Bismarck's Health Insurance and the Mortality Decline*

Stefan Bauernschuster, Anastasia Driva, and Erik Hornung †

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Abstract

We investigate the impact on mortality of the world's first compulsory health insurance, established by Otto von Bismarck in 1884. We use Prussian administrative panel data and employ a multi-layered empirical setup drawing on time-series, international comparisons and difference-in-differences approaches that exploit differences in eligibility for insurance across occupations. All approaches yield a consistent pattern suggesting significant negative effects of Bismarck's Health Insurance on mortality. The effects are largely driven by a reduction of deaths due to infectious diseases. We present *prima facie* evidence that hygiene knowledge diffusion through physicians might have been an important channel.

Keywords: health insurance, mortality, demographic transition, Prussia
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†Bauernschuster: University of Passau, Innstr. 27, 94032 Passau, Germany; stefan.bauernschuster@uni-passau.de; Driva: LMU Munich, Ludwigstr. 33, 80539 Munich, Germany; anastasia.driva@econ.lmu.de; Hornung: University of Bayreuth, Universitätsstr. 30, 95447 Bayreuth, Germany; erik.hornung@uni-bayreuth.de

"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

Improving population health is a well-accepted social objective whose impact on development and economic growth is still debated (see Acemoglu and Johnson, 2007; Ashraf et al., 2008; Bleakley, 2007; Clark and Cummins, 2009; Hansen and Lønstrup, 2015; Jayachandran and Lleras-Muney, 2009; Lorentzen et al., 2008; Weil, 2007). To estimate the effect of health on productivity and growth, most studies focus on major improvements in health and life expectancy. Such improvements take center stage during the first phase of the demographic transition at the end of the 19th century. This level shift in health, which has been coined the Epidemiological Transition, encompasses a period of marked decline in mortality particularly from infectious diseases.

The ongoing debate on the sources of this unprecedented decline in mortality that many industrializing countries experienced at that time has sprawled a range of seminal contributions. Some argue that improvements in nutrition played a crucial role (McKeown, 1979; Fogel, 2004). Others stress the role of directed public health interventions such as the roll-out of sanitation infrastructure and the diffusion of new health knowledge (Deaton, 2013; Preston, 1975; Szreter, 1988).[1] So far, little is known about the role that access to health insurance played during the Epidemiological Transition.[2]

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. To our knowledge, we are the first to empirically evaluate the impact on mortality of the Health Insurance Act passed by Chancellor Otto von Bismarck as part of his social legislation program (henceforth Bismarck's Health Insurance or BHI). From December 1st, 1884, the statutory health insurance became compulsory for all industrial wage earners. Subsequently, it acted as a blueprint

[1] For recent surveys see Costa (2015) and Cutler et al. (2006).

[2] Cross-country studies by Winegarden and Murray (1998) and Bowblis (2010) suggest that the expansion of health insurance across Europe is negatively associated with mortality. The exact channels through which the insurance schemes affected health outcomes remain unclear.

for Germany's current health system and served as a role model for many health systems across the world. According to Vögele (1998, p. 199-208), BHI might have been able to increase health by 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. As such, BHI could work through a pecuniary channel by smoothing income and providing nutrition during times of hardship. Alternatively, it could work through a medication and knowledge channel by providing access to physicians who could treat patients and disseminate new knowledge on hygiene and prevention of infectious diseases. To this day, econometric evidence to disentangle these channels remains largely missing.

Using administrative panel data from Prussia, the largest of the German states, we present a multi-layered empirical approach that accumulates in a step-wise manner evidence for the mortality effects of Bismarck's Health Insurance and the potential mechanisms at work.[3] After looking at the development of mortality in Prussia around the introduction of BHI, we investigate Prussia's mortality decline relative to the experience of other Western European countries. Next, we exploit the fact that BHI was mandatory for blue collar workers but not for other occupations such as public servants. Newly digitized administrative data from Prussian districts allow us to compute occupation-specific mortality rates on an annual basis from 1877 to 1900. We bring these panel data to a generalized difference-in-differences model, in which we compare the mortality trend of blue collar workers (treatment group) to the mortality trend of public servants (control group) while allowing for heterogenous 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 different empirical approaches yield a consistent pattern suggesting negative effects of Bismarck's Health Insurance on mortality. Long-run time series data of mortality show that the beginning of the mortality decline in 19th century Prussia coincides with the introduction of BHI. Moreover, while mortality decreased in all Western European countries at 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

[3]By focusing on mortality rather than morbidity as the outcome of interest, we avoid any moral hazard and prinicpal agent problems that might arise after the introduction of BHI.

1884 to the turn of the century, BHI reduced blue collar workers' mortality by 7.8 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 reforms nor by improved working conditions for blue-collar workers, nor by selection into the blue-collar sector after the introduction of BHI. Using data on causes of death in a district fixed effect framework, we show that a large part of the effect is driven by a reduction of mortality due to airborne infectious diseases, especially tuberculosis. In addition, we find some significant but smaller effects of BHI on mortality due to non-infectious diseases.

Surprisingly, the insurance was able to reduce infectious disease mortality in the absence of effective medication for many of the prevailing infectious diseases. This finding is in line with 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 Ewald, 1914; Condrau, 2000; Koch, 1901). The introduction of BHI extended access to multipliers of health knowledge to poor worker families, a group of people formerly unable to afford consultation of physicians. The sickfunds launched information campaigns and encouraged the contracted physicians to disseminate the newly gained knowledge on the transmission of infectious diseases; one particular aim was to tackle tuberculosis. And it seems these measures have been effective. We provide additional support for this interpretation by showing that expenditures for doctor visits and expenditures for medication are negatively related to mortality in a district fixed effects model, while expenditures for sick pay are not.

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, 2015). However, there are key differences between these 20th century expansions and the introduction of BHI. Current health insurance schemes work in an environment of chronic diseases and have the ability to provide health care by medical treatment. In contrast, BHI worked in an environment of infectious diseases and most of the infectious diseases could not be treated effectively. One might also think about how our study relates to the situation in developing countries which start rolling-out compulsory health insurance. Infectious diseases played a major role in Bismarck's 19th century Prussia and they do so in many developing countries today. Moreover, information on how to avoid infections was scarce in the Prussian population and is still not very well established in the population of some developing countries. However, in contrast to the late 19th century, infectious diseases can in principle be treated very well today (although a lack of adequate institutions may still often limit medical success).

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[2005]). Longevity 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).

Knowledge about the precise factors that might have caused the observed mortality decline remains vague. 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. Especially, the 'urban penalty' was gradually removed through a range of public interventions 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, 2016) strongly reduced mortality from waterborne diseases.

Seminal contributions by McKeown (1979) and Fogel (2004) reach the conclusion that improvements in living standards and nutrition were responsible for the mortality decline in 19th century Europe. Their analysis left little merit for public health interventions or medication as potential drivers of the decline. Sieveking (1903); Leonard and Ljungberg (2010) argue that medical treatments hardly contributed to the mortaliy 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., 2014; 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. 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. 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.[4] 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. (2015) 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. (2015), Norwegian 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

[4] 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.

early 20th century. In 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.[5]

Public interventions 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. 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.[6] In addition, Tennstedt (1983, p. 461) discusses 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' responsibilites.

Earlier work by Winegarden and Murray (1998) provides evidence that health insurance coverage across five European countries was associated with mortality reductions.[7] 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. 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

[5] 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.

[6] 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.

[7] 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.

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).[8] 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 burden of poor relief on the 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 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[9] 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.

[8] Scheubel (2013) provides an excellent overview of Bismarck's social security system. Fenge and Scheubel (2014) 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 accidence insurance. Lehmann-Hasemeyer and Streb (2016) find that Bismarck's social security system as a whole crowded out private savings.

[9] Contributions were confined to a maximum range of 3 to 6 percent of the wage.

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 benefit 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 national law only specified maximum contributions and minimum benefits. 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. 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, evidence from Leichter (1979) suggests that, in 1905, 59 percent of all insured individuals were insured in either local or parish funds.

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 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.[10] Some of the workers preferred to buy insurance from voluntary funds (*Hilfskassen*) which provided lower benefits until legal adjustments were made in 1892. Voluntary funds did not require contributions by the employer who preferred to hire workers with such insurances (Tennstedt, 1983, pp. 318-322). Initially, around 40 percent of the

[10] 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).

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.[11] 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 1874, the latest available pre-BHI year, after which the insured population triples 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 insurance 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).[12] Contracted physicians often received a lump sum payment of 2 Marks per insured from the insurance funds, irrespective of the frequency of treatment. As a consequence, the insured increasingly made use of consultation hours. Soon, the stereotypical doctor's complaint that patients came for consultation only when it was too late had turned into complaints that patients came in for petty indispositions (Huerkamp, 1985, p. 201).[13] Moreover, BHI became a key driver of the increased utilization of hospital capacity in the 1890s (Spree, 1996).

From a political perspective, supporters of BHI argue that the reform was costly but that it "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 the German mortality decline.

[11] Table A.1 in the Appendix shows the exact numbers of insured and total population of Prussia over the years.

[12] 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).

[13] This notion is supported by contemporary sources that suggest that only half of the consultations justified a period of sick leave (Huerkamp, 1985, pp.202).

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. By 1885, her territory covered roughly two thirds of the total area and population of the German Empire. The Royal Prussian Statistical Office is unique in publishing 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 to provide evidence on potential channels through which the reform affected mortality. The period of observation for the main analyses covers the period 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.[14] This information is consistently reported annually for 28 occupational groups from 1877. Deaths are reported for men and women, as well as 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 and can be classified as part of the blue collar industries (i.e. metals, textiles, chemicals, food) or the public sector (administration, education, health care). This categorization is particularly helpful for our analysis since it allows to compare mortality across occupational groups that differ in their obligation to obtain compulsory 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

[14] In the Prussian administrative hierarchy the district (*Regierungsbezirk*) ranks above the county (*Landkreis*).

of rapid industrialization in Germany.[15] 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.[16]

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 and urbanization rates. The literature discusses two main drivers of change in urban mortality occurring at the end of the nineteenth 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 on 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 district level share of the total urban population with access to waterworks and sewerage. The Prussian Statistical Office conducted population censes including urban population counts every five years. These data, available from (Galloway, 2007), were linearly interpolated to generate annual estimates of the urban population and urbanization rates. In subsequent specifications we will also be able to aggregate these data to the county level.

