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Education Promoted Secularization

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Education Promoted Secularization*

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Why did substantial parts of Europe abandon the institutionalized churches around 1900? Empirical studies using modern data mostly contradict the traditional view that education was a leading source of the seismic social phenomenon of secularization. We construct a unique panel dataset of advanced-school enrollment and Protestant church attendance in German cities between 1890 and 1930. Our cross-sectional estimates replicate a positive association. By contrast, in panel models where fixed effects account for time-invariant unobserved heterogeneity, education – but not income or urbanization – is negatively related to church attendance. In panel models with lagged explanatory variables, educational expansion precedes reduced church attendance.

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1. Introduction

Secularization, understood as churches' loss of importance in society, is a European phenomenon of the late 19th and early 20th century that brought fundamental change to societies. In our micro-regional German data, participation in Holy Communion per Protestant declined on average by nearly a third of its initial value from 1890 to 1930. Despite hot academic debates, empirical evidence on the sources of this seismic social change is scarce. Classic secularization hypotheses argued that, besides improved material conditions, increased education may have been a leading source of secularization, as advanced scientific knowledge may have reduced belief in supernatural forces (e.g., Hume (1757 [1993]); Freud (1927 [1961]); cf. McCleary and Barro (2006a)). However, cross-sectional evidence generally contradicts this view and finds a positive association between education and church attendance (Iannaccone (1998)). But such association may well emerge from bias due to omitted variables: For example, more orderly people may go both to church and to school. Consistently, in contrast to the cross-sectional finding, in a randomized laboratory setting Gervais and Norenzayan (2012) find that analytical thinking induced by priming promotes religious disbelief. However, it is unclear how such modern experience from short-term lab interventions transmits to the analysis of decade-long societal developments during important historical phases of secularization.

To provide evidence from an historical setting, this paper constructs a unique panel dataset on education and Protestant church attendance in German cities from 1890 to 1930. We confirm a positive cross-sectional association. But results are reversed in panel estimates that use city and year fixed effects to take out unobserved time-invariant city characteristics and common trends. Exploiting city-specific variation over time, enrollment increases in advanced schools are related to decreases in church attendance. While recent comparable findings suggest that increased income did not lead to reduced church attendance (Becker and Woessmann (2013)), this panel result is in line with the traditional view that modernization facilitated secularization.

Panel models with lagged explanatory variables show that increased enrollment in advanced schools preceded decreases in church attendance, whereas the reverse is not true. In contrast to education, measures of income (municipal tax) and urbanization (city population) as alternative dimensions of modernization are not significantly related to changes in church attendance. The main result is confirmed in a series of robustness tests, such as controlling for the age and gender structure of a city, migration patterns, alternative secular movements, university enrollment, and

secular vs. religious schools, as well as in alternative specifications of the education variable and in subsamples.

Our analysis makes two main contributions to the literature. First, it provides insight into the mechanisms and reasons of a fundamentally important social phenomenon in Europe in the late 19th and early 20th century. In contrast to the income and urbanization measures, education is the only measure of modernization that is significantly associated with sizeable changes in church attendance. Second, by replicating the existing cross-sectional patterns and overturning them in panel analyses, the analysis suggests that the existing cross-sectional literature is likely to suffer from severe identification problems. In line with recent (quasi-)experimental evidence from modern times, our panel evidence suggests that educational expansion was negatively related to religious behavior, confirming traditional secularization hypotheses during an historical period of mass secularization when they were very prominent but lacked empirical validation.

Section 2 provides context and reviews the literature. Sections 3 and 4 introduce our data and empirical model. Sections 5 and 6 report results and robustness analyses. Section 7 concludes.

2. Background and Existing Literature

During the 19th and early 20th centuries, Western Europe underwent a profound process of secularization. While the very concept of secularization has been questioned for the United States (e.g., Finke and Stark (1992)), it is generally accepted that key indicators of secularization, in particular reduced church attendance, have been “dramatically increasing” in Western Europe (Berger (1996)). For instance, Becker and Woessmann (2013) find that church attendance fell by 3.6 percentage points per decade, on average, across Prussian counties between 1850 and 1931.¹

The period and location we study is therefore of chief interest when analyzing the determinants of the process of secularization. In fact, the very religious census that our analysis draws on was established mainly to analyze the “decay” in religiosity in Germany (Hölscher (2005)), indicating a process of secularization observed by the clergy. This is also confirmed in Hölscher (2001), who locates the process from the late 18th century onwards.² Nipperdey (1988) supports the existence of this process and points to a large decline in church attendance between

¹ Franck and Iannaccone (2014) show that the process of declining church attendance continued, albeit at a slower pace, in several Western countries over 1925-1990.

² Hölscher (2005) reports that since the mid-18th century, it became unusual to punish people’s absence from church, hence changing it from a duty to a convention. As we focus on a time period after this substantial change in social pressure, our results are unlikely to be influenced by this anymore, adding credibility to our estimates.

1870 and 1912. He narratively attributes this pattern to increases in urbanization, technological progress, and the advancement of a rational view on the world.

The latter is a key aspect that is often discussed as part of classic secularization hypotheses: Increased education may have been a leading source of demand-induced secularization.³ In one form or another, this view on education and religiosity has been advanced by such eminent scholars as Hume (1757 [1993]), Marx (1844), Weber (1904 [2001]), and Freud (1927 [1961]), among many others. McCleary and Barro (2006b) summarize this traditional view as follows: “one argument for secularization is that more educated people are more scientific and are, therefore, more inclined to reject beliefs that posit supernatural forces” (p. 151).⁴

Despite hot academic debates about secularization, empirical evidence on the sources of this phenomenon is scarce. In the sociological literature, this debate has been going on for a long time (Sherkat and Ellison (1999)). The existing evidence, which is mostly cross-sectional, is generally not in line with the traditional view that education reduces church attendance. Iannaccone (1998) summarizes the literature as follows: “In numerous analyses of cross-sectional survey data, rates of religious belief and religious activity tend not to decline with income, and most rates increase with education” (p. 1470).

