Dealing With Adversity: Religiosity or Science? Evidence from the Great Influenza Pandemic*

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Abstract

How do societies respond to adversity? After a negative shock, separate strands of research document either an increase in religiosity or a boost in scientific progress. In this paper, we show that both reactions can occur at the same time, driven by different individuals within society. The setting of our study is the 1918–1919 influenza pandemic in the United States. To measure religiosity, we construct a novel indicator based on the naming patterns of newborns. We measure scientific progress through the share of people in STEM occupations and the universe of granted patents. Exploiting plausibly exogenous county-level variation in exposure to the pandemic, we provide evidence that more affected counties become both more religious and more scientific. Within counties, we uncover heterogeneous responses: individuals from more religious backgrounds further embrace religion, while those from less religious backgrounds become more likely to choose a scientific occupation. Facing adversity widens the distance in religiosity between science-oriented individuals and the rest of the population and increases the polarization of religious beliefs.

Keywords: Religiosity, Science, Innovation, Great Influenza Pandemic

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I Introduction

Throughout history, the occurrence of adverse events—such as natural disasters and pandemics—has posed challenges to societies worldwide and continues to do so today. Understanding how individuals cope with adverse events has key social, economic, and political implications and has been the focus of a vast literature in economics and other social sciences. Specifically, a strand of research documents that negative shocks lead to an increase in religiosity (Bentzen, 2019). Another strand finds that economies react by boosting scientific efforts (Miao and Popp, 2014; Moscona, 2022).[1]

In this paper, we show that these two responses can occur *simultaneously*, making societies both more religious *and* more science-oriented—a finding at odds with the existing evidence documenting a negative relationship between religiosity and science (e.g., Bénabou *et al.*, 2015, 2022; Lecce *et al.*, 2021). To investigate the possible mechanism behind this pattern, we study how individuals *within* society react to an adverse shock. We uncover heterogeneous responses, with religion and science acting as substitute ways through which different individuals react to adversity. These individual-level findings help reconcile our aggregate results with the existing literature.

The setting of our study is the Great Influenza Pandemic (1918–1919) in the United States. Historical records document that many people turned to or strengthened their religious faith to cope with the pandemic. At the same time, the period following the pandemic saw increased scientific progress and fundamental medical advances.[2] To conduct our empirical analysis, we construct a novel data-driven measure of religiosity at a geographically disaggregated level. This measure is based on naming patterns of babies born between 1900 and 1930 from the historical full-count censuses. Complementing this dataset with information from the Census of Religious Bodies, we empirically identify religious names and construct a measure of "revealed religiosity." The underlying idea is that the first name given to a child conveys information on the religiosity of their parents. Our metrics of scientific progress are the share of people in STEM occupations and the universe of geo-coded patents granted in the U.S.[3]

Using a difference-in-differences framework, we first show that counties hit harder by the shock experienced an increase in religiosity. A one-standard-deviation increase in excess deaths—our main measure for the intensity of the influenza shock—led to a 0.16 standard deviation increase in overall religiosity. We further document

[1] For example, Bentzen (2019) documents that, across countries and within regions, individuals become more religious when hit by earthquakes. Moscona (2022) finds an increase in innovation efforts towards technologies that mitigate environmental distress in U.S. counties more exposed to the Dust Bowl during the 1930s.

[2] An increase in religiosity and scientific progress has also been documented after the COVID-19 outbreak. Bentzen (2021), using Google search data, finds a sharp increase in the intensity of prayers during the early days of the pandemic. Agarwal and Gaule (2022) show that the COVID-19 pandemic catalyzed R&D expenditure on pharmaceuticals and digital technologies.

[3] We refer to science and scientific progress interchangeably, and we use two main proxies mentioned in the text.

that these same counties also experienced an increase in scientific progress. A one-standard-deviation increase in excess deaths led to a 0.17 standard deviation increase in overall patenting activity. In addition, we find that employment in scientific occupations grew in counties hit harder by the pandemic. This effect is mainly due to the occupational choices of young cohorts. Event-study analyses illustrate the absence of pretrends, providing further support for the validity of the research design. Interestingly, as a result of the contemporaneous increase of religiosity and science, their relationship turned from negative before the pandemic to positive afterward. This is especially puzzling because it contrasts with the existing evidence documenting a negative relationship between the two (Bénabou *et al.*, 2015, 2022; Lecce *et al.*, 2021).

What is the mechanism behind the contemporaneous increase in religiosity and science? To answer this question, in the second part of the analysis, we move to a within-county analysis and study individual-level reactions to the pandemic. We obtain three main results.

First, we find that individuals from more religious backgrounds were more likely to turn to religion in the aftermath of the pandemic, while those from less religious backgrounds were more likely to select a scientific occupation.[4] This pattern suggests that individuals coped with negative shocks in heterogeneous ways: some turned to religion, while others turned to science. Second, we show that science-oriented individuals became less religious than the rest of the population after the shock. Third, we document that the pandemic widened preexisting differences in religious sentiment. Individuals from more (less) religious backgrounds became even more (less) religious. Consequently, the distribution of religiosity in counties more exposed to the pandemic became more polarized. Importantly, the individual-level analysis reconciles the county-level findings with the existing literature. While a county may have become more religious and innovative, individuals seemed to react differently to the same shock—based, for instance, on their religious background or pre-pandemic scientific orientation. Religiosity and science appear to have been alternative ways of reacting to the pandemic, with individuals becoming even more distant in terms of their religious sentiment than before the shock.

We perform several checks to gauge the robustness of our findings. For both religiosity and scientific progress, we show that our results are robust to weighting regressions by county population and when running our analysis at the city level. These exercises suggest that our findings are not driven by small counties or by individuals residing in rural areas. Second, we perform a series of checks on our religiosity measure. In particular, we validate it internally across several dimensions (e.g., by computing our indicator excluding firstborn babies and accounting for potential heterogeneity in fertility patterns). In addition, we run a few robustness checks to ensure that the increase in religiosity is not driven by migration.[5] Next, we externally validate our data-driven

[4] We measure religious background using individuals' names (as opposed to their children's), aiming to capture the religious upbringing of a person instead of their current faith.

[5] In particular, we first run a placebo exercise where we test for the impact of the pandemic on the names of adults. The results show no impact of the shock on adults' names, which we interpret as evidence that the observed increase in religiosity was not driven by ex-ante

measure of religiosity by using alternative indicators. In particular, we show that results are robust when using the share of biblical and saints' names and the share of people affiliated with a religious denomination. We also perform a series of robustness checks on our measure of innovative activity (e.g., we show that the increase in patenting was not driven by low-quality innovations). Finally, we address the concern that other factors may be related to the pandemic and may have contemporaneously affected the evolution of religiosity and science, confounding our results. To do so, we start by documenting that initial religiosity and scientific progress are not related to the intensity of the shock. Then, using an event-study design, we show that religiosity and innovation were on a similar path across treated and control groups before the pandemic. Additionally, we rule out that a separate yet overlapping shock—World War I—may partly explain our findings.[6] Taken together, our empirical results, supported by historical records, provide evidence that the influenza pandemic was conceivably the main driver behind the aggregate increases in both religiosity and scientific progress.

Concerning our within-county results, one key question is why some individuals became more religious while others selected a scientific occupation. Our findings on religiosity are in line with the religious coping hypothesis, which posits that religious faith can represent a coping device to deal with personal distress following a negative shock.[7] What motivated people to turn to science is less obvious. We propose a broad interpretation of "scientific coping," with individuals turning to science either to deal with their psychological distress—as in the case of religious coping—or to try to actively mitigate the negative (e.g., health- and economic-related) effects of the pandemic.[8] While our findings cannot directly uncover the individual-level motivations behind these different behaviors—this would go beyond the scope of this paper—they show that people from different backgrounds may have reacted in different ways to the same shock and that this may have increased the polarization of religiosity within society.

Related Literature. This paper is closely related to the literature studying how societies react to negative shocks. Previous work has shown that, in accordance with the religious coping hypothesis (Pargament, 2001; Ano and Vasconcelles, 2005; Norenzayan and Hansen, 2006), natural disasters are associated with an increase in religiosity, both historically (e.g. Belloc *et al.*, 2016; Bentzen, 2019) and in contemporary scenarios (Sibley and Bulbulia, 2012; Bentzen, 2021).[9] Another set of studies documents that economic crises (Babina *et al.*,

more religious people moving to areas hit harder by the shock. Then, we show that our results hold when excluding from the estimation sample all those who, in the 1930 census, reside in a state different from the one where they were born.

[6] For a systematic overview of alternative mechanisms and the corresponding robustness, see the summary table in Appendix C.

[7] An alternative explanation could be that individuals turn to religion as an insurance mechanism against the negative economic effects of the pandemic. While we cannot fully exclude this channel, we believe it is unlikely (as discussed in Section VI).

[8] Another possibility is that individuals turned to science because of increased labor demand in STEM occupations. However, the heterogeneity by religious background suggests that, beyond market forces, individual preexisting religiosity played a key role in their decision to turn to science.

[9] The religious coping hypothesis, first developed in the psychology literature, posits that people who are subject to economic and social

2022), wars (Gross and Sampat, 2021), climate change (Miao and Popp, 2014; Clemens and Rogers, 2020; Moscona, 2022), and pandemics (Gross and Sampat, 2021; Agarwal and Gaule, 2022) all shape innovation activity. To the best of our knowledge, this is the first study to provide evidence that natural disasters may foster a contemporaneous increase in religiosity *and* innovation, and also the first to document the ensuing polarization of religiosity within society.[10]

Additionally, we inform the broad literature on the economics of religion, pioneered by Weber (1905). In particular, we contribute to those studies that analyze the linkage between religiosity and science.[11] While most papers adopt a historical (Deming, 2010; Mokyr, 2011), theoretical (Bénabou *et al.*, 2022), or cross-sectional perspective (Bénabou *et al.*, 2015, 2022), to our knowledge, we are the first to study the interaction between religion and science in a panel setting and to uncover the individual-level dynamics behind their coevolution.[12]

Finally, we contribute to a growing literature that exploits the informational content of names to capture individuals' characteristics. Names have been used, for example, to measure race and ethnicity (Abramitzky *et al.*, 2016; Fouka, 2019), individualism (Bazzi *et al.*, 2020), socioeconomic background (Biavaschi *et al.*, 2017; Olivetti *et al.*, 2020), nationalism (Jurajda and Kovač, 2021), and religiosity (Abramitzky *et al.*, 2016; Andersen and Bentzen, 2022). While all of these papers assume a preexisting rule to classify names (e.g., whether one has a biblical or saint name or a name shared by a major religious figure), to the best of our knowledge, we are the first to identify the religiosity of names *directly from the data*.[13]

OUTLINE OF THE PAPER. The paper is structured as follows. Section II describes the Great Influenza Pandemic in the United States and discusses the historical evidence on its effects on religiosity and innovation. In Section III, we present the data and our indicator of religiosity. In Section IV, we explain the empirical strategy and the core results. In Section V, we explore the possible mechanisms underlying our findings. Section VI discusses the results and the limitations of the analysis. Section VII concludes.

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shocks turn to religious faith as a coping device to deal with personal distress.

[10]Many studies have looked at the impact of natural disasters on, among others, social norms (e.g. Posch, 2022), migration (e.g. Boustan *et al.*, 2012), and economic activity (e.g. Boustan *et al.*, 2020).

[11]Other studies analyze the relationship between religion and accumulation of human capital, more broadly (Becker and Woessmann, 2009; Botticini and Eckstein, 2012; Squicciarini, 2020). For an overview of the literature on the economics of religion, see Iannaccone (1998), Iyer (2016), and Becker *et al.* (2021).

[12]Lecce *et al.* (2021) study how religiosity impacts the birth and migration of scientists in 19th-century French cantons, and Andersen and Bentzen (2022) show that individuals with religious names are less likely to become engineers, scientists or doctors and that cities with more religious individuals grew slower. However, none of these studies analyze how an adverse shock affects society's heterogeneous response in terms of religion and science and the underlying individual-level dynamics.

[13]For details on how we construct our religiosity measure, see Section III.

II Historical Background

This section provides an overview of the Great Influenza Pandemic in the United States and how it influenced religion and innovation.

II.A The Great Influenza Pandemic

Between 1918 and 1919, the Great Influenza Pandemic—also known as the "Spanish Flu"[14]—killed an estimated 40 million people worldwide (approximately 1 in 30 people); it was one of the deadliest natural disasters in modern times (Barro *et al.*, 2020). In the United States, the pandemic started in the spring of 1918 with sporadic outbreaks. Then, a second, more severe wave began in September 1918. The final wave started in January 1919, ending that spring. In total, it killed about 500,000 Americans, corresponding to 0.7% of the U.S. population (Crosby, 1989).[15]

Historical and modern accounts suggest that the pandemic hit across the U.S. quasi-randomly. The National Research Council stated that neither demographic characteristics, such as the ethnic composition of the population, nor geographic factors seemed to explain the difference in the intensity of the pandemic across the country. Crosby (1989) writes that the states with the highest mortality displayed diverse geographical, climatic, and demographic characteristics. The pandemic hit with varying intensity within states as well. For example, in Minnesota, the death rate in Saint Paul was about 70% higher than in Minneapolis, despite the two cities being just 8 miles apart. In Ohio, Dayton experienced an 80% higher mortality rate than Columbus, even though the two cities had similar demographic characteristics (Huntington, 1923; Almond, 2006).

The infection was caused by strains of the A/H1N1 influenza virus, whose origin is still unknown. Neither antiviral drugs to treat the primary disease nor antibiotics to cure secondary bacterial infections were available. Doctors had to rely on an array of mostly ineffective—sometimes fatal—medicines such as aspirin and quinine (Spinney, 2018). It is debated whether nonpharmaceutical interventions (NPIs)—such as using masks, canceling public events, closing schools, and implementing isolation measures and quarantines—were effective in limiting the spread of the disease.[16]

[14] The Great Influenza Pandemic is popularly known as "Spanish Flu" because media in Spain—which was neutral during World War I (WWI)—were free to report news on this disease. Conversely, countries involved in WWI imposed press censorship on the topic. This gave the (incorrect) impression that Spain was either more severely hit by the disease or that the pandemic had originated in Spain.

[15] By comparison, COVID-19 caused 1.13 million deaths in the United States, approximately 0.3% of the U.S. population, between March 2020 and February 2023 (https://covid.cdc.gov/covid-data-tracker/#datatracker-home; accessed February 12, 2023).

[16] Some authors assert that NPIs were effective in reducing mortality (e.g., Markel *et al.*, 2007; Berkes *et al.*, 2022), while others show that the effect of NPIs on overall deaths was small and statistically insignificant (e.g., Barro, 2022).

II.B The Pandemic and Religion

A large literature documents that individuals become more religious in response to adverse events. One explanation comes from the "religious coping hypothesis," which posits that individuals turn to religious beliefs or practices as a way to cope with sudden dramatic circumstances (Pargament, 2001).[17]

The influenza pandemic inflicted substantial emotional and socioeconomic distress and could have acted as a powerful amplifier of religious sentiments (Phillips, 2020). Historical records document that spiritualism gained momentum in the aftermath of the pandemic. Not all confessions reacted in the same way. In the United States, modern evangelism benefited from the pandemic, as evidenced by a sharp rise in the circulation of evangelical magazines (Frost, 2020). Membership in Christian Science also soared during these years, reaching an all-time peak in the 1930s.[18] Catholics and Orthodox Jews identified the influenza as a manifestation of divine anger, the expiation of which called for prayers. On the other hand, some groups of progressive Protestants called for a more scientific interpretation of the pandemic (Phillips, 2020).[19]

II.C The Pandemic and Science

Historical evidence suggests that the period after 1918 was one of sharp intellectual and scientific progress and that the Great Influenza Pandemic was particularly influential in shaping the development of medical sciences (Barry, 2020). Despite being ineffective during the pandemic, medicine evolved enormously in subsequent years. In 1928, Alexander Fleming discovered the medical use of penicillin in treating bacterial infections. By the 1930s, virology had become an established branch of medicine, and the first influenza vaccines were being developed (Spinney, 2018). During this time, medicine became more "scientific" and, hence, effective (Barry, 2020).

These advancements in medicine went hand in hand with increased trust in scientific progress. For instance, in her personal journal, Canadian author L. M. Montgomery wrote, "[...] the Spirit of God no longer works through the church for humanity. It did once but it has worn out its instrument and dropped it. Today it is

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[17] For example, Bentzen (2019) documents that individuals become more religious when hit by earthquakes. Religion may also represent an insurance mechanism when negative shocks occur: Ager *et al.* (2016) shows that after the 1927 Great Mississippi Flood, demand for social insurance led to higher churchgoing, while Ager and Ciccone (2018) document that in the 19th-century United States, a larger share of the population was organized in religious communities in counties that were exposed to higher common agricultural risk.

[18] Christian Science, founded in 1879, is part of the religious movements belonging to the metaphysical family. It seeks to restore the healing and thaumaturgic virtues of primitive Christianity and has been associated with avoidance of mainstream medicine (Stark, 1998).

[19] There were also conservative Protestant churches, such as those in the Bible Belt—i.e., the region chiefly comprising Alabama, Arkansas, Georgia, Kentucky, Louisiana, Mississippi, Missouri, North Carolina, Oklahoma, South Carolina, Tennessee, and large parts of Florida and Texas—refractory to scientific and medical advancements.

working through Science" (Montgomery, 1992[1924], p. 211). Barry (2020) argues that the pandemic was the key driver behind this paradigm shift because it fostered scientific thinking in the face of such a sudden and dramatic shock.

This overview suggests that the 1918–1919 pandemic fostered both scientific progress and religiosity—a result that might seem at odds with theoretical and empirical evidence, which depicts religion and science as opposing forces (e.g., Bénabou *et al.*, 2015, 2022; Lecce *et al.*, 2021). In this paper, we provide causal evidence that the influenza shock led to a simultaneous increase in religiosity and scientific progress at the aggregate level. We then reconcile this apparent puzzle by showing that it induced polarization within society, with some people turning to religion and others turning to science.

III Data

To conduct our analysis, we construct a new dataset that combines information on religiosity, scientific progress, and the incidence of the Great Influenza Pandemic. This section describes the outcome variables and the main explanatory variables. Appendix A describes the data in detail. In the first part of the analysis, counties are the geographical unit of observation.[20] In the second part of the analysis, we use individual-level data. Table B.1 provides descriptive statistics for the main variables.

III.A Religiosity Measure

The key challenge when studying religiosity is that it is not easy to measure it, today as well as in the past. It is especially challenging to find an indicator of religiosity that combines geographical granularity and high-frequency time variation.[21]

In our analysis, we propose a novel measure of revealed religiosity based on the naming patterns of newborn babies. The motivating argument is that parents who give comparatively more religious names are more likely to be religious themselves. Therefore, naming patterns provide a measure of "revealed religiosity" of parents, rather than of the children themselves.[22]

[20] To address concerns related to counties changing their boundaries over time, we use 1920 counties as our geography of reference.

[21] This lack of data is clear in historical settings—Squicciarini (2020), for instance, uses different measures of religiosity, available for only a few points in time—but it poses substantial limitations to contemporary studies as well. Recent papers leverage information from surveys such as the World Value Survey to measure religiosity (Bénabou *et al.*, 2015, 2022). Yet, because waves are typically years apart, survey-based measures are not useful for studying the dynamics of religiosity at high time frequency.

[22] A natural corollary is that names carry informational content on the religiosity of an individual's background: while we cannot infer that an individual called "Paul" is comparatively more religious than one named "Harold," we assume that the parents of "Paul" are likely to be more religious than those of "Harold."

We now describe how we compute the religiosity score associated with first names. The key advantage of this approach is that it allows us to obtain a disaggregated yearly measure of religiosity and to study its changes in the short-to-medium term. The metric we define is conceptually similar to that developed by Abramitzky *et al.* (2016) and Andersen and Bentzen (2022), who measure religiosity, depending on whether children have a biblical/saint names or names of a major religious figure, respectively. Our approach differs from theirs: we *empirically* identify our religious names, using data on the entire population of newborns and existing indicators of religiosity.

