ONLINE APPENDIX TO THE ECONOMIC EFFECTS OF THE PROTESTANT REFORMATION: TESTING THE WEBER HYPOTHESIS IN THE GERMAN LANDS

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Appendix B: Additional Results

B.1. Results in Subsamples (Section 4.3)

As described in Section 4.3, I apply the regression setup described in equation (4) to a series of subsets of my data. First, it is questionable to what extent city size can be used as an indicator of economic progress when free movement of labor from the countryside is hampered. In the territories east of the Elbe stronger forms of serfdom persisted until the early 19th century. Given that almost all cities east of the Elbe are Protestant, this may explain why their economic performance as reflected in city growth was not too strong. However, column (1) in Table B.1, which reports results from a regression corresponding to the setup in equation (4), seems to disprove this conjecture. Even considering only cities west of the Elbe, the basic pattern is unaffected.

The substantial disruptions of the 17th century motivate another robustness check: controlling explicitly for the handicap caused by the Thirty Years' War (1618–1648) in Protestant parts of the Empire. For those cities that have reported population sizes for both 1600 and 1700 (this reduces the number of cities in the sample to 114), I interact the log-difference in population sizes from 1600 to 1700 with all time dummies relating to the years 1750 onward. This controls in a flexible fashion for the catchup process necessary in those cities that have experienced the largest levels of destruction during the Thirty Years' War. In addition, I include a set of triple interactions of "destruction during the 17th century," "Protestantism," and year dummies. These interactions test the hypothesis that Protestant cities were faster/slower in recovering from their destructions. In fact, while the estimates of the main coefficients on the Protestantism/year interactions are now generally larger (see column (2)), especially in the 19th century, they still fail to reach conventional levels of significance.

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The panel dataset with city sizes is unbalanced, with only a small part of the cities having population sizes reported for all years. In column (3) I report results from a regression on the balanced dataset of cities for which population sizes are reported in all years.[1] The results are now more clearly negative for Protestant cities, relative to the baseline regressions. In almost all years after the Reformation the coefficients are negative.

Additionally, column (4) checks whether the selection of cities into the dataset drives the results. Bairoch et al. (1988) include all cities that reach the threshold of 5000 inhabitants at any time before 1800. Therefore, presence in the dataset is already conditional on successful city growth. Instead, one could limit the regression to those cities that were already successful by 1500, as measured by the fact that they have a population size reported for that year in Bairoch et al. (1988). This leaves 126 cities in the dataset, and hence excludes all localities that were very small or did not exist in 1500. Reassuringly, the results are not very different from the baseline estimates.

The 38 Free Imperial cities in the dataset enjoyed a radically different institutional setup than the territorial states of the Empire. In those cities, the decision whether to become Protestant was taken by a city council representing the urban elites, and not imposed by princely fiat. Furthermore, these cities (which were by a large majority Protestant) are often considered a relic of the medieval structure of the Empire, structurally unable to compete with the dominant polity of the early modern era, the territorial state. The results in column (5), which exclude Free Imperial cities from the sample, suggest that their growth performance in the years after the Reformation was indeed below average. The estimated coefficients are now larger and mainly positive, but again not significant.

To increase their size, cities relied mostly on migration from the surrounding countryside; the institutional structure of land tenure could therefore be a determining factor of city growth. In early modern Germany, regions with partible inheritance existed alongside areas with impartible inheritance. Based on Huppertz (1939, map I), I determine the prevailing inheritance rule in the region surrounding each city in the dataset; in general, the Rhineland, Baden, Württemberg, and parts of Hesse and Thuringia had partible inheritance rules, whereas the north, the east, and the southeast of the Empire had impartible inheritance rules. Columns (6) and (7) report results from regressions in the subsets of cities with impartible and partible inheritance rules separately. In fact, it appears that inheritance rules do not affect the main results substantially, as the estimates are similar in both cases, and comparable to the baseline case of Table 4.

Finally, column (8) looks only at the subset of cities that were part of Prussia in 1871, after the unification of Germany; this is the region considered in the analysis of Becker and Woessmann (2009). While the Electorate of Brandenburg-Prussia

1. Due to the Black Death which hit Europe in the 14th century, most cities have missing data for the year 1400; I therefore exclude the year 1400 from the balanced sub-dataset. Imposing the condition that the panel be balanced for all years, including 1400, would have further reduced the number of cities from 45 to 26.

was originally Lutheran, it acquired several Catholic regions over the course of the centuries, in particular after the Congress of Vienna (1815). Here, again, there appears to be no strong effect of Protestantism on city size.

B.2. Alternative City Size Thresholds and Outcomes

The Bairoch et al. (1988) dataset includes all cities that "have had, at some time between 800 and 1800, 5,000 or more inhabitants" (p. IX). The compilation also includes "those cities which [the authors] could not exclude from having possibly reached this population level." As already discussed in Online Appendix B.1, this inclusion threshold implicitly conditions on successful city growth. Looking at the subset of cities that are already included in the dataset before the introduction of the Reformation, as done in Online Appendix B.1, Table B.1, column (4), is therefore one way to check whether this inclusion threshold leads to biased findings.[2]

Here, I explore two further robustness checks. First, I include only those cities that did demonstrably pass the threshold of 5000 inhabitants by or in 1800, thus excluding those cities that Bairoch et al. include merely because of plausibility. Then, I apply a more stringent threshold, namely 10,000 inhabitants by 1800. The results are presented in Table B.2, columns (1) and (2), and confirm the main findings of the paper. The inclusion threshold does not seem to determine the findings (in particular, it does not appear to lead to downward bias in the point estimates).