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

In a range of specifications we will draw on aggregate mortality rates instead of using occupation specific mortality rates. Such information is provided by Galloway

[15] Note that this period is not a period of substantial warfare. Deaths related to the Franco Prussian war of 1871 are unlikely to substantially change the mortality pattern of the period 1877-1900.

[16] 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 typically 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.

(2007) for all Prussian counties during the period 1875 to 1904. Due to the fact that several counties were split up due to administrative reforms, we aggregate the data to reflect the borders of 1875, resulting in a constant set of 441 counties. These data can be further aggregated to the district level. Next to total mortality, the data gender-specific mortality and infant mortality by legitimacy status.[17]

To gain insights into potential mechanisms through which BHI affected health, we digitized rich data on the causes of death from the *Preussische Statistik* Königliches Statistisches Bureau in Berlin (1861-1934). In particular, we draw on district-level information on fatalities reported by the full universe of thirty distinct causes of death that were reported annually from 1875 to 1904. This wealth of data allows us example 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 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.[18] To study potential consequences on morbidity instead of mortality, we digitized district level hospitalization rates based on the annual number of treated patients and individual cases, reported in Königliches Statistisches Bureau in Berlin (1861-1934) for the period 1880-1904.

Specifications using aggregate or cause-specific mortality as dependent variables will use the aggregate 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

[17]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.

[18]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 of 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 were 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 in such regions. Using the 'unknown cause of death' rate as an outcome in a district fixed effects model, 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.

contributors which allows us to calculate the share of insured in the total population.[19]

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.[20] Moreover, we digitized district-level data from population censuses reported in KSBB, 1861-1934 to generate information on the age structure of the population during the period 1875 to 1904. These allows us to capture changes in 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 should capture 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 will draw on newly digitized data on the expenditures structure of insurance funds. KSA, 1884-1942 reports district-level data from insurance funds balance sheets. Expenditures include categories such as sick pay, medication and doctor visits reported in German Mark, as well as the number of sick days. Using the 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 to 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 support of our claims, we address

[19] 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.

[20] These elections were chosen because they were the preceded the first year of five year intervals to which we aggregate.

selection issues and potential confounding factors using changes in crude mortality rates at the district and county level, respectively. Finally, to explore the potential channels through which BHI affected mortality, we draw on causes of death statistics and insurance benefits 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 1815-1913. 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.

The introduction of BHI coincides with a period of declining mortality rates across industrializing countries. Yet, our findings suggest that the noticeably more accentuated drop of German mortality rates might be related to the establishment of the health insurance. This finding is established using mortality rates across European countries for an international comparison.[21] 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 hardly any 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.[22] Figure 4 shows that the mortality

[21] 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

[22] Again, we apply local regressions using a tricube weighting function and a bandwidth of 0.15 to smooth the time trends.

decline for Prussia was indeed considerably stronger than the mortality decline of all other countries during this period. Only the Netherlands experienced a comparably strong decline.[23]

Although remarkable, these findings from simple time-series and cross-country statistics should not be interpreted as evidence for an 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.

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 for continuation 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 important for our identification assumption, their benefits were not subject to change in the period 1873 to 1914.

[23]The comparison also suggests a *less* pronounced mortality decline prior to 1884 in Prussia than in most other countries.

Our main specification is based on data aggregated over all blue collar and public servant subgroups and over four year periods.[24] 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):

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

$Death_{iot}$ is the average death rate of people with occupation $o \in (BlueCollar, PublicServant)$, measured in district i at period $t \in (1877-1880, 1881-1884, 1885-1888, 1889-1892, 1893-1896, 1897-1900)$. α_{io} are occupation by district fixed effects accounting for any time-constant occupation-specific mortality differences between districts. θ_{it} are district by period fixed effects that flexibly allow mortality trends to differ across districts. Thus, these fixed effects pick up a range of shocks affecting the district-level health environment relevant for both occupational groups. These could be 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. β_t measures the unbiased reduced form effect of BHI if there are no time-varying unobservables that affect blue collar workers' mortality differently from public servants' mortality. ε_{iot} is a mean zero error term. Standard errors are clustered at the district level to allow for serial autocorrelation within districts.

By letting β 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 β_t 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.

[24] 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).

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 also in the U.S. (see Alsan and Goldin, 2015; Beach et al., 2016; Ferrie and Troesken, 2008; Kesztenbaum and Rosenthal, 2016). 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 θ_{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 expected in the 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 district by occupation fixed effects and district by period fixed effects. The period immediately preceding Bismarck's reform, i.e., the period 1881-1884, constitutes the reference category. 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.[25] 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 1.654 deaths per 1,000 individuals, i.e. by 7.8 percent (-1.654/21.184).

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 graphical support against the concern that the roll-out of waterworks and sewerage confounds the health insurance effect. We

[25] 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.

depict 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 of 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 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. 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 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).[26] For all three groups, the effects gradually increase over time.[27]

Given the economic literature on early human capital development and the fetal origin hypothesis (Douglas and Currie, 2011; Deaton, 2007), we should not be surprised to find that children respond to changes in the health environment although they are not themselves targeted by the reform. In particular, we expect children to benefit from BHI via a range of intra-family spillovers. First, such intra-family spillovers might for example be induced by sick pay. Sick payments stabilize family income, they might facilitate continuous calorie intake, enhance nutritional prospects of the family and thus be beneficial for infant health (see Subramanian and Deaton, 1996; Case and Paxson, 2008). Second, access to health care for the insured individual might affect the health of the entire family. Even in the absence of effective medication against most infectious diseases, spillovers might occur through the prevention of transmission of infectious diseases. As indicated above, contracted

[26]Note that instead of using the full occupation-specific population, regressions for adult males and females use the occupation-specific working population. Especially for the male population, this is arguably very close to capturing exactly the insured population in the treatment group. Results are qualitatively similar when using the full occupation-specific population as denominators.

[27]In further unreported regressions, we tested the robustness of our finding to excluding the district of Berlin, at the same time Prussia's biggest city, from the sample.

physicians provided individuals, in particular female household heads, with knowledge on personal hygiene matters, which might reduce the incidence of infectious diseases at the household level and consequently improve children's health.

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.[28] We aim at mitigating concerns related to potentially diverging pre-treatment trends across treatment and control group by including occupation specific linear time trends. 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 accelerates 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. While this might indeed be the case, we argue that the relevant counterfactual is a world in which blue collar workers did not receive access to compulsory health insurance. In such a setting, a similar decline can only be achieved with similar health benefits. Besides, we can test whether the reduction of blue collar workers' mortality constitutes mean reversion by using crude death rates measured on a logarithmic scale as the dependent variable (see Figure A.1 in the Appendix). The results confirm the established pattern, hence revealing that our estimates do not merely capture regression to the mean effects.[29]

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

[28] This basic version of the model is without the health infrastructure and urbanization covariates.
[29] Note that the insignificant but positive short-run effects on mortality completely disappear in this specification.

break. The esimates show that blue-collar workers' relative mortality starts to decline well before 1891 and proceeds to do so subsequently (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 and working conditions

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. This 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.[30]

[30] 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).

Indeed, the 1880s saw only very few improvements in workplace regulation. Federal regulatory reforms were minor and restricted to very specific industries.[31] 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. Such 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 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 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., Vorstand des Deutschen Metallarbeiter-Verbandes, 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.[32]

We gain additional insights regarding the potential changes in working conditions by exploiting the heterogeneity of our occupation-specific mortality data. Imagine that workers in some industries were more successful in improving working

[31] 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).

[32] Below, we provide evidence from causes of death data confirming that the mortality decline is not driven by a reduction of workplace accidents.

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 actions, we do not expect working conditions to systematically improve across industries or time periods. 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 blue-collar and public-servant occupations.[33] The results of this exercise are displayed in Figure 9. Three findings are noteworthy: First, the negative mortality effect is not driven by a single (large) industry but systematically occurs across many industries. Second, the mortality decline occurs at the same point in time across many 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 already 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 of blue collar workers.

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, spillovers might not just occur within the family but also affect other 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 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 health insurance. At the same time, compliance of blue-collar workers might be incomplete due to

[33] While the treatment group constitutes only one specific blue collar industry in each specification, the control group consists of all five public servant occupations in each specification.

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.[34] Therefore, we should 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-tread design rules out selection into insurance take-up, selection into occupations might 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 decrease leading to lower aggregate mortality in the treatment group. On the other hand, new workers from rural areas might particularly suffer from 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.

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 empirical specification based on aggregate mortality. This specification allows us to estimate the effect of BHI on the full population, capturing all potential spillovers. At the same time, the alternative 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

[34] 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.

the mortality trends of counties with a lower share.[35] This model can be described by the following Equation (2):

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

Y_{it} is the average death rate of county i in year-block $t \in (1875-1879, 1880-1884, 1885-1889, 1890-1894, 1895-1899, 1900-1904)$. α_i are county fixed effects capturing unobserved time-invariant heterogeneity between counties, and θ_t are year-block 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 dummies. Thus, β_t captures any year-block 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. ε_{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 β_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 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 β_t is indeed zero in the pre-treatment years. Below, we also 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

[35] 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.

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.[36] 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, where infant mortality is defined as deaths of children during their first year of life per 1,000 births. We find statistically significant negative effects for males and females after 1885. In further unreported regressions, we observe significant negative effects of BHI on both the group of individuals younger than 15, and the group of individuals aged between 15 and 44. For infants below age one, the post-BHI coefficients are large and negative but only marginally significant. Yet, 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, or 2.4 percent, measured at the turn of the century.[37] Thus, mortality reductions due to BHI are only found for infants that grow up in households with both parents present. The fact that there are no BHI effects for illegitimate infants might be explained by the lack of intra-family spillovers 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.

