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# 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 innovation efforts. 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 naming patterns of newborns. We measure innovation through 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 innovative. Looking 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 it increases the polarization of religious beliefs.

Keywords: religiosity, science, innovation, great influenza pandemic
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# 1 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 in 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 innovation 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 innovative—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 an increase in innovation activity 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 main metric of scientific progress is the universe of geo-coded patents granted during this period in the U.S. (Berkes, 2018).[3]

[1] For example, Bentzen (2019) documents that, across countries and within regions, individuals become more religious when hit by earthquakes. Moscona (2022), instead, 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 innovation activity 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: the number of granted patents and the share of individuals in scientific occupations.

Using a difference-in-differences framework, we first show that counties hit harder by the shock experienced an increase in religiosity, an effect stronger for Catholicism. A one-standard-deviation increase in excess deaths—our main measure of intensity of the influenza shock—led to a 0.11 standard deviations increase in overall religiosity. We further document that these same counties also experienced an increase in innovative activities, an effect driven by patents granted in pharmaceuticals. A one-standard-deviation increase in excess deaths led to a 0.21 standard deviations increase in overall patenting activity. In addition, we find that employment in scientific occupations—our alternative indicator of scientific progress—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. As a result of the contemporaneous increase of religiosity and science, their relationship turned from negative before the pandemic to positive afterward. The latter 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 study individual-level responses *within* counties. 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 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, who were initially less religious than the rest of the population, became even less religious 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. As a consequence, 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. In fact, while a county may have become both more religious and more innovative, individuals seemed to react differently to the same shock—based, for instance, on their religious background or on their prepandemic scientific orientation. Religiosity and science appear to have been alternative ways of reacting to the pandemic, with individual becoming even more distant in terms of their religious sentiment than they were before

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

the shock.

We perform several checks to gauge the robustness of our findings. First, we internally validate our measure of religiosity across several dimensions (e.g., by computing our indicator excluding firstborn babies and accounting for potential heterogeneity in fertility patterns). Second, we externally validate our data-driven measure of religiosity by using alternative indicators. In particular, we show that results are robust to using the share of biblical and saints' names, as well as the share of people affiliated with a religious denomination. In addition, to ensure that the increase in religiosity is not driven by internal or external migration, we 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 more-religious people moving to areas hit harder by the shock. Third, we show that the increase in patenting activity was not driven by low-quality innovations. Patent quality increased after the pandemic in exposed counties, especially in pharmaceuticals. 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 innovation activity are not related to the intensity of the shock. Using an event-study design, we then show that religiosity and innovation were on a similar path across treated and control groups before the shock. Additionally, we rule out that a separate yet overlapping shock—World War I—may partly explain our findings.[5] 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.[6] 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.[7]

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

[6] 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 5).

[7] 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

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 most 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).[8] Another set of studies documents that economic crises (Babina et al., 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.[9]

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.[10] 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.[11]

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), and religiosity (Andersen and Bentzen, 2022).

played a key role in their decision to turn to science.

[8]The religious coping hypothesis, first developed in the psychology literature, posits that people who are subject to economic and social shocks turn to religious faith as a coping device to deal with personal distress.

[9]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).

[10]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) and Iyer (2016).

[11]Lecce et al. (2021) study how religiosity impacts the birth and migration of scientists in 19th-century French cantons, but they do not analyze how an adverse shock affects society's dual response in terms of religion and science and the underlying individual-level dynamics.

While all of these papers assume a preexisting rule to classify names (e.g., whether one has a biblical or saint name), to the best of our knowledge, we are the first to identify the religiosity of names directly from the data.[12]

The rest of the paper is structured as follows. In Section 2, we summarize the Great Influenza Pandemic in the United States and discuss the historical evidence concerning its effects on religiosity and innovation. In Section 3, we describe the data and our new indicator of religiosity. In Section 4, we present the empirical strategy and results. In Section 5, we discuss our findings. Section 6 concludes.

## 2 Historical Background

In this section, we provide an overview of the Great Influenza Pandemic in the United States and how it influenced religion and innovation.

### 2.1 The Great Influenza Pandemic

Between 1918 and 1919, the Great Influenza Pandemic—also known as the "Spanish Flu"[13]—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).[14]

Historical and modern accounts suggest that the pandemic hit the U.S. in a quasi-random fashion. 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 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

[12] For details on how we construct our religiosity measure, see Section 3.

[13] 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.

[14] 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).

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, cancelling public events, closing schools, and implementing isolation measures and quarantines—were effective in limiting the spread of the disease.[15]

## 2.2 The Pandemic and Religion

A large literature documents that individuals become more religious in response to adverse events. One explanation for why 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).[16]

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.[17] 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).[18] These heterogeneous responses find empirical

[15] 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).

[16] 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 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.

[17] 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).

[18] 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.

support in our analysis discussed in Section 4.

### 2.3 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 working through Science" (Montgomery, 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, and we reconcile this apparent puzzle by showing that it induced polarization within society, with some people turning to religion and others turning to science.

## 3 Data

To conduct our analysis, we construct a new dataset that combines information on religiosity, on innovation activity, and on 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.[19] In the second part of the analysis, we use individual-level data. Table 1 provides descriptive statistics of the main variables.

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

## 3.1 Religiosity Measure

The key challenge when studying religiosity is that it is difficult to measure, both today and in the past. It is especially challenging to find an indicator of religiosity that combines geographical granularity and high-frequency time variation.[20]

In our analysis, we propose a novel measure of revealed religiosity based on 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.[21]

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 Andersen and Bentzen (2022) who measure, in premodern and early-modern times, revealed parental religiosity, depending on whether children were named after church-dedicated saints. Our approach differs from theirs: we *empirically* identify our religious names, using data on the entire population of newborns and existing indicators of religiosity.

### 3.1.1 *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 babies born 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.[22] 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

[20] This is clear in historical settings—Squicciarini (2020), for instance, uses different measures of religiosity, but these are 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 and geographically aggregated, survey-based measures are not useful for studying the dynamics of religiosity at high time frequency and fine spatial granularity.

[21] 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 called "Harold," we assume that the parents of "Paul" are likely to be more religious than those of "Harold."

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

with a Catholic or Protestant one.[23]

To obtain the religiosity scores, we proceed in two steps. First, we compute the relative frequency of names. More precisely, let $N_{cd}$ be the total number of individuals born in county $c$ in decade $d$. We define the relative frequency of a given name (Name$^k$) in decade $d \in [t-10, t)$ as the ratio of all babies in that cohort called (Name$^k$) to the overall size of that cohort $N_{cd}$:

$$\text{Name Share}_{cd}^{k} \equiv \frac{1}{N_{cd}} \sum_{i=1}^{N_{cd}} \mathbf{1}(\text{Name}_{icd} = \text{Name}^k) \tag{1}$$

where $\mathbf{1}(\text{Name}_{icd} = \text{Name}^k)$ is an indicator function that returns the value one if individual $i$ in county $c$ born in decade $d$ is called (Name$^k$), and zero otherwise. In the second step, we estimate the following model:

$$y_{csd} = \alpha_c + \alpha_{s\times d} + \sum_{k=1}^{K} \delta^k \times \log\left(1 + \text{Name Share}_{csd}^{k}\right) + \varepsilon_{csd} \tag{2}$$

where $y$ denotes either the share of people affiliated to any denomination, or the share of Catholics, or the share of Protestants; $d$ corresponds to the two prepandemic decades of the religious censuses (1906 and 1916); $\alpha_c$ and $\alpha_{s\times d}$ are, respectively, county and state-by-decade fixed effects.[24] The term $K$ is the total number of names that occur in at least 0.3% of the overall sample.[25] To measure name shares, we include all babies born within ten years before each prepandemic census, hence we restrict the sample to cohorts between 1896 and 1916. Then, we aggregate these shares by decade to estimate equation (2).

We label the coefficient ($\delta^k$) as the *religiosity score* associated with name $k$; we interpret names with larger estimated religiosity scores ($\hat{\delta}^k$) as conveying a more-intense religious sentiment. Because model (2) 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 1, we report the estimated religiosity scores from model (2), where the outcome variable is the share of people affiliated with any religious organization. The figure shows that typically religious-sounding names, such as "Esther," "Paul," and "Grace," all feature positive and large estimated religiosity scores. Because our estimation method seeks to isolate *distinctively* religious

[23] 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.

[24] In one of our robustness checks, we compute an alternative measure of religiosity that does not include any fixed effect. The results are robust.

[25] We follow Fouka (2020) and restrict the number of names included in model (2) 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 in our overall sample is at least 0.3% and run checks around this threshold to assess the robustness of our results.

names, relatively common ones such as "Mary" or "John" end up not having large scores. A zero-religiosity score does not imply that the name carries no religious content. In the case of "Mary," for instance, its popularity during this period is such that religious and nonreligious people alike used it, thus preventing it from being associated with distinctively religious people. Moreover, we find that names with little connection to saints or biblical episodes are associated with negative religiosity scores. This is the case for Germanic names, such as "Edith", "George," and "Harold". 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.

### *3.1.2 A Yearly County-Level Measure of Religiosity*

From model (2), we obtain a set of estimated religiosity scores $\{\hat{\delta}^k\}_{k=1}^K$, 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 (2):

$$\hat{y}_{ct} = \sum_{k=1}^{K} \hat{\delta}^k \times \log\left(1 + \text{Name Share}_{ct}^k\right) \tag{3}$$

where $t$ denotes a cohort between 1900 and 1930. In addition, by considering religiosity scores associated with different denominations, we can construct 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 discuss a number of 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 the right column, instead, compare synthetic and measured religiosity in 1926.[26] 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 provides reassuring evidence that naming patterns capture meaningful variation in overall religiosity and further validates our measure.

One caveat of our religiosity measure is that we do not observe the religious affiliation of individuals. If we knew, for every person, their name and religion, we could infer the relative "Catholi-

[26]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, therefore, consider it less reliable and exclude it from our analysis.

cism" of a name by measuring how frequent that name occurs within the Catholic population, relative to the overall population.[27] This is not possible using U.S. data, as the census does not contain questions about individuals' religious faith. This information is, however, available in Canadian censuses, which explicitly report the religion of every registered individual (Abramitzky et al., 2020). We therefore construct alternative religiosity scores using the 1881, 1911, and 1921 Canadian censuses.[28] We focus on Protestantism and Catholicism as the two major denominations in Canada and, for each name, we calculate two separate scores expressing the intensity of Catholicism and Protestantism that each name conveys.[29] In Online Appendix A.6, we elaborate on how we construct this index. Additionally, following Abramitzky et al. (2016), we use biblical and saint names as an alternative name-based measure of religiosity.

Finally, we also use as another indicator of religiosity 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 the years 1906, 1916, and 1926.

### 3.2 Measuring Scientific Production

We measure local innovative activities 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 crosswalk provided by the National Bureau of Economic Research (Hall et al., 2001).[30]

[27] As explained above, in this paper, we compute the intensity of Catholicism or Protestantism conveyed by each name by estimating model (2) separately for the (Share of Catholics) or the (Share of Protestants) as reported in the Census of Religious Bodies.

[28] Unfortunately, the 1891 and 1901 individual census records no longer exist. The 1881 census covers the universe of the Canadian population, whereas the 1911 and 1921 censuses cover a 25% sample of the population.

[29] Each score is calculated as the excess frequency a given name appears within that denomination, relative to the overall population.

[30] 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." This is because most patents classified as "drugs" would also appear as "chemical," since each patent is usually assigned multiple US Patent Classification codes. All results for the pharmaceutical class hold also if we consider drug and chemical patents separately. An example of pharmaceutical patent is shown in Figure B.3. For historical consistency, we relabel the "computer and

In a second step, we build a measure of scientific inclination for a given county by looking at the share of individuals employed in STEM occupations. The underlying idea is that STEM occupations require science-based education. Thus, individuals in STEM occupations are plausibly more science-oriented than non-STEM ones. 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.[31] We also use these two classifications into STEM and non-STEM occupations when performing the individual-level analysis.

### 3.3 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 cover 1274 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

$$\text{Excess Deaths}_c = \frac{\frac{1}{2}\sum_{t=1918}^{1919} \text{Deaths}_{ct}}{\frac{1}{3}\sum_{t'=1915}^{1917} \text{Deaths}_{ct'}} \tag{4}$$

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 2 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 that WWI deaths are confounding our results. However, this does not seem to be the case. In Figure B.4, we show that there is no significant correlation between deaths from WWI and our measure of excess deaths. In Section 4, we show that our results are robust to controlling for a post-1918 time

communication" class as simply "communication."

[31]This second measure increases the comparability of the control group with STEM individuals. Table B.1 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.

indicator interacted with WWI-related deaths.

# 4 Empirical Results

In this section, we describe two main results. First, we show that exposure to the influenza pandemic led to an increase in both religiosity and innovation activity across counties. Second, we provide evidence of heterogeneous responses to the pandemic *within* counties. Specifically, we find that individuals from more religious backgrounds further embraced religion, while those from less religious backgrounds were more likely to choose a scientific occupation. In addition, we show that the pandemic widened the distance in religiosity between science-oriented individuals and the rest of the population, and that it led to the polarization of religiosity.

## 4.1 County-Level Evidence

In the first part of the analysis, we study the impact of the pandemic separately on religiosity and innovation at the county level. Our sample consists of a panel of U.S. counties observed over the 1900–1929 period at a 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:

$$y_{ct} = \alpha_c + \alpha_t + \delta \times (\text{Post}_t \times \text{Excess Deaths}_c) + \boldsymbol{x}'_{ct}\boldsymbol{\beta} + \varepsilon_{ct} \tag{5}$$

where the subscripts $c$ and $t$ denote county and year, respectively; $y_{ct}$ measures either religiosity or innovation activity; $\alpha_c$ and $\alpha_t$ are county and year fixed effects; $\text{Post}_t$ is an indicator variable equal to one if $t \geq 1918$ and zero otherwise; $\text{Excess Deaths}_c$ measures the intensity of the pandemic in terms of excess deaths, as explained in Section 3.3; and $\varepsilon_{ct}$ is the error term. In addition, in all regressions we control for the interaction between 1900-population and the post indicator $\boldsymbol{x}'_{ct}$. Standard errors are clustered at the county level. Our coefficient of interest, $\delta$, captures the impact of the pandemic on religiosity or innovation. 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:[32]

$$y_{ct} = \alpha_c + \alpha_t + \sum_{\tau \in \mathcal{T}} \delta^{\tau} \left[\mathbf{1}(\tau \leq t \leq \tau + 1) \times \text{Excess Deaths}_c\right] + \boldsymbol{x}'_{ct}\boldsymbol{\beta} + \varepsilon_{ct} \tag{6}$$

where $\mathcal{T} = \{1912, 1914, \ldots, 1928\}$ and $\mathbf{1}(\tau \leq t \leq \tau + 1)$ is an indicator variable that takes value one if $t$ is in the two-year window indexed by $\tau$, and zero otherwise.

[32] In the dynamic DiD specifications, we code time periods over two-year windows to reduce noisy fluctuations in estimated treatment effects and to improve efficiency by pooling observations.