Columns (3) and (4) explore whether growth in Protestant territories occurred on the extensive, rather than the intensive margin of cities. As a variation to the estimation results looking at total urban population in each territory (Table 4, column (6)), I consider the number of cities in each territory with reported population sizes as the dependent variable (column (3)), or the average size of cities in each territory (column (4)). Again, the results are qualitatively similar (if anything, Protestant territories tend to have fewer entry of cities into the dataset, especially after 1800).

B.3. Heterogeneity of Effects (Section 4.3)

While the main regression results have shown that there is no broad impact of Protestantism on city growth over the entire set of cities in the dataset, it could be the case that, as discussed at the and of Section 4.3, some cities, sharing a certain set of characteristics, benefited from the Protestant faith, whereas cities lacking those characteristics were not able to reap any benefits. This potential heterogeneity of effects across subgroups could plausibly give hints as to which mechanisms are at work.[3]

2. Note that, however, the data are not truncated at 5000 in the classical statistical/econometric sense. If a city passes (or plausibly passes) the threshold of 5000 inhabitants by 1800, its population size is recorded for previous years whenever data are available, even if the population is below 5000. In fact, of the 1876 observations in the baseline sample, 486 refer to population sizes strictly below 5000.

3. It should be noted, however, that any arguable exogeneity of the assignment to Protestantism need not carry through in selected subgroups.

TABLE B.1. OLS estimation, subsamples.

Dependent Variable	ln(City size)							
	(1) West of the Elbe	(2) 30 Years' War	(3) Balanced Sample	(4) Sample Selection	(5) Without Free cities	(6) Impartible Inheritance	(7) Partible Inheritance	(8) Prussia only (as of 1871)
Protestant × Year 1300	0.086	-0.137	-0.276	0.043	-0.012	0.094	0.094	-0.046
	[0.198]	[0.173]	[0.168]	[0.163]	[0.169]	[0.185]	[0.290]	[0.170]
Protestant × Year 1400	0.070	-0.053		0.101	-0.001	0.216	-0.441	0.240
	[0.211]	[0.212]		[0.180]	[0.250]	[0.198]	[0.506]	[0.204]
Protestant × Year 1600	0.143	-0.052	-0.066	0.041	0.144	0.210	-0.398*	0.334
	[0.213]	[0.142]	[0.148]	[0.130]	[0.245]	[0.272]	[0.218]	[0.274]
Protestant × Year 1700	-0.252	-0.103	-0.318	-0.173	-0.030	-0.016	-0.689**	0.186
	[0.156]	[0.118]	[0.268]	[0.155]	[0.147]	[0.143]	[0.327]	[0.191]
Protestant × Year 1750	0.047	0.133	-0.188	0.026	0.237	0.191	-0.295	0.311*
	[0.164]	[0.145]	[0.219]	[0.152]	[0.156]	[0.168]	[0.311]	[0.185]
Protestant × Year 1800	-0.112	0.034	-0.243	-0.070	0.127	0.073	-0.460	0.076
	[0.160]	[0.170]	[0.233]	[0.156]	[0.169]	[0.190]	[0.287]	[0.222]
Protestant × Year 1850	-0.030	0.171	-0.163	-0.034	0.256	0.095	-0.237	0.157
	[0.198]	[0.202]	[0.291]	[0.184]	[0.216]	[0.241]	[0.266]	[0.266]
Protestant × Year 1875	0.033	0.303	-0.064	-0.020	0.339	0.141	-0.155	0.205
	[0.210]	[0.251]	[0.305]	[0.201]	[0.228]	[0.248]	[0.261]	[0.274]
Protestant × Year 1900	0.029	0.343	-0.064	-0.044	0.330	0.069	-0.030	0.108
	[0.236]	[0.297]	[0.342]	[0.221]	[0.253]	[0.278]	[0.278]	[0.315]
p-value for joint significance Protestant	0.000	0.010	0.419	0.568	0.018	0.007	0.008	0.032
p-value for joint significance Dist. to Atlantic	0.015	0.613	0.213	0.049	0.053	0.001	0.595	0.022
p-value for joint significance City size in 1300	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
p-value for joint significance 30-Yrs-War's effects		0.000						
p-value for joint significance 30-Yrs-War's effects, interacted with Protestantism		0.006						
Controls Distance to Atlantic ports	Y	Y	Y	Y	Y	Y	Y	Y
Controls City size in 1300	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1527	965	405	1040	1579	1436	440	879
Number of cities	221	114	45	126	234	207	65	131

Notes: *: Significant at 10%; **: 5%; ***: 1%. All regressions contain a full set of city and year fixed effects. Control variables are entered as a full set of control × year dummy interactions. P-values refer to a joint test significance of all coefficients relating to the post-Reformation period (interactions of respective variable with year dummies, 1600 and onwards). Robust standard errors, clustered by territory, in brackets. The regression in column (3) omits observations relative to the year 1400 due to reduced data availability (cf. footnote 1).

TABLE B.2. Alternative city size thresholds and outcomes.

Dependent Variable	ln(City size)		N. of cities	Avg. city size
	Inclusion threshold:			
	5000 inh. by 1800	10000 inh. by 1800		
	(1)	(2)	(3)	(4)
Protestant × Year 1300	-0.173	-0.138	-0.095	-0.133
	[0.217]	[0.218]	[0.178]	[0.194]
Protestant × Year 1400	0.012	-0.075	-0.224	0.113
	[0.207]	[0.178]	[0.180]	[0.168]
Protestant × Year 1600	0.057	-0.018	-0.105	-0.031
	[0.214]	[0.195]	[0.195]	[0.151]
Protestant × Year 1700	-0.229	-0.094	0.033	-0.349**
	[0.166]	[0.249]	[0.171]	[0.153]
Protestant × Year 1750	-0.012	0.154	0.143	-0.147
	[0.162]	[0.227]	[0.225]	[0.166]
Protestant × Year 1800	-0.145	0.109	-0.660	-0.033
	[0.168]	[0.248]	[0.463]	[0.159]
Protestant × Year 1850	-0.027	0.106	-0.597	0.023
	[0.182]	[0.294]	[0.453]	[0.174]
Protestant × Year 1875	0.005	0.101	-0.601	0.025
	[0.189]	[0.315]	[0.460]	[0.186]
Protestant × Year 1900	-0.003	0.088	-0.601	0.092
	[0.204]	[0.348]	[0.460]	[0.202]
p-value for joint significance Protestant	0.057	0.560	0.456	0.131
Observations	1665	655	1280	986
Number of cities/territories	235	76	128	128