Similarly, in a cross-country analysis, McCleary and Barro (2006b) find that while religiosity is negatively related to overall economic development,⁵ it is actually positively related to education. Using individual-level data from the British National Child Development Study (NCDS), Brown and Taylor (2007) also find a positive association between education and church attendance.⁶ A positive effect of education on religiosity could be explained by alternative hypotheses based on advantages of educated people in the kind of abstract thinking required for

³ While our analysis contributes to testing of “demand-side” secularization hypotheses, there is substantial evidence that elements of a “supply-side” religion-market model also contribute to understanding religiosity (e.g., Finke and Iannaccone (1993); Stark and Iannaccone (1994); McCleary and Barro (2006b)). Gruber and Hungerman (2008) show that church attendance faces competition from secular time uses.

⁴ Modern psychological literature suggests that, while religiosity comes natural at early age, rational reflection can reduce it (Bloom (2007); Kapogiannis et al. (2009); Shenhav, Rand, and Greene (2012)).

⁵ Similarly, Lipford and Tollison (2003) find a negative effect of income on church membership using U.S. state-level data. Becker and Woessmann (2013) study the two-way interplay between church attendance and income in a panel of Prussian counties in 1886-1911. They find that once county fixed effects are controlled for, the link disappears. Quite to the opposite of the general pattern in the literature, Franck (2010) finds that the secular politicians who voted in favor of the separation between church and state in France in 1905 were elected in areas with high literacy but lower values on some economic indicators.

⁶ Against this general finding, Ruiter and van Tubergen (2009) and Deaton (2011) find negative associations of education with indicators of religiosity in cross-country analyses, although not uniformly across all countries.

religion (McCleary and Barro (2006b)) or based on educated people getting higher returns from social networking in church (Glaeser and Sacerdote (2008)).

It is, however, not clear to what extent the existing cross-sectional findings suffer from omitted-variable bias. For example, it is easily conceivable that more orderly people are more inclined to go both to church and to school, and that more conservative people put particular emphasis on religious rituals and on educational achievement. These examples would give rise to a positive correlation between education and church attendance that does not necessarily stem from a causal effect of education.⁷

Recent evidence from modern times underlines such concerns with cross-sectional studies. When exploiting exogenous variation from changes in compulsory schooling laws, Hungerman (2011) finds that education led to a reduction in reported religious affiliation in Canada in post-WW II data. Similarly, recent psychological evidence from laboratory experiments where participants are primed with analytic processing before being asked about their belief in God suggests that analytic thinking promotes religious disbelief (Gervais and Norenzayan (2012)).⁸ Our analysis complements this literature by going back to a crucial period of secularization in a core European country during which church attendance decreased long before official church membership figures declined. Observing this social phenomenon over various decades goes beyond what modern setups can possibly capture.

3. Data

To provide new evidence from a historical setting that avoids important sources of bias, we construct a unique new panel dataset of German cities between 1890 and 1930 that covers measures of both education and church attendance (see Data Appendix for details).

First, we digitized data on enrollment in advanced schools (*Höhere Unterrichts-Anstalten*) from different volumes of the Statistical Yearbooks of German Cities from Neefe (1892) through Deutscher Städtetag (1931).⁹ Covering all German cities with a population over 50,000

⁷ There may also be bias from reverse causality; e.g., Meyersson (2014) provides evidence that religion – specifically, Islamic party rule – affects female education, using a regression-discontinuity design that compares Turkish municipal elections where an Islamic party barely won or lost mayor seats.

⁸ Sander (2002) and Brown and Taylor (2007) are other notable exceptions which explicitly address concerns of endogeneity, although it is not clear to what extent their use of parental education (and, in the latter case, school characteristics and ability measures) as instruments for education can solve the indicated omitted-variable problems.

⁹ We are not aware of a source where data on advanced-school enrollment and tax revenues are consistently available for 1890-1930 at the city or county level other than this source for large cities.

inhabitants, these yearbooks regularly report the number of students enrolled in advanced schools. Variation in enrollment in advanced schools seems particularly relevant to test the traditional view of education and secularization, both because advanced schools are most likely to convey the kind of scientific thinking stressed by secularization hypotheses and because enrollment in elementary schools was as good as universal in Germany throughout our period of observation, in particular in Protestant regions (Becker and Woessmann (2009)).

Our main measure of the enrollment rate in advanced schools is the number of enrolled students as a share of the city population. Although the number of students who are actually enrolled does not constitute a large part of the Protestant population of a city, the structure of the school system is indicative of a city's educational orientation as parents arguably decide upon investment in the children's education. In our view, this measure hence best reflects the overall educational and scientific orientation of the city.¹⁰

The yearbooks provide us with data for an unbalanced panel of a total of 61 German cities observed over eight waves that cover every five years from 1890 to 1930 (with the exceptions that, due to the interruptions of World War I, there are no data for 1920 and values for 1913 are used instead of 1915). Apart from the education data, the yearbooks also report data on municipal tax per capita, a proxy for income, and on the total population of the city, which captures different degrees of urbanization.

We match these data with unique data on church attendance of Protestants. This exceptional database stems from the practice of the Protestant Church to count the number of participations in Holy Communion every year.¹¹ At the time, church *membership* was close to 100 percent and hardly reveals information about actual religious involvement. In contrast, data on participation in Holy Communion reflects "demonstrations of churchly life ... that were considered already by contemporaries as indicators of churchliness" (Hölscher (2001), p. 4).¹² As the communion data are based on direct head counts, they also do not suffer from the usual unreliability of survey data on church attendance (Hadaway, Marler, and Chaves (1998); Woodberry (1998)).

¹⁰ Results are similar when using the size of the population aged below 20 (rather than the total population), available in a subsample, as the numerator (Table A1, column 2). Results are also robust to controlling for cities' age structure as a potential influence (Table A1, column 1; see Azzi and Ehrenberg (1975)).

¹¹ Since the norm was to participate in Holy Communion only once per year (Hölscher (2001), p. 37), changes in the opportunity cost of participation are unlikely to drive changes in this measure, in contrast to measures of weekly church attendance.

¹² Hölscher (2005) reports that religious behavior in Germany in the 19th century mainly involved publically visible actions like church attendance.