Did the influenza spread randomly? We perform three main exercises to test this in the data. First, in Table B.2, we report the correlation between the intensity of the pandemic and a set of census-measured county covariates measured in 1910, the last census before the pandemic, accounting for population and state-level fixed effects.[33] Counties more exposed to the pandemic are observationally equivalent with respect to all variables, except for the share of men, and the share of foreigners. This is in line with the pandemic being comparatively more severe in urban areas and for men. Then, to rule out that these differences confound our analysis, we check whether control and treatment counties were on different trends before the shock, and we estimate an event study.

Formally, in Equation (6), this implies that the estimates of $\delta^{\tau}$ would not be statistically different from zero before the pandemic hit,[34] i.e., for all $\tau < 1918$. We find support for the parallel-trends assumption. However, our 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, in Tables B.3, B.4, B.5, and B.11, we control for the number of deaths in WWI in our regression model and show that the results remain robust.

#### 4.1.1 *The Effect of the Influenza Pandemic on Religiosity*

Table 2 displays the DiD estimates obtained using religiosity as dependent variable. The estimates reported in column (1) show that counties comparatively more exposed to the pandemic experienced an increase in overall religiosity. A one-standard deviation increase in excess deaths led to a 0.11 standard deviations increase in religiosity at the county level. 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 7%. In columns (2) and (3), we explore possible heterogeneous effects of the pandemic on Catholics and Protestants and our results seem to be stronger for Catholicism. In columns (4)–(6), we weight regressions by county-level population to make sure that our findings are not driven by

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

[34]Since the setting is not staggered—because the pandemic hit each county in the same period—models (5) and (6) 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.

low-population counties.[35] The results hold.

In Figure 3, we report the coefficients of the interactions between the treatment variable and the biennial dummies for overall religiosity. The flexible specification supports the patterns observed in the DiD analysis and confirms the absence of pretrends. In addition, we observe that the increase in religiosity seems to persist over the decade after the pandemic. This is in line with the literature documenting a substantial persistence of religiosity (e.g., Squicciarini, 2020).

In Table B.3, we show that our results hold through a series of robustness checks. First, we code the treatment as a binary variable equal to one if the baseline treatment is above its median, and zero otherwise (column 2). Second, we explicitly control for mortality due to WW1 in column (3). One concern related to our religiosity measure could be that firstborns are often named after a deceased grandparent and thus their names 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 simply reflect a mechanical effect. Column (4) reports estimates 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. In addition, 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. Finally, 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 effect of the pandemic on name-based religiosity would reflect movers' religiosity and their fertility. To deal with 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. Tables B.4 and B.5 reproduce the robustness checks of Table B.3 for Catholics and Protestants, separately. The results of the respective baseline specifications are confirmed throughout.

In a second step, we test whether the results are robust to alternative ways of constructing our religiosity measure. First, in Table B.6 we report the baseline result, but using religiosity scores

[35] We use 1900 population, but results are similar when using 1910 population.

estimated through equation (2) *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. In Table B.7, we show that our findings are qualitatively unchanged under different frequency thresholds. Finally, a possible concern could be that the results are capturing 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.8, we regress a set of indices reflecting the concentration of the name distribution against our baseline treatment and find no evidence of such mechanism.

In the third set of robustness checks, we perform our analysis using alternative indicators of religiosity. First, we validate the distinction between Catholic and Protestant names by using the Canadian census. The advantage of this census is that, unlike in the United States, individuals were explicitly asked to report their religious affiliation. Columns (1) and (2) of Table B.9 replicate the baseline results using the Canada-based religiosity scores assigned to the names of newborns in the United States—these confirm the increase in the intensity of Catholicism in counties that were more exposed to the pandemic. Since the near-universe of the Canadian population in this period reported being Catholic or Protestant, religiosity scores can measure only the intensity of Catholicism relative to Protestantism, and vice versa.[36] Second, in columns (3)–(5) of Table B.9, we use biblical and saint names as an alternative name-based measure of religiosity, following Abramitzky et al. (2016). We find that the pandemic exerted a positive impact on the share of either biblical or saint names. Interestingly, this effect is stronger for saints' names—a result in line with previous findings on Catholicism and Protestantism.[37]

Finally, we study the effect of the pandemic on religiosity by looking directly at the Census of Religious Bodies. This has the advantage of including the U.S. population across different age

[36]In the Canadian census, fewer than 1% report either a religious affiliation different than Catholic or Protestant or no religious affiliation at all. For details on the construction of the Canadian-census religiosity scores, see Appendix Section A.6.

[37]Perl and Wiggins (2004) argue that historically Catholic parents tended to give newborns the name of a saint, required for the child's baptism. Conversely, Protestants—who stress the centrality of the Bible but do not recognize the cult of saints—tended to give biblical names. In addition, in Figure B.5, we show that the county-level share of Biblical and Saints names, computed using data from Abramitzky et al. (2016), is strongly and positively correlated with the religiosity measure constructed using our data-driven approach.

groups—not just individuals who had children a decade before and a decade after the pandemic. On the other hand, this measure has two 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. Bearing this in mind, we show in Table B.10 that our baseline results are confirmed when using the census-measured share of people affiliated with religious denominations as our outcome variable.[38]

Throughout different specifications and indicators, we find that the pandemic had a positive effect on religiosity. 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). In addition, the heterogeneity between Catholics and Protestants is in line with the psychology literature studying the impact of mental distress across confessions (Pargament, 2002), as well as with a recent study on the COVID-19 pandemic, showing that the increase in Google searches for Catholic prayers was substantially higher than for Protestant ones (Bentzen, 2021).

#### *4.1.2 The Effect of the Influenza Pandemic on Innovation*

We now turn to study how the influenza pandemic impacted innovation. We show that the pandemic had a positive impact on overall innovation (measured by the total number of patents granted during the period), driven mainly by an increase in patents in pharmaceuticals.

In column (1) of Table 3, we report the estimated impact of the influenza shock on the 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 an 0.21 standard deviations 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 19% in the number of patents granted by county-year. The results hold when weighting regressions by county-level population (column 7). Figure 4 displays the effect 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 (6). The coefficients show 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

[38]These coefficients are estimated using decade-level data. This can partly explain why the magnitudes seem particularly high, compared to those obtained using yearly-level data (as in Table 2).

shock.

We also investigate 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 filed in each county-year. Columns (2)–(6) and (8)–(12) of Table 3 show the results of this exercise. For each field, we report the estimated DiD coefficients. Columns (2)–(6) report the unweighted baseline estimates, while columns (8)–(12) report the observations weighted by county population in 1900. 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 11% in pharmaceutical patents, compared to one at the 25th percentile.

In Table B.11, we report a number of robustness checks, separately for the total number of patents irrespective of their field (columns 1–4) and for those in pharmaceuticals (columns 5–9). Columns (1) and (5) report the baseline estimates for comparison. 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 posttreatment 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 we add one to avoid dropping zeros. In Tables B.12 and B.13, we show that alternative transformations of the dependent variable yield quantitatively similar results, respectively for all patents and for patents in pharmaceuticals. In particular, while in the baseline regressions we control for the total number of patents—to show that the influenza shock altered the direction of innovation in favor of pharmaceuticals—in columns (7) and (8) of Table B.13, we use the share of patents in pharmaceuticals as our dependent variable. These exercises yield consistent results.

In Table B.14 we show that the positive impact of the influenza shock on innovation was driven both by the higher productivity of existing inventors (intensive margin) and by an increase in the number of inventors (extensive margin).[39] In Table B.14, the dependent variable is the number of patents per inventor (columns 1–3) and the total number of inventors (columns 4–6). We document a large increase in the productivity and number of inventors active in any field (columns 1 and 4), as well as in pharmaceuticals only (columns 2–3, 5–6), even when controlling for average productivity and for the overall number of active inventors.

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 in subsequent years. To address this concern, we use the text-based measures of quality developed by Kelly et al. (2021).[40] Table B.15 shows the results. Column (1) uses the measure of average patent quality in all sectors and shows no significant effect of the pandemic. In column (4), instead, we find that the average quality of pharmaceuticals patents increases. We then focus on "breakthrough" patents. Specifically, we assign to every patent a dummy equal to one if the patent's quality is in the top 20% of the distribution, and zero otherwise. We find that the number and share of breakthrough patents substantially increase in counties hit harder by the pandemic, both in all sectors and in only pharmaceuticals (columns 2–3, 5, and 7). In addition, in column (6) we show that the number of breakthrough patents in pharmaceuticals grows more than the average number in other sectors.

Another concern is that patents may be an imperfect measure for innovation and scientific attitudes, since not all innovation is patented (Moser, 2005). We complement our analysis by using the share of people employed in STEM occupations as an alternative indicator. 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, and more trust in, science (Deming and Noray, 2018; Bianchi and Giorcelli, 2020).

We start by running the same specification as in models (5) and (6) using as dependent variables 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). Columns (1) and (2) of Table 4 show an increase in the share of workers in STEM occupations in counties more severely hit by the pandemic. A one standard deviation increase in excess deaths is

[39] To disambiguate among homonym inventors, we use the sample of inventors linked to the U.S. full-count census developed by Bazzi et al. (2022).

[40] 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 high-quality patents based on the textual similarity of a given patent to previous and subsequent work. High-quality patents are those that are distinct from previous work, but are similar to subsequent innovations.

associated with a 0.86-standard deviations 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 29% increase in the share of individuals in STEM.[41]

To better understand what drives the change in occupational shares, we use individual-level data on occupations. 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 ten years later compared to older cohorts, in areas that were comparatively more exposed to the pandemic.[42] We estimate the following linear probability model, where we define as treated individuals aged 18 to 25 in 1918:

$$\text{STEM}_{hct} = \alpha_c + \alpha_t + \delta \times \left(\text{Excess Deaths}_c \times \text{Young}_h\right) + \boldsymbol{x}'_h\boldsymbol{\beta} + \varepsilon_{hct} \tag{7}$$

where $\alpha_c$ and $\alpha_t$ respectively denote county and cohort fixed effects, $\text{STEM}_{ict}$ is a dummy variable equal to one if head of household $h$ is employed in a STEM occupation, and zero otherwise; $\boldsymbol{x}_h$ includes urban status and race. The categorical variable $\text{Young}_h$ is equal to one if individual $h$ is between 18 and 25 in 1918, and zero otherwise. Our coefficient of interest is $\delta$, which measures the causal effect of the pandemic on the probability of being employed in a STEM occupation.

Columns (3)–(4) in Table 4 report the results: in counties more exposed to the pandemic, young individuals were significantly more likely to sort into STEM occupations.[43] 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 in similar ways, but young cohorts were the only ones in the process of choosing an occupation. The second is that the pandemic may have specifically affected the attitudes and preferences of individuals in their *impressionable years* (i.e., the young cohorts), and thus the differential occupation choice reflects a change in attitudes occurring only for these cohorts.[44] Next, we replicate the analysis of columns (1)–(4), using non-STEM high-skilled individuals as the comparison group. In particular, in columns (5)–(6), we use as dependent variable the share of STEM individuals relative to the number of people employed

[41]These coefficients are computed using decade-level data. This explains why the beta coefficients are particularly high, compared to those obtained using yearly-level data, as in Table 3.

[42]To construct the sample, we use the cross-section of all individuals in the 1930 full-count census. We drop all individuals born after 1905, as they may have been too young to have already selected an occupation, and we restrict the sample to the working population. We drop individuals who were in prison, retired, or reported no occupation.

[43]In the baseline specification, a young individual is someone between 18 and 25 years old in 1918; in Table B.16, we extend the sample to those aged 18 to 30 in 1918, and the results hold.

[44]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).

in high-skilled occupations and in columns (7)-(8), we only include individuals in STEM and other high-skilled in the sample. The results are quantitatively similar.

#### 4.1.3 *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:

$$\begin{aligned} y_{ct} =& \alpha_c + \alpha_t + \delta_1 \times (\text{Excess Deaths}_c \times \text{Post}_t) + \delta_2 \times \text{Religiosity}_{ct} + \\ &+ \delta_3 \times \left(\text{Excess Deaths}_c \times \text{Post}_t \times \text{Religiosity}_{ct}\right) + \boldsymbol{x}'_{ct}\boldsymbol{\beta} + \varepsilon_{ct} \end{aligned} \tag{8}$$

where $y_{ct}$ is the log(1+ total patents),[45] and (Religiosity$_{ct}$) is the religiosity measure described in Section 3.1. The coefficient $\delta_1$ measures the impact of the pandemic on innovation, $\delta_2$ captures the correlation between the outcome and religiosity before the pandemic, and the term $\delta_3$—alongside $\delta_2$—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 county population in 1900 and a posttreatment indicator.

In Table B.17, we report the estimates of model (8). The results suggest that counties that were comparatively more affected by the pandemic experienced a joint increase in religiosity and innovation.

Interestingly, as a consequence of this contemporaneous increase in religiosity and science, their relationship shifts from negative to positive—as shown in Figure B.6. In the period before the shock, there was a negative correlation between the intensity of innovation activity (measured as the number of patents per 10,000 individuals) and religiosity at the county level.[46] This is in line with contemporary evidence reported by Bénabou et al. (2015). In the lower panel, we show that, in the period after the pandemic, religiosity and science became positively correlated. In Section 4.2, we use individual-level data to uncover the possible mechanisms underlying these results.

[45]Total patents are normalized by county population in 1900, as in Bénabou et al. (2022).

[46]In Figure ??, we document similar patterns separately for Catholicism and Protestantism.

## 4.2 Mechanisms: Individual-Level Analysis

After observing a contemporaneous increase in religiosity and innovation, two questions naturally arise. 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 are heads of household in the 1930 census.[47]

First, we show that the pandemic led to an increase in the religiosity of individuals who came from initially more religious backgrounds while individuals from less religious backgrounds were more likely to select STEM occupations. Second, we show that STEM individuals, who were less religious before the pandemic, become even less religious compared to 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 heterogeneities 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.

### *4.2.1 Turning to Religion or Turning to Science*

We start by studying whether preexisting differences in individuals' religious background could have led to a heterogeneous response to the influenza shock. The full-count census data, on top of covering the universe of the U.S. population, have the advantage of being deanonymized. This allows 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, the religious background of an individual 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

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

in a STEM occupation, and zero otherwise.[48]

We estimate two triple-differences specifications, one for religiosity and one for the likelihood of selecting a STEM occupation:

$$\begin{aligned}\text{Religiosity}_{jict} &= \alpha_{c\times t} + \alpha_{c\times B} + \alpha_{B\times t} + \\ &+ \delta_1 \times \left(\text{Excess Deaths}_c \times \text{Post}_t \times \text{High Religious Background}_i\right) + \boldsymbol{x}_i'\boldsymbol{\beta} + \varepsilon_{jict}\end{aligned} \tag{9}$$

where $j$ represents a child, $i$ denotes the household head, $c$ and $t$ are respectively county of residence and child birth-year; and

$$\begin{aligned}\text{STEM}_{ict} &= \alpha_{c\times t} + \alpha_{c\times B} + \alpha_{B\times t} + \\ &\delta_2 \times \left(\text{Excess Deaths}_c \times \text{Young}_t \times \text{High Religious Background}_i\right) + \boldsymbol{x}_i'\boldsymbol{\beta} + \varepsilon_{ict}\end{aligned} \tag{10}$$

where $i$ denotes a head of household, residing in county $c$, born in year $t$.