Notes: *: Significant at 10%; **: 5%; ***: 1%. All regressions contain a full set of city (or territory) and year fixed effects, and controls for distance to Atlantic ports and city size in 1300, fully interacted with year dummies. *P*-values refer to a joint test significance of all coefficients relating to the post-Reformation period. Robust standard errors, clustered by territory, in brackets.

A general setup suitable for this purpose can be constructed in analogy to to equation (3):

$$\begin{aligned}\ln(u_{it}) \quad = \quad & \chi_i + \zeta_t + \sum_{\tau \in \Gamma} \alpha_\tau \cdot Prot_i \cdot I_\tau + \sum_{\tau \in \Gamma} \beta_\tau \cdot control_i \cdot I_\tau \\ & + \sum_{\tau \in \Gamma} \gamma_\tau \cdot control_i \cdot Prot_i \cdot I_\tau + \varepsilon_{it}\end{aligned} \tag{B.1}$$

While the coefficients β_t capture the baseline, time-varying effect of a certain city characteristic $control_i$ (analogously to equation (4)), the coefficients γ_i relating to the triple interaction report whether Protestantism affects city size when combined with certain city characteristics, and how this effect varies over time. The estimates from regression (B.2) are unwieldy to present, resulting in 27 estimated coefficients, besides the city and time fixed effects. For this reason, I will discuss the hypotheses in the context of this section by comparing graphically the performance of cities that lie at the upper and lower ends of the distribution of the respective variable $control_i$.[4]

Two potential sources of heterogeneity will be discussed here. First, one can consider the productive structure of cities. If Protestantism is associated with a kind of ethics particularly favorable to commercial enterprise, e.g. by allowing the charging of interest and more sophisticated financial instruments, rather than with a work ethic useful in all kinds of production, we should see a differential effect in those cities with a specific potential for commerce. A proxy for the potential for commerce is the geographic location on a (navigable) river or a seaport. Moreover, Protestant cities located on seaports could have been more rapid in capturing the gains arising from the transatlantic trade (Acemoglu et al. 2005).

In fact, the results of Figure B.1 lend no support to this hypothesis. The solid line with a grey shadow refers to the difference in log population between a Protestant and a Catholic city that are both located on a navigable river or on a seaport ($control_i = 1$), the dashed line and dashed confidence interval refers to the difference between a Protestant and a Catholic city located inland and away from rivers ($control_i = 0$). As evident from the picture, cities with rivers or seaports and those without have very similar trajectories.

Another form of heterogeneity could arise from the differential presence of the Church before the onset of the Reformation. A commonly held view is that Protestant states enriched themselves through the expropriation of Church holdings; if the confiscated capital is put to better use when in state or private hands, rather than if left to the Church, this would give a growth advantage to those cities that had more Church possessions at the time of the Reformation. A good proxy for the amount of capital that can be seized from the Church is the density of monasteries in a city around 1517 (measured as number of monasteries per 1000 inhabitants); monasteries held both prime pieces of real estate, as well as substantial swathes of agricultural land outside of the city walls.

Again, however, the results in Figure B.2 appear to disprove this hypothesis. There, I compare the performance of Protestant cities (relative to Catholic ones) with no monasteries around 1517 (42.6% of cities in the sample had no monasteries) and cities with two monasteries per 1000 inhabitants (75th percentile of the distribution of monasteries per capita). If the ability to seize large amounts of Church capital had been an advantage for Protestant cities, we should see the estimated effects to be larger for cities with a high number of monasteries. In fact, the comparison points again to the absence of differences in the estimated effects of Protestantism.

Full Results of Regressions Above and of Section 6.2.

4. Full regression results can be found in Online Appendix B.3.

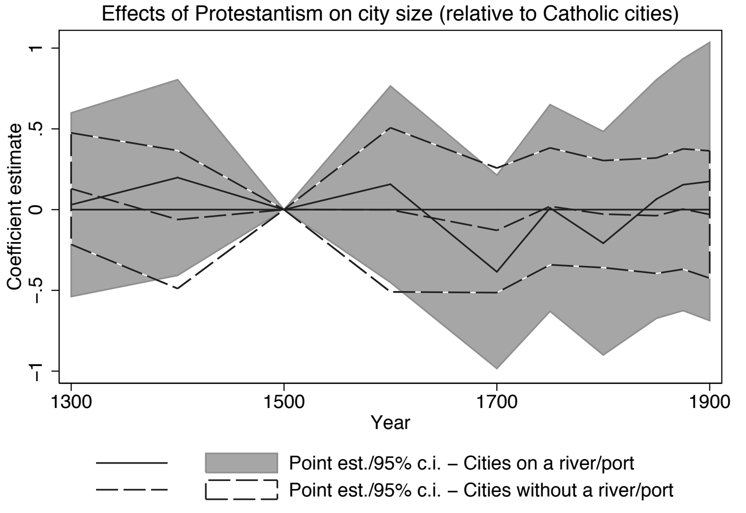

FIGURE B.1. Heterogeneity of estimated effects and 95% confidence intervals: Rivers and ports. Results from the OLS estimates of equation (B.2).

