Rethinking "Distance From": Lessons from Wittenberg and Mainz

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Abstract:

An influential literature in early modern economic history uses "distance from" as an instrumental or a control variable. I show that "distance from Wittenberg" and "distance from Mainz," two prominent instruments for the adoption of Protestantism and printing technology, have historical and econometric drawbacks that engender misleading conclusions. Historical data challenge the assumption that distance determined access to ideas or technology. Placebo tests and simulations reveal that "distance from" variables frequently produce falsely significant estimates in first stage and OLS regressions. My findings suggest caution in using "distance from" instruments for the adoption of ideas and technologies.

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1. Introduction

Protestantism's impact on economic development represents a classic question in the social sciences. This inquiry entails the challenge of causal identification: selection into Protestantism could reflect latent characteristics that favored economic growth. Sascha Becker and Ludger Woessmann's 2009 paper, "Was Weber Wrong? A Human Capital Theory of Protestant Economic History" (henceforth "BW (2009)") presented a methodological breakthrough by using "distance from Wittenberg" as an instrumental variable for the adoption of Protestantism. Citing high costs of information diffusion in the 16th century, BW reasoned that Protestantism spread outward from its birthplace, Wittenberg, in concentric circles, making its adoption more likely in territories closer to Wittenberg. For the exclusion restriction, since Wittenberg was an otherwise unremarkable town throughout German history, distance from Wittenberg should not have affected economic development except through its connection with Protestantism.

This innovation has both invigorated the literature on Reformation economic history and founded a methodological trend in broader empirical research. Papers using the "distance from Wittenberg" instrument now underlie our understanding of important issues such as Protestantism's effect on human capital and economic growth (BW 2008, 2009), on urban development (Cantoni 2015), and the Quantity-Quality trade-off in human fertility behavior (Becker, Cinnirella, and Woessmann 2010). Other scholars have used "distance from" other places as instruments for the impact of ideas and technologies originating from those places. For example, "distance from Mainz" for the impact of printing (Dittmar, Rubin, and co-authors, 2011, 2014, 2021, forthcoming). More generally, many recent economic history papers use "distance from" as a regressor or control variable, such as distance from Paris (Acemoglu et al.

2011, Scquicciarini 2020), distance from the nearest Huguenot colony (Hornung 2014), and distance from the nearest university (Cantoni and Yuchtman 2014). (Appendix A reports a likely incomplete list of such papers.) Although BW and co-authors' more recent works have focused on more complex mechanisms of Protestantism's diffusion such as networks (Becker et al. 2020, 2023), "distance from" instruments and the research findings they have generated have remained influential.

This paper re-examines the instrument "distance from Wittenberg" and its close relative "distance from Mainz." Historical evidence does not support a concentric spread model of Protestantism from Wittenberg or the hypothesis that distance from Wittenberg determined access to Protestant ideas. The same argument applies to distance from Mainz and printing. Thus, we have no reason to expect a valid IV first stage. Why, then, does distance from Wittenberg/Mainz appear correlated with Protestantism/printing in the first stage IV and OLS regressions? I find that the observed association likely reflects spatial autocorrelation: replication exercises with distances from alternative cities instead of Wittenberg show similar first-stage results. Monte Carlo simulations confirm that in general, "distance from a given point" frequently produces statistically significant estimates even the true diffusion process has a different, multiple, or no center at all. These findings imply two drawbacks for the second stage: a violation of the exclusion restriction, and problems with drawing qualitative conclusions from regression results.

Other scholars have challenged BW (2009)'s conclusions. Edwards (2021) shows that BW's results disappear after controlling for district. Kersting et al. (2020) uses an alternative instrument and new data to argue that ethnic, rather than religious, differences explain differences in prosperity in 19th-century Prussia; they forgo BW's instrument because, within the borders of 19th-century Prussia, distance from Wittenberg was correlated with Polish population presence, thus creating a channel other than religious differences for differential economic development. Dieterle and Snell (2016) finds that replacing BW's linear first stage with a quadratic model changes their second stage results qualitatively, suggesting that the treatment of Protestantism had different effects in different counties (more precisely, promoting literacy and prosperity in counties closer to Wittenberg but *hindering* these outcomes in farther-away counties). I know of no paper that re-examines research involving distance from Mainz.

This paper's goal is not to criticize any existing research. While it shows that distance from Wittenberg ("distwitt") or Mainz may not imply what authors intend, my results have more general implications: "distance from" variables may be unwise regressors or instrumental variables in a broad class of instances. My paper fills the lack of formal studies on "distance from" variables and finds more serious drawbacks, both historical and econometric, than scholars (such as Dippel and Leonard 2021) have surmised. My paper proceeds as follows. First it presents historical evidence that challenges the "concentric spread" of Luther's ideas and printing technology. This evidence cautions us against expecting a correlation between distance from Wittenberg/Mainz and the adoption of Protestantism/printing in the IV first stage. Then it studies why distwitt nonetheless produces statistically significant first-stage and OLS results. I show first-stage/OLS replication results, followed by Monte Carlo simulations, to argue Wittenberg's statistical correlation with adopting Protestantism reflects spatial autocorrelation. Two further sections discuss these findings' implications for the exclusion restriction and for second-stage results, respectively. The conclusion discusses the implications for future research.

2. *Did distance matter for the spread of early modern ideas?*

Citing high costs of travel and information diffusion in the 16th century, BW reasoned that Protestantism diffused outward from Wittenberg—where Martin Luther reportedly nailed his Ninety-Five Theses to a church door on October 31, 1517, and the seat of Electoral Saxony, an early adopter of Protestantism—in concentric circles. Therefore, distance from Wittenberg (measured "as the crow flies," or in great circle distance) predicted a territory's access to Protestant ideas as well as reform practices, and thus its likelihood of adopting Protestantism.

A closer look at the historical evidence, however, suggests a different picture: distance and travel costs unlikely posed significant barriers to the spread of Luther's ideas. Luther's writings travelled rapidly, reaching faraway places early on, and the spread soon ceased being concentric. Luther himself mentioned that his Ninety-Five Theses "almost raced through all of Germany in fourteen days" (Moeller 1972, p. 24). By the end of 1517, or two months after October 31, his writings had appeared in print in Wittenberg, Leipzig, Nuremberg, Landshut, and Basel (The Universal Short Title Catalogue (henceforth "USTC")). By January 1518, his theses had reached Rome and were examined by papal theologians (Wicks 1983, p. 523). In the year 1518, Luther was printed in Leiden (in the Netherlands), Augsburg, and Braunschweig (USTC data). The concentric spread hypothesis implies that places further away from Wittenberg should see Luther's ideas later than places closer to Wittenberg. Since Basel already stood near the border of the Holy Roman Empire and farther from Wittenberg than Augsburg and Braunschweig combined, if any concentric spread of Luther's ideas happened (plausibly, in the immediate aftermath of October 31), it had likely ended by the end of 1517.

More formally, I test the concentric spread hypothesis using data on the printing of Luther's works from the USTC, the most comprehensive database to date of early modern printed materials. Between 1517 and 1530, 63 cities across Europe printed Luther's ideas.[1] I stop at 1530 because after 1530, few new cities started publishing Luther; many territories of the HRE had formally adopted Protestantism by this time, and the Protestant cause had long become one the most controversial issues of the day.

Figure 1, Panel 1 plots the year each city first published Luther vs. the city's distance from Wittenberg and shows no apparent correlation. Table 1 presents regression results of the former on the latter. The baseline regression (column 1) provides no evidence that cities closer to Wittenberg printed Luther earlier. Columns 2-4 add various controls or restrictions: I limit the sample to 1517-26, because in 1526, when princes from all over the HRE attended the Diet of Speyer, Luther's ideas had presumably spread through the nation. I limit the sample to, or control for, cities with a printing press by 1516, because the availability of presses, rather than the availability of Luther's ideas, might have determined whether and when a city printed them. To account for possible differences in diffusion patterns outside the Holy Roman Empire, I also control for whether a city was in the HRE. Finally, since vernacular and Latin publications had different target audiences, publishing in the vernacular might represent a different stage of the spread of Luther than publishing in only Latin. Thus, I control for whether Luther's work appeared in a vernacular language during the first year it was printed in a city. All specifications show no correlation between a city's earliest printing of Luther and its distance from Wittenberg.

If we interpret "concentric circles" as literal, geographical circles, then the regression needs no further controls on city characteristics such as economic prosperity, university presence, and free imperial city status. If "concentric" means "concentric *ceteris paribus*," then

[1] Definition of a "Luther" publication: a printed matter searchable in the USTC with keyword "Luther, Martin." In most cases, Luther wrote the works published. In the few remaining cases, some are reports of Luther's activities or utterances (such as the Leipzig Debate), and some are other people's comments/replies to Luther's writings. If a city's earliest "Luther" publication was a comment/reply, then presumably Luther's ideas had already reached that city.

further controls would be necessary. Note, however, that BW (2009) uses *19th-century* data; its first stage controls only for *19th-century* characteristics of Prussian counties, such as the percentage of population aged below 10, Jewish, or female, and average household size. These factors unlikely affected the spread of ideas in the *16th century*. In addition, certain potentially important city characteristics, such as population size/density and literacy rates, have either unreliable or highly incomplete data for the early modern period (de Vries 1989 on Bairoch's 1988 data; Houston 2014, pp. 125-40 on literacy measures). Nonetheless, when we do control for many available city characteristics that have appeared in recent economic history papers, the results still hold (columns 5, 6). I have also run separate regression for Luther's vernacular and Latin publications to examine the spread of Luther's ideas among the masses and among elite circles, respectively. Appendix B presents those results, which suggest similar conclusions.

