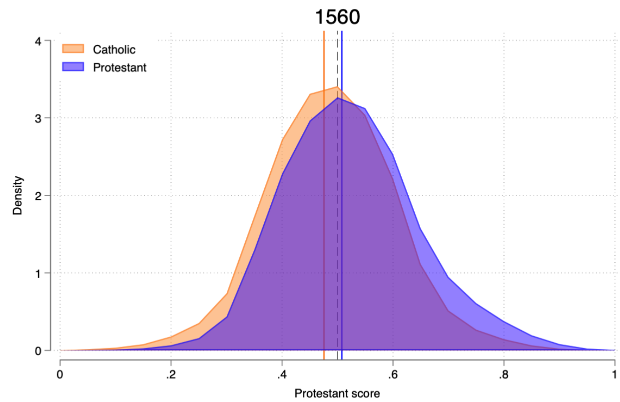
Notes: The table reports the top 5 most partisan names by selected decades for Catholics and Protestants. Panel A reports male names. Panel B reports female names.

Figure C.14: Name score distribution.

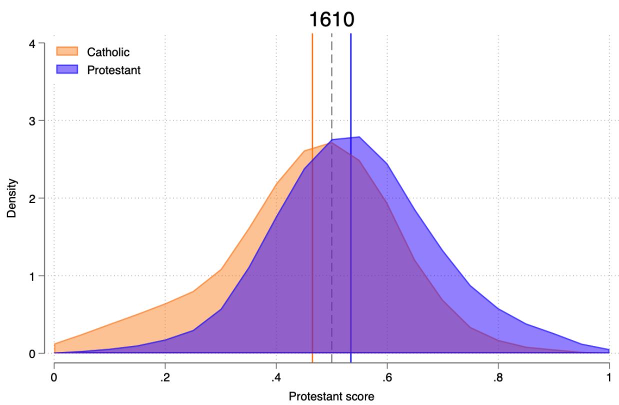
(a) 1510



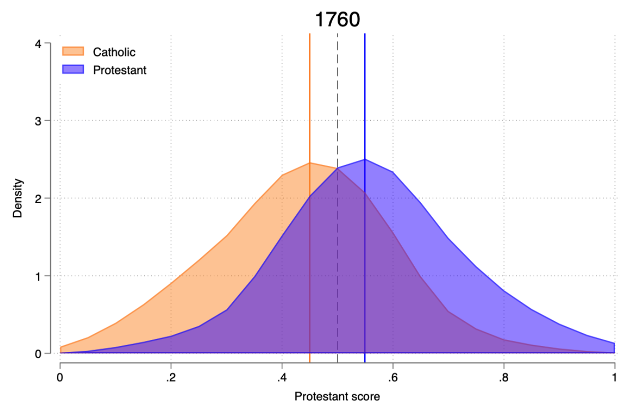
(b) 1560



(c) 1610

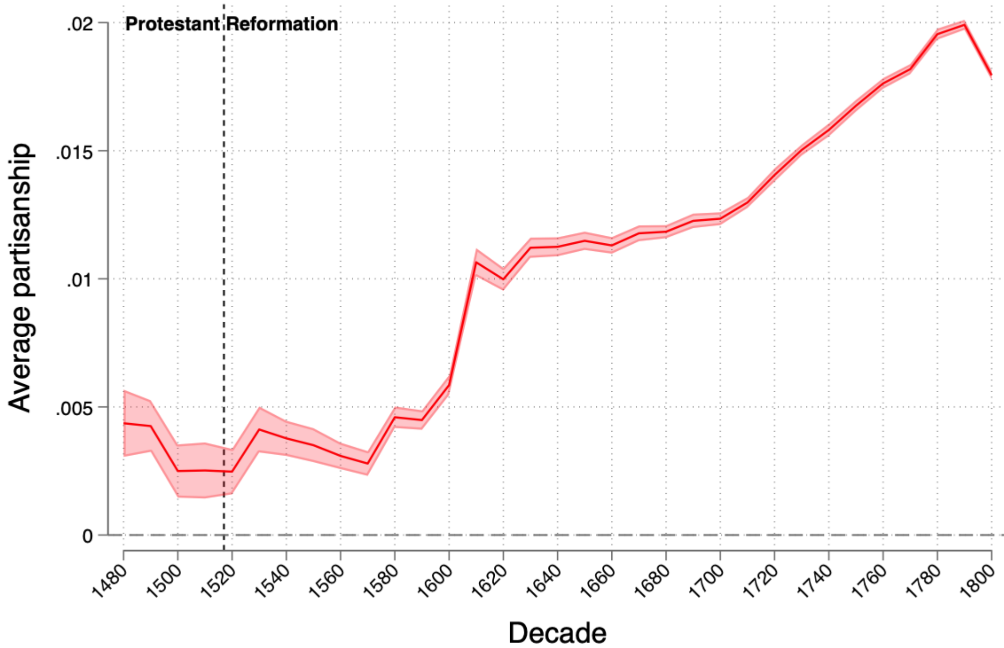


(d) 1760



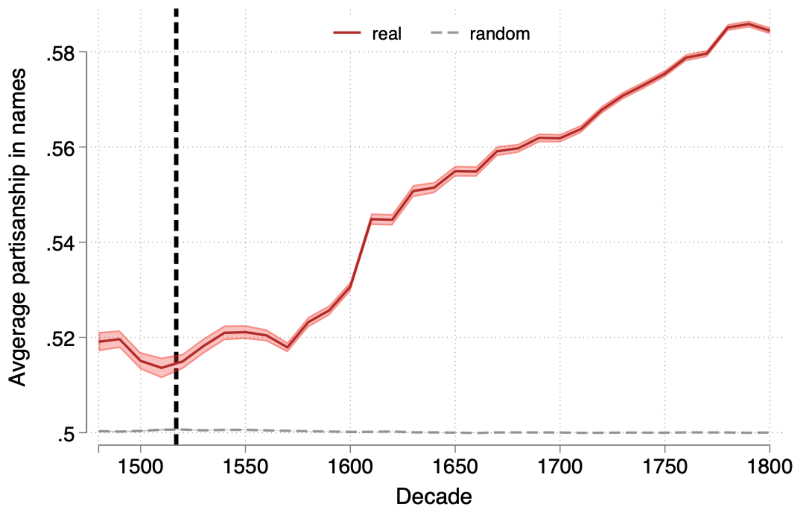
Note: The figure plots the distribution of the Protestant name score by group for four selected decades. The orange kernel density plot depicts the distribution of Protestant name scores of all names chosen by Catholics in the indicated decade. The blue kernel density plot depicts the distribution of Protestant name scores of all names chosen by Protestants in the indicated decade. The vertical dashed lines indicate the average name score for Catholics (orange) and Protestants (blue) respectively.

Figure C.15: Average partisanship in naming over time (Theil Index).



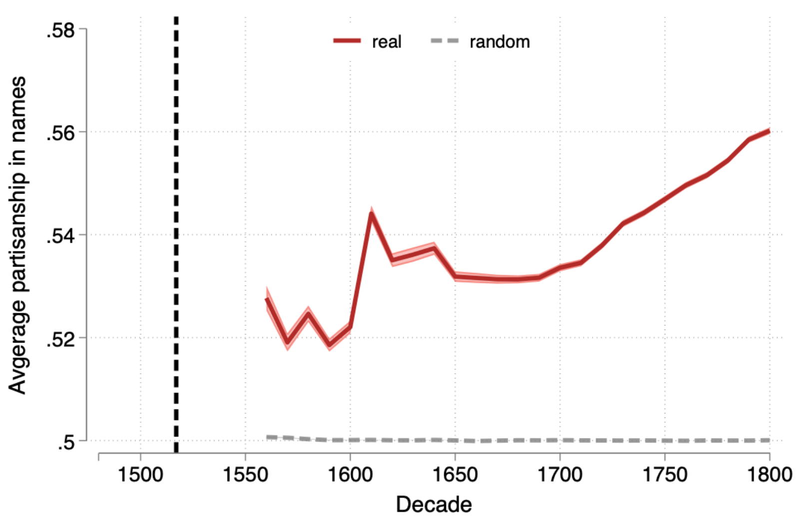
Note: This plot shows average partisanship measured by the Theil Index as described in Section C.5. The Theil index ranges from 0 (identical name distributions across confessions) to 1 (completely disjoint naming patterns). The trajectory closely mirrors the Protestant name score results, peaking in the mid-16th century and remaining elevated thereafter.

Figure C.16: Average partisanship in naming over time (Male only).



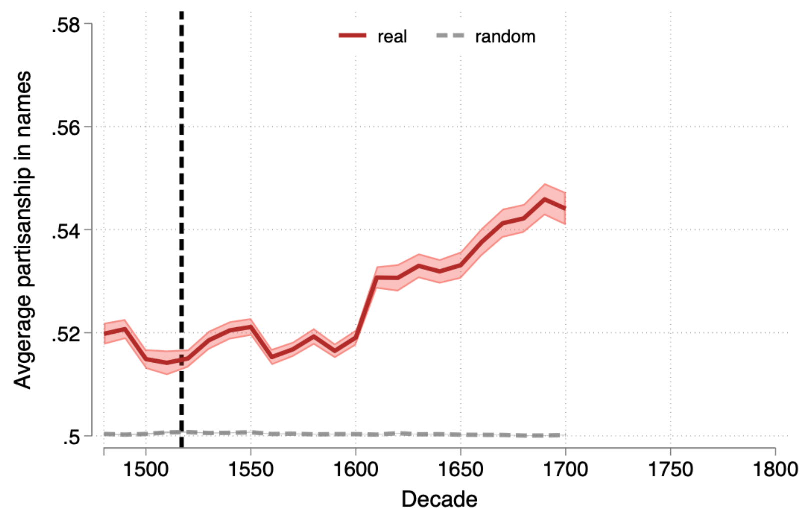
Note: This figure plots average name-based partisanship, Average Partisanship$_t^{\text{name}}$, by decade from 1480 to 1800 using male first names only, as defined in Equation 2. The solid red line reports the posterior probability that a neutral observer correctly infers confession from a randomly drawn male name, with 95% confidence intervals from the capped resampling distribution. The dashed gray line shows the average over 500 iterations with randomly reshuffled labels, providing a finite-sample benchmark. Names are classified using the parish's eventual confessional status. See Appendix Section C for details on data construction and robustness.