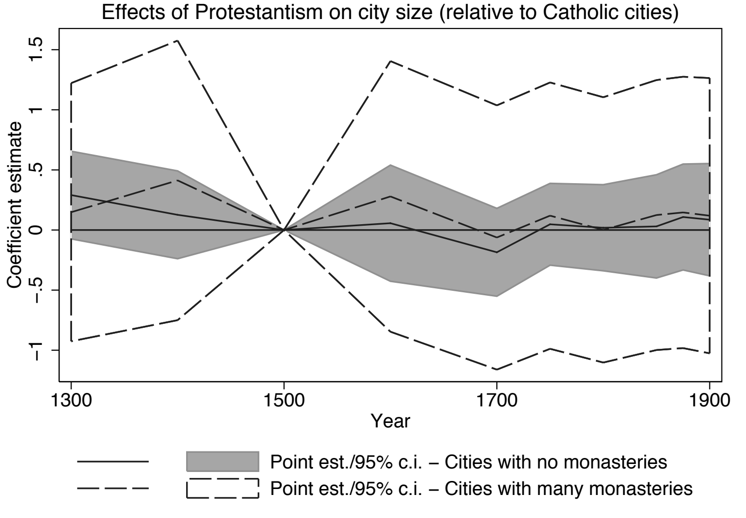

FIGURE B.2. Heterogeneity of estimated effects and 95% confidence intervals: Presence of monasteries. Results from the OLS estimates of equation (B.2).

TABLE B.3. Interactions of Protestantism and city characteristics.

Dependent Variable	ln(City size)		
Control Variable	River or Port	Monasteries (p.c.)	Religious Interaction
Corresponding to	Figure B.1	Figure B.2	Figure 5
	(1)	(2)	(3)
Protestant · Year 1300	0.130	0.291	0.297
	[0.176]	[0.186]	[0.316]
Protestant · Year 1400	-0.061	0.126	-0.115
	[0.218]	[0.186]	[0.437]
Protestant · Year 1600	-0.001	0.057	-0.006
	[0.259]	[0.246]	[0.363]
Protestant · Year 1700	-0.128	-0.185	-0.414
	[0.197]	[0.186]	[0.287]
Protestant · Year 1750	0.020	0.048	-0.176
	[0.185]	[0.174]	[0.291]
Protestant · Year 1800	-0.028	0.019	-0.375
	[0.169]	[0.183]	[0.312]
Protestant · Year 1850	-0.038	0.030	-0.351
	[0.182]	[0.219]	[0.344]
Protestant · Year 1875	0.003	0.108	-0.432
	[0.190]	[0.225]	[0.356]
Protestant · Year 1900	-0.030	0.086	-0.522
	[0.201]	[0.238]	[0.380]
Control · Year 1300	-0.016	0.371**	0.515
	[0.251]	[0.143]	[0.460]
Control · Year 1400	-0.234	0.264*	-0.219
	[0.312]	[0.156]	[0.634]
Control · Year 1600	0.155	0.178	-0.114
	[0.325]	[0.154]	[0.494]
Control · Year 1700	0.610*	0.167	-0.691
	[0.338]	[0.165]	[0.442]
Control · Year 1750	0.325	0.155	-0.596
	[0.347]	[0.156]	[0.495]
Control · Year 1800	0.586	0.193	-0.640
	[0.357]	[0.159]	[0.566]
Control · Year 1850	0.418	0.167	-0.791
	[0.352]	[0.158]	[0.589]
Control · Year 1875	0.395	0.164	-0.997*
	[0.370]	[0.156]	[0.589]
Control · Year 1900	0.404	0.154	-1.123*
	[0.393]	[0.156]	[0.598]
Protestant · Control · Year 1300	-0.099	-0.071	-0.349
	[0.324]	[0.305]	[0.656]
Protestant · Control · Year 1400	0.260	0.143	0.341
	[0.371]	[0.323]	[0.902]
Protestant · Control · Year 1600	0.158	0.111	0.080
	[0.367]	[0.303]	[0.722]
Protestant · Control · Year 1700	-0.257	0.062	0.090

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Table B.3, continued

Dependent Variable	ln(City size)		
Control Variable	River or Port	Monasteries (p.c.)	Religious Interaction
Corresponding to	Figure B.1	Figure B.2	Figure 5
	(1)	(2)	(3)
	[0.390]	[0.312]	[0.545]
Protestant · Control · Year 1750	-0.010	0.036	0.142
	[0.399]	[0.308]	[0.667]
Protestant · Control · Year 1800	-0.180	-0.009	0.498
	[0.408]	[0.312]	[0.670]
Protestant · Control · Year 1850	0.103	0.047	0.598
	[0.411]	[0.310]	[0.701]
Protestant · Control · Year 1875	0.151	0.019	0.980
	[0.431]	[0.310]	[0.714]
Protestant · Control · Year 1900	0.204	0.017	1.174
	[0.463]	[0.310]	[0.766]
Controls Distance to Atlantic ports	Y	Y	Y
Controls City size in 1300	Y	Y	Y
Observations	1876	1876	1876
R-squared	0.708	0.704	0.702
Number of cities	272	272	272
p-value for joint significance Protestant	0.292	0.037	0.079
p-value for joint significance triple interactions	0.242	0.095	0.532

Notes: *: Significant at 10%; **: 5%; ***: 1%. All regressions contain a full set of city and year fixed effects. *P*-values refer to a joint test significance of all coefficients relating to the post-Reformation period (interactions of respective variable with year dummies, 1600 and onwards). Robust standard errors, clustered by territory, in brackets.