III.A.1 Estimating Religiosity Scores for First Names

We use two main sources to compute religiosity scores. First, we construct naming patterns at the county-cohort level from the full-count U.S. censuses between 1900 and 1930 (Ruggles *et al.*, 2021). More precisely, we take the first name of all children born in the United States between 1896 and 1930 and collapse them at the name-county-cohort level, thus obtaining a panel of name-county pairs at a yearly frequency.[23] Next, we use county-level data from the Census of Religious Bodies. This census—taken once every ten years between 1906 and 1936—allows us to construct, for every county and census decade, the share of people affiliated with any religious denomination, as well as the share of people affiliated with a Catholic or Protestant one.[24]

To obtain the religiosity scores, we first compute the frequency of names. We denote with N_{cd} the total number of individuals born in county c in decade d and with Name Frequency$^k_{cd}$ the number of children with name k born over the years $[d-10, d)$ in county c. Then, we estimate the following model:

$$y_{cd} = \alpha_c + \alpha_d + \beta \times \log(N_{cd}) + \sum_{k=1}^{K} \gamma^k \times \log(1 + \text{Name Frequency}^k_{cd}) + \varepsilon_{cd}, \tag{1}$$

where y denotes either the share of people affiliated to any denomination, or the share of Catholics, or the share of Protestants in county c in decade d; d corresponds to the two prepandemic decades of the religious censuses (1906 and 1916); α_c and α_d are, respectively, county and decade fixed effects.[25] The term K is the total number of names that occur in at least 0.3% of the overall sample.[26] To measure name frequencies, we

[23] A cohort is defined as all babies born in a given year. The first cohort in our sample comprises all babies born in 1896. The underlying rationale is that the first Census of Religious Bodies was published in 1906, and we consider the ten cohorts preceding that year.

[24] To gather information on the number of religious members in each county, a report was obtained directly from local churches and congregations. The shares are computed as the number of people affiliated with these groups, normalized by the population of each county. Our analysis focuses on Catholics and Protestants, as they jointly account for more than 90% of the people enumerated by the census.

[25] In one of our robustness checks, we compute an alternative measure of religiosity that does not include any fixed effect. The results hold. Moreover, the results are robust to including the raw name frequencies in equation (1) or, alternatively, apply an inverse hyperbolic sine transformation and add .01 to the frequency inside the log.

[26] We follow Fouka (2020) and restrict the number of names included in model (1) primarily to avoid overfitting. Fouka (2020) uses a threshold of 1,000 for a name to be included in the analysis. In our preferred specification, we instead consider all names whose share

include all babies born within ten years before each pre-pandemic census. Hence, we restrict the sample to cohorts between 1896 and 1916. Then, we aggregate these frequencies from cohorts to decades to estimate equation (1).

We label the coefficient (γ^k) as the *religiosity score* associated with name k; we interpret names with larger estimated religiosity scores ($\hat{\gamma}^k$) as conveying a more intense religious sentiment. Because model (1) includes county fixed effects, larger religiosity scores are attached to names that become comparatively more frequent in counties that experienced larger increases in religiosity. In Figure I, we report the estimated religiosity scores from model (1), where the outcome variable is the share of people affiliated with any religious organization. The figure shows that typically religious-sounding names, such as "Joseph," "Paul," and "Elizabeth," all feature positive and large estimated religiosity scores. Because our estimation method seeks to isolate *distinctively* religious names, relatively common ones such as "Mary" or "John" are associated with negative scores. In the case of "Mary," for instance, its popularity during this period was such that religious and non-religious people alike used it, thus preventing it from being associated with distinctively religious people. Moreover, names with little connection to saints or biblical episodes are associated with negative religiosity scores. This is the case for Germanic names, such as "Bertha," "George," and "Harold," and other non-religious names, such as "Pearl." By considering the shares of people affiliated with Catholicism or Protestantism, we can also obtain religiosity scores for both religious denominations separately. Figure B.1 reports the results.

III.A.2 A Yearly County-Level Measure of Religiosity

From model (1), we obtain a set of estimated religiosity scores $\{\hat{\gamma}^k\}_{k=1}^K$, which we use to construct a *yearly* indicator of religiosity at the county level. More specifically, our synthetic measure of religiosity is defined as the predicted values of model (1):

$$\hat{y}_{ct} = \sum_{k=1}^{K} \hat{\gamma}^k \times \log(1 + \text{Name Frequency}_{ct}^k), \tag{2}$$

where t denotes a cohort between 1900 and 1930. In addition, by considering religiosity scores associated with different denominations, we can construct a synthetic series for Catholic and Protestant religiosity separately.

A concern about our religiosity indicator is how much variation in county-religiosity names explain, net of that captured by fixed effects. In Appendix B, we provide several robustness and validation exercises for our synthetic measure. First, Figure B.2 provides county-binned scatters of synthetic and measured religiosity by denomination. The figure summarizes the results from two distinct exercises. Plots in the left column show in-sample correlations, thus comparing Census-measured and predicted religiosity in 1906 and 1916. Plots in

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in our overall sample is at least 0.3% and run checks around this threshold to assess the robustness of our results. We include name frequencies in log to reduce their skewness and add one to avoid dropping all counties where there is at least one name with no newborn children (approximately 40% of the sample).

the right column compare synthetic and measured religiosity in 1926 instead.[27] We refer to this as an "out-of-sample" correlation, as data from the Censuses of Religious Bodies carried out after the pandemic are not used to estimate religiosity scores. All graphs show a positive correlation between actual and predicted religiosity across all denominations. This exercise provides reassuring evidence that naming patterns capture meaningful variation in overall religiosity and further validates our measure.

Next, following Abramitzky *et al.* (2016), we use biblical and saint names as an alternative name-based measure of religiosity. Finally, as additional indicator of religious sentiment, we use the county-level share of the population with a religious affiliation (for all affiliations, and separately for Catholics and Protestants) recorded by the Census of Religious Bodies for 1906, 1916, and 1926.

III.B Measuring Scientific Progress

We measure scientific progress at the local level using the share of individuals employed in STEM occupations. The rationale for this measure is that a STEM occupation requires that an individual receive a science-oriented education. In turn, receiving a science-based education plausibly correlates with more favorable attitudes toward science and scientific progress at the local level (Bianchi and Giorcelli, 2020; Biasi *et al.*, 2022). For each county and census year (1900 to 1930), we compute the share of individuals employed in a STEM occupation relative to (i) the entire population; (ii) the number of people employed in high-skilled occupations.[28] We also use these two classifications into STEM and non-STEM occupations when performing the individual-level analysis.

We complement our main measure of scientific progress by using patent data from the Comprehensive Universe of U.S. Patents (CUSP; Berkes (2018)). The CUSP contains information about the universe of U.S. patents issued between 1836 and 2015. The data for the time period considered in our paper (1900–1930) are extracted from digitized patent documents obtained from the U.S. Patent and Trademark Office. For the purpose of our analysis, we first assign each patent to a county, based on the residence of its inventor, and a year, based on the year in which the patent was filed. When a patent lists multiple inventors, we give equal weights to the location of each inventor. From the CUSP, we also collect the technology classes associated with each grant (according to the U.S. Patent Classification system) and assign them to technology groupings following the

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[27] Our results do not change if we include data from the 1936 Census of Religious Bodies. However, growing discontent resulted in substantially lower reporting rates in this last Census for some religious groups. Following Stark (1992), we consider it less reliable and exclude it from our analysis.

[28] This second measure increases the comparability of the control group with STEM individuals. Table B.2 lists the set of occupations that we label as STEM (Panel A) and the occupations that we classify as high-skilled (Panel B). By construction, STEM occupations are also high-skilled. Individuals in STEM occupations represent approximately 6% of those employed in skilled professions in the 1930 census.

crosswalk provided by the National Bureau of Economic Research (Hall *et al.*, 2001).[29] Importantly, while the use of patents as a proxy for scientific progress may be subject to debate (see, e.g., Moser, 2005), the main advantage of these data is that they are available at a high-frequency. We will especially use them to quantify the dynamic treatment effect of the influenza.

III.C Exposure to the Great Influenza Pandemic

To measure the incidence of the Great Influenza Pandemic across U.S. counties, we use mortality statistics assembled by the U.S. Department of Commerce. These were first collected in 1915, and throughout the 1915–1918 period, they covered 1,274 counties (40% of the total), accounting for more than 60% of the U.S. population. We follow the methodology developed by Beach *et al.* (2020) and measure mortality caused by the flu as average deaths during the flu period (1918–1919) relative to the three years before the pandemic (1915–1917). Formally, excess mortality in county c is defined as

$$\text{Excess Deaths}_c = \frac{\frac{1}{2}\sum_{t=1918}^{1919} \text{Deaths}_{ct}}{\frac{1}{3}\sum_{t'=1915}^{1917} \text{Deaths}_{ct'}}. \tag{3}$$

This measure represents our baseline treatment. We also report results from a categorical treatment variable equal to one if the baseline treatment (Excess Deaths$_c$) is above its median and zero otherwise. Figure II displays the geographical variation in the intensity of the pandemic in terms of excess deaths. We find that the severity of the pandemic varies substantially across counties, even geographically close ones. The rationale behind our excess-mortality measure is that—all else being equal—deaths during the pandemic that exceed those before the pandemic are likely due to the pandemic itself. A possible threat to this argument might be the U.S. involvement in WWI and the fact that WWI deaths are confounding our results. However, there does not appear to be a significant correlation between WWI morality and the pandemic, as displayed in Figure B.4. In Section IV, we show that our results are robust to controlling for a post-1918 time indicator interacted with WWI-related deaths.

IV Main Results: County-Level Analysis

In this section, we present the baseline empirical strategy and we show that exposure to the influenza pandemic led to an increase in both religiosity and scientific progress across counties. Then, we explore the mechanism behind the aggregate patterns and provide evidence of heterogeneous responses to the pandemic *within* counties.

[29] Whenever a patent is assigned to more than one field, we split it with equal weights across fields. We conflate the "chemical" and "drugs" NBER classes into a single class, which we label "pharmaceuticals." We follow this approach because of the high collinearity between the number of chemical and drug patents at the county level, which would make it difficult to study them separately. All results for the pharmaceutical class also hold if we consider drug and chemical patents separately. An example of a pharmaceutical patent is shown in Figure B.3. For historical consistency, we relabel the "computer and communication" class as simply "communication."

IV.A Empirical Strategy

In the first part of the analysis, we study the impact of the pandemic separately on religiosity and scientific progress at the county level. Our sample consists of a panel of U.S. counties observed over the 1900–1929 period at a decade or yearly frequency. In particular, we leverage quasi-random variation in exposure to the pandemic across U.S. counties in a difference-in-differences (DiD) setting and estimate regression models of the form:

$$y_{ct} = \alpha_c + \alpha_{s(c)\times t} + \boldsymbol{x}'_{ct}\boldsymbol{\beta} + \delta \times (\text{Post}_t \times \text{Excess Deaths}_c) + \varepsilon_{ct}, \tag{4}$$

where the subscripts c and t denote county and time (decade or year), respectively; y_{ct} measures either religiosity or scientific progress; α_c and $\alpha_{s(c)\times t}$ are county and state-by-time fixed effects; Post_t is an indicator variable equal to one if $t \geq 1918$ and zero otherwise; Excess Deaths_c measures the intensity of the pandemic in terms of excess deaths, as explained in Section III.C; and ε_{ct} is the error term. In addition, in all regressions, we control for an interaction term between 1910-population and the post indicator $\boldsymbol{x}'_{ct}$. Standard errors are clustered at the county level. Our coefficient of interest, δ, captures the impact of the pandemic on religiosity or scientific progress. To investigate possible heterogeneity of treatment effects over time, we also estimate a more flexible model that, rather than interacting Excess Deaths with the Post indicator, interacts Excess Deaths with biennial time dummies:[30]

$$y_{ct} = \alpha_c + \alpha_{s(c)\times t} + \boldsymbol{x}'_{ct}\boldsymbol{\beta} + \sum_{\substack{k=1909 \\ k\neq 1917}}^{1928} \delta^k \times I\{t = k \,|\, t = k+1\} \times \text{Excess Deaths}_c + \varepsilon_{ct}, \tag{5}$$

where $I\{t = k \,|\, t = k+1\}$ is an indicator variable that takes value one if t is in the two-year window indexed by k, and zero otherwise.

Did the influenza spread randomly? We perform three main exercises to test this in the data. First, in Table B.3, we report the correlation between the intensity of the pandemic and religiosity, scientific progress, and a set of county covariates, all measured in 1910, the last census before the pandemic (or in the decade 1901–1910 in the case of yearly variables) accounting for population and state-level fixed effects.[31] Counties more exposed to the pandemic are observationally equivalent with respect to all variables except the share of foreigners. This aligns with the pandemic being comparatively more severe in urban areas, where immigrants were clustered.

Additionally, to rule out that these differences confound our analysis, we check whether control and treatment counties were on different trends before the shock by estimating event studies. Formally, in Equation (5), this implies that the estimates of δ^k would not be statistically different from zero before the pandemic hit, i.e., for all

[30] In the dynamic DiD specifications, we code periods over two-year windows to reduce noisy fluctuations in estimated treatment effects and to improve efficiency by pooling observations. We consider a 10-year period before and after 1917-18.

[31] State fixed effects control for the fact that the pandemic spread from East to West between August 1918 and November 1918.

$k < 1917$.[32] Our estimates support the parallel-trends assumption. However, this approach could still be invalid in the presence of shocks correlated with the intensity of the pandemic that positively affected both science and religiosity but that were *not* caused by the pandemic itself. A plausible candidate is the number of soldiers that counties lost in WWI: their deaths might have driven either the religiosity of their families or the ability (or motivation) of a county to produce innovation (or both). To test for this possibility, in Tables B.5 and B.13, we control for the number of deaths in WWI in our regression model and show that the results remain robust.

IV.B The Effect of the Influenza Pandemic on Religiosity

Table I displays the DiD estimates focusing on religiosity. In columns (1–3), the dependent variable is the share of individuals affiliated with any religious denomination (column 1), with a Catholic religious denomination (column 2), or with a Protestant religious denomination (column 3), as enumerated in the Census of Religious Bodies. The estimates suggest that counties comparatively more exposed to the pandemic experienced an increase in religiosity, with no significant differences between Catholics and Protestants.

This measure of religiosity has the advantage of including the U.S. population across different age groups. However, it has two main caveats: (i) census-based religiosity is available only at three points in time (1906, 1916, and 1926) and thus does not allow us to study high-frequency variation in religiosity; (ii) the choice to join a religious denomination could be more likely to be affected by social insurance considerations, rather than by religious reasons, thus inducing an upward bias in our results (Ager and Ciccone, 2018). Thus, in columns (4–6), we use our main indicator of religiosity, the name-based measure described in Section III.A. This measure allows us to observe counties every year between 1900 and 1929. We find that a one-standard-deviation increase in excess deaths led to a 0.16 standard deviation increase in name-based religiosity at the county level (column 4). Similarly, moving from a county at the 25th percentile of the excess mortality distribution to one at the 75th percentile led to an increase in religiosity of 10%.

In Figure III, we report the coefficients of the interactions between the treatment variable and biennial dummies using the name-based measure of overall religiosity as the dependent variable. The event study estimates support the patterns observed in the DiD analysis and confirm the absence of pre-trends. In addition, we observe that the increase in religiosity appears to persist over the decade after the pandemic. These findings are in line with the literature documenting a substantial persistence of religiosity (e.g., Squicciarini, 2020).

We now perform a series of additional exercises to gauge the robustness of these findings. First, one may be

[32] Since the setting is not staggered—because the pandemic hit each county in the same period—models (4) and (5) can be estimated through standard two-way fixed effects (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). Callaway *et al.* (2021), however, caution against using continuous treatments. We code a binary indicator equal to one for counties with above-median excess deaths. Throughout the paper, we show that the continuous and binary treatments yield qualitatively similar results.

worried that our results were driven by small counties where the variation in the share of individuals affiliated with religious denominations and in naming patterns may have been more substantial. Table B.4 replicates the specifications of Table I, weighting regressions by county population in 1910. The baseline findings are confirmed.

Next, we focus on our name-based measure of religiosity and perform three sets of exercises. First, we show that our results hold when changing the specification or the sample. In column (2) of Table B.5, we code the treatment as a binary variable equal to one if the baseline treatment is above its median and zero otherwise. Then, column (3) controls for mortality due to WW1. In addition, one concern related to our religiosity measure could be that firstborns are often named after a deceased grandparent. Thus, their names would reflect the higher religiosity of previous generations rather than their parents' religious attitudes. If, due to higher mortality, households in areas more affected by the pandemic were also more likely to have recently lost a grandparent, then our results might reflect a mechanical effect. Column (4) reports estimates when dropping firstborn children in every household. Another concern is that numerous households may display different naming behaviors for later-born children. In column (5), we drop children beyond the fourth. Then, if religious families displayed higher fertility rates, one may worry that our results are driven by an increase in the number of religious names due to the higher fertility of already religious households. In column (6), we compute within-household average religiosity to check whether our findings are driven by larger households and differential fertility. All results hold through these alternative specifications. Another concern could be that comparatively more religious people moved into counties where the pandemic had been more severe, perhaps motivated by slacker labor markets. If that were the case, our estimated pandemic effect on name-based religiosity would reflect movers' religiosity and fertility. To address this concern, we compute a county-decade measure of religiosity based on the names of the adult population only. The in-migration mechanism would predict a positive impact of the pandemic on this variable. Estimates reported in column (7) show no evidence of any such effect, thus ruling out this potential alternative interpretation.

Table B.6 addresses the possibility that immigration confounds our results. In particular, one may be worried that immigration into more exposed areas by selectively more religious individuals may drive our estimates. We thus exclude all those who, in the 1930 census, are recorded residing in a state that is different from the one where they were born. The results remain unchanged. Finally, we explore the effect of the pandemic within urban areas using the city-level sample constructed by Clay *et al.* (2019) and discussed in Appendix A.VIII. In columns (1–3) of Table B.7, we estimate model (5) using a balanced panel of cities observed over the 1900–1929 period. The name-based religiosity measure is computed leveraging variation in naming patterns of children born in each city. Results are consistent with the baseline county-level analysis: Religiosity increased in cities more severely affected by the pandemic. This exercise ensures that our results are not driven by individuals residing in rural areas. Moreover, the city-level sample includes several cities in Southern states, which were plausibly more religious.[33] We thus view the city-level exercise as shedding additional internal validity to the county-level analysis.

In the second set of exercises, we test whether the results are robust to alternative ways of constructing our religiosity measure. First, in Table B.8, we report the baseline result, but using religiosity scores estimated through equation (1) *without* county fixed effects. These scores are thus obtained using the "stock" of religiosity in a given county instead of its deviations from the mean. The results from this alternative strategy are consistent with our baseline estimates. Second, we test the robustness of our results to the number of names included in the sample. In our baseline analysis, we exclude names appearing in less than 0.3% of the overall population. Table B.9 shows that our findings are qualitatively unchanged under different frequency thresholds. Next, a possible concern could be that the results capture a "fashion" effect, whereby more religious names became more fashionable after the pandemic. If this were the case, even though the initial increase in religious names would suggest a positive shift in religiosity, the effect for the following periods would be biased upwards and driven by this fashion effect. In Table B.10, we regress a set of indices reflecting the concentration of the name distribution against our baseline treatment and find no evidence of such a mechanism.

Finally, we perform our analysis using an alternative indicator of religiosity. In particular, in Table B.11, we use biblical and saint names as an alternative name-based measure of religiosity, following Abramitzky *et al.* (2016). We find that the number of children named after biblical or saint names increased in areas more exposed to the pandemic after 1918. In addition, in Figure B.5, we show that the county-level share of Biblical and Saint names, computed using data from Abramitzky *et al.* (2016), is strongly and positively correlated with the religiosity measure constructed using our data-driven approach.

Overall, we document that the pandemic positively affected religiosity through different specifications, samples, and indicators. This finding is consistent with the religious coping hypothesis, which posits that religion may serve as a coping device to deal with mental and psychological distress (e.g., Pargament, 2001; Bentzen, 2019, 2021).

IV.C The Effect of the Influenza Pandemic on Scientific Progress

We now turn to study how the influenza pandemic impacted scientific progress. We show that the pandemic positively impacted the share of people in STEM occupations and patenting activities.

