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Mortality Decline**

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# Bismarck's Health Insurance and the Mortality Decline\*

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## Abstract

We investigate the impact on mortality of the world's first compulsory health insurance, established by Otto von Bismarck, Chancellor of the German Empire, in 1884. Employing a multi-layered empirical setup, we draw on international comparisons and difference-in-differences strategies using Prussian administrative panel data to exploit differences in eligibility for insurance across occupations. All approaches yield a consistent pattern suggesting that Bismarck's Health Insurance generated a significant mortality reduction. The results are largely driven by a decline of deaths from infectious diseases. We present *prima facie* evidence that diffusion of new hygiene knowledge through physicians was an important channel.

*Keywords:* health insurance, mortality, demographic transition, Prussia

*JEL Classification:* I13, I18, N33, J11

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*“Rarely, if ever, in modern history has a single piece of legislation had such a profound worldwide impact as the German Sickness Insurance Law of 1883 - the cornerstone of German health care policy for almost one century.”*

– Leichter (1979)

## 1 Introduction

Between the end of the 19th and beginning of the 20th century, the industrializing world experienced an unprecedented decline in mortality rates. A large literature emphasizes the importance of this mortality decline for the demographic transition and discusses potential consequences of changes in longevity for aggregate long-run productivity. In similar vein, the microeconomic view holds that improvements in individual health are associated with increases in lifetime productivity.<sup>1</sup> An ongoing debate in this literature concerns the historic forces driving the mortality decline. Proponents of the nutrition view (e.g., McKeown, 1979; Fogel, 2004) consider changes in income and associated improvements in the standard of living most crucial for the decline, whereas proponents of the public health view (e.g., Deaton, 2013; Preston, 1975; Cutler and Miller, 2005) consider directed investments in public health infrastructure and the application of public health knowledge as the driving forces.<sup>2</sup> One aspect of the mortality decline that has been less-well studied is the role of access to health care and the mechanisms through which changes in access affected health.

This paper investigates the role in improving health of the first-ever widely-implemented compulsory health insurance in the world, introduced in the German Empire in 1884, by Chancellor Otto von Bismarck (henceforth Bismarck’s Health Insurance or BHI). By extending coverage to large parts of the working population, the health insurance was a first move toward democratized access to health care. Subsequently, it acted as a blueprint for Germany’s current health system and served as a role model for many social insurance systems across the world. We exploit the fact that BHI became compulsory only for the blue collar worker population while other occupations’ access to health care remained unchanged. Our findings suggest a considerable decline in mortality due to the introduction of BHI. We

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<sup>1</sup>While microeconomic evidence typically confirms the existence of health-induced productivity gains, the relationship between improvements in health and aggregate productivity is subject to ongoing debate. Many studies focus on specific periods of major improvements in health and life expectancy to test this relationship (for some important contributions, see Acemoglu and Johnson, 2007; Ashraf et al., 2008; Bleakley, 2007; Hansen and Lønstrup, 2015; Jayachandran and Lleras-Muney, 2009; Lorentzen et al., 2008; Murtin, 2013; Weil, 2007, 2014).

<sup>2</sup>For recent surveys see Costa (2015) and Cutler et al. (2006).

further contribute to the discussion on the historical roots of the mortality decline by inspecting heterogeneity in the causes of death and by comparing the effectiveness of insurance benefits. According to our findings, BHI led to a considerable decline in airborne infectious disease mortality. Further results indicate that a substantial share of deaths were avoided due to individual interactions with physicians and medical treatment, lending support to our hypothesis that BHI extended access to new health knowledge which became an important driver of the historical mortality decline.

We follow a multi-layered empirical approach that accumulates in a stepwise manner evidence for the mortality effects of Bismarck's Health Insurance and the potential mechanisms at work. After inspecting the development of aggregate mortality of Prussia, the largest of the German states around the introduction of BHI, we investigate Prussia's mortality decline relative to the development in other Western European countries. Next, we exploit the fact that BHI became mandatory for blue collar workers only, but not for other occupations such as public servants. Newly digitized administrative data from Prussian districts allow us to compute annual occupation-specific mortality rates for the period from 1877 to 1900. We bring these unique panel data to a generalized difference-in-differences model, which compares the mortality trend of blue collar workers (treatment group) to the mortality trend of public servants (control group) while allowing for heterogeneous reform effects over time. Finally, to address concerns regarding potential selection into occupation, we employ county and district fixed effects models that exploit regional differences in the blue-collar workers share *before* the introduction of BHI to estimate the health insurance effects on mortality.

These various empirical approaches yield a consistent pattern suggesting negative effects of Bismarck's Health Insurance on mortality. Long-run time series data show that the introduction of BHI indeed coincides with the beginning of the mortality decline in 19th century Prussia. Moreover, while mortality decreased in all Western European countries toward the end of the 19th century, Prussia's mortality decline was particularly pronounced. Further difference-in-differences estimates based on occupation-specific mortality rates indicate that from its introduction in 1884 to the turn of the century, BHI reduced blue collar workers' mortality by 8.9 percent. The results are robust to allowing for heterogeneous effects of urbanization, the establishment of waterworks and the roll-out of sewerage. Common pre-treatment mortality trends across occupations corroborate the validity of the

identification strategy. Additionally, we find that BHI created substantial spillovers from the insured to their uninsured family members. The results are neither confounded by contemporaneous social reforms nor by improved working conditions for blue-collar workers nor by selection into the blue-collar sector after the introduction of BHI. Using detailed data on causes of death in a district fixed effects framework, we show that a large part of the decline is driven by a reduction of mortality due to airborne infectious diseases, especially tuberculosis. In addition, we find some significant but smaller responses of non-communicable disease mortality to BHI.

The well-known absence of effective medication for most diseases at the turn of the 20th century suggests that the decline in infectious disease mortality was driven by other aspects of the BHI. We argue that BHI was instrumental in extending access to multipliers of health knowledge to poor working class families, a group formerly unable or unwilling to afford consultation of physicians. This hypothesis complements earlier conjectures in the historical literature arguing that the insurance contributed to the mortality decline by providing its members with access to new knowledge regarding hygiene and transmission of infectious diseases (see [Condrau, 2000](#); [Koch, 1901](#); [Vögele, 1998](#)). Our claim resonates with the more formalized approach of [Mokyr and Stein \(1996\)](#) who build a model of consumer behavior in which households produce health for its members based on their priors of the causes of disease. These priors were radically altered by the germ theory of disease at the end of the 19th century when inputs to the health production function substantially changed. In order for scientific knowledge to affect household priors, a vehicle of transmission is crucial. The sickness funds encouraged their contract physicians to disseminate the newly gained knowledge of infectious disease transmission and launched information campaigns to facilitate the application of hygiene knowledge. We provide support for this interpretation by presenting evidence on heterogenous effects of insurance benefits. While expenditures for doctor visits and expenditures for medication are negatively related to mortality in a district fixed effects model, expenditures for sick pay are not. These findings lend *prima facie* support to the public health view rather than the nutritional view of the historical mortality decline.

More recent expansions of compulsory health insurance have been highly effective in increasing access to health care and reducing mortality, at least for specific subgroups of the population. In this respect, our findings are in line with studies on major expansions in health insurance coverage in the U.S., such as Medicare for

the elderly (see [Card et al., 2008, 2009; Finkelstein, 2007; Finkelstein and McKnight, 2008](#)) and Medicaid for the poor (see [Currie and Gruber, 1996; Goodman-Bacon, 2017](#)). Yet, attempts generalizing our finding need to consider key differences between these 20th century expansions and the introduction of BHI. Present-day health insurance schemes work in an environment of chronic diseases and benefit from the ability to provide health care by medical treatment. In contrast, BHI was established in an environment of infectious diseases that could not be treated effectively. As such, our setting might be more readily compared to the situation in developing countries, where an increasing amount of compulsory health insurance schemes are currently introduced ([Lagomarsino et al., 2012](#)). These schemes are introduced in an environment with a high prevalence of infectious diseases. Moreover, large parts of the population often lack adequate knowledge on hygiene and on how to avoid infections. Yet, in contrast to the reality of late 19th century Prussia, infectious diseases can in principle be treated in present-day developing countries although a lack of adequate institutions might create considerable frictions.

The remainder of the paper is organized as follows. Chapter [2](#) discusses the literature on the causes of 19th century's mortality decline and provides background information on Bismarck's Health Insurance. Chapter [3](#) introduces the historical Prussian district-level data that we use in the empirical analysis. Chapter [4](#) lays out the multi-layered empirical approach, presents the results and provides a set of robustness and validity checks. Chapter [5](#) concludes.

## 2 Historical and institutional background

### 2.1 The mortality decline

Germany's mortality and fertility decline at the end of the 19th century is considered to be fairly representative for the demographic transition of many European countries ([Guinnane, 2011](#)). Yet, Germany experienced higher levels of mortality and fertility than most other countries at the beginning of the 19th century. With respect to mortality, [Leonard and Ljungberg \(2010\)](#) depict a 'German penalty' which was evident in 1870, but vanished around 1900 when Germany's mortality had declined to the levels of other countries. In the period 1875 to 1905, life expectancy of a German child below the age of one increased from 38.4 to 45.5; for those between the age of 20 and 25 it increased from 43.2 to 47.8 ([Imhof, 1994](#)). Life expectancy in rural areas of Europe was considerably higher than in urban areas

until the beginning of the 20th century (e.g., [Kesztenbaum and Rosenthal, 2011](#)). At birth, the average Prussian boy living in the countryside expected to outlive his urban counterpart by five years ([Vögele, 1998](#)).

The precise factors that caused the observed mortality decline remain disputed. The empirical literature has attempted to provide various explanations, ranging from nutritional improvements and public investments in sanitation infrastructure to the diffusion of knowledge as a tool against the fight of disease transmission. Seminal contributions by [McKeown \(1979\)](#) and [Fogel \(2004\)](#) argue that improvements in living standards and nutrition were responsible for the mortality decline in 19th century Europe — a conclusion that was criticized by [Sreter \(1988\)](#).

In contrast to the views of [McKeown \(1979\)](#) and [Fogel \(2004\)](#), the literature broadly agrees that especially the ‘urban penalty’ was gradually removed through a range of investments in public sanitation infrastructure during the second half of the 19th century. Many recent studies provide evidence for major reductions in infant and adult mortality which are associated with improvements in sanitation infrastructure ([Hennock, 2000](#); [Meeker, 1974](#)). More specifically, improvements in the water supply ([Brown, 1988](#); [Ferrie and Troesken, 2008](#); [Beach et al., 2016](#)), water purification ([Cutler and Miller, 2005](#)), and sewerage systems ([Alsan and Goldin, 2015](#); [Kesztenbaum and Rosenthal, 2017](#)) strongly reduced mortality from water-borne diseases.

[Cutler et al. \(2006\)](#) and [Leonard and Ljungberg \(2010\)](#) argue that medical treatments hardly contributed to the mortality decline before 1914. However, effective medication was not entirely absent at that time. Indeed, smallpox vaccination became available already in 1796 and largely reduced infant mortality ([Ager et al., 2017](#); [Hennock, 1998](#)). In the first decades of the 19th century, many German states introduced compulsory vaccination against smallpox. While this was not true for Prussia, smallpox vaccination and re-vaccination were a widespread practice and eventually became compulsory for all children as part of the Imperial German Vaccination Law in 1874. In 1885, rabies vaccine became available. In 1891, the first drugs against diphtheria emerged. A range of antiseptics and anesthetics were widely used. More generally, the end of the 19th century saw a significant progress in chemistry that allowed identifying effective ingredients of medicinal plants and producing new chemical drugs.

The medical sector expanded substantially at the end of the 19th century. From 1870 to 1890, the number of medical students in Prussian universities more

than tripled (from 2,600 to 8,724), whereas the number of students of all other faculties roughly doubled. As a result, by 1890, the medical faculty boasted the largest number of students of all faculties. Similarly, data from the Prussian Occupation Censuses show that the share of people working in the health sector grew by about 40 percent from 1882 to 1907.

Overall, scientific medical knowledge deepened considerably towards the turn of the century ([Mokyr and Stein, 1996](#)). The widely held belief, prevalent since medieval times, that diseases were transmitted through bad smells (*miasmas*) gradually faded out in the course of the 19th century. It was replaced by scientific findings identifying the role of bacteria as crucial transmitters of diseases. Major breakthroughs in epidemiology and bacteriology occurred during the second half of the 19th century (for details see [Easterlin, 1999](#)). These included the well-known discoveries of water as a transmitter of Cholera by John Snow and William Budd as well as numerous discoveries in bacteriology by Robert Koch, Louis Pasteur, Ignaz Semmelweis and others.<sup>3</sup> Advances in bacteriology had an impact on established knowledge across all types of infections related to waterborne and airborne diseases. In fact, [Mokyr \(2000, p. 15\)](#) recognizes germ theory to be “one of the most significant technological breakthroughs in history.” However, mere identification of the root cause of infections was insufficient to cure the sick, especially when no remedies were available. All that physicians could do was to “educate patients on hygiene” ([Thomasson, 2013, p. 177](#)).

The role of knowledge diffusion in improving health has recently gained the attention of economists. [Deaton \(2013\)](#), for instance, argues that upward shifts of the Preston curve are driven by the *application* of new knowledge. All around the developed world, new knowledge regarding hygiene was disseminated via health care centers, congresses and public information events at the turn of the 20th century. Nordic countries were particularly progressive in diffusing knowledge through well-child visits and health care centers. [Wüst \(2012\)](#) finds that Danish infant mortality was reduced due to home visiting programs which helped towards diffusing knowledge on nutrition as well as highlighting the positive health effects of breastfeeding. [Bhalotra et al. \(2017\)](#) show that a similar Swedish program managed to reduce chronic diseases of infants by providing nutritional information, non-financial support and monitoring. According to findings by [Bütikofer et al. \(2016\)](#), Norwe-

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<sup>3</sup>The role of hygiene, as an important tool to prevent infectious diseases in hospitals, became generally appreciated in the 1880s, twenty years after the death of Ignaz Semmelweis, the pioneer of modern antisepsis ([Murken, 1983](#)). Yet, it was not before Alexander Fleming discovered penicillin in 1928 that antibiotics became widely known as drugs that could fight bacteria.

gian health care centers also contributed to better long-term economic outcomes by providing home visits during the first year of life. [Ogasawara and Kobayashi \(2015\)](#) find similar effects when evaluating a program for inter-war Tokyo. [Hansen et al. \(2016\)](#) investigate the introduction of tuberculosis dispensaries in Denmark in the early 20th century. In the absence of a cure, these dispensaries disseminated knowledge related to the transmission of tuberculosis to patients and their families as well as to the general public significantly decreasing tuberculosis mortality.<sup>4</sup>

Public interventions also include the introduction of health insurance across European countries at the turn of the 20th century. According to [Vögele \(1998\)](#), health insurance funds might have contributed to the penetration of new knowledge and health education in the German Empire.<sup>5</sup> [Guinnane \(2003, p. 45\)](#) supports this hypothesis by noting that the insurance sickness funds played a major role in strengthening the role of physicians as advocates of hygiene. [Kintner \(1985\)](#) argues that contracted physicians and midwives represented a major source for disseminating information of the health effects of breastfeeding to pregnant women.<sup>6</sup> In addition, [Tennstedt \(1983, p. 461\)](#) contemplates that insurance funds fostered prevention of illness transmission by introducing new rules and benefits for workers' families, workers' hygiene and lifestyle, the workplace, and the employers' responsibilities. Yet, empirical evidence for the role of early health insurance programs for the mortality decline is still scarce.

[Winegarden and Murray \(1998\)](#) provide empirical evidence that health insurance coverage across five European countries was associated with mortality reductions in the periods before World War I.<sup>7</sup> They find that a ten percentage point increase in the insured population results in a mortality reduction of 0.9 to 1.6 per 1,000 people. While issues of endogeneity in insurance take-up are likely to remain unresolved in this study, it provides an interesting yardstick to our findings.

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<sup>4</sup>[Condran and Crimmins-Gardner \(1978\)](#) argue that 'similar' information campaigns in U.S. cities at the end of the 19th century played a minor role in reducing tuberculosis-driven mortality.

<sup>5</sup>[Vögele \(1998, p. 199-208\)](#) lists a range of potential channels through which BHI might have been able to increase health, including preventing families to fall into poverty due to sick pay, increasing access to doctors and hospitals, or by allowing the state to systematically educate and control the covered population with respect to their attention to health issues.

<sup>6</sup>According to [Kintner \(1985\)](#) and [Kintner \(1987\)](#), breastfeeding was more widespread in Prussia compared to the south of Germany; whereas some cities such as Baden or Munich saw an increase in breastfeeding, breastfeeding in the capital Berlin massively declined between 1885 and 1910. However, unfortunately, comprehensive data on breastfeeding are missing before 1910. It has been argued that improvements in the quality and supply of cow milk were only marginal in improving infant mortality conditions ([Vögele, 1998](#)). Legal regulations regarding the quality of milk did not become an issue in the German Empire until 1901.

<sup>7</sup>This finding is supported by [Strittmatter and Sunde \(2013\)](#) showing that the reduction in mortality due to the introduction of public health care systems across Europe translates into positive effects on growth in income per capita and aggregate income. Their estimates are based on data from twelve European countries, excluding Germany due to a lack of data availability.

[Bowblis \(2010\)](#) extends this study to eleven countries to study the effect of health insurance on infant mortality. He speculates that health insurance reduced mortality by “educating people about the benefits of clean houses, not re-using dirty bath water, washing hands, and isolation of sick family members from the rest of the household” ([Bowblis, 2010](#), p. 223). While these articles exploit across-country, over-time variation, our empirical setup exploits *within*-country, over-time, across-occupation variation. This setup allows us to flexibly control for general mortality trends within a country at the end of the 19th century. Moreover, we will take several steps to explore the potential channels of the effects.

## 2.2 Bismarck’s health insurance

The Compulsory Health Insurance Act of 1883 constituted the birth of Germany’s social security system. Bismarck’s Health Insurance was the first of the three main branches of the German Social Insurance System, followed by the Accident Insurance Act (1884) and the Disability/Old-age Pension System Act (1891).<sup>8</sup> Being the first ever implemented compulsory health insurance scheme in the world, it acted not only as a blueprint for Germany’s current health system but it also served as a role-model for many subsequent health insurance systems.

Chancellor Otto von Bismarck’s decision to introduce the compulsory health insurance was a reluctant reaction to mounting upheavals among the working class. The Industrial Revolution led to increasing social tension between the rising working class and the political and economic elite. The new Socialist Worker’s Party of Germany gained support among the lower strata of the population and became a threat to the political stability of conservative dominance in the German Reich Parliament. Against this backdrop, the health insurance reform was a *mass bribery* for Bismarck to win over votes from the socialist party and the worker unions ([Rosenberg, 1967](#)). Furthermore, it disburdened public funds by shifting the cost of poor relief to workers and employers. The Reichstag approved the law on May 31, 1883, against the votes of the Social Democrats who argued that this social reform would not really improve the workers’ situation ([Tennstedt, 1983](#)).