B.4. Additional Regressions: Potato and Black Death

Following up on the analysis of heterogeneity of effects in Section 6.2 and Online Appendix B.3, this Online Appendix considers two more potential confounding factors: the ravages brought by the Black Death epidemic of 1348–50, and the introduction of the potato as a New World crop particularly suitable for Northern European climates. Voigtländer and Voth (2013b,a) have convincingly argued how important the Black Death epidemic was in bringing Europe to a high income level that would eventually result in sustained economic growth; Nunn and Qian (2011) have shown how the potato was able to increase total population and urbanization in the areas most suitable for its cultivation.

These shocks could conceivably have a direct effect on city size; moreover, they could give rise to a form of heterogeneity, exerting different effects depending on the religious denomination of the city. For example, Protestant cities could have exploited the opportunities offered by the Black Death shock or by the introduction of the potato differently.

I use the same definition of "potato suitability" as in the paper by Nunn and Qian (2011): it is defined as the (logarithm of) the area within a 100km radius around a city classified as suitable for potato cultivation by the FAO-GAEZ (Global Agro-Ecological Zones) dataset. In interpreting regression coefficients, note that this variable has a sample average of 9.826 and a standard deviation of 0.329.[5]

It is more difficult to determine which cities were affected more or less strongly by the Black Death epidemic of 1348–50. Whereas older authors seem to conclude that the Black Death spared some areas, notably Franconia (see Hoeniger 1882; Vasold 2003), more recent research (Benedictow 2006) argues that the absence of evidence is, in fact, no evidence in favor of absence of the Black Death. More likely, Franconia and parts of Bavaria were reached by the epidemic only later because the cold winter stopped temporarily the expansion of the virus across the Alps and along the Danube. Overall, the sources seem to provide very scarce evidence that could be helpful in identifying regions more or less hit by the Black Death. Therefore, I follow two approaches.

First, I compute the urban population change over the fourteenth century (as log-difference) for all cities that are present in the Bairoch et al. (1988) dataset both in 1300 and in 1400. Only 42 cities fulfill these criteria. For the remaining 230 cities in the dataset, I estimate population losses as the average of the losses in the three closest cities with available data (weighting by inverse distance to these cities). This assumes that population losses in a city were a function of geography and thus spatially correlated, and not of, say, urban structure or urban institutions. Of course, this assumption is questionable.[6] As an alternative approach, I drop the regions identified by some authors (Hoeniger 1882; Vasold 2003) as spared by the Black Death: Bavaria and Franconia.[7]

The results of these regressions are in Table B.4. Columns (1) and (3) look at the effect of potato suitability and Black Death losses, without allowing for interactions of these effects with Protestantism. Interestingly, potato suitability appears to have a *negative* impact on city sizes, especially after 1800. Black Death population losses have no clear effect (perhaps because of the noisiness of the measured data). Column (5) excludes regions which were perhaps spared by the Black Death, and finds no differential effects in the remaining regions.

Regressions in columns (2) and (4), featuring triple interaction terms, are again best interpreted graphically. Figures B.3 and B.4 are constructed analogously to Figures B.1, B.2, and 5, and display the performance of Protestant relative to Catholic cities at the 25th and the 75th percentiles of the distribution of the control variables, respectively.

5. When no data for potato suitability for a given city are available in the replication dataset by Nunn and Qian (2011), I estimate it by considering the average of potato suitability for the three closest cities, weighted by inverse distance.

6. Still, the results for the 42 cities with data for 1300–1400 are at least consistent with the narrative in Hoeniger (1882) or Vasold (2003), as cities in Bavaria and Franconia such as Nuremberg and Passau are among those with the highest growth rates in that century.

7. In practice, I drop cities belonging to the Duchy of Bavaria, the Margraviates of Ansbach-Bayreuth, the Prince-Bishoprics of Würzburg, Bamberg, Eichstätt, Freising, Passau, the territory of the Upper Palatinate, the Principality of Pfalz-Neuburg, and the city of Nuremberg with its territory.

While Black Death losses do not interact in a meaningful way with Protestantism (Figure B.4), potato suitability seems to exert different effects in Protestant relative to Catholic cities (Figure B.3). Where conditions for growing potatoes were good, Protestant cities pick up in population relative to Catholic cities in similar terrain conditions. The difference in relative sizes becomes significant at the 5% level after 1800.

Areas with high potato growing suitability in the Holy Roman Empire are distributed along a north-south line, from Schleswig and Mecklenburg in the north, through Hanover and Saxony, to Swabia, parts of Bavaria, and Württemberg. Areas to the west of this region (such as the Rhineland, Hesse, or Westphalia), or to the east (such as Pomerania, Silesia, or Austria), are relatively less suitable. With this information, it is probably speculative to determine whether the triple interaction terms are really capturing a historical fact (e.g., Protestants being more open-minded and faster in reaping the gains of the potato), or rather just the correlation of potato suitability with some other geographically distributed omitted factor. For example, the graph could be reflecting the better performance of Protestant cities in Brandenburg and Saxony (a high suitability region) in the 19th century relative to Catholic cities in Bavaria (another high suitability region).

Data Sources

Variable	Description and source
Potato suitability	Suitability of terrain within a 100km radius of the city for potato cultivation. Source: FAO, Global Agro-Ecological Zones (GAEZ) 2002 database, as used in Nunn and Qian (2011) (see replication files). If a city is not present in the Nunn and Qian (2011) dataset, potato suitability is estimated as the average of potato suitability in the three closest cities, weighting by inverse distance (great circle distance).
Black Death population losses	Log difference in city size, 1300–1400. Source: Bairoch et al. (1988). Where city sizes are missing for either 1300 or 1400 (or both), log population change is estimated as the average of population changes in the three closest cities, weighting by inverse distance (great circle distance).

