Physicians and the production of health:

Returns to health care during the mortality transition

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Abstract

This paper investigates the returns to health care provision during the mortality transition. We construct a new panel data set covering German municipalities from 1928 to 1936. The endogeneity of health care supply is addressed by using the expulsion of Jewish physicians from statutory health insurance as exogenous variation in regional physician supply. Increases in the supply of physicians reduce infant mortality and mortality from common childhood diseases. Using a semiparametric control function approach, we find diminishing marginal returns to health care provision. The results are consistent with historical trends in infant mortality over the 20th century.

Keywords: infant mortality, physicians, health care supply, mortality transition, semiparametric IV

JEL classification: I10, I18, N34

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1 Introduction

The reduction in infant mortality among industrialized countries beginning in the late 19th century constitutes an unprecedented development in human history. Before, infant mortality rates in Europe had essentially been unchanged since the middle ages. In 1900, 225 children out of 1,000 born died within their first year of life in Germany. In 1950, it was less than 60 and in 2015, the rate was down to 3.1, a decrease of more than 98% over a century (see Figure 1). This mortality transition can be observed across all of Western Europe and the United States (Caselli 2015). The accompanying improvements in life expectancy have been shown to be important for long-run economic growth and development (Bloom et al. 2014). In contrast to this, infant mortality rates still remain high in many developing economies. Of 4.5 million infant deaths worldwide in 2015, 99% occurred in developing countries (You et al. 2015).

The mortality decline in developed economies coincides with important public health developments. Standards of living, nutrition and public hygiene started improving at the end of the 19th century. Moreover, health care supply and public health infrastructure were expanded substantially (e.g. Cutler et al. 2006). In Germany, the ratio of physicans per population increased more than twofold between 1900 and 1950 (see Figure 1). Physicians provide pre- and postnatal care, attend birth, administer medication and encourage health-related behavior and compliance with hygienic standards.

[Figure 1 about here]

In this paper, we analyze the role of physicians for infant health in the early 20th century, towards the end of the mortality transition. We find that improvements in health care supply can reduce infant mortality, stillbirths and mortality from common childhood diseases. Moreover, we investigate how health care supply and infrastructure interact and find that physicians matter especially in those regions where access to infrastructure is limited. To pinpoint where improving coverage is most effective, we provide non-parametric estimates of the marginal returns to physicians. In doing so, we evaluate the premise of diminishing returns to health care provision. Improving medical care may be especially effective in regions where coverage is sparse; whereas increasing supply in regions where levels are already high may contribute little to reducing mortality. To evaluate this, we develop a semiparametric instrumental variable (IV) estimation approach. We combine a control function approach with a partially linear model in the spirit of Robinson (1988) to derive a non-parametric estimate of the dose-response function. We find that health effects are broadly positive, but subject to rapidly diminishing returns. Physicians have large positive effects on infant mortality especially when baseline coverage is sparse.

Our analysis relies on a large sample of administrative data covering the period from 1928 to 1936. Detailed information on causes of death and disease incidence allows us to examine specifically which medical conditions physicians can influence. To identify the causal effect of physicians on mortality, we utilize a series of discriminatory policies introduced in Germany in 1933 which banned Jews and those with left-wing political affiliations from public positions and severely limited the professional activity of Jewish physicians. Within a system of universal public health insurance, laws were introduced which excluded Jewish physicians from being reimbursed for rendering medical services, leading many physicians to emigrate to other countries. Jews were disproportionately over-represented in the medical profession, with up to 17% of physicians considered Jewish by the Nazi definition in 1933, compared to less than 0.7% in the overall population (Kröner 1989). The distribution of the Jewish population varies across German municipalities, providing variation in treatment exposure. The variation induced by the occupational restrictions allows us to solve the problem of positive selection.

Our findings suggest that one additional doctor per 1,000 of population reduces infant mortality by about 18 cases per 1,000 live births. This corresponds to about a 23% reduction in baseline mortality. While this effect is sizeable, an additional physician is also a large increase in coverage—in our sample, increasing the coverage ratio by one additional doctor is approximately equivalent to doubling the supply of physicians. A one standard deviation increase in the number of physicians translates to 0.45 standard deviation decrease in infant mortality. In addition, our results show that reductions occur for deaths from inflammatory bowel diseases and stillbirths. We also find reductions for mortality from measles, influenza and bronchitis. Fatalities due to pre-term birth and congenital conditions, for which medical treatment is difficult, are unaffected. Mortality from these health conditions accounts almost completely for the remaining infant mortality in developed countries today. Due to the disparity in population size, we can rule out that the mortality effects we document are driven by higher mortality among the Jewish population alone.

We document pronounced effect heterogeneity. Mortality effects are larger in municipalities with limited hospital capacity and without specialized hospital infrastructure. Using the semi-parametric IV method, we demonstrate that the effects of physician supply are highly nonlinear and disappear after a coverage of about two physicians per 1,000 of population. Our results align with the historical development of infant mortality in Europe and the US over the 20th century—infant mortality rates have been largely stable since the 1980s, when most countries reached comparable coverage ratios.

Our work in this paper contributes to several distinct branches of literature. First, our results add to the literature on the value of health insurance and access to care. Several recent studies have evaluated the introduction of health insurance schemes and found broad health and mortality effects (e.g. Card et al. 2008, Finkelstein and McKnight 2008). Evaluating the introduction of Medicaid in the 1960s, Goodman-Bacon (2018) finds large reductions in infant and child mortality driven by permitting at-risk population groups access to health care and relates the reductions to improved acute care at birth. Bauernschuster et al. (2020) evaluate the introduction of health insurance in Germany in 1884 and find reductions in infant mortality through coverage extensions to dependent family members. The main mechanism driving their result is improved access to physicians. We complement these results by focusing on the intensive margin of care provision and the shape of the dose-response relationship. Our results suggest that the effect of additional supply depends crucially on the existing level of provision.

Second, our results add to the large literature on the effects of health care services and infrastructure on health. Many papers have documented broadly positive effects of the availability of health care infrastructure on child health (e.g. Lavy et al. 1996). Similar effects have been documented for increases in supply-side financing (e.g Gruber et al. 2014) and improved public infrastructure due to the germ theory of disease (e.g. Cutler et al. 2006). A lot of attention in the literature on the mortality transition has focused on water purification efforts (e.g. Cutler and Miller 2005, Alsan and Goldin 2019). More recent papers also demonstrate the effectiveness of public health campaigns and maternal and infant health care centers (e.g. Anderson et al. 2019, 2021, Hoehn-Velasco 2018, Bhalotra et al. 2017, Wüst 2012, Bütikofer et al. 2019).

Complementing the evidence from these papers, we focus on the role of physician human capital and its interaction with the availability of health care infrastructure. Our results suggest that physicians matter especially in places where access to health care infrastructure is limited.

Third, we contribute to the literature on the effects of physicians. Although previous work has established a connection between physician supply and health, causal evidence on how the supply of physicians affects health outcomes is limited. Many studies use cross-country data, relying on either cross-sectional comparisons or panel data methods to control for the influence of time-invariant unobservable factors (e.g. Kim and Moody 1992, Anand and Bärnighausen 2004, Farahani et al. 2009). Results from these studies vary. Although some find a negative relationship between physicians and mortality, many results are inconclusive. Micro data evidence is limited. Aakvik and Holmås (2006) use a dynamic panel approach to estimate the effect of general practitioner coverage on total mortality in Norwegian municipalities from 1986 to 2001, but do not find a significant relationship.

A limiting factor of these studies is their inability to account for time-varying endogenous changes in the supply of physicians. Moreover, sample sizes are often small and the data quality questionable. Most low- and middle-income countries do not have well-functioning vital registration systems and estimates of the infant mortality rate often rely on a mixture of sources and reporting systems. Our paper addresses both of these issues. Using exogenous variation in the supply of physicians due to discriminatory measures allows us to obtain a causal estimate of the effect of an additional physician on infant mortality. Furthermore, population statistics and vital registration systems were already well established in Germany in the early 20th century, allowing us to rely on a comparatively large administrative dataset of German municipalities.