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 term "High Religious Background" is a categorical variable returning the value one if the religiosity score of the household head's name is in the top 20% of the religiosity distribution, and zero otherwise. We estimate model (9) on the sample of children born between 1900 and 1929. The dependent variable is the religiosity score associated with the name of child $j$. Children are weighted by the inverse of the number of children in each household. In model (10), the sample is composed of heads of households, observed once in the 1930 census. 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. The coefficients $\delta_1$ and $\delta_2$ 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.[49]

Table 5 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, a dummy "Post" equal to one if a child is born after the pandemic, and the religious background of the household head. In columns (4)–(6), the outcome variable is a dummy equal to one if the household

[48] 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, due to data limitations, this is not possible, 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 any direct information on, or linking to, their parents.

[49] While in model (9) the treatment is at the level of the birth year of the children of the household head (i.e., *Post* refers to a child born after 1918), in model (10) the treatment is at the level of the cohort of the household head (i.e., *Young* refers to a household head who turned 25 years old after 1918).

head is employed in a STEM occupation. Our main variable of interest is the interaction between the excess-deaths measure, a dummy "Young" equal to one if a given individual was between 18 and 25 in 1918, and their religious background. [50] All regressions include year-by-county fixed effects, which also absorb the effects of the interaction between excess deaths and the birth year, as well as county-by-background and background-by-year fixed effects.

We find that individuals originating 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).[51] 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). Evidence in Table 5 suggests that an individual's religious background affected 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.

#### 4.2.2 *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.

In Appendix Table B.18, we show the average religiosity of STEM (column 2) and non-STEM (column 1) individuals before the pandemic, as well as their differences (columns 3–4).[52] STEM individuals are less religious than non-STEM ones. This holds both unconditionally (column 3), and when we condition on county fixed effects, cohort fixed effects, and household-level controls (column 4).

We now turn to study the impact of the pandemic on religiosity for these two types of individuals. We estimate the following triple-differences model:

$$\text{Religiosity}_{jict} = \alpha_{c\times\text{STEM}} + \alpha_{t\times\text{STEM}} + \alpha_{c\times t} + \delta \times (\text{Excess Deaths}_c \times \text{STEM}_i \times \text{Post}_t) + \boldsymbol{x}_i'\boldsymbol{\beta} + \varepsilon_{jict} \quad (11)$$

where $j$ denotes a child, $i$ denotes the household head, $c$ and $t$ are respectively county and birth-

[50] In Table 5 columns (1)–(3) we observe multiple realizations—one for each child—of a head of household's religious attitude. In columns (4)–(6), on the other hand, we observe a cross-section of individuals whose scientific attitudes—which we proxy with their occupational choices—are observed only once.

[51] 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).

[52] To construct these variables, we consider only children born before 1918, and we take the within-household average religiosity.

year of the child; $\text{Post}_t$ is a dummy variable taking the value one if child $j$ is born after 1918, and zero otherwise; $\text{STEM}_i$ is an indicator variable that takes the value one if the household head is employed in a STEM occupation, and zero otherwise; and $\boldsymbol{x}$ includes urban status and race of the household head. The coefficient $\delta$ compares STEM and non-STEM individuals, 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 6 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 become less religious than non-STEM ones in counties more exposed to the influenza shock (columns 1 and 4). This pattern is stronger for Catholics (columns 2 and 5) than for Protestants.

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

#### *4.2.3 Polarization of Religious Beliefs*

In this section, we analyze the impact of the influenza pandemic on the distribution of religiosity within counties. Precisely, we estimate heterogeneous treatment effects of the pandemic across the initial distribution of background religiosity.

To study this question, we discretize the distribution of background religiosity into quintiles, which we label $Q^{\text{BR}}$, and we estimate the following model:

$$\begin{aligned} \text{Religiosity}_{jict} &= \alpha_{c\times t} + \alpha_{c\times Q} + \alpha_{Q\times t} + \\ &+ \sum_{\ell=1}^{5} \delta^{\ell} \times \left[\text{Excess Deaths}_c \times \text{Post}_t \times 1\left(Q_i^{\text{BR}} = \ell\right)\right] + \boldsymbol{x}_i'\boldsymbol{\beta} + \varepsilon_{ict} \end{aligned} \tag{12}$$

where $j$ denotes a child, $i$ denotes the household head, $c$ and $t$ are respectively county and child birth-year; Equation (12) 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 $1\left(Q_i^{\text{BR}} = \ell\right)$ is a dummy variable that takes the value one if household head's background religiosity is in the $\ell$-th quintile, and zero otherwise. If the pandemic caused an increase in polarization of religiosity, the set of coefficients $\{\delta^{\ell}\}_{\ell=1}^{5}$ in equation (12) would be monotonically increasing in $\ell$. On the other hand, a decreasing sequence of coefficients would provide evidence that the pandemic led to a convergence of religiosity. In model (12), 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.

In Figure 5, we report the set of $\{\delta^\ell\}$ coefficients by religious denominations. We normalize the third quintile as the baseline category. The figure provides evidence in favor 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 suggests that, within the same county, individuals from different religious backgrounds become even more distant in terms of their religiosity, increasing the polarization of religiosity within society. In Table B.19 we report the results of the corresponding regressions.

Taken together, these three individual-level exercises help us understand the contemporaneous increase in both 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 on their prepandemic scientific orientation. Thus, while a county may have become both more religious *and* more innovative, individuals seemed to turn either to religion *or* to science, leading to within-county polarization of religiosity.

## 5 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 production of innovation across U.S. counties and, as a result of the shock, the same counties became both more religious and more innovative; (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 innovation activity are related to the intensity of the shock. Second, our event-study analysis shows the absence of pretrends, suggesting that religiosity and innovation 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. Taken together, the empirical evidence, supported by the 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 when 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, patents could be an imperfect measure of scientific progress (Moser, 2005). To address this concern, we show that our findings hold when using the share of individuals in scientific occupations as an alternative proxy.

One puzzle emerging when looking at the aggregate patterns is whether these results are driven by individuals becoming both more religious and more 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 further embrace religion, while those from less religious backgrounds are more likely to choose a scientific occupation. This 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: people in scientific occupations, already less religious than the rest of the population, moved further away from religion. Finally, the pandemic increased the polarization of religiosity in the population: 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 are not able to fully rule this out (and it goes beyond the scope of our paper), we read our evidence as being in favor of 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 to 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.

Finally, one 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.[53] However, since we know that science-oriented people are less religious than the overall population (Appendix Table B.18), it is plausible to assume that religious background and scientific background are similarly negatively correlated. 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.

## 6 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 innovative. 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.

[53] 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; this exercise would require tracking individuals across several census waves.

Our analysis 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 one that witnessed the 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.[54] 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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[54] 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 1: Summary Statistics

| | (1) Mean | (2) Std. Dev | (3) Min | (4) Max | (5) Counties |
|---|---|---|---|---|---|
| **Panel A. Mortality** | | | | | |
| Flu excess deaths (%) | 1.134 | .148 | .853 | 1.779 | 1220 |
| WW1 deaths | 20.864 | 85.414 | .5 | 2414 | 1220 |
| Net Flu Excess Deaths (%) | 1.099 | .159 | -.897 | 1.714 | 1220 |
| **Panel B. County Demographics** | | | | | |
| Population | 36.905 | 79.468 | .076 | 1298.405 | 1217 |
| Area | 218.356 | 283.698 | 0 | 5205.831 | 1217 |
| Share of Whites | .936 | .126 | .311 | 1 | 1217 |
| Share of African Americans | .058 | .127 | 0 | .689 | 1217 |
| Share of Foreign Born | .119 | .112 | 0 | .498 | 1217 |
| Share of Illiterates | .045 | .047 | .001 | .264 | 1217 |
| Income per Capita | 833.501 | 24.243 | 746.105 | 913.328 | 1217 |
| **Panel C. Religious Affiliations** | | | | | |
| Total Affiliated | 21.635 | 62.262 | .148 | 982.279 | 1219 |
| Catholics | 9.415 | 36.48 | 0 | 589.856 | 1219 |
| Protestants | 10.386 | 18.697 | .056 | 309.439 | 1219 |
| **Panel D. Innovation Activity** | | | | | |
| All | 106.456 | 409.376 | 0 | 5598.142 | 1220 |
| Pharmaceuticals | 13.785 | 54.099 | 0 | 710.225 | 1220 |
| Communications | 3.132 | 14.761 | 0 | 292.194 | 1220 |
| Electrical | 11.224 | 52.666 | 0 | 1039.469 | 1220 |
| Mechanical | 37.472 | 142.788 | 0 | 2026.215 | 1220 |
| Other | 40.843 | 152.003 | 0 | 1872.377 | 1220 |

*Notes:* This table displays the mean, standard deviation, minimum, maximum, and total number of counties of the main variables. Data are measured at the county level. Panel A and B report data from the 1910 census. Data in Panels C and D are at decade level. Hence, for instance, column (1) of Panel C "Total Affiliated" reports the average number of individuals affiliated with any denomination over the period 1900-1930. Column (1) of Panel D "All" reports the average number of patents in any class in each decade between 1900 and 1930. "Excess deaths" is defined as the ratio between total deaths during the pandemic, and total deaths in the three years before. County demographics are measured through the IPUMS full-count census (Ruggles et al., 2021). Income per capita is measured through occupational income scores based on the 1950 Census. Religious affiliation data are from the Census of Religious Bodies. Patent data are from Berkes (2018). All variables are crosswalked to 1920 borders.

Table 2: The Impact of the Influenza on Religiosity

| | Unweighted | | | Weighted | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | All | Catholics | Protestants | All | Catholics | Protestants |
| Excess Deaths × Post | 0.007** | 0.009*** | 0.006** | 0.009** | 0.009** | 0.004 |
| | (0.003) | (0.003) | (0.003) | (0.004) | (0.004) | (0.005) |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of Counties | 1201 | 1201 | 1201 | 1201 | 1201 | 1201 |
| Observations | 36030 | 36030 | 36030 | 36030 | 36030 | 36030 |
| $R^2$ | 0.450 | 0.306 | 0.471 | 0.606 | 0.498 | 0.640 |
| Std. Beta Coef. | 0.109 | 0.184 | 0.100 | 0.191 | 0.242 | 0.084 |

*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 yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic—i.e., over the years 1918 to 1929—or zero otherwise. The baseline treatment "Excess Deaths" is defined in Equation (4). Columns (1)–(3) report the baseline results; in columns (4)–(6) counties are weighted by population in 1900, i.e., at the beginning of the sample period. 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 year fixed effects and the interaction between population in 1900 and a post-treatment indicator. Standard errors, clustered at the county level, are reported in parentheses.

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

Table 3: The Impact of the Influenza on the Volume and Direction of Innovation

| | Unweighted | | | | | | Weighted | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| | All Patents | Pharmaceuticals | Communication | Electrical | Mechanical | Other | All Patents | Pharmaceuticals | Communication | Electrical | Mechanical | Other |
| Excess Deaths × Post | 0.503*** | 0.091*** | 0.000 | 0.022 | 0.014 | 0.018 | 0.695*** | 0.265*** | -0.068 | 0.050 | -0.009 | -0.014 |
| | (0.064) | (0.033) | (0.021) | (0.027) | (0.023) | (0.022) | (0.166) | (0.094) | (0.198) | (0.106) | (0.059) | (0.055) |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| All Patents | No | Yes | Yes | Yes | Yes | Yes | No | Yes | Yes | Yes | Yes | Yes |
| Number of Counties | 1220 | 1220 | 1220 | 1220 | 1220 | 1220 | 1220 | 1220 | 1220 | 1220 | 1220 | 1220 |
| Observations | 37820 | 37820 | 37820 | 37820 | 37820 | 37820 | 37820 | 37820 | 37820 | 37820 | 37820 | 37820 |
| $R^2$ | 0.832 | 0.836 | 0.717 | 0.819 | 0.925 | 0.935 | 0.950 | 0.950 | 0.868 | 0.939 | 0.979 | 0.983 |
| Std. Beta Coef. | 0.211 | 0.066 | 0.000 | 0.018 | 0.008 | 0.009 | 0.193 | 0.098 | -0.036 | 0.019 | -0.003 | -0.004 |

*Notes:* This table displays the impact of exposure to the Great Influenza Pandemic on the level and direction of 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 the years 1918 to 1929—or zero otherwise. The baseline treatment "Excess Deaths" is defined in Equation (4). Columns (1)–(6) display the baseline results; in columns (7)–(12), counties are weighted by their population in 1900, i.e., at the beginning of the sample period. In columns (1) and (7), the dependent variable is the (log) total number of patents granted. In the other columns, the dependent variable is the (log) number of patents granted in each column-field, controlling for the overall (log) number of patents. In all models, we take ln(1 + Patents) as the dependent variable to ensure that we do not drop counties without patents. Columns (1) and (7) estimate the impact of the pandemic on the level of innovation, while columns (2)–(6) and (8)–(12) display this on the direction of innovation because we control for the total number of patents. Regressions include county and year fixed effects and the interaction between population in 1900 and a post-treatment indicator. Standard errors, clustered at the county level, are reported in parentheses.

$^{*}$: $p < 0.10$, $^{**}$: $p < 0.05$, $^{***}$: $p < 0.01$

Table 4: Impact of the Influenza on Occupational Choice

| | Entire Population | | | | Skilled Population | | | |
|---|---|---|---|---|---|---|---|---|
| | STEM / Population | | Dummy = 1 if STEM | | STEM / Population | | Dummy = 1 if STEM | |
| | (1) Unweighted | (2) Weighted | (3) No Controls | (4) Controls | (5) Unweighted | (6) Weighted | (7) No Controls | (8) Controls |
| Excess Deaths × Post | 0.004*** | 0.007*** | | | 0.049*** | 0.080*** | | |
| | (0.000) | (0.002) | | | (0.007) | (0.017) | | |
| Excess Deaths × Young | | | 0.003*** | 0.004*** | | | 0.033*** | 0.031*** |
| | | | (0.001) | (0.001) | | | (0.010) | (0.009) |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Decade FE | Yes | Yes | – | – | Yes | Yes | – | – |
| Cohort FE | – | – | Yes | Yes | – | – | Yes | Yes |
| Household Controls | – | – | No | Yes | – | – | No | Yes |
| Number of Counties | 1218 | 1218 | 1217 | 1217 | 1216 | 1216 | 1217 | 1217 |
| Observations | 4868 | 4868 | 30679634 | 30679634 | 4864 | 4864 | 4285423 | 4285423 |
| $R^2$ | 0.656 | 0.717 | 0.002 | 0.002 | 0.584 | 0.702 | 0.005 | 0.010 |
| Std. Beta Coef. | 0.799 | 1.079 | 0.021 | 0.021 | 0.845 | 1.222 | 0.076 | 0.071 |

*Notes:* This table displays the effect of the pandemic on occupational choice. In columns (1–2) and (5–6), the unit of observation is a county, observed at decade frequency between 1900 and 1930. In columns (3–4) and (7–8), the unit of observation is an individual, observed once in the 1930 population census. In columns (1–2) and (5–6), the treatment interacts a "Post" variable equal to one for each census decade after the pandemic, or zero otherwise, with the standard "Excess Deaths" measure defined in Equation (4). In columns (1–2), the dependent variable is the (log 1+) share of people employed in STEM occupations, as a share of the population in 1910. In columns (5–6) the share is computed relative to the number of people employed in skilled occupations in 1910. The lists of STEM occupations and of high-skilled occupations are reported in Table B.1 In columns (3–4) and (7–8), the treatment is an interaction between a dummy variable equal to one if the person is employed in a STEM occupation and zero otherwise, and an indicator variable returning value one for all those aged 25 or less at the time of the inception of the pandemic. In columns (3–4) the sample includes the entire population; in columns (7–8) we only include individuals employed in skilled occupations. Columns (1) and (5) report the baseline estimates; in columns (2) and (6) we weigh counties by their 1900 population. In column (4) and (8) we control for race and urban status of the head of household. Regressions in columns (1–2) and (5–6) include county and year fixed effects and the interaction between population in 1900 and a post-treatment indicator; regressions in columns (3–4) and (7–8) include county and cohort fixed effects. Standard errors are clustered at the county level and are reported in parentheses.