[36] 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).

[37] 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.

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 initial 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.[38]

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.[39] 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, 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

[38]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).

[39]The choice of the first year is admittedly arbitrary to some degrees. Since results are qualitatively similar when using other years, we decided use 1885 to exclude the most pressing concerns of reverse causation and selection.

dependency ratio (under 15 year olds and 65+ year olds over 15 to 64 year olds), or the share of under 15 year olds as well as the share of 15 to 44 year olds as additional control variables. Moreover, to proxy for the strength of workers' movements and unions (and possibly related improvements in working conditions), we include the district-level vote share of the workers' party SPD (and its predecessors).[40] Our findings are virtually unaffected by these additional robustness checks. For details, see A.2 in the Appendix.

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 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.[41] 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 use them in the model of Equation (1). However, we can employ them in a regional fixed effects model along the lines of Equation (2).[42] Finally, we will present regression results using data on sick funds' expenditures that give us further insights into how BHI could reduce mortality.

[40] 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.

[41] We cannot exclude that these findings are driven by a supply side expansion as a consequence of Bismarck's reform.

[42] 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.

4.4.1 Further evidence against confounding factors

To address further concerns, the subsequent analysis relies on mortality data that distinguished among a range of thirty causes of death. 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 were 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 of deaths by accidents and unlikely to be related to the introduction of the accident insurance. 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 (2012) who show that the introduction of the accident insurance resulted in an increase of work-related accidents due to moral hazard.

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 infection with waterborne disease requires both knowledge about transmission and access to infrastructure, avoiding airborne diseases only requires knowledge about transmission channels. 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 is sufficient for reducing the risk of infection with airborne diseases.

And 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.[43] 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 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. Insurance funds employed inspectors whose task was to pay unexpected home visits to monitor the curfew and medication intake of patients.[44] Moreover, health insurance physicians tried to influence all members of the household through particularly addressing women, who were typically in charge of care and food (Frevert, 1981).[45]

[43] Initially, tuberculosis was typically assumed to be hereditary since usually the entire family suffered from its symptoms.

[44] 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).

[45] 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.*

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.[46]

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. Last but not least, we find some evidence for a reduction in maternal death in childbed (column 6). These results suggest that BHI did not exclusively affect communicable disease mortality and leave room for explanations related to improved access to medication and the provision of sick pay.

In an attempt to distinguish between these remaining 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, and maternity benefits. 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 and postpartum support 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

[46] 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.

expenditures per insured. To avoid most pressing concerns of reverse causality, expenditures are lagged by one year.[47] 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 for a mechanism of knowledge diffusion through physicians. An increase in expenditures for doctor visits by one standard deviation decreases the mortality rate by 0.13 standard deviations. Similarly, an increase in medication expenditures decreases the mortality rate (column 2), suggesting that advances in medical treatment were an important channel. However, we do not find significant effects for hospitalizations costs (column 3). Interestingly mortality does not respond to changes in the 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 though income smoothing. However, the negative effect of maternity benefits 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 newborns' health. In column 6, we find a positive relationship between death benefits and the mortality rate. This finding reminds us that these simple regression results are prone to endogeneity bias. Especially in the case of death benefits reverse causality is likely to be present.[48] In column 7, the model yields a significant negative effect of expenditures for administration on mortality. Since sickness inspectors, who were employed to monitor the hygienic situation of the insured, enter into this category, we consider these findings to be supportive of the knowledge diffusion channel.

In sum, this exercise provides further evidence that providing a new group of people with access to physicians played a crucial role for the mortality decline. New knowledge on hygiene provided by physicians and sickness funds' inspectors 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. At the same time, access to free medication and maternity benefits under BHI seem to have played a significant role for the mortality decline. However,

[47] Results are robust to changes in the lag structure.

[48] An alternative interpretation is that the death of an insured person is indicative of deterioration of the health prospects of the entire family by reducing access to health care.

we find not find any evidence in support of the view that sick pay was important for the 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 first 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 effects on mortality of the first compulsory health insurance in the world. 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 start with evidence from time-series data on long-run mortality in Prussia, move on to an international comparison, and finally use difference-in-differences type frameworks that exploit the compulsory nature of the health insurance scheme for blue-collar workers. The different empirical approaches yield a consistent pattern that suggests that BHI played 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 1.65 deaths per 1,000 blue collar workers by the end of the century. In other words, blue collar workers' mortality decreased by 7.8 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 free 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 families at the lower end of the income distribution with access to physicians and new knowledge on hygiene that they 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 U.S. health insurance, 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. Analytically, Bismarck's Health Insurance — by the end of the 19th century — had reduced the mortality of blue collar workers by 7.8 percent compared to baseline mortality. 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 papers.

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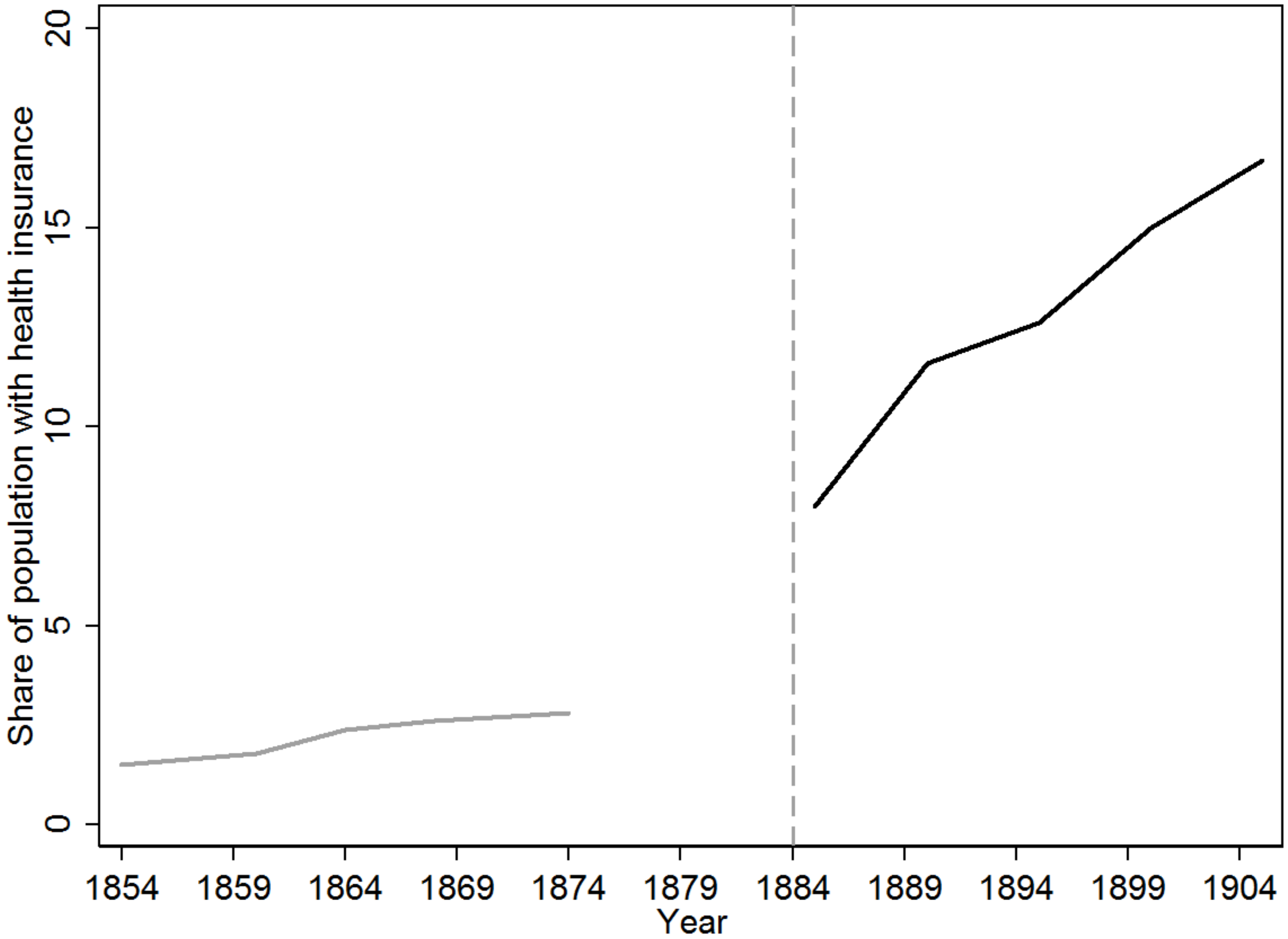