From December 1st, 1884, BHI was “compulsory for all industrial wage earners (i.e. manual laborers) in factories, ironworks, mines, ship-building yards and

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<sup>8</sup>[Scheubel \(2013\)](#) provides an excellent overview of Bismarck’s social security system. [Fenge and Scheubel \(2017\)](#) show that the introduction of the disability and old age pension system reduced fertility, while [Guinnane and Streb \(2011\)](#) provide evidence for moral hazard effects of the accident insurance. [Lehmann-Hasemeyer and Streb \(2016\)](#) find that Bismarck’s social security system as a whole crowded out private savings.

similar workplaces” (Act of June 15, 1883, see [Leichter, 1979](#)). Contributions were earnings-related, amounted to an average of 1.5 percent of the wage<sup>9</sup> and were paid jointly by employers (one-third) and employees (two-thirds). Other occupations, including public servants, farmers, domestic servants, day-laborers or self-employed were not eligible for BHI.

Benefits of BHI included free routine medical and dental care, prescribed medicine, incidental care for up to 13 weeks and treatment in hospitals for up to 26 weeks. In addition, the insurance provided maternity benefits encompassing free medical attention and a cash benefit (*Wochenhilfe*) for three weeks after giving birth. In case of death of an insured worker, the insurance paid a death grant to the worker’s family. Moreover, the insured were eligible to receive sick pay amounting to at least 50 percent of the average local wage for 13 weeks. Note that the Act only specified maximum contributions and minimum benefits. Moreover, individual funds were legitimized to extend free health care to family members. Thus, the individual sickness funds had “considerable discretion to set specific benefits and contribution levels” ([Leichter, 1979](#), p. 123).

The health insurance system was administered in a decentralized manner by local sickness funds (*Krankenkassen*). Generally, we can distinguish between six types of sickness funds. Where possible, Bismarck built upon previously existing organizations such as the building trade, the miners (*Knappschaften*), the guild and various industrial sickness funds.<sup>10</sup> This saved both time and state resources and was sensible from a political perspective because it respected the guilds’ and unions’ position as insurance providers for their members. In addition to these four types of funds, two new types were established: these included the local funds (*Ortskrankenkassen*) and the parish funds (*Gemeindekrankenkassen*) whose task was to insure all eligible workers not covered by other funds. These two new funds attracted the lion’s share of the newly insured workers after the 1884 reform. Indeed, 59 percent of all insured individuals were insured in either local or parish funds by 1905.

After issuing the Act in 1883, municipalities and other institutions had more than a year of preparatory time to set up the insurance funds. Yet, the very early period of BHI did not pass without frictions. Employers did not report their

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<sup>9</sup>Contributions were confined to a maximum range of 3 to 6 percent of the wage.

<sup>10</sup>Only few workers from minor industries were covered by these particular insurance funds; most of them were members of the *Knappschaften* which provided insurance against accident, illness, and old age from 1854 (see also [Guinnane and Streb, 2011](#)).

workers to the funds, workers opted to remain in pre-existing funds with lower benefits, collectors of insurance premia returned drunk and lost their lists.<sup>11</sup> The literature typically argues that workers were unenthusiastic about the insurance and concerned about employer's resentments regarding their share of contributions. As a result, some workers preferred to buy insurance from voluntary funds (*Hilfskassen*) that did not require employer contributions and employers preferred to hire workers with such insurances (Tennstedt, 1983, pp. 318-322). Initially, around 40 percent of the targeted workers took up insurance. In the following years, this share gradually increased, also due to more rigorous inspection.

Figure 1 depicts the share of the health insured in the total population over time.<sup>12</sup> Pre-1885 insured are either voluntarily insured or clustered in very few industries providing compulsory health insurance such as mining. The data suggest only slight increases in the insured population until 1876, the latest available pre-BHI data point, after which the insured population surged from 3 percent to 8 percent in 1885. The subsequent accelerated increase in insured population is likely due to several reasons, including the increased uptake after initial frictions in recording the eligible workers, an expansion in the blue collar worker population due to ongoing industrialization, and a stepwise extension of BHI towards white collar groups (*Angestellte*). The steady increase in the share of insured after 1885 suggest that any effects of BHI are likely to become stronger over time.

Historical accounts suggest that being covered by BHI increased the demand for health goods and services. The insured consulted physicians far earlier and more frequently than the uninsured and it is argued that a large share of the newly insured would not have been able to afford consulting a physician in the absence of BHI (Huerkamp, 1985, p. 202).<sup>13</sup> Contracted physicians often received a lump sum payment of 2 Marks per insured from the insurance funds, irrespective of the treatment frequency. Since the insured did not bear the immediate costs, they increasingly made use of consultation hours. Soon, the stereotypical doctor's complaint that patients came for consultation only when it was too late turned into complaints that patients came in for petty indispositions (Huerkamp, 1985, p. 201).<sup>14</sup> Moreover, BHI became a key driver of the increased utilization of hospital capacity in

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<sup>11</sup>Based on the occupation census of 1882, officials in Dresden were expecting 45,000 workers from 8,665 firms to be liable for compulsory insurance. By mid 1885, only 30,000 workers were registered and 3,000 employers had yet to report their workers. Similar compliance rates were reported from Leipzig (Tennstedt, 1983, p.319).

<sup>12</sup>Table A1 in the Appendix shows the exact numbers of insured and total population of Prussia over the years.

<sup>13</sup>The chairman of the Imperial Insurance Agency Tonio Bödiker argued that less than half of the worker's families would have consulted a doctor before the introduction of the compulsory insurance (Huerkamp, 1985, pp.207-208).

<sup>14</sup>This notion is supported by contemporary sources suggesting that only half of the consultations justified a period of sick leave (Huerkamp, 1985, pp.202).

the 1890s ([Spree, 1996](#)). [Vögele et al. \(1996\)](#) find half of the patients in Düsseldorf hospitals to be compulsory insured by 1901. The majority of inpatients were treated for non-communicable skin, eye and surgical diseases, i.e. treatment of injuries and wounds. Technological progress in antiseptics and improved sterilizing of wounds was highly supportive for successful treatment. Otherwise, the literature typically reports complaints regarding the insurance's main benefit – sick pay. In principle, sick pay constitutes an improvement to the previous situation in which workers received no income while being sick at home. However, workers were hardly able to subsist on half of the local daily wage and the historical accounts suggest that they preferred returning to work as soon as possible ([Ellerkamp, 1991](#)).

From a political perspective, contemporary supporters of BHI argued that the reform was costly but “bought social peace for Germany” ([Leichter, 1979](#), p. 124). However, there were also more critical voices arguing that Bismarck’s reform delayed the introduction of any major safety and health factory regulation ([Hennock, 2007](#)). Abstracting from the political arena, our subsequent analysis is the first study to quantify the contribution of BHI to improving health.

### 3 Data

To quantify the impact of BHI on mortality, we draw on unique administrative data from Prussia, the largest state of the German Empire. In 1885, Prussia’s territory covered roughly two thirds of the total area and population of the German Empire. The Royal Prussian Statistical Office is unique in having published annual death statistics by occupational group. We combine these data with Prussian population and occupation censuses. The resulting data set is additionally extended to include information on public sanitation infrastructure such as waterworks and sewerage. When further refining our analysis, we draw on heterogeneity in the causes of death and in sickfunds’ expenditures to provide evidence on potential channels through which the reform affected mortality. The period of observation for the main analyses covers the years from 1875 to 1905.

#### 3.1 Data on mortality by occupation

The *Preussische Statistik* ([KSBB, 1861-1934](#)), a series of statistical volumes published by the Royal Prussian Statistical Office, reports the number of deaths by

occupation for all 36 Prussian districts.<sup>15</sup> This information is consistently reported annually for 28 occupational groups from 1877. Deaths are reported for adult men and women, as well as for male and female children below fourteen years of age. Children and non-employed females are classified by the occupation of their father or husband, respectively.

We generate occupation-specific mortality rates building on the occupational structure given by the death statistics. We extract eighteen occupations that are consistently reported over time, following the classification of the Prussian Statistical Office into blue collar sector and public sector.<sup>16</sup> This categorization is particularly helpful for our analysis since it allows to compare mortality across occupational groups that differ in their eligibility for Bismarck's Health Insurance.

The death statistics are supplemented by occupation censuses that were exclusively conducted in the years 1882, 1895, and 1907 under the supervision of the Imperial Statistical Office ([KSA, 1884-1942](#)). The period under analysis is a period of rapid industrialization in Germany.<sup>17</sup> Accordingly, the occupational groups experienced differences in the growth of the working population leading to differences in the growth of the population at risk (see Table 1 for details). To take this into account, we generate mortality rates using population by occupation data from the occupation censuses. We were able to consistently match the eighteen occupations extracted from the death statistics to those reported in the occupation census. Gaps between census years were filled by linear interpolation and extrapolation to obtain estimates of the respective size of each occupational group.<sup>18</sup>

### 3.2 Data on public sanitation infrastructure and urbanization

We address concerns regarding changes specific to the urban environment coinciding with the introduction of the health insurance by supplementing our dataset with information on water and sanitation infrastructure as well as urbanization rates. The

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<sup>15</sup>In the Prussian administrative hierarchy the district (*Regierungsbezirk*) ranks above the county (*Landkreis*).

<sup>16</sup>Blue collar worker industries encompass thirteen groups listed as sector B in the statistics: mining and turf, minerals, metals, machinery, chemicals, fossil fuels, textiles, paper and leather, wood, food, apparel and cleaning, construction, and printing. Public servant groups encompass five categories listed as sector E in the statistics: public administration, military, church and education, health, and arts and entertainment.

<sup>17</sup>Note that this period is not a period of substantial warfare. The Franco-Prussian war of 1871 did not affect Prussian territory and civilians. The war created 30,000 veterans suffering from long-term disabilities and/or diseases. Even so, there is no evidence for the year 1884 constituting a crucial turning point in veteran's mortality.

<sup>18</sup>Different from the death statistics, the occupation censuses report occupation-specific numbers of working males and females but aggregate non-working family members into a single category. Thus, when calculating occupation-specific mortality rates we rely on the total occupational population (excluding servants). We tested the robustness of our results to replacing this denominator by the occupation- and gender-specific working population. The results are qualitatively similar. We further tested interpolation based on urban population growth, which does not require linear extrapolation to years prior 1882. The results are qualitatively similar.

literature discusses two drivers of change in urban mortality occurring at the end of the 19th century which are related to the provision of public sanitation infrastructure — waterworks and sewerage. We draw on [Grahn \(1898-1902\)](#) and [Salomon \(1906-1907\)](#) who report city level dates of the establishment of public water supply and sewerage respectively. Assuming that the entire city population benefited from the introduction of sanitation infrastructure, we calculate the county/district level share of the total urban population with access to waterworks and sewerage. The Prussian Statistical Office conducted population censuses including urban population counts every five years. These data, available from ([Galloway, 2007](#)), are linearly interpolated to generate annual estimates of the urban population and urbanization rates.<sup>19</sup> Depending on the specification, we are able to use county level data or aggregate the data to the district level.

### **3.3 Data on aggregate mortality, causes of death, and health insurance contributors**

In a range of specifications we draw on aggregate mortality rates for the period from 1875 to 1904 instead of using occupation specific mortality rates. These mortality rates are provided by [Galloway \(2007\)](#) for all Prussian counties. Due to the fact that several counties were split during administrative reforms, we aggregate the data to consistently reflect the borders of 1875, resulting in a constant set of 441 counties. When necessary, the data are aggregated to reflect the district level with 36 observations. Next to total mortality, the data include gender-specific mortality and infant mortality by legitimacy status.<sup>20</sup>

To gain insights into potential mechanisms through which BHI affected health, we digitized rich data on the causes of death from the *Preussische Statistik* ([KSBB, 1861-1934](#)). In particular, we draw on district-level information reporting the full universe of fatalities by the thirty distinct causes of death that were reported annually from 1875 to 1904. This wealth of data allows us, for example, to distinguish between deaths from waterborne infectious diseases such as Typhus, Typhoid fever, or Diarrhea, and deaths from airborne infectious diseases such as smallpox, scarlet fever, measles, diphtheria, pertussis, scrofula, tuberculosis, tracheitis, or pneumonia. Besides, the data provides us with information on the number of

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<sup>19</sup>The waterworks data is additionally extrapolated to cover the post 1898 period.

<sup>20</sup>We follow the standard approach to calculate crude death rates as the total number of deaths per year per 1,000 people. Infant mortality is defined as the number of deaths of children below the age of one per year per 1,000 live births. This applies for legitimate and illegitimate deaths and live-births respectively.

deaths due to accidents, deaths in childbed and deaths from non-infectious diseases such as cancer, edema, stroke, heart disease, brain disease, and kidney disease.<sup>21</sup> To study potential consequences on the demand for health services, we digitized district level hospitalization rates based on the annual number of treated patients and individual cases, reported in ([KSBB, 1861-1934](#)) for the period from 1880 to 1904.

In specifications with aggregate or cause-specific mortality as dependent variables we use the aggregate pre-BHI blue collar worker share as main explanatory variable. We calculate the county- and district-specific population share employed in blue collar occupations using the above mentioned occupation census of 1882. Alternatively, we draw on data recording the actual take-up of health insurance. [KSA, 1884-1942](#) provides annual district-level records on the number of health insurance contributors, which allows us to calculate the share of insured in the total population.<sup>22</sup>

Additional robustness checks rely on data from [Galloway \(2007\)](#) to calculate the district-level vote share of the workers' party SPD (and its predecessors) in the general elections of 1874, 1878, 1884, 1890, 1893 and 1898 to proxy changes in the regional strength of workers' movements and unions.<sup>23</sup> Moreover, we digitized district-level data from population censuses reported in [KSBB, 1861-1934](#) to generate variables that depict the age structure of the population during the period 1875 to 1904. In particular, we calculate the average age, the dependency ratio (0 to 14 and 65+ year olds over 15 to 64 year olds) and the population share of 0 to 14 year olds as well as the population share of 15 to 44 year olds. These variables aim at capturing changes in a district's age composition over time.

### 3.4 Data on health insurance fund expenditures

In an attempt to present *prima facie* evidence on the mechanisms at work, we draw on newly digitized annual data on the expenditures structure of insurance funds

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<sup>21</sup>Concerns regarding the quality of causes of death data from this period have been raised in the literature ([Kintner, 1999; Lee et al., 2007](#)). Note that it is possible that improved knowledge about diseases allowed registrars to better identify the accurate cause of death over the course of our period of observation. If regions with a higher share of insured were also regions where physicians or registrars were better able to identify the cause of death, we expect the residual category 'unknown cause of death' to show a stronger decline. Using 'unknown cause of death' as an outcome in a district fixed effects model similar to Equation 2, we do not find evidence of a systematic relationship with the blue-collar worker share in 1882. This finding indicates that there were no systematic improvements in the ability to identify the correct cause of death related to the introduction of BHI that could drive our findings on causes of death.

<sup>22</sup>Although this data is available for the entire German Empire, we confine our analysis to Prussia since our detailed mortality data are only available for the Prussian territory.

<sup>23</sup>These elections were chosen because they precede the first year of each five year interval to which we aggregate.

for the period from 1885 to 1904. [KSA, 1884-1942](#) reports district-level data from insurance funds balance sheets. Expenditures include categories such as sick pay, medication, hospitalizations, doctor visits, death benefits, maternity benefits and administration reported in German Mark, as well as the number of sick days. Using the annual number of insured, we calculate per capita expenditures by type.

## 4 Empirical Evidence

This section empirically analyzes the impact of BHI on mortality. To pin down the relationship of interest, we take a multi-layered approach. Bringing together evidence from various data sets with varying degrees of aggregation, we sequentially address a range of concerns regarding a causal interpretation of our findings of BHI on mortality. The sequence of specifications starts by comparing the mortality decline across countries using time-series data. We proceed with a disaggregated intention-to-treat difference-in-differences design that allows the inspection of occupation-specific changes in mortality rates related to the introduction of compulsory insurance for blue collar workers. In regional fixed effects analyses, we address selection issues exploiting pre-BHI variation in treatment eligibility at the district and county level, and provide further support against confounding factors. Finally, to explore the potential channels through which BHI affected mortality, we draw on causes of death statistics and sickness funds' expenditures data. Each subsection is structured to first lay down the econometric specification, then present the results and finally discuss advantages, concerns and drawbacks particular to the respective approach.

### 4.1 Time-series and Cross-country Statistics

We start our empirical analysis by inspecting the long-run development of mortality in Prussia from the early 19th to the early 20th century. Figure 2 plots the crude death rate, defined as the number of deaths per 1,000 inhabitants of Prussia over the period from 1815 to 1913. Mortality was rather volatile, due to higher prevalence of epidemics and war, until the early 1870s when the fluctuations notably ceased. However, it is not before the mid-1880s that we observe a distinct break in the long-run mortality trend. From 1885 to 1913, the crude death rate in Prussia declined from about 27 to about 17 deaths per 1,000 population, corresponding to a

substantial drop of almost 40 percent. Thus, we observe a remarkable coincidence of the introduction of BHI in December 1884 with the timing of the mortality decline.

At the end of the 19th century, mortality rates declined across all industrializing countries. Yet, the drop of German mortality rates was noticeably more accentuated than in other Western countries, which might be related to the introduction of BHI. This finding is established by investigating mortality rates across European countries in an international comparison.<sup>24</sup> In Figure 3, we plot the crude death rates for Prussia and various European countries against years from 1875 to 1913. To smooth the trends, we apply country-specific local regressions using a tricube weighting function (Cleveland, 1979) and a bandwidth of 0.15. Prussia's mortality rate was comparatively high at the time when Bismarck's Health Insurance was introduced. From 1884 to 1913, mortality rates declined across all countries. Yet, there is no other country in which the mortality decline was as pronounced as in Prussia. To highlight this fact, we plot the difference in the mortality rate of Prussia and every other country by year, while normalizing the respective mortality difference in 1884 to zero.<sup>25</sup> Figure 4 shows that the mortality decline for Prussia was indeed considerably stronger than the mortality decline in all other countries during this period.<sup>26</sup>

Although remarkable, these findings from simple time-series and cross-country statistics should not be interpreted as evidence for a causal effect of BHI on mortality. If Prussia experienced structural changes that other countries did not experience simultaneously and if these changes affect mortality and happen to coincide with the introduction of BHI, this might explain Prussia's comparatively strong mortality decline at the end of the 19th century. Therefore, in the remainder of this chapter, we will put together additional pieces of evidence to plausibly separate the effect of Bismarck's Health Insurance from that of other determinants of mortality.

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<sup>24</sup>The data come from a range of national sources that are collected and made available by the team of the Human Mortality Database, a joint project of the University of California, Berkeley (USA) and the Max Planck Institute for Demographic Research (Germany). For details, please visit <http://www.mortality.org> or [www.humanmortality.de](http://www.humanmortality.de)

<sup>25</sup>Again, we apply local regressions using a tricube weighting function and bandwidths of 0.8 to the left and 0.2 to the right of the cut-off year 1884 to smooth the time trends.