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

Table 5: 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 | 0.066*** | 0.037*** | 0.020 | | | |
| | (0.016) | (0.014) | (0.013) | | | |
| Excess Deaths × Young × High Religious Background | | | | -0.003* | -0.004** | -0.003** |
| | | | | (0.001) | (0.001) | (0.001) |
| 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 | 1217 | 1217 | 1217 | 1217 | 1217 | 1217 |
| Observations | 7641683 | 7641686 | 7641678 | 13569024 | 13569024 | 13569024 |
| $R^2$ | 0.029 | 0.023 | 0.026 | 0.006 | 0.007 | 0.006 |
| Std. Beta Coef. | 0.037 | 0.022 | 0.013 | -0.010 | -0.012 | -0.013 |

*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 the years 1918–1929—or zero for those born before the pandemic—i.e., before 1918. The baseline treatment "Excess Deaths" is defined in Equation (4). "STEM" is an indicator variable returning value one if an individual is employed in a STEM occupation—as defined in Table B.1—or zero otherwise. "Young" is an indicator variable equal to one if an individual is younger than 25 years old 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 20% 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.

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

Table 6: Effect of the Influenza on Individual Religiosity: STEM and Non-STEM

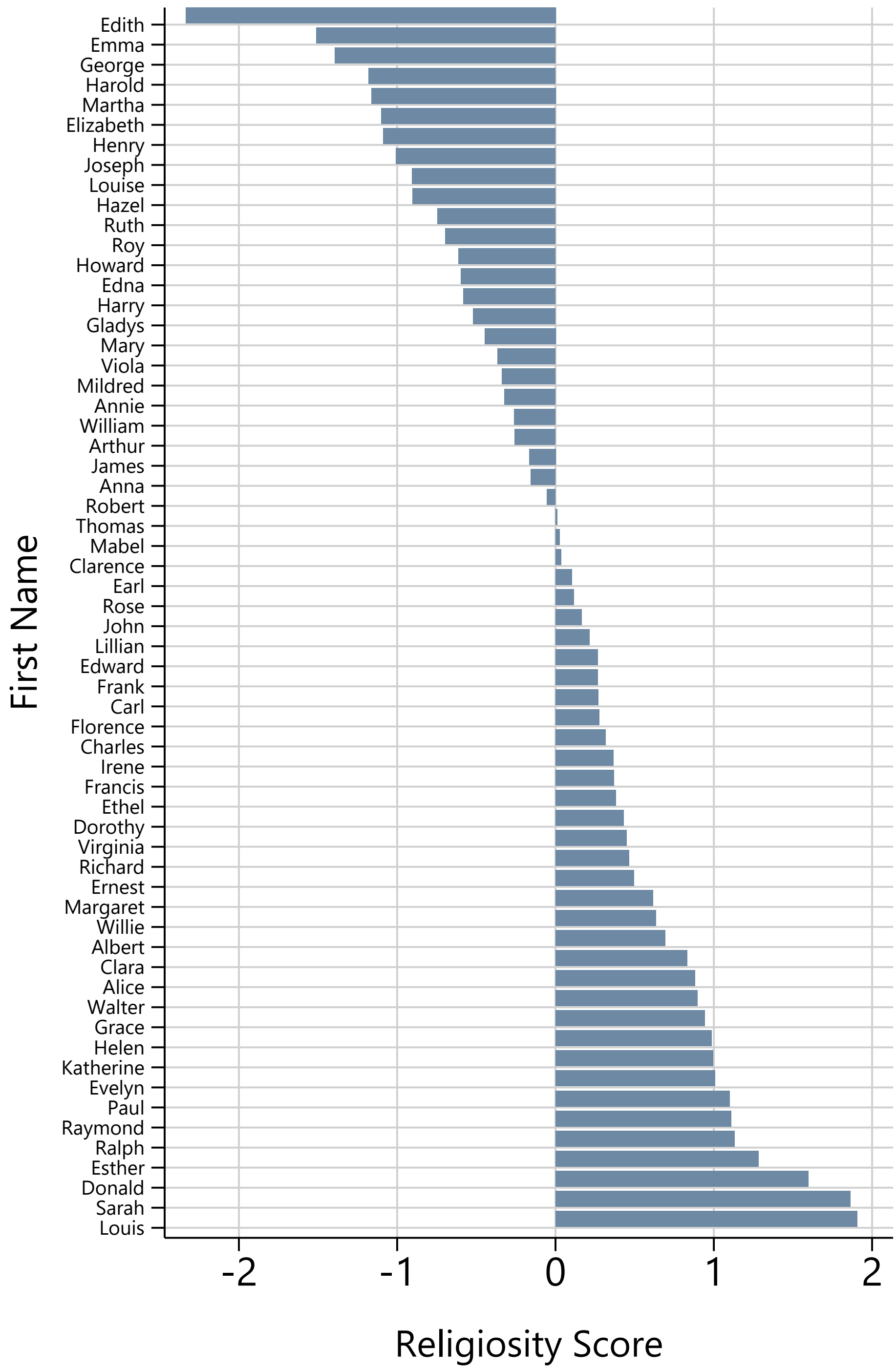
| | Entire Population | | | Skilled Population | | |
|---|---|---|---|---|---|---|
| | (1) All | (2) Catholics | (3) Protestants | (4) All | (5) Catholics | (6) Protestants |
| Excess Deaths × Post × STEM | -0.107** | -0.084*** | -0.030 | -0.081* | -0.060* | -0.011 |
| | (0.048) | (0.032) | (0.040) | (0.045) | (0.036) | (0.040) |
| 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 |
| Sample | All | All | All | Skilled | Skilled | Skilled |
| Number of Counties | 1217 | 1217 | 1217 | 1217 | 1217 | 1217 |
| Observations | 15096725 | 15096725 | 15096725 | 2275587 | 2275587 | 2275587 |
| $R^2$ | 0.009 | 0.006 | 0.008 | 0.024 | 0.022 | 0.022 |
| Std. Beta Coef. | -0.012 | -0.011 | -0.004 | -0.023 | -0.020 | -0.004 |

*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 (4). "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. In columns (1)–(3), the estimation sample includes all individuals; in columns (4)–(6) we include only those employed in skilled occupations, which we enumerate in table B.1. Standard errors are clustered at the county level and are reported in parentheses.

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

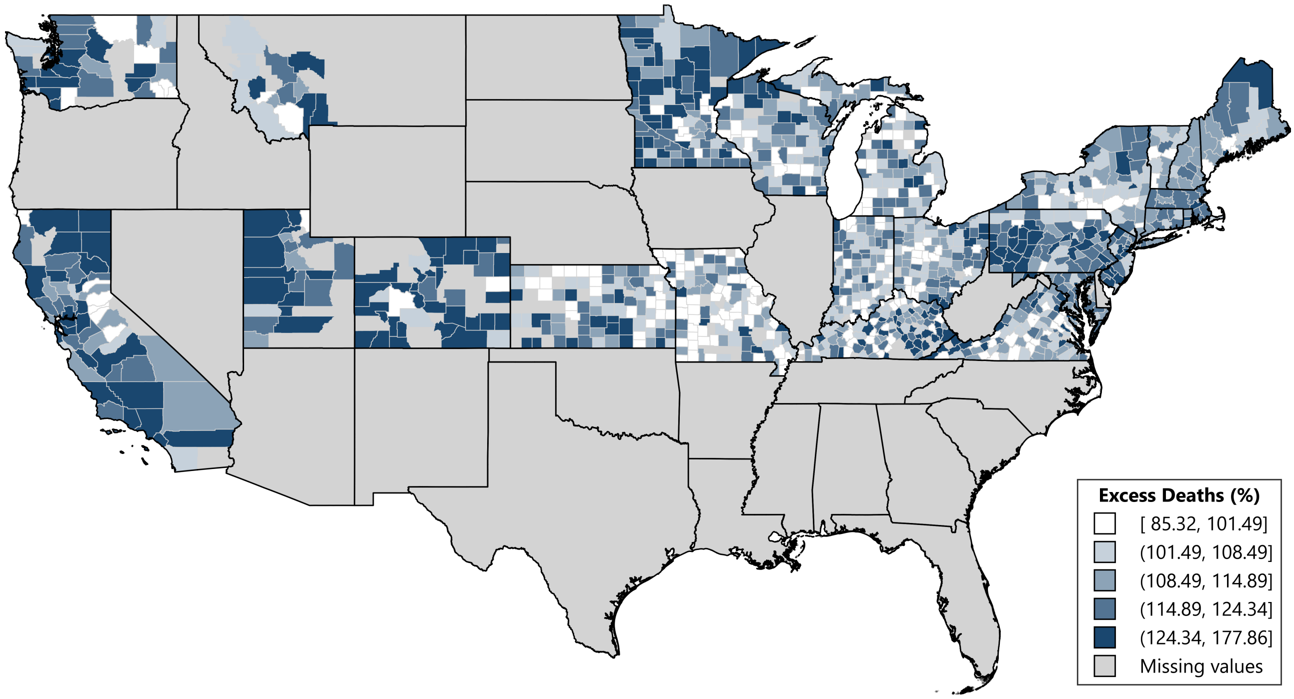
## Figures

FIGURE 1: ESTIMATED NAMES RELIGIOSITY SCORES



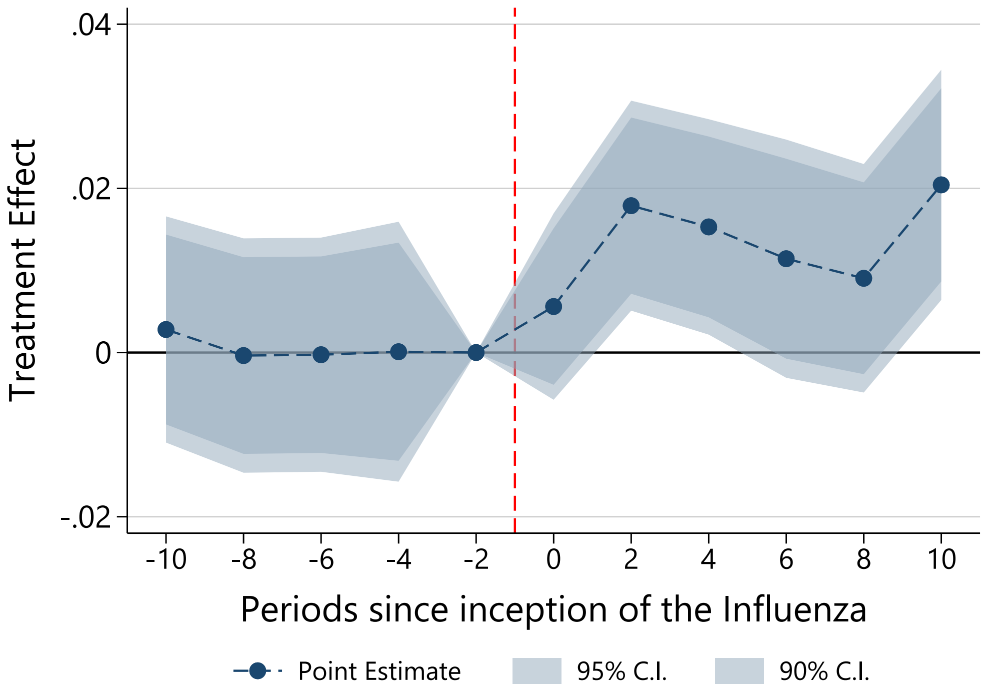

*Notes:* This figure displays the religiosity scores estimated from model (2). Regressions are based on data from the 1906–1916 Censuses of Religious Bodies; they include individuals born between 1896 and 1916. We estimate religiosity scores for names appearing in at least 0.3% of the overall sample. We conflate variations of a single name together—e.g., Anne and Anna—but keep endearments separate—e.g., Anna and Annie. Coefficients are reported in increasing order. In Figure B.1, we report religiosity scores split by confessions.

Figure 2: Spatial Distribution of Excess Mortality During the Great Influenza Pandemic



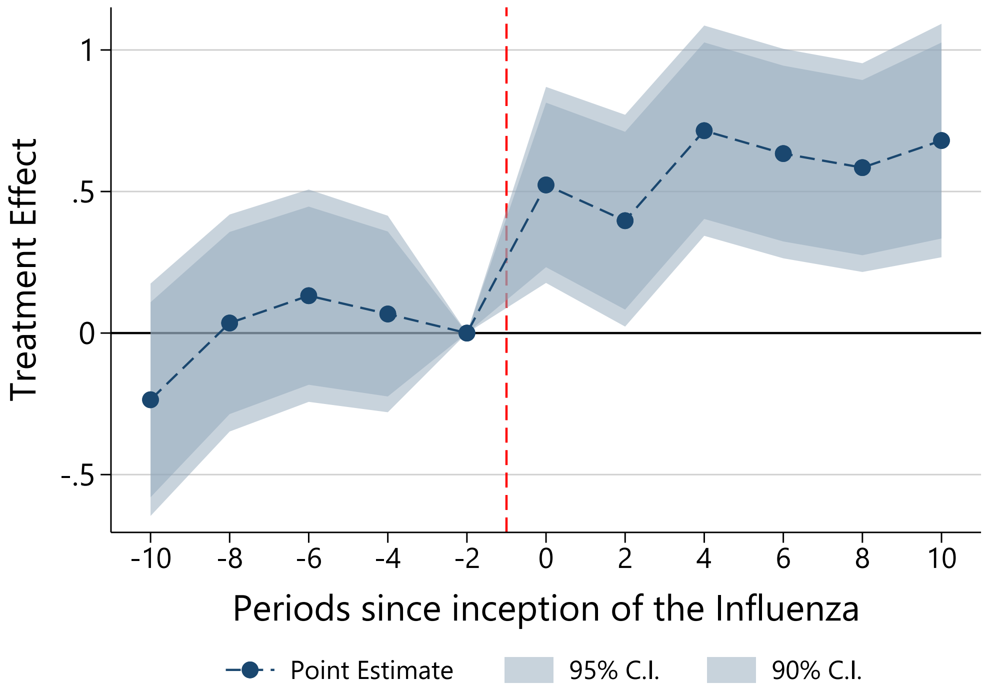

*Notes:* This figure displays geographic variation in influenza excess deaths, defined in Equation (4). Excess mortality is the ratio between the average number of deaths during the pandemic (1918–1919) and the average number of deaths in the three years before the pandemic (1915–1917). Mortality statistics prior to 1915 are not available. Excess mortality is displayed in percentage terms. Lighter to darker blue indicates increasing exposure to the influenza. Counties are displayed at their 1920 borders.