Fourth, we provide empirical evidence on the shape of the health production function. Diminishing marginal returns are a key feature of theoretical models (Grossman 1972), but real world empirical evidence is limited. Our estimated dose-response curve can be considered a partial estimate of the health care production function (Reinhardt 1972, Thurston and Libby 2002). We show that health care provision is especially effective at low levels of baseline provision, but subject to rapidly diminishing marginal returns. The nonparametric results also provide an intuitive explanation for previous null results in the literature on health care provision. Most developed countries reached levels of physician supply for which we find no effect any more in the early 1980s, and infant mortality rates have been mostly flat since.

Finally, our results add to the literature on negative human capital shocks and the consequences of large-scale discriminatory policies. Waldinger (2010, 2012, 2016) and Akbulut-Yuksel and Yuksel (2015) document negative effects of the Jewish persecution on educational attainment, research productivity and long-run output. We complement these findings by highlighting the severe consequences of physician brain drain and human capital loss in the health domain.

2 Institutional background

The 1933 German census registered 499,682 residents of Jewish faith. Overall, this constituted only a small share of the German population of about 65 Million, 0.77%. Since the census registered only citizens who confessed to the Jewish faith, this measure constitutes a lower bound for the share of the population considered Jewish by the extended definition introduced by the National Socialist German Workers Party (NSDAP). The Jewish population was distributed across all German regions, but concentrated in urban areas with over 70% of Jews residing in larger cities compared to 30% of the entire population.[1] Even though Jews constituted only a small part of the total population, they were disproportionately overrepresented in skilled occupations, especially in the medical profession. 10.9% of all doctors were of Jewish faith and approximately 17% were considered Jewish by the Nazis (Kröner 1989).

Shortly after seizing power in 1933, the Nazi government introduced discriminatory policies that limited the professional activity of Jewish citizens. On April 7, 1933, the NSDAP passed the *Gesetz zur Wiederherstellung des Berufsbeamtentums* [Law for the restoration of the professional civil service]. The law decreed that civil servants of non-Aryan descent or with dubious past political activity could not be trusted to be loyal to the state and were to be retired immediately. An implementation decree further specified that it was sufficient to have one Jewish grandparent to be considered of non-Aryan descent. Exceptions were granted to those who had been employed

[1] Figure A1 in the supplementary material shows the spatial distribution of the Jewish population across German districts. The smallest Jewish communities are located in rural areas in the north and the south. The largest Jewish communities lived in Frankfurt am Main (4.71%), Berlin (3.78%) and Breslau (3.23%). The corresponding map for infant mortality is shown in Figure A2.

since before August 1, 1914 or who had served in the First World War. However, this privilege could be declared exempt if a person was judged to be politically unreliable, an exception that was invoked frequently. This meant that non-Aryan doctors were forced into retirement at universities, publicly funded hospitals and all public medical institutions. By May 6, 1933 two executive orders extended the law to ordinary state employees, forcing resident physicians out of work as well.

On April 22, the law was followed by the *Verordnung über die Zulassung von Ärzten zur Tätigkeit bei den Krankenkassen* [Decree on the accreditation of doctors for health insurance funds], which withdrew the licence to practice for the compulsory health insurance fund from Jewish and other non-Aryan physicians. The same exceptions as above applied, but could again be declared void if the physician could be shown to have been active as a communist. The term communist was interpreted broadly and regularly included social democrats (Kröner 1989, Leibfried and Tennstedt 1980). Shortly after, an agreement between the association of German doctors and the association of private health insurance providers from July 1933 declared that bills from non-Aryan doctors were only reimbursed if they were subject to the exception clause or if the patient himself was of non-Aryan descent (Böhle 2003). This made it impossible for patients to be reimbursed for medical expenses when visiting Jewish physicians, and effectively deprived Jewish doctors from the possibility of treating privately insured patients as well.

Finally, a directive by the *Reichsärztekommissar* [Federal commissioner of physicians] from August 1933 specified that doctors of Aryan and non-Aryan descent were no longer allowed to stand-in for one another nor to refer patients to each other or to consult (Beddies et al. 2014). This also applied to non-Aryan physicians who profited from the exception clause and was especially harmful for specialists, who depended on referrals. According to the *Reichsvertretung der Juden in Deutschland* [Representation of German Jews], the directive rendered it virtually impossible for non-Aryan physicians to work in private hospitals and made the license for compulsory health insurance practice often worthless for those who still had one (Kröner 1989).

Germany has had universal public health insurance since 1883. With few exceptions, people are enrolled in a statutory health insurance plan, which provides a standardized level of coverage through one of the public or semi-private occupational insurance funds. Physicians treat patients on a fee-for-service basis and are reimbursed by the health insurance funds. At the time of our study, health insurance was compulsory for all employees with an income of less than 3,600 Reichsmark.[2] By barring Jewish physicians and those with left-wing political associations from submitting bills for reimbursement to the public insurance funds, the laws deprived physicians of their primary source of income.

The only remaining options for doctors whose health insurance licenses were revoked were to emigrate or to go into private practice. However, Klingenberger (2001) estimates that in 1930 there were only around 600,000 private patients in Germany, compared to a total population of around 65 million. Private practice could only provide a means of existence for a limited number of physicians and was associated with a severe reduction in income. Considering their future in Germany, many Jewish physicians opted to emigrate (Kröner 1989). Likewise, physicians who faced oppression for their political views or disagreed with the political development also left. The Jewish physicians who remained in Germany finally lost their medical licence in September 1938. The effects of migration can be seen in Figure 1, where the historically increasing trend in the number of physicians breaks in the 1930s and the supply of physicians decreases. Importantly, the occupational laws did not apply to other health personnel like nurses, but only affected physicians. Until 1938, occupational restrictions in the health care profession were limited to physicians.[3] Moreover, there is no evidence that the share of Jewish practitioners was as skewed among nurses as it was among physicians.

A detailed analysis of medical specialties is precluded by the fact that our settings predates the degree of specialization seen today. In the early 20th century, most physicians trained as general practitioners (GPs) and did not specialize in other domains. From aggregate data, we know that in 1933, 68% of all physicians in Germany were GPs. Those who specialized did so predominantly in internal medicine or surgery. Only 2.6% of physicians were trained in pediatrics, and 3.5% in gynaecology. These physicians mostly worked in larger cities with university-affiliated hospitals. Births were often attended by family physicians and Nazi health policies strengthened the role of

[2] This meant that only the very affluent could opt out as average income in 1927 was 1,742 Reichsmark (Klingenberger 2001). Treatment for cash payments was rare and restricted to a small minority of wealthy people.

[3] Only in September 1938, the *Gesetz zur Ordnung der Krankenpflege* [Law on the Regulation of Nursing Care] and accompanying ordinances decreed that Jewish nurses where solely allowed to treat Jewish patients or work in Jewish hospitals. These regulations stayed in place until 1945. Nursing was one of the few professions formally open to Jewish women in National Socialism (Steppe 2013).

the family physician in the care process surrounding child birth (Moissl 2005).[4]

In light of the escalating discrimination and persecution, we limit our study to the years 1936 and prior. In 1938, when Jewish physicians' medical licences were revoked, preparations for war were already under way. A year later, on September 1, 1939, Germany invaded Poland, initiating World War II and the Holocaust. Ghettos where set up to segregate Jews from the rest of the population. In the following years, thousands of detention sites were established all over German-occupied Europe. Deportation to forced labor and specialized extermination camps, mass shootings and pogroms commenced in 1941.