<sup>26</sup>The comparison also suggests a *less* pronounced mortality decline prior to 1884 in Prussia than in most other countries.

## 4.2 Difference-in-differences: eligibility by occupation

### 4.2.1 Econometric specification

To investigate the role of Bismarck's Health Insurance for mortality within Prussia, we proceed by exploiting the fact that BHI, introduced in December 1884, was mandatory for blue collar workers but not for other occupations. This constitutes a natural setting for a reduced form difference-in-differences model, in which we compare the mortality trend of blue collar workers (treatment group) to the mortality trend of a control group.

Two characteristics qualify public servants as our preferred control group. First, similar to blue-collar workers, public servants are likely to live in urban areas and thus experience the same structural changes to their living environment. Second, public servants did not become eligible for compulsory health insurance before 1914. Prussian civil servants were however eligible for continuation of salary payment during illness and a pension in case of disability or old age. These benefits were confirmed after German unification by the Imperial Law on the Legal Relationship with Public Servants of 1873. Yet, public servants did not receive benefits such as free doctor visits and medication. Most importantly for our identification assumption, their benefits were not subject to change in the period 1873 to 1914.

Our main specification is based on data aggregated over all blue collar and public servant subgroups and over four year periods.<sup>27</sup> Subsequently, we relax each respective type of aggregation. Exploiting differences in eligibility, we estimate a difference-in-differences model that can be expressed by the following Equation 1:

$$Death_{iot} = \alpha_{io} + \theta_{it} + \sum_{t=1877-1880}^{1897-1900} \beta_t (BlueCollar_{io} * T_t) + X'_{it} BlueCollar_{io} \gamma + \varepsilon_{iot} \quad (1)$$

$Death_{iot}$  is the average death rate of people with occupation  $o \in (BlueCollar, PublicServant)$ , measured in district  $i$  at period  $t \in (1877-1880, 1881-1884, 1885-1888, 1889-1892, 1893-1896, 1897-1900)$ .  $\alpha_{io}$  are occupation by district fixed effects accounting for any time-constant occupation-specific mortality differences between districts.  $\theta_{it}$  are district by period fixed effects that flexibly allow mortality

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<sup>27</sup>The use of four year periods is owed to the fact that the occupation-specific mortality was published from 1877 — eight years before BHI. We are thus able to create two pre-treatment periods and four post treatment periods. Results are robust to the choice of other period lengths and the use of annual data (see Section 4.2.4).

trends to differ across districts. These fixed effects pick up a range of shocks affecting the district-level health environment relevant for both occupational groups, such as overall improvements in nutrition due to variation in harvests and food prices, or differences in temperature especially affecting infant mortality.  $BlueCollar_{io}$  is a dichotomous variable that is unity for blue collar workers and zero for public servants. We interact this occupation indicator with period indicators  $T_t$  to flexibly allow the mortality difference between occupations to vary over time. The  $\beta_t$  coefficients measure an unbiased reduced form effect of BHI in the absence of time-varying unobservables differentially affecting blue collar workers' mortality and public servants' mortality.  $\varepsilon_{iot}$  is a mean zero error term. Standard errors are clustered at the occupation by district level to allow for serial autocorrelation within districts.

By letting  $\beta$  vary over time, we generalize the standard difference-in-differences model to allow for heterogeneous intention-to-treat effects over time. This makes particular sense in our setting in which we expect the mortality effects of BHI to expand gradually. At the same time, this specification allows us to perform a placebo treatment test. In particular, using the period from 1881-1884 as the reference period, we expect  $\beta_t$  to be zero in the pre-treatment years, suggesting that blue-collar workers and public servants followed the same mortality trend before BHI was introduced. Thus, this placebo treatment test would corroborate the validity of our identifying assumption, namely that the mortality of blue-collar workers and public servants follows the same time trend in absence of the treatment.

To further validate the empirical approach, in an extended specification, we introduce an interaction of the blue-collar worker dummy  $BlueCollar_{io}$  with a vector of time-varying district-level control variables  $X'_{it}$ . Public health interventions such as the construction of waterworks and sewerage in cities are among the most frequently cited explanations for decreased mortality in 19th century Europe and the U.S. (see [Alsan and Goldin, 2015](#); [Beach et al., 2016](#); [Ferrie and Troesken, 2008](#); [Kesztenbaum and Rosenthal, 2017](#)). Accordingly,  $X'_{it}$  includes the district's urbanization rate, the share of a district's urban population with access to public waterworks and the share of a district's urban population with access to public sewerage. Note that such time-varying district-level characteristics are already captured by the  $\theta_{it}$  in our basic specification as long as they affect both occupational groups equally. However, if their effects vary by occupation, they might still confound the estimates. Therefore, in the extended specification, we explicitly allow measures of public health infrastructure to have differential effects for blue-collar workers and

public servants. Furthermore, by allowing urbanization rates to differentially affect occupational groups, we account for the fact that city quarters with occupational clustering could be differentially affected by changes in population density due to city growth at the intensive margin.

#### 4.2.2 Main results

A first graphical depiction of the difference-in-differences setup is provided in Figure 5. Here, we plot the crude death rate of blue collar workers (treatment group, black solid line) and the crude death rate of public servants (control group, black dotted line) against years. The vertical line marks the introduction of BHI in 1884. In addition, the grey solid line depicts the counterfactual mortality trend of blue collar workers, i.e., the mortality trend followed in the absence of BHI, assuming that the actual mortality trend of public servants resembles an untreated mortality trend of blue collar workers. Throughout the entire period of observation, the level of blue-collar workers' mortality lies above the level of mortality of public servants. Prior to BHI, both groups follow approximately the same mortality trend. If at all, public servants' mortality is declining faster than blue-collar workers' mortality, which would rather bias downward the difference-in-differences estimate. Only after the introduction of BHI, the mortality of blue collar workers is falling more steeply than the mortality of public servants. This can most clearly be seen by the considerable departure of blue collar workers' actual mortality trend from the counterfactual trend. Blue collar workers' mortality declined substantially stronger than we would expect in absence of BHI. We interpret this graphic pattern as suggestive of a negative treatment effect of BHI on the mortality of blue collar workers.

In a next step, we bring the data to a regression framework and estimate the generalized difference-in-differences model of Equation 1. Column 1 of Table 2 reports the results from a basic specification, where we regress the crude death rate on the interactions of the blue-collar worker dummy and period fixed effects while controlling for occupation by district fixed effects and district by period fixed effects. The period immediately preceding Bismarck's reform, i.e., the period 1881-1884, constitutes the reference period. We find that blue-collar workers and public servants indeed followed a similar mortality trend in the years preceding BHI. This result provides supportive evidence for the common trend assumption of the difference-in-differences framework and thus corroborates the validity of the empirical approach. After 1884, the crude death rate of blue-collar workers first increases

as compared to the death rate of public servants. This short-lived deterioration of blue collar workers' health might be related to the adverse health environment of blue collar workers and the initial frictions of the BHI introduction.<sup>28</sup> For all subsequent periods, we observe highly significant negative effects that gradually increase in size. By the end of the 19th century, BHI had reduced the mortality of blue collar workers by 1.907 deaths per 1,000 individuals, i.e. by 8.9 percent (-1.907/21.519).

To account for occupation-specific urbanization effects, i.e., the crowding of factory workers into city quarters due to rapid city growth, we include an interaction of the time-varying urbanization rate with the blue-collar worker dummy as a covariate. The results from column 2 show a slight reduction of the point coefficients. Yet, the effects stay negative, statistically significant and economically meaningful. The same is true if we include occupation-specific interactions of access to waterworks (column 3) or access to sewerage (column 4) to make sure that the results are not confounded by occupation-specific heterogeneity in the effects of the roll-out of sanitation infrastructure. Across all specifications, the results point to a considerable negative effect of BHI on blue-collar workers' mortality, which increases over time.

Figures 6 and 7 provide further visual support against the concern that the roll-out of waterworks and sewerage confounds the health insurance effect. We plot the number of waterworks and sewerages established in Prussian cities per year as well as their cumulative distribution functions. Both, waterworks and sewerage coverage in Prussian cities clearly increases in the second half of the 19th century. However, we do not observe any conspicuous jumps in the roll-out around the introduction of Bismarck's Health Insurance in 1884 which could explain the absolute and relative mortality decline for blue-collar workers in the aftermath of the reform.

#### **4.2.3 Effect heterogeneity: men, women, and children**

So far, we have estimated an average reduced form effect of BHI eligibility on blue-collar workers' crude death rate. In order to obtain more information on the underlying components of this effect, we disaggregate the occupation-specific death rates to obtain separate death rates for men, women, and children. Mortality rates of children and non-employed females are classified by the occupation of their father or husband respectively. Table 3 presents the results of this exercise. We find a

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<sup>28</sup>However, the short-run increase in blue-collar workers' mortality is not robust across specifications and varies in magnitude with the choice of the reference period.

considerable part of the mortality decline to be driven by male blue-collar workers (column 2). At the same time, we observe a substantial decrease of child mortality (column 4), while the effect on females is somewhat smaller but also significantly different from zero (column 3).<sup>29</sup> For all three groups, the effects gradually increase over time.

Declining child mortality is one of the dominant features in the Epidemiological Transition and may be explained by changes in both nutritional and non-nutritional factors. Our findings indicate that children benefited massively from changes inflicted by the health insurance. There are three core channels that explain such findings but remain undistinguishable with the data at hand. First, since some funds extended treatment and medication to all family members, children might directly benefit from health care. Second, sick pay obtained by the insured parent might result in intra-family spillovers of insurance benefits. Sick pay stabilizes household income, facilitating continuous calorie intake and nutritional prospects of all family members, especially improving infant health (see [Subramanian and Deaton, 1996](#); [Case and Paxson, 2008](#)). Third, treatment and medication obtained by the insured parent might affect health of uninsured family members. Next to the possibility to share medication, patients could share knowledge on hygiene matters received during treatment by insurance physicians with all members of the household. In addition to behavioural changes of the insured, intra-family knowledge diffusion likely led to prevention of infectious disease transmission. Given the economic literature on early human capital development and the fetal origin hypothesis ([Douglas and Currie, 2011](#); [Deaton, 2007](#)), we expect children to respond stronger than spouses to such changes in the health environment, even if they are not themselves targeted by the reform.<sup>30</sup>

#### 4.2.4 Robustness checks

To gain a more nuanced view of the consequences of introducing BHI, we proceed by presenting results from a difference-in-differences model based on disaggregated annual mortality rates.<sup>31</sup> In this model, we also include occupation specific lin-

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<sup>29</sup>Note that results are qualitatively similar when using the occupation-specific population size by working males and females as denominator. Especially for the male population, this is arguably very close to capturing the insured population in the treatment group.

<sup>30</sup>Until the end of the 19th century, hygiene education was not part of schools' curricula, teachers were not particularly trained to educate pupils in hygiene issues, and doctors were not systematically integrated in the school system to examine pupils and inform parents (e.g., [Krei, 1995](#); [Leuhuscher, 1914](#); [Umehar, 2013](#)). Therefore, positive knowledge and health spillovers from pupils to their parents are rather unlikely.

<sup>31</sup>This basic version of the model excludes additional covariates.

ear time trends to mitigate concerns related to potentially diverging pre-treatment trends across treatment and control group. Figure 8 plots the annual difference-in-differences estimates using 1884 as reference year. The slight increase in mortality right after the reform remains insignificant in this specification. From 1886, we observe a mortality decline that gradually accumulates over time. Thus, this more fine-grained model with annual intention-to-treat estimates complements our previous findings. In fact, allowing for diverging pre-treatment trends increases the point estimates compared to those presented in Table 2.

Achieving an accentuated drop in mortality is easier if the pre-existing level is high. Even if the absolute decline is much larger for the group with initially higher mortality rates, the relative decline might be similar for both occupations. To rule out this concern, in another robustness check, we measure death rates on a logarithmic scale and run a log linear difference-in-differences model. The results confirm the established pattern (see Figure A.1 in the Appendix). The mortality decline for blue collar workers is steeper than the mortality decline for public servants both in absolute terms and in relative terms. Thus, this exercise corroborates the robustness of the findings with respect to changes in the specification.

#### 4.2.5 Threats to identification

##### *Bismarck's disability insurance and old age pension system*

Bismarck's disability insurance and old age pension system, the third pillar of his welfare system, was introduced in 1891, i.e., seven years after BHI. Using 1891 as the baseline year in the annual difference-in-differences model, we investigate whether the introduction of the third pillar constitutes a considerable trend break. The estimates show that blue-collar workers' relative mortality starts to decline well before 1891 and proceeds to do so thereafter (see Figure A.2 in the Appendix). Indeed, there is no particular pattern in the data suggesting that the year 1891 changed blue-collar workers' relative mortality in a meaningful way. This finding suggests that the disability insurance and the old age pension system do not confound the effects of Bismarck's Health Insurance.

##### *Factory regulation, working conditions, and wages*

One may be concerned that improvements in industrial working conditions coincide with the introduction of BHI. If this were true, these improvements might

lead to a stronger mortality decline for blue collar workers than for public servants and thus confound the BHI effects. Such a scenario is rather unlikely, as the period under analysis is a period of ongoing, rapid industrialization. The typical industrial job was physically extremely demanding, workers were remunerated via piece rate schemes, working hours were extensive, breaks were irregular and food intake during working hours insufficient (see, e.g., [Berg et al., 1989](#); [Paul, 1987](#); [Pietsch, 1985](#)). What is more, the relationship between workers and their employers was characterized by an authoritarian style, where employers disciplined employees using harsh measures (see, e.g., [Frevert, 1981](#)).

[Hennock \(2007, p.83\)](#) argues that the Trade, Commerce and Industry Regulation Act (*Gewerbeordnung*) of 1878, which barred children under twelve from working in factories, mines, foundries and stamping mills, marked “the end of the development of factory legislation in Germany for the next thirteen years.” Bismarck strongly opposed any further attempts aimed at improving working conditions since he considered new factory regulations to be detrimental to economic development. An anecdotal account is characteristic of Bismarck’s position: as the Pomerian factory inspector R. Hertel admonished Bismarck that there was a risk of explosion at his own paper factory in Varzin, he grumpily countered: “Where is danger ever completely ruled out?” ([Lerman, 2004, p.182](#)). According to [Hennock \(2007\)](#), Bismarck’s health insurance might even have delayed any major safety and health regulations in factories.<sup>32</sup>

Indeed, the 1880s saw only few improvements in workplace regulation. Federal regulatory reforms were minor and restricted to very specific industries.<sup>33</sup> It seems highly unlikely that such improvements have the ability to generate the aggregate BHI effects. Almost immediately after Bismarck resigned from office in 1890, regulations were passed to reduce maximum working hours for women. Similar legal restrictions in working hours for men were introduced only in 1919 (e.g. [Hennock, 2007, pp.125-128](#)). In 1891, an amendment of the Trade, Commerce and Industry Regulation Act (*Gewerbeordnung*) formally tightened regulations regarding safety at work. In line with earlier considerations regarding the introduction of the old age pension system, the absence of a particular trend break in mortality

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<sup>32</sup>Even if inspectors criticized employers for providing insufficient safety for their workers, employers had no incentives to comply because they could hardly be prosecuted ([Bocks, 1978](#)).

<sup>33</sup>In particular, these improvements consist of a regulation of the use of white phosphorus in the manufacture of matches (1884), a regulation for the manufacture of lead paints and lead acetate (1886) and for the manufacture of hand-rolled cigars (1888).

in 1891 mollifies concerns of confounding safety regulations (see Figure A.2 of the Appendix).

The absence of formal improvements in workplace regulation does not exclude the presence of informal improvements driven by changing incentives of employers to voluntarily improve working conditions. The benefits of a healthy workforce increase in the task-specific human capital of incumbent workers. We argue that the ratio of skilled to unskilled workers with little task specific human capital was likely relatively low at the end of the 19th century. Employers thus had little incentives to incur the marginal costs of improving working conditions to gain the marginal benefits of a more healthy but less disposable workforce. Due to the lack of employment protection, employers were legally unrestricted to substitute workers at any time. The historical narrative is mostly supportive of this view. Even by 1912, a metal workers' union reported the typical factory air condition to be extremely hot, dusty and toxic due to insufficient ventilation. Yet, employers refused to provide workers with free protective masks and goggles (see, e.g., [Deutscher Metallarbeiter-Verband, 1912](#), p.545). Next to such complaints, the metal workers' union reports the number of work-related accidents per 1,000 metal workers. This quantitative evidence clearly shows that the number of non-fatal accidents in the workplace increased considerably from 1886 until 1909.<sup>34</sup>

We gain additional insights regarding the potential changes in working conditions by exploiting the heterogeneity of our occupation-specific mortality data. Presume that workers in some industries were more successful in improving working conditions and/or employers in some industries improved working conditions voluntarily (further assuming no related reductions in work-related accidents). As a result of such uncoordinated, decentral actions, we would expect working conditions to improve in different industries and regions at different points in time. To analyze whether this is the case, we resort to the difference-in-differences model as introduced in Equation 1 based on annual data. Yet, instead of the aggregate blue-collar worker mortality rate, we draw on mortality rates across individual blue-collar occupations. The results of this exercise are displayed in Figure 9.<sup>35</sup> Three findings are noteworthy: First, the negative mortality effect is not driven by one single (large) industry but systematically occurs across many industries. Second,

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<sup>34</sup>Below, we provide evidence from causes of death data confirming that the mortality decline is not driven by a reduction of workplace accidents.

<sup>35</sup>Similarly, difference-in-differences analyses by single provinces presented in Figure A.3 find the decline to be uniformly occurring across regions. Note that in this figure the province of Brandenburg contains the imperial city of Berlin and the province Rheinland contains Sigmaringen due to the external administration of the health sector.

the mortality decline occurs at the same point in time across industries, namely shortly after the introduction of BHI. Third, we do not find a substantial post-1884 mortality decline in the mining industry, the only sector that introduced *compulsory* health insurance prior to BHI in 1854 and therefore did not experience a fundamental change in health benefits during the period of observation. We consider these findings to be convincing evidence against a substantial role of working conditions in confounding the mortality decline for blue collar workers.

A related concern might be that blue collar wages grew more rapidly than public servants' wages thus confounding the estimates. To be precise, a relative increase of blue-collar workers' wages would only confound the estimates if the reason for the relative increase were improvements in the productivity of workers unrelated to BHI.<sup>36</sup> Unfortunately, there are no data available that systematically report annual wages by occupation *and* district. Even the existing wage data without spatial variation is typically heroically interpolated. The wage data reported by Hoffmann (1965) shows the largest wage growth between 1884 and 1900 to occur in the construction (43%) and the wood (39%) industries, whereas metals (23%) and textiles (23%) experienced considerably lower wage growth. If our findings were confounded by an income effect, we would expect the mortality decline to be more prominent in industries with the largest improvements in wages. Again drawing on heterogeneity across industries displayed in Figure 9, we find the mortality decline in low wage growth industries such as metals (5.2 deaths per 1,000) and textiles (3.4) to be similar or even significantly larger than in high wage growth industries such as wood (3.6) and construction (1.6) by 1900. Based on these, admittedly limited, comparisons, we find no indication of wages as a confounder of the BHI effect.