Figure 1: Expansion of Health Insurance in Prussia

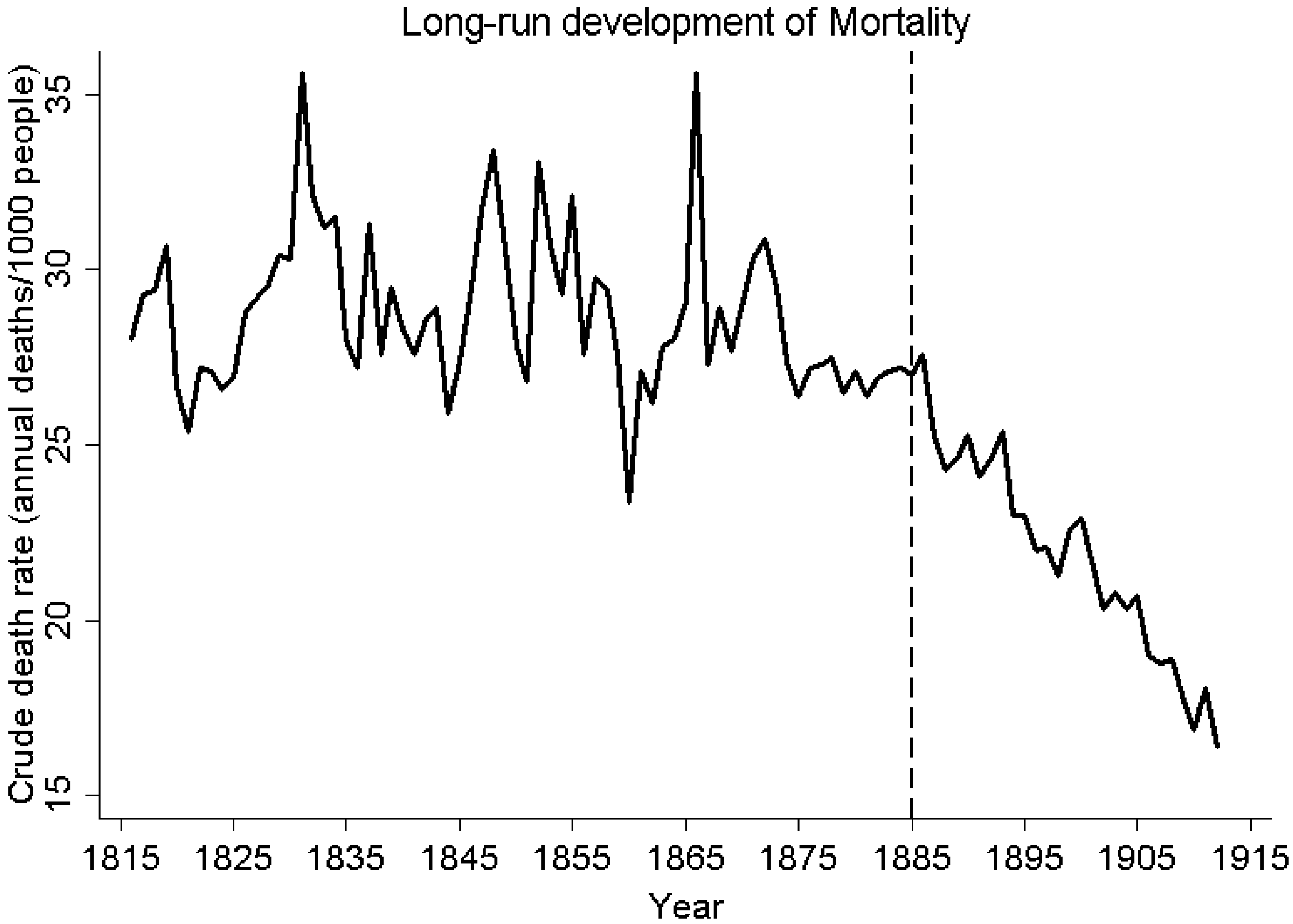

Figure 2: Long-run development of mortality in Prussia

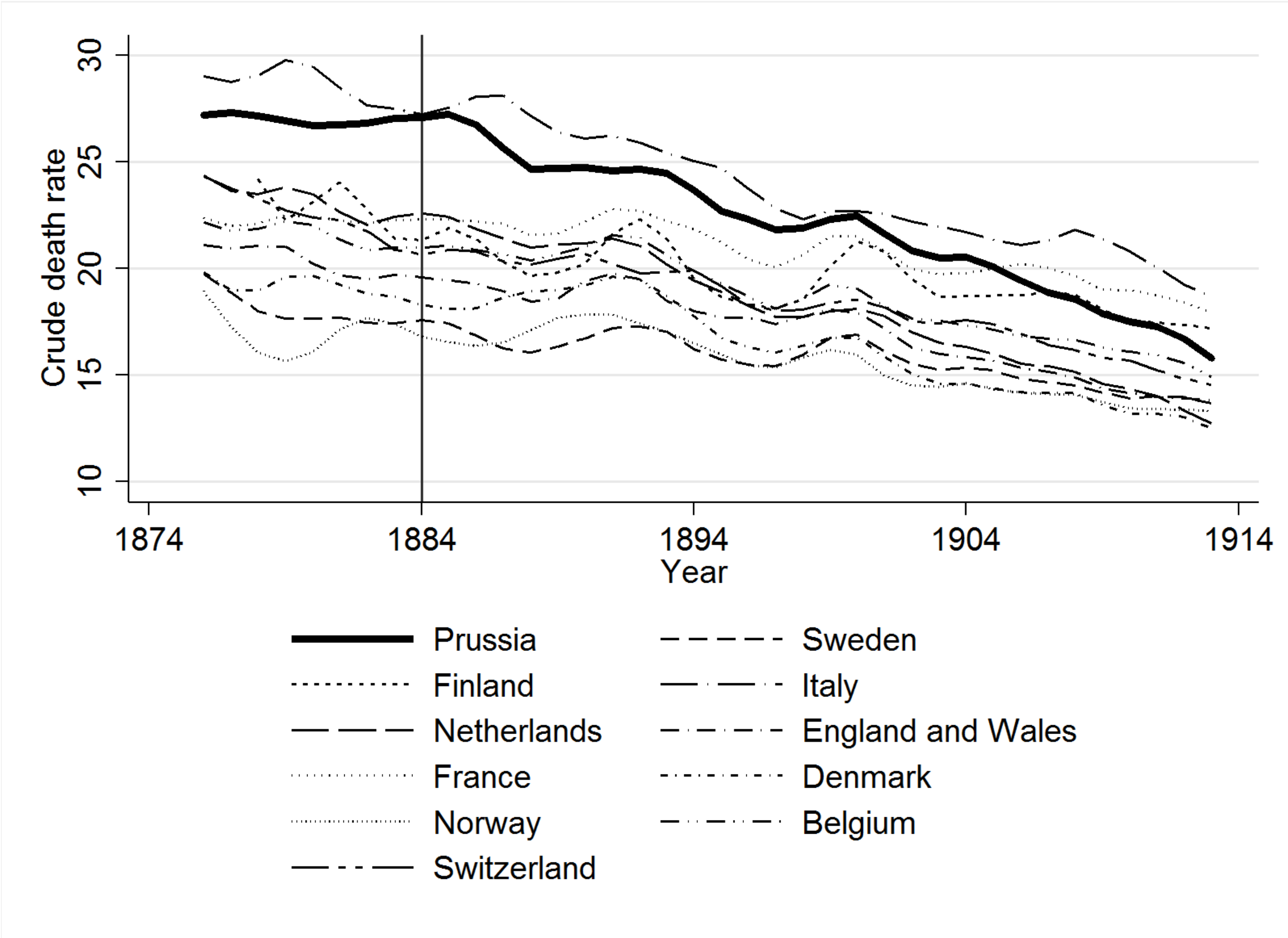

Figure 3: Mortality decline across Western European countries

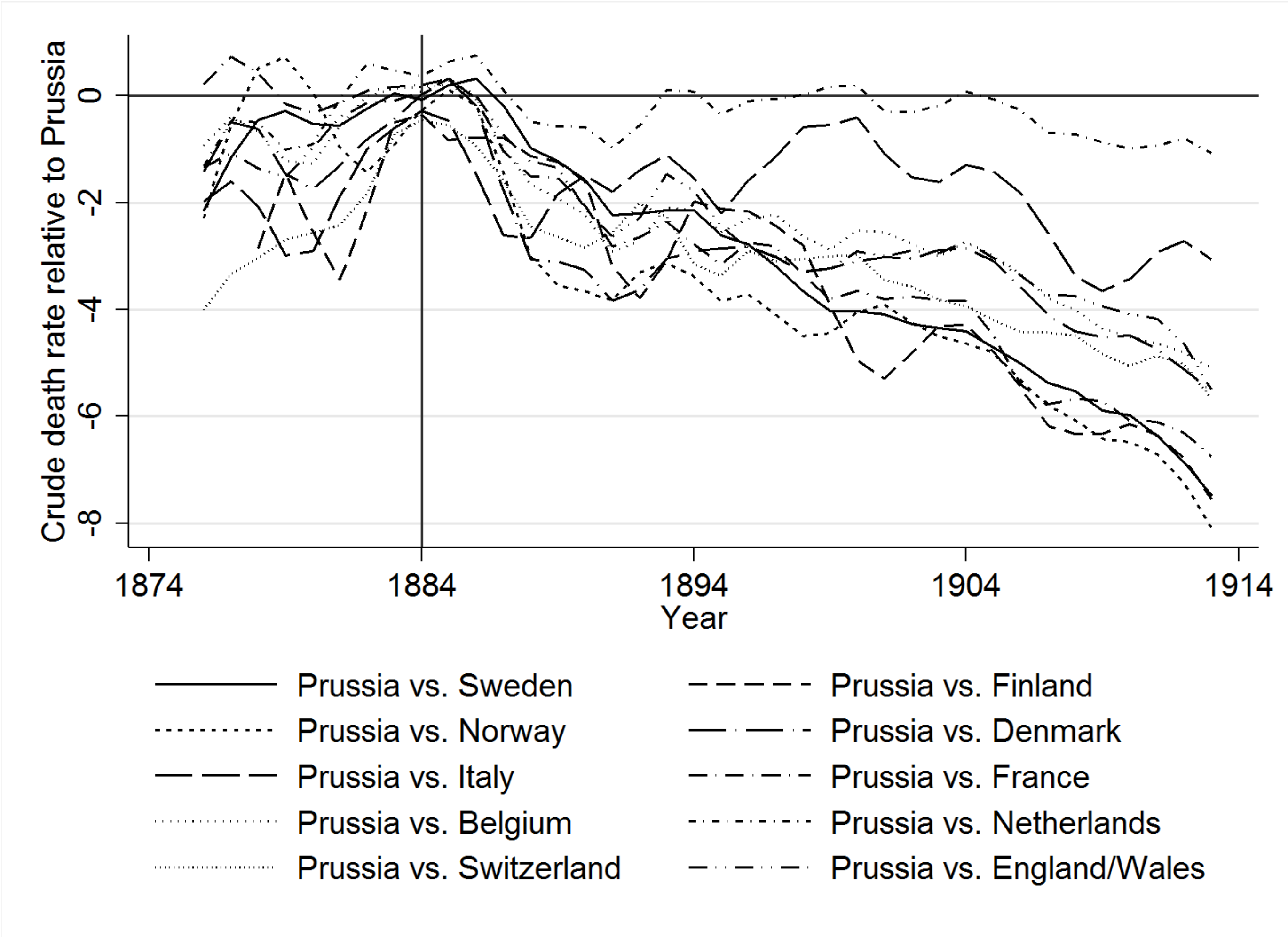

Figure 4: Mortality decline relative to Prussia

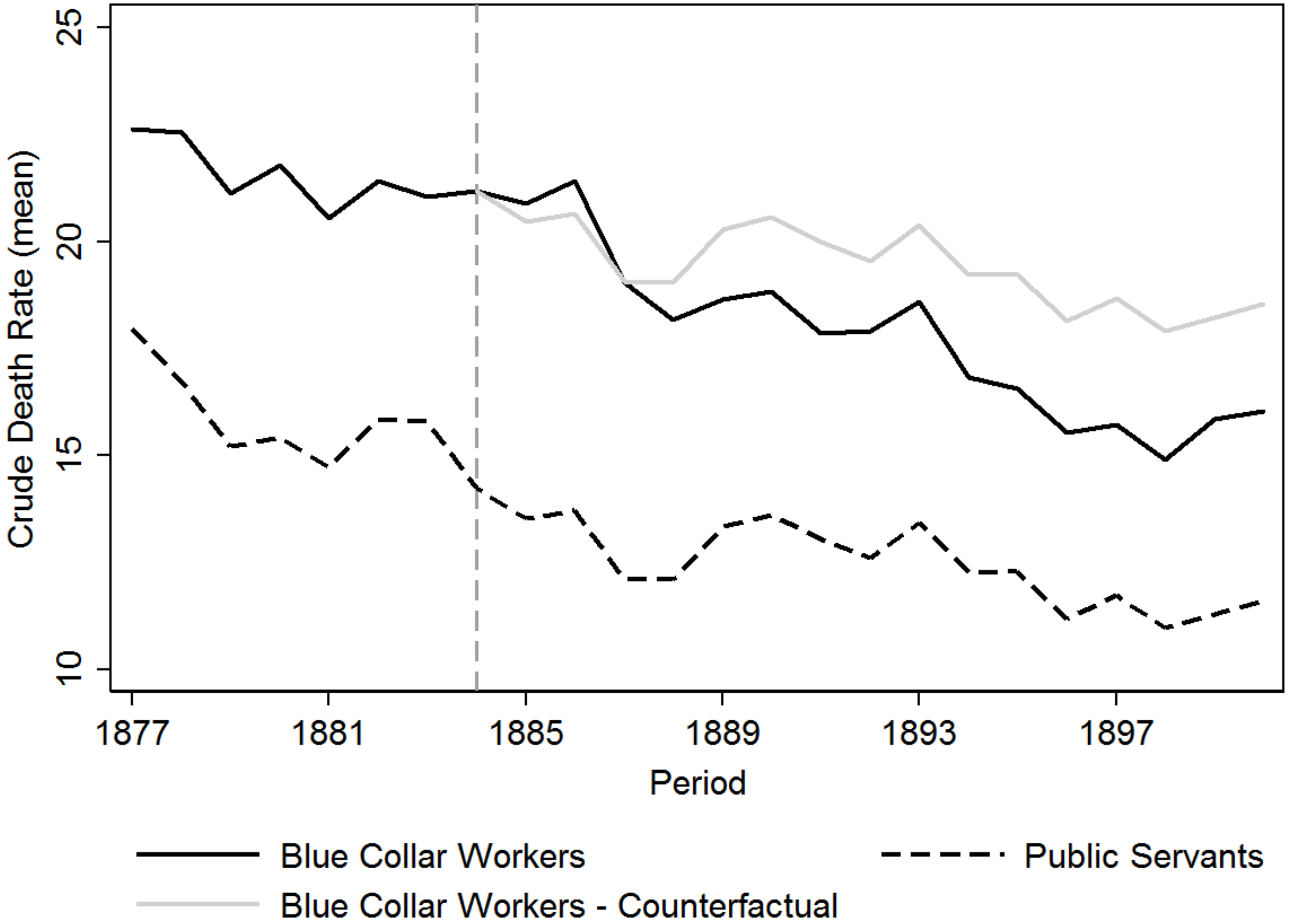

Figure 5: Crude death rates: Blue collar vs. public servants

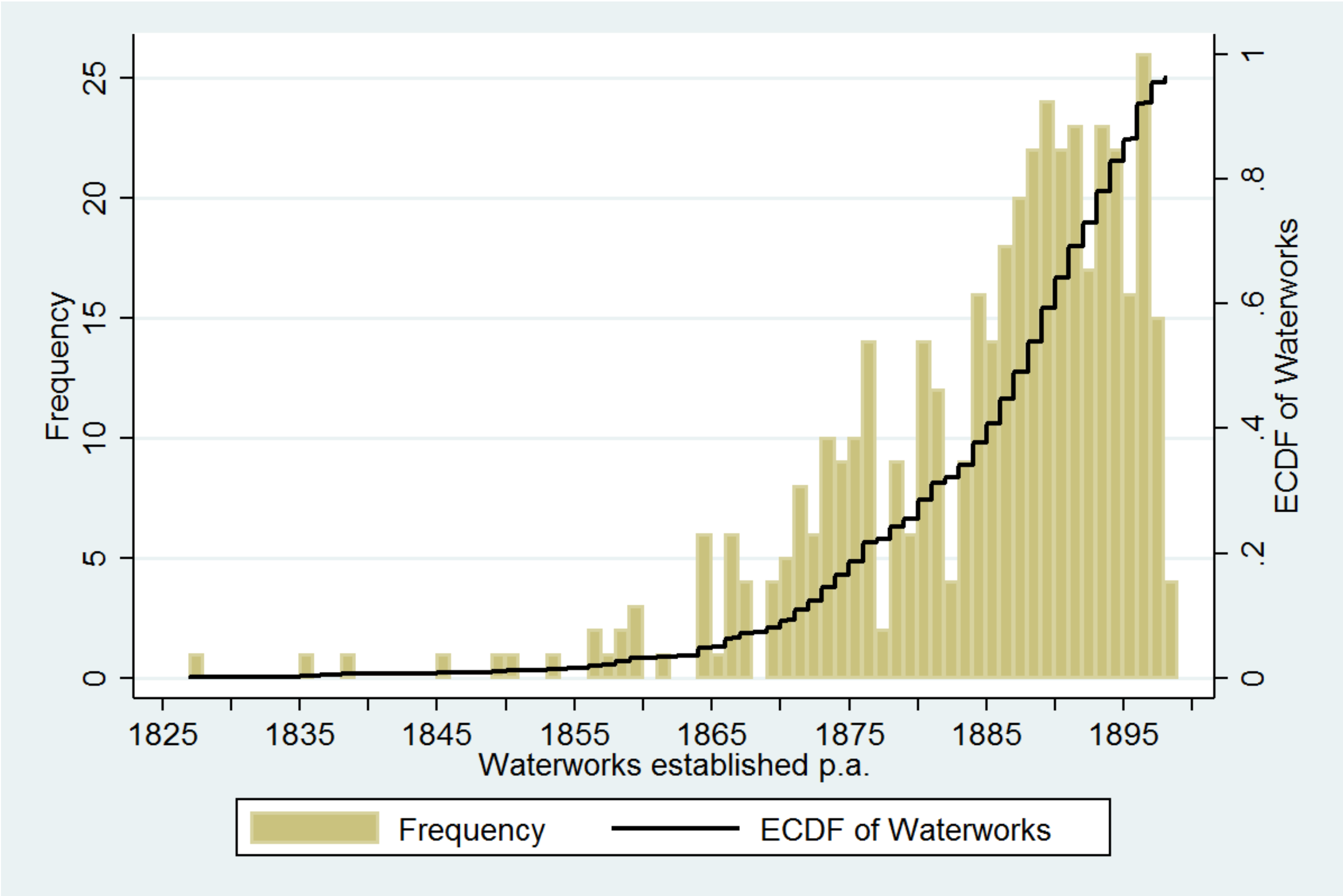

Figure 6: The roll-out of waterworks in Prussia

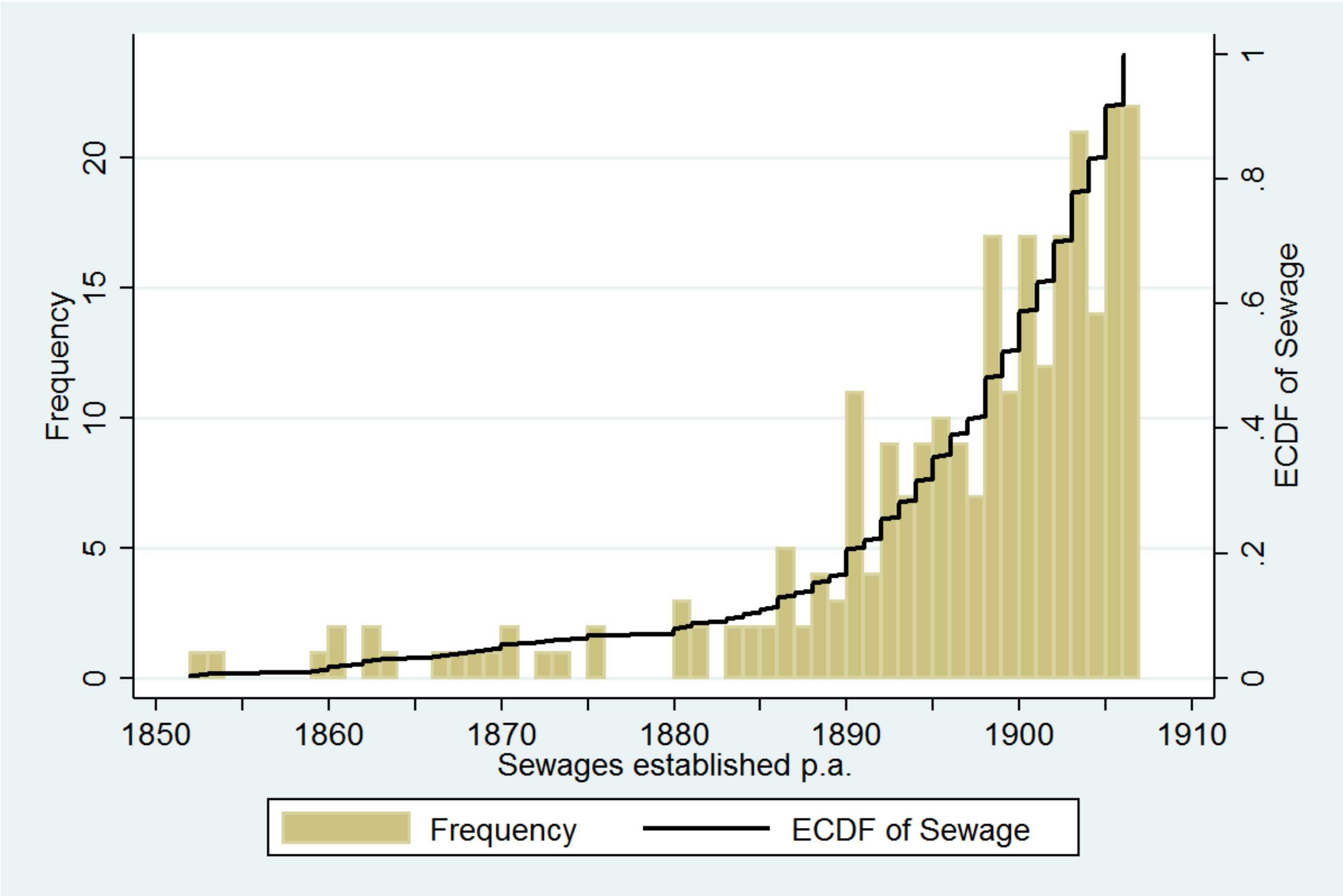

Figure 7: The roll-out of sewerage in Prussia

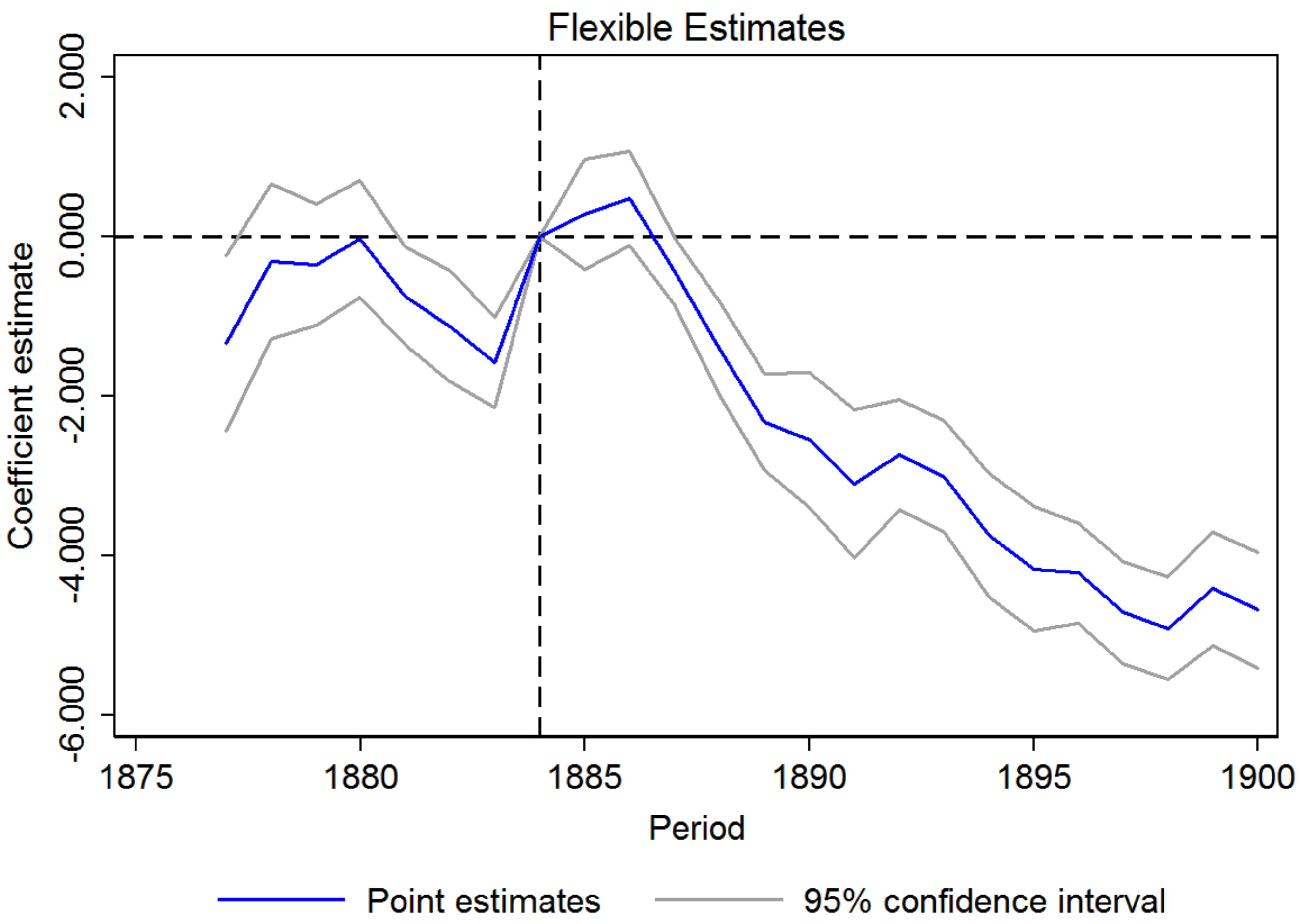

Figure 8: Annual DiD estimates

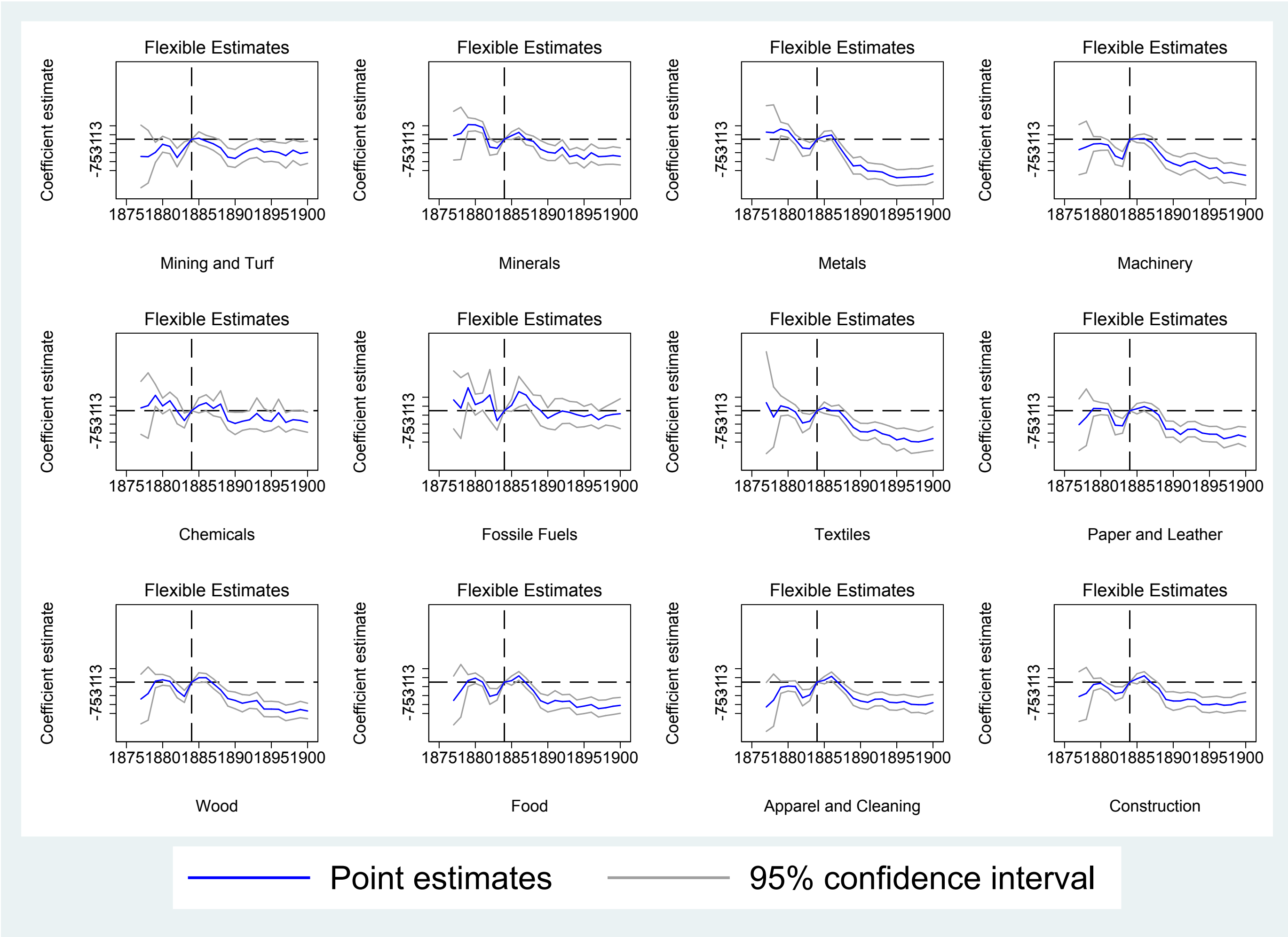

Figure 9: Annual DiD estimates for separate industries

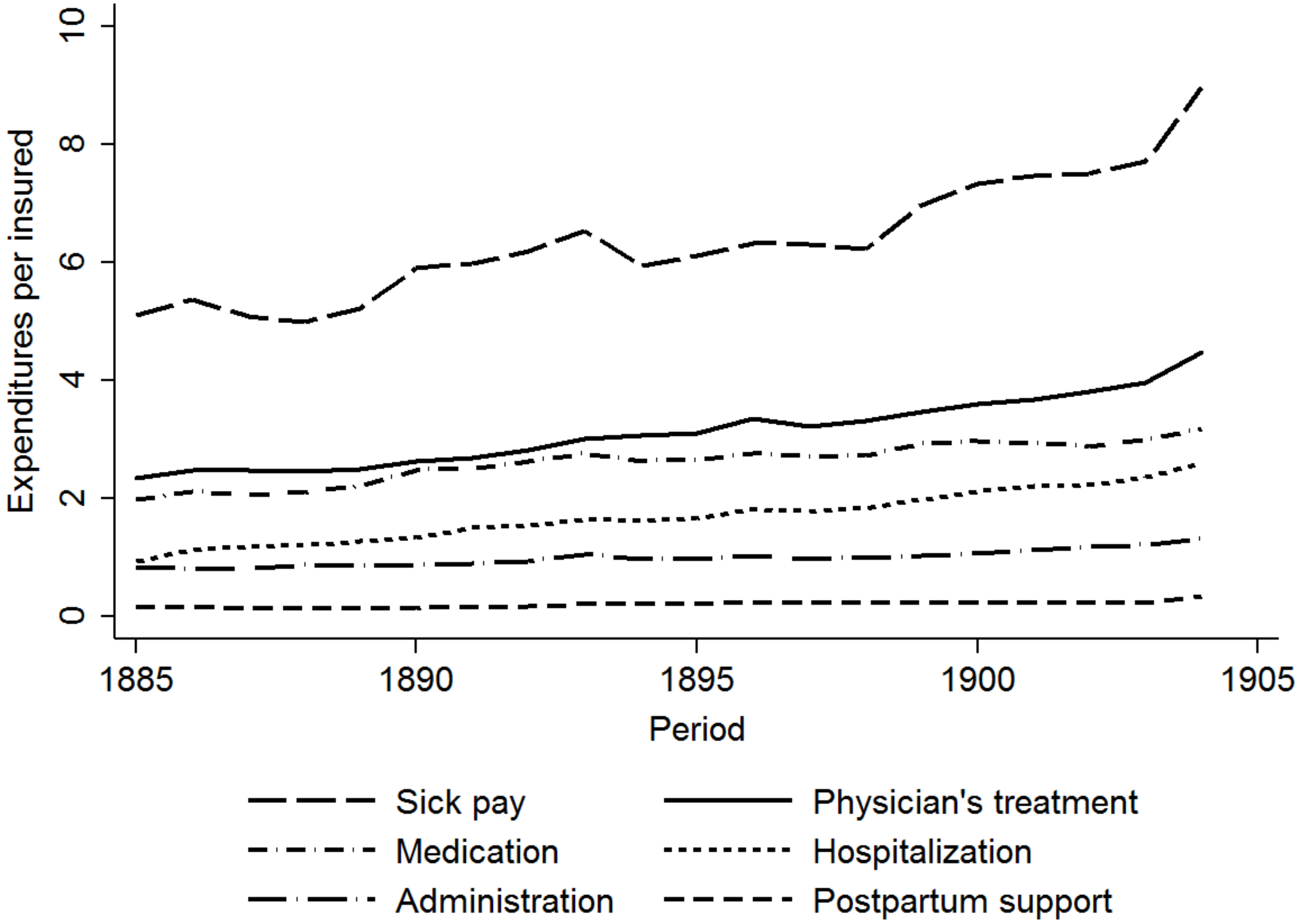

Figure 10: Sickness funds' expenditures per insured in Marks

Table 1: Occupational 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: Population in thousands. Data taken from occupation censuses conducted in the German Empire in 1882, 1895, and 1907. Sectors refer to official sector B (Industry) and E (public). *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.	Base (1)	Urban (2)	Waterworks (3)	Sewerage (4)
BlueCollar x 1877	-0.1905	-0.2767	-0.3233	-0.2004
	(0.2495)	(0.2784)	(0.2720)	(0.2515)