3 Data

We assemble a dataset from a combination of historical sources. The population and mortality figures originate from the *Reichsgesundheitsblatt* [Health bulletin] (eds. 1927–1936), a yearly statistical publication by the health ministry of the German Reich that tracks changes in the municipal population. The data covers all municipalities with a population above 15,000. Available information includes total population size, the number of births, marriages, and deaths. Mortality information is available in detailed categories in accordance with the International List of Causes of Death (ILCD), a classification system preceding the International Classification of Diseases (ICD) in use today. During the switch from ILCD-3 to ILCD-4 in 1930, the disease categories were expanded and partially redefined, restricting the coverage period for some variables in our data. Panel attrition is less than 3% since the population threshold of 15,000 was not adhered to strictly. We restrict the sample to those municipalities with full coverage. 7% of the municipalities in our sample merge or split during the observation period. We harmonize the affected municipalities to the municipal structure in 1933. For reasons outlined in the previous section, we restrict our sample to the time before 1936.

The main analysis focuses on infant mortality, i.e., the number of children dying within the first year after birth. We also look at two specific causes of death for infants, inflammatory bowel

[4]Like nurses, midwives were unaffected by the professional restrictions. The composition of home and hospital births was mixed, and the shift towards hospital births already underway. In rural areas, home births still accounted for the majority of births. In urban areas, the rate of hospital births often exceeded home births (e.g., accounting for 67.5% in Berlin and 80% in Düsseldorf).

diseases and pre-term birth/congenital conditions; and the number of stillbirths. As additional outcomes, we also analyze mortality of common childhood diseases. We measure infant mortality as deaths per 1,000 live births, stillbirths per 1,000 births and disease mortality per 1,000 of population. Similarly, we measure physician supply as physicians per 1,000 of population. We use lagged values for the reference population instead of current ones to contain endogenous feedback effects.

[Figure A1 about here]

Information about the number of physicians is taken from the *Reichs-Medizinal-Kalender/Verzeichnis der deutschen Ärzte und Heilanstalten* [Register of German physicians and hospitals], a yearly publication listing physicians in Germany for each municipality. We observe yearly emigration counts of physicians for every municipality. Prior to 1933 we observe only 145 physicians who emigrate. From 1933 to 1936 this number increases to over 2,700 physicians. We use these emigration statistics as our main measure of policy exposure.

In addition to emigration, we construct two alternative measures of policy exposure in 1933. Editions of the physician register after 1936 explicitly tag Jewish physicians (considered Jewish by the extended definition). From this, we construct a proxy measure of Jewish physicians per municipality. Furthermore, we know the exact amount of people of Jewish faith in each municipality from the official population census conducted in 1933, just when the discriminatory measures where implemented. We use both the number of Jewish physicians remaining in 1936 and the local Jewish population in 1933 to proxy for the number of Jewish physicians expelled in 1933. Note that even if these variables only proxy for policy exposure with measurement error, this is inconsequential as long as they correctly represent exposure in the cross-sectional dimension. We discuss this point in more detail in the next section.

To measure hospital infrastructure, we extract information about the prevalence and type of hospitals in each municipality from the 1933 edition of the physician register. Moreover, we also measure the capacity of hospitals by collecting information on the number of beds available at each facility. We further enrich the data with municipality-specific information from the population census, official labor statistics and election results.

[Figure 2 about here]

Our final sample is a balanced panel comprising 2,853 observations in 317 municipalities between 1928–1936. The dataset covers 29,168,080 people in 1933, about 45% of the total German population at the time. By construction, the sample is selected on larger, more populous and urban municipalities. Descriptive statistics are given in Table 1 in the appendix. The average municipality-level share of persons of Jewish faith is 0.6%. For every 1,000 live births, about 75 children die before reaching one year of age. The physician coverage ratio is approximately 1 per 1,000 of population. To illustrate the dynamics in the data, Figure 2 depicts unconditional trends in the municipal physician supply per 1,000 of population and infant mortality over time, partitioned by groups of municipalities with and without any outmigration of physicians. The graph shows a distinct drop in the number of physicians in 1933 in municipalities with a higher share of Jewish residents, and a corresponding rise in infant mortality.

4 Empirical strategy

4.1 Identification and linear model

Identifying the effect of physician coverage on health outcomes is hampered by positive selection. Physicians tend to locate in places where their services are in demand, leading to a positive association between sickness prevalence and health care services. Many European countries with public health insurance also traditionally regulate licensing to allocate medical services where they are deemed to be needed. These features of health care markets make it difficult to identify the degree to which changes in health care coverage influence population health.

We are using exogenous shocks to the supply of physicians to identify the causal effect of physicians on infant mortality. The 1933 law causes a drop in the number of registered physicians (see Figure 2). We measure the extent of the law's impact using the policy exposure variables outlined in the previous section. We discuss the advantages and disadvantages of the different measures in more detail below.

As a first step, we conduct a reduced-form event study analysis. This analysis helps us ascertain the direct effect of the policy change on infant mortality, and also allows us to assess the importance of pre-treatment changes which would invalidate the interpretation of our main estimates. We estimate the following equation:

$$y_{it} = \sum_{\substack{j \neq \tilde{t} \\ j = \underline{t}}}^{\bar{t}} \beta_j z_i + \eta_i + \delta_t + \varepsilon_{it} \ , \tag{1}$$

where $[\underline{t}, \bar{t}] = [1929, 1936]$ are the period limits, $\tilde{t} = 1932$ is the reference period, z_i is a municipality-level scaler measuring exposure to the policy and η_i and δ_t are municipality and year fixed effects.

However, this reduced-form approach does not isolate the effect of health care provision, i.e. the effect of an additional physician on health. For the remainder of the empirical analysis, we are using an IV approach to identify the effect of variations in physician supply. Consider a regression equation of mortality incidence y on physician supply s in municipality $i = 1, \dots, N$ in year $t = 1, \dots, T$,

$$y_{it} = \beta s_{it} + \eta_i + \delta_t + \varepsilon_{it} \ , \tag{2}$$

where η_i and δ_t are municipality and year fixed effects and $T \ll N$. Estimates for β_1 from this equation are typically biased upwards due to positive selection. We are augmenting the structural equation (2) with the first stage regression

$$s_{it} = \alpha z_{it} + \psi_i + \theta_t + \nu_{it} \ , \tag{3}$$

where z_{it} measures exposure during the period of the policy. This approach both accounts for endogeneity in the overall number of physicians and the intensity of exposure to the policy itself.

The triangular system outlined in equations 2 and 3 using physician emigration to measure exposure is our preferred model specification. We estimate the model using two-stage least squares. Further extensions including interactions with the treatment variable are specified using a control function approach, i.e., two-stage residual inclusion, estimated as a single GMM system specification to avoid the generated regressor problem for correct inference. We also rely on control function estimation for the semiparametric model we develop in section 4.3.

Our main results rely on variation in the number of registered physicians induced by the emigration of physicians. We observe the yearly number of physicians emigrating per municipality, i.e. the number of physicians who give up their residency and move to a foreign country. Although emigration is a downstream measure with regard to the policy, this does not invalidate our analysis. Using emigration as an instrument requires that the propensity to migrate as a response to the policy is homogeneous and unrelated to unobservable time-variant factors influencing mortality. If this assumption holds, emigration is a valid proxy for exposure and preferable to other time-invariant measures of the policy impact, as it offers richer variation for identification.

With this in mind, we also employ alternative measures of exposure to the policy. We use the two proxies for the number of Jewish physicians expelled in 1933 as alternative instruments, i.e., the number of Jewish physicians remaining in 1936 and the local Jewish population in 1933. In this case, the instrument z_{it} is equal to the interaction between the respective exposure measure and the time period of the occupational restrictions, $\mathbb{1}\{t \geq 1933\}$. Hence, these measures only proxy for treatment intensity at a single point in time and therefore offer less identifying variation compared to our preferred measure, physician emigration. Note that even though both variables are only imperfect proxies for the number of affected physicians, this is inconsequential as long as they correctly represent exposure in the cross-sectional dimension. Measurement error is not going to affect the estimates if the propensity to remain in 1936 or to become a physician is constant within the Jewish population.