TABLE B.4. Interactions of Protestantism and city characteristics

Dependent Variable	ln(City size)				
Control Variable			Black Death		
	Potato suitability		Population losses (1300–1400)		Drop Bavaria/ Franconia
Corresponding to		Figure B.3		Figure B.4	
	(1)	(2)	(3)	(4)	(5)
Protestant · Year 1300	0.070	-4.003	-0.182	-0.158	-0.255
	[0.221]	[5.322]	[0.188]	[0.189]	[0.222]
Protestant · Year 1400	0.076	0.916	-0.010	0.015	-0.080
	[0.189]	[6.149]	[0.197]	[0.200]	[0.230]
Protestant · Year 1600	-0.066	-0.727	0.007	0.033	-0.058
	[0.193]	[5.543]	[0.218]	[0.221]	[0.232]
Protestant · Year 1700	-0.319*	-9.437*	-0.266*	-0.235	-0.305*
	[0.163]	[4.778]	[0.154]	[0.150]	[0.176]
Protestant · Year 1750	-0.036	-9.237**	-0.030	0.001	-0.092
	[0.172]	[4.642]	[0.151]	[0.152]	[0.162]
Protestant · Year 1800	-0.061	-13.154***	-0.133	-0.104	-0.236
	[0.171]	[3.994]	[0.156]	[0.156]	[0.168]
Protestant · Year 1850	0.068	-12.809***	-0.048	-0.020	-0.163
	[0.182]	[3.979]	[0.179]	[0.176]	[0.186]
Protestant · Year 1875	0.157	-10.441**	0.008	0.036	-0.098
	[0.189]	[4.456]	[0.187]	[0.186]	[0.192]
Protestant · Year 1900	0.189	-11.097**	-0.008	0.018	-0.123
	[0.201]	[4.907]	[0.206]	[0.205]	[0.207]
Control · Year 1300	-0.809***	-1.096**	-0.649***	-0.922***	
	[0.264]	[0.430]	[0.193]	[0.319]	
Control · Year 1400	-0.349	-0.375	0.039	-0.356	
	[0.293]	[0.538]	[0.173]	[0.307]	
Control · Year 1600	0.141	0.100	0.155	-0.245	
	[0.235]	[0.512]	[0.195]	[0.286]	
Control · Year 1700	0.241	-0.374	0.231	-0.406	
	[0.241]	[0.447]	[0.232]	[0.286]	
Control · Year 1750	0.027	-0.588	0.062	-0.496*	
	[0.288]	[0.378]	[0.233]	[0.275]	
Control · Year 1800	-0.348	-1.245***	0.053	-0.559*	
	[0.256]	[0.347]	[0.234]	[0.295]	
Control · Year 1850	-0.502*	-1.386***	-0.064	-0.628**	
	[0.272]	[0.334]	[0.229]	[0.294]	
Control · Year 1875	-0.612**	-1.339***	-0.022	-0.590**	
	[0.271]	[0.384]	[0.239]	[0.291]	
Control · Year 1900	-0.772**	-1.547***	-0.067	-0.552*	
	[0.297]	[0.410]	[0.257]	[0.304]	
Protestant · Control · Year 1300		0.414		0.352	
		[0.540]		[0.403]	
Protestant · Control · Year 1400		-0.086		0.539	
		[0.618]		[0.360]	
Protestant · Control · Year 1600		0.060		0.560	
		[0.562]		[0.360]	

Continued on next page

Table B.4, continued

Dependent Variable	ln(City size)				
Control Variable			Black Death		
	Potato suitability		Population losses (1300–1400)		Drop Bavaria/ Franconia
Corresponding to		Figure B.3		Figure B.4	
	(1)	(2)	(3)	(4)	(5)
Protestant · Control · Year 1700		0.928*		0.898**	
		[0.481]		[0.393]	
Protestant · Control · Year 1750		0.937**		0.784**	
		[0.471]		[0.389]	
Protestant · Control · Year 1800		1.336***		0.876**	
		[0.405]		[0.401]	
Protestant · Control · Year 1850		1.314***		0.801*	
		[0.406]		[0.407]	
Protestant · Control · Year 1875		1.079**		0.807*	
		[0.457]		[0.419]	
Protestant · Control · Year 1900		1.150**		0.679	
		[0.504]		[0.447]	
Controls Distance to Atlantic ports	Y	Y	Y	Y	Y
Controls City size in 1300	Y	Y	Y	Y	Y
Observations	1,876	1,876	1,876	1,876	1,755
R-squared	0.703	0.709	0.693	0.695	0.689
Number of cities	272	272	272	272	254
p-value for joint significance Protestant	0.013	0.000	0.025	0.017	0.024
p-value for joint significance triple interactions		0.000		0.149	

*: Significant at 10%; **: 5%; ***: 1%. All regressions contain a full set of city and year fixed effects. *P*-values refer to a joint test significance of all coefficients relating to the post-Reformation period (interactions of respective variable with year dummies, 1600 and onwards). Robust standard errors, clustered by territory, in brackets.

B.5. Heterogeneity: By Timing of Introduction

A conceivable source of heterogeneity might arise from the timing of official introduction of the Reformation. Cities and territories that adopted the Reformation early on, in a situation with higher legal uncertainty, are likely to be more fervent in their beliefs, or more homogeneous in their enthusiasm for the new creed. I split the group of cities that eventually adopted the Reformation into two groups: those adopting until 1532, and those adopting after that date. The first wave of adopters acted in a period of considerable institutional uncertainty with regard to the legality of the new creed. As opposed to that, after 1532 (and even more clearly after 1555, the date of the Peace of Augsburg) the legal environment was safer, as the Emperor Charles V suspended the Edict of Worms and stopped all trials against Protestants. 58 cities adopted the Reformation officially until 1532.