#### *Spillovers and selection*

As indicated above, positive spillovers within the family of the insured are very likely, especially if reductions of infectious diseases drive the mortality decline. Yet, since families do not live in isolation, we might in addition observe spillovers to untreated individuals outside the family. This might be particularly relevant in our setting. On the one hand, we prefer members of our control group to be as similar as possible to members of our treatment group, i.e., they should for example live in the same area. On the other hand, this implies that members of the control group potentially benefit from an improved disease environment. Spillovers from

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<sup>36</sup>A related but countervailing concern is that employers passed on their part of insurance contribution by decreasing wages.

blue-collar workers to public servants due to reduced risk of disease transmission imply that our estimates constitute a lower bound of the BHI effects on mortality.

Moreover, the data at hand do not allow a clear distinction between insured and uninsured people, neither in the treatment nor in the control group. We cannot exclude the possibility that public servants voluntary bought additional health insurance. At the same time, compliance of blue-collar workers can be incomplete due to frictions in particular during the early years of BHI. Consequently, there might be people in the treatment group who are uninsured and people in the control group who are insured.<sup>37</sup> Therefore, we interpret our approach as an intention-to-treat design that identifies reduced form effects of BHI eligibility on mortality. On the positive side, this approach rules out any issues arising from the endogenous nature of actual insurance take up.

While the intention-to-treat design rules out selection into insurance take-up, selection into occupations will bias the estimates. The key concern is that the data do not allow us to fix assignment to treatment to a date before the introduction of BHI. Instead, we observe the mortality rate of the current occupational population at each point in time. This gives rise to concerns about systematic selection into treatment, i.e. people selecting into blue collar occupations whose health characteristics are systematically different from those in the treatment group prior to the introduction of BHI. If an increasing amount of young and healthy people from rural areas migrate to cities to pick up an industrial occupation, the average age structure of blue collar workers might change, leading to lower aggregate mortality in the treatment group, which results in an upward bias of the BHI effect. On the other hand, it might be the case that necessity drives poor and unhealthy rural agricultural workers into blue-collar occupations, which would downward-bias the BHI estimates. A similar downward bias occurs if the new workers from rural areas are prone to suffering under the dismal living conditions of urban working-class quarters and fall sick leading to higher mortality in the treatment group. Thus, the direction of the bias arising from selection into the industrial sector is a priori unclear.

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<sup>37</sup>Moreover, within both occupational groups we cannot distinguish between dependent workers and the self-employed who would not be captured by the mandatory nature of BHI.

### 4.3 Fixed Effects: Pre-Reform Differences at the County Level

In this section, we aim at resolving the discussed spillover and selection issues using an alternative empirical specification. This specification allows us to estimate the effect of BHI on the full population, capturing all potential spillovers. At the same time, this specification enables us to exclude problems from selection into treatment eligibility by holding fixed the treatment group at a point in time before the introduction of BHI.

#### 4.3.1 Econometric specification

To this end, we use a county fixed effects model and compare the mortality trend of counties with a high share of blue-collar workers at the time BHI was introduced to the mortality trends of counties with a lower share.<sup>38</sup> This model can be described by the following Equation 2:

$$Death_{it} = \alpha_i + \theta_t + \sum_{t=1875-1879}^{1900-1904} \beta_t (BlueCollar_{i,1882} * T_t) + X'_{it} \gamma + \varepsilon_{it} \quad (2)$$

$Death_{it}$  is the average death rate of county  $i$  in period  $t \in (1875-1879, 1880-1884, 1885-1889, 1890-1894, 1895-1899, 1900-1904)$ .  $\alpha_i$  are county fixed effects capturing unobserved time-invariant heterogeneity between counties, and  $\theta_t$  are period fixed effects that flexibly account for general time trends.  $BlueCollar_{i,1882}$  is the share of blue-collar workers in county  $i$  in year 1882. We hold this variable constant at its 1882 level and interact it with period indicators  $T_t$ . The omitted reference period is 1880-1884. Thus,  $\beta_t$  captures any period specific associations between the share of blue-collar workers in 1882 and the outcome variable. By holding the share of blue-collar workers constant at the 1882 level, we avoid any issues due to potentially systematic selection into the industrial sector after the introduction of BHI.  $X'_{it}$  is a vector of time-varying county-level covariates, including the urbanization rate and the share of population with access to sewerage and waterworks.  $\varepsilon_{it}$  is a mean-zero error component. Standard errors are clustered at the county level to account for serial autocorrelation within counties.

In order for  $\beta_t$  to identify reduced form intention-to-treat effects of BHI on mortality, we rely on the assumption that there are no time-varying unobserved

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<sup>38</sup>A county is the administrative unit below the district. In her borders of 1867, Prussia consists of 441 counties with an average area of less than 800 square kilometers.

determinants of mortality that are correlated with the share of blue-collar workers in 1882. In other words, counties with a high and counties with a low share of blue-collar workers in 1882 follow the same mortality trend in absence of the treatment. Again, we provide evidence in support of this assumption by performing a placebo treatment test. In particular, we analyze whether  $\beta_t$  is indeed zero in the pre-treatment years. Additionally, we will employ a placebo treatment group to test the validity of this alternative approach.

#### 4.3.2 Main results

In column 1 of Table 4, we document that the established pattern of results can also be found using a basic version of the county fixed effects model described in Equation 2. Again, the reduced form effect of Bismarck's Health Insurance increases over time. In the average county (with a blue-collar share of 7.4 percent in 1882), BHI reduced mortality by roughly 0.9 deaths per 1,000 inhabitants, or 3.4 percent, measured at the turn of the century. The insignificant placebo treatment effect in the pre-treatment period corroborates the validity of this alternative empirical approach. In column 2, we add the urbanization rate, the share of population with access to waterworks, and the share of population with access to sewerage to control for changes in urbanization patterns and the roll-out of public health infrastructure. Adding these controls does not change the findings qualitatively.<sup>39</sup> Yet, the reduced form effects become slightly smaller as waterworks and sewerage pick up part of the reduction in mortality over time.

In columns 3 to 7 of Table 4, we disaggregate the outcome variable to analyze the effects of BHI on males, females, and infants separately. We find statistically significant negative effects for males and females after 1885. Infant mortality is defined as deaths of children during their first year of life per 1,000 births. For such infants, the post-BHI coefficients are large and negative but only marginally significant. Yet, there is considerable heterogeneity related to the legitimacy of infants. Once we distinguish between legitimate infants (column 6) and illegitimate infants (column 7), we find highly significant negative effects for the former but not for the latter. In the average county (with a blue-collar share of 7.4 percent in 1882), BHI reduced mortality of legitimate children by roughly 5.5 deaths per 1,000,

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<sup>39</sup>Findings are qualitatively similar if we interact the urbanization and public infrastructure variables with time dummies to allow for differential effects of these covariates over time (results available upon request).

or 2.4 percent, measured at the turn of the century.<sup>40</sup> Thus, mortality reductions due to BHI are only found for infants that most likely live in households with both parents present. The lack of BHI-induced reductions for illegitimate infants might be explained by the lack of intra-family diffusion of insurance benefits for this group of infants: if a single mother is not a blue collar worker, the absence of a father leaves the family without access to insurance benefits. Finally, note that in those cases where the pre-treatment interaction is significant, the sign is always negative, implying increasing mortality of blue collar workers. Consequently, if we accounted for these deviating trends in the pre-treatment period, the treatment effects would become even larger. In further unreported regressions, we observe significant negative effects of BHI on the mortality of individuals younger than 15, and of individuals aged between 15 and 44.

Column 8 tests the hypothesis that differences in a counties' pre-BHI share of public servants (e.g. a placebo treatment group) create a similar pattern of results as the blue-collar workers share. We reject this hypothesis finding only very small and insignificant coefficients for the relationship between the 1882 public servants share and changes in mortality. Consequently, we can rule out that our treatment indicator picks up mortality trends common to other occupational groups.<sup>41</sup>

#### 4.3.3 Robustness checks

In a next step, we move the specification to the district level since this level of regional aggregation allows us to use new data on the share of individuals registered with health insurance funds. Columns 1-5 of Table 5 replicate the earlier county-level findings using district-level data. Columns 6-10 substitute the share of blue-collar workers with the share of individuals registered with a health insurance fund as the treatment variable of interest. Similar to the previous specification, we hold the share of health insured constant at the earliest possible year in 1885, right after the introduction of BHI, and interact it with year dummies.<sup>42</sup> The fact that results from both treatment variables are qualitatively similar (comparing columns 1-5 and 6-10), corroborates our interpretation that the blue collar worker mortality decline is in fact related to the health insurance. Comparing the estimates in more detail,

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<sup>40</sup>From 1880 to 1884, the average death rate of legitimate children across all counties was 228.2, while it was 368.5 for illegitimate children. Deaths of illegitimate children accounted for 11.5 percent of all deaths of children below age one.

<sup>41</sup>Note that running a horse race regression between the share of blue collar workers and the share of public servants yields similar results (available upon request).

<sup>42</sup>The choice of the first year is admittedly arbitrary to some degree. Since results are qualitatively similar when using other years, we decided use 1885 to exclude the most pressing concerns of reverse causation and selection.

we find that the point coefficients for the share of health insured (columns 6-10) are consistently larger than the point coefficients for the share of blue-collar workers (columns 1-5). This is exactly what we expect if the share of insured is a more direct measure for the treatment than the share of blue-collar workers.

A remaining concern might be that changes in the age composition of the population, for example due to selective migration, confound the BHI effect. In particular, if changes in a district's age composition are correlated with its share of blue-collar worker in 1882 or its share of individuals registered with a health insurance fund in 1885, the BHI estimates might be biased — at least as long as the changes in the age composition are not due to the mortality effect of BHI. To address this concern, we run district-level fixed effects regressions in which we control for the age composition in various ways. In addition to the urbanization, sewerage, and waterworks controls, we either include the mean age of the population, the dependency ratio (the number of people younger than 15 years and older than 65 years divided by number of people between 15 and 64 years), or the share of people younger than 15 years as well as the share of people between 15 and 44 year as additional control variables. Our findings are virtually unaffected by these additional robustness checks. For details, see [A2](#) in the Appendix.

The earlier concern of improvements in working conditions and wages is addressed here again – this time by controlling for a proxy for the strength of workers' movements and unions. In particular, we add the district-level vote share of the workers' party SPD (and its predecessors) as a control variable. This variable is expected to be highly correlated with spatial and intertemporal heterogeneity in the power of unions.<sup>43</sup> Our findings are virtually unaffected by the inclusion of this variable in columns 4 and 8.

Moreover, to investigate the utilization of health benefits, the district level analysis can be supplemented by hospitalization data. Since insurance benefits include free treatment in hospitals, we expect hospitalizations to increase comparatively more in regions with a larger share of blue collar workers. Using the number of treated patients per capita as an outcome variable in the district fixed effects model along the lines of Equation 2, we find that post-BHI hospitalization increases

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<sup>43</sup>From 1878 to 1890, the Anti-Socialist Law prohibited assemblies of social democratic groups, in particular of the Socialist Worker Party and related organizations such as unions. Consequently, the strength of unions was curbed until the ban was lifted and organized strikes were suspended. Due to the lack of information on union activity, we resort to using information on the election of social democratic politicians who were still allowed to run as individuals without party affiliation. The vote share of social democratic politicians (during and after the ban) is probably the single available and best proxy for the strength of worker movements and unions during this period.

in relation to the blue collar worker share. This finding is in line with [Spree \(1996\)](#) who argues that Bismarck's Health Insurance increased the utilized capacity of hospitals. Detailed results are available from the authors upon request.

#### 4.4 Exploiting data on causes of death and sick funds' expenditures

In this section, we use panel data on causes of death to provide further evidence against contemporaneous reforms and public health improvements biasing the results. Moreover, the causes of death data allow us to better investigate the channels via which BHI reduced mortality rates. Since the causes of death data are not recorded by occupation, we cannot exploit heterogeneity across occupations as given by the model in Equation 1. However, we can employ them in a regional fixed effects model along the lines of Equation 2.<sup>44</sup> Finally, we will present regression results using data on sick funds' expenditures that provide further insights into the mechanisms through which BHI affected mortality.

##### 4.4.1 Further evidence against confounding factors

The subsequent analysis relies on mortality data that distinguishes among a range of thirty causes of death, some of which allow addressing further concerns. In 1885, shortly after the introduction of BHI, Otto von Bismarck introduced the second pillar of the German welfare system, namely the accident insurance. If the aggregate mortality reduction was driven by a reduction of work accidents, this could raise suspicions regarding a confounding effect of Bismarck's accident insurance. The results when using deaths by accident as the outcome variable in our district fixed effects model are depicted in column 1 of Table 6. We do not find any negative association between the share of blue-collar workers in 1882 and changes in mortality due to accidents. In fact, the coefficients that cross the significance level of 10% are even positive. This implies that the overall mortality effect is not driven by a decline in deaths by accidents and unlikely to be related to the introduction of the accident insurance.<sup>45</sup> Instead, the results are in line with [Hennock \(2007\)](#) who argues that BHI delayed any major safety and health regulations in factories and with [Guinnane and Streb \(2015\)](#) who show that the accident insurance compensated workers for their losses due to accidents but did not result in a decline in work-related accidents.

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<sup>44</sup>Note that, due to changes in the original reporting of the causes of death after 1902, the last period only contains three years of data from the period 1900-1902.

<sup>45</sup>Note however, that, similar to the health insurance, the accident insurance provided sick pay and disability pensions to casualties of work accidents that likely improved living standards.

Despite controlling for the spread of waterworks and sewerage, there might be remaining concerns that nonlinearities in the improvements of public sanitation infrastructure confound the estimates. To mollify this concern, we take a thorough look at deaths by waterborne diseases. As indicated by the literature (i.e. see [Ferrie and Troesken, 2008](#)) we expect waterborne disease mortality to react to the introduction of water supply and sewerage. If our effects were driven by a reduction of waterborne-disease mortality, this would leave room for the BHI estimates being confounded by improvements of water supply. Yet, column 2 of Table 6 shows that there is no association between the share of blue collar workers in 1882 and deaths by waterborne diseases. Indeed, the coefficients are far from any conventional significance levels and even positive in all of the four post-treatment periods.

#### 4.4.2 Understanding the channels

Below, we will argue that one of the most important channels through which BHI affected the mortality of insured was through the diffusion of knowledge about disease transmission and avoidance of infection. Yet, there is an important difference in the transmission of waterborne and airborne diseases. While avoiding airborne diseases only requires knowledge about transmission channels, avoiding infection with waterborne diseases requires both knowledge about transmission and access to infrastructure. If individuals learned about the risk of contaminated water, they might boil drinking water, yet remain at higher risk of contact with contaminated water until proper infrastructure were installed. Consequently, investment in infrastructure is a necessary condition for the reduction of waterborne diseases. In contrast, knowledge transmission channels are sufficient for reducing the risk of infection with airborne diseases.

Indeed, while BHI had no effects on waterborne disease mortality, we find considerable negative effects on airborne disease mortality (column 3). Among airborne infectious diseases, tuberculosis was the most prominent at that time. For the group of 20 to 70 year olds, tuberculosis was responsible for about 30% of the deaths. A major breakthrough in fighting the disease was achieved in 1882, when Robert Koch identified the bacterium causing tuberculosis.<sup>46</sup> Since tuberculosis strongly affected the working age population, the sickness insurance funds were particularly interested in reducing the incidence of this disease. Yet, as the cure

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<sup>46</sup>Initially, tuberculosis was typically assumed to be hereditary since usually the entire family suffered from its symptoms.

was not developed until 1946, the focus was set on preventing infections. Robert Koch (1901, p. 575) himself argued that only preventive action could reduce tuberculosis mortality, including the diffusion of knowledge about its contagiousness to increasingly larger circles.

The hygienic situation of workers' housing became the center of attention of funds' interventions. A characteristic excerpt from Tennstedt (1983, p.458) mentions that *research by Preysing and Schütz found tuberculosis germs underneath 21.2% of 66 toddler's fingernails, which they absorbed by crawling on the floors of worker dwellings contaminated by sputum.* Such deficits in the hygienic situation were detected by sickness inspectors (*Krankenkontrolleure/Krankenbesucher*) who became instrumental in educating workers on hygiene. Initially hired to detect malingering via unannounced home visits, sickness inspectors eventually became mandated to monitor the curfew and medication intake of patients.<sup>47</sup> Moreover, physicians and inspectors specifically aimed at triggering changes in the hygienic behavior of all members of the household through particularly addressing women, who were typically in charge of care and food (Frevert, 1981).<sup>48</sup> The literature further indicates that insurance funds initiated publicly well-attended educational talks by insurance doctors (see Ewald, 1914).

Our results suggest that these measures were successful. As can be seen from columns 4 and 5 of Table 6, the share of blue-collar workers in 1882 is associated with a decline in the number of deaths by lung diseases and especially tuberculosis (and the related scrofula). These effects are meaningful in size, statistically significant and become gradually stronger over time.<sup>49</sup>

In column 6 of Table 6, we see that BHI also reduced deaths due to non-infectious diseases although coefficients are smaller in magnitude than for infectious airborne diseases. This result suggests that BHI did not exclusively affect communicable disease mortality and leaves room for explanations related to improved access to medication such as antiseptics and the provision of sick pay. However, a large share of cardiovascular diseases are ultimately triggered by infectious diseases such as simple influenza and we cannot exclude a certain overlap. Last but not least, we find some evidence for a reduction in maternal death in childbed (column 7) which

<sup>47</sup>In 1896, the municipality fund of Leipzig conducted 79,332 visits by voluntary inspectors and 149,899 visits by professional inspectors (Tennstedt, 1983, p. 451).

<sup>48</sup>According to Tennstedt (1983, p. 458) funds specifically considered to deploy female inspectors to give advice to *their uneducated sisters on how to ventilate and clean the apartment, curtains and other dust catchers.*

<sup>49</sup>As technology to detect the actual cause of death was limited, deaths classified as unspecified lung diseases may in fact have been tuberculosis and vice versa.

may be related to several channels including doctor visits, medical treatment, and sick pay.

In an attempt to distinguish between these channels, the remainder of this section presents *prima facie* evidence based on expenditure data by health insurance funds. The aggregate district level expenditure data from administrative sources distinguishes between several types of expenditures, i.e. expenditures for doctor visits, medication, hospitalization, sick pay, maternity benefits, death benefits, and administration. Figure 10 presents the evolution of sickness funds' expenditures per insured from 1885 to 1905. While we observe a steady increase in expenditures per insured over the full period of observation, the relative importance of the different kinds of expenditures is remarkably stable. Roughly a third – and thus the largest share – of total expenditures is due to sick payments for the insured. Expenditures for doctor visits make up for another 20-25 percent of total expenditures, followed by expenditures for medication, and hospitalization. Expenditures for administration, death benefits, and maternity benefits complete the picture.