We consider emigration to be the preferred measure since it is time-varying and measures treatment exposure more fine-grained. Although the discriminatory policies cause a decline in the number of licensed physicians, vacant spots can be filled again. Younger physicians who finished their residency may open up practice in municipalities where positions are left vacant. Physician emigration as a time-variant measure can account for this. However, new residents would have opened up practice eventually anyways—if they are more likely to do so in municipalities with vacant spots, using a time-invariant measure of exposure will only bias estimates downwards. To the same effect, the time-invariant measures also miss non-Jewish physicians affected by the occupational restrictions (e.g., those affiliated with social-democratic or socialist/communist organizations). For our main results, we present estimates for all three measures. We find that the results are very similar and differ in the expected direction.

4.2 Threats to identification

We discuss the assumptions implicit to the estimation procedure in turn. Monotonicity (or homogeneity in the linear case), i.e., that emigration of physicians can only influence the total number of physicians in one direction or not at all is likely fulfilled: Outmigration reduces the number of registered physicians. Relevance is an empirical issue. Emigration of physicians naturally reduces the count of registered doctors. Our first stage results show a significant negative effect that is statistically indistinguishable from a one-to-one relation. This result is as expected and suggests the instrument is sufficiently relevant.

The exclusion restriction required for identification necessitates that emigration of physicians only influences mortality by changing the overall supply of physicians. In other words, changes in the instrument (i.e., the propensity of physicians to migrate/the level of Jewish physicians/the level of Jewish population) within a municipality over time only influence population health by affecting the total number of physicians available. This means physician migration cannot influence mortality directly and must be unrelated to other unobserved time-variant factors that influence mortality. While the first mechanism—physician emigration directly affecting health—can credibly be ruled out, we discuss possible confounding factors via the second mechanism in more detail.

One potential confounding factor could be the effects of the laws on other professions. However, other health care professions were not affected by the laws and did not feature a similar overrepresentation of Jewish individuals. Other affected professions (university professors, public officials) were both smaller in size and not related to health. Another concern is that discrimination and a hostile environment may have led to a higher mortality among the Jewish population. We consider this issue in our sensitivity analysis and show that it is virtually impossible that this mechanism is driving our results given the small share of Jews among the general population (0.7%).

More generally, a valid concern is that a violation may be caused by non-random assignment of the instrument, i.e. if the spatial distribution of Jewish physicians is correlated with other features that impact population health in a way that is not absorbed by municipality or time fixed effects. Jewish communities were more prevalent in certain regions within Germany and in metropolitan areas. Should health and population dynamics differ between more and less populous or urbanized regions, the exclusion restriction may be violated. This concern is alleviated by the fact that our sample is already preselected on more populous, urban municipalities. Nevertheless, we address the issue of regional confounding factors in detail in the robustness checks.

Importantly, we find that municipalities with larger shares of Jewish inhabitants do not differ from other areas in our sample.[5] In Table A1 in the supplementary material, we contrast the means for mortality outcomes and political and socio-demographic characteristics for municipalities with Jewish population shares below and above the median prior to the introduction of the occupational restrictions. We find no difference between municipalities in any of the major outcomes we look at with the exception of a small difference in the pneumonia mortality rate. Moreover, municipalities are very similar with regard to political preference. There is no difference in the share of votes for the Nazi Party, and only a minor difference in the middle of the political spectrum.[6] Areas with a larger Jewish population share are slightly more populous, but do not exhibit any differences in population growth. There are also no differences in key labor market characteristics, with both groups having similar rates of labor force participation, unemployment and social assistance receipt. This makes us confident that our identifying assumptions are credible.

4.3 Semiparametric model

Diminishing returns matter for vital outcomes like mortality. A simple linear specification as given in (2) may not fully capture the mortality effects of physicians. The effect of physicians is most likely nonlinear, as an additional doctor in a region with sparse coverage can prevent more infant deaths compared to an additional doctor in a saturated region. After a certain coverage has been reached, mortality reductions are likely to taper off. Most deaths resulting from treatable

[5]Strictly, balance in observable characteristics is not a test for time-variant confounding unobservable factors. However, the absence of such confounding factors and the exclusion restriction are more credible if municipalities are similar in observable characteristics ex-ante.

[6]Voters in municipalities with larger Jewish populations vote slightly more for the Centre Party, while voters in other municipalities cast more votes for the Social Democrats or abstain from voting.

conditions will have been prevented and the remaining cases will consist of conditions which are increasingly difficult to treat. While morbidity effects may persist, systematic mortality reductions beyond a lower bound of medically difficult cases are unlikely. We investigate the hypothesis of diminishing returns by employing a semiparametric estimation approach using a partially linear model. We combine the Baltagi and Li (2002) semiparametric fixed-effects estimator with a control function (e.g. Heckman and Robb 1985) to derive a flexible estimate of the effect of physicians on infant mortality.

Consider a general panel model of the form

$$y_{it} = f(s_{it}) + \eta_i + \varepsilon_{it} \tag{4}$$

$$s_{it} = g(z_{it}) + \psi_i + \nu_{it} \; . \tag{5}$$

Time fixed effects are dropped for notational convenience. To control for the endogeneity in the physician supply s_{it}, we add the control function $\hat{\nu}_{it}$ for s_{it} obtained from (3) to the model as a linear term,

$$y_{it} = f(s_{it}) + \hat{\nu}_{it}\rho + \eta_i + \zeta_{it} \; . \tag{6}$$

The control function approach assumes that the correlation between the structural error ε_{it} and the first stage error ν_{it} can be described as a linear relationship $\varepsilon_{it} = \nu_{it}\rho + \zeta_{it}$ with $\mathrm{E}[\nu_{it}\zeta_{it}] = 0$. The endogenous variation in s_{it} is corrected using the estimated control function $\hat{\nu}_{it}$.

We estimate this model using the first difference approach described in Baltagi and Li (2002), as the conventional Robinson (1988) double residual estimator cannot accommodate the municipality-specific intercepts. To eliminate the fixed effects, take first differences of (6) with respect to time,

$$\Delta y_{it} = \left\{ f(s_{it}) - f(s_{it-1}) \right\} + \Delta\hat{\nu}_{it}\rho + \Delta\zeta_{it} \; . \tag{7}$$

Baltagi and Li (2002) show that $f(s)$ can be approximated by a power series $p^k(s)$, and

$\{f(s_{it}) - f(s_{it-1})\}$ by $\left\{p^k(s_{it}) - p^k(s_{it-1})\right\}\gamma$. Equation (7) can be rewritten as

$$\Delta y_{it} = \left\{p^k(s_{it}) - p^k(s_{it-1})\right\}\gamma + \Delta\hat{v}_{it}\rho + \Delta\zeta_{it} \ . \tag{8}$$

We use cubic B-splines to approximate $f(s_{it})$ and $f(s_{it-1})$. Having estimated the series terms, γ and ρ can be estimated with least squares from (8). The parameters and the nuisance estimates are used to fit the fixed effects $\hat{\eta}_i$ by residualizing and averaging within panel units. Together, the estimates can be used to get the partialled-out residuals

$$\hat{u}_{it} = y_{it} - \hat{v}_{it}\hat{\rho} - \hat{\eta}_i = f(s_{it}) + \zeta_{it} \ . \tag{9}$$

We then fit $f(\cdot)$ by regressing the residual $\hat{u}_{it}$ on s_{it} using local polynomial regression. The first derivative of the obtained function with respect to s is the desired marginal effect, $\mathrm{d}y/\mathrm{d}s = \mathrm{d}f(s)/\mathrm{d}s$. Valid inference in this multi-stage estimation approach needs to account for the estimation error of the plug-in estimates. We rely on the bootstrap to obtain confidence intervals for the residualized outcome and the marginal effect.

5 Results

5.1 The effects of the occupational restrictions on physician supply and infant mortality

We first estimate the effect of exposure to the occupational restrictions on the supply of physicians. Results in Table 2 are based on equation 3, using the rate of physicians per 1,000 of population as an outcome and substituting the different exposure measures. We find that one additional physician emigrating after 1933 reduces the number of physicians by about 1.1. We cannot reject the null hypothesis that the coefficient is equal to one, indicating a one-to-one relationship as expected. If we measure exposure as the number of Jewish physicians post 1933, we find that one additional Jewish physician leads to a reduction of about 0.4 physicians. Corrected for order of magnitude, the estimate for exposure measured as overall Jewish population is of similar size.