BlueCollar x 1885	1.1325***	1.2109***	1.2217***	1.1590***
	(0.1616)	(0.1475)	(0.1603)	(0.1595)
BlueCollar x 1889	-0.7339***	-0.5881**	-0.4385*	-0.6635**
	(0.2573)	(0.2310)	(0.2536)	(0.2658)
BlueCollar x 1893	-1.3014***	-1.0734***	-0.8695***	-1.1875***
	(0.2560)	(0.2724)	(0.2886)	(0.3065)
BlueCollar x 1897	-1.6540***	-1.2496**	-1.1373***	-1.4888***
	(0.2842)	(0.4798)	(0.3366)	(0.4069)
Urbanization x Occupation	No	Yes	No	No
Waterworks x Occupation	No	No	Yes	No
Sewerage x Occupation	No	No	No	Yes
District x Occupation 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

Notes: Table reports flexible DiD estimates. All variables are averaged over four year periods from 1877-1900. 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. Standard errors clustered at the district level in parentheses. * 10%, **5%, *** 1% confidence level

Table 3: Flexible DiD: heterogeneity

Dep. var.: Deaths per occ. Pop.	Base	Adult Male	Adult Female	Kids
	(1)	(2)	(3)	(4)
BlueCollar x 1877	-0.1905	-0.2996	-0.7517***	0.0292
	(0.2495)	(0.2822)	(0.1640)	(0.1689)
BlueCollar x 1885	1.1325***	0.9058***	0.1703	0.6533***
	(0.1616)	(0.1570)	(0.1074)	(0.1167)
BlueCollar x 1889	-0.7339***	-0.6812***	-0.2988*	-0.2516
	(0.2573)	(0.2044)	(0.1538)	(0.1648)
BlueCollar x 1893	-1.3014***	-0.9128***	-0.3604**	-0.7350***
	(0.2560)	(0.2205)	(0.1644)	(0.1692)
BlueCollar x 1897	-1.6540***	-1.3008***	-0.5787***	-0.9260***