If knowledge diffusion by physicians played a crucial role in reducing mortality, we expect mortality to decrease in association with an increase of expenditures on doctor visits. Similarly, if the progress in medical treatment were important, we expect mortality to decrease in response to positive changes in medication or hospitalization expenditures. On the other hand, if changes in income and nutritional status were crucial drivers of the mortality decline, we expect a more stable family income due to sick pay to reduce overall mortality rates. To compare the contribution of these alternative channels to the BHI induced mortality decline, we regress district level mortality rates on time-varying measures for the different types of expenditures per insured. To avoid most pressing concerns of reverse causality, expenditures are lagged by one year.<sup>50</sup> District fixed effects account for time-invariant heterogeneity between districts, while time fixed effects flexibly capture mortality trends common to all districts.

Column 1 of Table 7 provides estimation results that lend empirical support to a mechanism of knowledge diffusion through physicians. An increase in expenditures for doctor visits by one standard deviation decreases the mortality rate by 0.11 standard deviations. Similarly, an increase in medication expenditures decreases the mortality rate (column 2) by 0.16 standard deviations, suggesting that

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<sup>50</sup>The results are robust to changes in the lag structure. For reasons of consistency, control variables are lagged by one year as well.

advances in medical treatment were a similarly important channel. We do not find a significant response to changes in hospitalization expenditures (column 3).

Interestingly, mortality does not respond to changes in sick pay, the lion's share of sickness funds' expenditures; the point coefficient is small and far from conventional levels of significance (column 4). We interpret this finding to show that the sick pay provided under BHI was not instrumental in reducing (short-run) mortality rates through income smoothing. Sick pay might have been just insufficient to prevent deterioration of health status. In fact, sickness was the reason for requesting poor relief in 27% of cases as late as 1913. However, we find a negative coefficient on maternity benefits that reaches marginal significance (column 5). Paid maternity leave for three weeks could smooth family income after birth and give the mother the opportunity for breastfeeding, which both can have positive effects in particular on infant health. In column 6, we find a positive albeit insignificant relationship between death benefits and the mortality rate. This finding reminds us that such simple regression results are prone to endogeneity concerns. Especially in the case of death benefits reverse causality is likely to be present.<sup>51</sup> The model yields a negative coefficient of expenditures for administration on mortality in column 7. On the one hand we expect high administration costs per insured to be a sign of inefficient organization, e.g. related to fund size, resulting in higher mortality. On the other hand, high administration costs might be indicative of an extensive deployment of sickness inspectors monitoring the hygienic situation of the insured, arguably resulting in lower mortality. Column 8 presents results of a joint regression of mortality on all categories of expenditures. Due to concerns of multicollinearity, we group expenditures into 'health care' (doctor visits, medication, hospitalization), 'compensation' (sick pay, maternity benefits, death benefits), and administration. The results confirm the findings of mortality reductions due to health care but not compensation.<sup>52</sup>

An alternative interpretation of these results is that doctor visits are indicative of periods of sick leave. If infectious-disease mortality is a function of interactions with co-workers, staying at home reduces the probability of disease transmission. In column 9, we test this hypothesis using data on sick days per insured as the explanatory variable.<sup>53</sup> We do not find evidence for a significant

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<sup>51</sup>An alternative interpretation is that the death of an insured person is indicative of deterioration of the health prospects of the entire family due to income loss and reduced access to health care.

<sup>52</sup>Including maternity benefits, which were not exclusively pecuniary, into the 'care' category reinforces the findings.

<sup>53</sup>The data is only available from 1889, hence the lower number of observations.

relationship between the average length of sick leaves and mortality, which speaks against the argument that isolation from the public could have been a driver of the mortality reduction.

In sum, this exercise provides further evidence that providing people with access to physicians and health care played a crucial role for the mortality decline. New knowledge on hygiene provided by physicians and embodied in medication was thus more easily diffused to a population living under poor hygienic condition. This in turn resulted in the prevention of infections from airborne diseases such as tuberculosis. Contrary to the widely held belief, we do not find evidence in support of the view that sick pay contributed to a BHI-induced mortality decline, despite being responsible for roughly a third of sickness funds' expenditures.

## 5 Conclusions

Which role did health insurance schemes play during the time of demographic transition? This question is interesting in many respects. First, understanding the role of public institutions for demographic change and economic growth is crucial for the design of effective public policies. Second, from a demographic perspective, there has been a lot of work on determinants of the demographic transition. Yet, the role of the early health insurance schemes has been largely neglected so far. Third, from a historical perspective, it is interesting to understand the effects of Bismarck's Health Insurance as the first compulsory health insurance scheme in the world.

We use newly digitized Prussian administrative panel data to analyze the effect of the first compulsory health insurance in the world on mortality. In December 1884, Otto von Bismarck, Chancellor of the German Empire, introduced this health insurance as the first pillar of the Empire's social system. We amassed evidence from time-series data on long-run mortality in Prussia, international comparisons, and several difference-in-differences type frameworks that exploit the compulsory nature of the health insurance scheme for blue-collar workers. These various empirical approaches yield a consistent pattern assigning BHI a crucial role for Prussia's sharp mortality decline at the end of the 19th century.

In an intention-to-treat design, we find a mortality reduction of 1.91 deaths per 1,000 blue collar workers by the end of the century. In other words, blue collar workers' mortality decreased by 8.9 percent due to the introduction of Bismarck's Health Insurance. The negative mortality effects are robust to an extensive set of

robustness and validity checks. The estimates show that a large part of the reduction in mortality is driven by a decline in airborne infectious diseases. Additional evidence suggests that providing unrestricted access to doctors and medication was more important for the mortality decline than paying sick pay. This evidence is supportive of our hypothesis that Bismarck's Health Insurance provided households at the lower end of the income distribution with access to physicians and new knowledge on hygiene that they were unlikely to buy under a voluntary regime. While we do not find any evidence that sick pay was crucial for the effect of BHI on mortality, it might well be that sick pay affected other outcomes such as workers' long-run morbidity or their political support for Bismarck.

If we draw parallels between our findings and the findings in the literature on modern health insurance, i.e. from the U.S., we see that the reduced form effects under Bismarck were considerably larger compared to extending Medicare or Medicaid eligibility, although medication was less effective at Bismarck's time. Under the U.S. setting, [Card et al. \(2008\)](#) find that extending Medicare eligibility for the 65+ group led to a mortality reduction between 2 and 4 percent one year later. In another study, [Currie and Gruber \(1996\)](#) find that increasing Medicaid eligibility to low-income children is associated with a 3.4 percent mortality reduction. Yet, in the absence of clean information on insurance take-up, we would like to remain cautious when comparing reduced form intention-to-treat effects across various research designs.

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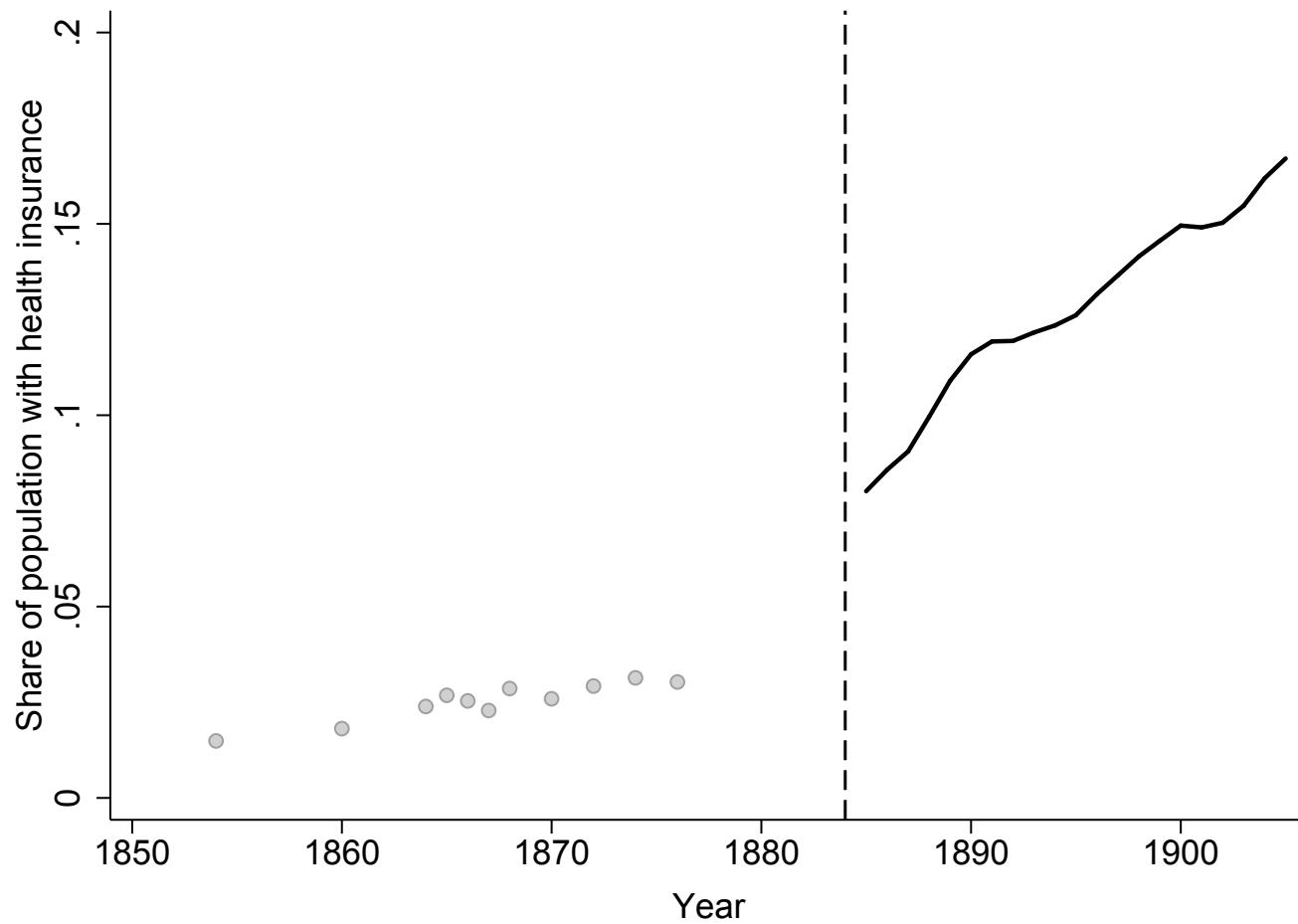


Figure 1: Expansion of Health Insurance in Prussia. The figure shows the share of health insured in the total population per year. Data refer to Prussia within its respective borders. Insurance benefits vary pre- and post-1884. The vertical bar indicates the introduction of BHI in 1884. Data source: pre 1884 - [Handelsministerium, 1876](#); post 1884 - [KSA, 1884-1942](#)

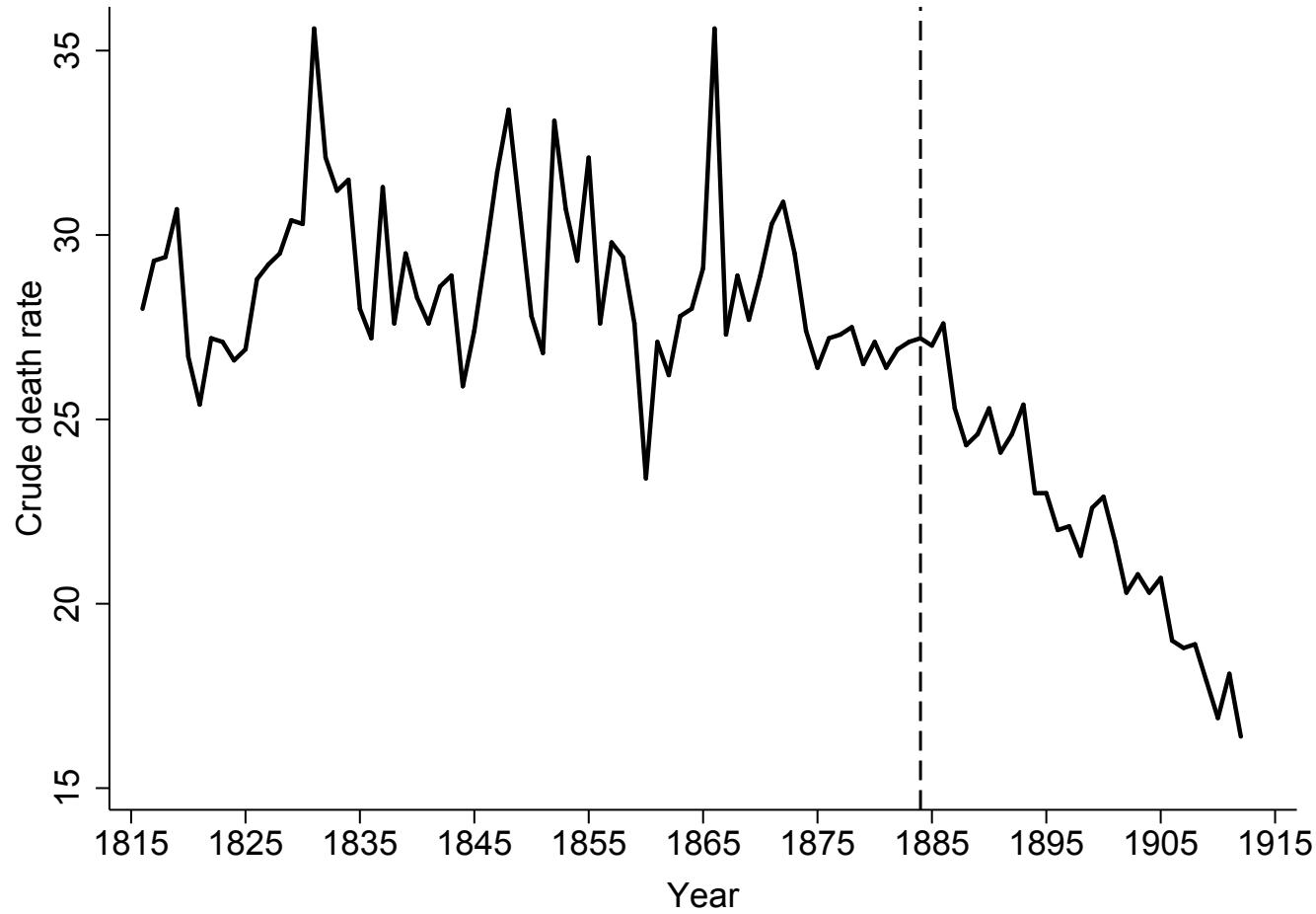


Figure 2: Long-run development of mortality in Prussia. *The figure shows the crude death rate defined as the total number of deaths per year per 1,000 people. The vertical bar indicates the introduction of BHI in 1884. Data source: Hohorst (1978)*

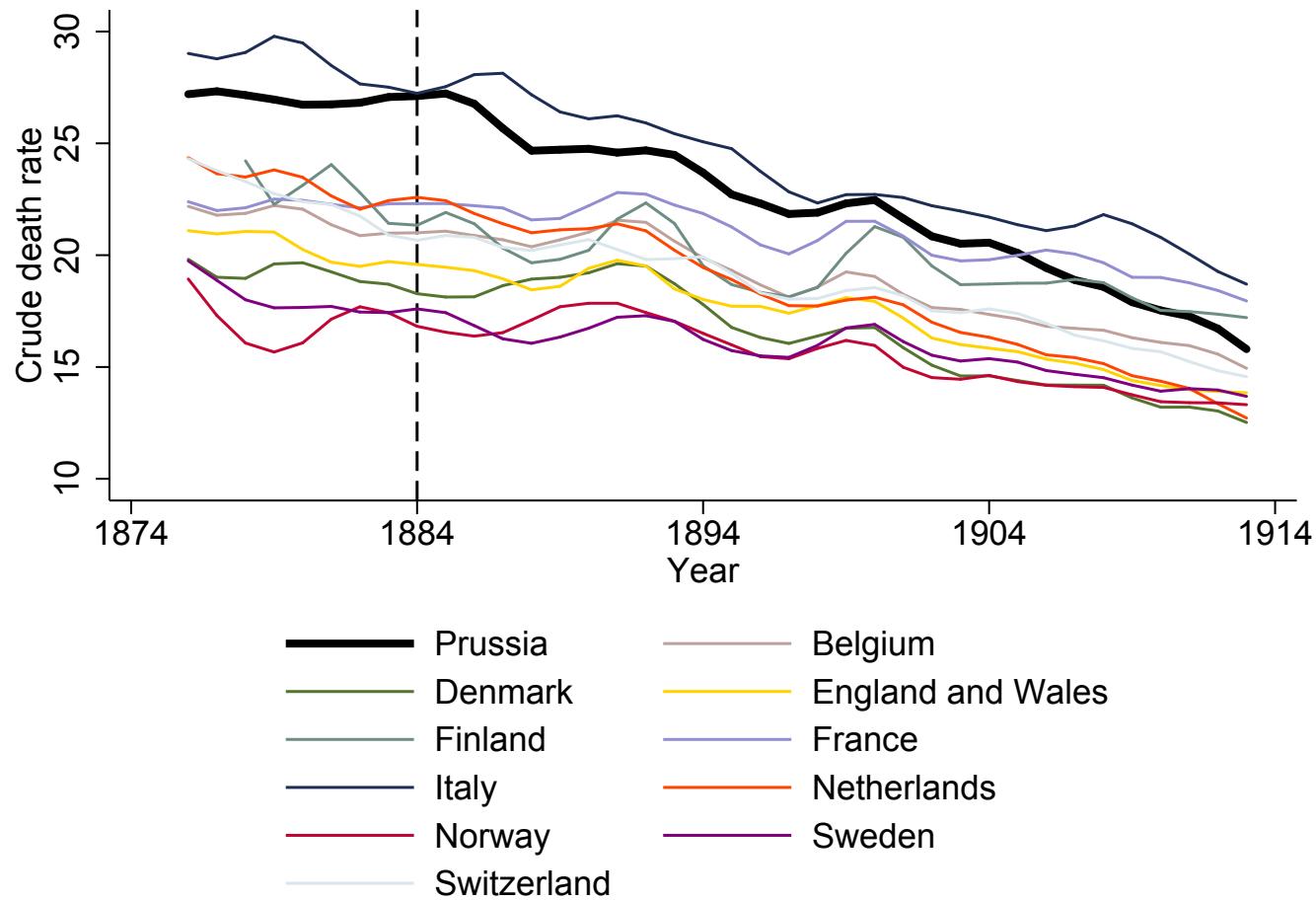


Figure 3: Mortality decline across Western European countries. *The figure shows the total number of deaths per year per 1,000 people for several countries. The trends are smoothed by applying country-specific local regressions using a tricube weighting function (Cleveland, 1979) and a bandwidth of 0.15. The vertical bar indicates the introduction of BHI in 1884. Data source: Human Mortality Database (<http://www.mortality.org>)*