[Table 2 about here]

Next, we conduct a reduced-form event study analysis to ascertain the direct effect of the policy exposure on infant mortality. Figure 3 shows the results from estimations based on the reduced-form specification in equation 1. We find that all exposure measures lead to higher infant mortality post 1932. All three series of estimates show the same pattern and are about the same size and magnitude (accounting for scaling). Given the policy was only implemented in mid-1933, the effect for 1933 is smaller. Comparing panel (a) to the other measures in panels (b) and (c), we find that the estimates using physician emigration for exposure are slightly larger and more precise. Importantly, none of the estimates prior to 1933 are significant, indicating there are no pre-trends that invalidate the remainder of our results. The post-1932 effects are comparable in size to a simple single-coefficient reduced-form estimate. Given the intuitive one-to-one first-stage relationship and more precise reduced-form, emigration is our preferred exposure measure. We provide additional event study estimates for our main outcomes in Figure A3 in the supplementary material.

[Figure 3 about here]

5.2 The effect of physicians on infant mortality and stillbirths

Our main results are presented in Table 3. The reported coefficients can be interpreted as the effect on infant mortality if the physician supply increases by one additional physician per 1,000 people. Since the average physician coverage in the sample is 1.05, the estimates also approximately indicate the effect of doubling the physician supply.

The first panel in Table 3 shows the results when directly regressing health outcomes on physician coverage. For any outcome, the effect is indistinguishable from zero. Estimates are biased due to positive selection, since physicians locate in regions where health care is needed and demand high. The next panel shows our preferred specification, instrumenting physician supply with the emigration figures. Looking at column (1), we find that one additional physician reduces overall infant mortality by about 18 cases per 1,000 live births. The average yearly infant mortality prior to 1933 is about 80. In relative terms, doubling the coverage ratio reduces infant mortality by about 23%. While this effect is large, doubling the number of physicians is also a large-scale intervention. In terms of standard deviations, a one standard deviation increase in the number of physicians implies a reduction in mortality of about 0.45 standard deviations.

[Table 3 about here]

For infant deaths caused by inflammatory bowel diseases (ulcerative colitis, enteritis, diarrhea or other ulcerations of intestines), we also find a significant negative effect. Inflammatory bowel diseases are a frequent cause of death for infants, especially in conditions of sub-standard hygiene and contaminated drinking water. Gastrointestinal diseases were already known as a prominent cause of infant mortality in the 1930s. In contrast, the estimates in column (3) show that increasing the number of physicians has no significant effect on infants dying from pre-term birth or congenital defects. Although all estimates are negative, they are noisy and indistinguishable from zero at conventional levels. This is an intuitive result. Medical complications arising from pre-term birth and congenital debility are difficult to treat and account almost completely for the remaining cases of infant mortality in developed economies today. Note that the number of observations for this set of regressions is lower due to a shorter coverage period in the data. Regarding stillbirths, our results in column (4) show that an additional physician reduces the number of stillbirths by about 8 cases. Stillbirths were mostly associated with obstetric emergencies (intrapartum) and intrapartum deaths are frequently associated with suboptimal care during birth (Lawn et al. 2011).

Comparing the results using emigration as the instrument (second panel) to the alternative instruments (panels three and four), results are very similar. All estimates are significant and results are comparable in magnitude, albeit consistently slightly smaller for the time-invariant instruments. We attribute this to the fact that these measures only factor in the magnitude of the policy change, but cannot account for the year-to-year variation in physicians. In addition, the emigration measure also accounts for variation due to non-Jewish physicians who were affected by the occupational restrictions and left the country for political reasons.

5.3 Disease incidence

We extend our analysis to mortality from diseases that, although not limited to, are a common cause of death for children. We rely on our main specification using physician emigration as the instrument. Bronchitis, diphtheria, influenza and pneumonia are among the more frequent causes of death with between 0.1 to 0.8 deaths per 1,000 individuals. All variables are stated as incidence per 1,000 of population. The results for disease incidence are given in Table 4.

We find that one additional physician can decrease mortality due to bronchitis by about 4.7 cases per 100,000. The effect for influenza mortality is of comparable magnitude with a reduction of 4.5 cases. The effect for pneumonia is sizeable, with a reduction of 32 cases per 100,000 people (*p*-value: 0.12). For measles, we find a reduction in incidence by about 1.3 cases per 100,000. We cannot reject a zero effect for diphtheria and scarlet fever at conventional significance levels.

[Table 4 about here]

In the previous section, we find large effects on infant mortality due to inflammatory bowel diseases, which have mostly bacterial causes. When looking at the overall population, we find negative effects for a mixture of bacterial and viral diseases. While influenza and measles are viral diseases, bronchitis and pneumonia can have both bacterial and viral causes[7] The null effect for diphtheria is not surprising, as an effective antiserum for diphtheria had been approved since 1893 and baseline incidence was already low. Similarly, scarlet fever has a very low fatality rate in the overall population and almost exclusively affects children rather than infants or adults. In both cases, the low baseline incidence results in loss of power. We interpret these results to mean that physicians can broadly influence disease mortality.

Without microdata it is difficult to pinpoint the mechanism by which physicians influence infant and disease mortality. One potential explanation is that quality of care deteriorates due to congestion and people delay seeking treatment. Given universal health insurance, it is unlikely that people at the margin are foregoing medical care completely, but they may delay seeking treatment and receive lower quality care due to congestion. Mothers with children who develop symptoms of sickness may potentially delay visits to the physician in anticipation of long waiting times, and only seek treatment after the infant's health condition has worsened. At the same time, physicians which are pressed on time are less likely to diagnose diseases early, prescribe treatment and give behavioral advice.

[7] The medical literature suggests a large number of pneumonia cases at the time were bacterial pneumonia following influenza infections (e.g. Metersky et al. 2012).

5.4 Hospital infrastructure

To analyze how the provision of physicians interacts with the availability of health care infrastructure, we augment our preferred model for infant mortality from section 5.2. We add an interaction of the physician variable with various infrastructure proxies to the model. Specifically, we consider whether the municipality lacks a hospital of a certain type of specialization, and whether the bed capacity of the public hospitals in a municipality is below the median or in either of the lower terciles. All infrastructure variables are measured in 1933 at the beginning of the employment restrictions. To allow for consistent estimation and inference of the interaction coefficient without additional instruments, we estimate the model using a control function approach. We implement the control function approach as a single GMM system to obtain consistent standard errors. The results are shown in Table 5.

[Table 5 about here]

Considering the results for hospital type, we do not find any additional effect for physicians in municipalities which lack a public hospital. This is not surprising, as public hospitals are common and more than 76% of municipalities have at least one. We then look at specialized institutions. We also do not find any additional effect of physicians if the municipality has a university-affiliated hospital (column 2). However, we do find additional effects for physicians in municipalities which do not have hospitals specializing in infant or maternal care (columns 3 and 4). This implies that losing a physician in a municipality without specialized hospital infrastructure carries a larger mortality penalty compared to a situation where this type of infrastructure is in place. This result suggests that health care infrastructure is an imperfect substitute for physician and that physicians are relatively more important when specialized inpatient care infrastructure is not available. We confirm this finding by looking at an alternative infrastructure measure, inpatient capacity. We distinguish between municipalities at the lower and upper end of the hospital bed capacity distribution, i.e., above and below the median of bed capacity. Again, we find that the effect of physicians is larger in municipalities which have comparatively worse infrastructure. The effects are of comparable magnitude as those for maternity and children hospitals.

5.5 Nonlinear mortality effects

In this part we evaluate whether there are diminishing returns to health care provision. The marginal patient is likely to differ when approaching the zero-mortality lower bound. As the number of physicians increases and mortality decreases, the remaining cases will be characterized by more severe and difficult-to-treat conditions. Results for the semiparametric model developed in 4.3 are presented in Figure 4.