Table II (column 1) runs the specification of model (4) using as the dependent variable the share of individuals employed in STEM relative to the overall population. We perform the analysis at the decade level since this measure is taken from population censuses (1900–1930). We observe an increase in the share of workers in

[33] Figure B.7 reports the number of cities included in the sample by state and their location.

STEM occupations in counties more severely hit by the pandemic. A one-standard-deviation increase in excess deaths is associated with a 0.985 standard deviation increase in the share of individuals in scientific occupations. Equivalently, moving from the 25th to the 75th percentile of the excess mortality distribution leads to a 31% increase in the share of individuals in STEM. Column (2) replicates this specification, focusing on the skilled sub-sample of the population, and provides similar results.

Next, we use individual-level data on occupations to better understand what drives the change in occupational shares. Specifically, we test whether individuals who were young at the time of the shock, i.e., between 18 and 25 years old, were more likely to be employed in a STEM occupation in 1930 compared to older cohorts in areas that were comparatively more exposed to the pandemic.[34] We estimate the following linear probability model, where we define the treated individuals as those aged between 18 and 25 years old in 1918:

$$\text{STEM}_i = \alpha_{c(i)} + \alpha_{t(i)} + \boldsymbol{x}_i'\boldsymbol{\beta} + \delta \times \left(\text{Excess Deaths}_{c(i)} \times \text{Young}_i\right) + \varepsilon_i, \qquad (6)$$

where i denotes an individual living in county $c(i)$ and born in year $t(i)$. The terms $\alpha_{c(i)}$ and $\alpha_{t(i)}$ respectively denote county and cohort fixed effects, STEM_i is a dummy variable equal to one if i is employed in a STEM occupation, and zero otherwise; $\boldsymbol{x}_i$ includes urban status and race. The categorical variable Young_i equals one if individual i is between 18 and 25 in 1918, and zero otherwise. Our coefficient of interest is δ, which measures the causal effect of the pandemic on the probability of being employed in a STEM occupation.

Table III reports the results. In counties more exposed to the pandemic, young individuals were significantly more likely to sort into STEM occupations. Why did young cohorts respond disproportionately more to the shock? We have two potential explanations for this finding. The first is mechanical: the pandemic may have affected everyone similarly, but young cohorts were the only ones choosing an occupation. The second one is that the pandemic may have specifically affected the attitudes and preferences of individuals in their *impressionable years* (i.e., the young cohorts). Thus, the differential occupation choice reflects a change in attitudes occurring only for these cohorts.[35]

Then, we use our second proxy for scientific progress and focus on patents. Even if patents may be an imperfect measure for innovation and scientific attitudes (see, e.g., Moser, 2005), the main advantage of the patents data is that they allow us to construct a high-frequency indicator of scientific progress. Column (3) of Table II runs

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[34] To construct the sample, we use the cross-section of all individuals in the 1930 full-count census. We exclude all individuals born after 1900, as they may have been too young to select an occupation, and we restrict the sample to the working population. We drop individuals with no valid occupational response, and we exclude farmers because they display disproportionately high intergenerational occupational persistence (e.g., Long and Ferrie, 2013).

[35] According to the "impressionable years" hypothesis—which represents a long-standing argument in psychology—economic, social, and cultural attitudes and beliefs are formed during early adulthood, approximately between the ages of 18 and 25, and change slowly thereafter (Giuliano and Spilimbergo, 2023). Another explanation could be that there is a higher demand for STEM jobs.

the specification of model (4) and reports the estimated impact of the influenza shock on the total volume of innovation—measured as the log(1 + number of patents) in a given county-year. We find that a one standard deviation increase in excess deaths led to a 0.17 standard deviation increase in the number of patents. Similarly, moving from a county at the 25th percentile to one at the 75th percentile of the excess-deaths distribution leads, on average, to an increase of 7% in the number of patents granted by county year.

Figure IV shows the results in an event-study framework. Each dot in the plot reports the dynamic treatment effect of the pandemic on innovation in the indicated two-year window, as specified in model (5). The coefficients suggest that the number of patents granted after the pandemic increased significantly more in more exposed counties, implying that the pandemic induced a sizable increase in innovative activities that persisted for at least one decade after the shock.

We then investigate the heterogeneous effects of the pandemic across technology classes. Specifically, we ask whether the influenza shock affected not only the volume but also the *direction* of innovation. To do so, we study the effect of the shock on the number of patents in each sector, controlling for the total number of patents. Columns (4)–(8) of Table II show the results of this exercise. For each field, we report the estimated DiD coefficients. We find that the influenza shock has a positive and statistically significant effect only on pharmaceutical patents. Keeping the total number of patents constant, a county at the 75th percentile of the excess-deaths distribution saw an average increase of 6% in pharmaceutical patents, compared to one at the 25th percentile.

We now run a series of robustness checks. First, as in the case of religiosity, one concern is that our results were driven by small counties where scientific progress was comparatively low during the pandemic. In Table B.12, we replicate the specification of Table II weighting regressions by their 1910 population. The results hold.

Next, we focus on the patent data. Table B.13 uses as a dependent variable the total number of patents irrespective of their field (columns 1–4) and the total number of patents in pharmaceuticals (columns 5–9). In columns (2) and (7), we restrict the sample to an unbalanced county-year panel that includes only county-years with at least one filed patent. Columns (3) and (8) report results coding the treatment as a binary variable. Columns (4) and (9) control for WWI deaths interacted with the post-treatment indicator and confirm that WWI-related mortality is not driving our result. Column (6) omits the total number of patents as a control, thus reporting the impact of the pandemic on the volume of pharmaceutical patents. The corresponding coefficient should be interpreted as the impact of the pandemic on the total number of pharmaceutical patents. The estimated DiD coefficients remain positive and statistically significant throughout.

In the baseline specifications, we take the logarithm of the number of patents and add one to avoid dropping zeros. In Tables B.14 and B.15, we use alternative transformations of the dependent variable—e.g., the share of patents in pharmaceuticals—and obtain quantitatively similar findings. Additionally, we estimate the baseline model as a Poisson Quasi-Maximum Likelihood regression. The results hold.

A plausible concern is that our results may be driven by "low-quality" innovation. Newspapers of the day often advocated remedies for influenza that were not science- or evidence-based, some of which may have been granted a patent. To address this concern, we use the text-based measures of "importance" developed by Kelly *et al.* (2021).[36] Table B.16 shows the results. In particular, we assign to every patent a dummy equal to one if the patent's importance is in the top 20% of the distribution and zero otherwise. The number and share of these "breakthrough patents" substantially increase in counties hit harder by the pandemic, both in all sectors (columns 1–2) and in pharmaceuticals (columns 3–4). In addition, in column (5), we show that the number of breakthrough pharmaceutical patents grows more than the average number in other sectors.

Table B.17 deals with the potentially confounding role of immigration. In particular, we exclude from the estimation sample all those who, in the 1930 census, are recorded residing in a state that is different from the one where they were born. The results remain unchanged. Finally, while most innovation activity clusters in urban areas, we perform our baseline analysis at the level of counties. To ensure that the results do not conflate rural-urban disparities, we estimate the effect of the pandemic on innovation at the city level. Table B.18 reports the estimates of model (1) for the panel of cities described in Section A.VIII. The results confirm the county-level evidence: despite the smaller sample size, we estimate the pandemic's positive and statistically significant effect on innovation, especially in pharmaceuticals.

IV.D Joint Dynamics of Religiosity and Innovation

After studying the impact of the pandemic separately on religiosity and scientific progress, we now look at their joint evolution. Specifically, we test whether the *same* counties were affected along both dimensions or whether some counties saw an increase in religiosity while others saw an increase in scientific progress.

We estimate the following model:

$$\begin{aligned} y_{ct} = & \alpha_c + \alpha_{s(c)\times t} + \boldsymbol{x}'_{ct}\boldsymbol{\beta} + \delta_1 \times (\text{Excess Deaths}_c \times \text{Post}_t) + \delta_2 \times \text{Religiosity}_{ct} + \\ & + \delta_3 \times (\text{Religiosity}_{ct} \times \text{Post}_t) + \delta_4 \times (\text{Religiosity}_{ct} \times \text{Excess Deaths}_c) + \\ & + \delta_5 \times (\text{Excess Deaths}_c \times \text{Post}_t \times \text{Religiosity}_{ct}) + \varepsilon_{ct}, \end{aligned} \tag{7}$$

where y_{ct} is the number of patents normalized by county population in 1910, following Bénabou *et al.* (2022), and Religiosity_{ct} is the religiosity measure described in Section III.A. The coefficient δ_1 measures the impact

[36] As discussed by Berkes (2018) and Andrews (2021), citation-based quality measures during this period are noisy and mostly uninformative due to the lack of a mandatory reference section until 1947. The measure built by Kelly *et al.* (2021) identifies important patents based on the textual similarity of a given patent to previous and subsequent work. Important patents are those that are distinct from previous work but are similar to subsequent innovations.

of the pandemic on scientific progress, δ_2 captures the correlation between the outcome and religiosity before the pandemic, and the term δ_5 captures how the correlation between the outcome and religiosity changes after 1918 as a function of exposure to the pandemic. As before, the vector $\boldsymbol{x}_{ct}$ includes an interaction term between the county population in 1910 and a post-treatment indicator.

In Table B.19, we report the estimates of model (7). The results suggest that counties comparatively more affected by the pandemic experienced a joint increase in religiosity and innovation. Columns (1) and (2) report the correlation between innovation and religiosity before and after the pandemic, respectively. Interestingly, this relationship shifts from negative to positive—as shown in Figure B.6. Indeed, in the period before the shock, there was a negative correlation between scientific progress and religiosity at the county level. This pattern aligns with contemporary evidence reported by Bénabou *et al.* (2015). After the pandemic, however, religiosity and science became positively correlated. This pattern is confirmed in column (3), which pools together observations before and after the pandemic. In column (4), we then estimate regression (7) and find that this shift in the correlation between religiosity and innovation co-moved with county-level exposure to the pandemic. In Section V, we use individual-level data to uncover the possible mechanisms underlying these results.

V MECHANISMS: INDIVIDUAL-LEVEL ANALYSIS

Two questions naturally arise after observing a contemporaneous increase in religiosity and scientific progress. Within counties, who turns to religion, and who turns to science? Are these the same or different individuals? In this section, we leverage individual-level data to answer these questions. In particular, we focus on individuals who were heads of household in the 1930 census.[37]

First, we show that the pandemic led to an increase in the religiosity of individuals from initially more religious backgrounds, while individuals from less religious backgrounds were more likely to select STEM occupations. Second, we show that STEM individuals became less religious than the rest of the population. Third, we document that the pandemic led to the polarization of religiosity.

Taken together, these three results suggest that the pandemic shock led to different reactions within society—based, for instance, on individuals' religious background or initial scientific orientation—with people becoming even more distant in terms of their religiosity than they were before the pandemic. This within-county analysis reveals important heterogeneity in how individuals react to a negative shock, and it helps reconcile our aggregate findings with the existing literature on the negative relationship between religion and scientific progress.

[37] The "head of household" variable is provided by the census. During this period, the father and/or husband were usually the head of the household whenever present.

V.A Turning to Religion or Turning to Science

We first study whether preexisting differences in individuals' religious backgrounds could have led to a heterogeneous response to the influenza shock. The full-count census data, in addition to covering the universe of the U.S. population, has the advantage of being deanonymized. These data allow us to construct two measures of religiosity for each individual: one is their revealed religiosity, based on the names individuals gave to their children; the other is their religious background, based on their own name. Specifically, we interpret an individual's own name as conveying information about the religiosity of their parents and, thus, the religious background of that individual.

Combining these measures, we first study how an individual's religious background shaped their response to the pandemic in terms of religiosity. Next, we explore whether, following the pandemic, an individual's religious background may have also shaped their propensity to work in a scientific occupation. To measure this, we use an indicator equal to one if they were employed in a STEM occupation and zero otherwise.[38]

We estimate two triple-difference specifications, one for religiosity and one for the likelihood of selecting a STEM occupation. In the first case, we observe each household multiple times, once per child, and estimate the following regression:

$$\begin{aligned}\text{Religiosity}_{it} = {} & \alpha_{c(i)\times t} + \alpha_{c(i)\times B(i)} + \alpha_{B(i)\times t} + \boldsymbol{x}_i'\boldsymbol{\beta} + \\ & + \delta_1 \times \big(\text{Excess Deaths}_{c(i)} \times \text{Post}_{it} \times \text{High Religious Background}_i\big) + \varepsilon_{it},\end{aligned} \tag{8}$$

where Religiosity_{it} denotes the religiosity score of a child born in year t in household i, living in county $c(i)$. The term $(\text{High Religious Background}_i)$ is an indicator variable returning a value of one if the average religiosity of the adults in the household is in the top 50% of the overall distribution of the religiosity background and zero otherwise. The term Post_t is a categorical variable equal to one for children born after 1918 and zero otherwise. We estimate model (8) on the sample of children born between 1900 and 1929, and each child is weighted by the inverse of the number of children in each household.

To investigate the heterogeneous responses of occupational choice, we estimate the following regression:

$$\begin{aligned}\text{STEM}_i = {} & \alpha_{c(i)\times t} + \alpha_{c(i)\times B(i)} + \alpha_{B(i)\times t(i)} + \boldsymbol{x}_i'\boldsymbol{\beta} + \\ & + \delta_2 \times \big(\text{Excess Deaths}_{c(i)} \times \text{Young}_i \times \text{High Religious Background}_i\big) + \varepsilon_i,\end{aligned} \tag{9}$$

where i denotes an adult individual born in year $t(i)$. In this case, each adult is observed once, and the term Young_i is an indicator equal to one for individuals between 18 and 25 when the pandemic hit. The background

[38] A natural way to construct a measure of scientific background, symmetric to the religiosity one, would be to look at whether individuals had a parent working in a scientific occupation. Unfortunately, this is not possible due to data limitations, as this exercise would require tracking individuals across several census waves, thus greatly reducing our sample size. The advantage of our measure of religious background is that it can be constructed for every individual without requiring direct information on or linking to their parents.

religiosity term (High Religious Background$_i$) denotes an indicator returning a value of one if the religiosity score of the name of individual i is in the top 50% of the overall distribution and zero otherwise. The dependent variable is an indicator variable returning the value one if the head of household i is employed in a STEM occupation in 1930 and zero otherwise.

In both equations, the terms $\alpha_{c\times t}$, $\alpha_{c\times B}$, and $\alpha_{B\times t}$ denote, respectively, county-by-year, religious-background-by-county, and religious-background-by-year fixed effects, and $\boldsymbol{x}_i$ includes urban status and race of the household head. The coefficients δ_1 and δ_2 quantify the effect of county-level exposure to the pandemic, comparing individuals in the top quintile of the background religiosity distribution with the rest of the population on, respectively, religiosity and STEM employment. Table IV presents the results of the analysis. In columns (1)–(3), the dependent variable is revealed religiosity. Our variable of interest is the interaction between the excess-deaths measure, the "Post" dummy, and the religious background of the household head. In columns (4)–(6), the outcome variable is a dummy variable for STEM occupations, and the main variable of interest is the interaction between the excess-deaths measure, a "Young" dummy, and their religious background.

We find that individuals from more religious backgrounds, who were already more religious before the influenza shock, became even more religious afterward in more exposed counties (columns 1–3).[39] By contrast, individuals who were young during the pandemic and came from less religious backgrounds were more likely to choose a scientific occupation (columns 4–6). These findings suggest that an individual's religious background affects their way of dealing with negative shocks. In particular, those who were raised by religious parents were more likely to resort to religion to deal with adversity. On the other hand, growing up in a less religious household made individuals more likely to approach science, possibly as a coping device in the face of the negative shock.

V.B Science-Oriented Individuals Became Less Religious

In this part of the analysis, we focus on science-oriented individuals and study whether their religiosity changed after the pandemic compared to the rest of the population.

We estimate the following triple-differences model:

$$\begin{aligned}\text{Religiosity}_{it} = \alpha_{c(i)\times \text{STEM}_i} + \alpha_{t\times \text{STEM}_i} + \alpha_{c(i)\times t} + \boldsymbol{x}_i'\boldsymbol{\beta} + \\ + \delta \times \left(\text{Excess Deaths}_{c(i)} \times \text{STEM}_i \times \text{Post}_t\right) + \varepsilon_{it},\end{aligned} \tag{10}$$

where the dependent variable is the religiosity of a child born in household i living in county $c(i)$ in year t. The term Post$_t$ is a dummy variable taking the value one if the child is born after 1918, and zero otherwise; STEM$_i$

[39] The correlation between revealed religiosity and background religiosity is equal to 0.13 and highly statistically significant (p< .001), in line with a large literature on cultural transmission (Bisin and Verdier, 2001).

is an indicator variable that takes the value one if at least one member of the household is employed in a STEM occupation; and $\boldsymbol{x}_i$ includes urban status and race of the household head. The coefficient δ compares STEM and non-STEM households, before and after the pandemic, by county-level exposure to the pandemic. The sample is composed of all children born between 1900 and 1929. Children are weighted by the inverse of the number of children in each household. Table V shows the results. In columns (1)–(3), the comparison group is the entire population, while in columns (4)–(6), we focus on high-skilled workers. We find that, for both comparison groups, individuals in STEM occupations became less religious than non-STEM ones in counties more exposed to the influenza shock (columns 1 and 4). This pattern is stronger for Protestants (columns 3 and 6) than Catholics.

These findings further show that different groups within society reacted differently to an adverse shock. In particular, STEM individuals appeared to turn further away from religion than their non-STEM counterparts.

V.C Polarization of Religious Beliefs

We conclude the individual-level analysis by studying the impact of the influenza pandemic on the distribution of religiosity within counties. In particular, we estimate the heterogeneous treatment responses to the pandemic across the initial distribution of background religiosity.

To study this question, we first discretize the distribution of background religiosity into quintiles, which we label Q^{BR}. Then, we estimate the following model:

$$\begin{aligned}\text{Religiosity}_{it} = {} & \alpha_{c(j)\times t} + \alpha_{c(j)\times Q(i)} + \alpha_{Q(i)\times t} + \\ & + \sum_{\substack{k=1 \\ k\neq 3}}^{5} \delta^k \times \left[\text{Excess Deaths}_{c(i)} \times \text{Post}_{it} \times I\left(Q_i^{\text{BR}} = k\right)\right] + \boldsymbol{x}_i'\boldsymbol{\beta} + \varepsilon_{it},\end{aligned} \tag{11}$$

where the outcome variable measures the religiosity score associated with the name of a child born in household i living in county $c(i)$ in year t. As in the previous analyses, the term Post_{it} is an indicator variable equal to one for children born after the pandemic and zero otherwise. Equation (11) includes county-by-time, county-by-background, and background-by-time fixed effects, and the term $\boldsymbol{x}_i$ includes urban status and race of the household head. The term $I\left(Q_i^{\text{BR}} = k\right)$ is a dummy variable that takes the value one if the household average background religiosity is in the k-th quintile and zero otherwise. If the pandemic caused an increase in polarization of religiosity, the set of coefficients $\{\delta^k\}_{k=1}^5$ in equation (11) would be monotonically increasing in k. On the other hand, a decreasing sequence of coefficients would provide evidence that the pandemic led to a convergence of religiosity. In model (11), the sample comprises all children born between 1900 and 1929. Children are weighted by the inverse of the number of children in each household.

In Figure V, we report the set of $\{\delta^k\}$ coefficients by religious denominations. We normalize the third quintile as the baseline category. The figure provides evidence of an increase in polarization: for individuals with below-median religious backgrounds, the coefficients on exposure to the pandemic are negative, while they are positive for those with above-median religious backgrounds. This pattern suggests that, within the same county, individuals from different religious backgrounds became even more distant in terms of their religiosity, increasing the polarization of religiosity within society.

These three individual-level exercises help us understand the contemporaneous increase in religiosity and science at the county level. They suggest that, within counties, individuals reacted differently to the same shock, based, for instance, on their religious background or their pre-pandemic scientific orientation. Thus, while a county may have become more religious *and* more innovative, individuals seemed to turn either to religion *or* to science, leading to within-county polarization of religiosity.

VI Discussion: Interpretation and Limitations of the Results

Our analysis shows two clear patterns: (i) the 1918–1919 influenza pandemic led to an increase in religiosity and scientific progress across U.S. counties and, as a result of the shock, the same counties became both more religious and more scientific; (ii) *within* counties, there was a heterogeneous response to the same shock, with some individuals turning to religion and others turning to science.