Hölscher (2001) gathered the historical Sacrament Statistics for the modern-day German territory from regional archives, providing data on church attendance as measured by the number of participations in communion divided by the Protestant population (see also Becker and Woessmann (2013)). The communion data are available at the level of church districts, which we match to our city database. Given that advanced schools draw their enrollment both from the city itself and from the surrounding countryside, the availability of the communion data at the church district level (and of the education data only for larger cities) provides a reasonable match.¹³ Throughout, standard errors in all regressions are clustered at the level of church districts. Also, while the church attendance measure refers to Protestants only, educational enrollment refers to the whole city population; models in subsamples of cities with a mostly Protestant population confirm that this is not driving the results (Table A2, columns 5-8).

The number of cities with complete available data for our analysis from both sources increases from 25 in 1890 to 51 in 1930. In total, there are 291 city-year observations over the eight waves in our unbalanced panel.¹⁴ To compare city characteristics of the initial dataset of up to 522 city-year observations with our estimation sample, Table 1 shows descriptive statistics for both datasets. None of the differences is statistically significant. In the estimation sample, average church attendance is 25 percent, ranging from 5 percent to 74 percent.

Overall, the data document a process of secularization in German cities over the period 1890 to 1930: Panel estimates with city fixed effects reveal a statistically highly significant average decrease in church attendance by about 2 percentage points per decade (not shown).

4. Empirical Model

The panel setup of our database allows us to estimate panel models with fixed effects that identify only from within-city variation over time. Such models avoid identification from unobserved time-invariant regional characteristics that may be correlated with both education and church attendance. Specifically, we estimate the following equation:

$$y_{it} = \mu_i + \mu_t + \gamma s_{it} + \beta x_{it} + \varepsilon_{it}$$

¹³ Results are robust in subsamples where the city size coincides closely with the size of the church district (Table A2, column 3).

¹⁴ Results are qualitatively the same when the sample is restricted to the initially observed 25 cities (Table A2, column 1). For 32 observations, church attendance values were imputed from neighboring years by linear interpolation (see Data Appendix). Results are also robust when dropping these observations (Table A2, column 2).

where y_{it} denotes participations in Holy Communion per Protestant population, μ_i and μ_t denote fixed effects for city (i) and time (t), respectively, s_{it} is advanced-school enrollment per city population, x_{it} is a set of covariates, and ε_{it} is an error term. Our parameter of interest is γ . In the baseline fixed effects specification, the covariates include municipal tax revenues (a proxy for income) and city population (a proxy for urbanization). In a pooled OLS specification that replicates the cross-sectional nature of existing studies, the city fixed effects are replaced by a dummy that indicates whether the city ever belonged to Prussia and the share of Protestants (in 1910, the only year for which this highly stable measure is available at the city level).

The inclusion of city fixed effects addresses potential omitted variable bias from unobserved time-invariant city characteristics. The inclusion of time fixed effects addresses potential bias from nation-wide common trends in the variables of interest. The remaining question is whether such identification from city-specific variation over time can be given a causal interpretation. Our analysis does not directly model exogenous variation and hence cannot unequivocally claim causal inference. Although we cannot fully exclude the possibility of bias from reverse causality, regressions with lagged explanatory variables indicate that we capture an effect from education to religiosity, and not vice-versa. The estimates would still be biased if there were omitted variables whose city-specific change over time correlates with changes in the two variables of interest. For example, systematic changes in the composition of a city's population would not be captured in the fixed effects. However, additional controls for changes in the demographic composition of the city – age structure, gender structure, and recent migration – included in robustness specifications do not alter our results. In our historical data, measurement error in the independent variables is also likely to attenuate coefficient estimates towards zero; in this sense, our estimates are, if anything, conservative.

5. Results

Using the cross-sectional dimension of our data, columns 1 and 2 of Table 2 replicate the common finding from the literature that education is significantly positively associated with church attendance in the cross-section, both in the full 1890-1930 period and before World War I. In the pooled OLS estimation with year fixed effects, an advanced-school enrollment rate that is higher by one standard deviation is associated with 3.9 percentage points (or 0.28 standard

deviations) higher church attendance in 1890-1930. This positive association is easily visible in a simple scatterplot depicted in panel (a) of Figure 1 for the example of the year 1900.

However, when using only within-city variation over time in a panel model with city and year fixed effects, this result is turned around (columns 3-4). Increases in education are now significantly negatively related to changes in church attendance.¹⁵ This pattern is in line with the traditional view that education leads to secularization. In the panel fixed effects estimation, an increase in the advanced-school enrollment rate by one standard deviation is related to a decrease in church attendance by roughly 1.5 percentage points (or 0.10 standard deviations), in both the full and the pre-WW I sample.¹⁶ This negative association between changes in education and in church attendance is again easily visible in the scatterplot, depicted in panel (b) of Figure 1 for the pre-WW I period. To put the magnitude in perspective, consider the 24 cities observed both in 1890 and in 1913: If, rather than their actual mean change in education, they would have only had the change in education of the city at the 10th percentile, the reduction in church attendance would have been 1.3 percentage points smaller. This would account for about one quarter of the actual mean reduction in church attendance of 4.6 percentage points.

The change in the sign of the estimated effect between the cross-sectional and the fixed effects model is consistent with the presence of unobserved time-invariant city-specific characteristics that give rise to a positive correlation in cross-sectional regressions. For instance, a city's population may have historically been of more conservative attitude, putting a stronger emphasis on educational attainment as well as on institutionalized religiosity – hence the initial positive correlation. However, with an increase in advanced-school enrollment, also these cities witnessed a drop in church attendance over time.

Interestingly, education is the only measure of modernization that yields sizeable and significant estimates. Although municipal tax revenue may be an imprecise proxy for income, the positive point estimate casts doubt on the hypothesis that income adversely affected religiosity. This is in line with Becker and Woessmann (2013) who find that, once fixed effects

¹⁵ We understand our education measure – the number of advanced-school students as a share of the total city population – as an indicator of the inclination of families towards advanced education. Due to diverse obstacles to actual enrollment such as school fees, this number is likely a truncated measure of the latent propensity to enroll.