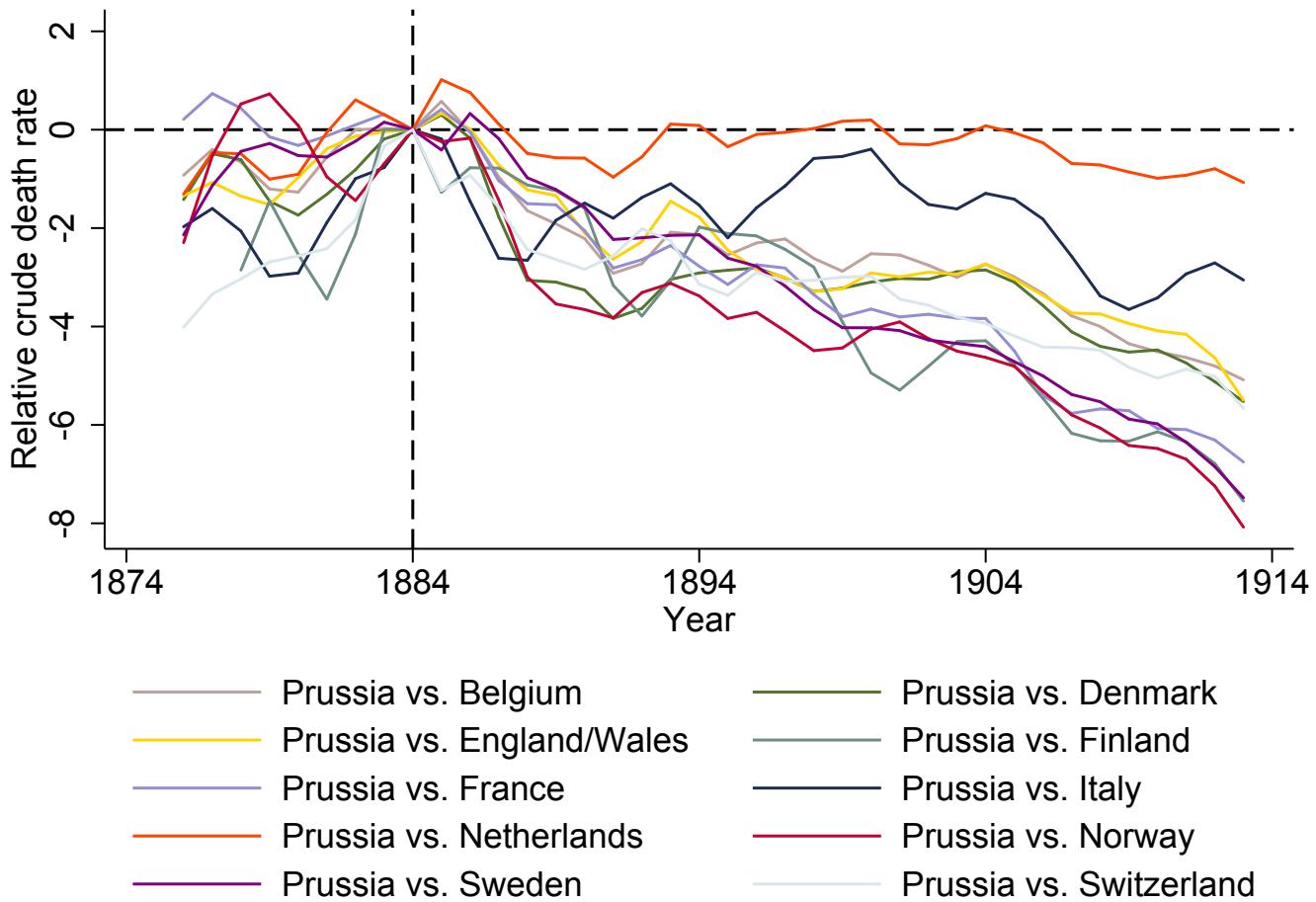


Figure 4: International mortality decline relative to Prussia. The figure shows the number of deaths per 1,000 people in Prussia minus the number of deaths per 1,000 people in the respective other country over time. The variable is normalized to zero in 1884. The trends are smoothed by applying country-pair-specific local regressions using a tricube weighting function (Cleveland, 1979) and bandwidth of 0.8 to the left and 0.2 to the right of the cut-off year 1884. The vertical bar indicates the introduction of BHI in 1884. Data source: Human Mortality Database (<http://www.mortality.org>)

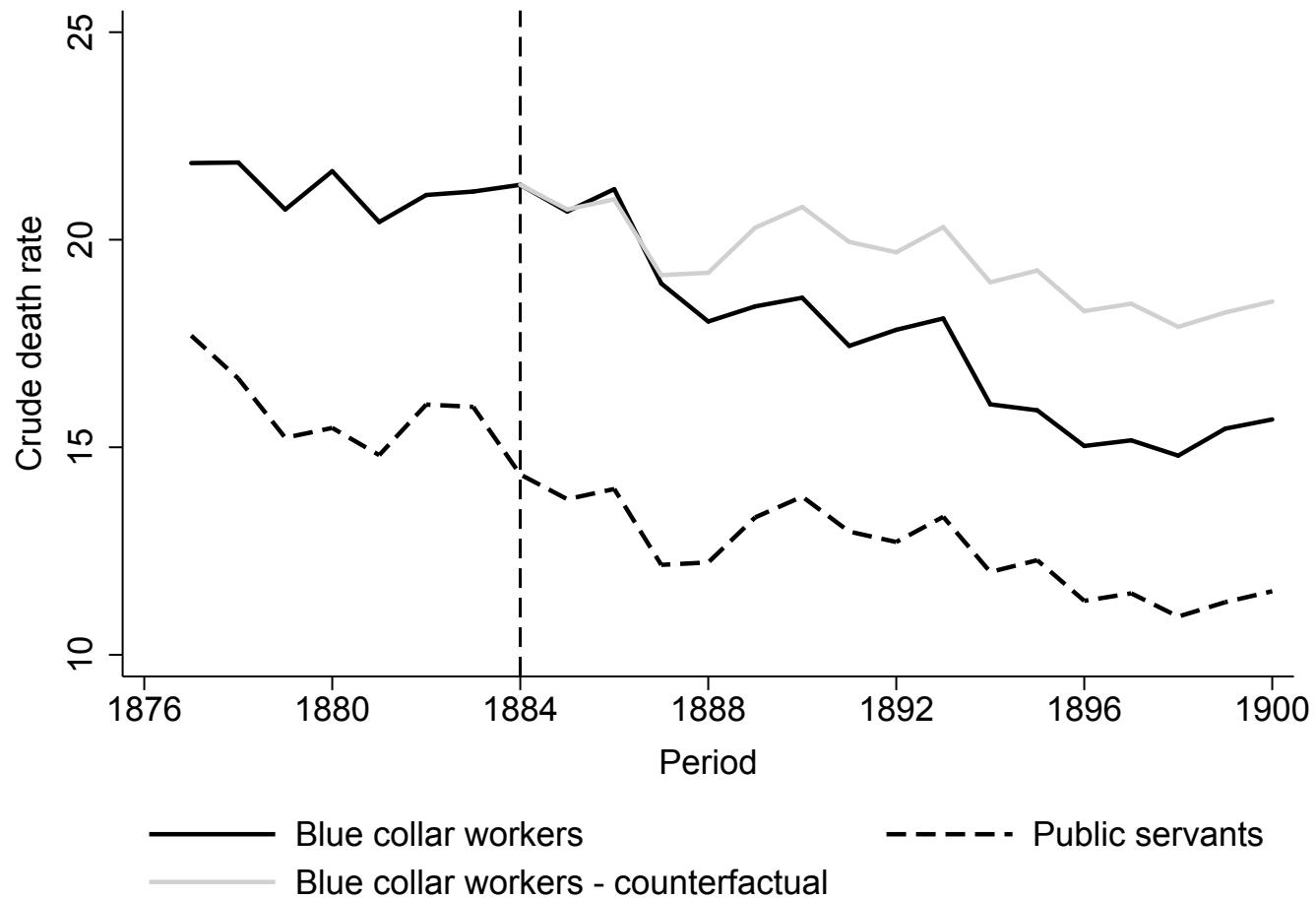


Figure 5: Crude death rates by sector (blue collar workers and public servants). *The figure shows the number of deaths per 1,000 people by sectoral groups of blue-collar workers and public servants. Following a difference-in-differences logic, the counterfactual is computed by parallel-shifting the trend of the public servants up to the blue collar workers level in 1884. The vertical bar indicates the introduction of BHI in 1884.*

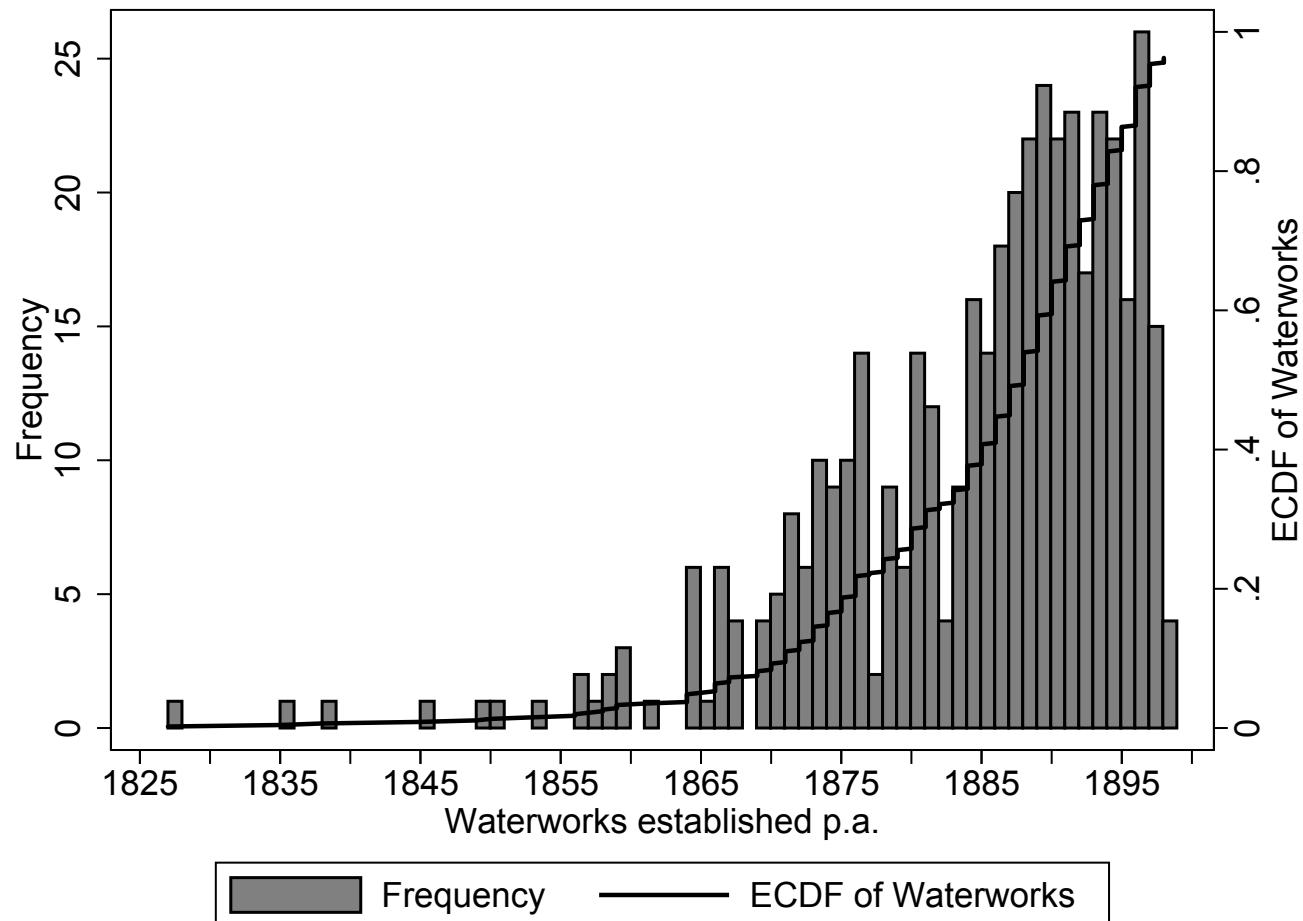


Figure 6: The roll-out of waterworks in Prussia. *The figure shows the roll-out of waterworks in Prussian cities over time. The bars indicate the annual number of new waterworks put into service. The line indicates the cumulative distribution function. Data source: Grahm (1898-1902)*

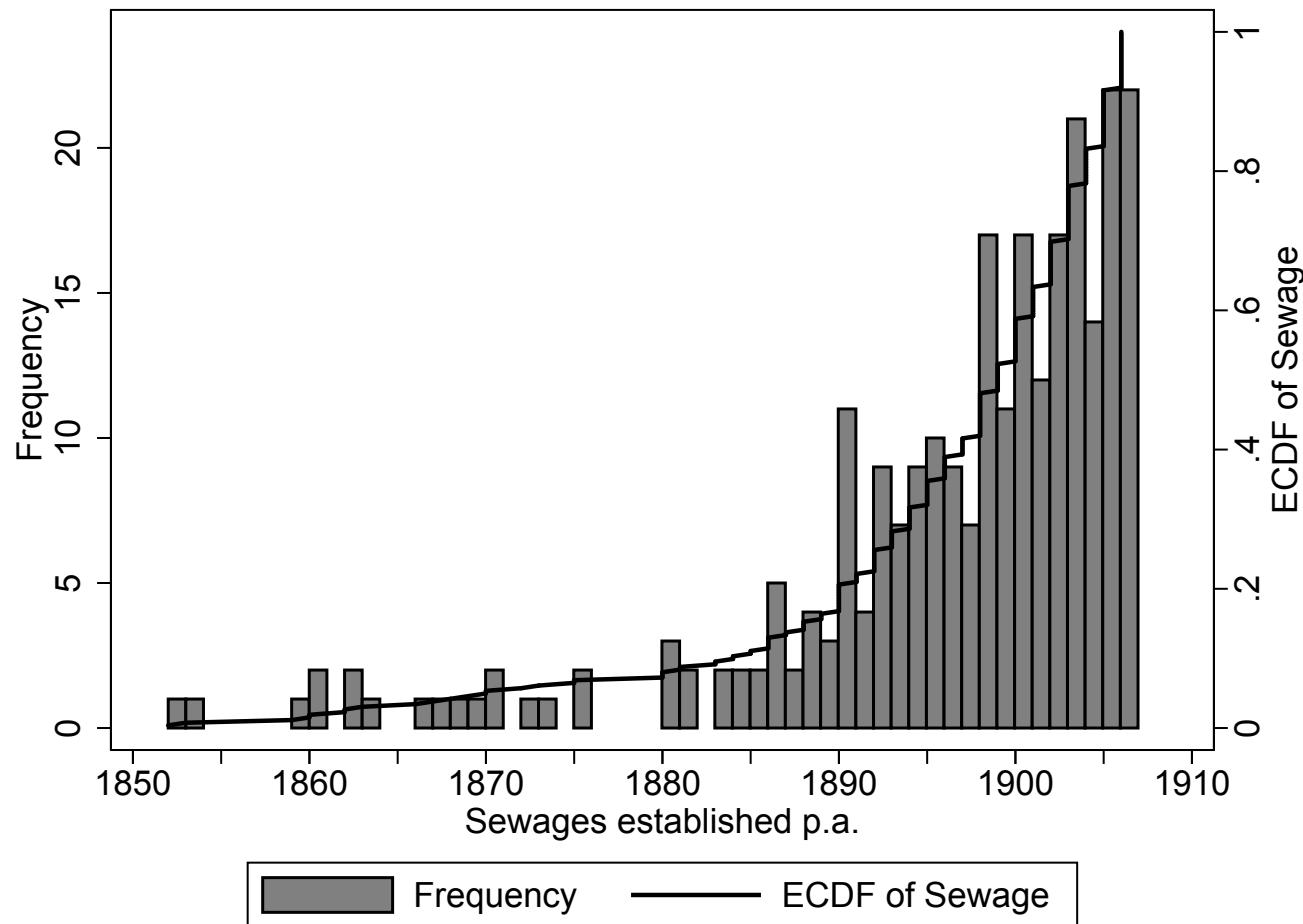


Figure 7: The roll-out of sewerage in Prussia. *The figure shows the roll-out of sewerage systems in Prussian cities over time. The bars indicate the annual number of new sewerage put into service. The line indicates the cumulative distribution function. Data source: Salomon (1906-1907)*

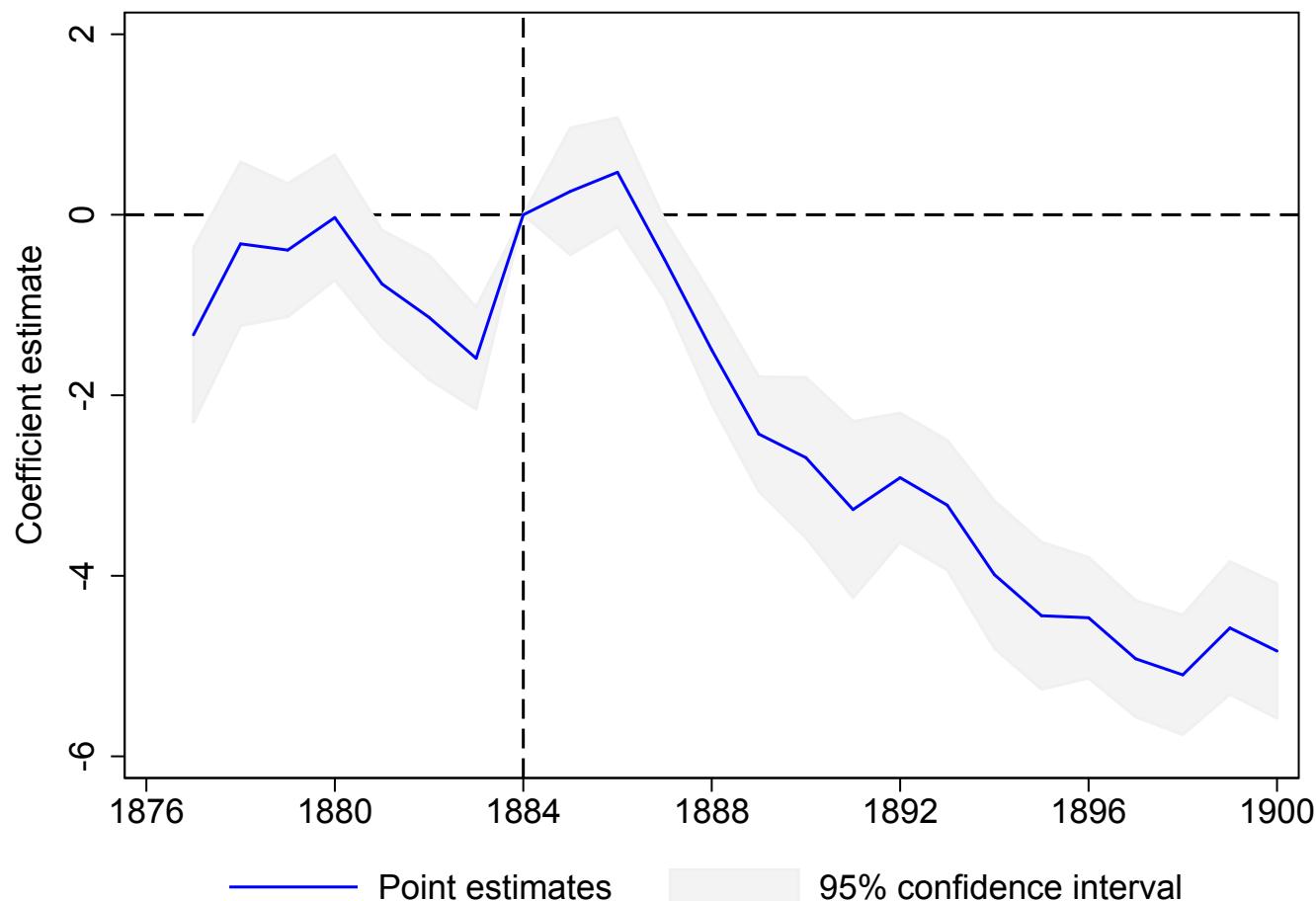


Figure 8: Flexible-DiD estimates. *The figure shows estimated coefficients and 95% confidence intervals from a difference-in-differences model similar to Equation 1 using annual data and accounting for pre-trends. The omitted year 1884 is marked by the vertical line and indicates the introduction of BHI.*