One concern is that other factors related to the pandemic may have affected the evolution of religiosity and science, confounding our results. To address this concern, we proceed in three steps. First, we document that neither initial religiosity nor scientific progress is related to the intensity of the shock. Second, our event-study analysis shows the absence of pretrends, suggesting that religiosity and science were on a similar path in treated and control groups before the shock. Third, we account for other potentially confounding characteristics, such as differential fertility, WWI deaths, and migration patterns. Our results are robust in all these cases. The empirical evidence, supported by historical records, makes it hard to imagine that the pandemic did not trigger an increase in both religiosity and scientific progress.

A second concern regards our main measures of religiosity and scientific progress. First, does our name-based indicator indeed capture religiosity at the local level? We show that our results are robust to alternative ways of constructing our naming measure and using alternative classifications of religious names. In addition, we show that in counties hit harder by influenza, the share of people affiliated with a religious denomination increases, providing further evidence that the pandemic led to an increase in local religiosity. Similarly, we use two measures for scientific progress: the share of individuals in scientific occupations and patents. While each may have some caveats, they provide consistent and robust results.

One puzzle emerging when looking at the aggregate patterns is whether these results are driven by the same individuals becoming more religious and innovative or by different individuals reacting differently to the same shock. Our findings suggest that the second mechanism is at play. Individuals from more religious backgrounds embrace religion, while those from less religious backgrounds are more likely to choose a scientific occupation. This finding suggests that a group of individuals within society used religion as a coping device, while a separate group turned to science. In addition, we show that the shock widened the distance in religiosity between science-oriented individuals and the rest of the population, as people in scientific occupations moved away from religion. Finally, the pandemic increased the polarization of religiosity in society: individuals from more (less) religious backgrounds became even more (less) religious.

One key question regarding our individual-level results is, what explains the increase in religiosity or the choice of a scientific occupation? The findings on religiosity are in line with the religious coping hypothesis, which suggests that religious faith can represent a coping device to deal with personal distress following a negative shock. An alternative explanation for why individuals may turn to religion is for social insurance. While we cannot fully rule this out (and it goes beyond the scope of our paper), we read our evidence as favoring the religious coping hypothesis. First, this is in line with the literature showing that intrinsic religiosity (rather than churchgoing) responds to unexpected events, as noted by Bentzen (2019). Second, as the increase in religiosity persists up to ten years after the shock, it is more likely to be related to a change in beliefs rather than a temporary increase in the need for social insurance.

What motivates people to turn to science is less obvious. Individuals may turn to science to deal with their psychological distress, similarly to religious coping, or in an attempt to actively mitigate the negative (e.g., health-related or economic) effects of the pandemic. Another possibility could be that individuals turn to science because of increased labor demand in STEM occupations, but our results suggest that, beyond market forces, the individual's religious background plays a key role in the decision to turn to science. While our findings cannot directly speak to the individual-level motivations behind these different behaviors, they provide evidence of a heterogeneous response to the same adverse event.

One further limitation of our individual-level analysis is that, while we can construct the religious background for every individual, we cannot directly measure their scientific one. This is due to our measure of scientific orientation based on occupational choice, which—contrary to our measure of religious background—does not allow us to know an individual's occupation and the parents' occupation from the same census.

Taken together, we interpret our results as suggestive evidence that, while individuals from religious backgrounds turned to religion as a coping device in the aftermath of the pandemic, those from a scientific background turned to science.

VII Conclusions

In this paper, we provide new evidence on how societies react to adversities, studying an exemplary historical episode: the Great Influenza Pandemic of 1918–1919.

First, we show that society reacted to the pandemic by becoming both more religious and more scientific. Second, using individual-level data from full-count censuses, we suggest that religiosity and science are substitute ways of reacting to the shock. When facing adversity, individuals from more religious backgrounds turned to religion, while those from less religious backgrounds turned to science. Third, we show that the pandemic shock widened the distance in religiosity between scientific-oriented individuals and the rest of the population and that it increased preexisting differences in religious sentiment. As a consequence, the distribution of religiosity in counties more exposed to the pandemic became more polarized.

Our paper sheds new light on the relationship between religiosity and science. Throughout history, science and religion have often been in conflict, and recent evidence by Bénabou *et al.* (2015, 2022) shows that the two are negatively correlated, both across countries and across U.S. states. We provide novel evidence that—at the individual level—the two are substitute ways of dealing with adversity.

Our analysis also helps shed light on modern events such as the reaction of society to the COVID-19 pandemic. Even though the modern context differs in many ways from the period of influenza pandemic, including the medical advancements of the past century, the reaction of today's society seems in line with what we document for the 1918–1919 pandemic.[40] In particular, our findings can help explain the opposing views that have emerged since the COVID-19 pandemic on science-based responses to the shock, such as the opposing attitudes toward vaccines.

Finally, our results suggest that, in the aftermath of a negative shock, societies may become more polarized in their religiosity. Because religion has become an increasingly polarizing element in the current political debate, facing adversity may strongly affect not only religious polarization but also the polarization of political views and, more broadly, the polarization of society itself.

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[40]One key difference between the two pandemics is that no medical remedy or vaccine became available until many years after the earlier pandemic ended.

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TABLES

TABLE I. THE IMPACT OF THE INFLUENZA ON RELIGIOSITY

	Share of Affiliated			Name-Based Religiosity		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Catholics	Protestants	All	Catholics	Protestants
Post $\times$ Excess Deaths	0.220***	0.078***	0.094***	0.863***	0.269**	0.688***
	(0.025)	(0.013)	(0.017)	(0.199)	(0.111)	(0.171)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Decade FE	Yes	Yes	Yes	–	–	–
State-Year FE	–	–	–	Yes	Yes	Yes
Number of Counties	1275	1275	1275	1274	1274	1274
Observations	3825	3825	3825	38220	38220	38220
R^2	0.851	0.904	0.929	0.648	0.522	0.678
Std. Beta Coef.	0.678	0.327	0.335	0.157	0.112	0.159

Notes: This table displays the impact of exposure to the Great Influenza Pandemic on religiosity. The unit of observation is a county observed at a decade frequency between 1906 and 1926 (in columns 1–3) and yearly frequency between 1910 and 1929 (in columns 4–6). "Post" is a categorical variable equal to one during and after the pandemic—i.e., over 1918 to 1929—or zero otherwise. The baseline treatment "Excess Deaths" is defined in Equation (3). In columns (1–3), the dependent variable is the number of individuals affiliated with religious denominations enumerated in the Census of Religious Bodies, normalized by county population in 1900; in columns (4–6), the dependent variable is the name-based religiosity measure described in the main text. Columns (1) and (4) report the effect of the influenza on overall religiosity, whereas columns (2) and (5)—resp. (3) and (6)—display it on the intensity of Catholicism—resp. Protestantism. Regressions include county and state-by-time (decades in columns 1–3 and years in columns 4–6) fixed effects and control for an interaction term between population in 1910 and a post-treatment indicator. Standard errors, clustered at the county level, are reported in parentheses. Referenced on page(s) 13, 13.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE II. THE IMPACT OF THE INFLUENZA ON INNOVATION

	STEM Employment Share		log(1 + Number of Patents)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Whole Population	Skilled Population	All Patents	Pharmaceuticals	Communication	Electrical	Mechanical	Other
Post × Excess Deaths	0.008***	0.103***	0.370***	0.115***	0.015	0.037	0.026	-0.002
	(0.001)	(0.017)	(0.056)	(0.030)	(0.019)	(0.025)	(0.019)	(0.020)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Decade FE	Yes	Yes	–	–	–	–	–	–
State-Year FE	–	–	Yes	Yes	Yes	Yes	Yes	Yes
All Patents	–	–	No	Yes	Yes	Yes	Yes	Yes
Number of Counties	1274	1274	1275	1275	1275	1275	1275	1275
Observations	3822	3822	38250	38250	38250	38250	38250	38250
R^2	0.765	0.625	0.858	0.830	0.715	0.815	0.923	0.934
Std. Beta Coef.	0.985	1.105	0.171	0.085	0.020	0.030	0.014	-0.001

Notes: This table displays the impact of exposure to the Great Influenza Pandemic on innovation. The unit of observation is a county, observed at a decade frequency between 1900 and 1930 in columns (1–2) and at a yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic—i.e., over 1918 to 1929—or zero otherwise. The baseline treatment "Excess Deaths" is defined in Equation (3). In column (1), the dependent variable is the share of people employed in STEM occupations within the population; in column (2), we restrict the denominator to include only those employed in skilled occupations. The dependent variable in columns (3–8) is the (log 1+) number of patent grants. We use this specification of the dependent variable to ensure that we do not drop counties without patents. In columns (4–8), we also control for the overall (log 1+) number of granted patents. Column (3) estimates the impact of the pandemic on the level of innovation, while columns (4)–(8) display this on the direction of innovation. All regressions include county-fixed effects and control for an interaction term between the population in 1910 and a post-treatment indicator. Regressions (1–2) include state-by-decade-fixed effects, while regressions (3–8) include state-by-year-fixed effects. Standard errors, clustered at the county level, are reported in parentheses. Referenced on page(s) 15, 16, 17, 17.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE III. IMPACT OF THE INFLUENZA ON OCCUPATIONAL CHOICE

	Entire Population		Skilled Population	
	(1)	(2)	(3)	(4)
	No Controls	Controls	No Controls	Controls
Young × Excess Deaths	0.005**	0.005**	0.013*	0.012*
	(0.003)	(0.003)	(0.007)	(0.007)
County FE	Yes	Yes	Yes	Yes
State-Cohort FE	Yes	Yes	Yes	Yes
Household Controls	No	Yes	No	Yes
Number of Counties	1275	1275	1275	1275
Observations	13983936	13983936	5676407	5676407
R^2	0.003	0.004	0.006	0.007
Std. Beta Coef.	0.017	0.016	0.025	0.025

Notes: This table displays the effect of the pandemic on occupational choice. The unit of observation is an individual, observed once in the 1930 population census. Young is a dummy equal to one for all individuals aged between 18 and 25 at the time of the inception of the pandemic. The baseline treatment "Excess Deaths" is defined in Equation (3). The dependent variable is a dummy variable equal to one if the person is employed in a STEM occupation and zero otherwise. The sample includes the entire population in columns (1–2) and only individuals employed in skilled occupations in columns (3–4). In columns (2) and (4), we control for race and urban status of the head of household. Regressions include county and state-by-cohort fixed effects. Standard errors are clustered at the county level and are reported in parentheses. Referenced on page(s) 16.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE IV. RELIGIOUS BACKGROUND, RELIGIOSITY, AND STEM OCCUPATIONS

	Religiosity			STEM Occupation		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Catholics	Protestants	All	Catholics	Protestants
Excess Deaths × Post × High Religious Background	0.048***	0.033***	0.033***			
	(0.010)	(0.006)	(0.008)			
Excess Deaths × Young × High Religious Background				-0.709**	-0.311	-0.551*
				(0.336)	(0.321)	(0.333)
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County-Background FE	Yes	Yes	Yes	Yes	Yes	Yes
Background-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of Counties	1275	1275	1275	1275	1275	1275
Observations	5857923	5857923	5857923	5347751	5347751	5347751
R^2	0.035	0.044	0.028	0.014	0.014	0.014
Std. Beta Coef.	0.047	0.053	0.040	-0.016	-0.007	-0.012

Notes: This table displays the impact of exposure to the pandemic on religiosity—columns (1)–(3)—and occupational choice—columns (4)–(6)—by individual-level background religiosity. The unit of observation in columns (1)–(3) is a head of household, who is observed once for each child born between 1900 and 1930 in the household. In columns (4)–(6), the unit of observation is an adult. Religiosity is defined as the religiosity score associated with the child's name. "Post" is a categorical variable equal to zero for children born during and after the pandemic—i.e., over 1918–1929—or zero for those born before the pandemic—i.e., before 1918. The baseline treatment "Excess Deaths" is defined in Equation (3). "STEM" is an indicator variable returning value one if an individual is employed in a STEM occupation—as defined in Table B.2—or zero otherwise. "Young" is an indicator variable equal to one if an individual is aged between 18 and 25 in 1918 or zero otherwise. "High Background Religiosity" is an indicator variable returning the value one if the religiosity score of the name of the head of the household is in the top 50% of the overall distribution, or zero otherwise. The table displays the coefficient of the interaction between these terms. Each regression includes county-by-cohort, county-by-background, and background-by-cohort fixed effects. We include race and urban status as further household-level controls in each regression. Standard errors are clustered at the county level and are reported in parentheses. Referenced on page(s) 21.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE V. EFFECT OF THE INFLUENZA ON INDIVIDUAL RELIGIOSITY: STEM AND NON-STEM

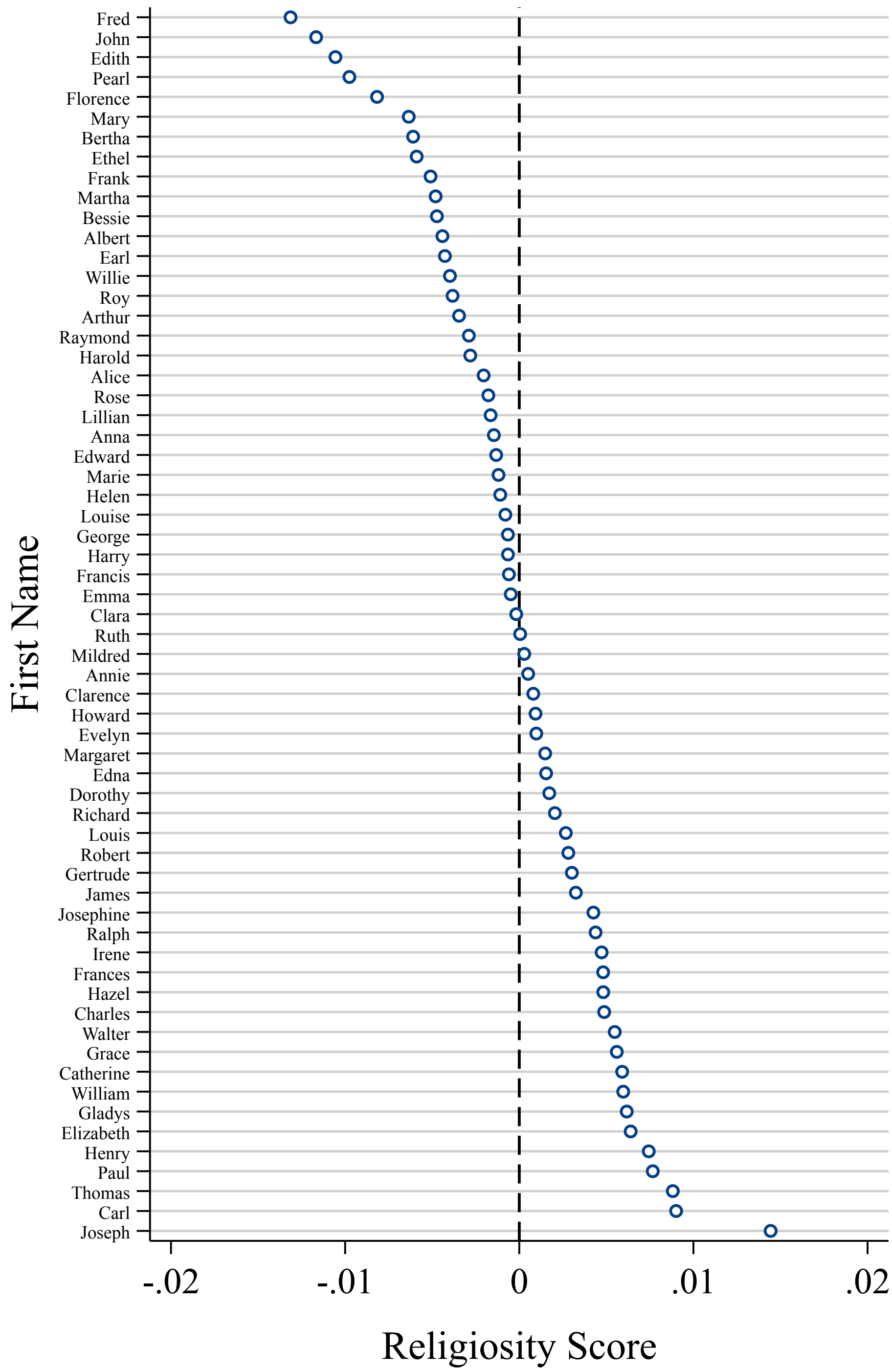
	Entire Population			Skilled Population		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Catholics	Protestants	All	Catholics	Protestants
Excess Deaths × Post × STEM	-0.061***	0.009	-0.047**	-0.059***	0.011	-0.046**
	(0.021)	(0.013)	(0.019)	(0.022)	(0.014)	(0.018)
STEM-County FE	Yes	Yes	Yes	Yes	Yes	Yes
STEM-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
County-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Household Controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of Counties	1275	1275	1275	1274	1274	1274
Observations	10616377	10616377	10616377	3859347	3859347	3859347
R^2	0.006	0.014	0.007	0.012	0.018	0.013
Std. Beta Coef.	-0.061	0.009	-0.047	-0.059	0.011	-0.046

Notes: This table displays the impact of exposure to the pandemic on STEM and non-STEM individuals' religiosity. The unit of observation is a child born between 1900 and 1930. Religiosity is defined as the religiosity score associated with the child's name. "Post" is a categorical variable equal to zero for children born before the pandemic—i.e., before 1918—or one for those born after the pandemic—i.e., after 1918. The baseline treatment "Excess Deaths" is defined in Equation (3). "STEM" is an indicator variable returning a value of one if one parent of the child is employed in a STEM profession or zero otherwise. The table displays the coefficient of the interaction between these terms. This estimates the causal effect of the influenza shock on the religiosity of heads of households employed in STEM occupations *vis-à-vis* non-STEM occupations, leveraging variation in county-level exposure to the influenza. All models include STEM-by-county, STEM-by-cohort, and county-by-cohort fixed effects. The sample includes the entire population in columns (1–3) and only individuals employed in skilled occupations in columns (4–6). Standard errors are clustered at the county level and are reported in parentheses. Referenced on page(s) 21.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

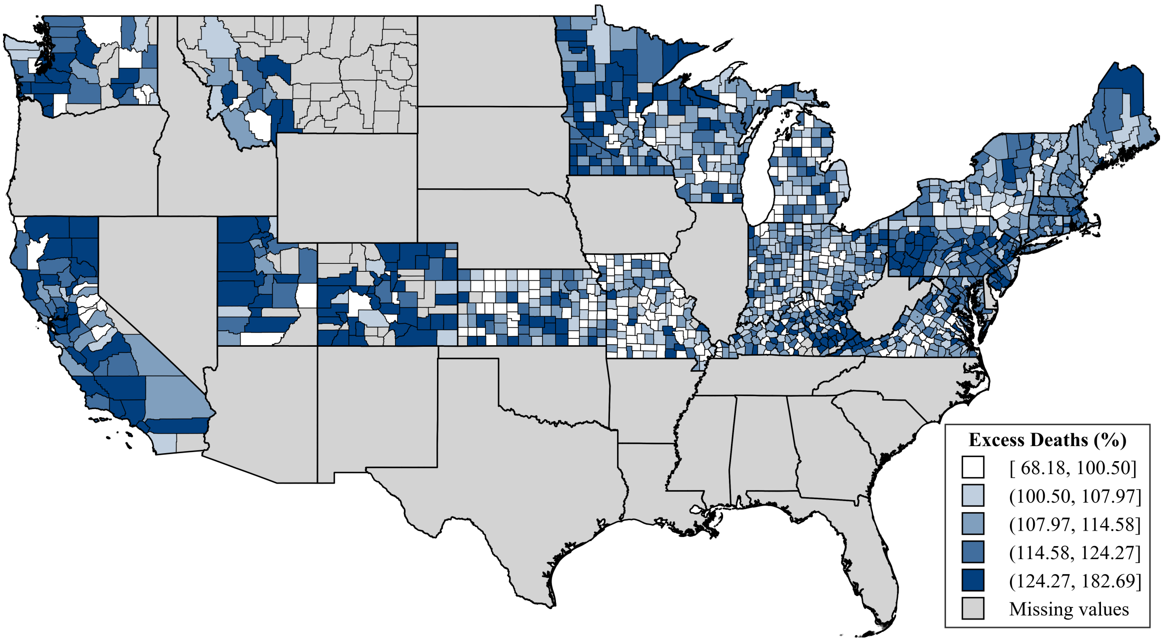
FIGURES

FIGURE I. ESTIMATED NAMES RELIGIOSITY SCORES



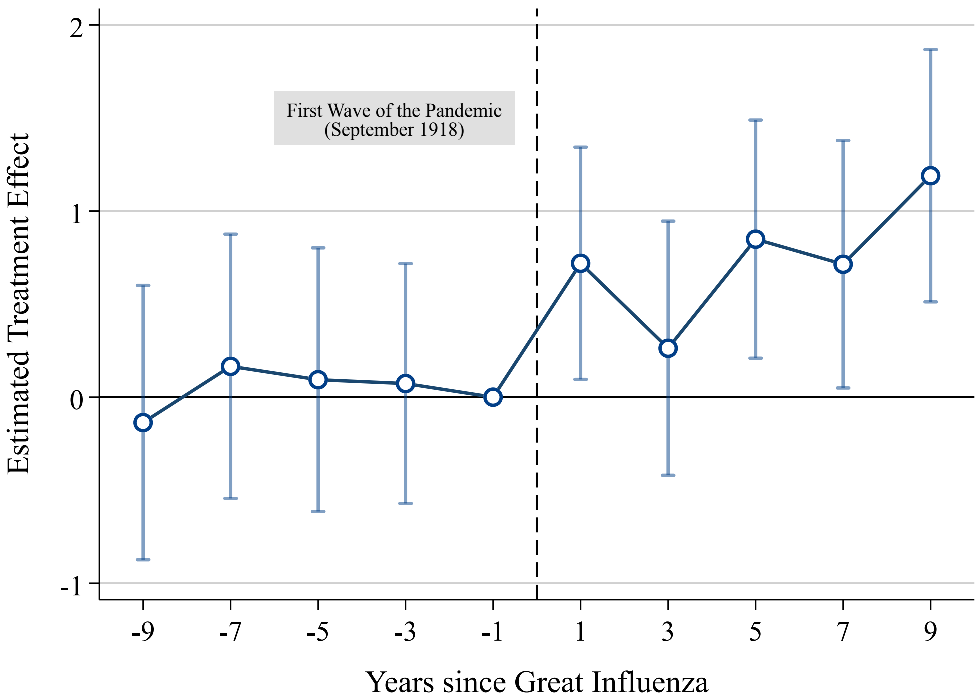
Notes: This figure displays the religiosity scores estimated from model (1). Regressions are based on data from the 1906–1916 Censuses of Religious Bodies; they include individuals born between 1896 and 1916. We estimate religiosity scores for names appearing in at least 0.3% of the overall sample. Coefficients are reported in increasing order. Referenced on page(s) 9.