Figure C.17: Average partisanship in naming over time (Female only).



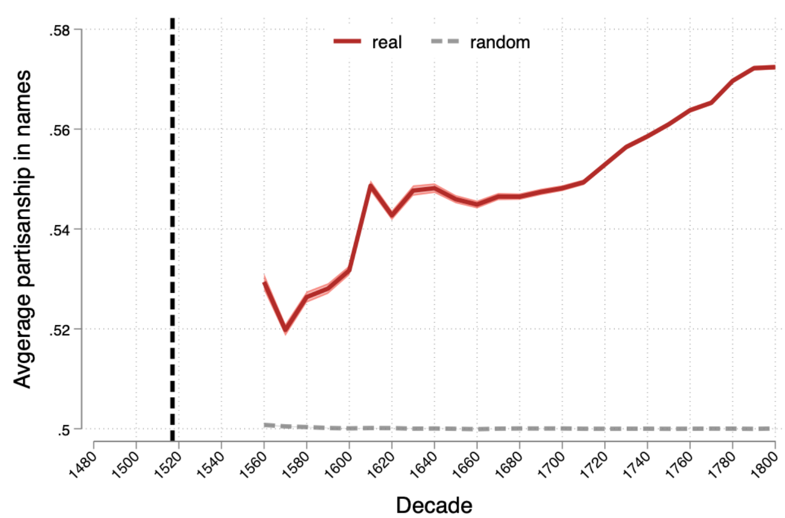
Note: This figure plots average name-based partisanship, Average Partisanship$_t^{\text{name}}$, by decade from 1480 to 1800 using female first names only, as defined in Equation 2. The solid red line reports the posterior probability that a neutral observer correctly infers confession from a randomly drawn female name, with 95% confidence intervals from the capped resampling distribution. The dashed gray line shows the average over 500 iterations with randomly reshuffled labels, providing a finite-sample benchmark. Names are classified using the parish's eventual confessional status. See Appendix Section C for details on data construction and robustness.

Figure C.18: Average partisanship in naming over time (Elites only).



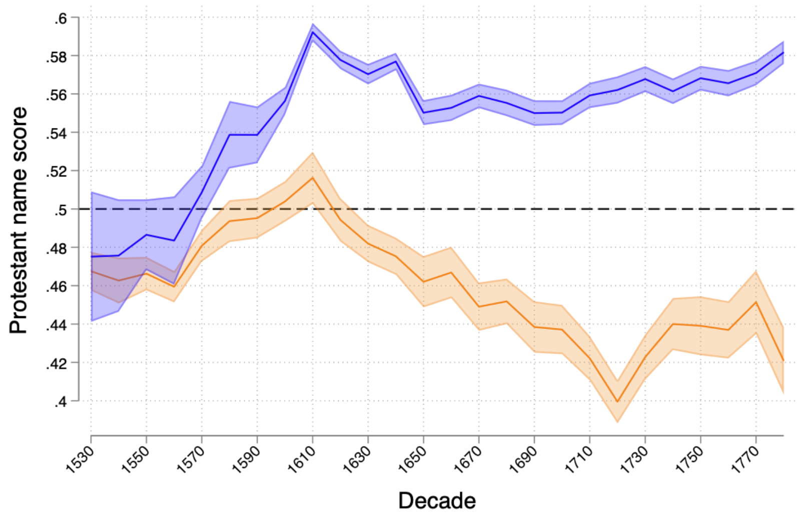
Note: This figure plots average name-based partisanship, Average Partisanship$_t^{\text{name}}$, by decade from 1480 to 1700 using elite (students and notable individuals) first names only, as defined in Equation 2. The solid red line reports the posterior probability that a neutral observer correctly infers confession from a randomly drawn elite name, with 95% confidence intervals from the capped resampling distribution. The dashed gray line shows the average over 500 iterations with randomly reshuffled labels, providing a finite-sample benchmark. Names are classified using the parish's eventual confessional status. See Appendix Section C for details on data construction and robustness.

Figure C.19: Average partisanship in naming over time (Non-elites only).



Note: This figure plots average name-based partisanship, Average Partisanship$_t^{\text{name}}$, by decade from 1480 to 1800 using non-elite (no students or notable individuals) first names only, as defined in Equation 2. The solid red line reports the posterior probability that a neutral observer correctly infers confession from a randomly drawn non-elite name, with 95% confidence intervals from the capped resampling distribution. The dashed gray line shows the average over 500 iterations with randomly reshuffled labels, providing a finite-sample benchmark. Names are classified using the parish's eventual confessional status. See Appendix Section C for details on data construction and robustness.

Figure C.20: Partisanship in naming over time (Augsburg).



Note: This figure plots the average Protestant name score by confession in the bi-confessional city of Augsburg over time. The blue line and corresponding 95% confidence intervals depict the average score of Protestants baptized in Protestant parishes. The orange line shows the average score of Catholics baptized in Catholic parishes. Even within a single city, naming patterns diverge sharply along confessional lines, ruling out purely geographic explanations for the observed polarization.

D Short Titles

This section documents the construction and analysis of the printing dataset used to measure confessional differentiation in published texts. The dataset comprises 91,788 short titles from the *Universal Short Title Catalogue* (USTC), covering printed works produced in German towns between 1480 and 1700. I first describe the multi-stage data processing pipeline that transforms raw historical titles into machine-readable text suitable for quantitative analysis (Section D.1). I then detail the computation of document-level and aggregate partisanship measures using both embedding-based and bigram-based classification approaches (Section D.2). Finally, I present robustness checks and additional analyses examining the sensitivity of results to alternative specifications and exploring thematic patterns in confessional language (Section D.3).

D.1 Data Processing

Before computing partisanship measures, I implement a comprehensive seven-stage data processing pipeline to ensure the resulting text corpus is both historically faithful and suitable for machine learning methods. Each stage addresses specific challenges posed by early modern texts: non-standard spelling, Latin and old German orthography, extensive publication metadata, and reprints.

Stage 1: Translation

Short titles in the USTC appear in old German, Latin, and occasionally other languages, with non-standardized spelling reflecting pre-orthographic conventions. To enable consistent analysis, I translate all titles into modern German and English using OpenAI's `gpt-4o-mini` model with zero temperature (ensuring deterministic outputs). The translation prompt explicitly instructs the model to preserve historical content while modernizing language:

> `You are an expert on Early Modern German history and handwritings. Translate the following text into modern [German/English]. Output only a valid JSON object with a single key 'translation' that contains the translated text. Do not include any additional text or explanation.`

Responses are parsed as JSON, with failed calls retried up to five times using exponential backoff. I process translations in parallel batches of 1,000 titles using a multithreaded executor, completing the full corpus in approximately 6 hours.

Stage 2: Metadata Removal

Historical short titles contain extensive publication metadata (author names, university affiliations, publication places, dedicatory formulas, disputation details) that would dominate any text analysis but do not reflect the substantive content of works. To isolate semantic content, I submit each translated title to OpenAI with a structured cleaning prompt:

```
Clean the following historical German title by removing all formal or technical
publication metadata. This includes names of authors, supervisors, presenters;
dates and places of publication or presentation; university names; edition details;
references like 'Seite 368', 'Anmerkung 347', or '5. Kapitel des Matthäus'; and
any formal dedications or disputation formulas. Keep the remaining wording in
German as close to the original as possible. Do not paraphrase or summarize. Output
only a valid JSON object with a single key 'cleaned' that contains the cleaned
German title as a string.
```

This process yields four parallel versions of each title: (i) original text, (ii) modern German translation, (iii) modern English translation, and (iv) cleaned German translation with metadata removed.

Stage 3: Lemmatization and Linguistic Processing

I lemmatize all four text versions using `spaCy`'s large German (`de_core_news_lg`) and English (`en_core_web_lg`) language models. Lemmatization reduces inflected forms to dictionary headwords (e.g., German *glaubten* → *glauben*; English *believing* → *believe*), improving comparability across texts.

I then apply extensive filtering to remove noise:

- **Stopword removal**: Eliminate function words (articles, prepositions, pronouns) using extended stopword lists
- **Named entity filtering**: Remove person names, places, and organizations identified by `spaCy`'s named entity recognizer
- **Custom stemming**: Merge near-duplicate lemmas sharing roots (e.g., *evangelisch*, *evangelium* → *evangel*)
- **Generic term removal**: Drop academic (*disputation*, *professor*, *dissertation*) and geographic terms (*Leipzig*, *Wittenberg*, *Ingolstadt*) that dominate frequency distributions but carry no confessional signal

The output consists of tokenized, lowercased, lemmatized texts ready for quantitative analysis.

Stage 4: Embedding Construction

I create dense vector representations of titles using OpenAI's `text-embedding-3-small` model, which produces 1,536-dimensional embeddings. These embeddings capture semantic content in a continuous vector space, enabling machine learning classification. I generate three parallel embedding sets:

1. **Raw translation embeddings**: Based on full translated titles including metadata
2. **Cleaned semantic embeddings**: Based on metadata-removed titles
3. **TF-IDF filtered embeddings**: Based on texts after stopword and rare-term removal

Embeddings are computed in parallel batches and serialized in a single `.pkl` file for efficient access. The main analysis uses cleaned semantic embeddings to balance content preservation with noise reduction.