	(0.2842)	(0.2668)	(0.1962)	(0.2094)
District x Occupation FE	Yes	Yes	Yes	Yes
District x Time FE	Yes	Yes	Yes	Yes
Observations	432	432	432	432
R-squared	0.94	0.91	0.89	0.93

Notes: Table reports flexible DiD estimates. All variables are averaged over four year periods from 1877-1900. Dependent variable measures crude deaths rates using deaths by occupation of the household head per alive occupational population (including dependents) in thousands. *Adult Male and Adult Female* use occupational working population by gender as denominator. The omitted period is 1881-84. Standard errors clustered at the district level in parentheses. * 10%, **5%, *** 1% confidence level

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

	Base (1)	Controls (2)	Male (3)	Female (4)	Infants (5)	LegInf (6)	IllegInf (7)	Placebo (8)
Treatment x 1875	1.891	1.606	-5.714***	0.711	-20.264*	-23.030*	10.630	2.946
	(1.623)	(1.621)	(1.713)	(1.764)	(11.363)	(11.948)	(56.422)	(3.650)
Treatment x 1885	-7.768***	-7.417***	-7.433***	-6.690***	4.018	1.761	27.650	1.452
	(1.693)	(1.689)	(1.965)	(1.596)	(11.484)	(11.989)	(47.676)	(3.098)
Treatment x 1890	-4.754**	-3.983*	-6.971***	-6.550***	-19.437	-30.964**	45.986	-1.712
	(2.041)	(2.053)	(2.395)	(2.079)	(15.279)	(15.668)	(55.537)	(3.798)
Treatment x 1895	-8.631***	-7.619***	-5.881**	-8.454***	-34.598*	-55.767***	79.660	-3.007