FIGURE II. SPATIAL DISTRIBUTION OF EXCESS MORTALITY DURING THE GREAT INFLUENZA PANDEMIC



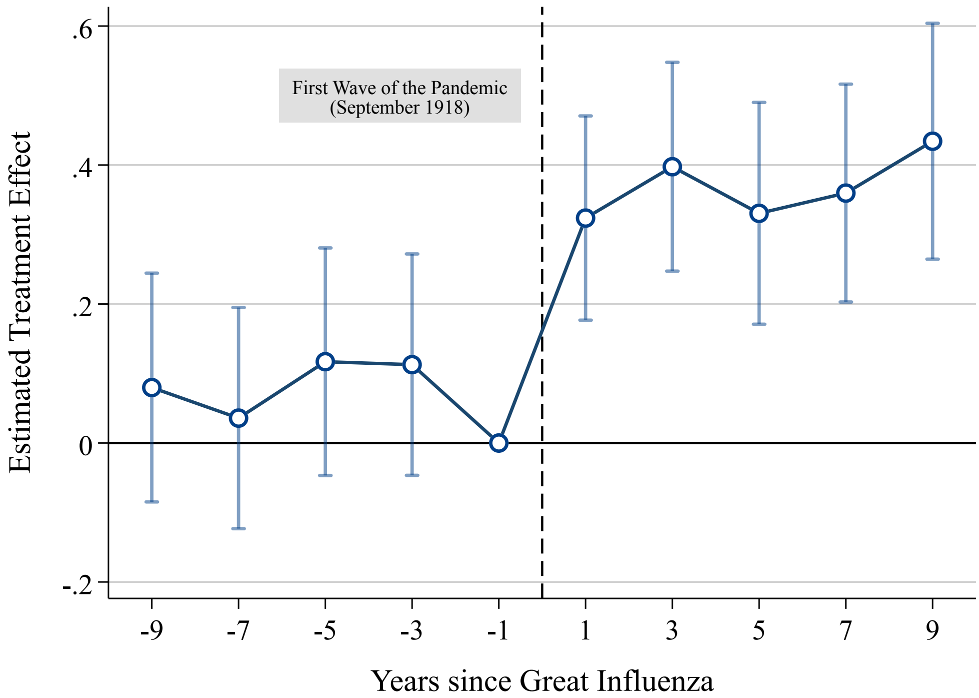
Notes: This figure displays geographic variation in excess deaths, defined in Equation (3). Excess mortality is the ratio between the average number of deaths during the pandemic years (1918–1919) and the average number of deaths in the three years before the pandemic (1915–1917). Mortality statistics before 1915 are not available. Excess mortality is displayed in percentage terms. Lighter to darker blue indicates increasing excess deaths. Counties are displayed at their 1920 borders. Morality data are not available for states displayed in gray. Counties displayed in gray are excluded from the analysis sample. Referenced on page(s) 11.

FIGURE III. IMPACT OF THE INFLUENZA ON RELIGIOSITY



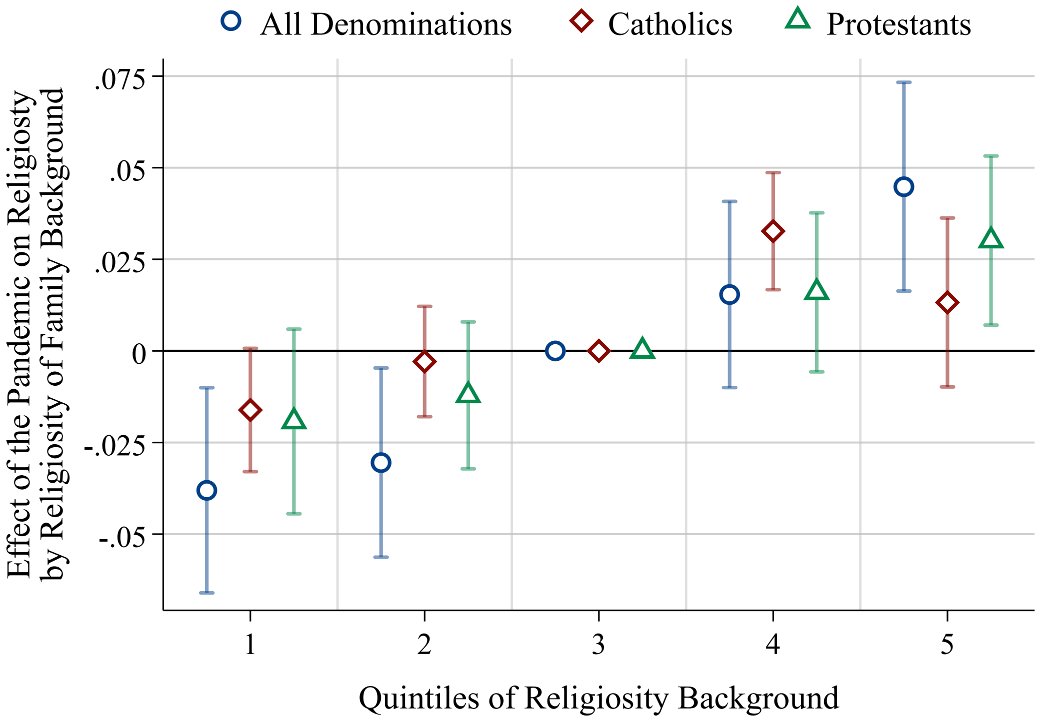
Notes: This figure displays the dynamic treatment effects of the pandemic on overall religiosity. The unit of observation is a county observed at a biennial frequency. Each dot reports the coefficient of an interaction between the baseline measure of excess deaths, defined in Equation (3), and a biennial time dummy. The coefficient for the biennial 1917–1918 serves as the baseline category. The model includes county and state-by-year-fixed effects and further controls for an interaction term between the population in 1910 and a post-treatment indicator. Bands report 95%confidence intervals. Standard errors are clustered at the county level. The dashed vertical line indicates the timing of the first wave of the pandemic (October 1918). Referenced on page(s) 13.

FIGURE IV. IMPACT OF THE INFLUENZA ON INNOVATION



Notes. The Figure reports the dynamic treatment effects of the pandemic on innovation. The dependent variable is the (log 1+) total number of patents filed in a given year. The unit of observation is a county observed at a biennial frequency. Each dot reports the coefficient of an interaction between the baseline measure of excess deaths and a biennial time dummy. The coefficient for the biennial 1917–1918 serves as the baseline. The model includes county and state-by-year-fixed effects and controls for an interaction term between the population in 1910 and a post-treatment indicator. Bands report95% confidence intervals. Standard errors are clustered at the county level. The dashed vertical line indicates the timing of the first wave of the pandemic (October 1918). Referenced on page(s) 17.

FIGURE V. IMPACT OF THE INFLUENZA ON THE POLARIZATION OF RELIGIOUS BELIEFS



Notes: This figure reports the estimated impact of the pandemic on the polarization of religious beliefs by religious denominations. Each dot reports the coefficient of an interaction between the baseline measure of excess deaths, a post-treatment indicator, and an indicator for the quintile of background religiosity. The unit of observation is a child born between 1900 and 1930. Treated children are those born after the influenza, i.e., after 1918. The dependent variable is the religiosity score associated with the child's name. Background religiosity is measured as the religiosity score of the child's head of household. Results are reported by confession, and the third quintile serves as the baseline. Regression models include fixed effects for county by cohort, county by quintile of religious background, and cohort by quintile of religious background. Standard errors are clustered at the county level, and the bands report the 95% confidence interval for each coefficient. Referenced on page(s) 22.

Online Appendix

Dealing with Adversity: Religiosity or Science?

Evidence from the Great Influenza Pandemic

Enrico BERKES, Davide M. COLUCCIA, Gaia DOSSI, and Mara P. SQUICCIARINI

July, 2024

A Data Appendix

This section lists the data sources and describes how we construct the variables used in the analysis.

A.I Names

Data on names are from the individual full-count US 1930 population census (Ruggles *et al.*, 2021). Since first name records in the census conflate first and middle names, we consider only the first word that appears in each string. The baseline analysis includes all names that appear in at least 0.3% of the population of American-born children between 1900 and 1930. We perform a robustness exercise and show that the baseline results are not sensible to alternative frequency thresholds. We exclude first-generation immigrants because their names would not reflect exposure to the pandemic as they were not born in the United States. Moreover, the frequency threshold we apply implies that only English names are included in the sample.

A.II Religious Affiliations

Data on religious affiliations are supplied by NHGIS and are originally from the Census of Religious Bodies, which took place at decade frequency between 1906 and 1936. We discard the 1936 census because previous research shows that the uptake was low and unequal across counties (Stark, 1992). Census enumerators asked churches, congregations, and other local organizations to report the number of affiliates. The data was then aggregated at the county level. In our analysis, "Total Religiosity" is computed as the simple sum of religious members across all possible denominations; "Catholics" are enumerated as such. We collectively refer to "Protestants" as a set of denominations that we manually mapped to some branch of Protestantism (including, e.g., the Methodist, Evangelical, and–various–Baptist churches).

We use data on the names of saints and biblical characters from Abramitzky *et al.* (2016) to develop an additional indicator of county-level religiosity. To construct this additional measure, we employ the baseline equation (2) and substitute the estimated $\hat{\gamma}^k$ coefficients with three indicator variables equal to one if the given name is that of a saint, a biblical character, or both.

A.III Patents

Patent Data Patent data are from Berkes (2018), who performed optical character recognition (OCR) on original patent documents issued by the United States Patents and Trademark Office between 1836 and 2010. Information includes the filing and issue year, author name, latitude and longitude of the inventor(s), and inferred USPC technology class. The data contain a set of additional variables, including the complete text of the patent document and the issue year of the patent, not used in our analysis. We geo-code each patent to its

1920 county using boundary shapefiles supplied by NHGIS. When we collapse by county year, we weigh each patent by the inverse of the number of technology classes and by the inverse of the number of authors. Hence, a patent with two authors and two technological classes appears four times in the original patent-level dataset, and each instance is assigned a .25 weight when aggregating at the county level. We code USPC classes to the NBER classification (Hall *et al.*, 2001). We modify the canonical NBER classification and conflate the "Chemical" and "Drugs" categories into a single "Pharmaceuticals" class. Since multiple USPC codes are typically assigned to a single patent, most patents that fall under "Drugs" would also appear as "Chemical." To avoid this, we recast them into one category. It is worth noting that all the results we present regarding pharmaceutical patents also hold if we keep the "Chemical" and "Drugs" classes separate.

Importance of Patents We measure patent 'importance' using the measure developed by Kelly *et al.* (2021). From their data, we derive two metrics. One is the number of "Breakthrough" innovations, which are defined as any patent whose importance is in the top 20% of the overall quality distribution. The second variable is the share of breakthrough patents relative to the overall number of issued patents. Both measures are net of grant-year fixed effects. We take forward and backward similarity within a 5-year window around the issue year of the patent.

A.IV Occupational Structure

Individual-level data on occupations is extracted from the 1930 individual-level population census. More precisely, we use the 1950 harmonized occupation classification. We then manually map occupational codes to STEM occupations as described in Table B.2.

A.V Controls & Mortality Statistics

We extract individual-level information on race and urban-rural status from the IPUMS full-count data.

County-level covariates are from NHGIS. This aggregates individual-level data from population censuses and reports data from manufacturing and agricultural censuses. All data come at historical county borders.

Mortality statistics are likewise provided by NHGIS. For the period we are interested in, 1915-1919, they were collected for about 1,200 counties, covering approximately 60% of the US population. We measure Influenza-related mortality as the ratio between deaths during the pandemic and deaths in the three years that preceded the Influenza.[1]

[1] The original documents report, for major cities, deaths broken down by (alleged) cause. We do not use this data for two main reasons. First, they are incomplete and are only available for cities. Second, Beach *et al.* (2020) criticizes the methodology adopted to impute the cause of deaths.

A.VI Other Data

In several robustness regressions, we control for WW1 mortality. The underlying data were kindly shared by Ferrara and Fishback (2020).

A.VII Boundary Harmonization

County-level data from NHGIS and other sources are typically provided at historical borders. To ensure comparability and consistency, we adopt the method developed by Eckert *et al.* (2018) to compute geographical crosswalks between US counties over time. In a nutshell, their methodology is as follows. Suppose we know the distribution of a given variable y across counties at decade frequency between 1900 and 1930. To harmonize borders to one year, Eckert *et al.* (2018) overlay the shapefile of counties in a given year, say, 1900, to that in the reference year, say, 1920. They then compute the percentage of land that a given county shares with itself between the two years and that assigned to other counties. To construct the harmonized variable, one multiplies these overlapping area weights by the variable recorded in 1900 and aggregates up by 1920 counties. The underlying assumption is that y is evenly distributed over the county territory. While this may seem untenable in most cases, departures from this assumption are plausibly innocuous in our setting. County borders had undergone major consolidations before 1900 and remained stable thereafter. Moreover, mortality data are mostly available for the Northwest and Midwest areas. Boundary changes in these regions were rare and minor after the 1890s. In our application, we map all county-level variables to 1920-borders.

A.VIII Details on Sample Construction

In this paragraph, we provide additional technical details on how we construct the estimation samples. The main sample restriction that we impose descends from the fact that we observe mortality for 1,302 out of 3,100 counties. We then discard 27 counties with values of excess mortality below or above, respectively, the bottom 1% (85% of the pre-pandemic mortality) and top 99% (180% of the pre-pandemic level) of the excess-mortality distribution. Because such figures are due to scarcely-inhabited areas, these 27 counties account for less than 0.1% of the population in the 1302 counties sample. We are left with a set of 1275 counties. In the rest of the paragraph, we explain why we may not always be able to leverage all 1275 for the estimation.

County-Level Religiosity The county-level religiosity estimation sample is a balanced panel dataset where each county is observed at a yearly frequency between 1900 and 1929. This implies that the number of counties in this balanced panel may not be 1275 as long as at least one county is not observed at least once between 1900 and 1929. This happens because, especially in scarcely-inhabited areas, our name-frequency threshold may imply that we cannot match any newborn in a given cohort. If that is the case, the county's religiosity will not be observed every year of the sample, and the county will subsequently be dropped from the estimation sample. This is the case for one county, so the estimation sample, in this case, consists of 1274 counties accounting for 99.5% of the population in the 1302-counties sample.

In one robustness check shown in column (7) of Table B.5, counties are observed at decade frequency instead. In this case, the sample is constructed from adults observed once per census decade between 1900 and 1930, and the post-treatment indicator returns a value of one for decades 1920 and 1930 and zero otherwise.

County-Level Innovation The county-level innovation sample is a balanced panel dataset where each county is observed at a yearly frequency between 1900 and 1929. Thus, an observation in the dataset can either be a number above zero (if one or more patents are observed in that county year) or zero (if no patents are observed). The estimation sample in this case thus encompasses all 1220 counties for which we observe mortality. In columns (2) and (7) of Table B.13 we don't include county-year when no patents are observed. This results in an unbalanced panel dataset where a county may not be observed yearly over the estimation period.

Other County-Level Samples In Table B.10, we use various measures of name concentration as dependent variables. These are the Hirschman-Herfindahl index, the Comprehensive Concentration index, the Rosenbluth index, and concentration ratios equal to the share of children born with the most common k names, for various levels of the threshold k.

Individual-Level We construct two individual-level datasets. The first sample ("adult sample") comprises all those born before 1900. Each individual is observed once in the 1930 census. Because the adult sample is used to study the evolution of the occupational structure, we discard (i) individuals with no valid occupational response, (ii) farmers, who have been shown to display disproportionate occupational persistence (Long and Ferrie, 2013), and (iii) women, since the female labor force participation was extremely low. The second sample ("children sample") comprises all those born between 1900 and 1930. Each child is considered a realization of their household. Each household is thus observed once per child. In the children sample, we exclude households when (i) we do not observe a valid occupational response, and (ii) there is no male head, since here, too, the labor force participation of women was very low. We identify a household as "STEM" if at least one of its members is employed in a STEM occupation. Consistently with the county-level analysis, we do not assign a religiosity score to first-generation immigrants.

City-Level To build the city-level sample, we construct the baseline excess deaths treatment variable from data by Clay *et al.* (2019). The dataset contains mortality information on 976 cities. For 444, however, we observe the number of deaths in one year only, and we do not observe 48 other cities continuously between 1915 and 1919. Moreover, we do not observe population data in 1900 for 41 additional cities. The final sample consists of 443 cities. In Figure B.7 we report the location of each city and the number of cities included in the sample, by state. We geo-code patents to historical city borders and construct the name-based religiosity measure from the individuals recorded living in each city in the 1930 census. The city-level sample is used in the regressions displayed in Tables B.7 and B.18.