Pamphlets: a closer look at information spread among the masses

I now focus on the most popular type of print during the Reformation period: pamphlets. Unlike theological treatises or Bible translations, pamphlets were cheap to produce and purchase, easy to carry around, and understandable even to uneducated minds. Massively popular in the 1520s, they were crucial to the Protestant movement's success. Historians estimate that at least 10,000 pamphlet editions were printed between 1500-30, with an average of 1250 or more copies per edition (Brockmann 1997, p. 125; Chrisman 1982, p. 5; Schoener 2002, pp. 73, 87). The Reformation's early years (1518-30) saw the production of some ten million pamphlet copies (Köhler 1987, p. 337). I compiled data from *Bibliographie der Flugschriften des 16. Jahrhunderts*, a reportedly random and representative sample of about 5,000 editions compiled by Tübingen historian Hans-Joachim Köhler (Köhler 1991, p. IX; Brockmann 1997, p. 9;

Appendix C presents the sample's summary statistics and verifies its representativeness). Köhler records 904 pamphlet editions written by Luther between 1517 and 1530. Their places of printing represent 31 cities across the Empire; almost half (48.45%) of the editions were printed more than 400 km away from Wittenberg and the vast majority (86.5%) in German, the language of the masses. (To put 400 km in perspective, Cologne, near the western border of modern-day Germany, and Augsburg, near the southern border of modern-day Germany, both stand at about 410 km from Wittenberg.) Thus, without considering the numerous other authors and reformers at the time, this sample by Luther alone already attests to the extensive reach and intensity with which Reformation ideas diffused.

Was that diffusion concentric around Wittenberg? Regressing the year each city published its first Luther pamphlet on the city's distance from Wittenberg shows no support for the "concentric spread" hypothesis (results are in Table 2; scatter plot in Figure 1, Panel 2). Absent city characteristic controls (column 1), the negative and significant coefficient estimate for distwitt suggests cities farther from Wittenberg started printing Luther's pamphlets *earlier*; including city characteristic controls (column 2) produces a statistically insignificant estimate.

But we can do better. Köhler's collection permits easy identification of editions belonging to the same title, thus allowing us to track each title's spread across the Empire as it reached the presses in different cities. The 904 editions by Luther in this collection represent 357 titles, 186 of which were printed in multiple cities. Of these 186 "multi-city" titles, I examine whether distance from Wittenberg predicts when a city printed a given title for the first time. I first determine the earliest year a title appeared in each city that printed at least one edition of it during this period. (For example, the pamphlet *Sermo de poenitentia* ("Sermon on penitence") has one edition printed in Augsburg (1518), two editions printed in Basel (both 1518), two editions in Leipzig (1518 and 1519), and one edition in Wittenberg (1518), then its year of first printing (which I will call its "publication year") is 1518 in all four cities.) Then I regress a title's publication year in a city on the city's distance from Wittenberg, first in a pooled specification (Table 2, columns 3 and 4), then with title-fixed effects (Table 2, columns 5 and 6), which allow us to measure whether the same title would be published earlier in cities closer to Wittenberg. All specifications produce either a *negative* and significant coefficient estimate or an insignificant estimate for distwitt. Thus, we have no evidence that cities closer to Wittenberg would publish the same title earlier than cities farther away from Wittenberg. In an earlier month of the same year, maybe (since I only observe publication year, not the exact date), but not early enough to fall in a previous year.

Appendix D presents additional analyses that circumvent the potential shortcomings of OLS, confirming the findings above but highlighting two additional observations. To begin with, a substantial portion of titles were never printed in Wittenberg. For these titles, we cannot speak of any "concentric spread from Wittenberg." Any concentric spread would have centered around the city of printing. Moreover, recall that 400 km from Wittenberg represents a substantial distance, as it already draws near the borders of modern-day Germany. The majority of the "multi-city" titles in the sample were printed on both sides of the 400 km radius; most (83.2%) of which appeared on both sides in the same year. The mean difference in a title's publication year outside and inside the 400 km radius is 0.094 years. That means, about a month later in the "outside 400 km" region. A short time to traverse the Empire indeed.

In sum, the data reveal two things so far. First, Luther's ideas traveled rapidly and widely, usually traversing the Empire within a year. Even if a print originated from Wittenberg, distance would cause little difference in the timing of access. Second, multiple cities often printed, and thus distributed, the same information in large quantities each year, and Wittenberg was not always among them. If the spread of information had been at all concentric, we have no reason to believe it had a single center at Wittenberg.

Before proceeding, we should understand what the printing data can and cannot tell us. For one, the catalogues contain only titles with copies or identifying information surviving. My data do indicate that some titles did not survive.[2] However, survival bias undermines my results only if prints produced closer to Wittenberg had lower survival chances than those produced farther away from Wittenberg. Since places close to Wittenberg were statistically more likely to be Protestant and thus more friendly to Protestant publications, we have no reason to believe this concern is true.

A more serious problem rests with the difference between printing and the availability of ideas. Observing Luther's work printed in a city means Luther's ideas reached that city. Not observing it being printed, however, does not mean Luther's idea never arrived there. Books were traded over long distances, and itinerant merchants and travellers carried news, ideas, and print media with them as they moved from city to city (Künast 2011). In addition, some historians have argued that reformation ideas spread mostly orally, through informal conversations, reading printed materials out loud in public, and preaching (Scribner 1984). None of these modes required the ideas to be printed in the destination city. Thus, print productions provide only an upper bound for when Reformation ideas reached a city and a lower bound for how far they spread each year.

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[2] For example, in 1521, English bishop John Fisher published "The sermon made agayn the pernicious doctryn of Martin Luther." This is the earliest English publication associated with Luther searchable on the USTC (with "Luther" as keyword), but the title suggests that readers in England had access Luther's ideas before this date. Fisher's writing in English instead of Latin also suggests Luther's idea was likely already known to readers beyond erudite circles, which means considerable spread. So the publication date indeed only provides an upper bound. (However, in this case, the non-survival of an earlier publication biases the coefficient for distwitt upward, because London is farther from Wittenberg than most places in the sample. I am not aware of similar cases for other cities.)

We will probably never know when Protestant ideas first arrived in a city through informal channels.[3] However, unobserved informal spread will challenge my regression results only if 1) it was concentric from Wittenberg *and* 2) it reached places faster than the "formal" spread that I observe. If it was not concentric, that would confirm, rather than undermine, my previous findings; if it happened slower than the "formal" spread, then the earliest date Luther's idea reached a place would still be my observed date, also not changing my findings.

We have little reason to expect either condition 1) or 2) to hold. Since the formal spread was not concentric, we have no reason to expect a concentric informal spread, especially beyond the immediate surroundings of Wittenberg. An informal spread that happened faster than our observed "formal" spread adds to the argument that information spread so quickly, such that distance, travel costs, and language differences were unlikely to have been significant obstacles. Indeed, historian Robert Scribner observes that "[a] feature of the German Reformation which has always fascinated historians is the speed with which new religious ideas were so widely spread throughout Germany, precipitating within a few months what became one of the major social and intellectual upheavals in European history" (Scribner 1984, p. 237).

In all, distance did not seem to have hindered the spread of Reformation ideas, and cities that encountered Luther's ideas earlier only did so marginally than those who received them later. Appendix E discusses travel time, a natural alternative to great circle distance. Given the available information on early modern transportation, travel time from Wittenberg should also not have affected a territory's decision to adopt Protestantism, as even the slowest transportation

[3] Cantoni (2012), which examines predictors for a city's adoption of Protestantism, finds no correlation between the earliest arrival year of a Protestant preacher and a city's distance from Wittenberg. But preachers' arrivals usually responded to existing demands for hearing Reformation ideas (see, for example, Blickle (1992): "The demand of the 'Gospel' was first of all an appeal for preaching in accordance with the Scripture" (p. 74). Also see the examples of Erfurt (p. 65) and of Kitzingen (p. 68).) Thus, while a preacher's arrival might have signalled Protestant ideas' gaining popular ground in a city, it unlikely reflects their earliest arrival. (Nor might it have a systematic relationship with the earliest arrival, such as equaling the earliest arrival plus a constant.)

could traverse the Empire in months, while the first territories to officially turn Protestant did so only around 1525, by which time Luther's ideas had long penetrated the Empire, not least because 27 cities had published at least 778 Luther's pamphlet editions.

Did distance matter for total access?

Still, could places closer to Wittenberg have had more *total* access to Protestant ideas, even though they did not have *earlier* access? This question escapes the purview of observable data. In addition to the survival, trade, and unobserved informal spread issues mentioned above, two problems exist. First, total access, which includes both printed and oral media, is difficult to quantify. For example, we cannot compare "total access" between a city with more printed medium but little oral medium and another city with more oral medium but little printed medium. Second, the local availability of a medium might not indicate the local *consumption* of that medium. With printed materials, for example, we can observe the number of titles or editions printed, but often not the number of copies in each city, or how many people each copy reached. On the one hand, early modern printed materials were shared between friends, re-sold, or read aloud in public; on the other hand, historians have doubts about whether and how often people actually read the books they owned (Houston 2014, p. 127; Ogilvie et al. 2022, p. 17). Moreover, due to varying literacy rates and living standards, even if we knew the title-to-consumer ratio for one city, we still could not generalize and compare across regions.