Figure 3: Impact of the Influenza on Religiosity



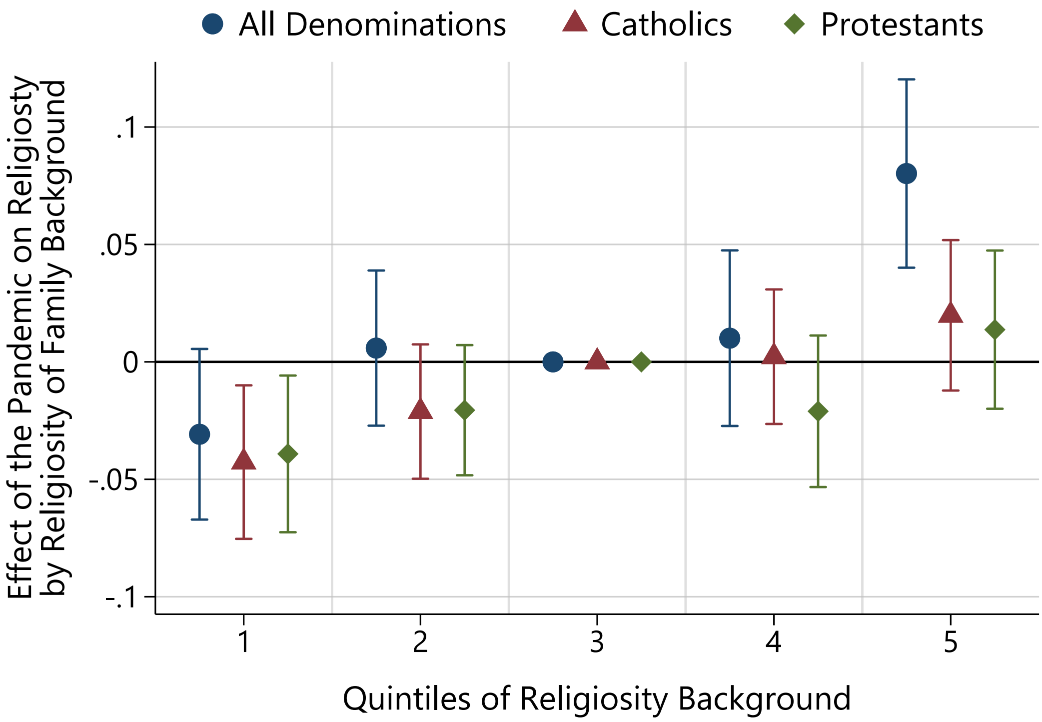

*Notes:* This figure displays the dynamic treatment effects of the pandemic on overall religiosity. The unit of observation is a county, observed at a biennial frequency. Each dot reports the coefficient of an interaction between the baseline measure of excess deaths, defined in Equation (4), and a biennial time dummy. The coefficient for the biennial 1916–1917, i.e., the last two-year window prior to the inception of the Great Influenza Pandemic, serves as the baseline. The model includes county and year fixed effects and the interaction between population in 1900 and a post-treatment indicator. Bands report 90% and 95% confidence intervals. Standard errors are clustered at the county level. The dashed vertical line indicates the timing of the pandemic.

Figure 4: Impact of the Influenza on Innovation




*Notes.* The Figure reports 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 1916–1917, i.e., the last two-year window prior to the inception of the Great Influenza Pandemic, serves as the baseline. The model includes county and year fixed effects and the interaction between population in 1900 and a post-treatment indicator. Bands report 90% and 95% confidence intervals. Standard errors are clustered at the county level. The dashed vertical line indicates the timing of the pandemic.

Figure 5: 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 denomination. Each dot reports the coefficient of an interaction between the baseline measure of excess deaths, a posttreatment 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.

# Online Appendix

Dealing With Adversity: Religiosity or Science?

Evidence From the Great Influenza Pandemic

Enrico Berkes Davide M. Coluccia Gaia Dossi Mara P. Squicciarini

# A Data: Description and Sources

In this section, we list the sources of the data and describe how we construct the variables used in the analysis.

## A.1 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, as well as 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 would fall under "Drugs" would also appear as "Chemical." To avoid this, we simply recast them into one single category. It is worth noting that all the results that we present in terms of pharmaceutical patents also hold if we keep the "Chemical" and "Drugs" classes separate.

**Quality Data** We measure patent quality using the measure developed by Kelly et al. (2021). From their data, we derive two metrics. One is the average quality. The second, which we label "Breakthrough", is an indicator variable returning value one if the patent's quality is in the top 25% of the overall quality distribution, and zero otherwise. 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.

**Linked Inventor-Census Data** Patent data alone do not allow to uniquely identify an inventor. Because in Table B.14 we need to measure inventor productivity as well as the number of unique inventors, we exploit a novel sample of inventors linked to the census by Bazzi et al. (2022). This allows us to assign a unique identifier to each inventor in our sample, and compute the related statistics. We defer the interested reader to the accompanying paper describing the data in more detail. An inventor can be matched to multiple census entries. In this paper, we disregard all inventors with more than five matches (about 5% of the overall stock). Then, we weigh the remaining by the inverse of the number of matches, as is standard in the census-linking literature.

### A.2 Names

We take name data from the individual full-count US 1930 population census (Ruggles et al., 2021). First names require some cleaning. First, we remove non-ASCII characters and drop all those reporting the initial only. Then, we manually identify common diminutives (e.g., “Thos” for “Thomas”). Finally, we agglutinate variations and minor spelling mistakes on the same underlying name. To do so, we code a simple script that collects a set of reference names as those appearing more than 50 times in the entire population census. We then compute the Jaro-Winkler similarity between each name and the reference names, and normalize it to lie between 0 and 1. If, for a given name, there is one reference name with a similarity above .99 we conflate that name to the reference. Otherwise, we just keep the name as is. This simple procedure is not intended to agglutinate either translations (e.g., “Tommaso” and “Thomas) or endearments (e.g., “Willie” and “William”). We thus take a conservative stand as to whether the same name in different languages–or its endearments–may convey different religious attitudes. It is merely an algorithmic approach to correct minor spelling mistakes. Overall, after the manual trimming we are left with 1,366,844 single names, which decrease to 623,792 after the algorithmic trimming procedure. However, weighting these figures by the number of children reveals that less than 20,000 names account for more than 95% of the total number of newborns.

### A.3 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 provide a list of their members. 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 as “Protestants” to a set of denominations which we manually mapped to some branch of Protestantism (including, e.g., the Methodist, Evangelical, and–various–Baptist churches.)

### A.4 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.1.

### A.5 Controls & Mortality Statistics

We extract a battery of individual-level characteristics from the IPUMS full count data. Among those, we use the the race and urban-rural status as additional individual-level controls.

County-level covariates are provided by NHGIS, which in turn 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, namely, 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 which preceded the Influenza.[1]

### A.6 Canadian Data

Following Abramitzky et al. (2020) we use the Canadian Census to construct an alternative measure of religiosity. This has the advantage of reporting information on individuals' religious affiliations as well as their first names. We use three waves of the census: 1881 (full count), 1911, and 1921, and construct religiosity scores by first name for the cohorts born between 1800 and 1916. Using the same procedure outlined in Fouka (2019), we construct the following metric:

$$\text{Religiosity score}_{\text{name},r,c} = \frac{\Pr\left(\text{name} \mid I_{r,c}\right)}{\Pr\left(\text{name} \mid I_{r,c}\right) + \Pr\left(\text{name} \mid I_{R\setminus r,c}\right)} \times 100 \tag{A.1}$$

where (name) is first name, $r$ is religion, $c$ is birth cohort, and $I$ is an indicator for individuals of a given religion and birth cohort. $I_{R\setminus r,c}$ indicates individuals of religion other than $r$. The score ranges from 0 to 100: a score of 0 implies a name is never found among individuals of religion $r$; a score of 100 implies a name is never found among individuals of a different religion. We define two

[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) criticize the methodology adopted to impute the cause of deaths.

religious groups in the Canadian Census: Catholics and Protestants.[2] First, we compute the scores in equation A.1 for each religion and birth cohort. Second, we average the score within-decade (where one decade corresponds to ten birth cohorts), for each religion and name. Finally, we generate a Catholic dummy and a Protestant dummy. Each dummy takes the value 1 if the corresponding religiosity score is larger than its 80, and zero otherwise.

### A.7 Other Data

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

### A.8 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 that we know the distribution of a given variable $y$ across counties at decade frequency between 1900 and 1930. To harmonize borders to one single 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 which is assigned to other counties. To construct the harmonized variable, one simply 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 in fact 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.9 Details on Sample Construction

In this paragraph, we provide additional technical details on the way we construct the estimation samples. The main sample restriction that we impose descends from the fact that we observe mortality for 1265 out of 2917 counties. We then discard 45 counties with implausibly large (above 200%)

[2]We only define two groups as, over this period, less than 1% of individuals reported a religious affiliation other than Catholic or Protestant.

or low (below 50%) values of excess mortality during the pandemic. Because such figures are due to scarcely-inhabited areas, these 45 counties account for less than 1% of the population in the 1265-counties sample. We are left with a set of 1220 counties. In the rest of the paragraph, we explain why we may not always be able to leverage all 1220 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 1220 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, the name-frequency threshold that we impose may imply that we are not able to 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 19 out of 1220 counties, so the estimation sample, in this case, consists of 1201 counties accounting for 98.5% of the population in the 1220-counties sample.

In one robustness check shown in column (7) of Tables B.3, B.4, and 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 value one for decades 1920 and 1930, and zero otherwise. In table B.6 we employ an alternative measure of religiosity from the 1906 Census of Religious Bodies that does not include fixed effects in the estimation equation of the names religiosity scores. This measure is considerably more volatile than the baseline, so we exclude the top and bottom 5% most extreme observations in the associated synthetic religiosity distribution.

**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 there are one or more patents 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.11 we do not fill the panel with zeros when no patents are observed. This results in an unbalanced panel dataset where a county may not be observed every year over the estimation time period.

**Other County-Level Samples** In Table B.8 we use as dependent variables various measure of name concentration. Because these measures display sizable variability, we restrict the sample to exclude counties at the top and bottom 1% of the excess deaths distribution. Results would remain qualitatively unchanged using the full sample, but they would conflate pre-treatment statistically significant–selection–effects that would induce a spurious downward bias in the estimated treatment effects.

**Individual-Level** We construct two individual-level datasets. In both samples, the unit of observation is the head of the household. In the first, each head of household is observed once. Regressions (7) and (10) are estimated on this "adult" sample. In the second sample we observe the kids of each head of household. We interpret the kids as realizations of the religiosity of their parent. Regressions (9), (11), and (12) are estimated on this "kid" sample.

# B Additional Tables and Figures

## B.1 Tables

Table B.1: STEM Professions

| Occ. Code | Occupation Label | Share (%) | Occ. Code | Occupation Label | Share (%) |
|---|---|---|---|---|---|
| (1) | (2) | (3) | (4) | (5) | (6) |
| **Panel A. STEM Occupations** | | | | | |
| 12 | Agricultural sciences | 0.00 | 18 | Mathematics | 0.00 |
| 61 | Agricultural scientists | 0.03 | 19 | Medical sciences | 0.03 |
| 13 | Biological sciences | 0.00 | 772 | Midwives | 0.42 |
| 62 | Biological scientists | 0.32 | 69 | Miscellaneous natural scientists | 0.08 |
| 14 | Chemistry | 0.04 | 26 | Natural science (n.e.c.) | 0.00 |
| 7 | Chemists | 6.07 | 92 | Surveyors | 1.05 |
| 32 | Dentists | 12.39 | 67 | Mathematicians | 0.01 |
| 34 | Dietitians and nutritionists | 0.64 | 240 | Officers, pilots, pursers and engineers, ship | 8.06 |
| 16 | Engineering | 0.01 | 94 | Technicians, medical and dental | 1.49 |
| 49 | Engineers (n.e.c.) | 0.88 | 70 | Optometrists | 1.33 |
| 41 | Engineers, aeronautical | 0.05 | 71 | Osteopaths | 0.73 |
| 42 | Engineers, chemical | 0.61 | 25 | Statistics | 0.00 |
| 43 | Engineers, civil | 13.31 | 75 | Physicians and surgeons | 30.98 |
| 44 | Engineers, electrical | 9.32 | 68 | Physicists | 0.04 |
| 45 | Engineers, industrial | 0.34 | 23 | Physics | 0.00 |
| 46 | Engineers, mechanical | 7.13 | 17 | Geology and geophysics | 0.00 |
| 47 | Engineers, metallurgical, metallurgists | 0.31 | 98 | Veterinarians | 1.77 |
| 48 | Engineers, mining | 1.11 | 83 | Statisticians and actuaries | 1.07 |
| 63 | Geologists and geophysicists | 0.33 | 61 | Agricultural Scientists | 0.07 |
| **Panel B. Other Skilled Occupations** | | | | | |
| $1 \leq \cdot \leq 99$ | Liberal and Skilled Professions | | $200 \leq \cdot \leq 299$ | Managers | |
| $700 \leq \cdot \leq 790$ | Service Workers | | | | |

*Notes*: Panel A displays the occupations which we classify as Science, Technology, Engineering, and Mathematics (STEM). Panel B displays the occupations that we classify as skilled: these include all STEM occupations, in addition to the ones listed. Occupation codes and labels are from the IPUMS harmonized 1950 occupation taxonomy. Column "Share" indicates the percentage share of individuals in the given occupation, relative to total employment in STEM occupations in the baseline individual-level sample. STEM occupations account for about 6% of total skilled employment, which in turn accounts for approximately 14% of total employment.

Table B.2: Balance Checks Regressions

| | (1) Coefficient | (2) Standard Error | (3) 95% C. I. |
|---|---|---|---|
| **Panel A. Income and Demographics** | | | |
| Population Density | −0.098 | (0.221) | [−0.531, 0.336] |
| Income per Capita | 0.332 | (0.407) | [−0.466, 1.131] |
| Share of Men | 0.520*** | (0.163) | [0.200, 0.840] |
| Share of Illiterates | 0.339 | (0.362) | [−0.369, 1.048] |
| Share of Young | 0.366 | (0.311) | [−0.244, 0.976] |
| **Panel B. Ethnic Composition** | | | |
| Share of Whites | 0.255 | (0.246) | [−0.228, 0.738] |
| Share of African Americans | −0.308 | (0.235) | [−0.769, 0.153] |
| Share of Foreign Population | 0.444*** | (0.159) | [0.131, 0.757] |
| Immigrants from: | | | |
| Italy | 0.287 | (0.282) | [−0.265, 0.840] |
| Ireland | 0.088 | (0.126) | [−0.159, 0.336] |
| Austria | 0.172 | (0.356) | [−0.527, 0.870] |
| France | 0.244 | (0.188) | [−0.123, 0.612] |
| Spain | 0.419 | (0.365) | [−0.295, 1.134] |
| Portugal | 0.000 | (0.291) | [−0.569, 0.570] |
| **Panel C. Religion** | | | |
| All Denominations | −0.126 | (0.246) | [−0.609, 0.356] |
| Catholics | 0.175 | (0.226) | [−0.269, 0.619] |
| Protestants | −0.263 | (0.288) | [−0.827, 0.302] |
| **Panel D. Patents** | | | |
| Total | 0.169 | (0.103) | [−0.032, 0.370] |
| Pharmaceutical | 0.132 | (0.097) | [−0.058, 0.321] |
| Communication | 0.100 | (0.101) | [−0.097, 0.297] |
| Electrical | 0.226 | (0.172) | [−0.110, 0.562] |
| Mechanical | 0.186* | (0.096) | [−0.003, 0.375] |
| Other | 0.147 | (0.091) | [−0.031, 0.324] |

*Notes*: This table displays the correlation between the Excess Death (defined in (4)) 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 total population, except for population density. Regressions control for county population and include state fixed effects.