[Figure 4 about here]

Panel (a) plots an estimate of the conditional mean function using the partialled-out residuals from equation 9. This function provides an estimate of the average residual infant mortality by number of physicians after controlling for endogenous selection of physicians, municipality and time fixed effects. Residual infant mortality is positive for smaller values of physician coverage and decreases as the number of physicians decreases. For large regions of the support of physicians, residual infant mortality is indistinguishable from zero. We estimate the dose-response function as the first derivative of the residualized outcome function. Panel (b) shows our main result, the marginal effect of physician supply on infant mortality.

We find that physicians can strongly reduce infant mortality when coverage is sparse, but that the effect declines as supply increases. Mortality effects are restricted to a specific interval of low coverage. After the supply reaches about 1.8 physicians per 1,000 of population, mortality effects subside. The estimated marginal effect is indistinguishable from zero for higher coverage levels, in a region where the data is still dense (panel c). Physicians most likely still influence morbidity and quality of life in the population. However, after the physician-to-population ratio exceeds a critical level, any remaining cases of infant mortality are unaffected by changes in physician supply.

Our results mirror the development of the industrialized world. In most developed countries, infant mortality rates have barely changed since the 1980s, when physician levels reached a comparable magnitude (for a historical comparison, see also section A.4 in the supplementary material). Together with positive selection, our finding also offers a comprehensive explanation for the prevalence of null results in studies which investigate the effect of physicians using more recent data from developed countries.

6 Sensitivity analysis

6.1 Exclusion restriction

We conduct a series of robustness and specification checks to show that our results are valid and not driven by alternative mechanisms or the choice of a specific model. We first focus on violations of the main identifying assumption, the IV exclusion restriction, and devise checks for specific confounding mechanisms.

In our analysis, the exclusion restriction can be violated if districts with a higher rate of Jewish physicians evolve differently over time in a way that is related to mortality. Since we are only considering mortality outcomes, any potential confounding mechanism will manifest in a municipality's population. We replicate our main results while controlling for measures of population and population growth to adjust for population dynamics. The results in Table A2 in the appendix replicate those in Table 3 almost exactly and precision improves for some estimates.

Next, we consider another check to show that our results are not driven by unobservable time-variant trends in health outcomes. If there are other factors that influence municipal-level mortality over time, these will most likely also influence other municipalities nearby. We utilize this fact and extend our analysis by allowing for year-by-state fixed effects and linear time trends on different regional levels. First, in Table A3 we allow the year fixed effects to vary at the state level. This should pick up any unobserved heterogeneity at the regional level without imposing additional functional form assumptions. All coefficients are very similar to our main results, suggesting that potentially confounding regional unobservable trends are absent. Since the number of region-by-year parameters to estimate is too large for sub-state regional units, we then proceed to allow for linear time trends at smaller regional levels instead. In Table A4, we decrease the size of the regional trend unit incrementally and find that we can closely replicate our main results when accounting for state-level, province-level and vote district-level time trends. Although the estimates are slightly more noisy due to the larger number of parameters, the confidence intervals include the original estimates. This corroborates that our results are not driven by unobservable regional trends over time.

We also conduct several placebo analyses (Table A5). First, we check whether future instrument values (post-occupational restrictions) can predict past outcomes (pre-occupational restrictions). We do not find any evidence that future instrument values are related to past outcomes, all coefficients are indistinguishable from zero at conventional significance levels (panel a). Second, we gathered data on emigration of psychiatrists and re-estimate our main specification using this measure as a placebo instrument (panel b). The results indicate that variation in the number of physicians induced by the number of psychiatrists emigrating does not influence any of our main infant mortality outcomes, unlike physician emigration. All estimates are insignificant at conventional levels. Third, we repeat our main specification for outcomes which should not be affected by the treatment. We test whether physicians affect the mortality incidence of strokes, deaths due to old age, and those classified as dying of unknown reasons (panel c). As expected, we do not find any evidence that physicians influence the number of deaths due to these conditions. These results suggest that our findings are unlikely to be driven by differential underlying trends in health and health behavior, as these would most likely also impact these outcomes.

Another concern is that our results are driven by factors related to political sentiment and persecution more generally. Although Jews formally retained their citizenship and continued to have full access to health insurance, the general environment grew increasingly hostile towards Jewish citizens and other population groups after 1933. Beginning in late 1935, the *Nürnberger Gesetze* limited Jewish citizenship and marriage rights. After 1936, when preparations for war began, further policies essentially removed Jews' citizenship rights gradually and targeted discriminatory measures against anybody with Jewish heritage intensified. Following the events of the *Novemberpogrome* in 1938, Nazi politics escalated from discrimination to systematic persecution, displacement and dispossession. For these reasons, we do not include data after 1936 in our analysis. The results are also robust to whether the year 1936 is included in the analysis or not.

In addition, we devise a series of checks to rule out that our results are driven by factors related to prosecution and political violence. First, we gather election data for three major elections during our pre-treatment period (May 1928, September 1930, March 1933) and try to predict post-treatment emigration based on the election results. We then test whether the election coefficients are jointly significant using an F-test. Results are reported in Table A6 in the supplementary material. Neither the joint test statistics nor any of the single coefficient estimates are significant at conventional levels. Second, we add controls for violent deaths (suicides and murders) as a measure of political violence to our main specification. Results reported in Table A7 indicate that our main results are unaffected by this. Third, to show that our results are not driven by other forms of discrimination, we digitized additional data from crime statistics. In Table A8, we add controls for 38 different categories of criminal activity to our main specification (including theft, damage of property, battery, assault, murder, voluntary/involuntary manslaughter, resisting the police and other categories). Again, our results are robust to the inclusion of these controls.

A related explanation for our findings is that the mortality effects we find are restricted to Jewish families. The previous checks rule out the possibility of confounding discriminatory measures. However, effects could still be driven by differential access to health care among Jewish citizens compared to the rest of the population, even though Jewish citizens formally retained health insurance coverage. Regrettably, we are unable to distinguish mortality by population groups. However, a simple back-of-the-envelope calculation reveals that it is impossible that the mortality effects we find are driven by the Jewish community alone because of their disproportionately small population share. To account for the effect, the Jewish population would need to have birth rates much higher than the overall population and not a single Jewish baby could survive infancy. This scenario is impossible. Aggregate data from Prussia show that birth rates among Jews were only about 40% that of the remainder of the population. This suggests that the effect cannot be driven by mortality among the Jewish population.

In order to fully dispel the possibility that our results are driven by confounding factors related to changes in the size of the Jewish population only and not by changes in the number of physicians induced by emigration, we conduct another set of checks. We estimate a specification in which we control for the size of the Jewish population interacted with a dummy for 1933 and after (our alternative instrument), and use physician emigration as the instrument. This means we only rely on exogenous variation in physician emigration orthogonal to the size of the Jewish population for identification. The results are shown in Table A9. This specification is associated with a reduction in power, as we lose some of the identifying variation. Still, we find that our estimates are unchanged in size and retain significance. Moreover, they also remain stable if we sequentially add the political control variables, measures of violent deaths and crime on top of each other to the regression. This demonstrates that our effect is driven by changes in health care supply among the general population and not by other factors.

Although other health care professions were not affected by the occupational restrictions, we collected additional data to verify that our results are robust to the inclusion of controls for other health care personnel. In Table A10, we sequentially add controls for the number of midwives and the number of pharmacists to our main specification. We focus on the specification using physician emigration as the instrument, but results for the other measures are consistent. Our main estimates for the effect of a physician all remain unchanged.

Finally, one remaining concern is substitution, i.e., that health care demand is satisfied by visiting out-of-municipality physicians. If this behavior were to occur, it would likely bias our results towards zero. Moreover, the municipalities we consider are sufficiently large such that outside substitution is likely to be negligible and we only consider mortality of individuals registered within the municipality in question. To show that spillovers related to health care supply in nearby municipalities are negligible, we conduct a robustness check where we control for the average physician supply in municipalities within a given distance (see Table A11 in the appendix). We find that our results are stable and unchanged when accounting for this. In addition, we also conduct another check by correcting standard errors for spatial correlation using Conley's (1999) approach (see Table A12). The results indicate that our results are consistently significant when allowing for spatial correlation of residuals within a wide range of distance thresholds.