Stage 5: Deduplication

Historical printing includes many reprints and near-duplicate editions (e.g., second edition with minor revisions, reprints in different cities). To prevent overweighting popular works, I perform near-duplicate detection:

- Compute cosine similarity between all title pairs sharing the same author
- Group titles with similarity > 0.95 into connected components using graph clustering
- Within each group, retain only the earliest publication (by year)

This removes approximately 37,942 reprints and near-duplicates, reducing the final corpus to 53,846 unique substantive titles.

Stage 6: Topic Modeling

Finally, I enrich the dataset with topic assignments using BERTopic (**grootendorst˙bertopic˙2022**), a transformer-based topic modeling approach. I fit the model on cleaned semantic embeddings using a UMAP–HDBSCAN pipeline to identify latent thematic clusters. The algorithm initially identifies 90+ topics, which I reduce to 30 clusters through hierarchical clustering of related topics.

Topic assignments enable heterogeneity analysis (do results differ by subject matter?) and serve as control variables in robustness checks.

Stage 7: TF-IDF Filtering

To focus on semantically informative words for the bigram-based approach, I apply term frequency–inverse document frequency (TF-IDF) filtering. This retains words that are common within individual documents but not ubiquitous across the corpus—precisely the terms most likely to differentiate Catholic from Protestant texts.

Filtering criteria:

- Minimum document frequency: 5 (excludes extremely rare words likely to be OCR errors or idiosyncrasies)
- Maximum document frequency: 90% (excludes filler terms appearing in nearly all documents)
- Within-document TF-IDF threshold: 0.05 (retains only terms with sufficient weight)
- Inverse document frequency ceiling: 8 (excludes terms too rare to be informative)

D.2 Computation of Partisanship Measures

With the cleaned and deduplicated corpus prepared as described in Section D.1, I compute measures of confessional partisanship at both the document and decade level. I employ two complementary approaches: (1) an embedding-based logistic regression classifier that captures semantic content in high-dimensional vector space, and (2) a bigram-based classifier following Gentzkow, Shapiro, and Taddy (2019) that provides interpretable linguistic features. Both approaches yield consistent results, validating that the measured differentiation reflects genuine confessional content rather than methodological artifacts.

Document-Level Predicted Probabilities

For each decade t, I estimate the probability that each title originates from a Protestant town using the following procedure:

Step 1: Balanced sampling. Draw a balanced sample of Catholic and Protestant titles (downsampling the majority group if necessary to achieve 50-50 balance). This prevents the classifier from simply learning that one confession produced more titles.

Step 2: Classifier training. Fit an ℓ_2-penalized (ridge) logistic regression using the 1,536-dimensional embedding vectors as features. The ridge penalty ($\lambda = 1.0$) prevents overfitting in the high-dimensional feature space. The model estimates:

$$\Pr(y_i = \text{Protestant} \mid \mathbf{x}_i) = \frac{1}{1 + \exp(-\mathbf{x}_i' \boldsymbol{\beta})}, \tag{33}$$

where $\mathbf{x}_i$ is the embedding vector for title i and $\boldsymbol{\beta}$ is the coefficient vector.

Step 3: Prediction. For each title i in decade t, compute the predicted probability $\widehat{p}_i^{(r)}$ in repetition r.

Step 4: Bootstrap aggregation. Repeat steps 1-3 for $n = 500$ bootstrap iterations, each time resampling the training data. Compute the mean predicted probability:

$$\widehat{p}_i = \frac{1}{n} \sum_{r=1}^{n} \widehat{p}_i^{(r)}. \tag{34}$$

The distribution across repetitions provides bootstrap-based standard errors and 95% confidence intervals.

Step 5: Placebo benchmark. To assess whether observed differentiation could arise from chance alone, repeat steps 1-4 with randomly permuted Protestant/Catholic labels (preserving the within-decade confession share). This generates a null distribution of predicted probabilities under the hypothesis of no confessional differentiation.

The resulting document-level dataset contains, for each title: mean predicted probability $\widehat{p}_i$, standard error, confidence interval, and placebo prediction. Appendix Table D.1 reports examples of highly predictive titles by decade.

Decade-Level Average Partisanship

I aggregate document-level predictions to construct the main time-series measure of confessional differentiation. For each title i with true confessional label $y_i \in \{0, 1\}$ (where 1 = Protestant), compute the posterior probability of correct classification:

$$\text{CorrectPost}_i = y_i \widehat{p}_i + (1 - y_i)(1 - \widehat{p}_i). \tag{35}$$

This equals $\widehat{p}_i$ if the title is Protestant, and $1 - \widehat{p}_i$ if Catholic—i.e., the probability that an observer who sees only the title text would correctly infer its confessional origin.

The decade-level measure is:

$$\text{Average Partisanship}_t^{\text{text}} = \frac{1}{N_t} \sum_{i \in t} \text{CorrectPost}_i, \tag{36}$$

the mean probability of correct classification across all titles in decade t. By construction:

- Average Partisanship$_t^{\text{text}} = 0.5$ indicates no information: titles are equally likely Catholic or Protestant regardless of content
- Average Partisanship$_t^{\text{text}} \to 1$ indicates nearly perfect separation: titles' confessional origin is evident from text alone

I compute 95% confidence intervals from the bootstrap distribution of $\widehat{p}_i$ across 500 repetitions. For the placebo benchmark, I replace $\widehat{p}_i$ with predictions from permuted labels to obtain a null distribution. Appendix Figure D.3 plots the resulting time series with confidence intervals and placebo bands.

Out-of-Sample Performance (AUC)

As a complementary measure robust to class imbalance, I compute the area under the receiver operating characteristic curve (AUC). In each of the 500 bootstrap repetitions:

1. Randomly split decade t's titles into 80% training and 20% hold-out test sets
2. Train the classifier on the training set
3. Evaluate performance on the held-out test set
4. Compute the AUC: the probability that a randomly chosen Protestant title receives a higher predicted probability than a randomly chosen Catholic title

I report the mean AUC across 500 repetitions, along with bootstrap confidence intervals and the placebo distribution under permuted labels. The AUC provides a fully out-of-sample measure of predictive power, ensuring results are not driven by overfitting. Values near 0.5 indicate random guessing; values near 1.0 indicate perfect discrimination.

Bigram-Based Classification

For interpretability, I replicate the analysis using bigram frequencies as features instead of embeddings. This approach sacrifices some predictive power (bigrams cannot capture long-range semantic relationships) but provides transparent linguistic markers of confessional affiliation.

For each decade, I:

1. Construct a document-term matrix of bigram frequencies (e.g., "heilig geist," "göttlich gesetz")
2. Retain bigrams appearing in at least 5 documents (reducing noise from rare terms)

3. Fit ℓ_2-penalized logistic regressions on balanced samples

4. Compute predicted probabilities and aggregate to average partisanship using the same formula

Additionally, I record the mean coefficient $\bar{\beta}_{\text{bigram}}$ for each bigram across 500 bootstrap iterations and rank bigrams by absolute coefficient magnitude. Appendix Table D.2 lists the ten most predictive bigrams for Protestant and Catholic affiliation by decade, providing a transparent view of the linguistic markers driving differentiation. For example, in the 1560s, top Protestant bigrams include ”evangelisch kirch” and ”göttlich wort,” while top Catholic bigrams include ”römisch kirch” and ”heilig sakrament.”

D.3 Robustness and Thematic Analysis

This section documents robustness checks and additional analyses that complement the main results on partisanship in printing presented in Section 4. I first assess sensitivity to methodological choices (text representation, out-of-sample validation, finite-sample bias). I then explore thematic patterns in confessional language using moral foundations dictionaries.

Robustness to Text Representation

A key concern is whether results depend on the specific text representation (embeddings vs. bigrams). If so, findings might reflect artifacts of the embedding model rather than genuine confessional differentiation. Appendix Figure D.2 reports average partisanship using bigram-based ℓ_2-penalized logistic regression instead of embeddings. Panel A shows results for all titles; Panels B and C split by religious versus non-religious subjects.

The bigram-based series closely mirrors the embedding-based measure (Figure D.1), with a sharp rise in differentiation after the 1520s and persistently elevated levels thereafter. The correlation between decade-level measures is 0.94, indicating that both approaches capture the same underlying pattern. This robustness across feature representations confirms that observed differentiation reflects genuine confessional content rather than methodological choices.

Most Predictive Titles and Bigrams

To verify that classifiers capture meaningful theological content rather than noise, Appendix Tables D.1 and D.2 list the most predictive titles and bigrams by decade.

Appendix Table D.1 shows that top Protestant titles in the 1560s include Luther’s writings, Reformed catechisms, and polemics against ”papist” errors, while top Catholic titles include Counter-Reformation treatises, defenses of the Mass, and Jesuit educational works. These examples confirm that the classifier identifies genuine confessional markers.

Appendix Table D.2 reveals that top Protestant bigrams emphasize ”göttlich wort” (divine word), ”evangelisch lehr” (evangelical teaching), and ”recht glaub” (true faith), while top Catholic bigrams emphasize ”heilig sakrament” (holy sacrament), ”römisch kirch” (Roman church), and ”alt glaub” (old faith). These linguistic patterns align with known theological differences: Protestant emphasis on scripture and doctrine versus Catholic emphasis on sacraments and church authority.

Out-of-Sample Validation (AUC)

To ensure results are not driven by overfitting, I re-estimate decade-level partisanship using held-out test sets and report the area under the ROC curve (AUC). Appendix Figure D.3 plots AUC by decade for all titles (Panel A), religious titles (Panel B), and non-religious titles (Panel C).