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

Table B.3: Impact of the Influenza on Religiosity: Robustness on All Denominations

| | 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 | 0.007** | | 0.007** | 0.006* | 0.007** | 0.003** | 0.014 |
| | (0.003) | | (0.003) | (0.003) | (0.004) | (0.001) | (0.012) |
| Excess Deaths Dummy × Post | | 0.003** | | | | | |
| | | (0.001) | | | | | |
| WW1 Deaths × Post | | | 0.000 | | | | |
| | | | (0.000) | | | | |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample | Baseline | Baseline | Baseline | No Firstborn | < 5 Kids | Household | Adults |
| Number of Counties | 1201 | 1201 | 1201 | 1200 | 1200 | 1201 | 1201 |
| Observations | 36030 | 36030 | 36030 | 36000 | 36000 | 36030 | 4804 |
| $R^2$ | 0.450 | 0.450 | 0.450 | 0.366 | 0.370 | 0.364 | 0.860 |
| Std. Beta Coef. | 0.109 | 0.021 | 0.110 | 0.086 | 0.099 | 0.101 | 0.081 |

*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 (4). 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 year fixed effects; the regression in column (7) includes county and decade fixed effects. Additionally, each regression includes the interaction between population in 1900 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses.

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

Table B.4: Impact of the Influenza on Religiosity: Robustness on Catholics

| | 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 | 0.009*** | | 0.009*** | 0.009*** | 0.008** | 0.003** | 0.019 |
| | (0.003) | | (0.003) | (0.003) | (0.003) | (0.001) | (0.011) |
| Excess Deaths Dummy × Post | | 0.003*** | | | | | |
| | | (0.001) | | | | | |
| WW1 Deaths × Post | | | -0.000 | | | | |
| | | | (0.000) | | | | |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample | Baseline | Baseline | Baseline | No Firstborn | < 5 Kids | Household | Adults |
| Number of Counties | 1201 | 1201 | 1201 | 1200 | 1200 | 1201 | 1201 |
| Observations | 36030 | 36030 | 36030 | 36000 | 36000 | 36030 | 4804 |
| $R^2$ | 0.306 | 0.306 | 0.306 | 0.242 | 0.251 | 0.352 | 0.778 |
| Std. Beta Coef. | 0.184 | 0.030 | 0.182 | 0.158 | 0.140 | 0.122 | 0.136 |

*Notes*: This table displays the impact of exposure to the Influenza on Catholic 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 (4). 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 year fixed effects; the regression in column (7) includes county and decade fixed effects. Additionally, each regression includes the interaction between population in 1900 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses.

$^{*}$: $p < 0.10$, $^{**}$: $p < 0.05$, $^{***}$: $p < 0.01$

Table B.5: Impact of the Influenza on Religiosity: Robustness on Protestants

| | 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 | 0.006** | | 0.006** | 0.006** | 0.004 | 0.001 | 0.009 |
| | (0.003) | | (0.003) | (0.003) | (0.003) | (0.001) | (0.011) |
| Excess Deaths Dummy × Post | | 0.002 | | | | | |
| | | (0.001) | | | | | |
| WW1 Deaths × Post | | | 0.000*** | | | | |
| | | | (0.000) | | | | |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Sample | Baseline | Baseline | Baseline | No Firstborn | < 5 Kids | Household | Adults |
| Number of Counties | 1201 | 1201 | 1201 | 1200 | 1200 | 1201 | 1201 |
| Observations | 36030 | 36030 | 36030 | 36000 | 36000 | 36030 | 4804 |
| $R^2$ | 0.471 | 0.471 | 0.472 | 0.384 | 0.401 | 0.377 | 0.858 |
| Std. Beta Coef. | 0.100 | 0.015 | 0.103 | 0.102 | 0.057 | 0.064 | 0.062 |

*Notes*: This table displays the impact of exposure to the Influenza on Protestant 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 (4). 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 year fixed effects; the regression in column (7) includes county and decade fixed effects. Additionally, each regression includes the interaction between population in 1900 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses.

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

Table B.6: 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 | 0.037** | 0.029* | 0.014 | 0.100*** | 0.089*** | 0.021 |
| | (0.018) | (0.016) | (0.011) | (0.034) | (0.030) | (0.025) |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of Counties | 1201 | 1201 | 1201 | 1201 | 1201 | 1201 |
| Observations | 29612 | 29612 | 29612 | 29612 | 29612 | 29612 |
| $R^2$ | 0.387 | 0.540 | 0.282 | 0.636 | 0.804 | 0.557 |
| Std. Beta Coef. | 0.130 | 0.098 | 0.082 | 0.392 | 0.288 | 0.128 |

*Notes*: This table displays the impact of exposure to the Influenza on religiosity. The unit of observation is a county, observed at yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over the years 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (4). Religiosity is measured using religiosity scores obtained by estimating equation (2), 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 year fixed effects and the interaction between population in 1900 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses.

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

Table B.7: Impact of the Influenza on Religiosity: Alternative Thresholds

| | All | | | Catholics | | | Protestants | | |
|---|---|---|---|---|---|---|---|---|---|
| | ($\tau = 2$) | ($\tau = 3$) | ($\tau = 5$) | ($\tau = 2$) | ($\tau = 3$) | ($\tau = 5$) | ($\tau = 2$) | ($\tau = 3$) | ($\tau = 5$) |
| Excess Deaths × Post | 0.012 | 0.007** | 0.004** | 0.007* | 0.009*** | 0.004* | 0.003 | 0.006** | 0.004 |
| | (0.008) | (0.003) | (0.002) | (0.003) | (0.003) | (0.002) | (0.004) | (0.003) | (0.002) |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of Counties | 1201 | 1201 | 1200 | 1201 | 1201 | 1200 | 1201 | 1201 | 1200 |
| Observations | 36030 | 36030 | 36000 | 36030 | 36030 | 36000 | 36030 | 36030 | 36000 |
| $R^2$ | 0.378 | 0.450 | 0.356 | 0.248 | 0.306 | 0.483 | 0.353 | 0.471 | 0.416 |
| Std. Beta Coef. | 0.115 | 0.109 | 0.097 | 0.117 | 0.184 | 0.093 | 0.047 | 0.100 | 0.091 |

*Notes*: This table displays the impact of exposure to the Influenza on religiosity. The unit of observation is a county, observed at yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over the years 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (4). Religiosity is measured using religiosity scores obtained estimating equation (2). The term $\tau$ 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. We report the baseline results, with $\tau = 3$, as well as those with lower and larger thresholds. As $\tau$ decreases, the number of names for which we compute a religiosity score increases. Regressions include county and year fixed effects and the interaction between population in 1900 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses.

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

Table B.8: Impact of the Influenza on the Concentration of Names

| | HHI | CCI | Rosenbluth | C-5 | C-7 | C-9 | C-10 |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Excess Deaths × Post | -0.090 | -0.005** | -0.115* | -0.007 | -0.007 | -0.007 | -0.008 |
| | (0.065) | (0.002) | (0.059) | (0.004) | (0.005) | (0.006) | (0.006) |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of Counties | 1150 | 1150 | 1150 | 1150 | 1150 | 1150 | 1150 |
| Observations | 34490 | 34490 | 34490 | 34490 | 34490 | 34490 | 34490 |
| $R^2$ | 0.853 | 0.779 | 0.875 | 0.779 | 0.807 | 0.824 | 0.830 |
| Std. Beta Coef. | -0.092 | -0.162 | -0.109 | -0.109 | -0.094 | -0.085 | -0.085 |

*Notes*: This table displays the impact of exposure to the Influenza on name concentration. The unit of observation is a county, observed at yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over the years 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (4). 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, $HHI \equiv \sum_{n=1}^{N} s_n^2$; $CCI \equiv s_1 + \sum_{n=2}^{N} s_n^2(2 - s_n)$, $RI \equiv \frac{1}{2\sum_{n=1}^{N} ns_n - 1}$; $C_K \equiv \sum_{n=1}^{K} s_n$. Regressions include county and state-by-year fixed effects and the interaction between population in 1900 and a post-treatment indicator. Standard errors are clustered at the county level, and are reported in parentheses.

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

Table B.9: Impact of the Influenza on Religiosity measured With Canadian and Saint/Biblical Scores

| | Canada Scores | | Biblical and Saints Scores | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| | Catholics | Protestants | Biblical/Saints | Saints | Biblical |
| Excess Deaths × Post | 0.013** | 0.006 | 0.055*** | 0.051*** | 0.013*** |
| | (0.005) | (0.020) | (0.009) | (0.008) | (0.003) |
| County FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Number of Counties | 1200 | 1200 | 1200 | 1200 | 1200 |
| Observations | 36000 | 36000 | 36000 | 36000 | 36000 |
| $R^2$ | 0.327 | 0.491 | 0.959 | 0.958 | 0.958 |
| Std. Beta Coef. | 0.173 | 0.016 | 0.151 | 0.147 | 0.095 |

*Notes*: This table displays the impact of exposure to the Influenza on religiosity. The unit of observation is a county, observed at yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over the years 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (4). In columns (1)-(3), religiosity is measured using religiosity scores obtained as described in section B from the Canadian census. In column (3), the dependent variable is the share of children by cohort whose name either appears in the bible, or is carried by a saint; in column (4), the dependent variable only includes biblical names; in column (5), it only includes names of saints. Biblical and saints names are from Abramitzky et al. (2016). Regressions include county and year fixed effects and the interaction between population in 1900 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses.

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

Table B.10: Impact of the Influenza on Religiosity Measured as the Share of Individuals in Each Denomination, From the Census of Religious Bodies

| | Share of Affiliated | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | All | Catholics | Protestants |
| Excess Deaths × Post | 0.202*** | 0.082*** | 0.082*** |
| | (0.027) | (0.015) | (0.015) |
| County FE | Yes | Yes | Yes |
| Decade FE | Yes | Yes | Yes |
| Number of Counties | 1219 | 1219 | 1219 |
| Observations | 3657 | 3657 | 3657 |
| $R^2$ | 0.861 | 0.908 | 0.931 |
| Std. Beta Coef. | 0.635 | 0.337 | 0.301 |

*Notes*: This table displays the impact of exposure to the Influenza on religiosity. The unit of observation is a county, observed at decade frequency between 1906 and 1926. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over the years 1920-1930–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (4). Religiosity is measured as the share of people affiliated to a given denomination, normalized by county population in 1910. Column (1) reports the results for total religiosity; column (2) refers to Catholics; column (3) refers to Protestants. Regressions include county and decade fixed effects and the interaction between population in 1900 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses.

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

Table B.11: 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 | 0.503*** | 0.474*** | | 0.503*** | 0.091*** | 0.276*** | 0.134*** | | 0.091*** |
| | (0.064) | (0.087) | | (0.064) | (0.033) | (0.047) | (0.051) | | (0.033) |
| Excess Deaths Dummy × Post | | | 0.118*** | | | | | 0.032*** | |
| | | | (0.020) | | | | | (0.010) | |
| WW1 Deaths × Post | | | | 3.739 | | | | | 3.574 |
| | | | | (17.019) | | | | | (3.057) |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| 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 | 1220 | 1184 | 1220 | 1220 | 1220 | 1220 | 1184 | 1220 | 1220 |
| Observations | 37820 | 23909 | 37820 | 37820 | 37820 | 37820 | 23909 | 37820 | 37820 |
| $R^2$ | 0.832 | 0.861 | 0.832 | 0.832 | 0.836 | 0.786 | 0.824 | 0.836 | 0.836 |
| Std. Beta Coef. | 0.211 | 0.223 | 0.036 | 0.211 | 0.066 | 0.201 | 0.085 | 0.017 | 0.066 |

*Notes*: This table displays the impact of exposure to the Influenza on innovation. The unit of observation is a county, observed at 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 drugs fields, according to the NBER standard classification. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over the years 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (4). 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 year fixed effects and the interaction between population in 1900 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. *: $p < 0.10$, **: $p < 0.05$, ***: $p < 0.01$

TABLE B.12: IMPACT OF THE INFLUENZA ON INNOVATION: ALTERNATIVE MEASURES OF OVERALL INNOVATION

| | $f$ (All Patents) | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | $ln(1+\cdot)$ | Count | arcsinh($\cdot$) |
| Excess Deaths × Post | 0.503*** | 6.787** | 0.617*** |
| | (0.064) | (3.343) | (0.078) |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Number of Counties | 1220 | 1220 | 1220 |
| Observations | 37820 | 37820 | 37820 |
| $R^2$ | 0.832 | 0.908 | 0.810 |
| Std. Beta Coef. | 0.211 | 0.069 | 0.220 |

*Notes*: This table displays the effect of the Influenza on overall innovation. The unit of observation is a county, observed at yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over the years 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. In column (2) the dependent variable is the raw patent count. In column (3) the dependent variable is the inverse hyperbolic sine of the raw count of patents. Each regression includes county and year fixed effects and the interaction between population in 1900 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses.

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

Table B.13: Impact of the Influenza on Innovation: Alternative Measures of Pharmaceutical Innovation

| | $f$ (Pharmaceutical Patents) | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | $ln(1+\cdot)$ | $ln(1+\cdot)$ | Count | Count | arcsinh($\cdot$) | arcsinh($\cdot$) | Share | $ln(1+\text{Share})$ |
| Excess Deaths × Post | 0.091*** | 0.246*** | 0.793*** | 1.777*** | 0.117*** | 0.302*** | 0.110*** | 0.071*** |
| | (0.033) | (0.045) | (0.228) | (0.592) | (0.042) | (0.055) | (0.031) | (0.017) |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Total Patents | Yes | No | Yes | No | Yes | Yes | No | No |
| Number of Counties | 1220 | 1220 | 1220 | 1220 | 1220 | 1220 | 1220 | 1220 |
| Observations | 37820 | 37820 | 37820 | 37820 | 37820 | 37820 | 37820 | 37820 |
| $R^2$ | 0.836 | 0.788 | 0.959 | 0.856 | 0.820 | 0.773 | 0.171 | 0.228 |
| Std. Beta Coef. | 0.224 | 0.179 | 0.059 | 0.132 | 0.177 | 0.178 | 0.190 | 0.191 |

*Notes*: This table displays the effect of the Influenza on innovation in pharmaceuticals. The unit of observation is a county, observed at yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over the years 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. In columns (3) and (4), the dependent variable is the raw patent count. 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 log. Each regression includes county and year fixed effects and the interaction between population in 1900 and a post-treatment indicator. In columns (1), (3), and (6) we further control by 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.