¹⁶ The estimate for the post-WW I period (not shown) is only about half as large (in absolute terms) and does not reach statistical significance. The within-city variation explained by the regressors drops by nearly half in the post-WW I model, possibly reflecting the large disturbances in society due to the war. For example, Hölscher (2001) reports that society's trust in the clergy collapsed after WW I throughout Germany. However, the weaker finding in the post-WW I sample might also reflect that this sample includes only two waves of data.

are included, there is no significant effect of income on religious participation. Similarly, the size of the city population as a proxy of urbanization is not significantly related to church attendance.

When adding the lagged enrollment variable to the model, both enrollment measures have negative coefficients, but the lagged variable is about twice as large in absolute terms and is the only one capturing statistical significance (columns 5-6). This pattern is in line with a causal time pattern where increased enrollment in advanced schools is followed by subsequent decreases in church attendance. By contrast, lagged church attendance does not significantly predict enrollment rates (not shown). Finally, first-differenced models that regress the change in church attendance from 1890 to 1913/1930 on the change in the explanatory variables over the same time, although based on small samples, confirm the panel fixed effects models (columns 7-8).

6. Robustness Analyses

A factor often argued to be relevant for religious participation is the age of a person (e.g., Azzi and Ehrenberg (1975)). To control for this potential influence, column 1 of Table A1 in the Appendix includes measures of a city's age structure, namely the shares of under-20-year-olds and of over-60-year-olds. Despite being available only for the census years 1890 and 1910, results in a sample of 46 observations leave the effect of education qualitatively unchanged and even larger.¹⁷ Both age measures have the expected sign but are statistically insignificant.

While conceptually, the share of advanced-school students in the entire city population is our preferred indicator of a city's educational and scientific orientation, in this smaller sample we can also calculate the enrollment rate as a fraction of under-20-year-olds. Using this variable leaves the significant negative coefficient on education unaffected, indicating that our results are robust to this change in the measure of education (column 2).¹⁸

Traditionally, women participated more regularly in church activities, possibly because of lower opportunity costs (Azzi and Ehrenberg (1975)). When including the share of females in the model, our result remains robust and the female share is insignificant (column 3).

¹⁷ The specification includes a dummy for Kassel in 1910, an outlier in this specification.

¹⁸ Because the within-variation of the share of under-20-year-olds in a city is small (standard deviation of 0.01 vs. 0.05 between), we conducted the same exercise using a city's share of under-20-year-olds in 1890 or 1910 for all observations. Hence, for the sake of the argument, we assumed there was no within-variation at all in the share of young people. Both alternative measures yield smaller, but significantly negative estimates.

To account for possible effects of migration on church attendance, we include the share of recently arrived citizens in a city (column 4). The coefficient on the share of new citizens is insignificant and small, while the coefficient on education is qualitatively unaffected.

Activities in secular associations might have increasingly crowded out participation in church activities during our period of observation. However, when including the vote share for social-democratic parties in national elections over time as a proxy for the strength of alternative secular movements, results on our main coefficient of interest are unaltered and the vote share for social-democratic parties is itself statistically insignificant (column 5).¹⁹

To ensure that results are not related to foundations of new universities in the cities, column 6 controls for an indicator for openings of new universities, as well as for the share of university students in the city population. Neither enters the model significantly, and the coefficient on the enrollment rate in advanced schools remains qualitatively unaffected.

During our period of observation, some advanced schools changed from being religiously affiliated into non-religious schools.²⁰ To the extent that this entailed a change towards a less religious curriculum, our main effect might capture a change in curriculum and educational content rather than an increase in education per se (e.g., Franck and Iannaccone (2014)). However, when including the share of Protestant advanced schools in a city as an additional control, it does not enter the model significantly and does not qualitatively change our main result (column 7).²¹

Results also prove robust in different subsamples. They are qualitatively unaffected in the sample of the 25 cities already observed in 1890 (Table A2, column 1). Results are also robust when dropping the 32 observations whose church attendance data were imputed by linear interpolation (column 2). We further test the robustness of our results to discrepancies in the number of Protestants between the church district and the city in 1910 (the only year for which both measures are available). In a subsample of cities whose size was close to that of the church

¹⁹ The same qualitative results are obtained when controlling for the vote share of all left-leaning parties (not shown).

²⁰ In our data, the average number of advanced schools hardly changed between 1885 and 1905 (average increase of 0.13 schools per city), but the number of Protestant schools decreased by an average of 0.38 schools, whereas the number of secular schools increased by an average of 0.55 schools (the difference being made up by an average decrease of Catholic schools of 0.05).

²¹ Similarly, the share of Catholic or secular advanced schools does not enter the model significantly or change our main result. The same is true for the total number of advanced schools, indicating that our result does not capture an effect of increased supply of advanced schools, but rather a demand-side effect.

district, the education coefficient remains qualitatively unchanged (column 3). Over time, new advanced schools may have been set up in medium-sized cities in the neighborhood of the large cities included in our sample, pulling medium-city students out of the large-city advanced schools that they had previously attended.²² To test that results are not driven by such changes, we exclude all cities situated in urban regions, restricting the analysis to only those cities situated in rural regions or regions with rudiments of suburbanization (*Regionen mit Verstädterungsansätzen*);²³ results hold (column 4).²⁴

Finally, when we split the sample along the share of Protestants in the city in the census year 1910 (columns 5-8), we find that the result is largely the same in cities with a Protestant majority and minority. In cities that are overwhelmingly Protestant, the coefficient is still negative and large, but loses statistical significance. This may reflect the reduced sample size, but it may also indicate that determinants of demand decreases like education play a lesser role in cities with a very large Protestant share because social pressure motives make it more common that people attend church as an investment in social capital (Hölscher (2005)).

7. Conclusion

Despite extensive academic debates, there is fairly little empirical evidence on the sources of secularization during its crucial phase in European history. To overcome potential bias in existing cross-sectional analyses of the association between education and secularization, we constructed a unique panel dataset of German cities between 1890 and 1930 that has not been analyzed so far. Using cross-sectional methods, we replicate the existing finding that education and church attendance are positively correlated. But the finding turns around when city fixed effects control for time-invariant city characteristics.