The temporal pattern remains qualitatively unchanged: AUC rises sharply from 0.5 (random guessing) in 1500-1520 to 0.75-0.80 in 1560-1600, then remains elevated through 1700. This out-of-sample validation confirms that classifiers genuinely learn confessional patterns rather than memorizing training data.

Finite-Sample Benchmark

A concern with any classification exercise is whether observed differentiation could arise from chance alone given finite sample sizes. To assess this, I benchmark results against a random-label placebo. In each bootstrap iteration, I randomly permute Protestant/Catholic labels within each decade and re-estimate the classifier. The resulting placebo distribution captures the amount of "partisanship" that would arise purely from sampling variability.

The gray series in Appendix Figures D.2 and D.3 plot the mean placebo measure with 95% confidence intervals. The observed series lies well outside the placebo distribution in all decades after 1520, with the gap widening through 1560 and remaining large thereafter. This indicates that the measured increase in average partisanship is not driven by finite-sample bias (Gentzkow, Shapiro, and Taddy 2019).

Thematic Patterns: Universalism vs. Communitarianism

Finally, I explore what moral and ideological themes associate with confessional differentiation. Following Figueroa and Fouka (2023), I use the Moral Foundations Dictionary to compute the relative frequency of universalist (emphasizing shared humanity, individual rights, abstract principles) and communitarian (emphasizing group identity, tradition, collective obligations) terms in short titles. While applying modern moral dictionaries to early modern texts is necessarily anachronistic, this exercise provides suggestive evidence on thematic patterns.

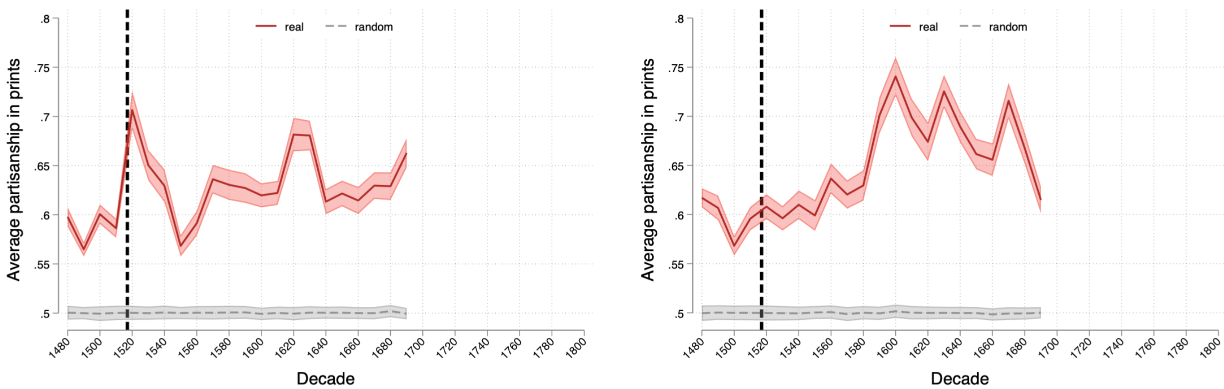
Appendix Figure D.4 plots mean universalist scores (Panel A), communitarian scores (Panel B), and net universalism (Panel C) by 25-year intervals for Catholic and Protestant prints. Panel B reveals a sustained increase in communitarian language in Protestant prints from 1550 onward, especially after the start of the Thirty Years' War (1618). Catholic prints maintain relatively stable communitarian scores. Panel C shows that Protestant prints exhibit declining net universalism, driven primarily by rising communitarianism rather than falling universalism.

To formalize this pattern, Appendix Table D.3 regresses the predicted Protestant score of each title on its universalism score (universalist minus communitarian term frequency). Columns differ by included fixed effects (decade, linguistic region, subject, topic, author). Across all specifications, universalism negatively predicts Protestant classification: titles emphasizing universalist themes are less likely to be classified as Protestant, even controlling for subject matter, author, and time period.

This pattern supports the interpretation that confessional differentiation reflects substantive ideological divergence: Protestant texts increasingly emphasize communal identity, group boundaries, and collective obligations, while Catholic texts maintain more universalist framing.

Figure D.1: Partisanship in short titles over time.

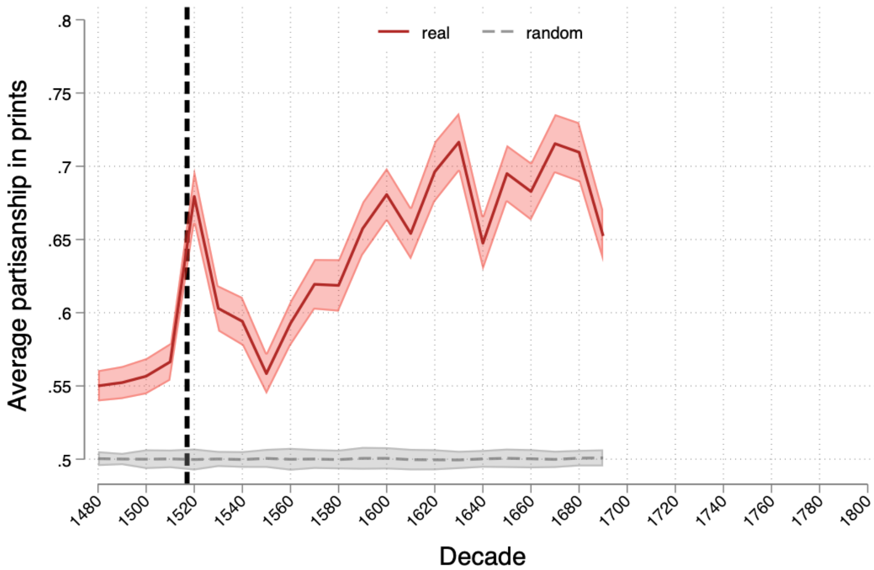
(a) Religious prints **(b)** Other prints



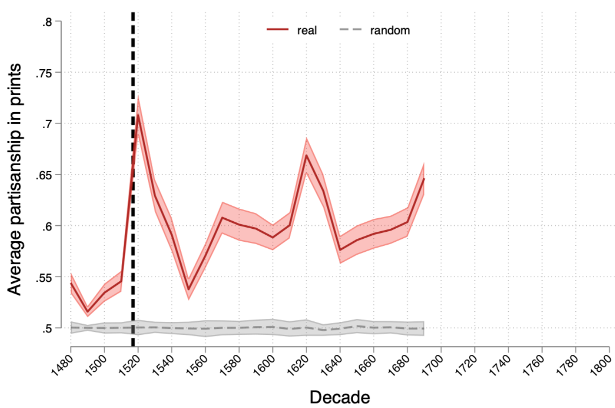
Note: This figure plots average partisanship in print Average Partisanship$_t^{\text{text}}$ by decade, as defined in Equation 3. The red line shows estimates based on true Protestant and Catholic labels, with 95% confidence intervals from 500 bootstrap iterations. The gray line shows the distribution under randomly reshuffled labels (500 iterations), providing a reference for finite-sample informativeness. Partisanship scores are estimated using a penalized logistic classifier on short-title embeddings. Panel A restricts to religious subjects; Panel B to all other subjects. Short titles are from the *Universal Short Title Catalogue* (*Universal Short Title Catalogue* 2025) and cover the period 1480–1700. Details on data construction are provided in Appendix Sections D.1–D.2.

Figure D.2: Partisanship in short titles over time (bigram-based approach).