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

Table B.14: Impact of the Influenza on Innovation: Intensive and Extensive Margins

| | Patents Per Inventor | | | N. of Inventors | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | All | Pharma | Pharma | All | Pharma | Pharma |
| Excess Deaths × Post | 0.164*** | 0.079*** | 0.038** | 0.348*** | 0.175*** | 0.070** |
| | (0.025) | (0.020) | (0.018) | (0.059) | (0.040) | (0.031) |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| N. of Inventors | No | No | No | No | No | Yes |
| Patents Per Inventor | No | No | Yes | No | No | No |
| Number of Counties | 1220 | 1220 | 1220 | 1220 | 1220 | 1220 |
| Observations | 37820 | 37820 | 37820 | 37820 | 37820 | 37820 |
| $R^2$ | 0.477 | 0.464 | 0.509 | 0.823 | 0.750 | 0.791 |
| Std. Beta Coef. | 0.249 | 0.140 | 0.068 | 0.164 | 0.132 | 0.053 |

*Notes*: This table displays the impact of exposure to the Influenza on the (log 1+) number of patents per inventor (intensive margin) and the (log) number of inventors (extensive margin). The unit of observation is a county, observed at yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over the years 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (4). In column (1) the dependent variable is the number of patents per inventor in any field; in columns (2)–(3) we restrict to pharmaceutical patents per inventors; in column (4) the dependent variable is the number of inventors; in columns (5)–(6) we only consider inventors with at least one patent in pharmaceuticals. In column (3) we control for the average productivity, measured as the number of patents per inventor, to capture differential trends in productivity of pharmaceuticals, relative to the aggregate productivity; similarly, in column (6) we control for the number of inventors to disentangle differential patterns for the subgroup of inventors active in pharmaceuticals. Regressions include county and year fixed effects and the interaction between population in 1900 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses.

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

Table B.15: Impact of the Influenza on the Quality of Innovation

| | All Patents | | | Pharmaceuticals | | | |
|---|---|---|---|---|---|---|---|
| | (1) Avg. Quality | (2) Breakthrought | (3) Share Breakthrough | (4) Avg. Quality | (5) Breakthrought | (6) Breakthrought | (7) Share Breakthrough |
| Excess Deaths × Post | 0.099 | 1.548** | 0.021 | 0.302*** | 0.991*** | 0.609*** | 0.021** |
| | (0.150) | (0.642) | (0.015) | (0.107) | (0.309) | (0.225) | (0.009) |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Total Patents | No | No | No | No | No | Yes | No |
| Number of Counties | 1220 | 1220 | 1220 | 1220 | 1220 | 1220 | 1220 |
| Observations | 37818 | 37818 | 37818 | 37818 | 37818 | 37818 | 37818 |
| $R^2$ | 0.314 | 0.786 | 0.131 | 0.477 | 0.681 | 0.793 | 0.107 |
| Mean Dep. Var. | 0.037 | 1.790 | 0.080 | 0.081 | 0.480 | 0.480 | 0.020 |
| Std. Beta Coef. | 0.031 | 0.092 | 0.062 | 0.220 | 0.197 | 0.011 | 0.131 |

*Notes*: This table displays the impact of the Influenza on the quality of innovation. In the first three columns, the quality indicators refer to the total patent flow; in the last four columns we restrict the sample to patents in pharmaceuticals. The unit of observation is a county, observed at yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over the years 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (4). Quality measures are from Kelly et al. (2021). They measure the "innovativeness" of a patent based on 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 (4), "Avg. Quality" denotes their baseline quality measure (equation (10) in Kelly et al. (2021)); in columns (2) and (5)–(6) "Breakthrough" is the raw count of patents in the top quintile of the quality distribution; in columns (3) and (7) "Share Breakthrough" is the share of patents in the top quintile in the quality distribution. Regressions include county and year fixed effects and the interaction between population in 1900 and a post-treatment indicator. Standard errors are clustered at the county level, and are displayed in parentheses.

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

Table B.16: Impact of the Influenza on Occupational Choice: Alternative Threshold

| | Dummy = 1 if in STEM | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | Baseline | No Controls | Controls |
| Excess Deaths × Younger than 30 in 1918 | 0.005*** | 0.006*** | 0.006*** |
| | (0.002) | (0.001) | (0.002) |
| County FE | Yes | Yes | Yes |
| Cohort FE | Yes | – | – |
| State-Cohort FE | No | Yes | Yes |
| Household Controls | No | No | Yes |
| Number of Counties | 1217 | 1217 | 1217 |
| Observations | 13573144 | 13573098 | 13573098 |
| $R^2$ | 0.003 | 0.003 | 0.004 |
| Std. Beta Coef. | 0.026 | 0.030 | 0.030 |

*Notes*: This table displays the effect of the pandemic on the probability of being employed in a STEM occupation. The unit of observation is an individual, observed once in the 1930-population census. For every person, we define a dummy equal to one if the person is employed in a STEM occupation–enumerated in Table B.1– and zero otherwise. We drop individuals born after 1905 because they could still be completing their education spell in 1930, *i.e.* when we observe their occupational choice. An individual is defined to be treated if she is 30 years old or less in 1918, *i.e.* at the beginning of the pandemic. Compared to the baseline estimates, we enlarge the sample of treated individuals to those that were between 25 and 30 at the time of the inception of the pandemic. The baseline treatment "Excess Deaths" is defined in equation (4). Column (1) reports the baseline estimates; in column (2) we add state-by-year fixed effects to the baseline model. Column (3) further includes a set of individual-level controls. Individual controls are race and urban status. Standard errors are clustered at the county level and are reported in parentheses.

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

Table B.17: Religiosity and the Intensity of Innovation by Exposure to the Influenza

| | (log) Patents per Capita | | |
|---|---|---|---|
| | (1) All Affiliations | (2) Catholics | (3) Protestants |
| Excess Deaths × Post | 0.032*** | 0.039*** | 0.033*** |
| | (0.012) | (0.013) | (0.012) |
| All Affiliations | -0.042 | | |
| | (0.025) | | |
| Excess Deaths × Post × All Affiliations | 0.104** | | |
| | (0.041) | | |
| Catholics | | 0.023 | |
| | | (0.026) | |
| Excess Deaths × Post × Catholics | | 0.097** | |
| | | (0.047) | |
| Protestants | | | -0.079* |
| | | | (0.041) |
| Excess Deaths × Post × Protestants | | | 0.085* |
| | | | (0.048) |
| County FE | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes |
| Number of Counties | 1201 | 1201 | 1201 |
| Observations | 36030 | 36030 | 36030 |
| $R^2$ | 0.749 | 0.750 | 0.749 |

*Notes*: This table displays the correlation between innovation and religiosity by exposure to the pandemic. The dependent variable is the log of patents, normalized by county-population in 1900. The unit of observation is a county, observed at yearly frequency between 1900 and 1929. "Post" is a categorical variable equal to one during and after the pandemic–*i.e.* over the years 1918-1929–and zero otherwise. The baseline treatment "Excess Deaths" is defined in equation (4). Religiosity by denomination is measured as described in the main text. Counties are weighted by their population in 1900. Regressions include county and year fixed effects and the interaction between population in 1900 and a post-treatment indicator. Standard errors are clustered at the county level and are reported in parentheses.

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

Table B.18: Religiosity of Individuals in STEM Compared to the Rest of the Population

| | Non-STEM | STEM | Difference | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| All | 0.021 | 0.015 | $-0.006^{*}$ | $-0.011^{***}$ |
| | | | (0.086) | (0.001) |
| Catholics | $-0.156$ | $-0.176$ | $-0.020^{***}$ | $-0.017^{***}$ |
| | | | (0.000) | (0.000) |
| Protestants | 0.027 | 0.010 | $-0.016^{***}$ | $-0.022^{***}$ |
| | | | (0.000) | (0.000) |
| County FE | No | No | No | Yes |
| Birth Year FE | No | No | No | Yes |
| Controls | No | No | No | Yes |

*Notes*: Columns (1) and (2) report the average religiosity of the non-STEM and the STEM populations; columns (3)–(4) report the difference between the two groups. Denomination varies by row (hence, for instance, average Catholic religiosity for Non-STEM is .181, it is .085 for STEM individuals, and their unconditional difference is -.096). To construct religiosity, we take all children in our baseline sample born before the Influenza, *i.e.* 1917. Observations are weighted by the inverse of the total number of kids in each household. In columns (1), (2), and (3) we report the unconditional statistics. In column (4) we include a set of county and (child) birth year fixed effects and we control for race and urban status. Standard errors are clustered at the county level. In columns (3)–(4), we report in parentheses the $p$-value associated with the estimates.

$^{*}$: $p < 0.10$, $^{**}$: $p < 0.05$, $^{***}$: $p < 0.01$

Table B.19: Impact of the Influenza on the Polarization of Religious Beliefs

| | All | Catholics | Protestants |
|---|---|---|---|
| | (1) | (2) | (3) |
| Overall Religiosity Background=1 × Excess Deaths × Post | -0.031* | | |
| | (0.019) | | |
| Overall Religiosity Background=2 × Excess Deaths × Post | 0.006 | | |
| | (0.017) | | |
| Overall Religiosity Background=4 × Excess Deaths × Post | 0.010 | | |
| | (0.019) | | |
| Overall Religiosity Background=5 × Excess Deaths × Post | 0.080*** | | |
| | (0.020) | | |
| Catholic Religiosity Background=1 × Excess Deaths × Post | | -0.043** | |
| | | (0.017) | |
| Catholic Religiosity Background=2 × Excess Deaths × Post | | -0.021 | |
| | | (0.015) | |
| Catholic Religiosity Background=4 × Excess Deaths × Post | | 0.002 | |
| | | (0.015) | |
| Catholic Religiosity Background=5 × Excess Deaths × Post | | 0.020 | |
| | | (0.016) | |
| Protestant Religiosity Background=1 × Excess Deaths × Post | | | -0.039** |
| | | | (0.017) |
| Protestant Religiosity Background=2 × Excess Deaths × Post | | | -0.021 |
| | | | (0.014) |
| Protestant Religiosity Background=4 × Excess Deaths × Post | | | -0.021 |
| | | | (0.016) |
| Protestant Religiosity Background=5 × Excess Deaths × Post | | | 0.014 |
| | | | (0.017) |
| County × Background FE | Yes | Yes | Yes |
| County × Birthyear FE | Yes | Yes | Yes |
| Background × Birthyear FE | Yes | Yes | Yes |
| Household Controls | Yes | Yes | Yes |
| N. of Counties | 1217 | 1217 | 1217 |
| Observations | 7641690 | 7641690 | 7641690 |
| $R^2$ | 0.026 | 0.021 | 0.024 |

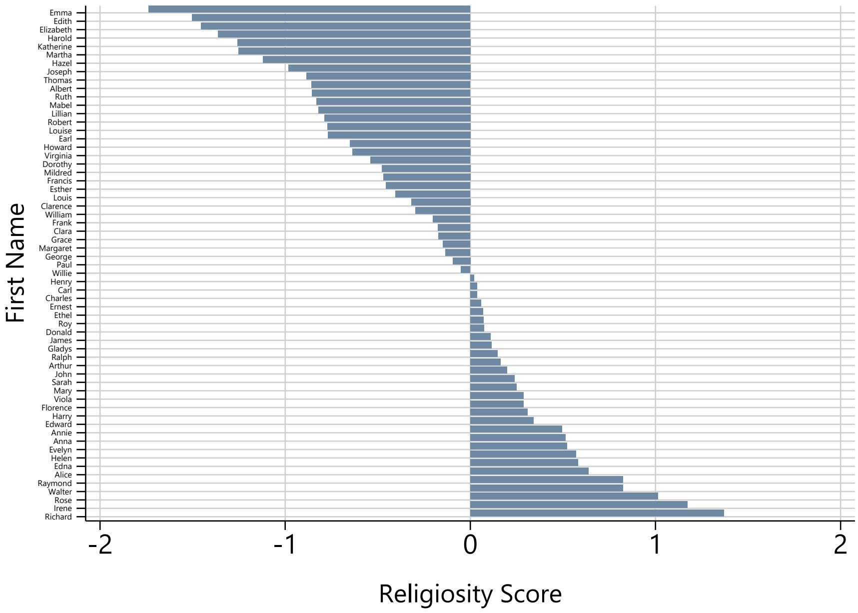
*Notes*: This table displays the impact of exposure to the pandemic on the polarization of religious beliefs, for all denominations. The unit of observation are children born between 1900 and 1930. “Post” is a categorical variable equal to zero for children born before the pandemic–*i.e.* before 1918–and one for those born after the pandemic–*i.e.* after 1918. The baseline treatment “Excess Deaths” is defined in equation (4). Background religiosity is measured as the religiosity score of the name of the head of the household, and it is discretized in quintiles. The third quintile serves as the baseline category and its coefficient is not reported. The dependent variable is overall religiosity (column 1), Catholic religiosity (column 2), and Protestant religiosity (column 3). Each regression includes county-by-background, background-by-year, and county-by-year fixed effects. Children are weighted by the inverse of the number of children within each household. Standard errors are clustered at the county level, and are reported in parentheses.

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

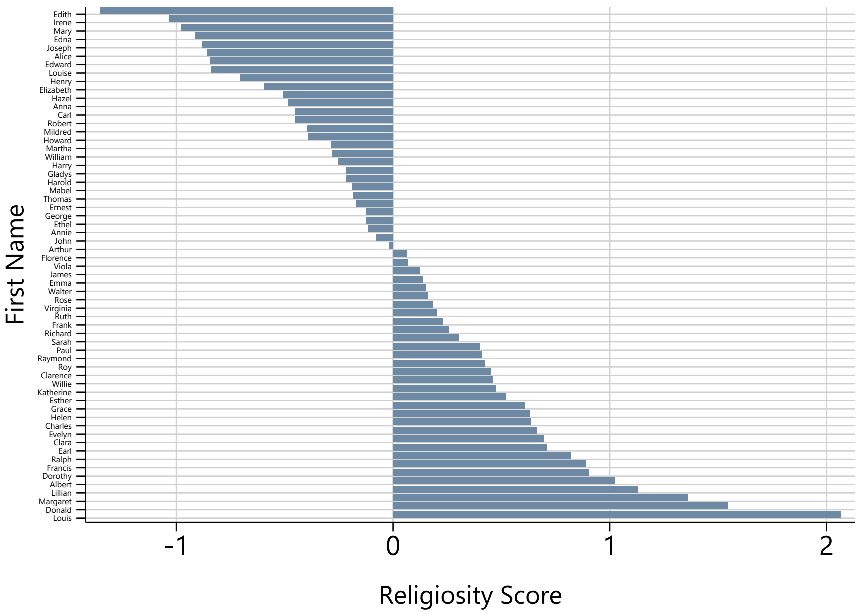
## B.2 Figures

Figure B.1: Estimated Names Religiosity Scores, by Confession

(a) Catholics



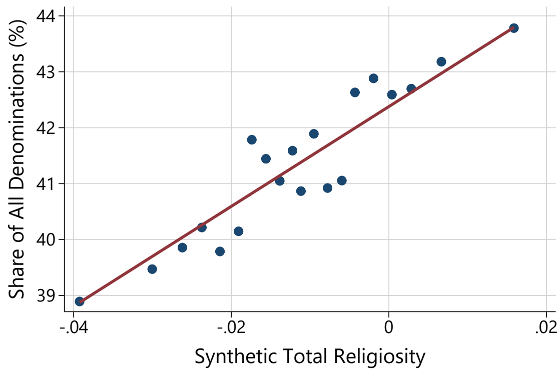

(b) Protestants




*Notes*: The Figures display the religiosity scores estimated from model (2). 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. We conflate variations of a single name together–e.g. Anne and Anna—but keep endearments separate—*e.g.,* Anna and Annie. Coefficients are reported in increasing order. Panel B.1a reports scores for Catholicism; Panel B.1b reports scores for Protestantism.