6.2 Model choice and functional form

Throughout the analysis, we express all dependent variables as rates. To rule out contemporaneous feedback effects, we lag all birth and population counts used in the denominator of the dependent variable by one period. We prefer rates over logged counts for interpretability and to avoid the log-of-zero problem, since some of the rarer disease outcomes have a higher rate of zeros. To demonstrate that the choice of transformation (logged counts vs. rates) is inconsequential for our results, we log-transform the dependent variable and reestimate a standard linear model. Except for total infant mortality, this results in loss of observations for outcomes due to zeros in the data. Still, if we re-estimate our main specification using logged outcomes while controlling for log population and log births, we find that all results carry over and the effects are of similar magnitude (Table A13).[8] The log specification also addresses concerns about possible denominator effects. Since our results carry over in the log specification based on total counts and controlling for both population and birth cohort size, we can rule out that our findings are driven by denominator effects.

In addition, the unadjusted mortality distributions feature overdispersion and a long right tail typical for count data. Normalizing by reference population reduces skewness and alleviates these issues, improving the viability of standard linear models. Figure A4 compares the original and the scaled distributions for selected variables and illustrates that anchoring mortality incidence to a reference population leads to a distribution that is approximately normal. For some variables, especially those with a comparatively large share of zero observations, the scaled distribution remains right-skewed. To address this issue and to demonstrate that the choice of model (linear vs. log-linear) is inconsequential, we repeat our main analysis using an exponential conditional mean model outlined in Appendix section A.3. The exponential model provides a straightforward solution to IV estimation in a nonlinear setting using GMM. Results are given in Table A14. All findings from our main analysis carry over.

Finally, we also devise a robustness check to assess our reliance on the linearity of the control function in the semiparametric analysis. We repeat the analysis while including higher-order polynomials of the control function to see whether the results change. Theoretically, any non-linear transformation of the control function can be used as a control function as well. Comparing the different estimates in Figure A5, the marginal effects are stable, making us confident that our findings reflect a causal relationship in the population and are not driven by the linear functional form of the first stage.

[8] Alternatively, using the inverse hyperbolic sine transformation (e.g. Burbidge et al. 1988) also gives comparable results for all outcomes.

7 Discussion

We analyze the effects of changes in physician supply on infant mortality and disease incidence. Our results highlight the important role of physicians during the mortality transition. Reductions in infant mortality occur mainly through lower mortality due to inflammatory bowel conditions and other common childhood diseases. We find that mortality effects are larger in regions without specialized hospital infrastructure or with limited hospital capacity.

Our estimate indicates that one additional physician per 1,000 people—equivalent to doubling baseline coverage—can reduce infant mortality by about 23% relative to the pre-1933 level. The magnitude is comparable to estimates from Scandinavian public health interventions around the same time.[9] Bhalotra et al. (2017) evaluate a campaign in Sweden that provided information about nutrition and sanitation to mothers and monitored infant care through home visits and clinics. They estimate that the program reduced the baseline risk of infant death by about 24%. Wüst (2012) evaluates a home visit program with a focus on promoting breast feeding in Denmark, and the estimates imply reductions in infant mortality between 22% to 35% for the treated group. Similar to our findings, their results indicate that reductions in gastroenteritis from promoting breastfeeding and proper nutrition are the main mechanism driving the result. Bütikofer et al. (2019) study the rollout of health care centers for mothers and children in Norway and find an 18% decrease in infant mortality compared to the pre-rollout level, and a 50% decrease in infant mortality from diarrhea.[10] Given the intent-to-treat estimates and the heterogeneity and unknown scale of the different programs, comparing cost-effectiveness of the interventions is difficult. Doubling physician supply is undoubtedly a large-scale intervention, equivalent to the rollout of large social programs.

Our semiparametric analysis provides evidence for diminishing marginal returns to health

[9] The effect for stillbirths also lines up with studies in the medical literature which find large effects of care in developing countries (e.g. Lawn et al. 2011, McDiehl et al. 2021).

[10] Effects for larger infrastructure programs or the introduction of health insurance are also in the same range. Alsan and Goldin (2019) evaluate the sewerage and safe water treatments in the Boston area from 1880–1920 and find a 22.8 log points decline in the infant mortality rate. Cutler and Miller (2005) find similar effects for US cities during the time period 1900–1936. Looking at the introduction of health insurance in Prussia half a century earlier, Bauernschuster et al. (2020) find an intent-to-treat effect of insurance corresponding to a 1.6% reduction in infant mortality relative to the pre-1884 baseline. The relative effect is smaller since only 15% of the population were directly affected and insurance coverage did not always extend to dependent family members, i.e., wives and children (about 40% of the population were affected either directly or indirectly). Infant mortality in 1884 was also considerably higher—about three times the level in 1930.

care provision. Mortality reductions are large in regions where coverage is sparse, but decrease quickly when coverage increases. These findings are consistent with historical trends over the 20th century. Most developed countries reached physician supply ratios for which we do not find any effect anymore during the late 1980s, and infant mortality rates have been stable since, despite further growth in health care supply. Much of the remaining infant mortality today is accounted for by congenital conditions and complications from pre-term birth, medically difficult conditions for which we find no effect at any margin of supply. The nonlinear relationship also provides an explanation why more recent studies analyzing developed countries often fail to detect a significant relation between physician supply and mortality. Variations in physician supply in developed countries post-1990 occur in regions of the support where additional physicians have little effect on mortality.

Although our analysis focuses on mortality, health effects are unlikely to be restricted to fatalities alone. Higher disease mortality is caused by a general increase in disease prevalence among the population. Increased morbidity at a young age can reduce well-being later in life and lower life expectancy. We are unable to capture morbidity effects with our analysis due to a lack of data. Quantifying effects on disease prevalence and morbidity remains a task for future research.

We are hesitant to generalize our findings to the present day context and draw quantitative conclusions from the historical analysis for present-day health policy in developing countries. Medical technology and risk factors have changed drastically over the last century. Increasing the supply of physicians in countries with similar mortality rates today will not have the same effect it did in 1930. Historical data can provide only a minimal benchmark—since technological progress increases the treatment efficiency of physicians, our estimates may be thought of as a lower bound. In appendix A.4 in the supplementary material, we provide a detailed discussion of historical developments and highlight differences and parallels between 20th century Germany and developing countries today.

Still, our analysis underscores the importance of access to basic health care. Establishing and maintaining a level of baseline health care coverage has historically been vital to prevent infant mortality and an important complement to other public health policies. Qualitatively, the non-linear shape of the health care production function offers some perspective for policy design. Many countries are experiencing regional shortages of physicians and are considering supply-side regulation or incentive schemes to ensure sufficient regional provision. Our analysis underscores that ensuring basic health care supply is important, as the costs of under- and overprovision are not symmetric. In many of the least developed countries, where child mortality and health issues are already grave problems, current physician supply ratios are maintained by humanitarian aid and foreign health professionals. Hospital infrastructure is often lacking or insufficient, exacerbating negative health effects. As a development policy, training more physicians may not be the most cost-effective option to improve public health, when improving sanitation and health-related behavior are cheaper (and possibly more effective) alternatives. Nevertheless, our results emphasize the need to ensure sufficient access to basic health care when service provision is low or non-existent.