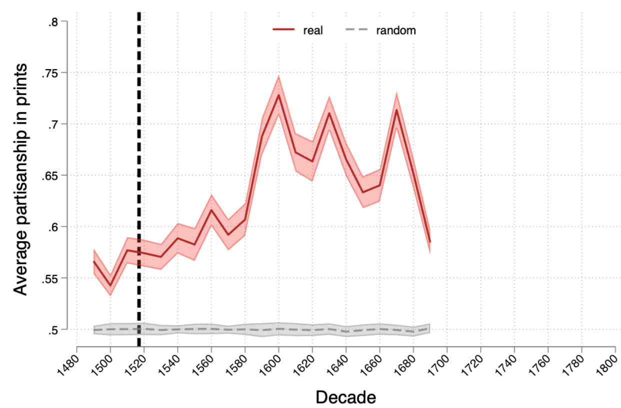
(a) All prints



(b) Religious prints



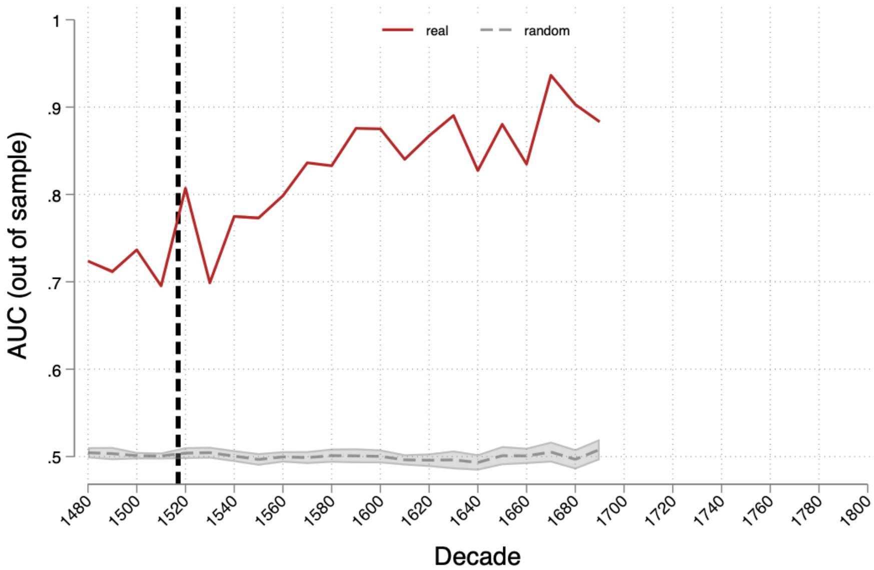
(c) Other prints



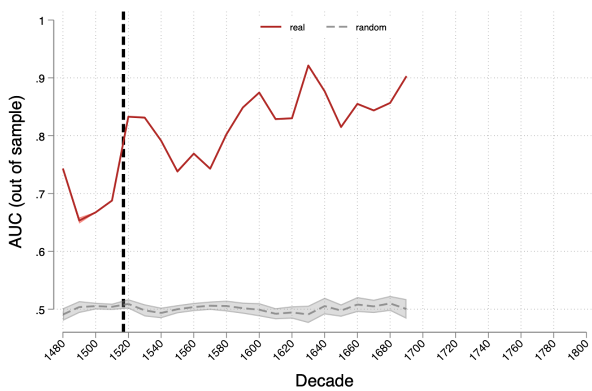
Note: This figure plots average partisanship of short titles Average Partisanship$_t^{\text{text}}$ by decade using bigram frequencies as features. The red line shows estimates from 500 iterations of an ℓ_2-penalized logistic regression classifier, with 95% confidence intervals. The dashed gray line shows the corresponding placebo distribution based on randomly permuted labels. Panel A reports results for all short titles; Panel B restricts to titles with subject "Religious"; Panel C to all other subjects. The temporal pattern closely mirrors the embedding-based results (Figure D.1), with correlation 0.94 between decade-level measures, confirming robustness across text representations. Short titles are from the *Universal Short Title Catalogue* (2025) and cover 1480–1700.

Figure D.3: Partisanship in short titles over time (out-of-sample AUC).

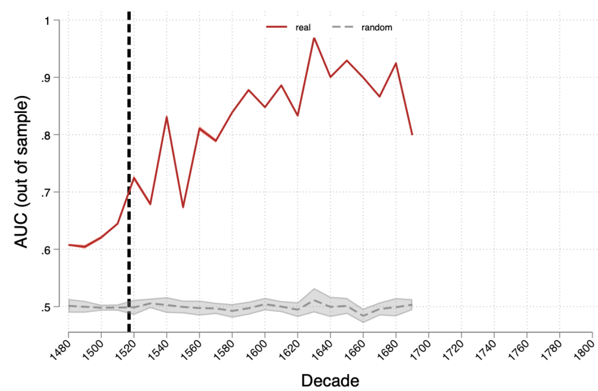
(a) All prints



(b) Religious prints



(c) Other prints



Note: This figure plots the predictive accuracy of short titles measured by the out-of-sample area under the ROC curve (AUC). For each decade, I repeatedly split the sample into an 80% training set and a 20% hold-out test set, fit an ℓ_2-penalized logistic regression classifier on the embedding vectors, and compute the AUC on the hold-out set. The red line shows the mean AUC across 500 iterations with 95% confidence intervals; the dashed gray line shows the corresponding placebo distribution from 500 iterations with randomly permuted labels. Panel A reports results for all short titles; Panel B restricts to titles with subject "Religious"; Panel C to all other subjects. AUC rises from 0.5 (random guessing) pre-1520 to 0.75-0.80 post-1560, indicating that titles' confessional origin becomes increasingly predictable from content alone. The out-of-sample validation confirms results are not driven by overfitting. Short titles are from the *Universal Short Title Catalogue* (2025) and cover 1480–1700.

Table D.1: Most partisan short titles by decade (embedding-based).

Catholic				Protestant			
Decade	Author	Text	Pr(P)	Decade	Author	Text	Pr(P)
1520s	Eck, Johannes	*Handbook of Common Places Against the Lutherans*	0.19	1520s	Luther, Martin	*A sermon. On the unjust mammon, Luke 16, and how we should do good works that are pleasing to God and that He desires from us.*	0.89
1570s	Peltanus, Theodor Anton	*Catholica. An explanation of the number of canonical books, their authority, and legitimate interpretation.*	0.10	1570s	Hamelmann, Hermann	*A sermon. Against the accusers, deceivers, crystal seekers, sorcerers, witnesses, and seers.*	0.96
1620s	Rader, Matthäus	*On the Life of Peter Canisius of the Society of Jesus, the First Companions from Germany, Three Books*	0.08	1620s	Arndt, Johann	*Apologetic Repetition. That is: Repetition and Defense of the Doctrine of True Christianity: for further information or instruction of those who love Christ and piety, so that they may not be led astray by the godless world.*	0.88
1670s	Haunold, Christoph	*Four Books of Speculative Theology Adapted to Scholastic Lectures and Exercises: Responding to the Parts of the Summa of St. Thomas, in which Selected Opinions of Recent Theologians are Discussed to Address Major Difficulties*	0.06	1670s	Strauch, Aegidius	*The days of Purim, of the Evangelical-Lutheran named Christian hearts: for their holy observance, initiated by Luther for the expulsion of the papist sourdough, has been encouraged by Aegidius Strauch out of a sense of duty.*	0.81

Notes: This table lists the most partisan short titles by decade, based on the embedding-based ℓ_2-penalized logistic regression classifier. For each selected decade, the table reports the title, author, and predicted probability of being from a Protestant town ($\widehat{p}_i$). The left panel displays the top Catholic-leaning titles (lowest $\widehat{p}_i$), and the right panel displays the top Protestant-leaning titles (highest $\widehat{p}_i$). Predictions are averaged over 500 bootstrap iterations; ties are broken by earliest publication date. Examples confirm that the classifier captures genuine theological content: Protestant titles emphasize scripture, evangelical teaching, and critiques of Catholic practice, while Catholic titles emphasize sacraments, church authority, and Counter-Reformation themes. See Appendix Section D.2 for details on model construction.

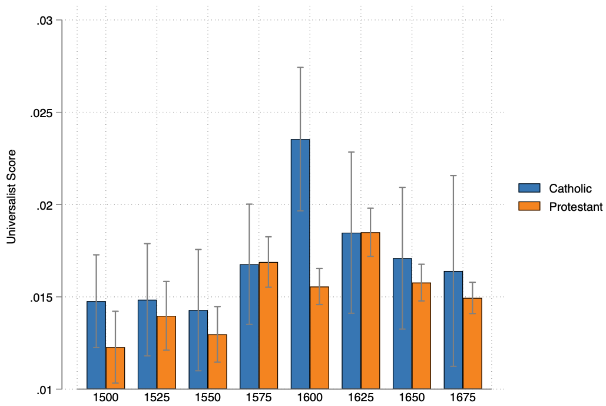
Table D.2: Most partisan bigrams by decade.

Catholic		Protestant	
Rank	Bigram	Rank	Bigram
	1520s		
1	enchiridion gemeinsam	1	predigen evangel
2	christ glauben	2	auslegung evangel
3	gebrauch kind	3	predigen sonntag
4	gott buch	4	prosodie syntax
5	gemeinsam ort	5	christ freiheit
	1570s		
1	postill auslegung	1	unser herr
2	latein sprache	2	heilig abendmahl
3	schön christ	3	folgend thes
4	christ welt	4	versuchen folgend
5	heilig christ	5	alt testament
	1620s		
1	diskussion unterbreiten	1	heilig buch
2	spanisch kanzlei	2	gott wort
3	theologie philosophie	3	unser herr
4	ursprung fortschritt	4	alt testament
5	verehrung heilig	5	wahr christentum
	1670s		
1	theologie these	1	akademisch bürger
2	kanonisch recht	2	heilig geist
3	historisch politik	3	studierend jugend
4	bemerkenswert ereignis	4	alt neu
5	jurist diskurs	5	kapitel vers

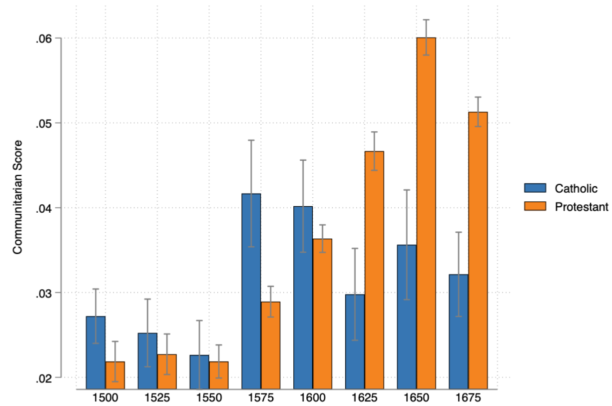
Notes: This table reports the five most predictive bigrams by decade, estimated using an ℓ_2-penalized logistic regression on bigram frequencies. For each decade, the left panel lists the top Catholic-associated bigrams (most negative coefficients), and the right panel lists the top Protestant-associated bigrams (most positive coefficients). Coefficients are averaged across 500 bootstrap iterations, and bigrams are ranked by absolute coefficient magnitude. Linguistic patterns align with known theological differences: Protestant bigrams emphasize "göttlich wort" (divine word), "evangelisch lehr" (evangelical teaching), and scripture, while Catholic bigrams emphasize "heilig sakrament" (holy sacrament), "römisch kirch" (Roman church), and church authority. See Appendix Section D.2 for details on model construction and averaging procedure.

Figure D.4: Moral foundations in short titles: universalism vs. communitarianism.