B ADDITIONAL TABLES AND FIGURES

B.I TABLES

TABLE B.1. SUMMARY STATISTICS

	(1) Mean	(2) Std. Dev	(3) Min	(4) Max	(5) Counties
Panel A. Mortality					
Flu Excess Deaths (%)	1.127	0.163	0.682	1.827	1275
WW1 Deaths	39.834	167.083	0.000	4828.000	1275
Panel B. Religion					
All Denominations - Census Based	40.525	14.099	0.000	94.264	1275
Catholics - Census Based	11.024	12.007	0.000	65.853	1275
Protestants - Census Based	27.448	13.807	0.000	94.264	1275
All Denominations - Name Based	0.016	0.021	-0.039	0.103	1274
Catholics - Name Based	0.001	0.008	-0.030	0.045	1274
Protestants - Name Based	0.014	0.017	-0.027	0.084	1274
Panel C. Patents					
Total	278.976	1095.265	0.000	15000.000	1275
Pharmaceutical	49.756	200.266	0.000	2708.315	1275
Communication	11.166	54.019	0.000	1110.340	1275
Electrical	40.552	196.034	0.000	4005.376	1275
Mechanical	136.779	533.283	0.000	7834.571	1275
Other	149.124	568.327	0.000	7290.446	1275
Share of STEM	0.480	0.313	0.054	2.334	1275
Panel D. Income and Demographics					
Population	130.432	329.177	1.061	4819.392	1275
Area	216.975	280.512	0.277	5205.795	1275
Occupational Score per Capita	835.998	74.425	311.939	2302.648	1275
Share of Men	52.305	4.856	19.177	131.582	1275
Share of Illiterates	72.620	9.326	24.633	196.336	1275
Share of Young	33.241	5.981	12.927	86.595	1275
Share of Whites	94.634	11.952	26.163	100.000	1275
Share of African Americans	5.366	11.952	0.000	73.837	1275
Share of Foreign Born	10.680	9.928	0.008	44.052	1275

Notes: This table displays the main variables' mean, standard deviation, minimum, maximum, and total number of counties. Data are measured at the county level. Influenza mortality in Panel A is constructed from the mortality statistics; WW1 deaths are from Ferrara and Fishback (2020). Data in Panel B are from either the Census of Religious Bodies at the decade level or are constructed from name frequencies at the year level. Data in Panel C are from Berkes (2018) and are aggregated at the decade level. The share of STEM is computed from the census and is in 1,000 units. Panel D reports data from the 1910 census. County demographics are measured through the IPUMS full-count census (Ruggles *et al.*, 2021). Panel B data and Panel D population are expressed in thousand units. All variables are crosswalked to 1920 borders. Referenced on page(s) 7.

TABLE B.2. STEM PROFESSIONS

Code	Occupation Label	Code	Label
(1)	(2)	(3)	(4)
Panel A. STEM Occupations			
7	Chemists	58	Nurses, Professional
12	Agricultural Sciences	59	Nurses, Student Professional
13	Biological Sciences	61	Agricultural Scientists
14	Chemistry	62	Biological Scientists
16	Engineering	63	Geologists and Geophysicists
17	Geology and Geophysics	67	Mathematicians
18	Mathematics	68	Physicists
19	Medical Sciences	69	Miscellaneous Natural Scientists
23	Physics	70	Optometrists
25	Statistics	71	Osteopaths
26	Natural Sciences (n.e.c.)	73	Pharmacists
32	Dentists	75	Physicians and Surgeons
34	Dietitians and Nutritionists	83	Statisticians and Actuaries
41	Engineering, Aeronautical	92	Surveyors
42	Engineering, Chemical	98	Veterinarians
43	Engineering, Civil	240	Officers, Pilots, Purses, and Engineers, Ships
44	Engineering, Electrical	541	Locomotive Engineers
45	Engineering, Industrial	563	Opticians and lens grinders and polishers
46	Engineering, Mechanical	583	Stationery Engineers
47	Engineering, Metallurgical, Metallurgists	772	Midwives
48	Engineering, Mining	781	Practical Nurses
49	Engineering (n.e.c.)		
Panel B. Skilled Occupations (Includes STEM)			
$1 \leq \cdot \leq 99$	Liberal Professions	$200 \leq \cdot \leq 299$	Managers
$500 \leq \cdot \leq 595$	Craftsmen		

Notes: Panel A displays the occupations that we classify as Science, Technology, Engineering, and Mathematics (STEM). Panel B displays the occupations that we classify as skilled: these include all STEM occupations and those listed. Occupation codes and labels are from the IPUMS harmonized 1950 occupation taxonomy (variable "OCC1950"). Referenced on page(s) 10, 34, 34, A2, B22, B22.

Table B.3. Balance Checks Regressions

	(1) Coefficient	(2) Standard Error	(3) 95% C. I.
Panel A. Religion			
All Denominations (Name Based)	0.211	(0.224)	$[-0.229, 0.651]$
Catholics (Name Based)	-0.079	(0.196)	$[-0.464, 0.307]$
Protestants (Name Based)	0.350^{*}	(0.191)	$[-0.025, 0.725]$
All Denominations (Census Based)	0.057	(0.176)	$[-0.288, 0.403]$
Catholics (Census Based)	0.168	(0.147)	$[-0.121, 0.457]$
Protestants (Census Based)	-0.094	(0.149)	$[-0.386, 0.198]$
Panel B. Patents and Science			
Total	0.063	(0.055)	$[-0.045, 0.172]$
Pharmaceutical	0.063	(0.059)	$[-0.053, 0.180]$
Communication	0.019	(0.050)	$[-0.078, 0.116]$
Electrical	0.056	(0.066)	$[-0.073, 0.185]$
Mechanical	0.073	(0.057)	$[-0.038, 0.185]$
Other	0.059	(0.052)	$[-0.043, 0.161]$
STEM Employment Share	0.389	(0.281)	$[-0.162, 0.939]$
Panel C. Income and Demographics			
Population Density	-0.009	(0.206)	$[-0.413, 0.395]$
Occupational Score per Capita	0.100	(0.075)	$[-0.047, 0.246]$
Share of Men	-0.092^{*}	(0.054)	$[-0.197, 0.013]$
Share of Illiterates	-0.147	(0.138)	$[-0.417, 0.123]$
Share of Young	0.097	(0.146)	$[-0.190, 0.384]$
Panel D. Ethnic Composition			
Share of Whites	0.096	(0.092)	$[-0.084, 0.276]$
Share of African Americans	-0.096	(0.092)	$[-0.276, 0.084]$
Share of Foreign Population	0.310^{***}	(0.110)	$[0.095, 0.525]$
Immigrants from:			
Italy	0.191	(0.138)	$[-0.079, 0.461]$
Ireland	-0.141	(0.123)	$[-0.381, 0.100]$
Austria	0.275^{**}	(0.116)	$[0.046, 0.503]$
France	-0.044	(0.074)	$[-0.189, 0.101]$
Spain	-0.007	(0.055)	$[-0.114, 0.100]$
Portugal	-0.063	(0.157)	$[-0.372, 0.245]$

Notes: This table displays the correlation between the Excess Death (defined in (3)) and a set of covariates in 1910, i.e., the last census year before the pandemic. Column (1) reports the standardized coefficient of a regression between the row variable and our measure of excess deaths; column (2) reports the associated standard error in round brackets; column (3) reports the confidence interval of the point estimate at the 95% confidence level in square brackets. All variables are expressed as shares of the total population, except for population density. Regressions control for county population and include state-fixed effects. Referenced on page(s) 12.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE B.4. IMPACT OF THE INFLUENZA ON RELIGIOSITY: WEIGHTED REGRESSIONS

	Share of Affiliated			Name-Based Religiosity		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Catholics	Protestants	All	Catholics	Protestants
Excess Deaths × Post	0.264**	0.119**	0.137***	1.052***	0.298	1.084***
	(0.135)	(0.056)	(0.029)	(0.274)	(0.247)	(0.229)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Decade FE	Yes	Yes	Yes	–	–	–
State-Year FE	–	–	–	Yes	Yes	Yes
Number of Counties	1275	1275	1275	1274	1274	1274
Observations	3825	3825	3825	38220	38220	38220
R^2	0.775	0.888	0.925	0.843	0.768	0.844
Std. Beta Coef.	0.696	0.398	0.626	0.165	0.108	0.228

Notes: This table displays the impact of exposure to the Great Influenza Pandemic on religiosity. The unit of observation is a county observed at a decade frequency between 1906 and 1926 (in columns 1–3) and yearly frequency between 1900 and 1929 (in columns 4–6). "Post" is a categorical variable equal to one during and after the pandemic—i.e., over 1918 to 1929—or zero otherwise. The baseline treatment "Excess Deaths" is defined in Equation (3). In columns (1–3), the dependent variable is the number of individuals affiliated with religious denominations enumerated in the Census of Religious Bodies, normalized by county population in 1910; in columns (4–6), the dependent variable is the name-based religiosity measure described in the main text. Columns (1) and (4) report the effect of the influenza on overall religiosity, whereas columns (2) and (5)—resp. (3) and (6)—display it on the intensity of Catholicism—resp. Protestantism. Regressions include county and state-by-time (decades in columns 1–3 and years in columns 4–6), fixed effects, and control for an interaction term between the population in 1910 and a post-treatment indicator. Counties are weighted by population in 1910. Standard errors, clustered at the county level, are reported in parentheses. Referenced on page(s) 13.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE B.5. IMPACT OF THE INFLUENZA ON RELIGIOSITY

	Baseline Sample			Family Size Cuts		Household	Adults
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Cont. Treat.	Disc. Treat.	WW1	No firstborn	< 5 Kids		
Excess Deaths × Post	0.827***		0.824***	1.182***	0.921***	0.123***	0.001
	(0.199)		(0.199)	(0.234)	(0.191)	(0.026)	(0.002)
Excess Deaths Dummy × Post		0.252***					
		(0.060)					
WW1 Deaths × Post			-0.000				
			(0.001)				
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Baseline	Baseline	Baseline	No Firstborn	< 5 Kids	Household	Adults
Number of Counties	1274	1274	1274	1274	1274	1274	1267
Observations	38220	38220	38220	38220	38220	38220	5068
R^2	0.648	0.648	0.648	0.623	0.631	0.754	0.782
Std. Beta Coef.	0.151	0.033	0.150	0.224	0.174	0.162	0.028

Notes: This table displays the impact of exposure to the Influenza on overall religiosity. The unit of observation is a county, observed at a yearly frequency between 1900 and 1929 in columns (1)-(6) and at a decade frequency in column (7). "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over the years 1918-1929 in columns (1)-(6) and the decades 1920-1930 in column (7)–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (3). The dependent variable is the name-based measure of aggregate religiosity described in the main text. Column (1) displays the baseline results. Column (2) reports the results coding the treatment as a binary variable returning value one if the continuous treatment is above its median and zero otherwise. In column (3), we control for WW1-related deaths. Column (4) drops first-born children in every household. In column (5), we compute religiosity, dropping all children beyond the fourth in each household. In column (6) we first compute within-household average religiosity and then aggregate the resulting religiosity series at the county-year level. Column (7) reports results measuring county religiosity using the names stock of adults–which serves as a placebo check. All regressions in columns (1)-(6) include county and state-by-year-fixed effects; the regression in column (7) includes county and decade-fixed effects. Additionally, each regression controls for an interaction term between the population in 1910 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses. Referenced on page(s) A4, C32, C32, C32, C32.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

Table B.6. Impact of the Influenza on Religiosity: Accounting for Migration

	Religiosity Excluding Internal Migrants		
	(1)	(2)	(3)
	All	Catholics	Protestants
Excess Deaths × Post	0.876***	-0.038	0.740***
	(0.199)	(0.103)	(0.172)
County FE	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes
Number of Counties	1274	1274	1274
Observations	38220	38220	38220
R^2	0.646	0.525	0.668
Std. Beta Coef.	0.161	-0.016	0.174

Notes: This table displays the impact of exposure to the Influenza on religiosity. The unit of observation is a county observed at a yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (3). Religiosity is measured using religiosity scores obtained by estimating equation (1). Differently from the main text, we exclude from the sample all those who, in the 1930 census, are recorded residing in a state that is different from the one where they were born. Regressions include county and state-by-year-fixed effects and controls for an interaction term between the population in 1910 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses. Referenced on page(s) 14, C32.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

Table B.7. Impact of the Influenza on Religiosity: City-Level Analysis

	Dep. Var.: Religiosity		
	(1)	(2)	(3)
	All	Catholics	Protestants
Excess Deaths $\times$ Post	0.012***	-0.000	0.010***
	(0.003)	(0.002)	(0.003)
City FE	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes
Number of Cities	439	439	439
Observations	13170	13170	13170
R^2	0.640	0.788	0.666
Std. Beta Coef.	0.012	-0.000	0.010

Notes: This table displays the city-level effect of exposure to the Influenza on religiosity. The unit of observation is a city observed at a yearly frequency between 1900 and 1929. We report the location of each city in the sample in figure B.7. The baseline sample is from Clay *et al.* (2019). We include only cities where we can construct the baseline excess mortality measure. The dependent variable is the name-based religiosity measure constructed on the universe of children born between 1900 and 1929 and residing in each city in the 1930 census. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (3). Each regression includes city and state-by-year-fixed effects. Standard errors, clustered at the city level, are reported in parentheses. Referenced on page(s) 14, A4, B30, B30, C32.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE B.8. IMPACT OF THE INFLUENZA ON RELIGIOSITY: NAMES SCORES WITHOUT FIXED EFFECTS

	Unweighted			Weighted		
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Catholics	Protestants	All	Catholics	Protestants
Excess Deaths × Post	2.508***	3.003***	-0.595	3.572**	4.368**	-0.986
	(0.878)	(0.717)	(0.515)	(1.772)	(1.872)	(0.973)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of Counties	1274	1274	1274	1274	1274	1274
Observations	38220	38220	38220	38220	38220	38220
R^2	0.910	0.902	0.563	0.977	0.965	0.841
Std. Beta Coef.	0.068	0.091	-0.040	0.058	0.093	-0.049

Notes: This table displays the impact of exposure to the Influenza on religiosity. The unit of observation is a county observed at a yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (3). Religiosity is measured using religiosity scores obtained by estimating equation (1), except that we do not include the fixed effects in the regression specification. In columns (4)–(6), counties are weighted by their population in 1900. Columns (1) and (4) report the results for total religiosity; columns (2) and (5) refer to Catholics; columns (3) and (6) refer to Protestants. Regressions include county and state-by-year-fixed effects and control for an interaction term between the population in 1910 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses. Referenced on page(s) 15, C32, C32.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE B.9. IMPACT OF THE INFLUENZA ON RELIGIOSITY: ALTERNATIVE THRESHOLDS

	All			Catholics			Protestants		
	$(\tau = 1)$	$(\tau = 2)$	$(\tau = 4)$	$(\tau = 1)$	$(\tau = 2)$	$(\tau = 4)$	$(\tau = 1)$	$(\tau = 2)$	$(\tau = 4)$
Excess Deaths × Post	0.790***	0.879***	0.407**	0.300***	0.260**	0.022	0.811***	1.061***	0.219
	(0.244)	(0.238)	(0.168)	(0.116)	(0.121)	(0.108)	(0.210)	(0.214)	(0.140)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Counties	1274	1274	1274	1274	1274	1274	1274	1274	1274
Observations	38220	38220	38220	38220	38220	38220	38220	38220	38220
R^2	0.396	0.421	0.692	0.494	0.467	0.529	0.577	0.614	0.778
Std. Beta Coef.	0.143	0.171	0.079	0.099	0.097	0.010	0.165	0.226	0.049

Notes: This table displays the impact of exposure to the Influenza on religiosity. The unit of observation is a county observed at a yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (3). Religiosity is measured using religiosity scores obtained by estimating equation (1). The term τ denotes the frequency threshold a name must exceed to be included in our sample, in ‰ terms. For instance, $\tau = 2$ implies that at least 2‰ children in our sample must be called with a given name, for that name to be included in the sub-sample of names used to compute the religiosity score. In the various columns, we report the estimated coefficients for different frequency threshold values. As τ decreases, the number of names for which we compute a religiosity score increases. Regressions include county and state-by-year-fixed effects and control for an interaction term between the population in 1910 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses. Referenced on page(s) 15, C32.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE B.10. IMPACT OF THE INFLUENZA ON THE CONCENTRATION OF NAMES

	HHI	CCI	Rosenbluth	C-5	C-6	C-7	C-8
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Excess Deaths × Post	0.168^{*}	0.002	0.160^{*}	0.004	0.006	0.007	0.009^{*}
	(0.095)	(0.002)	(0.092)	(0.004)	(0.004)	(0.005)	(0.005)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Counties	1275	1275	1275	1275	1275	1275	1275
Observations	38250	38250	38250	38250	38250	38250	38250
R^2	0.696	0.823	0.708	0.820	0.833	0.841	0.846
Std. Beta Coef.	0.062	0.039	0.058	0.049	0.064	0.075	0.084

Notes: This table displays the impact of exposure to the Influenza on name concentration. The unit of observation is a county observed at a yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (3). The dependent variables measure the concentration of names and are: in column (1) the Herfindahl-Hirschman (HHI) index; in column (2) the Comprehensive Concentration index (CCI), which relative to the HHI assigns more weight to relatively uncommon names; in column (3) the Rosenbluth index (RI), which further refines the CCI because it is more sensitive to the number of uncommon names. In columns (4)–(7), the dependent variable is the k-concentration ratio, *i.e.* the share of children called with the k most common names. More formally, let s_n denote the share of kids with name n, and let N be the total number of names. Suppose that shares are ranked in increasing order, meaning that rank$(n) \leq$ rank(n') if and only if $s_n \geq s_{n'}$, and rank$(n) <$ rank(n') if and only if $s_n > s_{n'}$ for all n, n'. Then, $HHI \equiv \sum_{n=1}^{N} s_n^2$; $CCI \equiv s_1 + \sum_{n=2}^{N} s_n^2(2 - s_n)$, $RI \equiv \frac{1}{2\sum_{n=1}^{N} n s_n - 1}$; $C_K \equiv \sum_{n=1}^{K} s_n$. Regressions include county and state-by-year fixed effects and the interaction between the population in 1910 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses. Referenced on page(s) 15, A4, C32.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE B.11. IMPACT OF THE INFLUENZA ON RELIGIOSITY MEASURED WITH SAINT AND BIBLICAL NAMES

	Abramitzky et al's Religiosity		
	(1)	(2)	(3)
	Saints/Biblical	Saints	Biblical
Excess Deaths × Post	3.446**	3.076**	0.865**
	(1.452)	(1.357)	(0.359)
County FE	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes
Number of Counties	1274	1274	1274
Observations	38220	38220	38220
R^2	0.991	0.991	0.984
Std. Beta Coef.	0.032	0.030	0.029

Notes: This table displays the impact of exposure to the Influenza on religiosity. The unit of observation is a county observed at a yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (3). In column (1), the dependent variable is the log number of children by cohort whose name either appears in the bible or is carried by a saint; in column (2), the dependent variable only includes biblical names; in column (3), it only includes names of saints. Biblical and saints' names are from Abramitzky *et al.* (2016). Regressions include county and state-by-year-fixed effects and control for an interaction term between the population in 1910 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses. Referenced on page(s) 15, C32, C32.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

Table B.12. Impact of the Influenza on Innovation: Weighted Regressions

	STEM Employment Share		log(1 + Number of Patents)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Whole Population	Skilled Population	All Patents	Pharmaceuticals	Communication	Electrical	Mechanical	Other
Post × Excess Deaths	0.010***	0.106***	0.794***	0.320***	0.167	0.075	0.031	0.003
	(0.003)	(0.021)	(0.134)	(0.081)	(0.140)	(0.080)	(0.039)	(0.038)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Decade FE	Yes	Yes	–	–	–	–	–	–
State-Year FE	–	–	Yes	Yes	Yes	Yes	Yes	Yes
All Patents	–	–	No	Yes	Yes	Yes	Yes	Yes
Number of Counties	1274	1274	1275	1275	1275	1275	1275	1275
Observations	3822	3822	38250	38250	38250	38250	38250	38250
R^2	0.811	0.771	0.958	0.949	0.883	0.941	0.978	0.983
Std. Beta Coef.	1.152	1.436	0.231	0.119	0.089	0.028	0.010	0.001