Nonetheless, the evidence we do have suggests little correlation between distance from Wittenberg and total access to Reformation ideas. Using an older source, the British Museum's Short Title Catalogue from 1962, Cantoni (2012) finds no correlation between the number of Luther's works printed and a city's distance from Wittenberg. I consider pamphlets, which might better reflect the availability of Protestant ideas to the masses. Using Köhler's sample, I test whether cities closer to Wittenberg printed more pamphlet editions or titles by Luther between 1517 and 1530. Among the 31 cities that printed at least one pamphlet edition by Luther, I regress the total number of editions or titles a city printed on its distance from Wittenberg. The concentric spread hypothesis predicts a negative and significant coefficient estimate. Results using various specifications (Table 3, columns 1-8), however, all show insignificant estimates.

Did cities closer to Wittenberg print more editions *of the same title*? I create a balanced panel where the outcome variable y_{ik} equals the number of editions city i printed for title k. I then estimate $y_{ik} = \alpha + \beta\ distwitt_i + \vec{\gamma}\ controls + \epsilon$ with title fixed-effects, where $distwitt_i$ is city i's distance from Wittenberg. The regression produces a robust negative correlation (Table 3, column 9). However, this result seems to depend on a small number of titles; most titles show no support for the concentric spread hypothesis. In addition, when I exclude all editions printed in Wittenberg (about 25% of all editions) from the sample, the correlation reverses, meaning that cities farther away from Wittenberg printed *more* editions of each title (Table 3, column 10). The closest city to Wittenberg in Köhler's dataset, Leipzig, stood about 65 km away. Therefore, while proximity to Wittenberg might have improved access to Luther's ideas in the immediate vicinity of Wittenberg, this relationship does not hold for the rest of the Empire. Furthermore, the result for "distance from Wittenberg" holds for "distance from Vienna," the seat of the staunchly Catholic Habsburgs, as well (Table 3, columns 11). Thus, what the regression coefficient really shows is unclear. (Appendix F simulates a model of spread that accounts for the potential long-distance movement of printed materials, with similar results.)

We will find even less support for the "concentric spread from Wittenberg" hypothesis if we consider not just Luther, but all Reformation authors. Non-Wittenberg-based authors and reformers likely outnumbered their Wittenberg-based colleagues. We cannot expect the ideas of any non-Wittenberg author to have diffused from Wittenberg. Of the ten leading reformers who attended the Marburg Colloquy (1529) and signed the resulting articles, seven were not based in Wittenberg.[4] As for lay authors of Reformation pamphlets, Chrisman (1996) concludes that, "Lay pamphlet writer centered on southwest Germany, Franconia, Alsace, and the Rheinish Palatinate" (p. 11). None of these places were close to Wittenberg. By output, for pamphlets by all authors between 1517 and 1530, historians agree that Augsburg led the production by far (Künast 2011, esp. pp. 329, 333; Thomas 2022; Appendix G provides my own tally based on UTSC data). The concentric spread hypothesis also weakens if we consider the widespread practice of false imprints (the cover of a print stating a false place of production, printer, year, etc.), which skewed the location data of printed materials towards Wittenberg (Thomas 2019, 2021, 2022; more explanations in Appendix G).

To sum up, historical evidence does not support the assertion, at least for most of the HRE, that places closer to Wittenberg had either *earlier* or *more* access to Protestant ideas in the first decade of the Reformation.[5] Proponents of the "concentric spread" hypothesis likely overestimated the role of travel costs in early modern information diffusion. Also, Protestant ideas had become available nationwide long before any territory officially adopted Protestantism. Thus, even if distwitt affected the diffusion of Protestant ideas, information availability might not have played a decisive role in the adoption of Protestantism.

[4] Andreas Osiander, Setphan Agricola, Johannes Brenz, Johannes Oecolampadius, Ulrich Zwingli, Martin Bucer, and Caspar Hedio

[5] Appendix H explains in more detail why investigating the "concentric spread" hypothesis beyond 1530, when Luther's ideas had long penetrated the Empire, might not be meaningful.

Distance from Mainz and the Spread of Printing Technology

The issues above extend beyond the Reformation and Wittenberg. A series of influential papers by Dittmar, Rubin, and co-authors (2011, 2014, 2021, forthcoming), for example, use "distance from Mainz" to provide exogenous variation for the adoption of printing technology by 1500, with similar reasoning to BW's. In Appendix I, I investigate the relationship between distance from Mainz and the development of the printing industry. Again, historical data show no evidence that distance from Mainz constrained the spread of printing technology. By 1480, printing presses had spread to more than 120 cities in 12 modern-day countries across Europe. The local/regional availability of this technology was already so high that we have no reason to suppose distance from Mainz mattered for the adoption of printing *as late as 1500*. In addition, just as the early availability of Protestant ideas might not have affected a territory's decision to become and to remain Protestant, the early presence of a printing press does not predict whether a city's printing industry thrived or even continued in the long run.

3. *Why was the first stage statistically significant?*

If we have no reason to expect distance from Wittenberg (or distance from Mainz) to shape the spread of Protestant ideas (or printing technology), then why do the IV first stage and OLS regressions show a statistically significant correlation between distwitt (Mainz) and the adoption of Protestantism (printing)? Cantoni (2012) finds distance from Wittenberg an especially consistent predictor for the 16^{th}-century adoption of Protestantism. Also doubting the role of information availability, Cantoni proposes the following alternative explanation: distance from Wittenberg is significant because Wittenberg was the capital of Electoral Saxony, one of the first official adopters of Protestantism. A territory's adoption of Protestantism became more likely when its neighbors had already done so, because a Protestant neighbor would provide military alliance against the (Catholic) imperial forces, and a neighbor's institutional reforms would provide a model for similar reforms. Thus, Saxony's support for Protestantism made its neighbors, then its neighbors' neighbors, etc. more likely to adopt Protestantism, propagating Protestantism from Saxony outward.

This reasonable-sounding explanation, however, has a few drawbacks. First, Electoral (i.e., Ernestine) Saxony territorially intertwined with Albertine Saxony, a stronghold of German Catholicism which led the Empire in anti-Lutheran policies as well as Catholic propaganda in the 1520s, and a formidable military threat that allied with the (Catholic) Emperor in the Schmalkaldic War (Volkmar 2011). (See the map in Appendix Figure J1 for the two Saxonies' territories.) Thus, simultaneous proximity to Albertine Saxony probably offset any advantages Electoral Saxony's neighbors enjoyed. Second, although Electoral Saxony adopted Protestantism early, several other territories did so around the same time or earlier (depending on what actions qualify as official adoption).[6] Therefore, we have no reason to favor distance from Saxony over distance from any of these territories. (Indeed, Appendix J replicates Cantoni's regressions that support his hypothesis with "distance from" a number of cities *outside* Saxony replacing "distance from Wittenberg" and finds similar or even stronger results than Wittenberg. Saxony's role in the Reformation could not possibly explain these results.) Third, under Cantoni's explanation, the correct instrument would not be distance from *Wittenberg*, but distance from Saxony or a neighbor's adoption of Protestantism. Neither alternative satisfies the exclusion restriction. While Wittenberg might have been otherwise economically unremarkable in German

[6] Saxony officially became Protestant somewhere between Frederick the Wise's death in 1525 and the official church visitations in 1527, while Prussia turned Protestant in 1525 and the Landgraviate of Hesse, 1526. Sources: Wengert and Haemig, pp. 201-212. Hoffmann-Dieterich, pp. 58, 80. Krause and Müller, entries "Hessen", "Philipp von Hessen, Landgraf (1504-1567)", "Preußen."

history, this condition fails for Saxony, a major political power with historically prosperous cities along trade routes, and a key strategic possession at the Congress of Vienna (Pallain 1881). (More on this point in section 4.) A neighbor's adoption of Protestantism also violates the exclusion restriction because a neighbor's adoption and subsequent economic development could clearly influence a territory's economy through channels other than the territory's own confessional choice.

I propose a different explanation for the first-stage significance of distwitt and distMainz: spatial autocorrelation. Due to the geographical nature of "distance from" variables, Wittenberg and Mainz did not need *factual* significance to generate *statistical* significance. This finding is analogous to Kelly (2019)'s argument for "persistence" regressions. Below, I first replicate first-stage/OLS regressions involving distwitt and show that distances from many other cities yield similar or stronger correlations with Protestantism than "distance from Wittenberg" does. Thus, distwitt's statistical significance does not imply Wittenberg was special but likely reflects mere spatial correlation. Then I use Monte Carlo simulations to show that more generally, "distance from location X" frequently appears statistically significant even when X has no role in the spread of an event. This property explains distwitt's statistical significance in first-stage and OLS regressions and causes concern about using similar variables in such regressions. As sections 4 and 5 will show, it also creates issues for the second stage.

First, I describe the replications. Cantoni (2012) regresses the binary variable "Protestantism by 1600" on an array of possible predictors of adoption in an OLS specification and find distwitt to be consistently significant. I re-run these regressions, but with "distance from" each of 25 alternative cities/locations replacing "distance from Wittenberg." Figure 2 plots coefficient estimates for Cantoni's fully controlled city-level regression (Cantoni's Table 4, column 5). Remarkably, distances from 24 of these 25 cities were statistically significant: 21 at the 1% level, 2 at the 5% level, and 1 at the 10% level, all with negative coefficient estimates.