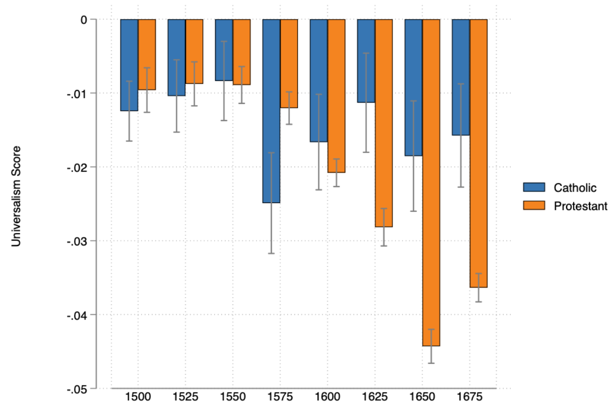
(a) Universalist Score



(b) Communitarian Score



(c) Net Universalism (Universalist minus Communitarian)



Note: This figure plots mean moral foundation scores with 95% confidence intervals by 25-year interval for Catholic (orange) and Protestant (blue) short titles. Panel A shows the average universalist score (emphasizing shared humanity, individual rights, abstract principles); Panel B shows the average communitarian score (emphasizing group identity, tradition, collective obligations); Panel C shows net universalism (universalist minus communitarian scores). Scores are computed using the Moral Foundations Dictionary following Figueroa and Fouka (2023). Protestant prints show a sustained increase in communitarian language from 1550 onward, especially after the Thirty Years' War (1618), while Catholic prints maintain relatively stable scores. This suggests Protestant texts increasingly emphasize confessional community boundaries, consistent with historical accounts of Protestant identity formation.

Table D.3: Universalism and predicted Protestant score of titles.

	Probability Protestant							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Universalism Score	-0.254***	-0.194***	-0.182***	-0.023**	-0.143***	-0.048***	-0.015*	-0.043***
	[0.010]	[0.010]	[0.007]	[0.009]	[0.010]	[0.007]	[0.006]	[0.007]
Decade FEs	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Linguistic region FEs	No	No	Yes	No	No	No	No	No
Subject FEs	No	No	No	Yes	No	No	Yes	No
Topic FEs	No	No	No	No	Yes	No	No	Yes
Author FEs	No	No	No	No	No	Yes	Yes	Yes
R^2	0.012	0.062	0.558	0.298	0.157	0.629	0.716	0.645
Observations	53846	53846	53399	51829	53846	52771	50827	52771

Notes: This table reports estimates from regressions of the predicted Protestant score of a document (from the embedding-based ℓ_2-penalized logistic regression classifier) on its universalism score, defined as the difference between normalized universalist and communitarian word frequencies. Columns differ by the set of fixed effects included: decade, linguistic region, subject (USTC classification), topic (from the BERTopic model), and author. The negative coefficient indicates that titles emphasizing universalist themes are less likely to be classified as Protestant, even controlling for subject matter, authorship, and time period. This supports the interpretation that confessional differentiation reflects substantive ideological divergence: Protestant texts increasingly emphasize communal identity and group boundaries, while Catholic texts maintain more universalist framing. Standard errors are clustered at the author level. *, **, and *** denote significance at the 10, 5, and 1 percent levels, respectively.

E Conceptual Framework: Technical Details

This appendix provides formal derivations and extensions of results stated in Section 7. Section E.1 derives intergenerational transmission dynamics. Section E.1 shows how individual choices aggregate to reduced-form gap-closing. Section E.2 derives S-shaped diffusion from feedback loops. Section E.3 connects the framework to existing theories. Section E.4 sketches how doctrine and enforcement arise from church competition.

E.1 Microfoundations and Formal Derivations

This appendix provides detailed derivations of results stated in Section 7.

Cultural Transmission: Imperfect Empathy

Following Bisin and Verdier (2001), I model intergenerational preference transmission through imperfect empathy. Individual i's child enters period $t+1$ with preference:

$$\theta_{i,t+1} = (1-\tau)\theta_{i,t} + \tau \cdot a_{i,t} + \eta_{i,t+1} \tag{1}$$

where $a_{i,t}$ is the parent's chosen identity expression in period t, $\tau \in [0,1]$ measures socialization effectiveness, and $\eta_{i,t+1} \sim N(0, \sigma_\eta^2)$ is an idiosyncratic shock.

Interpretation. If $\tau = 0$, preferences are purely innate (biological transmission). If $\tau = 1$, children perfectly internalize parental behavior (complete socialization). Intermediate values capture *imperfect empathy*—children partially adopt parental identities but retain innate disposition.

Aggregate dynamics. Averaging over individuals in confession g at location ℓ:

$$\bar{\theta}_{g,\ell,t+1} = (1-\tau)\bar{\theta}_{g,\ell,t} + \tau \cdot \bar{a}_{g,\ell,t} \tag{2}$$

where $\bar{a}_{g,\ell,t} = \mathbb{E}[a^*_{i,\ell,t} | i \in g, \ell]$ is mean identity expression.

Steady state. With stable institutions (κ_ℓ constant), preferences converge to $\bar{\theta}^*_{g,\ell} = \bar{a}^*_{g,\ell}$ at rate $(1-\tau)^t$. This creates **cultural persistence**: temporary enforcement (κ_ℓ high for several periods, then drops) has permanent effects because elevated past identity $\bar{a}_{g,\ell,t-s}$ raises future preferences $\bar{\theta}_{g,\ell,t}$.

Connection to Bisin-Verdier. While Bisin and Verdier (2001) focus on *strategic* socialization effort (minorities socialize harder), I take τ as given but add: (i) spatial networks—transmission occurs horizontally across locations, not just vertically within families, and (ii) institutional enforcement—exogenous church pressure competes with family socialization. The conformity parameter λ in equation (17) captures Bisin-Verdier's oblique/horizontal transmission through geographic neighbors.

Aggregation and Reduced-Form Dynamics

I derive equation (22) from individual optimality and aggregation.

Group-level identity. Averaging the first-order condition (equation 18) over individuals in confession

g at location ℓ:

$$\bar{a}_{g,\ell,t} = \mathbb{E}[a^*_{i,\ell,t}|i \in g, \ell] = \frac{\kappa_\ell \theta^g + \lambda \sum_{\ell'} w_{\ell\ell'} \bar{a}_{g,\ell',t-1} + \gamma \bar{\theta}_{g,\ell,t}}{\kappa_\ell + \lambda + \gamma} \tag{3}$$

Differencing. Subtracting $\bar{a}_{g,\ell,t-1}$ from both sides:

$$\begin{aligned} \bar{a}_{g,\ell,t} - \bar{a}_{g,\ell,t-1} = & \frac{\lambda}{\kappa_\ell + \lambda + \gamma} \left[\sum_{\ell'} w_{\ell\ell'} \bar{a}_{g,\ell',t-1} - \bar{a}_{g,\ell,t-1} \right] \\ & + \frac{\kappa_\ell}{\kappa_\ell + \lambda + \gamma} [\theta^g - \bar{a}_{g,\ell,t-1}] + \frac{\gamma}{\kappa_\ell + \lambda + \gamma} \left[\bar{\theta}_{g,\ell,t} - \bar{a}_{g,\ell,t-1} \right] \end{aligned} \tag{4}$$

This shows three gap-closing forces:

- **Conformity gap** (coefficient $\phi_\ell = \lambda/(\kappa_\ell + \lambda + \gamma)$): Distance to network neighbors
- **Institutional pull** (coefficient $\psi_\ell \cdot \kappa_\ell = \kappa_\ell/(\kappa_\ell + \lambda + \gamma)$): Distance to doctrine, weighted by enforcement
- **Preference gap** (coefficient $\gamma/(\kappa_\ell + \lambda + \gamma)$): Distance to mean innate preference

Approximation. If preferences evolve slowly—either (a) socialization is weak (τ small in equation 1), or (b) preferences have approximately converged ($\bar{\theta}_{g,\ell,t} \approx \bar{a}_{g,\ell,t}$)—then the preference gap term is negligible. Dropping it:

$$\Delta \bar{a}_{g,\ell,t} \approx \frac{\lambda}{\kappa_\ell + \lambda + \gamma} \left[\sum_{\ell'} w_{\ell\ell'} \bar{a}_{g,\ell',t-1} - \bar{a}_{g,\ell,t-1} \right] + \frac{\kappa_\ell}{\kappa_\ell + \lambda + \gamma} [\theta^g - \bar{a}_{g,\ell,t-1}] \tag{5}$$

Location-Varying Parameters and Linearization

The coefficients in equation (5) vary with local enforcement:

$$\phi_\ell = \frac{\lambda}{\kappa_\ell + \lambda + \gamma} \tag{6}$$

$$\psi_\ell = \frac{1}{\kappa_\ell + \lambda + \gamma} \tag{7}$$

For empirical implementation, I estimate *average* parameters $\bar{\phi}$ and $\bar{\psi}$ evaluated at mean enforcement $\bar{\kappa} = \mathbb{E}[\kappa_\ell]$. This is a first-order Taylor approximation around $\bar{\kappa}$.

Validity of approximation. This linearization is accurate when enforcement variation is modest relative to its mean. The empirical robustness checks show results are stable when allowing ϕ to vary with enforcement quartiles, confirming the pooled approximation does not meaningfully bias inference.

Within-Group Homogeneity Assumption

Equation (19) assumes $a^*_{i,g,\ell} \approx \bar{a}^*_{g,\ell}$ (within-group homogeneity). This allows aggregating the individual multinomial logit to group-level probabilities.

Empirical validation. Decomposing variance in name-based identity scores shows that within-confession variance is an order of magnitude smaller than between-confession variance. Most identity heterogeneity is across confessions, not within, supporting the approximation that $a^*_{i,g} \approx \bar{a}^*_g$ loses little information for the analysis.