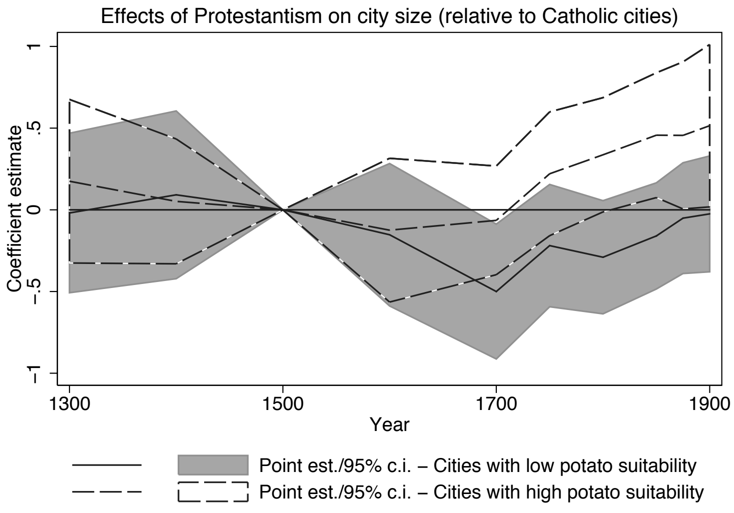

FIGURE B.3. Heterogeneity of estimated effects and 95% confidence intervals: Potato growing suitability. Results from OLS estimates based on equation (7).

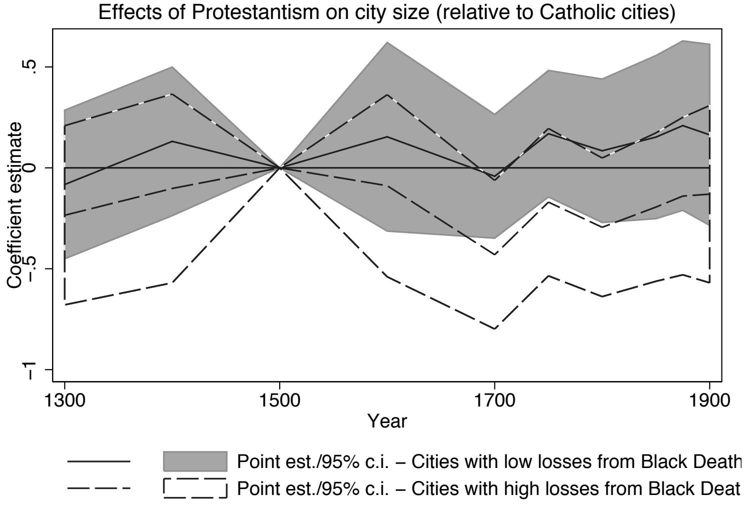

FIGURE B.4. Heterogeneity of estimated effects and 95% confidence intervals: Black Death population losses (1300–1400). Results from OLS estimates based on equation (7).

I run a slight modification of the baseline OLS regression:

$$\ln(u_{it}) = \chi_i + \zeta_t + \sum_{\tau \in \Gamma} \alpha_\tau^{early} \cdot ProtUntil1532_i \cdot I_\tau + \sum_{\tau \in \Gamma} \alpha_\tau^{late} \cdot ProtAfter1532_i \cdot I_\tau + \sum_{\tau \in \Gamma} \beta_\tau \cdot control_i \cdot I_\tau + \varepsilon_{it} \quad \text{(B.2)}$$

where the coefficients of interest are the sequences $\{\alpha_\tau^{early}\}$ and $\{\alpha_\tau^{late}\}$, representing the effect of Protestantism on city size (relative to Catholic cities) for early and late adopters, respectively. The estimated effects, and their related confidence intervals, are depicted in Figure B.5 below. As can be seen, no substantial difference in effects is discernible.[8]

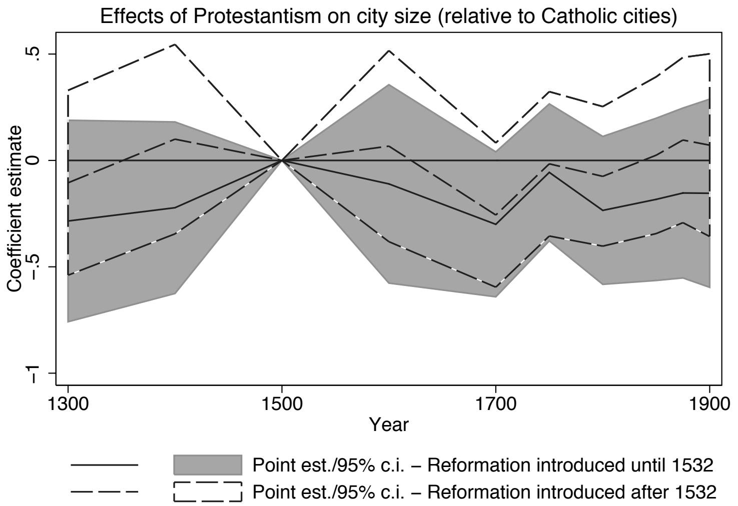

FIGURE B.5. Heterogeneity of estimated effects and 95% confidence intervals: Timing of introduction. Results from OLS estimates.

B.6. Fertility: A Back-of-the-Envelope Calculation

In Section 6.1, I discuss evidence on differences in fertility behavior between Protestants and Catholics in early modern Germany. This is relevant because the absence of effects of Protestantism on city size could be shrouding an actual effect of Protestantism on productivity, which is however not turned into higher population numbers because of a concurrent transition to a different fertility regime.