²² This may also explain why the average share of advanced-school students in the total population does not increase on average in our city sample, although it does increase steadily in the German Reich at least since 1895 (Müller (1987)). Still, the absolute number of students more than doubled in our city sample from 1890 to 1930.

²³ These classifications are based on the modern classification of regions (*Siedlungsstrukturelle Regionstypen*) by the Federal Institute for Research on Building, Urban Affairs and Spatial Development; given the long-run process of urbanization, this restricts the sample even further than would be required by historical standards.

²⁴ Results are also robust to dropping Berlin, by far the largest city in the sample (not shown). As additional robustness checks, we included dummy variables for structural breaks in municipal borders, such as when smaller neighboring towns were incorporated into Berlin before the 1925 observation and into several cities in the Ruhr area before 1930; qualitative results are unaffected (not shown). Furthermore, to test that results are not driven by jumps due to measurement error in the data, we included dummy variables for periods whenever particularly large changes occurred in the variable of interest between two points in time, such as upward or downward jumps of 50 percent, 30 percent, or even only 20 percent; results are fully robust, and if anything, point estimates become larger, as would be expected by classical measurement error theory (not shown).

This result suggests that the reason why the vast majority of the existing empirical literature in both sociology and economics rejects the traditional view that education led to secularization may well be due to its cross-sectional nature. When using within-city variation over time in panel fixed effects models, our evidence appears to resurrect the traditional view. The stark contrast between cross-sectional and panel evidence, the robustness of the result in several sensitivity analyses, and the fact that increases in education precede declines in church attendance suggest that it may be too soon to lay the traditional view of secularization hypotheses to rest, after all.

The results do not imply that advanced scientific knowledge is incompatible with belief in God or that education is generally hostile to religious participation. They simply show that, in this historical setting, increased advanced schooling was related to declining church attendance, an indication of secularization understood as the loss of influence of the organized church. Education may well have led people who believe in God to break with the institution of the church, without weakening their belief in God. It is also possible that, for example, people who did not have a strong belief in God in the first place were led by education to abandon their custom to attend church. Still, the results show that increases in education were closely related to people's reduced active involvement with the institutionalized church and its rituals, one of the most seismic changes in social history since the 19th century.

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Data Appendix

A.1 Data on Enrollment in Advanced Schools

Our main measure of the enrollment rate in advanced schools is calculated as the number of students enrolled in advanced schools as a share of the city population. Consecutive volumes of the Statistical Yearbooks of German Cities (Neefe (1892) through Deutscher Städtetag (1931)) report data on enrollment in advanced schools (*Höhere Unterrichts-Anstalten*). The yearbooks cover all German cities with a population of over 50,000 inhabitants, providing data on up to 61 German cities observed over eight waves that cover every five years from 1890 to 1930. Exceptions due to the interruptions of World War I are that there are no data for 1920 and that 1913 values are used instead of 1915. When sub-district values of a city are reported, we use the joint value. Where the yearbooks in some cases provide values of adjacent years (while other values in the table are reported for the actual year), we use these values. If data were reported as double years, e.g., 1892/93, we chose the first year of a double year where differences were minor and the available value if the year of interest was only reported once.

When only the number of male students is reported in a given year but both male and female students are reported in adjacent years, we estimate the number of female students in the year of interest by assuming that the relative share of male and female students remained constant relative to the adjacent year (four cases in 1890, one in 1900). If the variation over time was large and values appeared not comparable, we refrained from doing so. Where every measure was missing in one year, we took the adjacent year's value. This occurred in the 1895 sample, for which we took the values of the school year 1893/94 (also one case in 1913; two cases in 1895 have values from 1896/97). Where it was not possible to use adjacent years, we used the average relative size of female to male students (one case in 1930). Where outliers were obtained, we refrained from imputing the number of students.

In several waves, enrollment data are reported by school type (e.g., municipal vs. state schools). Where one of the parts was indicated as missing, we treated the entire value as missing. We do not include so-called pre-schools (*Vorschulen*), which are sometimes connected with advanced schools, in our measure. The separation is generally possible for male students, whereas pre-schools are not separately indicated for female students. The relatively low share of students relative to the city population in 1925 is pointed out in the 1927 yearbook's introduction and justified by small birth cohorts between 1915 and 1918, during World War I.

A.2 Data on Church Attendance

Our measure of church attendance stems from an exceptional database that “unlocks source material that is internationally unique in its scope and historical depth: the statistical surveys of the Protestant Regional Churches of Germany on the Expressions of Churchly Life” (Hölscher (2001), p. 29). The uniform annual surveys were organized by the Statistical Central Office at the Protestant Higher Church Council in Berlin from 1880 (with precursors) to World War II. Parish priests collected the data on preprinted forms following uniform surveying directives. Regional Consistories combined these parish data into registers at the level of church districts.

Our main indicator of church attendance is the “sacrament participation” (Hölscher (2001)), measured as the number of participations in Holy Communion divided by the number of Protestants in a church district. Counting sacrament participations is deemed relatively reliable because the established practice was to count the number of issued wafers from the number of wafers before and after the sacrament. The general Protestant norm at the time was to have Holy Communion only once a year (Hölscher (2001), p. 37), although there were regional differences. The possibility of multiple individual attendances, the possibility of attending Holy Communion outside the home parish, and the counting of non-confirmed children among the number of Protestants in the denominator constitute reasons why the indicator does not directly measure the actual sacrament participation of parishioners (see Becker and Woessmann (2013)).

At the time, the data were regularly published in a comparative manner at the level of the Regional Churches. Hölscher (2001)’s “Data Atlas on the Religious Geography in Protestant Germany: From the Mid-19th Century to the Second World War” for the first time brings together the district-level data, gathered from regional archives, for the geographic coverage of modern Germany. Hölscher kindly provided us with digital versions of the data as published in the Data Atlas. After assigning IDs to every church district (*Kirchenkreis*) and cross-checking the data, we combined the data into one panel dataset. In few cases where data referred to double years, we assigned attendance numbers to the first of the double year.