Among these 25 locations, some, such as Augsburg (a major imperial city, commercial center, and early leader of Protestant printing), Worms (where the Imperial Diet of 1521 condemned Luther but publicized his cause throughout the Empire), Memmingen (birthplace of the peasants' Twelve Articles), or Rome (the papal seat), had historical significance in the Reformation and possibly either aided or hindered its spread.[7] Others, like Frankfurt, Dresden, or Berlin, did not necessarily matter for the Reformation but had other geopolitical importance in the 16th or the 19th century. The rest consist of entirely arbitrary "placebos": an arbitrary point in the Black Forest, the Lorelei, Karl Marx's birthplace in Trier, and the random city of Neumuenster. One cannot think of a substantive explanation for why distance from almost all these locations mattered for the adoption of Protestantism. In addition, both distance from Vienna and distance from Rome have *negative* and significant coefficient estimates just like distance from Wittenberg does. Vienna was the seat of the adamantly Catholic Habsburg dynasty, and Rome, the heart of the Catholic Church. It is difficult to believe proximity to these places *increased* the chance of adopting Protestantism.

Moreover, when we standardize the regression coefficients (results in Figure 2, Panel 2), proximity to Rome apparently has the second strongest effect on adopting Protestantism, in fact, more than 3 times the effect of proximity to Wittenberg, which ranks merely 23rd out of the 26 locations. (Appendix K shows that replicating Cantoni's territory-level analog of the above regressions (his Table 2) yields similar results. 21 of the 25 alternative locations appear

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[7] Other cities in this category are Leipzig, where Luther unsuccessfully debated Catholic theologian Johann Eck (1519); Wartburg Castle, where Luther was abducted to after the Diet of Worms and where he translated the Bible into German; Erfurt, where Luther went to university and was ordained. Muenster, the failed Anabaptist kingdom turned Catholic stronghold; and Konstanz, where the Luther's Bohemian predecessor Jan Hus was burned alive in 1415.

statistically significant in the fully controlled specification.)

A potential problem with the fully controlled specification above, and with using "distance from" in linear regressions in general, concerns whether a regression can include simultaneously latitude, longitude, and distance from a point (such as Wittenberg) as independent variables. Certainly, latitude and longitude can represent factors that work independently from distance from Wittenberg: the former two can represent geographical conditions, and the latter, the influence of a given point. However, latitude and longitude alone already determine an observation's distance from Wittenberg. The regression coefficient for distwitt equals the expected change in the outcome when we hold latitude and longitude constant and vary distance from Wittenberg. But doing so is impossible. So what the coefficient for distwitt means is unclear. An imperfect remedy drops either latitude or longitude. Since Cantoni finds latitude statistically significant, I drop longitude, to similar replication results. Another solution replaces either latitude or longitude with a dummy variable for being east or west (or north/south) of Wittenberg (or the location of the instrument). I replace longitude with an east/west indicator, also to similar results (see Appendix K).

In all, the replication exercises show that Cantoni's OLS/first-stage results hold for "distance from" many other cities, some with even stronger results, than "distance from Wittenberg." These exercises function as placebo tests that imply Wittenberg might not be as special as we assumed. Replications of BW (2009)'s first stage are shown later together with second stage results. We will see that BW's first stage remains significant with a considerable number of these "placebo locations." However, Cantoni's first stage is more meaningful because first, Cantoni uses "Protestant by 1600" as the endogenous variable which measures the adoption of Protestantism, while BW (2009) uses Protestant population share in a county in the *19th*

century. Since distance from Wittenberg could only have affected the adoption of Protestantism in the *16th century*, BW (2009) assumes 19th-century Protestant population shares to be proportional to their 16th-century counterparts. That is a strong assumption. In the city of Augsburg, for example, the Lutheran-Catholic ratio was 7-3 in 1645, achieved parity by the 1750s, and the Lutherans became a minority soon after (Plummer and Christman 2018, p. 187). Austria once "swarmed with Protestants" until Emperor Rudolf II started suppressing Protestant worship in 1578, but only after 1620 did the Protestant population start to dramatically decline (Kaplan 2007, Ch. 6). Second, Cantoni's data include all of Germany, while BW (2009)'s, only 19th-century Prussia.

Monte Carlo Simulations

The replications so far suggest a deeper econometric problem: the regressor "distance from X" can appear statistically significant even when it has no plausible effect on the outcome. I now study this phenomenon more generally with Monte Carlo simulations. Each simulation exercise below asks, if distance from a point X has no factual role in determining an outcome, how likely will OLS regressions produce "false positive" results that suggest distance from X was correlated with that outcome?

First, suppose an outcome of interest, such as a territory's likelihood of adopting Protestantism, is unrelated to distance from a city (which I will call "False Center") but is entirely determined by distance from another city ("True Center"). How likely will my regression find "distance from False Center" statistically significant? That is, if

$$P(adoption) = 1 * \ distance\ from\ True\ Center \qquad (1)$$

represents the true underlying relationship, but I estimate

$$P(adoption) = \alpha + \beta \; distance \; from \; False \; Center + \epsilon \quad (2)$$

how often will the regression produce a statistically significant $\hat{\beta}$ (which rejects $\beta = 0$)?

Strictly speaking, the coefficient in front of "distance from True Center" in (1) could be any nonzero real number (whose sign depends on whether True Center aided or hindered the spread of Protestantism, and whose magnitude ensures $P(adoption) \in [0, 1]$). But we can assume it to be 1 without loss of generality, because varying this coefficient simply means scaling β by a (nonzero) constant in equation (2). The scaling does not change whether $\beta = 0$, the only thing we care about. Also, for simplicity and generalizability, suppose that we observe a territory's exact chance of adoption instead of the binary outcome adoption vs. no adoption.

Let the unit square in $\mathbb{R}^2$ centered at the origin represent Germany. On this square, I randomly draw a pair of (x, y) coordinate values representing a True Center and another pair representing False Center. This randomization will show that the result is not driven by any specific coordinate choices of True Center and False Center. The outcome $P(adoption)$ is a function on the entire "Germany", but the researcher can only observe its value at a finite number of coordinate locations (e.g., a finite sample of cities, territories, etc.). Each Monte Carlo trial randomly draws 1,000 observation locations and tests whether regressions based on these observations produce falsely significant $\hat{\beta}$'s. That is, I randomly select a sample of 1,000 points in "Germany," calculate their $P(adoption) = distance \; from \; True \; Center$ according to equation (1), then regress $P(adoption)$ on $distance \; from \; False \; Center$ as in equation (2). To make sure results are not driven by the choice of observation locations, I conduct 1000 Monte Carlo trials, drawing a random sample of 1000 observation locations each time. Then I repeat this procedure with 99 other pairs of randomly drawn locations for (True Center, False Center). In sum, I consider 100 different pairs of (True center, False center) and perform 1000 experiments on each. In these 100*1000 trials, $\hat{\beta}$ is significant at the 5% level 94.30 percent of the time and significant at the 1% level 92.35 percent of the time.[8] (Table 4 summarizes these simulation results as well as results from the rest of this section.) That means the regression almost always produces false positive results.

These results do not depend on the simulation model's parameters. The size and location of the square that represents Germany in $\mathbb{R}^2$ do not matter, as re-sizing and re-locating the square simply mean scaling the regression coefficients and changing the constant term in the linear regression equation. (Appendix L shows that the shape of "Germany" also does not matter.) The Monte Carlos demonstrate that the abundant statistically significant results in the replications for Cantoni (2012) are not a coincidence of German geography or Reformation history but reflect a general statistical property. Given an arbitrary geographical region (Germany or any other country/region), a historical process might have a true center. If so, distance from some other city (e.g., Wittenberg) will likely exhibit strong, false statistical significance even if that city has no role in the outcome.

This insight extends to cases where only one true center exists, but we use distances from two falsely identified centers as regressors.[9] Concretely, suppose the adoption of Protestantism in a city depended on distance from Vienna, but I regress adoption on distance from Wittenberg *and* distance from Augsburg. In such cases, the coefficients on distance from both False Centers will be statistically significant at the 1% level about 96 percent of the time (see row 2 of Table 4), even though neither False Center has a role in the true story. The situation improves if we

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[8] I use robust standard errors. I conducted these simulations in Matlab. I consider a coefficient estimate to be statistically significant if absolute value of the point estimate > 10^{-4} and the t-statistic is above 1.96 for the 5% significance level and 2.576 for the 1% significance.

[9] That means the true model is $outcome = distance\ from\ True\ Center$, but I regress $outcome = \alpha + \beta\ distance\ from\ False\ Center\ 1 + \gamma\ distance\ from\ another\ False\ Center\ 2 + \epsilon$

know the true center. Suppose that I regress the outcome on “distance from False Center” *and* “distance from True Center” (and a constant term). Concretely, suppose adoption reflects distance from Vienna, and I regress it on distance from Wittenberg *and* distance from Vienna. The Monte Carlos imply that the distance from True Center (Vienna) will always be significant, and distance from False Center (Wittenberg) will always be insignificant (row 3 of Table 4).

This result is less comforting than it appears. We might now think that although regression cannot confirm whether a supposed center is factually significant, it can rule out those that are not: thus far we have seen false positive results but not false negative results. But this intuition misleads. Consider as the true model $outcome = dist\ from\ True\ Center + dist\ from\ another\ city$. To be concrete, suppose the True Center means Vienna and the other city means Berlin (the capital of Prussia). The outcome, 19th-century Protestant population share, reflects the combined influences of Vienna in the 16th century and of another city, say Berlin in the 19th century. If the regression uses Wittenberg (incorrectly) as the true center and Berlin as the other city, the two regression coefficients will be significant at the 1% level 93.00 percent and 97.10 percent of the time, with average point estimates of 0.1172 and 1.2831, respectively (row 4 of Table 4). Here, not only is Wittenberg falsely significant most of the time, Berlin is occasionally *falsely insignificant*. Moreover, the ratio between the coefficients of the two “distance from” variables is wrong. If we intend to compare the influences of two forces (in this case, of the Reformation, thought to be emanating from Wittenberg but actually emanating from True Center, and of some 19th-century political influence emanating from Berlin), the result would be misleading.