Notes: This table displays the impact of exposure to the Great Influenza Pandemic on innovation. The unit of observation is a county, observed at a decade frequency between 1900 and 1930 in columns (1–2) and at a yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic—i.e., over 1918 to 1929—or zero otherwise. The baseline treatment "Excess Deaths" is defined in Equation (3). In column (1), the dependent variable is the share of people employed in STEM occupations within the population; in column (2), we restrict the denominator to include only those employed in skilled occupations. The dependent variable in columns (3–8) is the (log) number of patent grants. We use this specification of the dependent variable to ensure that we do not drop counties without patents. In columns (4–8), we also control for the overall (log) number of granted patents. Column (3) estimates the impact of the pandemic on the level of innovation, while columns (4)–(8) display this on the direction of innovation. All regressions include county-fixed effects and control for an interaction term between the population in 1910 and a post-treatment indicator. Regressions (1–2) include state-by-decade-fixed effects, while regressions (3–8) include state-by-year-fixed effects. Standard errors, clustered at the county level, are reported in parentheses. Counties are weighted by population in 1910. Referenced on page(s) 17.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE B.13. IMPACT OF THE INFLUENZA ON INNOVATION: ROBUSTNESS REGRESSIONS

	All Patents				Pharmaceutical Patents				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Baseline	Unbalanced	Disc. Treat	WW1 Deaths	Baseline	No All Patents	Unbalanced	Dummy	WW1 Deaths
Excess Deaths × Post	0.336***	0.416***		0.336***	0.099***	0.209***	0.183***		0.099***
	(0.056)	(0.077)		(0.056)	(0.029)	(0.040)	(0.054)		(0.029)
Excess Deaths Dummy × Post			0.080***					0.035***	
			(0.019)					(0.010)	
WW1 Deaths × Post				3.326					2.136
				(6.144)					(2.115)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
All Patents	No	No	No	No	Yes	No	Yes	Yes	Yes
Number of Counties	1275	1227	1275	1275	1275	1275	1227	1275	1275
Observations	38250	23689	38250	38250	38250	38250	23689	38250	38250
R^2	0.858	0.888	0.858	0.858	0.831	0.792	0.813	0.831	0.831
Std. Beta Coef.	0.336	0.416	0.080	0.336	0.099	0.209	0.183	0.035	0.099

Notes: This table displays the impact of exposure to the Influenza on innovation. The unit of observation is a county observed at a yearly frequency between 1900 and 1929. In columns (1)–(4), the dependent variable is the number of patents across all fields; in columns (5)–(9), it is the number of patents in chemical and drug fields, according to the NBER standard classification. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (3). Columns (1) and (5) display the baseline results. Columns (2) and (7) report results for the unbalanced panel of counties (*i.e.*, the subsample of county-year observations for which we observe at least one filed patent). Columns (3) and (8) report the results when the treatment is coded as a binary variable equal to one if the continuous variable is above its median and zero otherwise. Columns (4) and (9) further control for WW1 deaths interacted with the post-treatment indicator. In column (6), we report the estimated effect without controlling for the total number of patents. All regressions include county and state-by-year-fixed effects and control for an interaction term between the population in 1910 and a post-treatment indicator. Columns (5,7-9) further control for the total number of patents. Standard errors are clustered at the county level and are reported in parentheses. Referenced on page(s) 12, 17, A4, C32, C32, C32.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE B.14. IMPACT OF THE INFLUENZA ON INNOVATION: ALTERNATIVE MEASURES OF OVERALL INNOVATION

	f (All Patents)			
	(1)	(2)	(3)	(4)
	$\ln(1+\cdot)$	Count	$\text{arcsinh}(\cdot)$	Poisson
Excess Deaths × Post	0.336***	3.736**	0.410***	1.260***
	(0.056)	(1.790)	(0.069)	(0.226)
County FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes
Number of Counties	1275	1275	1275	1256
Observations	38250	38250	38250	37680
R^2	0.858	0.911	0.840	0.907
Std. Beta Coef.	0.336	3.736	0.410	3.526

Notes: This table displays the effect of the Influenza on overall innovation. The unit of observation is a county observed at a yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over 1918-1929–and zero otherwise. In column (1), the dependent variable is the log number of patents, to which we add one to avoid dropping zeros. The dependent variable in column (2) is the raw patent count. In column (3), the dependent variable is the inverse hyperbolic sine of the raw count of patents. In column (4), the model is estimated as a Poisson regression, and the dependent variable is the raw patent count. Each regression includes county and state-by-year-fixed effects and controls for an interaction term between the population in 1910 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses. Referenced on page(s) 17, C32, C32.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE B.15. IMPACT OF THE INFLUENZA ON INNOVATION: ALTERNATIVE MEASURES OF PHARMACEUTICAL INNOVATION

	f (Pharmaceutical Patents)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	$\ln(1+\cdot)$	$\ln(1+\cdot)$	Count	Count	arcsinh$(\cdot)$	arcsinh$(\cdot)$	Share	$\ln(1+\text{Share})$	Poisson
Excess Deaths $\times$ Post	0.099***	0.209***	0.629***	1.375***	0.125***	0.256***	0.037***	0.031***	1.953***
	(0.029)	(0.040)	(0.202)	(0.443)	(0.037)	(0.050)	(0.010)	(0.009)	(0.306)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Total Patents	Yes	No	Yes	No	Yes	Yes	No	No	No
Number of Counties	1275	1275	1275	1275	1275	1275	1275	1275	1125
Observations	38250	38250	38250	38250	38250	38250	38250	38250	33736
R^2	0.831	0.792	0.958	0.860	0.815	0.777	0.208	0.231	0.795
Std. Beta Coef.	0.327	0.209	0.629	1.375	0.318	0.256	0.037	0.031	7.049

Notes: This table displays the effect of the Influenza on innovation in pharmaceuticals. The unit of observation is a county observed at a yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over 1918-1929–and zero otherwise. In columns (1) and (2), the dependent variable is the log number of patents, to which we add one to avoid dropping zeros. The dependent variable is the raw patent count in columns (3) and (4). In columns (5) and (6), the dependent variable is the inverse hyperbolic sine of the raw count of pharmaceutical patents, with and without controlling for the inverse hyperbolic sine of the total number of patents. In column (7), the outcome is the number of pharmaceutical patents relative to patents in all other fields. In column (8), this is taken in logs. Each regression includes county and state-by-year-fixed effects and controls for an interaction term between the population in 1910 and a post-treatment indicator. In column (9), the model is estimated as a Poisson regression, and the dependent variable is the raw patent count. In columns (1), (3), and (6), we further control the total number of patents by county year, transformed according to the column-specific labeled function. Standard errors are clustered at the county level and are reported in parentheses. Referenced on page(s) 17, C32, C32.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE B.16. IMPACT OF THE INFLUENZA ON PATENT IMPORTANCE

	All Patents		Pharmaceuticals		
	(1)	(2)	(3)	(4)	(5)
	Breakthrought	Share Breakthrough	Breakthrought	Breakthrought	Share Breakthrough
Excess Deaths × Post	0.166***	0.016**	0.185***	0.160***	0.042***
	(0.043)	(0.008)	(0.039)	(0.035)	(0.010)
County FE	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes
Total Patents	No	No	No	Yes	No
Number of Counties	1275	1275	1275	1275	1275
Observations	38250	38250	38250	38250	38250
R^2	0.797	0.265	0.704	0.731	0.340
Mean Dep. Var.	638.000	638.000	638.000	638.000	638.000
Std. Beta Coef.	0.166	0.016	0.185	0.007	0.042

Notes: This table displays the impact of the Influenza on patent importance. The unit of observation is a county observed at a yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic– *i.e.* over 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (3). Importance measures are from Kelly *et al.* (2021). They measure the "importance" of a patent based on the textual similarity between that patent and previous and future works and flag it as important if it is different from previous work but similar to subsequent ones. In columns (1) and (3–4), the dependent variable is the share of breakthrough patents, defined as those in the top 5% of the quality distribution. In columns (2) and (5), the dependent variable is the share of breakthrough patents. Regressions include county and state-by-year-fixed effects and control for an interaction term between the population in 1910 and a post-treatment indicator. In column (4), we further control for the total number of patents. Standard errors are clustered at the county level and displayed in parentheses. Referenced on page(s) 18, C32.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE B.17. IMPACT OF THE INFLUENZA ON INNOVATION: ACCOUNTING FOR MIGRATION

	No Internal Migrants	
	(1)	(2)
	Full Sample	High-Skilled
Excess Deaths × Post	0.013***	0.293***
	(0.004)	(0.076)
County FE	Yes	Yes
State-Decade FE	Yes	Yes
Number of Counties	1275	1274
Observations	38230	38220
Sample	Full	Skilled
R^2	0.825	0.739
Std. Beta Coef.	0.031	0.063

Notes: This table displays the effect of the pandemic on the probability of being employed in a STEM occupation. The observation unit is a county at a decade frequency between 1900 and 1930. The dependent variable is the share of individuals employed in STEM occupations relative to the overall population (column 1) or the number of people employed in skilled occupations (column 2). We exclude from the sample internal migrants, defined as those who were born in a different state relative to where they are recorded in the 1930 census. STEM and skilled occupations are enumerated in Table B.2. The baseline treatment "Excess Deaths" is defined in equation (3) and is interacted with a post-Flu indicator. All regressions include county and state-by-decade fixed effects and further control for an interaction term between the population in 1910 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses. Referenced on page(s) 18, C32.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE B.18. IMPACT OF THE INFLUENZA ON INNOVATION: CITY-LEVEL ANALYSIS

	Dep. Var.: Number of Patents	
	(1)	(2)
	All Patents	Pharmaceutical
Excess Deaths $\times$ Post	0.554**	0.743**
	(0.253)	(0.327)
City FE	Yes	Yes
State-Year FE	Yes	Yes
Number of Cities	476	474
Observations	14280	14206
R^2	0.949	0.851
Std. Beta Coef.	1.740	2.101

Notes: This table displays the city-level effect of exposure to the Influenza on innovation. The unit of observation is a city observed at a yearly frequency between 1900 and 1929. We report the location of each city in the sample in figure B.7. The baseline sample is from Clay *et al.* (2019). We include only cities where we can construct the baseline excess mortality measure. The dependent variable is the number of patents (column 1) and pharmaceutical patents (column 2). "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (3). Each regression includes city and state-by-year-fixed effects. Standard errors, clustered at the city level, are reported in parentheses. Referenced on page(s) 18, A4, B30, B30, C32.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE B.19. RELIGIOSITY AND THE INTENSITY OF INNOVATION BY EXPOSURE TO THE INFLUENZA

	Dep. Var.: Patents per Capita			
	(1)	(2)	(3)	(4)
	Pre Flu	Post Flu	Pooled	DiD
Religiosity	-0.059*	0.084**	-0.207***	-0.649***
	(0.035)	(0.041)	(0.042)	(0.241)
Religiosity × Post			0.659***	
			(0.084)	
Religiosity × Excess Deaths				0.404*
				(0.211)
Excess Deaths × Post				0.053***
				(0.014)
Excess Deaths × Religiosity × Post				0.556***
				(0.072)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Number of Counties	1274	1274	1274	1274
Observations	22932	15288	38220	38220
R^2	0.548	0.734	0.578	0.580

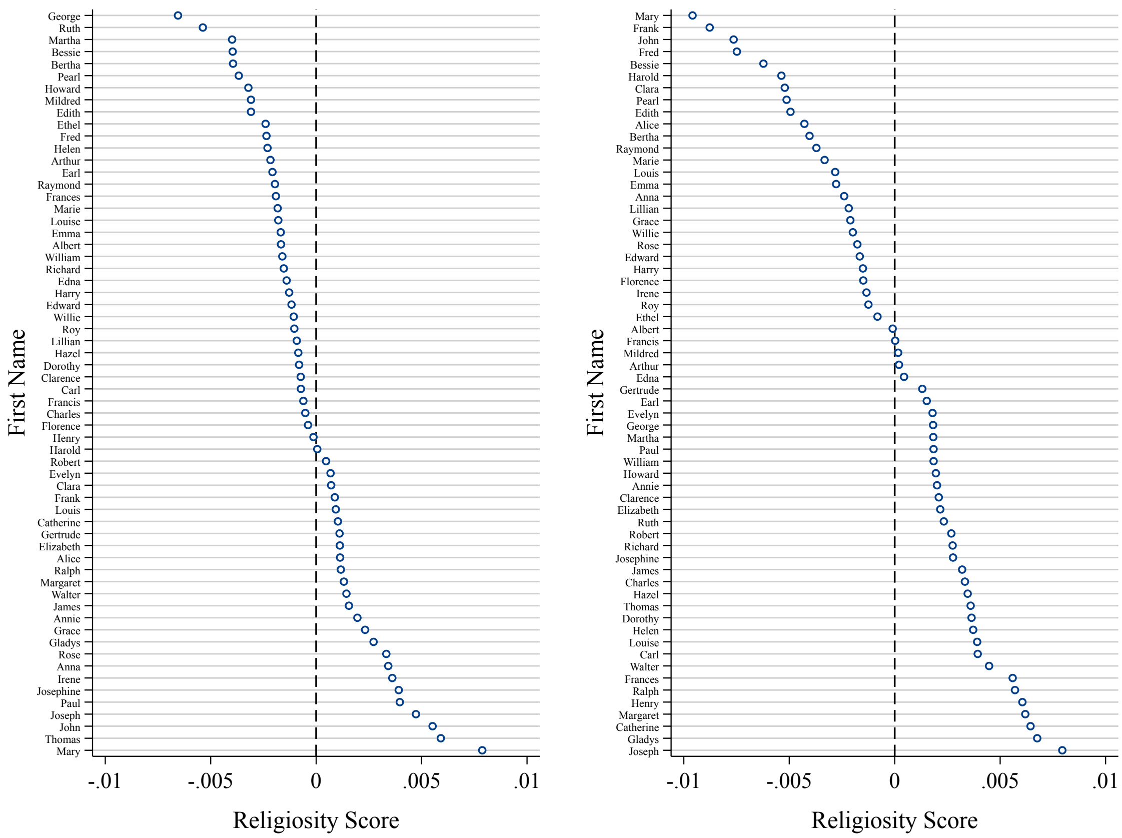
Notes: This table displays the correlation between innovation and religiosity by exposure to the pandemic. The dependent variable is the number of patents normalized by county population in 1910, expressed in 1,000 units. The unit of observation is a county observed at a yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (3). In column (1), we display the correlation between religiosity and innovation before the Flu (before 1918); in column (2), we replicate this exercise for the post-Flu years; in column (3), we pool the years together, and interact religiosity with a post-Flu indicator; finally, column (4) reports the differential effect of the pandemic by religiosity. Regressions include county and state-by-year-fixed effects and control for an interaction term between the population in 1910 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses. Referenced on page(s) 19.

*: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

B.II Figures

FIGURE B.1. ESTIMATED NAMES RELIGIOSITY SCORES, BY CONFESSION

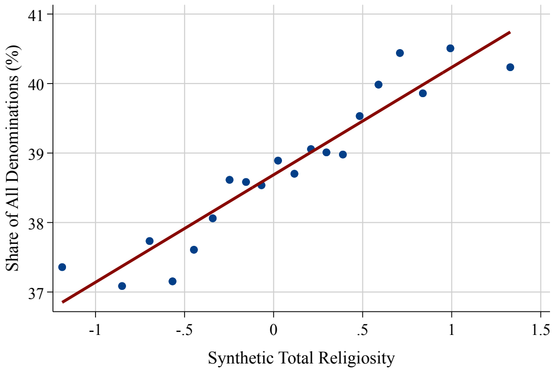
(A) CATHOLICS (B) PROTESTANTS



Notes: The Figures display the religiosity scores estimated from model (1). Bars report the point estimate of each coefficient. Regressions are based on data from the 1906-1916 Censuses of Religious Bodies and include individuals born between 1896 and 1916. We estimate religiosity scores for names appearing in at least 0.3% of the overall sample. Panel B.1a reports scores for Catholicism; Panel B.1b reports scores for Protestantism. Referenced on page(s) 9.

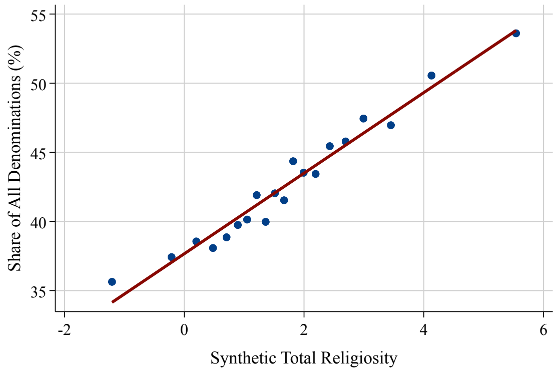
FIGURE B.2. IN-SAMPLE AND OUT-OF-SAMPLE FIT OF THE RELIGIOSITY MEASURE

(A) IN-SAMPLE: ALL DENOMINATIONS



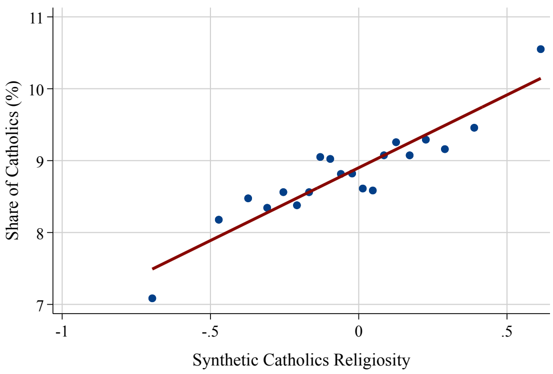
Notes. Regression Coefficient = 1.546 (Std. Err. = 0.227). R²=0.775.

(B) OUT-OF-SAMPLE: ALL DENOMINATIONS



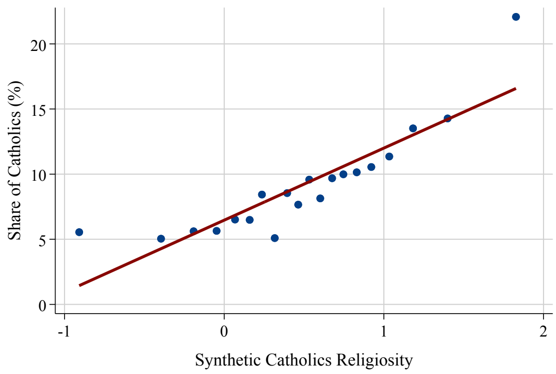
Notes. Regression Coefficient = 2.913 (Std. Err. = 0.142). R²=0.384.

(C) IN-SAMPLE: CATHOLICS



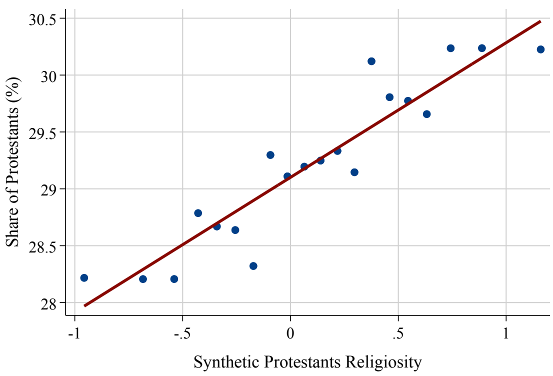
Notes. Regression Coefficient = 2.025 (Std. Err. = 0.248). R²=0.888.

(D) OUT-OF-SAMPLE: CATHOLICS



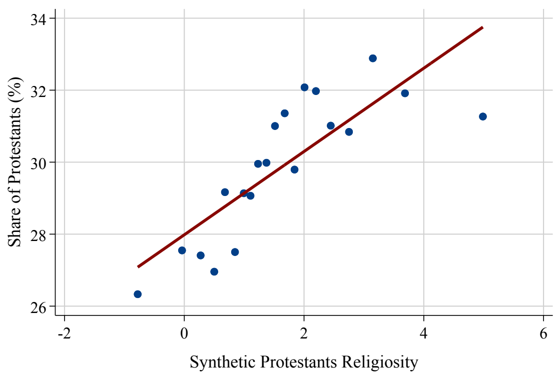
Notes. Regression Coefficient = 5.534 (Std. Err. = 0.341). R²=0.438.

(E) IN-SAMPLE: PROTESTANTS



Notes. Regression Coefficient = 1.183 (Std. Err. = 0.159). R²=0.887.

(F) OUT-OF-SAMPLE: PROTESTANTS



Notes. Regression Coefficient = 1.157 (Std. Err. = 0.145). R²=0.532.

Notes: These figures are county-level binned scatter plots reporting the correlation between our religiosity measure and the number of affiliated members to all denominations (B.2a-B.2b), Catholicism (B.2c-B.2d) and Protestantism (B.2e-B.2f) normalized by population in 1900. In-sample figures report data for the 1906 and 1916 censuses of religious affiliations. Out-of-sample figures instead report data for 1926. In-sample regressions control for county-fixed effects; out-of-sample regressions include state-fixed effects. In the note, we report the regression coefficients and the associated R^2. Referenced on page(s) B26, B26.