The church attendance data have gaps for some church districts in some years. Where the year of interest was unavailable but a precedent and a subsequent year were available, we computed the linear interpolation between the years available to obtain an estimate of the church attendance in the year of interest. This was done in a total of 17 observations (3 in 1890, 4 in 1900, 1 in 1905, 3 in 1910, 1 in 1913, 1 in 1925, and 4 in 1930). Where only the precedent year,

only subsequent years, or years very far from the point of time of interest were available, we computed a city-specific linear time trend and interpolated the closest available church attendance measure to the year of interest. This was applied in 30 church district observations translating to 34 city-year observations (9 in 1890, 4 in 1895, 1 in 1900, 2 in 1905, 4 in 1910, 3 in 1913, 4 in 1925, and 7 in 1930). Of the 51 imputed dependent variables, 36 enter the merged dataset. Of these 36 observations, 32 enter our regression analyses. When dropping all observations that have an imputed dependent variable, the number of observations that can be used in the regression drops to 259; the coefficient of education remains qualitatively unaffected in this smaller sample (Table A2, column 2).

Some notable changes occurred in the borders of church districts. Where a city changed districts, we assigned the district value in which the city was at the point of time. For the values of the number of Protestants in a church district taken from Hölscher (2001), we proceeded analogously to the imputation of church attendance. Where the value for either Protestants or church attendance was available and the other one was missing, we took the reported value and imputed the other one, where possible.

A.3 Additional Data

The Statistical Yearbooks of German Cities provide a number of additional data used in our analyses. As an indicator of city income, we use municipal tax revenues, which refer to total municipal tax revenues (*Gemeindesteuereinnahmen Gesamt*) in a city in a year. Because numbers were unavailable for 1925, we used the 1927 values, the first year after the year of interest in which tax revenues were available. Again, if double years were reported, we chose the first year where differences were minor. Where some of the entries were reported as double and others as single years, we took the single-year values where possible.

As a general rule, the population data are taken from the December count. This was not applicable in 1913, where we used the latest possible population data, from October 1913, to be as close as possible to the December standard. In 1925, the only population count available was from June. In 1930, only the average value over the entire year was available. If census data were available, we referred to this data source (in 1890, 1895, and 1910). The same sources also provide data on the share female.

The data on share of Protestants in 1910 were taken from the 1910 census. Where data were missing for the share of Protestants in 1910 in the yearbooks, we took the value of the 1910

census. The age structure is also extracted from the 1910 census and from the statistical yearbook of 1893, which includes tables of the 1890 census. For the data on new citizens, the yearbooks note in the introduction that figures are not very accurate as some cities counted persons and others households. In 1913, we used the 1912 data for availability and in 1925, the 1924 data.

Yearbook data were taken from the statistical yearbooks of 1892, pp. 26-386, and 1893, p. 274 (both for 1890); 1895, p. 162, 1897, pp. 149-383, and 1898, pp. 162-264 (all for 1895); 1902, pp. 101-423, and 1903, pp. 110-267 (both for 1900); 1907, pp. 52-69, and 1908, pp. 380-474 (both for 1905); 1912, pp. 26-679 (for 1910); 1914, p. 481, and 1916, pp. 7-696 (for 1913); 1927, p. 66-355, and 1930, pp. 441-455 (for 1925); and 1931, pp. 367-519 (for 1930). The data from the census in 1910 were taken from pp. 132-162.

Data on vote shares were obtained from election results for the National Parliament (*Reichstag*) for the years 1890 to 1912 (available from the GESIS Data Archive for the Social Sciences, study number ZA8145, Voter movement in Wilhelminian Germany: The Reichstag elections of 1890 to 1912). Elections are not at 5-year intervals, so we matched 1890 election data to 1890 church attendance data; 1893 to 1895; 1898 to 1900; 1903 to 1905; 1907 to 1910; and 1912 to 1913. We linked our city-level data to the precinct (*Wahlbezirk*) in which they are located.

Data on the opening of universities and on the number of university students are taken from Eulenburg (1904) until 1900 and subsequently from different volumes of the Statistical Yearbook of the German Reich.²⁵ In our city sample, only three universities opened during the time period (Frankfurt/Main 1914, Hamburg 1919, and re-founding of Cologne 1919).

We coded data on the number of Protestant, Catholic, and secular advanced schools, as well as the total number of advanced schools, in our city sample from volumes VII, XVI, and XXVI of the Statistical Yearbook of Advanced Schools and Remedial Institutions in Germany, Luxembourg and Switzerland (Statistisches Jahrbuch der höheren Schulen und heilpädagogischen Anstalten Deutschlands, Luxemburgs und der Schweiz).

A.4 Merging the Datasets

We used the church district data to merge the two datasets. Where it was unclear in which church district a city was at the time, we used geographical matching to assign the church district

²⁵ We are grateful to Stephan Heblich for providing us with the latter data.

in which the city was in 1910. Where cities changed in their adherence to a church district, we used the largest available district for clustering and assigned the fraction of communion attenders of the church district in which the city was at the specific point in time.

If a city's value was reported in more than one data point (e.g., large cities where the church district was split into urban and rural districts), we used the largest church district related to the city (that is, the urban one). In cases in which the split and merge of church districts was unclear, we refrained from using the data. Hölscher (2001) reports data for modern Germany, with the exception of the Province of Brandenburg (except for Berlin) and Western Pomerania. Due to this limitation, our analysis does not cover some Eastern parts of modern Germany and the parts of the German Reich not located in modern Germany (esp. the Eastern Provinces located in modern Poland and Russia).