Steady-State Properties

With preferences converged ($\bar{\theta}_{g,\ell} = \bar{a}_{g,\ell}$) and $\Delta \bar{a}_{g,\ell} = 0$, equation (3) yields:

$$\bar{a}^*_{g,\ell} = \frac{\kappa_\ell \theta^g + \lambda \sum_{\ell'} w_{\ell\ell'} \bar{a}^*_{g,\ell'}}{\kappa_\ell + \lambda} \tag{8}$$

This is a system of $2L$ linear equations (one per confession-location) with unique solution via matrix inversion (assuming the network is connected).

Property 1: Direct enforcement effect. Differentiating equation (8) with respect to κ_ℓ yields $\frac{\partial |\bar{a}^*_{g,\ell}|}{\partial \kappa_\ell} > 0$ (identity moves toward doctrine θ^g). Diminishing returns follow from convexity.

Property 2: Spatial spillovers. For locations with $\kappa_\ell = 0$ (no enforcement), equation (8) becomes $\bar{a}^*_{g,\ell} = \sum_{\ell'} w_{\ell\ell'} \bar{a}^*_{g,\ell'}$. Identity equals the weighted average of neighbors. If some neighbors have $\kappa_{\ell'} > 0$, then $\bar{a}^*_{g,\ell'} \neq 0$, so $\bar{a}^*_{g,\ell} \neq 0$ through network effects.

Property 3: Social amplification. The total effect of enforcement at location ℓ' on identity at location ℓ includes direct and indirect (network) channels. The network term amplifies the direct effect, with magnitude decreasing in network distance between ℓ and ℓ'.

Property 4: Path dependence. With $\tau > 0$ in equation (1), a temporary shock to enforcement at time t_0 affects preferences at all future dates. Even if κ_ℓ returns to baseline, elevated past identity $\bar{a}_{g,\ell,t_0}$ increases future $\bar{\theta}_{g,\ell,t}$, sustaining higher steady-state identity through socialized preferences.

E.2 S-Shaped Diffusion

This appendix derives the S-shaped temporal dynamics from the feedback loop between name choice and belief updating.

Formal Derivation

Define name polarization:

$$\Delta_{n,\ell,t} \equiv \pi_{n|P,\ell,t} - \pi_{n|C,\ell,t} \tag{9}$$

measuring the Protestant-Catholic usage gap for name n, ranging from -1 (only Catholics use it) to $+1$ (only Protestants use it).

Sorting dynamics. From the multinomial logit (equation 19), when ξ is moderate and signals are not at extremes, a first-order Taylor expansion around neutrality yields:

$$\pi_{n|P,\ell,t} - \pi_{n|C,\ell,t} \approx 2\xi(\bar{a}^*_P - \bar{a}^*_C) \cdot (s_{n,\ell,t} - 0.5) \cdot \pi^{\text{base}}_n (1 - \pi^{\text{base}}_n) \tag{10}$$

where π^{base}_n is the baseline popularity of name n. Usage divergence is proportional to signal clarity ($s_n - 0.5$) and identity gap ($\bar{a}^*_P - \bar{a}^*_C$).

Belief dynamics. From DeGroot updating (equation 21):

$$s_{n,\ell,t+1} - s_{n,\ell,t} = \beta \left[\sum_{\ell'} w_{\ell\ell'} \tilde{s}_{n,\ell',t} - s_{n,\ell,t} \right] \tag{11}$$

where $\tilde{s}_{n,\ell,t}$ is the observed Protestant share. Since $\tilde{s}_{n,\ell,t} = 0.5 + f(\Delta_{n,\ell,t})$ for some increasing function f, belief changes respond to usage polarization.

Coupled system. Taking time derivatives and assuming smooth evolution, the system dynamics can be approximated as:

$$\frac{d\Delta_n}{dt} \approx r_n \cdot \Delta_n \cdot \left(1 - \frac{|\Delta_n|}{\Delta_n^{\max}}\right) \tag{12}$$

where $r_n = 2\xi\beta(\bar{a}_P^* - \bar{a}_C^*)\pi_n^{\text{base}}(1 - \pi_n^{\text{base}})$ is the growth rate and $\Delta_n^{\max}$ is maximum polarization.

This is a logistic differential equation—the canonical model of S-shaped growth. The approximation captures how the feedback between choices and beliefs generates self-reinforcing dynamics that saturate as polarization approaches its maximum.

Three Phases of Diffusion

The logistic solution exhibits three phases:

Phase 1: Slow start. When $\Delta_n \approx 0$, signals are unclear ($s_n \approx 0.5$), so sorting is weak. Small initial differences don't amplify rapidly.

Phase 2: Rapid acceleration. The feedback loop engages: sorting intensifies $\rightarrow$ usage patterns diverge $\rightarrow$ signals clarify $\rightarrow$ more sorting. Growth rate peaks when $\Delta_n = \Delta_n^{\max/2}$ (halfway to saturation). This is the *takeoff phase*.

Phase 3: Deceleration. As $\Delta_n \rightarrow \Delta_n^{\max}$, two brakes engage: (i) few remaining individuals to sort ($\pi_{n|P} \rightarrow 1$, $\pi_{n|C} \rightarrow 0$), and (ii) signal-identity gap closes ($s_n \rightarrow \bar{a}_P^*$). Convergence to steady state.

Cross-sectional heterogeneity. The growth rate r_n varies across names. Names with clearer initial signals ($|s_n(0) - 0.5|$ large) enter Phase 2 earlier. Names in locations with high enforcement experience larger identity gaps, increasing r_n.

E.3 Connection to Literature

This appendix details connections between the framework and existing models of cultural transmission, learning, and identity formation.

Bisin and Verdier: Cultural Transmission

Bisin and Verdier (2001) model cultural transmission via *direct* (parental), *oblique* (teachers/institutions), and *horizontal* (peer) socialization. Their key insight: parents strategically invest more in socialization when their trait is minority, creating strategic complementarity.

What I adopt:

- **Imperfect empathy** (equation 20): Children partially internalize parental identity, with effectiveness $\tau \in [0,1]$
- **Endogenous preferences:** Today's choices affect tomorrow's preferences through socialization

What I add:

- **Spatial networks:** Horizontal transmission occurs through geographic proximity ($w_{\ell\ell'}$), not random matching, generating spatial diffusion and local spillovers
- **Institutional enforcement:** Churches exert exogenous pressure (κ_ℓ) competing with family socialization
- **Endogenous cultural content:** What traits *mean* (name signals s_n) evolves through learning; Bisin-Verdier take trait characteristics as fixed

What I abstract from:

- **Strategic socialization:** I take τ as exogenous. Incorporating $\tau = \tau(\text{minority status})$ would add strategic complementarity but complicate identification—I lack data on parental socialization effort
- **Vertical vs. oblique distinction:** My τ combines both parental and institutional socialization

The conformity parameter λ maps to Bisin-Verdier's horizontal socialization intensity. My innovation is making this spatially structured via networks, enabling study of diffusion patterns.

Fernández: Cultural Learning

Fernández (2013) models learning about returns to female labor supply. Households observe neighbors' labor choices and earnings, updating beliefs about costs/benefits. Learning generates S-shaped diffusion.

What I adopt:

- **Learning from observation:** Agents update beliefs by observing aggregate outcomes in their network
- **S-shaped dynamics:** Feedback between choices and beliefs generates acceleration then saturation
- **DeGroot-style updating:** Simple averaging rather than full Bayesian inference (bounded rationality)

Key difference: Learning-about-returns vs. learning-about-meanings

	Fernández (2013)	My Model
What is learned?	Returns to female labor supply (productivity, costs)	Confessional association of names (meanings, signals)
Type of learning	Discovery of underlying state (optimality)	Coordination on equilibrium (conventions)
Welfare implications	Pareto improvements possible	Pure coordination (no Pareto ranking)
Belief convergence	Converge to truth (returns are fixed)	Converge to self-fulfilling equilibrium

Multiple equilibria: Any name could become associated with either confession. Which equilibrium emerges depends on initial conditions and enforcement shocks. This contrasts with Fernández where learning reveals a fixed optimal practice.

What I add:

- **Conformity motive:** Equation (17) includes preference-based conformity, not just information-based learning
- **Two-stage choice:** Identity choice separate from name choice, allowing distinction between conformity in aggregate identity and learning about specific names
- **Enforcement shocks:** Exogenous institutional pressure creates variation in identity gaps

Shayo: Identity-Based Utility

Shayo (2009) models identity choice with utility depending on distance to ingroup prototype. Individuals choose which group to identify with based on material payoffs and conformity to prototypes.

What I adopt:

- **Conformity to ingroup:** $-\lambda(a - \bar{a}_{\text{ingroup}})^2$ term in equation (17)
- **Endogenous identity salience:** Enforcement κ_ℓ increases salience of religious identity

What I add:

- **Spatial heterogeneity:** Prototype is location-specific ($\bar{a}_{g,\ell}^{\text{local}}$), generating within-confession variation
- **Endogenous prototype:** The ingroup prototype evolves through conformity dynamics; Shayo takes prototypes as exogenous
- **Learning about practices:** What practices signal group membership is learned; Shayo assumes associations are known
- **Institutional variation:** Enforcement heterogeneity provides identifying variation

What I abstract from:

- **Distance to outgroup:** Bonomi, Gennaioli, and Tabellini (2021) and Gennaioli and Tabellini (2023) add $-\delta(a-\bar{a}_{\text{outgroup}})^2$ (aversion to outgroup). I omit for parsimony—incorporating would strengthen polarization predictions but complicate estimation without adding testable implications
- **Material payoffs:** Shayo includes economic returns to identity choice. I focus on purely expressive utility, plausible for name choice in this historical setting

Network structure of conformity: My key innovation over Shayo is spatially-structured conformity via geographic networks rather than global prototypes, enabling study of diffusion patterns and network effects.