To stack the cards against the argument in this paper, assume that the fertility schedules of Protestants and Catholics differ by the largest difference observed in the data, about 12% at any level of income, and that the mortality schedule does not differ across religions. The equilibrium level of income per capita, given by the intersection of the fertility schedule $B(y)$ and the mortality schedule $D(y)$, will then differ by an amount that will depend on the elasticities of fertility and mortality with respect to

8. Very similar results obtain when different cutoffs for the separation of early and late adopters are used, e.g. 1555.

income; under reasonable assumptions, the change in income per capita will be similar to the change in fertility: $d \ln y \approx 0.12$.[9]

Given a Malthusian economy as sketched above in Section 4.2, the production function implies that $d \ln L = 1/(1-\beta)(d \ln A - d \ln y)$, where $\beta < 1$ is the elasticity of output with respect to labor. A change in productivity by $d \ln A$ can thus be "neutralized" by an equivalent proportional change in income per capita, and have no effect on population. What if, instead, the purported change in productivity due to the Protestant ethic had not been translated into increases in income per capita (through a change in fertility behavior), but into higher population? For the case of $\beta = 0.5$, this implies that a change in productivity of 0.12 log points would have increased population by 0.24 log points.

According to the baseline specification (with controls) of Table 4, column (3), the median effect of Protestantism on log city size across the years 1600–1900 is 0.021, and would have thus increased by an order of magnitude under the assumptions above. Still, the effect would be far, for example, from the 0.7–1.1 log points effect of Atlantic trade (Acemoglu et al. 2005). Moreover, note again that in this counterfactual exercise all assumptions were made to maximize the effect of fertility (e.g. by taking the largest estimates in the literature, or by ignoring lower mortality among Protestants).

B.7. Comparison with Becker and Woessmann (2009)

Table B.5 replicates the main correlations presented by Becker and Woessmann (2009, Table V, cols. 1–3) separately for the whole sample of Prussian counties (*Kreise*), and for those counties that contain one or more of the cities analyzed in the main part of this paper, i.e. the cities of the Bairoch et al. (1988) dataset. This reduces the sample size by about three quarters, and, by focusing on counties with large urban centers, limits the analysis to the larger and economically more successful counties (see Table B.6). In these counties, the main relationships between Protestantism and economic outcomes

9. Formally, one can linearize $B(y) = D(y)$ around the equilibrium level, assuming fertility schedules that differ simply by a proportional amount: $B^{prot}(y) = 0.88 B^{cath}(y)$. The (approximate) change in equilibrium income per capita is $dy/y \approx 0.12 \cdot 1/[(d \ln B(y)/d \ln y) - (d \ln D(y)/d \ln y)]$—it will decrease in the elasticity of fertility with respect to income per capita, and increase in the elasticity of mortality. I will assume that the term in brackets (the difference of the two elasticities) is not too far from 1. Patrick Galloway ("Basic Patterns in Annual Variations in Fertility, Nuptiality, Mortality, and Prices in Pre-industrial Europe," *Population Studies*, 1988, *42* (2), 275–302) reviews evidence on the Malthusian model for a variety of locations in pre-industrial Europe. In particular, he has data on fertility and mortality in Minden-Kleve (1695–1751) and central Prussia (1696–1755). He estimates the elasticity of births and deaths with respect to changes in rye prices, looking at a four-year horizon after the shock (a plausible window of time for demographic reactions to occur). For Minden-Kleve, he finds that the cumulative elasticity of births to price changes is -0.174 (-0.202 for central Prussia); the cumulative elasticity of deaths is 0.510 (0.207 for central Prussia). Thus the sum of elasticities is 0.684 for Minden-Kleve, and 0.409 for central Prussia. Note, however, that these are elasticities with respect to one-year changes in food prices. To the extent that food prices only partly translate into changes in real incomes per capita, and that reactions to permanent changes to incomes are stronger, one could arguably expect that the sum of elasticities is clearly larger than 0.4–0.7.

do not hold any more, becoming much smaller in magnitude, even reversing sign, and insignificant (compare columns 2, 4, and 6 with 1, 3, and 5).

TABLE B.5. Replication of Table V, Becker/Woessmann (2009)

Dependent Variable	Per capita income tax		ln(Teacher income)		Share manuf. & services	
	All counties	Only counties with cities in Bairoch et al.	All counties	Only counties with cities in Bairoch et al.	All counties	Only counties with cities in Bairoch et al.
	(1)	(2)	(3)	(4)	(5)	(6)
% Protestants	0.154*	0.092	0.063***	-0.001	0.035**	-0.018
	[0.091]	[0.295]	[0.019]	[0.053]	[0.015]	[0.035]
Observations	426	96	452	115	452	115
R-squared	0.328	0.186	0.534	0.519	0.611	0.626

Notes: *: Significant at 10%; **: 5%; ***: 1%. Other controls included (coefficients not reported): % age below 10, % Jews, % females, % born in municipality, % of Prussia origin, average household size, ln(population size), population growth 1867–1871 (in %), % blind, % deaf-mute, % insane.

TABLE B.6. County summary statistics: Becker/Woessmann (2009)

	Mean			
	in sample	not in sample	Difference	*p*-value
Per capita income tax	234.7	187.7	46.9	0.000
ln(teacher income)	701.1	682.6	18.5	0.000
Share manuf. & services	44.3	30.4	13.9	0.000
% literate	93.5	85.5	8.1	0.000
% Jewish	1.07	1.16	-0.09	0.514
ln(population size)	11.01	10.73	0.27	0.000
Population growth 1867–1871 (in %)	3.15	1.06	2.08	0.001

Notes: *: Difference significant at 10%; **: 5%; ***: 1%. *P*-values based on t-tests of differences in means, allowing for unequal variances. The second column present sample averages for the Prussian counties (*Kreise*) that contain one of the cities in the Bairoch et al. (1988) dataset, the third column presents averages for the remaining Prussian counties. Number of observations: 115 (in sample), 337 (not in sample). For per capita income tax, number of observations: 96 (in sample), 330 (not in sample).

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