TABLE A1: ROBUSTNESS CHECKS: AGE, GENDER, MIGRATION, SECULAR MOVEMENTS, UNIVERSITIES, AND SECULAR SCHOOLS

<i>Enrollment rate:</i>	Per city population	Per under- 20-year-olds	Per city population				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Enrollment rate in advanced schools	-4.191* (2.250)	-1.837* (0.888)	-1.671** (0.767)	-1.164** (0.560)	-1.864* (0.945)	-1.612*** (0.532)	-3.226** (1.303)
Municipal tax per capita	0.001 (0.002)	0.001 (0.002)	-0.0001 (0.0004)	-0.00006 (0.0003)	0.0009 (0.0008)	0.00002 (0.0003)	0.0016 (0.0017)
Population (in millions)	0.052 (0.063)	0.043 (0.062)	0.011 (0.012)	0.009 (0.011)	0.074 (0.057)	0.009 (0.011)	-0.023 (0.064)
Share under 20 year olds	-0.160 (0.492)	-0.422 (0.449)					
Share over 60 year olds	1.349 (0.993)	1.275 (0.982)					
Share female			0.161 (0.246)				
Share new citizens				-0.009 (0.087)			
Share social-democratic votes					0.009 (0.095)		
Foundation of university						0.014 (0.012)	
Enrollment rate in universities						0.093 (1.304)	
Share of Protestant advanced schools							-0.044 (0.034)
City and year fixed effects	yes	yes	yes	yes	yes	yes	yes
Observations	46	46	193	257	190	291	73
Clusters	23	23	54	57	47	58	33
R ² (within)	0.673	0.684	0.492	0.465	0.507	0.494	0.368

Dependent variable: church attendance (participations in Holy Communion over Protestants). Unbalanced panel of 61 German cities with over 50,000 inhabitants observed in eight waves that cover every five years from 1890 to 1930 (no data for 1920; 1913 instead of 1915). Columns 1 and 2: waves 1890 and 1910. Column 5: waves before WW I. Column 7: waves 1890, 1895, and 1905. Enrollment rate in advanced schools refers to enrolled students over city population; in column 2, it refers to enrolled students over city population aged below 20 years. Standard errors clustered at the church district level in parentheses: significance at * 10%, ** 5%, *** 1%.

Data sources: church attendance: Hölscher (2001) based on Sacrament Statistics; other variables: Statistical Yearbooks of German Cities (Neeffe (1892) through Deutscher Städtetag (1931)).

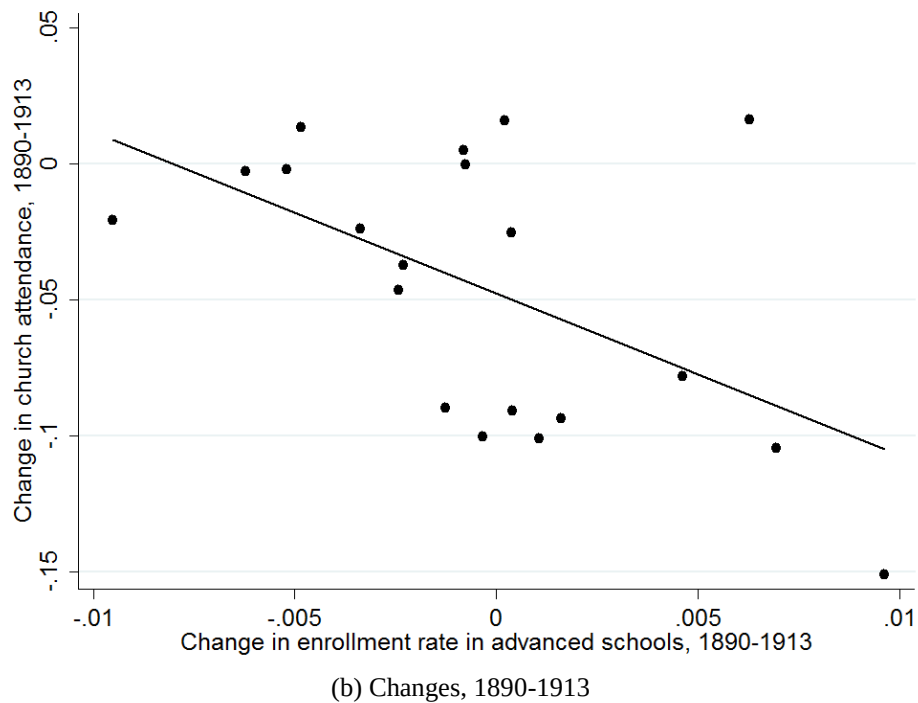
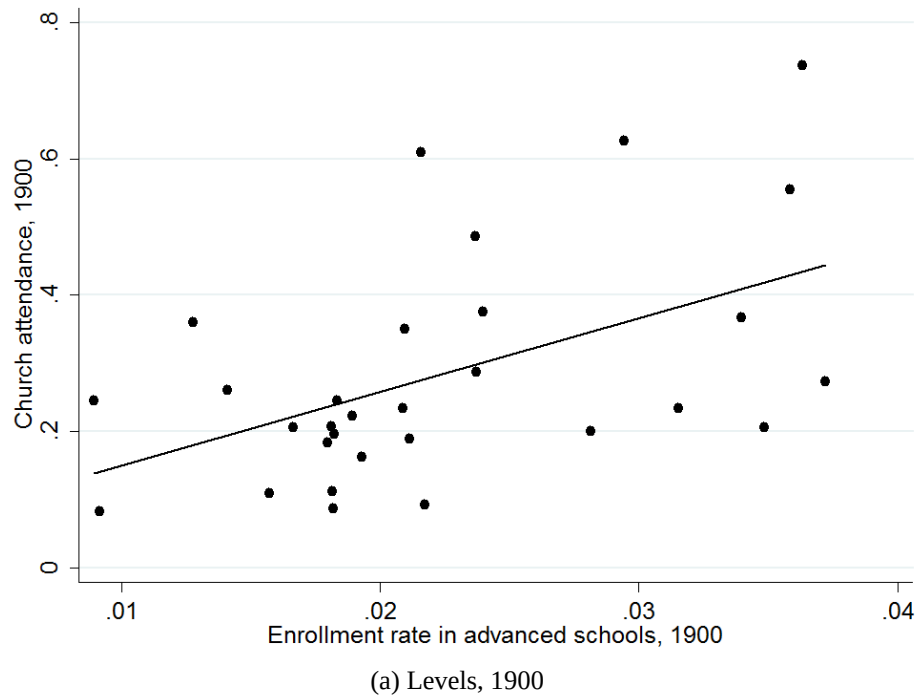
TABLE A2: ROBUSTNESS SAMPLES AND SAMPLES BY PROTESTANT SHARE