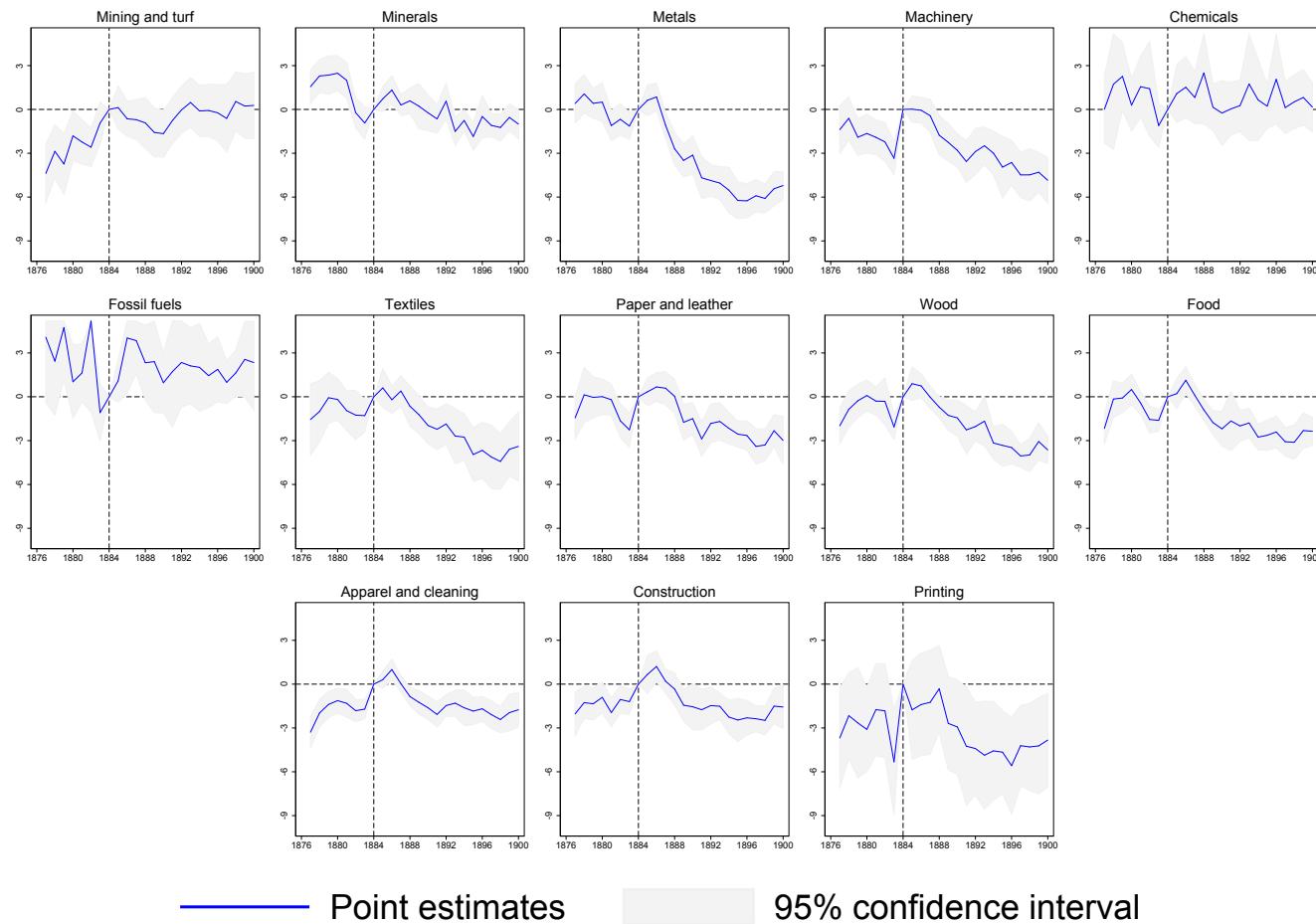


Figure 9: Flexible-DiD estimates by industry. *The figure shows estimated coefficients and 95% confidence intervals from a difference-in-differences model similar to Equation 1 using annual data and individual blue collar industries. The omitted year 1884 is marked by the vertical lines and indicates the introduction of BHI.*

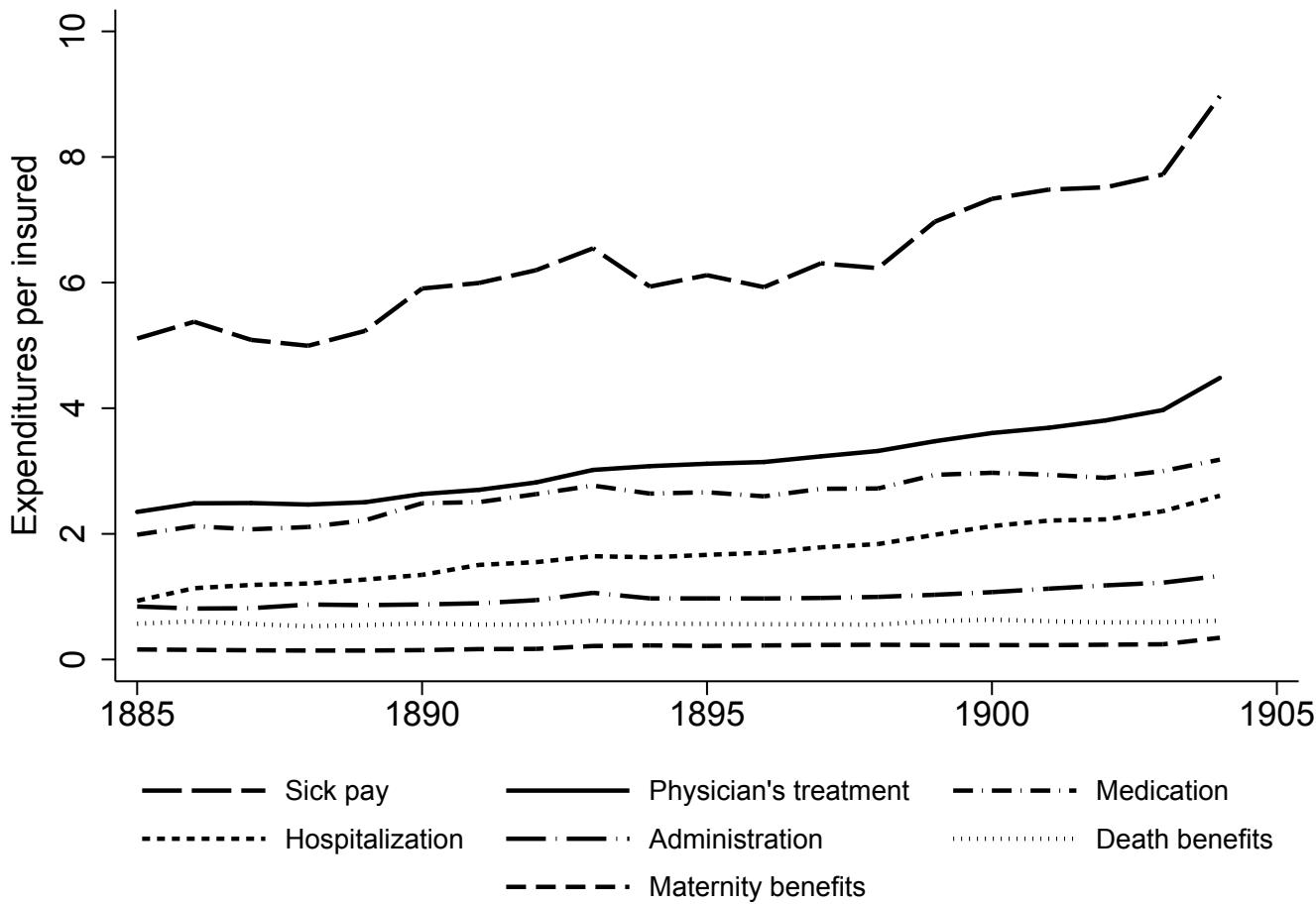


Figure 10: Sickness funds' expenditures per insured in Marks. *The figure shows types of sickness funds' expenditures per insured in Marks over time. Data source: KSA, 1884-1942*

Table 1: Sectoral occupation structure

	Industrial sector			Public sector		
	1882	1895	1907	1882	1895	1907
Total occupational population	9,394	12,196	16,244	1,306	1,672	2,043
Working population	3,651	4,756	6,688	587	823	1,027
Female working population	585	761	1,078	61	97	166
Share in total population	34.4%	38.3%	43.6%	4.8%	5.2%	5.5%
Share in working population	31.2%	35.9%	37.1%	5.0%	6.2%	5.7%
Share in female working population	17.5%	22.3%	18.7%	1.8%	2.8%	2.9%

*Notes:* The table reports descriptive statistics based on the occupation censuses conducted in the German Empire in 1882, 1895, and 1907. The industrial sector refers to the official sector B (Industry), while the public sector refers to the official sector E (Public). Absolute population numbers are reported in thousands. The *Total Occupational Population* includes children and non-employed family members and assigns to them the occupation of the father or husband, respectively.

Table 2: Flexible DiD: main results

Dep. var.: Deaths per occ. Pop.	Baseline (1)	Urbanization (2)	Waterworks (3)	Sewerage (4)
Blue Collar x 1877	-0.1617 (0.2272)	-0.3161 (0.2410)	-0.3335 (0.2146)	-0.2319 (0.2255)
Blue Collar x 1885	1.0627*** (0.1604)	1.2011*** (0.1499)	1.1798*** (0.1800)	1.1250*** (0.1554)
Blue Collar x 1889	-0.9193*** (0.2761)	-0.6621** (0.2563)	-0.5766 (0.3603)	-0.7835*** (0.2811)
Blue Collar x 1893	-1.6119*** (0.2747)	-1.2193*** (0.2856)	-1.0993** (0.4285)	-1.3931*** (0.3101)
Blue Collar x 1897	-1.9076*** (0.2869)	-1.3332*** (0.4146)	-1.2747*** (0.3831)	-1.5905*** (0.3766)
Urbanization x Sector	No	Yes	No	No
Waterworks x Sector	No	No	Yes	No
Sewerage x Sector	No	No	No	Yes
District x Sector FE	Yes	Yes	Yes	Yes
District x Time FE	Yes	Yes	Yes	Yes
Observations	432	432	432	432
R-squared	0.94	0.94	0.94	0.94

5

Notes: The table reports flexible DiD estimates. All variables are averaged over four year periods from 1877 to 1900. The dependent variable measures crude deaths rates using deaths by occupation of the household head per alive occupational population (including dependents) in thousands. The omitted period is 1881-84. Column (2) adds the urbanization rate as a covariate, Column (3) adds waterworks per capita as a covariate, while Column (4) adds sewerage per capita as a covariate. Standard errors are clustered at the occupation by district level and given in parentheses. \* 10%, \*\*5%, \*\*\* 1% confidence level

Table 3: Flexible DiD: heterogeneity

Dep. var.: Deaths per occ. Pop.	Baseline (1)	Male (2)	Female (3)	Children (4)
Blue Collar x 1877	-0.1617 (0.2272)	0.0573 (0.0695)	-0.2390*** (0.0584)	0.0200 (0.1585)
Blue Collar x 1885	1.0627*** (0.1604)	0.3741*** (0.0701)	0.0663 (0.0425)	0.6223*** (0.1126)
Blue Collar x 1889	-0.9193*** (0.2761)	-0.4118*** (0.1079)	-0.1578*** (0.0536)	-0.3497** (0.1738)
Blue Collar x 1893	-1.6119*** (0.2747)	-0.5091*** (0.1011)	-0.1783*** (0.0548)	-0.9244*** (0.1774)
Blue Collar x 1897	-1.9076*** (0.2869)	-0.5659*** (0.0927)	-0.2307*** (0.0669)	-1.1110*** (0.2112)
District x Sector FE	Yes	Yes	Yes	Yes
District x Time FE	Yes	Yes	Yes	Yes
Observations	432	432	432	432
R-squared	0.94	0.92	0.91	0.93

Notes: The table reports flexible DiD estimates. All variables are averaged over four year periods from 1877 to 1900. The dependent variable measures crude deaths rates using deaths by occupation of the household head per alive occupational population (including dependents) in thousands. The omitted period is 1881-84. The dependent variable in Column (1) is total mortality, the dependent variable in Column (2) is adult male mortality, the dependent variable in Column (3) is adult female mortality, and the dependent variable in Column (4) is child mortality. Standard errors are clustered at the occupation by district level and given in parentheses. \* 10%, \*\*5%, \*\*\* 1% confidence level

Table 4: County fixed effects using 1882 blue-collar workers' share

Dep. var.: Crude death rate	Base (1)	Controls (2)	Male (3)	Female (4)	Infants (5)	LegInf (6)	IllegInf (7)	Placebo (8)
Treatment x 1875	1.891 (1.623)	1.606 (1.621)	-5.714*** (1.713)	0.711 (1.764)	-20.264* (11.363)	-23.030* (11.948)	10.630 (56.422)	2.946 (3.650)
Treatment x 1885	-7.768*** (1.693)	-7.417*** (1.689)	-7.433*** (1.965)	-6.690*** (1.596)	4.018 (11.484)	1.761 (11.989)	27.650 (47.676)	1.452 (3.098)
Treatment x 1890	-4.754** (2.041)	-3.983* (2.053)	-6.971*** (2.395)	-6.550*** (2.079)	-19.437 (15.279)	-30.964** (15.668)	45.986 (55.537)	-1.712 (3.798)
Treatment x 1895	-8.631*** (2.080)	-7.619*** (2.092)	-5.881** (2.570)	-8.454*** (2.153)	-34.598* (20.049)	-55.767*** (20.287)	79.660 (77.044)	-3.007 (4.627)
Treatment x 1900	-12.330*** (2.399)	-11.380*** (2.372)	-16.123*** (2.813)	-13.119*** (2.500)	-45.863* (24.973)	-73.912*** (24.832)	103.370 (79.473)	1.023 (4.984)
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2645	2645	2645	2645	2645	2645	2645	2645
Counties	441	441	441	441	441	441	441	441
Periods	6	6	6	6	6	6	6	6
R-squared	0.75	0.75	0.74	0.74	0.24	0.27	0.03	0.75

Notes: The table reports county-level fixed effects estimates. All variables are averaged over five year periods from 1875 to 1904. The dependent variable measures crude death rates using total deaths per alive population in thousands. The treatment variable is the blue collar workers' population share observed in 1882, interacted with time-period dummies. Column (2) adds the urbanization rate, waterworks per capita, and sewerage per capita as covariates. The dependent variable in Column (3) is male mortality, the dependent variable in Column (4) is female mortality, and the dependent variable in Column (5) is infant mortality (< 1 year) per 1,000 births. The dependent variable in Column (6) is infant mortality (< 1 year) born in wedlock per 1,000 births in wedlock, while the dependent variable in Column (7) is infant mortality (< 1 year) born out of wedlock per 1,000 births out of wedlock. The dependent variable in Column (8) is total mortality, where the treatment variable is public servants' population share observed in 1882, interacted with time-period dummies. Standard errors are clustered at the county level and given in parentheses. \* 10%, \*\*5%, \*\*\* 1% confidence level

Table 5: District fixed effects using blue collar workers and insured population

Dep. var.: Crude DR	Initial Blue Collar Workers (1882)					Initial Insured (1885)				
	Base (1)	Controls (2)	Male (3)	Female (4)	LegInf (5)	Base (6)	Controls (7)	Male (8)	Female (9)	LegInf (10)
Treatment x 1875	3.37 (3.35)	3.41 (4.47)	-3.19 (4.41)	2.01 (5.40)	13.30 (42.84)	5.04 (4.30)	4.67 (5.12)	-3.48 (4.70)	3.56 (6.44)	17.71 (49.36)
Treatment x 1885	-11.19** (4.60)	-11.14** (5.30)	-12.34** (5.72)	-12.20** (5.31)	-23.87 (41.39)	-15.01** (6.11)	-14.96** (6.64)	-15.55** (7.33)	-15.69** (6.75)	-54.41 (47.36)
Treatment x 1890	-7.07 (7.90)	-7.27 (8.55)	-12.25 (11.15)	-12.51 (9.37)	-93.97 (71.55)	-11.27 (10.65)	-11.90 (11.13)	-18.87 (14.44)	-18.04 (12.21)	-153.11* (83.26)
Treatment x 1895	-11.64 (6.98)	-11.97 (7.23)	-12.13 (9.97)	-15.64* (7.82)	-154.22 (93.18)	-16.73* (9.16)	-17.76* (9.08)	-19.48 (12.68)	-22.39** (9.72)	-234.92** (105.54)
Treatment x 1900	-15.05** (7.27)	-14.91** (6.80)	-19.21** (9.42)	-19.20** (7.18)	-162.05* (88.70)	-21.83** (9.00)	-22.70*** (8.17)	-28.81** (11.68)	-28.66*** (8.52)	-242.41** (99.95)
Controls	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	216	216	216	216	216	216	216	216	216	216
Districts	36	36	36	36	36	36	36	36	36	36
Periods	6	6	6	6	6	6	6	6	6	6
R-squared	0.88	0.88	0.85	0.86	0.47	0.88	0.89	0.86	0.87	0.49