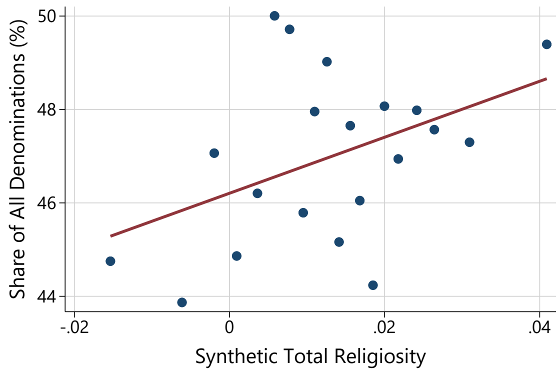
Figure B.2: In-sample and Out-of-sample Fit of the Religiosity Measure

(a) In-sample: All Denominations



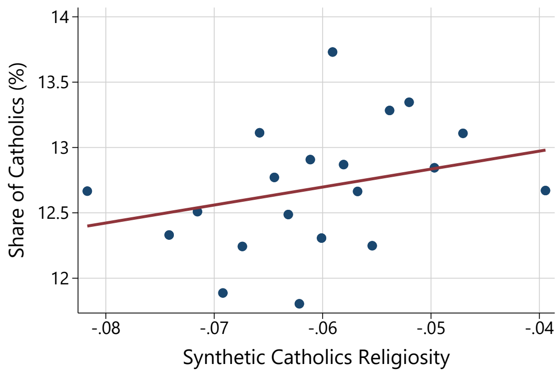

*Notes*. Regression Coefficient = 89.222 (Std. Err. = 12.079). $R^2$=0.856.

(b) Out-of-sample: All Denominations



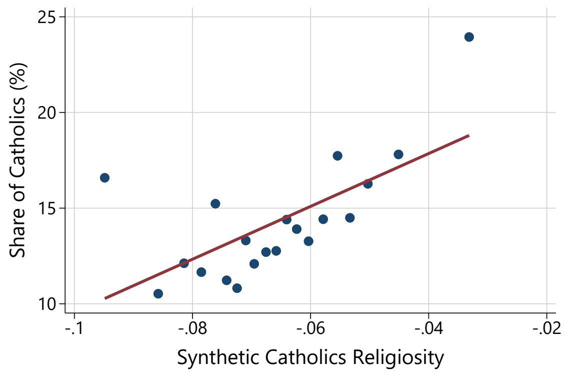

*Notes*. Regression Coefficient = 59.950 (Std. Err. = 31.614). $R^2$=0.349.

(c) In-sample: Catholics



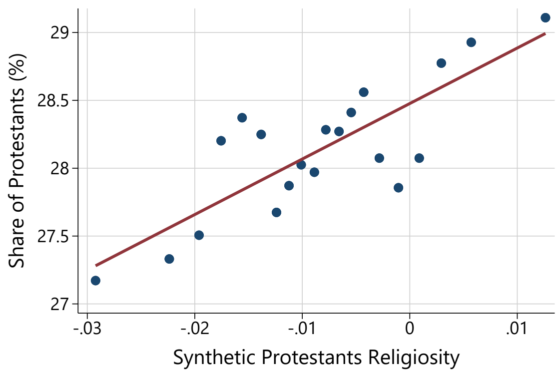

*Notes*. Regression Coefficient = 13.746 (Std. Err. = 12.634). $R^2$=0.911.

(d) Out-of-sample: Catholics



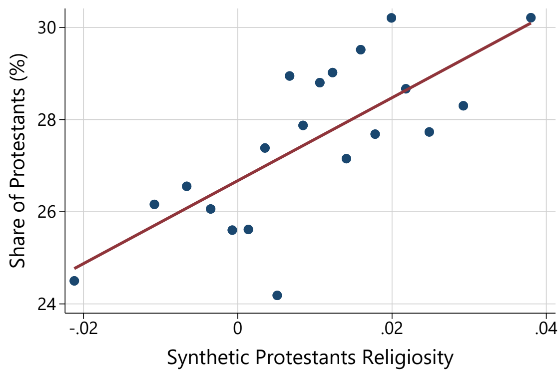

*Notes*. Regression Coefficient = 138.124 (Std. Err. = 41.434). $R^2$=0.432.

(e) In-sample: Protestants




*Notes*. Regression Coefficient = 40.904 (Std. Err. = 7.767). $R^2$=0.947.

(f) Out-of-sample: Protestants




*Notes*. Regression Coefficient = 90.009 (Std. Err. = 22.835). $R^2$=0.606.

*Notes*: These figures are county-level binned scatter plots reporting the correlation between our religiosity measure and the number of affiliated member 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 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. Counties are weighted by their population in 1900. In the note we report the regression coefficients and the associated $R^2$.

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

Filed Jan. 11, 1922 2 Sheets-Sheet 2

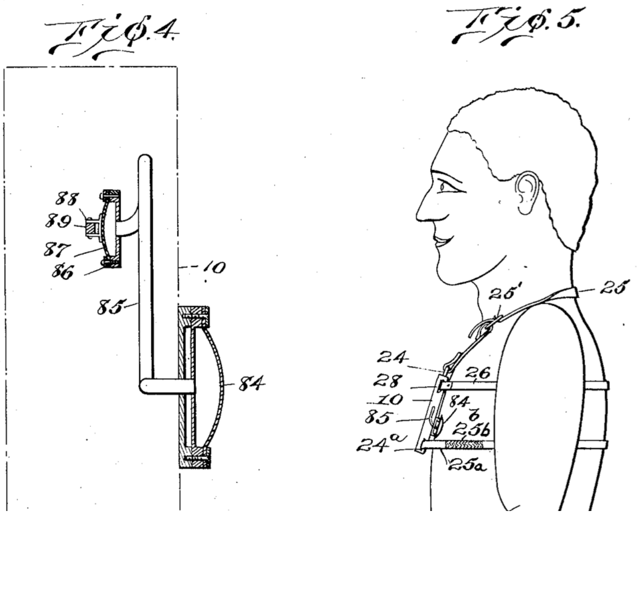
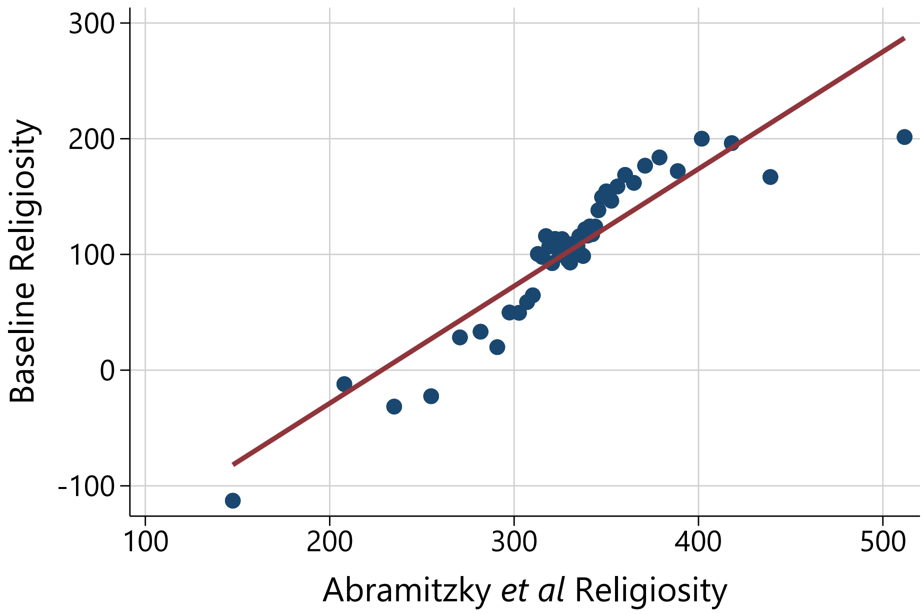


Figure B.4: Correlation Between WW1 and Influenza Deaths


*Notes*. Regression Coefficient = 0.170 (Std. Err. = 0.157). $R^2$=0.001.

*Notes*: This figure displays the correlation between WW1 and Influenza-related 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, along with the $R^2$ of the model. Data on WW1 deaths are from Ferrara and Fishback (2020).

Figure B.5: Correlation Between Abramitzky et al. (2016) Religiosity and Baseline Religiosity

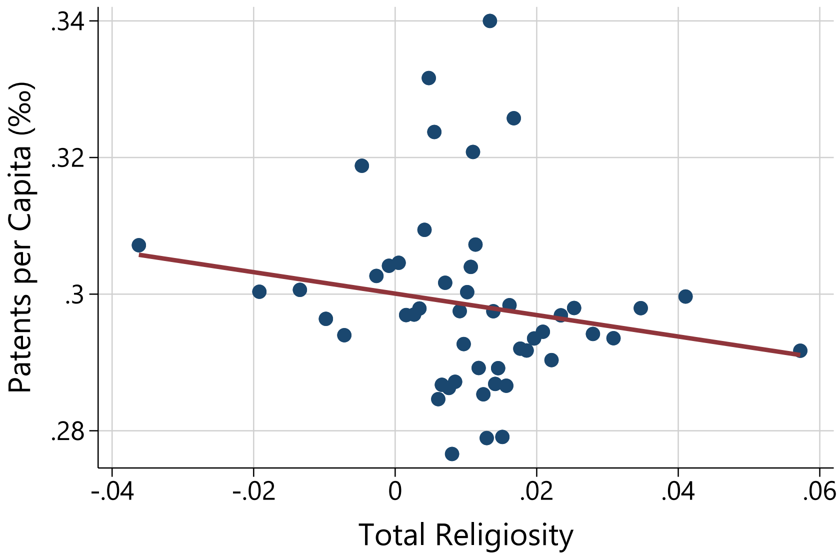



*Notes*. Regression Coefficient = 1.013 (Std. Err. = 0.022). $R^2$=0.390.

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

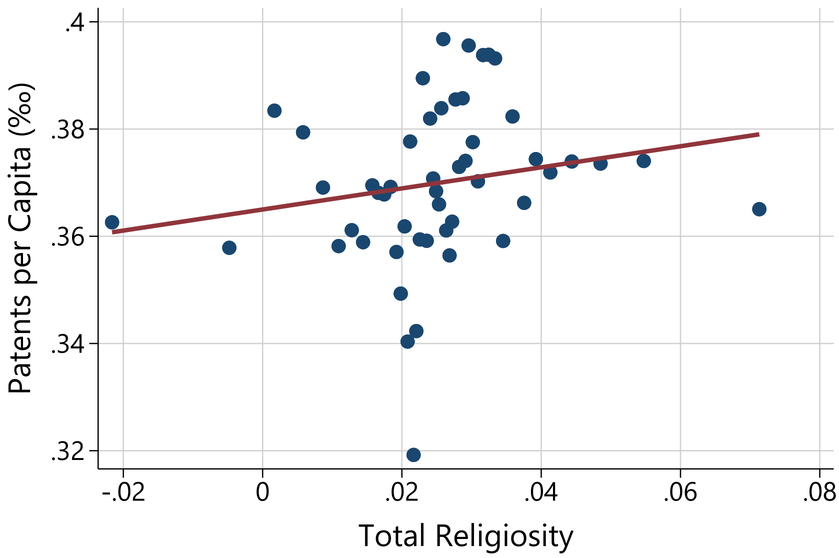
Figure B.6: Correlation Between Religiosity and Science

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




*Notes*. Regression Coefficient = -0.157 (Std. Err. = 0.066). $R^2$=0.868.

(b) After the Great Influenza Pandemic (1920–1929)




*Notes*. Regression Coefficient = 0.197 (Std. Err. = 0.084). $R^2$=0.915.

*Notes*: These figures display county-level binned scatter plots reporting the correlation between science–measured as patenting activity normalized by 1900-county population–and religiosity. The unit of observation is a county, observed at yearly frequency. Counties are weighted by their 1900-population. Religiosity is defined as described in section 3.1 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.

# C Summary of Robustness Analyses

Panel A: Religiosity

| | Exhibit | Topic | Description |
|---|---|---|---|
| 1. Is our name-based measure indeed capturing religiosity? | | | |
| i) | Table B.3, Table B.4, 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 Tables B.3, B.4, and 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 | 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. |
| iii) | Table B.6 | Religiosity scores without county FE | Our results may be sensitive to the specific way in which we compute religiosity scores. In Table B.6 we show that our findings hold when we drop county fixed effects from the measurement equation. Religiosity scores computed in this way reflect the stock of religiosity in a given county, rather than its change. |

| | | | |
|---|---|---|---|
| iv) | Table B.7 | 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 to using alternative frequency thresholds. |
| v) | Figure B.2 | Predicted vs. measured religiosity | We find a positive correlation between predicted religiosity and religiosity reported in the Census of religious bodies. |
| vi) | Figure B.5 | Saint/ biblical names | Our measure of religiosity is strongly and positively correlated with the one developed by Abramitzky et al. (2016). |
| vii) | Table B.9, Table B.10 | Alternative religiosity measures | Our results are robust to using three alternative indicators. In Table B.9 (cols. 1-2) we compute a religiosity score of first names from the Canadian census, which reports first names, as well as the religious affiliation, of individuals. In Table B.9 (cols. 3-5), we compute a religiosity indicator of first names based on the names of biblical figures and saints collected by Abramitzky et al. (2016). In Table B.10, we use data from the Census of Religious Bodies. |
| 2. Was the increase in religiosity caused by WW1? | | | |
| i) | Table B.3, Table B.4, 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.3, Table B.4, 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 (measure of an individual's background religiosity) as the dependent variable. As this variable is determined prior to the pandemic, a significant effect of the Influenza would suggest internal or international migrations correlated both with background religiosity and with the intensity of the shock. We do not find evidence in support of this mechanism. |

Panel B: Innovation

| | Exhibit | Topic | Description |
|---|---|---|---|
| **4. Is our measure indeed capturing innovation activity?** | | | |
| i) | Table B.11 | 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, where we include county-year observations with zero patents. |
| ii) | Table B.12, Table B.13 | Alternative measures of innovation | We use alternative transformations of the raw patent count (explained in the header of Tables B.12 and B.13). Results are quantitatively stable across specifications. |
| iii) | Table B.11 | 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. |
| **5. Was the increase in innovation caused by WW1?** | | | |
| i) | Table B.11, 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 deaths and influenza-related deaths are not correlated. |
| **6. Did the pandemic trigger an increase in high- (or low-) quality innovation?** | | | |
| i) | Table B.15 | 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. |

| | | | |
|---|---|---|---|
| 7. Who drove the increase in innovation: new inventors or existing ones? | | | |
| i) | Table B.14 | Extensive and intensive margin | The effect of the pandemic on innovation can be decomposed along two margins: increase of productivity of existing inventors ("intensive margin"), and entry of new inventors ("extensive margin"). |
| 8. Is the impact of the pandemic on occupational choice sensitive to the definition of young? | | | |
| i) | Table B.16 | Definition of young cohort | We define an alternative threshold that considers as treated all individuals aged 30 years old or younger in 1918. This compares to 25 years old in the baseline model. Results remain unchanged. |

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