	(2.080)	(2.092)	(2.570)	(2.153)	(20.049)	(20.287)	(77.044)	(4.627)
Treatment x 1900	-12.330***	-11.380***	-16.123***	-13.119***	-45.863*	-73.912***	103.370	1.023
	(2.399)	(2.372)	(2.813)	(2.500)	(24.973)	(24.832)	(79.473)	(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: Table reports county-level fixed effects estimates. All variables are averaged over five year periods from 1875-1905. Dependent variable measures crude death rates using total deaths per alive population in thousands. Treatment variable is blue collar workers share observed in 1882, interacted with time-period dummies. Column *Controls* adds the urbanization rate, waterworks per capita, and sewerage per capita. Dependent variable in Column *Male* is male mortality. Dependent variable in Column *Female* is female mortality. Dependent variable in Column *Infants* is infant mortality (< 1 year) per 1,000 births. Dependent variable in Column *LegInf* is infant mortality (< 1 year) born in wedlock per 1,000 births in wedlock; Dependent variable in Column *IllegInf* is infant mortality (< 1 year) born out of wedlock per 1,000 births out of wedlock. Dependent variable in Column *Placebo* is total mortality; treatment variable is public servants share observed in 1882, interacted with time-period dummies. Standard errors, clustered at the county level, in parentheses. * 10%, **5%, *** 1% confidence level