FIGURE B.3. EXAMPLE OF PHARMACEUTICAL PATENT

(A) TEXT

Patented Mar. 1, 1927. **1,619,005**

UNITED STATES PATENT OFFICE.

SAMUEL M. STRONG, OF GARDEN CITY, NEW YORK.

RESPIRATION AND PULSE RECORDER.

Application filed January 11, 1922. Serial No. 528,485.

This invention relates to a device or instrument for recording the character of the actions of the heart and respiratory organs of a person.

The primary object of the invention is to provide an instrument which will produce an accurate graphic representation of the rate, rhythm, and force of respiration and pulse of a human being over a short or a long period of time.

ing plate 13 and a vertical portion 19 adapted to be placed against a side plate of the casing 10. The main bearing plate fits snugly within the casing and one end of the horizontal portion 18 abuts against the cover 11 when the latter is in position. A side bearing plate 20 is located immediately adjacent to the detachable side plate 11 and an intermediate bearing plate 21 is interposed between the bearing plate 20 and the

(B) FIGURES

March 1, 1927. S. M. STRONG **1,619,005**

RESPIRATION AND PULSE RECORDER

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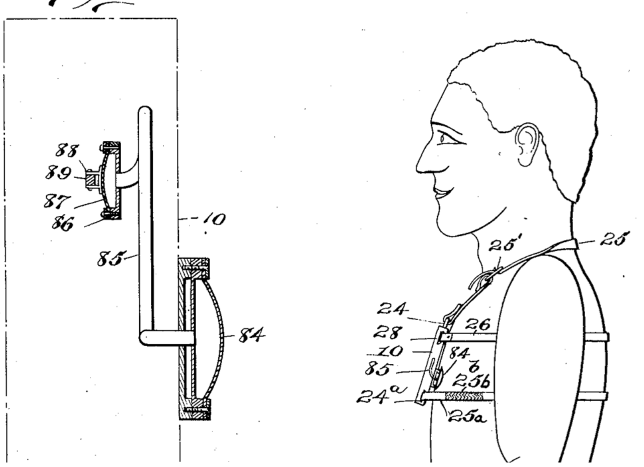
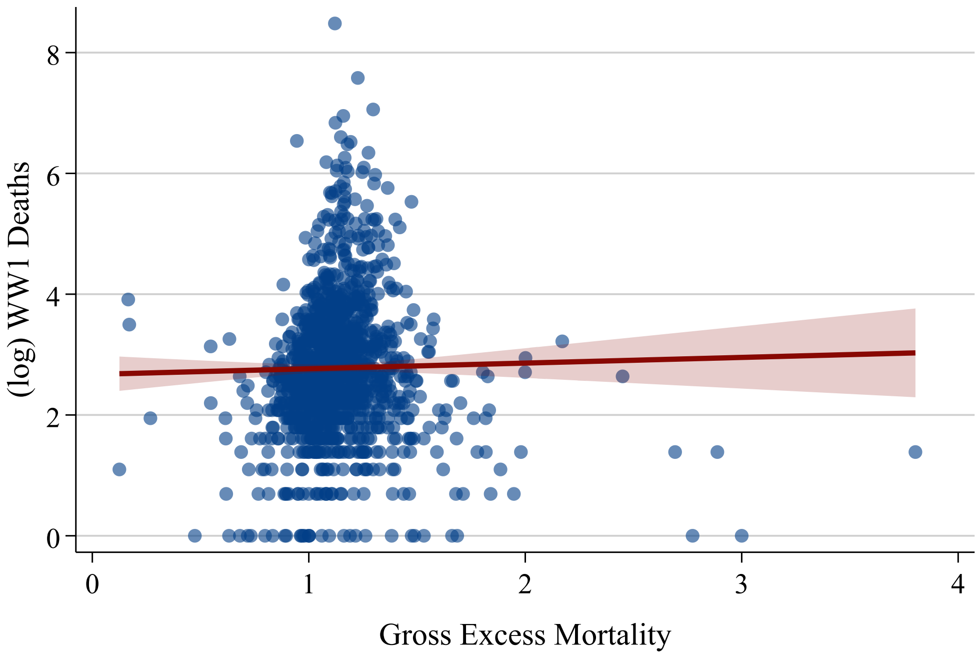
Fig. 4. Fig. 5.



Notes: This Figure displays the text and figures of one sample patent that our classification algorithm assigns to the pharmaceutical class. Referenced on page(s) 10.

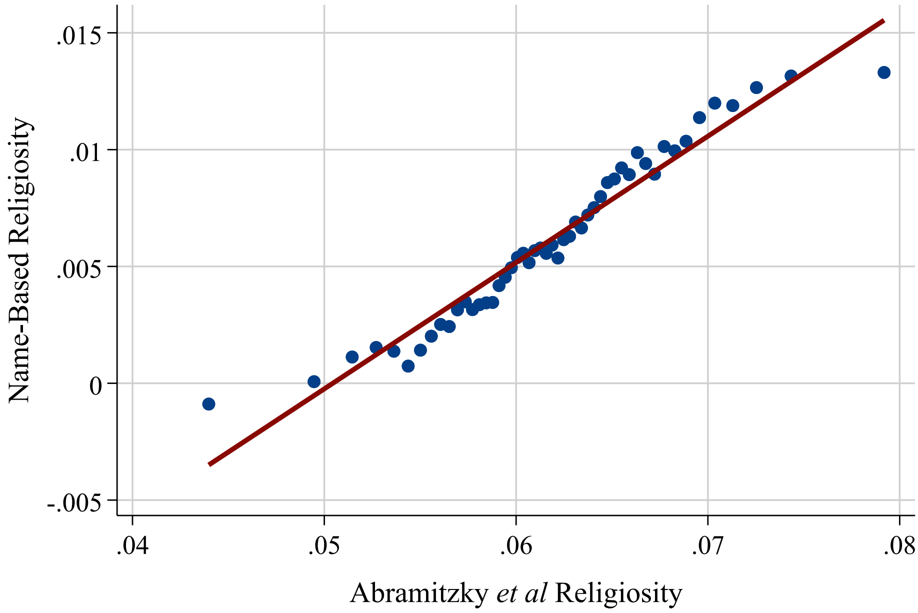
FIGURE B.4. CORRELATION BETWEEN WW1 DEATHS AND EXCESS DEATHS



Notes. Regression Coefficient = 0.094 (Std. Err. = 0.140). R^2=0.000.

Notes: This figure displays the correlation between WW1 deaths and excess deaths. Gross Excess Mortality is the baseline treatment. WW1 deaths are taken as logs. In the note, we report the regression coefficient between the two variables and the R^2 of the model. Data on WW1 deaths are from Ferrara and Fishback (2020). Referenced on page(s) 11, C32, C32, C32, C32.

FIGURE B.5. CORRELATION BETWEEN ABRAMITZKY *et al.* (2016) RELIGIOSITY AND BASELINE RELIGIOSITY

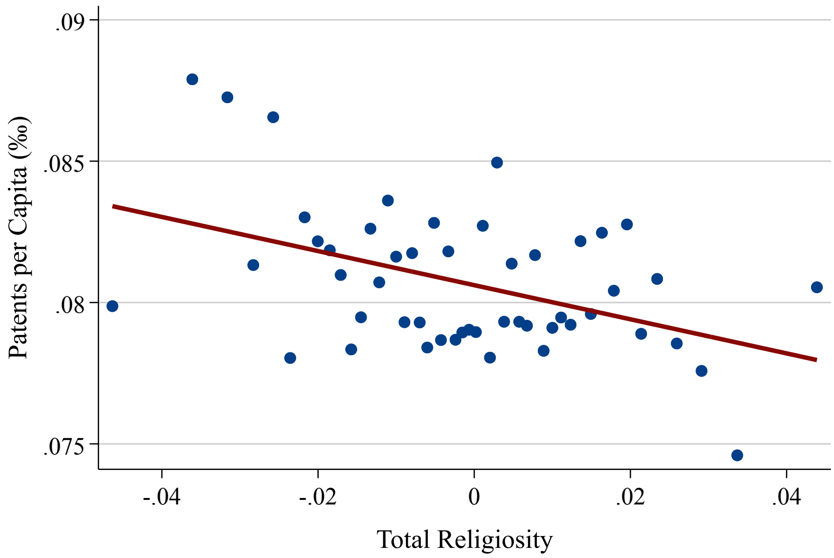


Notes. Regression Coefficient = 0.541 (Std. Err. = 0.018). R^2=0.584.

Notes: This figure reports the correlation between our baseline religiosity measure (multiplied by 100) and the share of biblical and saint names, as defined in Abramitzky *et al.* (2016). The unit of observation is a county observed at a yearly frequency between 1900 and 1930. The graph partials out county and year fixed effects. We report in note the regression coefficient and the associated standard error, clustered at the county level, and R^2 coefficient. Referenced on page(s) 15, C32.

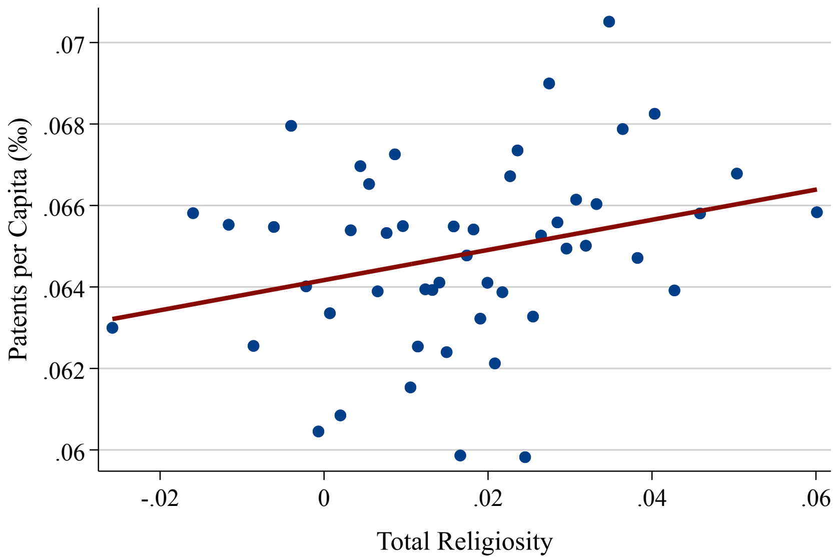
Figure B.6. Correlation Between Religiosity and Science

(a) Before the Great Influenza Pandemic (1910–1917)



Notes. Regression Coefficient = -0.060 (Std. Err. = 0.020). R^2=0.492.

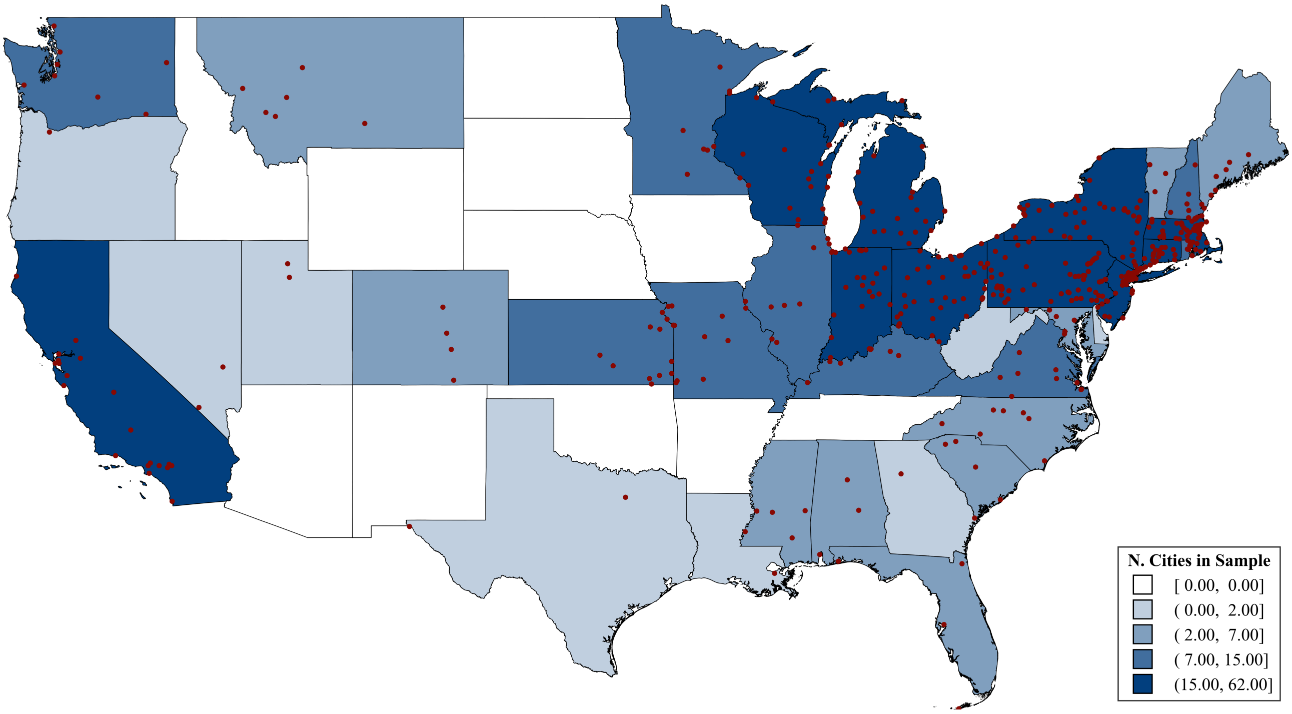
(b) After the Great Influenza Pandemic (1918–1929)



Notes. Regression Coefficient = 0.037 (Std. Err. = 0.020). R^2=0.576.

Notes: These figures display county-level binned scatter plots reporting the correlation between science–measured as patenting activity normalized by population–and religiosity. The unit of observation is a county observed at a yearly frequency. Religiosity is defined as described in section III.A and refers to overall religiosity. Graphs absorb for county and year-fixed effects. We report the regression coefficients and associated R^2 separately in each graph. Referenced on page(s) 19.

FIGURE B.7. DISTRIBUTION OF CITIES IN THE ALTERNATIVE SAMPLE



Notes: This figure reports the spatial distribution of the cities in the city-level sample used in Tables B.7–B.18. We use data from Clay *et al.* (2019), which contains information on 483 large cities. The red dots report the coordinates of the 478 cities for which we can construct the excess mortality treatment measure. Lighter to darker shades of blue indicate the state-level number of cities included in the final sample. Referenced on page(s) 14, A4, B12, B12, B23, B23.

C Summary of Robustness Analyses

	Panel A: Religiosity		
	Exhibit	**Topic**	**Description**
1. Is our name-based measure indeed capturing religiosity?			
i)	Table B.5	Accounting for birth order and fertility	One concern is that our results are driven by: i) firstborns, who may be more likely to be named after grandparents (who may have more religious names); ii) numerous families having idiosyncratic naming patterns correlated with religiosity; iii) more religious families having higher fertility. To address these concerns, in Table B.5, we show that our results hold when we drop firstborns (column 1); we drop children beyond the fourth (column 5); we compute household-level average religiosity by assigning to every child a weight that is inversely related to the number of children in the household (column 6)
ii)	Table B.8	Religiosity scores without county FE	Our results may be sensitive to how we compute religiosity scores. In Table B.8, we show that our findings hold when we drop county-fixed effects from the measurement equation. Religiosity scores computed this way reflect the stock of religiosity in a given county rather than its change.
iii)	Table B.9	Alternative name frequency thresholds	In our main analysis, we impose a threshold of 0.3% for names to be included in the sample for which we estimate religiosity scores. Our results are robust when using alternative frequency thresholds.
iv)	Table B.10	Fashion effects of names	Our results could be driven by a fashion effect: while more religious names may have indeed become more common immediately after the pandemic, their subsequent increase may have been driven by their increased popularity (independently from their religious content). We provide evidence against one simple corollary of this argument. Namely, we find that name concentration does not increase in counties more exposed to the shock.

v)	Table B.11	Alternative religiosity measures	Our results are robust to using three alternative indicators. In Table B.11 we compute a religiosity indicator of first names based on the names of biblical figures and saints collected by Abramitzky *et al.* (2016).
vi)	Figure B.2	Predicted vs. measured religiosity	We find a positive correlation between predicted religiosity and religiosity reported in the Census of religious bodies.
vii)	Figure B.5	Saint/ biblical names	Our measure of religiosity is strongly and positively correlated with the one developed by Abramitzky *et al.* (2016).
2. WAS THE INCREASE IN RELIGIOSITY CAUSED BY WW1?			
i)	Table B.5, Figure B.4	WW1 and religiosity	In column (3) we interact WW1 deaths with a post-pandemic indicator. The estimated coefficient is not significant. Additionally, in Figure B.4, we show that WW1 deaths and our measure of excess deaths are not correlated.
3. WAS THE INCREASE IN RELIGIOSITY DRIVEN BY MIGRATIONS?			
i)	Table B.5	Religiosity of adults	One results may be driven by selective migrations of more religious individuals towards areas more affected by the pandemic. In column (7), we use the religiosity score of own-name (a measure of an individual's background religiosity) as the dependent variable. As this variable was determined before the pandemic, a significant effect of the Influenza would suggest internal or international migrations correlated with background religiosity and the intensity of the shock. We do not find evidence to support this mechanism.
ii)	Table B.6	Internal migrants	We check that the effect of the influenza on religiosity is not driven by individuals who migrated to their residence in 1930 from another state, perhaps responding to a slacker labor market following the pandemic.
4. IS THE OBSERVATION UNIT (COUNTIES) DRIVING THE RESULTS?			
i)	Table B.7	Religiosity of cities	The baseline analysis is performed at the level of counties. We estimate the effect of the influenza on religiosity at the city level using the sample of Clay *et al.* (2019). We replicate the main results.

PANEL B: INNOVATION

	Exhibit	Topic	Description
5. IS OUR MEASURE INDEED CAPTURING INNOVATION ACTIVITY?			
i)	Table B.13	Exclude county-years without patents	In columns (2) and (7), we restrict the sample to counties where at least one filed patent is observed in the given year. The results are similar to our baseline, including county-year observations with zero patents.
ii)	Table B.13	Total volume of pharmaceutical innovation	In the baseline regressions with pharmaceutical patents as dependent variable, we control for the total number of patents to test whether the pandemic affects the direction of innovation. In column (6), we show that the unconditional level of innovation in pharmaceuticals increases in more exposed counties following the shock.
iii)	Table B.14, Table B.15	Alternative measures of innovation	We use alternative transformations of the raw patent count (explained in the header of Tables B.14 and B.15). Results are quantitatively stable across specifications.
6. WAS THE INCREASE IN INNOVATION CAUSED BY WW1?			
i)	Table B.13, Figure B.4	WW1 and innovation	In columns (4) and (9) we interact WW1 deaths with a post-pandemic indicator. The estimated coefficient is not significant. Additionally, in Figure B.4, we show that WW1 and influenza-related deaths are uncorrelated.
7. DID THE PANDEMIC TRIGGER AN INCREASE IN HIGH- (OR LOW-) QUALITY INNOVATION?			
i)	Table B.16	Patent quality	Using the measure defined by Kelly *et al.* (2021), we show that the average quality of patents is not affected by the pandemic (columns 1 and 4), but the number of high-quality patents, *i.e.* those in the upper 25% of the quality distribution, increases in more exposed counties (columns 2, 3, 6, and 7). These findings hold for the total number of patents, as well as for pharmaceutical patents.
8. IS THE OBSERVATION UNIT (COUNTIES) DRIVING THE RESULTS?			

i)	Table B.18	Innovation of cities	The baseline analysis is performed at the level of counties. We estimate the effect of the influenza on innovation at the city level using the sample of Clay *et al.* (2019). We replicate the main results.
9. Was the increase in STEM driven by migrations?			
i)	Table B.17	Religiosity of adults	One results may be driven by selective migrations of STEM individuals towards areas more affected by the pandemic. We explicitly test this by excluding individuals who were born in a different state than the one where they were recorded in the 1930 census. Results remain under both sample restrictions.

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