Another plausible model for the spread of early modern ideas posits not one, but multiple centers. The Reformation or printing technology might have arrived first in a few large cities, from which it radiated outward into smaller cities and the countryside, making the likelihood of adoption proportional to distance from the nearest large city, or "center" (Hamm 1996, p. 23; Moeller 1991, p. 117). (In other papers, "distance from the nearest center" could mean distance from the nearest university, from the nearest trading post or mission, from the nearest port, coalfield, etc.) I repeat the simulations above, but draw multiple "True Centers" instead of one in each trial.[10] The results (rows 5-10 of Table 4) show that "distance from a randomly chosen False Center" is still likely to appear statistically significant even if the true spread was *highly* decentralized: when the true spread has as many as 50 centers, "distance from False Center" will still be significant at the 1% level 60.22 percent of the time.

Since exchanging the explanatory and response variables in a linear regression preserves the statistical significance of estimates, the converse of the above also holds. If the true spread has one center but researchers mistakenly believe it has multiple—say, some outcome is related to distance from Berlin, but researchers think it is related to distance from the nearest university—they would very likely get falsely significant coefficients that confirm the role of multiple centers. A decrease in false positives under Conley standard errors suggests the spatial nature of the variables as the cause of false correlations (Appendix L). But Conley provides only partial relief; when the underlying model contains few true centers, false positive rates under Conley remain high.

Furthermore, the true story needs not involve any concentric diffusion at all. Suppose the outcome is entirely determined by a variable that is not "distance from" but still exhibits spatial continuity: for example, an indicator for "west of a certain river" or "German-speaking." Then distance from a randomly chosen location ("Wittenberg") is still likely to be significant (rows

[10] That means the true model is $outcome = distance\ from\ the\ nearest\ True\ Center$, but I regress $outcome = \alpha + \beta\ distance\ from\ False\ Center + \epsilon$

11-14 of Table 4).

In all, this section studies why distance from Wittenberg appears statistically significant in first stage IV and OLS regressions despite lacking a historical reason to do so. Replication exercises in the first half of this section show that the statistical significance of distwitt might not reflect anything special about Wittenberg, as placebo tests with other cities produce similar or even stronger results. The simulations suggest that this problem likely owes to the spatial nature of "distance from" variables and merits general concern: "distance from" very likely appears statistically significant in linear regressions even if the outcome had a different center, multiple centers, or simply shows continuity over space. Thus, when interpreting OLS regression results involving "distance from" variables, *statistical* significance does not imply *factual* significance, and cross-variable comparisons of coefficient estimates (i.e., "horse races") might yield unmeaningful results. These findings suggest caution for including and interpreting "distance from" variables in OLS regressions.

4. *Implications for the Second Stage: Exclusion Restriction*

So far, we have examined first stage and OLS regressions. But when using "distance from" as an instrument, qualitative conclusions depend on the second stage results. How do my findings thus far impact the second stage? The exclusion restriction poses an immediate issue. It requires the instrument to be unrelated to the outcome except through its connection with the endogenous variable. In this case, distance from Wittenberg should be uncorrelated with German economic development except through its association with Protestantism. An assumption about the true, underlying story, the exclusion restriction is not testable in any way. But here we have reasons to suspect its violation. Since factually unrelated concepts involving "distance from" often exhibit statistical correlation, what it means for distance from Wittenberg to be "uncorrelated" with another term is no longer clear. Distance from Wittenberg could appear statistically correlated with many variables, any of which could impact economic development.

For example, in BW's sample and in general, distance from Wittenberg is highly correlated with distance from Berlin.[11] Distance from Berlin clearly mattered for 19th-century German economic development. Thus distance from Wittenberg could be correlated with economic development through its connection with Berlin. If we control for distBerlin in the IV specification (intuitively, trying to "remove" the part of distwitt that is correlated with distBerlin), distwitt ceases to have a valid first stage, and Protestantism no longer appears to increase literacy.

A similar problem concerns Saxony. Distance from Wittenberg is naturally correlated with distance from Saxony, which had importance in German economic development for reasons unrelated to Protestantism. Thus, distance from Wittenberg could also violate the exclusion restriction through Wittenberg being a part of Saxony. We clearly cannot control for distance from Saxony while using distance from Wittenberg as an IV, as Wittenberg lies inside Saxony.

5. *Second-stage results are unstable*

A second issue for the second stage concerns the variability of qualitative results when choosing different cities as the instrument. Table 5 reports replication results of BW's first and second stages (BW's Table III: Protestantism's impact on literacy, and Table V: Protestantism's

[11] In BW's sample, regressing distance from Berlin on distance from Wittenberg yields a coefficient estimate of 0.8925, a t-statistic of 45.33 (robust standard errors), and an R^2 of 0.8203. The 95% confidence interval is [0.8539, 0.9313]. BW do control for (log) distance from Berlin in their Table II, but not in their IV regressions. Table II uses OLS regressions to show that the observed association between 19th-century Protestantism and literacy is robust to controlling for various variables. The IVs, however, underlie their main, causal argument, that Protestantism increased prosperity through expanding literacy and schooling.

impact on economic prosperity). A recent literature by Lee et al. (2020) argues that the appropriate F-statistic for a strong instrument exceeds 100. Then Wittenberg, with an F-statistic of 74, represents a weak instrument. If we take the older threshold of 10 instead (from Stock and Yogo 2005), then of the 25 candidate cities/locations, 13 have a first-stage F-statistic close to or above 10 and can thus be considered strong instruments. These cities' second-stage results, however, imply varied conclusions about Protestantism's effect on literacy and economic prosperity. BW's instrument, distance from Wittenberg, suggests Protestantism led to both higher literacy rates and more economic development. However, distance from Rome or Geneva suggests Protestantism led to lower literacy rates and worse or no economic development (though distance from Rome is now *positively* correlated with share Protestants in the first stage). Using Vienna, Kolberg, Graz, or Münster as the instrument implies Protestantism had no effect on literacy rates and had mixed effects on economic prosperity. Across many cases, BW's second measure of literacy (in addition to their direct measure, reported literacy rates), distance from school, shows especially noisily results. Also curious is the fact that places close to Wittenberg or inside (the post-1547, unified, and Protestant) Saxony, such as Berlin, Dresden, and Leipzig can produce qualitatively different results about Protestantism's impact on economic prosperity. Other places produced qualitatively similar regression coefficients, but with magnitudes several times the coefficients produced by Wittenberg.[12]

Since historical evidence does not suggest distance from Wittenberg played a special role in spreading Protestantism, we have no reason to prefer "distance from Wittenberg" to "distance from any other city" as an instrument. Then, which instrument should we rely upon, if different

[12] Note that scholars differ widely on what constitutes a valid instrument and valid 2SLS practice. Young (2022) finds judging an instrument's validity by the first-stage F-statistic is unhelpful; 2SLS estimates rarely outperform OLS results. If we take this view, then most IVs, including "distance from" IVs, are futile.

candidates lead to contradictory conclusions? If we take a more stringent view of instrumental variables that require a causal relationship in the first stage, then of course "distance from" none of the candidate locations, including Wittenberg, satisfies this criterion.

6. Conclusion

This paper shows that "distance from Wittenberg" and its derivative "distance from Mainz," two instrumental variables that underlie influential findings in economic history, suffer from substantive as well as econometric shortcomings. Historical records on the dissemination of Protestant materials and the early spread of printing technology challenge the view that distance from Wittenberg (Mainz) was correlated with access to Protestantism (printing technology). Thus, we have no reason to expect a valid first stage. Replication exercises show that distances from many other cities show similar first-stage correlations with Protestantism as "distance from Wittenberg" does, thus Wittenberg's statistical significance in the first stage does not imply Wittenberg is special but likely reflects spatial autocorrelation. Simulations confirm that "distance from X" often produces falsely significant results when the outcome in fact has a different or multiple centers. These statistical properties of "distance from" create two problems for the IV second stage: violation of the exclusion restriction, and multiple equally valid/invalid instruments producing conflicting qualitative conclusions.

Although this paper focuses on distances from Wittenberg and Mainz, its findings have broader implications. First, information travelled much faster in early modern Europe than conventional understandings suggest. Distance and travel costs unlikely posed significant barriers to the spread of ideas and technologies. Thus, we have no reason to assume concentric diffusion. Nor should we assume any correlation between the availability, adoption, and long-term development of an idea or technology. To study why new ideas and technology took hold and prospered in certain but not others, we should perhaps look for answers other than availability or the coincidence of geography. This reasoning also extends to later periods, where means of information diffusion had become even faster, cheaper, and more plentiful.

Econometrically, we should exercise caution when including “distance from” variables in regression analysis. Distance from a single location or from the nearest of multiple locations makes unreliable OLS regressors and instruments. In OLS and first-stage regressions, “distance from” variables often produce falsely significant estimates for their own coefficients and could also lead to false negative results for other variables in the regression equation. As instrumental variables, the spatial nature of these variables likely violates the exclusion restriction and can create misleading qualitative conclusions in the second stage.