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

	Initial Blue Collar Workers (1882)					Initial Insured (1885)				
Dep. var.: Crude DR	Base	Controls	Male	Female	LegInf	Base	Controls	Male	Female	LegInf
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment x 1875	3.37	3.41	-3.19	2.01	13.30	5.04	4.67	-3.48	3.56	17.71
	(3.35)	(4.47)	(4.41)	(5.40)	(42.84)	(4.30)	(5.12)	(4.70)	(6.44)	(49.36)
Treatment x 1885	-11.19**	-11.14**	-12.34**	-12.20**	-23.87	-15.01**	-14.96**	-15.55**	-15.69**	-54.41
	(4.60)	(5.30)	(5.72)	(5.31)	(41.39)	(6.11)	(6.64)	(7.33)	(6.75)	(47.36)
Treatment x 1890	-7.07	-7.27	-12.25	-12.51	-93.97	-11.27	-11.90	-18.87	-18.04	-153.11*
	(7.90)	(8.55)	(11.15)	(9.37)	(71.55)	(10.65)	(11.13)	(14.44)	(12.21)	(83.26)
Treatment x 1895	-11.64	-11.97	-12.13	-15.64*	-154.22	-16.73*	-17.76*	-19.48	-22.39**	-234.92**
	(6.98)	(7.23)	(9.97)	(7.82)	(93.18)	(9.16)	(9.08)	(12.68)	(9.72)	(105.54)
Treatment x 1900	-15.05**	-14.91**	-19.21**	-19.20**	-162.05*	-21.83**	-22.70***	-28.81**	-28.66***	-242.41**
	(7.27)	(6.80)	(9.42)	(7.18)	(88.70)	(9.00)	(8.17)	(11.68)	(8.52)	(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: Table reports district-level fixed effects estimates. All variables are averaged over five year periods from 1875-1905. Dependent variable in Columns 1, 2, 6, and 7 is crude death rates measured as total deaths per alive population in thousands. Dependent variable in Columns 3 and 8 is male mortality. Dependent variable in Columns 4 and 9 is male mortality. Dependent variable in Columns 5 and 10 is infant mortality (< 1 year) born in wedlock per 1,000 births in wedlock. Treatment variable in columns 1-5 is blue collar workers share observed in 1882, interacted with time-period dummies. Treatment variable in columns 6-10 is 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, clustered at the district level, in parentheses. * 10%, **5%, *** 1% confidence level

Table 6: District fixed effects: causes of death

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

Notes: Table reports district-level fixed effects estimates. All variables are averaged over five year periods from 1875-1905. Dependent variable measures death rates by cause of deaths using total deaths per alive population in thousands. Treatment variable is blue collar workers share observed in 1882, interacted with time-period dummies. *Accident* is death from accident; *Waterborne* death from typhus, typhoid fever, and three types of diarrheal diseases; *Airborne* death from smallpox, scarlet fever, measles, diphtheria, pertussis, scrofula, tuberculosis, tracheitis, pneumonia and other lung diseases; *Non-infectious* death from cancer, edema, stroke, heart disease, brain disease, and kidney disease; *Maternal* death in childbed. Controls include the urbanization rate, waterworks per capita, and sewerage per capita. Standard errors, clustered at the district level, in parentheses.
* 10%, **5%, *** 1% confidence level

Table 7: Mortality and health expenditures

Dep. var.: Total Mortality Rate	(1)	(2)	(3)	(4)	(5)	(6)	(7)
L.Doctor visits(std)	-0.132**						
	(0.050)						
L.Medication(std)		-0.160***					
		(0.046)					
L.Hospitalization(std)			-0.093				
			(0.074)				
L.Sickpay members(std)				-0.025			
				(0.065)			
L.Maternity ben.(std)					-0.068*		
					(0.040)		
L.Death ben.(std)						0.059	
						(0.038)	
L.Administration(std)							-0.130**
							(0.049)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	682	682	682	682	682	682	682
Districts	36	36	36	36	36	36	36
Periods	18	18	18	18	18	18	18
R-squared	0.80	0.80	0.80	0.80	0.80	0.80	0.80

Notes: Table reports OLS panel estimates. Dependent variable measures crude death rates using total deaths per alive population in thousands averaged over five year periods 1885-1905. Explanatory variable is sickness fund expenditure per insured measured at the beginning of each 5 year period (1885, 1890, 1895, 1900). All variables are standardized with mean zero and unit standard deviation. Controls include the urbanization rate, waterworks per capita, and sewerage per capita. Standard errors, clustered at the district level, in parentheses. * 10%, **5%, *** 1% confidence level

A Appendix

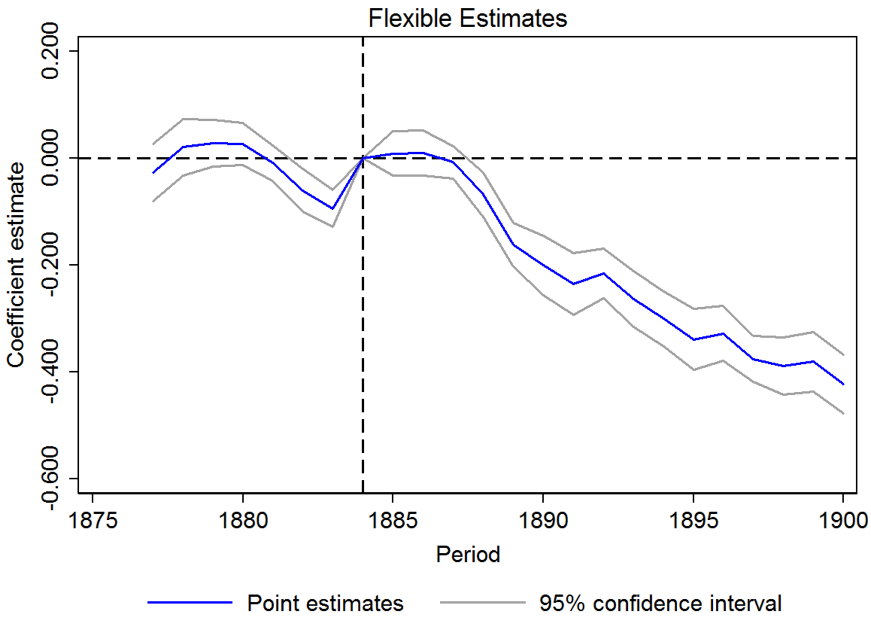

Figure A.1: Annual DiD estimates in log-specification

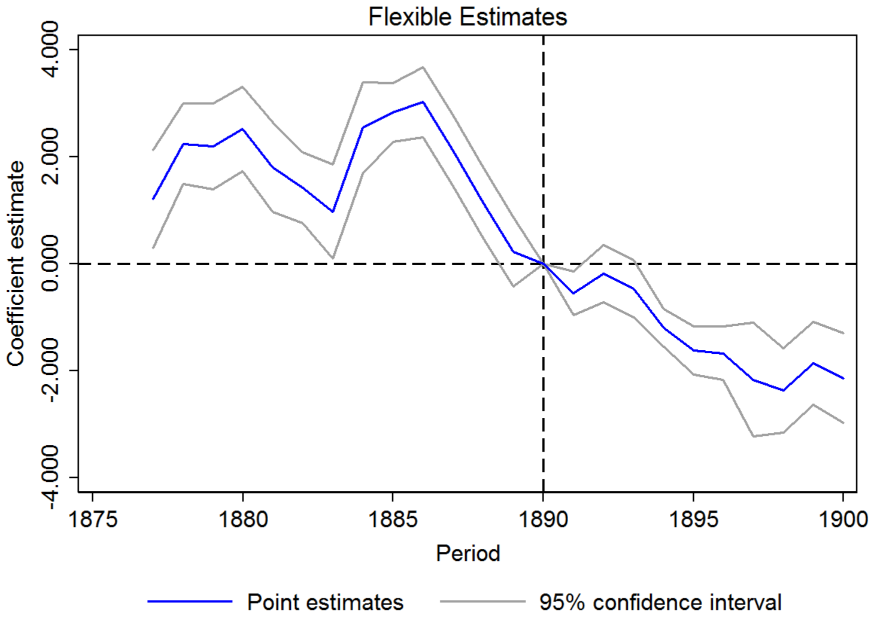

Figure A.2: Annual DiD estimates using 1890 as reference year

Table A.1: 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	628	2.6%
1874	25,352	715	2.8%
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: Population and insured in thousands. Data refer to Prussia within its respective borders. Insurance benefits vary pre- and post-1884.

Table A.2: District fixed effects including age composition and vote share controls

	Initial Blue Collar Workers (1882)				Initial Insured (1885)			
Dep. var.: Crude DR	Average age	Dependency ratio	Age groups	SPD vote	Average age	Dependency ratio	Age groups	SPD vote
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment x 1875	3.46	0.72	-0.78	2.88	4.59	2.98	-0.77	4.48
	(4.40)	(5.02)	(4.31)	(4.28)	(5.13)	(5.97)	(4.51)	(5.00)
Treatment x 1885	-11.77**	-9.73**	-11.11**	-10.91**	-15.21**	-12.62**	-14.80**	-14.85**
	(5.22)	(4.59)	(5.03)	(5.33)	(6.51)	(6.11)	(6.42)	(6.69)
Treatment x 1890	-8.49	-1.95	-10.80	-5.84	-12.53	-5.28	-16.57	-11.11
	(8.57)	(7.66)	(9.16)	(8.38)	(11.02)	(10.49)	(11.72)	(11.22)
Treatment x 1895	-12.96*	-5.12	-15.46**	-10.40	-17.87**	-8.43	-22.29**	-16.81*
	(6.82)	(6.04)	(7.20)	(7.23)	(8.52)	(8.34)	(8.73)	(9.50)
Treatment x 1900	-16.39**	-13.11**	-23.07***	-13.19*	-22.79***	-17.95***	-32.44***	-21.63**
	(6.31)	(5.15)	(8.34)	(6.85)	(7.40)	(6.36)	(9.11)	(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
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: Table reports district-level fixed effects estimates. All variables are averaged over five year periods from 1875-1905. Dependent variable is crude death rate measured as total deaths per alive population in thousands. Treatment variable in columns 1-4 is blue collar workers share observed in 1882, interacted with time-period dummies. Treatment variable in columns 5-8 is 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 inlcuded 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. The urbanization rate, waterworks per capita, and sewerage per capita are included as covariates in all regressions. Standard errors, clustered at the district level, in parentheses. * 10%, **5%, *** 1% confidence level