*Notes:* The table reports district-level fixed effects estimates. All variables are averaged over five year periods from 1875 to 1904. The dependent variable in Columns (1), (2), (6), and (7) is crude death rates measured as total deaths per alive population in thousands. The dependent variable in Columns (3) and (8) is male mortality, the dependent variable in Columns (4) and (9) is female mortality, and the dependent variable in Columns (5) and (10) is infant mortality (< 1 year) born in wedlock per 1,000 births in wedlock. The treatment variable in columns (1)-(5) is blue collar workers' population share observed in 1882 interacted with time-period dummies. The treatment variable in columns (6)-(10) is the share of the population covered by health insurance observed in 1885 interacted with time-period dummies. Controls include the urbanization rate, waterworks per capita, and sewerage per capita. Standard errors are clustered at the district level and given in parentheses. \* 10%, \*\*5%, \*\*\* 1% confidence level

Table 6: District fixed effects: causes of death

Dep. var.: Crude DR	Accident (1)	Infectious					Non-infectious (6)	Maternal (7)
		Waterborne (2)	Airborne (3)	Lung (4)	TB+Scruftola (5)			
Treatment x 1875	0.162 (0.155)	3.299* (1.749)	-3.962** (1.707)	-3.175* (1.627)	-0.736 (0.791)	-0.571 (0.873)	-0.030 (0.101)	
Treatment x 1885	-0.090 (0.151)	2.324 (2.053)	-8.606*** (2.721)	-1.493 (1.785)	-0.396 (0.714)	-0.874** (0.371)	-0.186*** (0.068)	
Treatment x 1890	0.271* (0.136)	1.803 (3.221)	-4.561 (4.315)	-3.911 (2.741)	-3.317*** (0.931)	-1.243** (0.563)	-0.186** (0.084)	
Treatment x 1895	0.290** (0.139)	0.422 (4.169)	-4.916 (3.638)	-7.272*** (2.387)	-5.842*** (1.215)	-1.891** (0.720)	-0.183** (0.078)	
Treatment x 1900	0.426** (0.169)	0.602 (3.968)	-8.062*** (2.891)	-11.007*** (1.982)	-8.464*** (1.745)	-3.436*** (0.990)	-0.053 (0.114)	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	216	216	216	216	216	216	216	216
Districts	36	36	36	36	36	36	36	36
Periods	6	6	6	6	6	6	6	6
R-squared	0.54	0.27	0.80	0.60	0.86	0.28	0.92	

*Notes:* The table reports district-level fixed effects estimates. All variables are averaged over five year periods from 1875 to 1904. The dependent variable measures death rates by causes of death using total deaths per alive population in thousands. The treatment variable is blue collar workers' population share observed in 1882 interacted with time-period dummies. *Accident* is death from accidents; *Waterborne* is death from typhus, typhoid fever, and three types of diarrheal diseases; *Airborne* is death from smallpox, scarlet fever, measles, diphtheria, pertussis, scrofula, tuberculosis, tracheitis, pneumonia and other lung diseases; *Lung* is death from pertussis, scrofula, tuberculosis, pneumonia and other lung diseases; *TB+Scruftola* is death from scrofula and tuberculosis; *Non-infectious* is death from cancer, edema, stroke, heart disease, brain disease, and kidney disease; *Maternal* is death in childbed. Controls include the urbanization rate, waterworks per capita, and sewerage per capita. Standard errors, are clustered at the district level and given in parentheses. \* 10%, \*\*5%, \*\*\* 1% confidence level.

Table 7: Mortality and health expenditures

Dep. var.: Crude death rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
L.Doctor visits(std)	-0.112** (0.053)								
L.Medication(std)		-0.152*** (0.041)							
L.Hospitalization(std)			-0.041 (0.071)						
L.Health care(std)							-0.151** (0.066)		
L.Sickpay (std)				-0.007 (0.069)					
L.Maternity ben.(std)					-0.067* (0.039)				
L.Death ben.(std)						0.073* (0.037)			
L.Compensation(std)							0.028 (0.061)		
L.Administration(std)							-0.076 (0.061)	-0.026 (0.052)	
L.Sick leave (std)								-0.025 (0.036)	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	684	684	684	684	684	684	684	684	576
Districts	36	36	36	36	36	36	36	36	36
Periods	19	19	19	19	19	19	19	19	16
R-squared	0.81	0.81	0.80	0.80	0.80	0.80	0.81	0.81	0.78

Notes: The table reports district-level fixed effects estimates. The dependent variable measures crude death rates using total annual deaths per alive population in thousands. Explanatory variables are sickness fund expenditures per insured or duration of sick leave per insured lagged by one year. All variables are normalized to a mean zero and a standard deviation of one. Controls include the urbanization rate, waterworks per capita, and sewerage per capita, each lagged by one year. Standard errors are clustered at the district level and given in parentheses. \* 10%, \*\*5%, \*\*\* 1% confidence level.

## A Appendix

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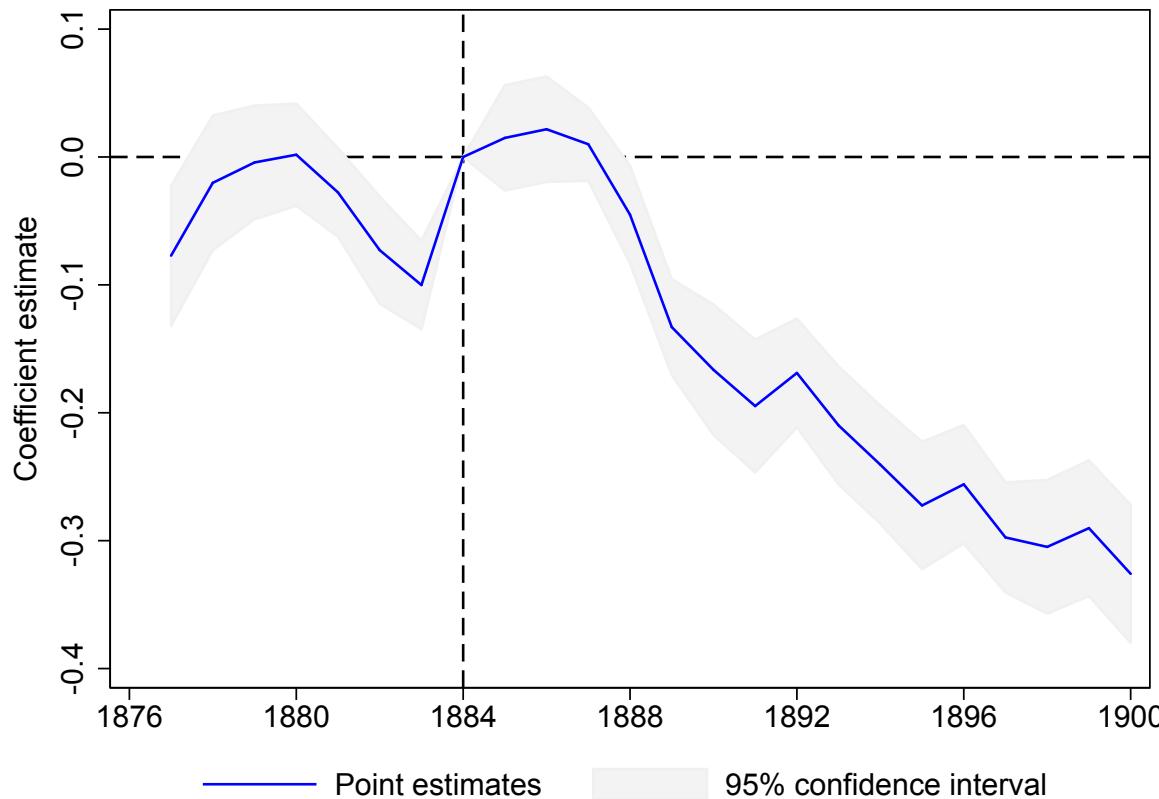


Figure A.1: Flexible-DiD estimates in log-specification. The figure shows estimated coefficients and 95% confidence intervals from a difference-in-differences model similar to Equation 1 using annual data and accounting for pre-trends. The omitted year 1884 is marked by the vertical bar and indicates the introduction of BHI.

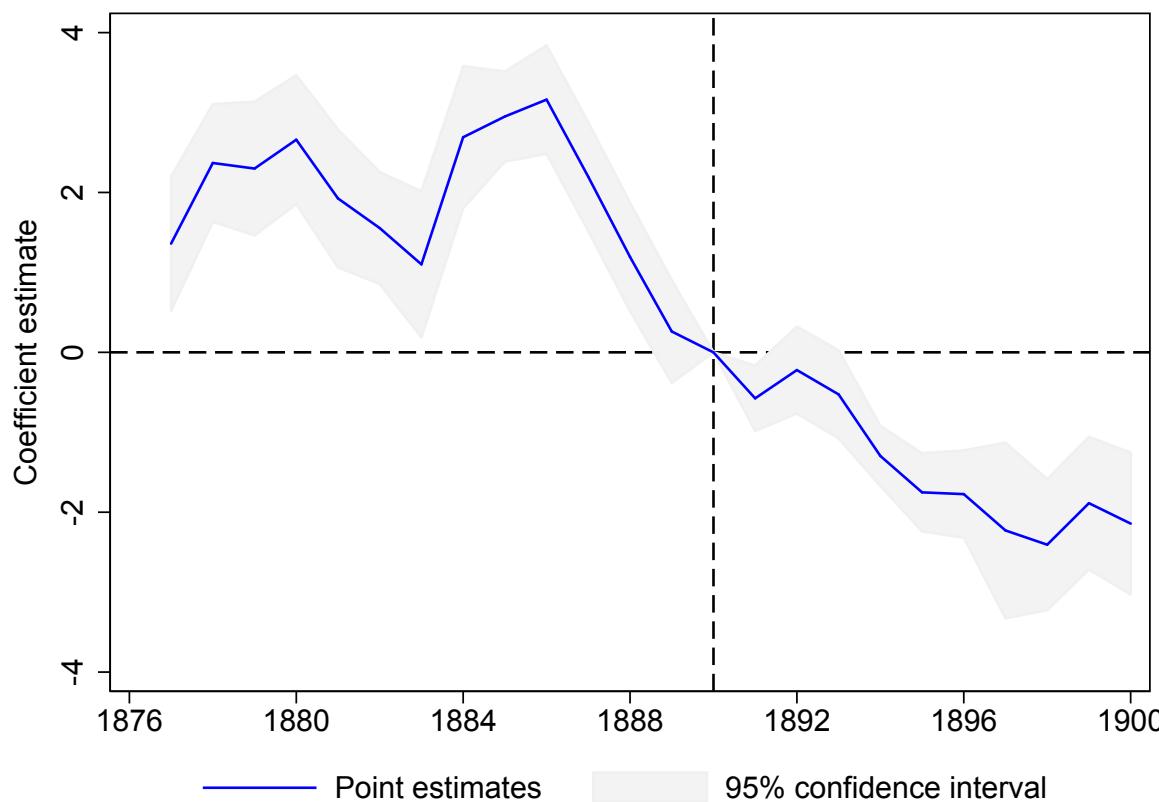


Figure A.2: Flexible-DiD estimates using 1890 as reference. *The figure shows estimated coefficients and 95% confidence intervals from a difference-in-differences model similar to Equation 1 using annual data and accounting pre-trends. The omitted year 1890 is marked by the vertical bar and indicates the introduction of the old-age pension system.*

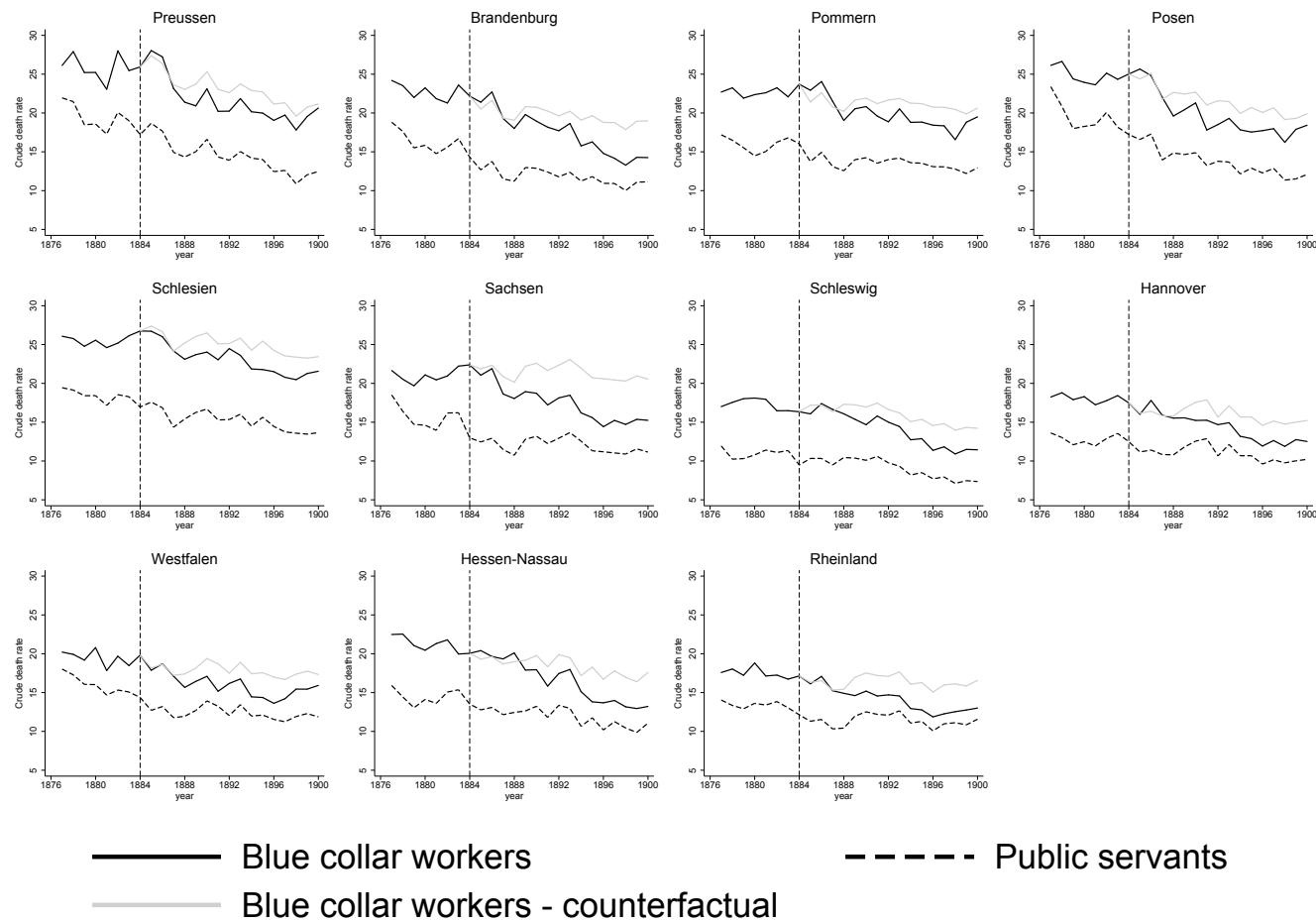


Figure A.3: Crude death rates by sector (blue collar workers and public servants) and province. *The figure shows the number of deaths per 1,000 people by sectoral groups of blue-collar workers and public servants for each Prussian province. Following a difference-in-differences logic, the counterfactual is computed by parallel-shifting the trend of the public servants up to the blue collar workers level in 1884. The vertical bars indicate the introduction of BHI in 1884.*

Table A1: Expansion of health insurance

Year	Population	Health Insured	% Insured Population
1854	17,077	254	1.5%
1860	18,136	329	1.8%
1864	19,149	458	2.4%
1868	24,069	688	2.9%
1870	24,403	632	2.6%
1872	24,801	725	2.9%
1874	25,352	795	3.1%
1876	25,922	785	3.0%
1885	28,232	2,263	8.0%
1890	29,819	3,457	11.6%
1895	31,697	3,998	12.6%
1900	34,254	5,123	15.0%
1905	37,058	6,192	16.7%

*Notes:* The table shows descriptive statistics on the expansion of health insurance in Prussia within its respective borders. Population and the number of insured are expressed in thousands. Insurance benefits vary pre- and post-1884.

Table A2: District fixed effects including age composition and vote share controls

Dep. var.: Crude DR	Initial Blue Collar Workers (1882)				Initial Insured (1885)			
	Average age (1)	Dependency ratio (2)	Age groups (3)	SPD vote (4)	Average age (5)	Dependency ratio (6)	Age groups (7)	SPD vote (8)
Treatment x 1875	3.46 (4.40)	0.72 (5.02)	-0.78 (4.31)	2.88 (4.28)	4.59 (5.13)	2.98 (5.97)	-0.77 (4.51)	4.48 (5.00)
Treatment x 1885	-11.77** (5.22)	-9.73** (4.59)	-11.11** (5.03)	-10.91** (5.33)	-15.21** (6.51)	-12.62** (6.11)	-14.80** (6.42)	-14.85** (6.69)
Treatment x 1890	-8.49 (8.57)	-1.95 (7.66)	-10.80 (9.16)	-5.84 (8.38)	-12.53 (11.02)	-5.28 (10.49)	-16.57 (11.72)	-11.11 (11.22)
Treatment x 1895	-12.96* (6.82)	-5.12 (6.04)	-15.46** (7.20)	-10.40 (7.23)	-17.87** (8.52)	-8.43 (8.34)	-22.29** (8.73)	-16.81* (9.50)
Treatment x 1900	-16.39** (6.31)	-13.11** (5.15)	-23.07*** (8.34)	-13.19* (6.85)	-22.79*** (7.40)	-17.95*** (6.36)	-32.44*** (9.11)	-21.63** (8.67)
Average age	Yes	No	No	No	Yes	No	No	No
Dependency ratio	No	Yes	No	No	No	Yes	No	No
Age groups	No	No	Yes	No	No	No	Yes	No
SPD vote	No	No	No	Yes	No	No	No	Yes
Standard controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	216	216	216	216	216	216	216	216
Districts	36	36	36	36	36	36	36	36
Periods	6	6	6	6	6	6	6	6
R-squared	0.89	0.90	0.89	0.88	0.89	0.90	0.89	0.89

*Notes:* The table reports district-level fixed effects estimates. All variables are averaged over five year periods from 1875 to 1904. The dependent variable is the crude death rate measured as total deaths per alive population in thousands. The treatment variable in Columns (1) to (4) is blue collar workers' population share observed in 1882 interacted with time-period dummies. The treatment variable in Columns (5) to (8) is the share of the population covered by health insurance observed in 1885 interacted with time-period dummies. In Columns (1) and (5), the average age of a district's population is included as a covariate. In Columns (2) and (6), the dependency ratio (0-14 and 65+ year olds over 15-64 year olds) is included as a covariate. In Columns (3) and (7), the population share of 0-14 year olds and the population share of 15-44 year olds are included as covariates. In columns (4) and (8), the vote share of the workers' party SPD (and its predecessors) measured at the latest general elections is included as a covariate. Standard controls include the urbanization rate, waterworks per capita, and sewerage per capita. Standard errors are clustered at the district level and given in parentheses. \* 10%, \*\*5%, \*\*\* 1% confidence level