For the reasons above, distance from the birthplace of an idea or technology might not be a good instrument for the adoption of that idea or technology. Since other mechanisms of information diffusion, such as networks, are clearly not exogenous, I refrain from proposing an alternative instrument. Conditions, if any, under which papers could safely include “distance from” instruments and variables, invite further research. At the very least, scholars still wishing to utilize such instruments should provide ample factual, not just statistical, justification, that 1) distance indeed posed a significant barrier in the spread of an idea or technology, and, if the paper studies the idea’s long-term impact, 2) either the barrier remained long after the idea’s invention, or the idea’s early availability and adoption mattered for sustained long-term subscription. With the results these instruments produce, researchers should conduct placebo tests whenever possible and exercise caution when drawing substantive conclusions.

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Table 1: Year of First "Luther" Publication vs. Distance from Wittenberg

Dependent variable: year of first printing of Luther. Unit of analysis: city

	(1)	(2)	(3)	(4)	(5)
distwitt	0.00166	-0.00149	0.00224	0.00204	0.00231
	(0.00238)	(0.00159)	(0.00241)	(0.00187)	(0.00744)
city was in the HRE			-1.925	-2.065	
			(1.749)	(1.311)	
Luther's work appeared in vernacular that year			2.620***	2.707***	2.373
			(1.017)	(0.942)	(2.727)
printing press by 1516				-1.797**	
				(0.736)	
printing output 1500-1516					0.000659
					(0.00433)
Constant	1,521***	1,521***	1,520***	1,522***	1,522***
	(0.900)	(0.759)	(2.432)	(1.835)	(6.124)
Observations	63	56	49	63	34
R-squared	0.015	0.022	0.211	0.286	0.483
Restrict sample to cities with a press by 1516	No	No	Yes	No	No
Sample timespan	1517-1530	1517-1526	1517-1530	1517-1530	1517-1526
Additional controls					Yes
SE of distwitt calculated with 200 bootstraps	(0.00205)	(0.00132)	(0.00238)	(0.00226)	(0.00757)
SE of distwitt calculated with jackknife	(0.00219)	(0.00137)	(0.00274)	(0.00230)	(0.00597)

Standard errors (in parentheses) are computed using bootstrap with 50 repetitions unless otherwise specified
*** p<0.01, ** p<0.05, * p<0.1

Notes: Regression (5) includes the following additional controls, most of which are from Cantoni (2012): river presence, log population in 1500, a dummy for whether the city's 1500 population was imputed by Cantoni, city population growth 1300-1500, a dummy for whether growth was imputed by Cantoni, university presence, age of the city, whether the city belonged to an ecclesiastical territory, monasteries per capita, free imperial city status, Hanseatic League membership, and Augustinian monasteries per capita. Due to space constraints, these estimates are not shown here, but none of them is statistically significant.

Sources: USTC for the year of first publication, whether a city printed Luther's work in a vernacular language that year, whether the city had a printing press by 1516, and the city's print output (by the number of editions) between 1500-1516. Cities' distances from Wittenberg are calculated using geographical coordinates provided by Google. I determined the boundary of the HRE by cross-checking Breasted et al. (1947), Bryce (1978), and Poole (1896), regarding entities that nominally belonged to the HRE but might have been autonomous, such as the Swiss Confederation and the Duchy of Savoy, as inside the HRE in the early 16th century. Other city characteristic variables and data are from Cantoni (2012). Whenever a city in my dataset is not in Cantoni's dataset, I consulted Cantoni's sources and manually entered values. I also added the following additional variables: an indicator for whether the city population at 1500 is imputed (Cantoni imputed missing values in Bairoch with 1); an indicator for whether the city population growth between 1300 and 1500 is imputed (for cities with either 1300 population or 1500 population missing in Bairoch, Cantoni imputed the mean growth among other cities).

Table 2: Did cities closer to Wittenberg print Luther's pamphlets earlier?

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Var	earliest year a pamphlet by Luther was printed	earliest year a pamphlet by Luther was printed	year of publication	year of publication	year of publication	year of publication
unit of analysis	city	city	edition	edition	edition	edition
distwitt	-0.00674** (0.00329)	-0.00344 (0.00610)	-0.00108* (0.000596)	-0.00624*** (0.00172)	0.000197 (0.000127)	-0.000222 (0.000482)
Constant	1,524*** (1.428)	1,524*** (2.818)	1,523*** (0.233)	1,524*** (0.728)	1,523*** (0.0523)	1,523*** (0.180)
Observations	31	31	541	541	541	541
R-squared	0.145	0.687	0.006	0.164	0.980	0.980
title FE	N/A	N/A	No	No	Yes	Yes
control for city characteristics?	No	Yes	No	Yes	No	Yes
s.e. calculation method	bootstrap with 50 reps	bootstrap with 50 reps	OLS Robust	OLS Robust	OLS Robust	OLS Robust
s.e. using bootstrap with 200 reps	(0.00371)*	(0.00722)	(0.000608)*	(0.00195)***		
s.e. using jackknife	(0.00396)*	(0.00727)	(0.000597)*	(0.00183)***	(0.000158)	(0.000558)

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: city characteristics controls are river, log population 1500, log population 1500 imputed (dummy), growth 1300-1500, growth imputed (dummy), university, free imperial city, Hanseatic, territory ecclesiastical in 1500, Augustinian monasteries p.c., and print output 1500-1516
Note that the concentric spread hypothesis predicts the coefficients for distwitt to be positive and significant.

Sources: Pamphlet data from Koehler (1991), *Bibliographie Der Flugschriften Des 16. Jahrhunderts*. City characteristics data are from the same sources as in Table 1.

Table 3: Did cities closer to Wittenberg publish more Pamphlet editions or titles by Luther?

The unit of analysis is city in columns (1)-(8); city x title in columns (9)-(11). Sample timespan in all regressions: 1517-1530.

Dependent var	(1) no. of editions	(2) no. of editions	(3) no. of editions	(4) no. of editions	(5) no. of titles	(6) no. of titles	(7) no. of titles	(8) no. of titles	(9) # editions for city x title	(10) # editions for city x title	(11) # editions for city x title
										(drop Wittenberg's output)	
distwitt	-0.0368 (0.0877)	-0.123 (0.164)	-0.000869 (0.00308)	-0.00454 (0.00599)	-0.0164 (0.0630)	-0.0925 (0.118)	-0.000497 (0.00300)	-0.00439 (0.00568)	-0.00125*** (0.000281)	0.00192*** (0.000285)	
distVienna											-0.000397** (0.000168)
Constant	40.73 (33.30)	44.88 (70.81)	3.630*** (1.236)	2.124 (2.951)	28.10 (23.49)	24.99 (51.93)	3.285** (1.207)	1.866 (2.850)	-1.677** (0.776)	-2.684*** (0.770)	-1.856** (0.778)
Observations	31	31	31	31	31	31	31	31	11,067	10,710	11,067
R-squared	0.011	0.438			0.004	0.459					
City characteristic controls	No	Yes	No	Yes	No	Yes	No	Yes	No	No	No
Title FE									Yes	Yes	Yes
regression model	OLS	OLS	Negative Binomial	Negative Binomial	OLS	OLS	Negative Binomial	Negative Binomial	Poisson	Poisson	Poisson

1. Columns (1) – (8) regresses the total number of editions or titles a city produced on the city's distance from Wittenberg. The unit of analysis is city. Columns (9)-(11) use a balanced panel recording the number of editions printed by each city for each title and ask whether cities closer to Wittenberg printed more editions per title; the unit of analysis is city x title.
2. Standard errors (in parentheses) are computed using jackknife in columns (1)-(8) due to the small sample size. Columns (9)-(11) use robust SE. *** $p<0.01$, ** $p<0.05$, * $p<0.1$
3. City characteristics controls are river, log population 1500, log population 1500 imputed (dummy), growth 1300-1500, growth imputed (dummy), university, free imperial city, Hanseatic, territory ecclesiastical in 1500, Augustinian monasteries p.c., and print output 1500-1516. All cities in this sample are in the HRE.
4. When the unit of analysis is city (columns (1)-(8)), I include results from a negative binomial model because the dependent variables are counts, and its distribution does not satisfy the "mean = variance" assumption of Poisson regressions. (The dependent variable in (1)-(4) has mean 29 and variance 3777. The dependent variable in (5)-(8) has mean 23 and variance 2025. Regression results during the Poisson model, however, do show similar conclusions as those displayed). In columns (9)-(11), I use a Poisson model because the dependent variable (# of editions a city produced for a given title) is still a count variable, but its mean (0.0817) is close to its variance (0.1189). OLS models also produce similar conclusions. More detailed results can be found in Appendix Table F1 .
5. I run the regression in Column (9) for one title at a time (but using an OLS rather than Poisson model, since Poisson runs into convergence issues for many titles) to see for how many titles the correlation holds. (i.e., I run "reg editions distwitt if title == i" for each of the 357 titles. Due to the small sample size (31 cities) for each title, I calculate standard errors using jackknife, and do not control for city characteristics). The table below shows the number of titles for which the coefficient estimate for distwitt is in each category. "Significant" means statistically significant at the 95% level. (Using 90% and 99% significance levels produce similar results.) Note that under the concentric spread hypothesis, all titles should fall under the "negative and significant" category. The results below do not support this prediction.