Synthesis: Dual-Level Mechanisms

Existing models typically emphasize ONE social force (Bisin-Verdier: transmission; Fernández: learning; Shayo: conformity).

My contribution: Show conformity and learning operate *simultaneously at different levels*:

1. **Group level (conformity):** Aggregate identity $\bar{a}_{g,\ell}$ converges toward neighbors' average through preference-based conformity

2. **Name level (learning):** Specific name usage $\pi_{n|g,\ell}$ converges through information-based learning

This dual structure generates richer predictions: spatial diffusion (conformity) + heterogeneous speeds by signal clarity (learning) + S-curves with conformity amplification (interaction).

The empirical strategy exploits this: aggregate regressions estimate *total* social force $\bar{\phi}$, while name-level regressions test for learning-specific predictions (heterogeneity by signal clarity).

E.4 Endogenizing Doctrine and Enforcement

The main model takes church doctrinal positions θ^C, θ^P and enforcement capacity κ_ℓ as exogenous. This section sketches how these parameters emerge from church competition dynamics. The framework provides intuition rather than a fully-specified equilibrium model.

Core Mechanism

Pre-Reformation (Catholic Monopoly): Without competition, the Catholic Church faced limited pressure to foster sharp confessional identity. Enforcement targeted external threats (heresies, paganism) rather than internal identity formation. Doctrine remained moderate, enforcement capacity for identity formation was minimal.

Post-Reformation (Competitive Entry): Protestant entry creates incentives for:

1. **Doctrinal differentiation:** To distinguish from competitors and reduce ruler defection risk

2. **Enforcement investment:** To prevent member defection and maintain commitment

Result: Divergent doctrines and high enforcement capacity focused on confessional identity formation.

Why differentiation over convergence? Two complementary forces:

1. Club good mechanism (Iannaccone 1992): More stringent requirements create higher commitment thresholds, screening out free-riders and raising average member quality. In the church context, clearer doctrinal positions and stricter enforcement create stronger boundaries, generating higher-quality congregations willing to contribute resources.

2. Political stability: Under cuius regio, eius religio, rulers chose their territory's confession. Greater doctrinal distance between churches reduces the ruler's temptation to switch allegiances (switching becomes costlier as churches become more distinct), stabilizing the revenue base for each church. This parallels product differentiation in oligopoly—minimal differentiation leads to intense competition for the same clients.

Stylized Model

Consider two churches $i \in \{C, P\}$ simultaneously choosing:

- $\theta^i \in \mathbb{R}$: Doctrinal position (0 = neutral, extreme values = distinctive doctrine)
- $E_i \geq 0$: Investment in enforcement capacity (colleges, visitation systems, disciplinary apparatus)

Enforcement technology: Capacity is $\kappa_i = f(E_i)$ where $f(0) = 0$, $f' > 0$, $f'' < 0$ (diminishing returns). Example: $\kappa_i = 1 - e^{-\alpha E_i}$.

Membership and revenue: Church i obtains members n_i depending on doctrine and enforcement. Higher enforcement κ_i retains members (prevents defection to rival church or heterodox movements). Doctrinal differentiation $\Delta \equiv |\theta^C - \theta^P|$ affects political stability and member commitment. Revenue is:

$$R_i(\theta^i, E_i, \theta^{-i}, E_{-i}) = r \cdot n_i(\theta^i, E_i, \theta^{-i}, E_{-i}) \cdot q(\Delta) \cdot s(\Delta) \tag{13}$$

where:

- $n_i(\cdot)$: Membership, increasing in own enforcement E_i (retains adherents)
- $q(\Delta)$: Member quality/commitment (club good benefit), $q' > 0$
- $s(\Delta)$: Political stability (lower ruler switching probability), $s' > 0$

Costs: Churches face:

$$c_{\text{doctrine}}(\theta^i) = \gamma_\theta (\theta^i)^2 \qquad \text{(cost of extreme positions)} \tag{14}$$

$$c_{\text{enforcement}}(E_i) = \gamma_E E_i^2 \qquad \text{(investment in capacity)} \tag{15}$$

$$c_{\text{implementation}}(\theta^i, E_i) = \gamma_{\text{int}} \cdot |\theta^i| \cdot e^{-\beta E_i} \qquad \text{(implementation difficulty)} \tag{16}$$

The implementation term captures that extreme doctrine ($|\theta^i|$ large) is costly to actualize among members, but enforcement (E_i) reduces this cost—i.e., enforcement and extreme doctrine are complements in production.

Total cost: $C_i = \gamma_\theta (\theta^i)^2 + \gamma_E E_i^2 + \gamma_{\text{int}} |\theta^i| e^{-\beta E_i}$

Each church maximizes $\Pi_i = R_i - C_i$.

Equilibrium Characterization

Monopoly benchmark. Without a competitor, there is no differentiation benefit ($\Delta = 0$ mechanically). The FOC for doctrine balances marginal doctrinal cost against implementation cost reduction. With symmetric costs and no differentiation gains, the optimum is $\theta^{\text{monopoly}} = 0$, $E^{\text{monopoly}} = 0$ for identity-formation enforcement (enforcement is costly; revenue gains are limited without competition).

Duopoly with differentiation. Consider a symmetric Nash equilibrium where $\theta^C = -\bar{\theta}$, $\theta^P = +\bar{\theta}$, yielding doctrinal distance $\Delta = 2\bar{\theta}$. First-order conditions:

Doctrine choice:

$$\underbrace{r \cdot \frac{\partial n_i}{\partial \Delta} \cdot 2 \cdot q(\Delta) \cdot s(\Delta) + r \cdot n_i \cdot [q'(\Delta) + s'(\Delta)] \cdot 2}_{\text{Marginal revenue from differentiation}} = \underbrace{2\gamma_\theta \bar{\theta} + \gamma_{\text{int}} e^{-\beta E^*}}_{\text{Marginal cost}} \tag{17}$$

The left side includes (i) membership gains from differentiation, (ii) quality improvements (club good), and (iii) stability gains. The right side is the cost of extreme positions.

Enforcement choice:

$$\underbrace{r \cdot \frac{\partial n_i}{\partial E_i} \cdot q(\Delta) \cdot s(\Delta)}_{\text{Marginal revenue from retention}} \quad + \quad \underbrace{\beta \gamma_{\text{int}} \bar{\theta} e^{-\beta E^*}}_{\text{Reduced implementation cost}} \quad = \quad \underbrace{2\gamma_E E^*}_{\text{Marginal investment cost}} \tag{18}$$

Enforcement has two benefits: (i) retaining members, (ii) reducing the cost of extreme doctrine.

[Competition Induces Extremism] Compared to monopoly ($\bar{\theta} = 0,\ E = 0$), duopoly equilibrium features:

(i) Divergent doctrines: $\bar{\theta}^* > 0$

(ii) Positive enforcement: $E^* > 0$

(iii) Strategic complementarity: Higher $\bar{\theta}$ increases marginal benefit of E (through implementation cost channel), and higher E reduces marginal cost of $\bar{\theta}$

Intuition: Differentiation creates value through club goods and political stability, justifying doctrinal extremism. Extreme doctrines require enforcement to implement, creating demand for investment in capacity. The two choices reinforce each other.

[Technology and Polarization] Improvements in enforcement technology (higher α in $\kappa = 1 - e^{-\alpha E}$ or lower marginal costs γ_E) lead to more extreme equilibrium doctrine $\bar{\theta}^*$ and higher enforcement E^*.

Intuition: Better technology reduces the marginal cost of enforcement, increasing optimal E^*. Higher enforcement reduces the implementation cost of extreme positions (via the $e^{-\beta E}$ term), lowering the marginal cost of $\bar{\theta}$. The two FOCs create positive feedback: technology improvements amplify both doctrinal extremism and enforcement investment.

Historical application. The printing press (mid-15th century) and subsequent developments in educational infrastructure dramatically reduced the cost of producing enforcement capacity (catechisms, parish schools, examination systems). In the model, lower cost raises the potential for divergence, so that, once competition arrives, the force for doctrinal divergence is stronger with better technology. Later moderation (post-1650) could reflect declining differentiation benefits as territorial boundaries stabilized and switching risks diminished.

Connection to Main Framework

This model provides a microfoundation for the parameters taken as exogenous in Sections 7:

- **Doctrinal positions:** $\theta^C = -\bar{\theta}^*$, $\theta^P = +\bar{\theta}^*$ (from equilibrium FOCs)

- **Enforcement capacity:** $\kappa_\ell = f(E_\ell^*)$ (from technology)

Spatial variation in enforcement κ_ℓ arises from differential access to enforcement technology across locations: proximity to theological colleges, administrative capacity for visitation systems, printing infrastructure, political support from territorial rulers, and local institutional presence.

Two-stage interpretation of the overall framework:

- **Stage 0** (this section): Churches compete, choosing doctrine and enforcement investment in response to post-Reformation competition
- **Stage 1** (main model): Individuals respond to established doctrine and enforcement through identity formation dynamics (conformity, learning, intergenerational transmission)

Limitations. This remains a sketch rather than a fully-specified game. A complete model would need to:

1. Specify how membership n_i depends on all four choice variables
2. Account for the territorial assignment mechanism (cuius regio) that determined which church governed each location
3. Incorporate dynamic considerations (sequential rather than simultaneous choice)
4. Model enforcement as location-specific (churches choose E_ℓ for each territory)

Despite these simplifications, the framework clarifies why the Reformation triggered doctrinal polarization and enforcement investment, providing a foundation for the institutional variation I exploit empirically.