<i>Sample:</i>	Cities observed in 1890	No imputations	Similar district size	Non-urban regions	Above 50% Protestants	Below 50% Protestants	Above 75% Protestants	Below 75% Protestants
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enrollment rate in advanced schools	-1.310* (0.791)	-1.798** (0.692)	-1.389** (0.624)	-1.054* (0.538)	-1.708* (0.919)	-0.909** (0.425)	-0.818 (0.997)	-1.901** (0.759)
Municipal tax per capita	0.0005 (0.0005)	-0.00004 (0.0003)	0.0003 (0.0004)	-0.0005 (0.0008)	0.00007 (0.0006)	-0.0005 (0.0003)	-0.00009 (0.0008)	-0.0001 (0.0003)
Population (in millions)	0.0008 (0.011)	0.008 (0.011)	0.005 (0.010)	0.551*** (0.145)	0.005 (0.011)	0.110* (0.060)	0.013 (0.013)	0.077 (0.056)
City and year fixed effects	yes	yes	yes	yes	yes	yes	yes	yes
Observations	173	259	223	111	175	116	130	161
Clusters	25	56	49	26	39	19	27	31
R^2 (within)	0.501	0.458	0.490	0.719	0.468	0.672	0.595	0.490

Dependent variable: church attendance (participations in Holy Communion over Protestants). Unbalanced panel of 61 German cities with over 50,000 inhabitants observed in eight waves that cover every five years from 1890 to 1930 (no data for 1920; 1913 instead of 1915). Enrollment rate in advanced schools refers to enrolled students over city population. Column 1 excludes cities not already observed in 1890. Column 2 excludes observations with imputed dependent variable. Column 3 excludes cities where the number of Protestants in the church district is more than twice as large as in the city in 1910. Column 4 excludes cities situated in urban regions. Columns 5-8 divide the sample along the share of Protestants in a city in 1910. Standard errors clustered at the church district level in parentheses: significance at * 10%, ** 5%, *** 1%.

Data sources: church attendance: Hölcher (2001) based on Sacrament Statistics; other variables: Statistical Yearbooks of German Cities (Neefe (1892) through Deutscher Städtetag (1931)).

FIGURE 1: EDUCATION AND CHURCH ATTENDANCE: LEVELS AND CHANGES



Scatterplots of church attendance against enrollment rate in advanced schools for German cities, (a) 1900 levels and (b) 1890-1913 changes. Church attendance refers to participations in Holy Communion over Protestants. Enrollment rate in advanced schools refers to enrolled students over city population.

Data sources: church attendance: Hölscher (2001) based on Sacrament Statistics; enrollment rate in advanced schools: Statistical Yearbooks of German Cities (Neefe (1892) and following).

TABLE 1: DESCRIPTIVE STATISTICS

	Full Sample		Estimation Sample		
	Mean (1)	Observations (2)	Mean (3)	Minimum (4)	Maximum (5)
Church attendance	0.258 (0.154)	386	0.246 (0.140)	0.049	0.738
Enrollment rate in advanced schools	0.023 (0.008)	443	0.023 (0.009)	0.006	0.050
Municipal tax per capita	33.405 (13.145)	436	32.950 (12.712)	10.747	75.889
Population (in millions)	0.208 (0.347)	522	0.247 (0.433)	0.053	4.337
Share of Protestants (1910)	0.637 (0.293)	514	0.613 (0.286)	0.077	0.968
Prussia	0.625 (0.485)	522	0.601 (0.490)	0	1
Observations			291	291	291

Notes: Sample means. Standard deviations in parentheses. Full sample denotes the entire original dataset. Estimation sample denotes the dataset that is used in the baseline regressions. Church attendance refers to participations in Holy Communion over Protestants. Enrollment rate in advanced schools refers to enrolled students over city population.

Data sources: church attendance: Hölscher (2001) based on Sacrament Statistics; other variables: Statistical Yearbooks of German Cities (Neefe (1892) through Deutscher Städtetag (1931)).

TABLE 2: EDUCATION AND CHURCH ATTENDANCE: PANEL ESTIMATES FOR GERMAN CITIES, 1890-1930

	<i>Estimation method:</i>							
	Pooled OLS		Fixed effects		Fixed effects, lagged explanatory variable		First difference	
<i>Sample period:</i>	Full	Pre-WW I	Full	Pre-WW I	Full	Pre-WW I	Full	Pre-WW I
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enrollment rate in advanced schools	4.508*** (1.483)	6.494*** (1.860)	-1.620*** (0.534)	-1.866** (0.832)	-0.497 (0.574)	-1.152 (0.958)	-2.598 (2.654)	-4.800** (2.183)
Lagged enrollment rate in advanced schools					-0.898* (0.491)	-2.051** (1.017)		
Municipal tax per capita	-0.002* (0.001)	-0.008*** (0.003)	0.00005 (0.0003)	0.0009 (0.0009)	0.0002 (0.0003)	0.0006 (0.0009)	0.0004 (0.0012)	0.0023* (0.0012)
Population (in millions)	-0.016 (0.017)	-0.004 (0.023)	0.009 (0.011)	0.076 (0.056)	0.002 (0.011)	0.051 (0.069)	0.005 (0.014)	0.068 (0.081)
Share of Protestants (1910)	-0.129* (0.068)	-0.113 (0.069)						
Prussia	-0.076** (0.034)	-0.087** (0.035)						
Year fixed effects	yes	yes	yes	yes	yes	yes	no	no
City fixed effects	no	no	yes	yes	yes	yes	no	no
Observations	291	190	291	190	231	142	20	20
Clusters	58	47	58	47	54	42	20	20
R ² (in FE: within)	0.318	0.404	0.493	0.507	0.470	0.548	0.083	0.415

Dependent variable: church attendance (participations in Holy Communion over Protestants). Unbalanced panel of 61 German cities with over 50,000 inhabitants observed in eight waves that cover every five years from 1890 to 1930 (no data for 1920; 1913 instead of 1915). In columns 7 and 8, all variables refer to the change between starting point and end point of the period. Enrollment rate in advanced schools refers to enrolled students over city population. Standard errors clustered at the church district level in parentheses: significance at * 10%, ** 5%, *** 1%.

Data sources: church attendance: Hölcher (2001) based on Sacrament Statistics; other variables: Statistical Yearbooks of German Cities (Neefe (1892) through Deutscher Städtetag (1931)).