	Positive	Negative
Significant	93	54
Insignificant	64	146

Table 4: Summary of Monte Carlo Simulation Results

True Model	Regressors	Frequency of getting 5%-level significant estimates (in N trials)	Frequency of getting 1%-level significant estimates (in N trials)	N	Notes
outcome = dist_TrueCenter	[1, dist_FalseCenter]	[~, 0.9430]	[~, 0.9235]	100*1000	
outcome = dist_TrueCenter	[1, dist_FalseCenter, dist_another city]	[~, 0.9572, 0.9640]	[~, 0.9446, 0.9554]	100*1000	
outcome = dist_TrueCenter	[1, dist_FalseCenter, dist_TrueCenter]	[~, 0, 1]	[~, 0, 1]	100*1000	
outcome = dist_TrueCenter + dist_another city	[1, dist_FalseCenter, dist_another city]	[~, 0.9432, 0.9758]	[~, 0.9300, 0.9710]	100*1000	Mean of beta hats are [~, 0.3276, 1.0744]
outcome = dist_nearest center	[1, dist_FalseCenter]	[~, 0.9024]	[~, 0.8731]	100*1000	True model has 4 centers
outcome = dist_nearest center	[1, dist_FalseCenter]	[~, 0.8682]	[~, 0.8299]	100*1000	True model has 6 centers
outcome = dist_nearest center	[1, dist_FalseCenter]	[~, 0.8767]	[~, 0.8269]	100*1000	True model has 10 centers
outcome = dist_nearest center	[1, dist_FalseCenter]	[~, 0.7920]	[~, 0.7234]	100*1000	True model has 20 centers
outcome = dist_nearest center	[1, dist_FalseCenter]	[~, 0.7374]	[~, 0.6732]	100*1000	True model has 30 centers
outcome = dist_nearest center	[1, dist_FalseCenter]	[~, 0.6769]	[~, 0.6022]	100*1000	True model has 50 centers

outcome = Z	[1, dist_FalseCenter]	[~, 0.9717]	[~, 0.9562]	100*1000	Z = 1 on the left of the vertical line x = 0.3 and = 0 otherwise. An interpretation could be: Z is an indicator for "west of Elbe"
outcome = Z	[1, dist_FalseCenter, dist_another city]	[~, 0.9702, 0.9464]	[~, 0.9523, 0.9276]	100*1000	*ibid.*
outcome = W	[1, dist_FalseCenter]	[~, 0.8217]	[~, 0.7689]	100*1000	W = 1 between the vertical lines x = 0.4 and x = -0.45. An interpretation could be: W is an indicator for "German-speaking"
outcome = W	[1, dist_FalseCenter, dist_another city]	[~, 0.9104, 0.8358]	[~, 0.8705, 0.7918]	100*1000	*ibid.*

This table summarizes the Monte Carlo simulation results mentioned in Section 4 of the text. All simulations are done on the unit square centered at the origin. Each row reports results for one simulation configuration. For each configuration I randomly draw 100 sets of {True Center(s), False Center, another city (whichever are present in the model)} locations, then for each set, I conduct 1000 Monte Carlo simulations randomizing the coordinates of the observation points. Each Monte Carlo trial draws 1000 observation points in the simulation region “Germany” (i.e., the unit square). I calculate outcome for these 1000 observation points according to the True Model (in column 1), then regress outcome on the set of regressors (listed in column 2). In each trial, a regressor’s point estimate is statistically significant at the 5% level if it has a t-statistic (the ratio between the point estimate and the robust standard error) above 1.96, and at the 1% level if it has a t-statistic above 2.576. I compute the frequency a regressor appears statistically significant by tallying across all 100*1000 trials. Results are in column 3 and column 4, where frequencies are listed in the order variables appear in the regression model. For example, in row 2, the true model is $outcome = distance\ from\ True\ Center$, but I estimate $outcome = \alpha + \beta\ distance\ from\ False\ Center\ 1 + \gamma\ distance\ from\ another\ False\ Center\ 2 + \epsilon$. Column 3 reports that in these 100*1000 trials, we do not care how often $\hat{\alpha}$ is statistically significant (thus “~”); $\hat{\beta}$ is statistically significant at the 5% level 95.72 percent of the time; and $\hat{\gamma}$ is statistically significant at the 5% level 96.40 percent of the time.

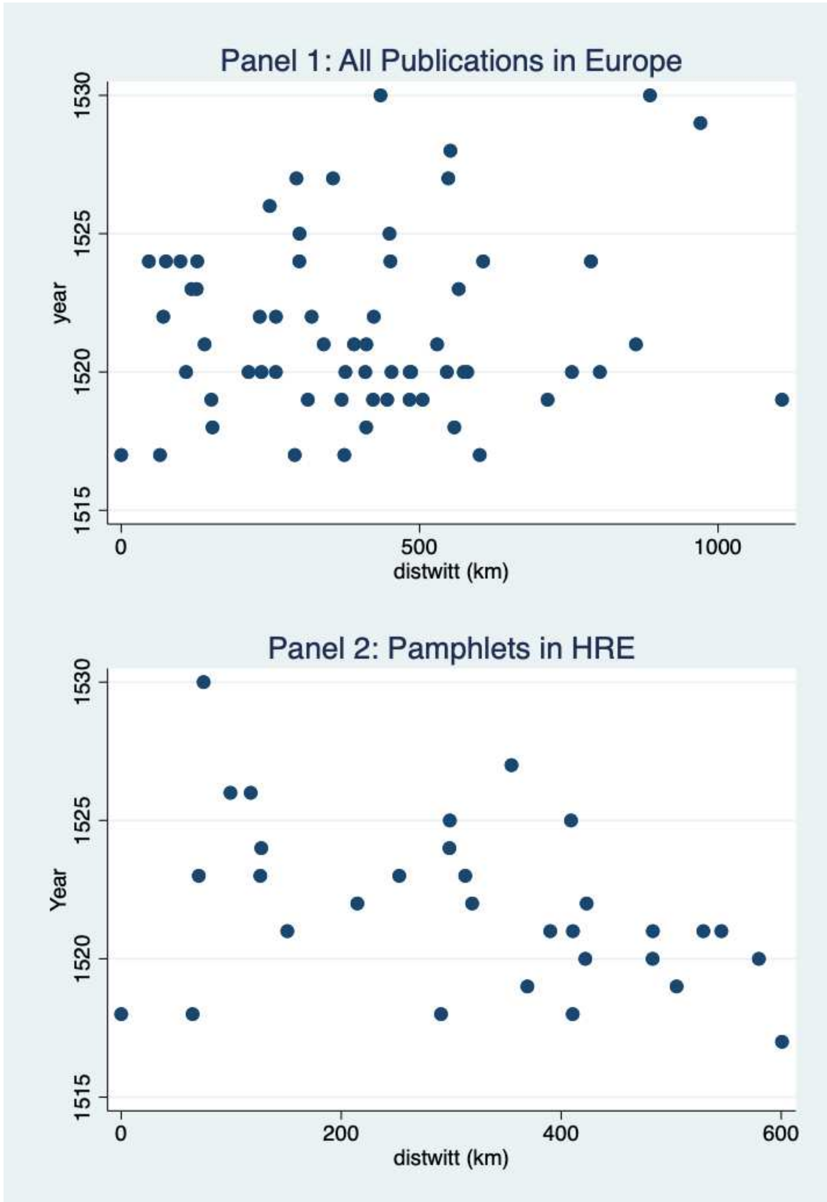
Table 5: Replication of BW's Second Stage (BW's Table III – the effect of Protestantism on literacy outcomes, and Table V – the effect of Protestantism on economic prosperity)

X is	**Wittenberg (BW's original)** (1st stage F statistic 74.19)					
	BW's Table III			**BW's Table V**		
	1st stage	2nd stage		2nd stage		
Dependent var:	Share Protestants (1)	Share literates (2)	Distance to school (3)	Per capita income tax (4)	ln(teacher income) (5)	Share manuf & serv (6)
distX	-0.095*** (0.011)					
Predicted % Protestants		0.189*** (0.028)	-0.025** (0.011)	0.586** (0.236)	0.105** (0.050)	0.082** (0.039)
R^2		0.689	0.356	0.291	0.529	0.602
X is	**Berlin** (1st stage F statistic 81.09)					
	1st stage	2nd stage		2nd stage		
	Share Protestants (1)	Share literates (2)	Distance to school (3)	Per capita income tax (4)	ln(teacher income) (5)	Share manuf & serv (6)
distX	-0.0989*** (0.0110)					
Predicted % Protestants		0.1438*** (0.0254)	-0.0157 (0.0105)	0.5006** (0.2297)	0.0322 (0.0480)	0.0202 (0.0368)
R^2		0.7248	0.3477	0.3046	0.5308	0.6099
X is	**Kolberg/Kolobrzeg** (1st stage F statistic 78.90)					
	1st stage	2nd stage		2nd stage		
	Share Protestants (1)	Share literates (2)	Distance to school (3)	Per capita income tax (4)	ln(teacher income) (5)	Share manuf & serv (6)
distX	-0.1054*** (0.0119)					
Predicted % Protestants		0.0351 (0.0263)	0.005 (0.0110)	0.087 (0.239)	-0.140** (0.054)	-0.083** (0.040)
R^2		0.7120	0.2986	0.327	0.4117	0.5522
X is	**Leipzig** (1st stage F statistic 56.25)					
	1st stage	2nd stage		2nd stage		
	Share Protestants (1)	Share literates (2)	Distance to school (3)	Per capita income tax (4)	ln(teacher income) (5)	Share manuf & serv (6)
distX	-0.0852*** (0.0114)					
Predicted % Protestants		0.2310*** (0.0344)	-0.0351*** (0.0123)	0.5955** (0.2618)	0.1527*** (0.0575)	0.1356*** (0.0454)
R^2		0.6318	0.3487	0.2899	0.5100	0.5689
X is	**Rome** (1st stage F statistic 42.92)					
	1st stage	2nd stage		2nd stage		
	Share Protestants (1)	Share literates (2)	Distance to school (3)	Per capita income tax (4)	ln(teacher income) (5)	Share manuf & serv (6)
distX	0.0902*** (0.0138)					
Predicted % Protestants		-0.1210** (0.0478)	0.0420** (0.0173)	0.2705 (0.3294)	-0.0874 (0.0675)	-0.1344** (0.0554)
R^2		0.4435	-0.0272	0.3256	0.4668	0.4910
X is	**Dresden** (1st stage F statistic 21.46)					
	BW's Table III			**BW's Table V**		

	1st stage	2nd stage		2nd stage		
Dependent var:	Share Protestants (1)	Share literates (2)	Distance to school (3)	Per capita income tax (4)	ln(teacher income) (5)	Share manuf & serv (6)
distX	-0.0558*** (0.0120)					
Predicted % Protestants		0.2294*** (0.0535)	-0.0313 (0.0191)	-0.0225 (0.3816)	0.0176 (0.0884)	0.0791 (0.0681)
R^2		0.6344	0.3534	0.3221	0.5275	0.6028
X is	**Münster** (1st stage F statistic 19.86)					
	1st stage	2nd stage		2nd stage		
	Share Protestants (1)	Share literates (2)	Distance to school (3)	Per capita income tax (4)	ln(teacher income) (5)	Share manuf & serv (6)
distX	-0.0438*** (0.0098)					
Predicted % Protestants		0.4861*** (0.09927)	-0.0372* (0.0198)	2.6519*** (0.6597)	0.7636*** (0.1833)	0.4896*** (0.1246)
R^2		-0.1689	0.3451	.	.	.
X is	**Graz** (1st stage F statistic 15.41)					
	1st stage	2nd stage		2nd stage		
	Share Protestants (1)	Share literates (2)	Distance to school (3)	Per capita income tax (4)	ln(teacher income) (5)	Share manuf & serv (6)
distX	0.0519*** (0.0132)					
Predicted % Protestants		-0.0592 (0.0668)	0.0221 (0.0251)	1.4961** (0.6845)	0.2253** (0.1100)	0.0112 (0.0782)
R^2		0.5849	0.1652	.	0.4563	0.6085
X is	**Vienna** (1st stage F statistic 11.43)					
	1st stage	2nd stage		2nd stage		
	Share Protestants (1)	Share literates (2)	Distance to school (3)	Per capita income tax (4)	ln(teacher income) (5)	Share manuf & serv (6)
distX	0.0411*** (0.012)					
Predicted % Protestants		0.0275 (0.065)	-0.002 (0.026)	2.188** (0.934)	0.423*** (0.155)	0.133 (0.093)
R^2		0.706	0.311	.	0.1533	0.571
X is	**Geneva** (1st stage F statistic 9.85)					
	1st stage	2nd stage		2nd stage		
Dependent var:	Share Protestants (1)	Share literates (2)	Distance to school (3)	Per capita income tax (4)	ln(teacher income) (5)	Share manuf & serv (6)
distX	0.0357*** (0.0114)					
Predicted % Protestants		-0.562** (0.220)	0.131** (0.057)	-2.775** (1.317)	-0.922*** (0.341)	-0.650*** (0.241)
R^2		-1.9121	-1.70	.	.	.

This table replicates the key regressions in BW (2009) with other cities replacing Wittenberg as the location of the "distance from" instrument. Except for the instrument location, each replication uses the exact same regression specification as BW's and includes all of BW's original controls (omitted here to save space.) The top panel reproduces BW's original results (with distance from Wittenberg as the instrument). In the 1st stage, BW regress each county's Protestant population share on the county's distance from Wittenberg (and other controls). In the 2nd stage, BW regress measures of literacy (Table III) and economic prosperity (Table V) on predicted share Protestants. Citing the 2nd stage results in Table III, BW argue that Protestantism promoted literacy. Citing the 2nd stage results in Table V, BW argue that Protestantism promoted economic development. This table shows that replications with different cities do not always support these conclusions.

Figure 1: Year of First Luther Publication vs. distwitt, 1517-1530



Source: Universal Short Title Catalogue (Panel 1); Köhler, H.-J., *Bibliographie Der Flugschriften Des 16. Jahrhunderts* (Panel 2).

Note: Each dot in this graph represents a city that published Luther between 1517 and 1530. The vertical axis plots the earliest year Luther was printed in this city, and the horizon axis plots the city's distance from Wittenberg. Panel 1 depicts the relationship between the year a city in Europe first published *any* publication by Luther vs. the city's distance from Wittenberg. Panel 2 depicts the relationship between the year a city in the HRE first published *a pamphlet* by Luther vs. the city's distance from Wittenberg. Neither panel shows a positive correlation between publication year and distwitt.

Figure 2: Replications of Cantoni (2012)'s Table 4, Column (5)

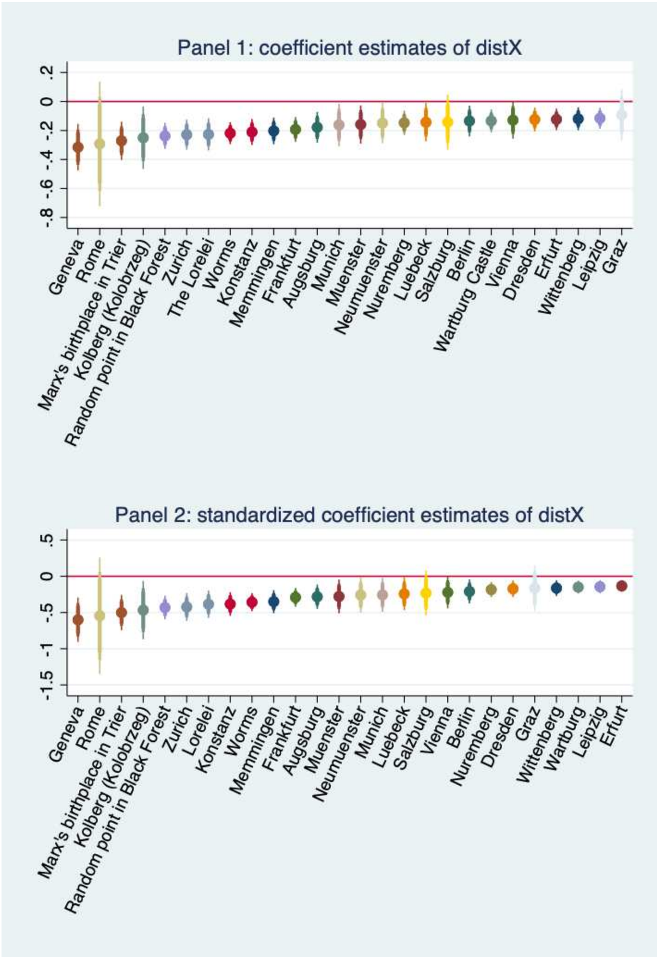


Table 4

Adoption of Protestantism – City-Level Regressions

	City Protestant by 1600				
	Geography	Economy	Institutions	Information	All
Dependent variable	(1)	(2)	(3)	(4)	(5)
Latitude	0.115***				0.055**
	(0.030)				(0.027)
Longitude	0.023*				−0.010
	(0.012)				(0.013)
River	−0.133**				0.013
	(0.058)				(0.042)
Population in 1500 (log)		0.009			−0.030
		(0.022)			(0.035)
Population growth 1300–1500		−0.000			−0.000
		(0.001)			(0.000)
Age of the city		−0.042***			−0.001
		(0.016)			(0.015)
Ecclesiastical			−0.530***		−0.526***
			(0.121)		(0.100)
Monasteries (p.c.)			−0.035*		−0.021
			(0.018)		(0.017)
University			−0.325***		−0.149
			(0.107)		(0.093)
Free Imperial City			−0.073		0.134*
			(0.092)		(0.077)
Hanseatic			0.200**		0.057
			(0.077)		(0.100)
Distance to Wittenberg				−0.147***	−0.119***
				(0.031)	(0.030)
Augustinian Monasteries (p.c.)				0.006	0.041
				(0.070)	(0.070)
Printing press				−0.190*	−0.101
				(0.114)	(0.112)
Constant	−5.263***	1.012***	0.894***	1.218***	−1.455
	(1.519)	(0.124)	(0.078)	(0.067)	(1.489)
No. observations	249	249	249	249	249
No. clusters	87	87	87	87	87
R-squared	0.234	0.111	0.269	0.248	0.519

Notes. Robust standard errors in parenthesis, clustered by territorial compound. OLS estimation (linear probability model). Significant at ***1%, **5%, *10%.

For reference: Cantoni (2012)'s Table 4 (testing for predictors of a city's adoption of Protestantism)

Sources: Cantoni (2012)'s replication data; geographical coordinates of the alternative candidate cities from Google.

Notes: this figure plots replication results for Cantoni's Table 4, Column (5). Each estimate here represents one replication exercise in which I substitute "distance from" the city indicated for "distance from Wittenberg." Panel 1 reports estimates when keep regression specifications otherwise identical to Cantoni's original. Panel 2 reports estimates when I standardize regression coefficients while keeping all other aspects of the regression specification identical to the original. For easy comparison, I plot only estimates for the "distance from" variables. The three different widths of the error bands represent the 90%, 95%, and the 99%-level confidence intervals.