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Appendix

Tables

Table 1: Descriptive statistics

	MEDIAN	MEAN	SD	MIN	MAX	N	DESCRIPTION
Infant mortality	72.308	75.080	24.076	8.772	229.358	2853	Total yearly municipal mortality rate of children dying before one year of age (per 1,000 live births).
Inflammatory bowel diseases	3.083	4.529	5.502	0.000	53.640	2853	Mortality rate of children dying from Colitis, Enteritis, diarrhea or other ulceration of intestines before one year of age (per 1,000 live births).
Pre-term birth/ cong. debility	35.461	37.454	15.233	0.000	99.768	1902	Mortality rate of children dying from congenital debility, malformations or as a consequence of pre-term birth before one year of age (per 1,000 live births).
Stillbirths	28.215	29.709	10.917	0.000	80.000	1902	Rate of stillborn children (per 1,000 births).
Measles	0.000	0.018	0.041	0.000	0.638	2853	Mortality rate due to Measles (per 1,000 of pop.).
Scarlet fever	0.000	0.014	0.028	0.000	0.427	2853	Mortality rate due to Scarlet fever (per 1,000 of pop.).
Diphtheria	0.045	0.076	0.108	0.000	1.285	2853	Mortality rate due to Diphtheria (per 1,000 of pop.).
Influenza	0.069	0.102	0.114	0.000	1.144	2853	Mortality rate due to Influenza (per 1,000 of pop.).
Bronchitis	0.127	0.153	0.124	0.000	0.966	1902	Mortality rate due to Bronchitis (per 1,000 of pop.).
Pneumonia	0.708	0.746	0.298	0.000	3.119	2853	Mortality rate due to Pneumonia (per 1,000 of pop.).
Population	32.004	91.598	270.226	15.192	4339.641	2853	Total municipal population (in 1,000s).
Physicians	0.944	1.048	0.613	0.139	6.034	2853	Registered physicians (per 1,000 of pop.).
Physician emigration	0	0.955	15.258	0	622	2853	Emigration of physicians
Jewish physicians	1	12.385	104.575	0	1817	2853	Jewish physicians
Jewish pop. (1933)	0.156	1.292	9.303	0.001	160.564	2853	Jewish population in 1933 (in 1,000s).
Jewish pop. (%, 1933)	0.451	0.629	0.643	0.004	4.713	2853	Share of Jewish population based on 1933 Jewish and total population in percent.

Notes: All statistics are based on the largest estimation sample covering the years 1928–1936. Sources: *Reichsgesundheitsblatt* [Health bulletin] (eds. 1928–1936), Reichsgesundheitsamt [Federal health ministry]. *Volks-, Berufs- und Betriebszählung* [Census] 1933, Statistisches Reichsamt [Federal statistical office]. *Reichsmedizinalkalender – Verzeichnis der deutschen Ärzte und Heilanstalten* [Register of German physicians and hospitals] (eds. 1928-1936), Thieme Verlag.

Table 2: The effect of emigration on the supply of physicians

Dependent variable: Registered physicians per 1,000 of population			
# of physicians emigrating	−1.149***		
	(0.211)		
# of Jewish physicians		−0.380***	
		(0.096)	
Jewish population in 1933			−0.004***
			(0.001)
Year FE	✓	✓	✓
Municipality FE	✓	✓	✓
First stage F-stat	29.69	15.60	14.88
N municipalities	317	317	317
N	2853	2853	2853

Notes: First stage estimates for different exposure variables using a linear regression model. The dependent variable in panel (a) is the rate of registered physicians per 1,000 of population. Included in all specifications are a full set of year and municipality dummies. Standard errors clustered at the municipality level given in parentheses. *, **, *** denote significance at the 0.1, 0.05, 0.01 level respectively.

Table 3: Mortality estimates

	(1)	(2)	(3)	(4)
	INFANT MORTALITY			
	TOTAL	INFL. BOWEL DISEASES	PREM. BIRTH, CONG. DEBILITY	STILLBIRTHS
	OLS			
# of registered physicians	2.71	–0.10	–0.43	–2.85
	(3.61)	(0.93)	(3.35)	(2.12)
	IV: PHYSICIAN EMIGRATION			
# of registered physicians	–18.79***	–7.11***	–2.48	–8.42***
	(4.17)	(0.98)	(5.09)	(2.24)
First stage F-stat.	29.70	29.70	36.18	36.18
	IV: JEWISH PHYSICIANS × YEAR ≥ 1933			
# of registered physicians	–13.62**	–4.18***	–0.92	–6.11***
	(6.41)	(1.20)	(4.02)	(1.64)
First stage F-stat.	15.60	15.60	17.79	17.79
	IV: JEWISH POPULATION × YEAR ≥ 1933			
# of registered physicians	–13.10**	–4.41***	–0.29	–6.88***
	(6.39)	(1.14)	(4.31)	(1.78)
First stage F-stat.	14.89	14.89	17.05	17.05
Year fixed effects	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
N municipalities	317	317	317	317
N	2853	2853	1902	1902

Notes: Infant mortality variables are measured per 1,000 live births, stillbirths per 1,000 births. Registered physicians are measured per 1,000 of population. Included instruments are a full set of year and municipality dummies. Excluded instruments are given in the respective paragraph header if applicable. Standard errors clustered at the municipality level given in parentheses. *, **, *** denote significance at the 0.1, 0.05, 0.01 level respectively.

Table 4: Disease incidence

	Incidence rate by cause of death per 1,000 of population IV: Physician emigration					
	Bronchitis	Influenza	Pneumonia	Measles	Diphtheria	Scarlet fever
# of registered Physicians	−0.047*	−0.045**	−0.325	−0.013***	0.008	−0.001
	(0.028)	(0.018)	(0.213)	(0.005)	(0.018)	(0.005)
Year FE	✓	✓	✓	✓	✓	✓
Municipality FE	✓	✓	✓	✓	✓	✓
First stage F-stat.	36.18	29.70	29.70	29.70	29.70	29.70
Unconditional mean	0.15	0.10	0.75	0.02	0.08	0.01
N municipalities	317	317	317	317	317	317
N	1902	2853	2853	2853	2853	2853

Notes: All dependent variables given as incidence rates per 1,000 of population. Registered physicians are measured per 1,000 of population. Included instruments are a full set of year and municipality dummies. Excluded instrument is yearly emigration of physicians. Standard errors clustered at the municipality level given in parentheses. *, **, *** denote significance at the 0.1, 0.05, 0.01 level respectively.

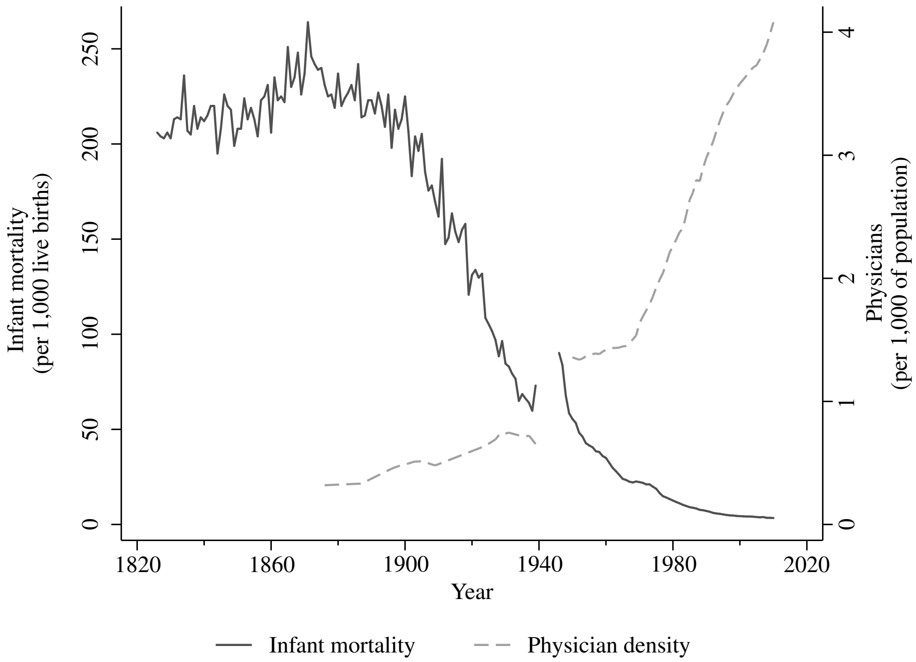
Table 5: Infant mortality and hospital infrastructure

	(1)	(2)	(3)	(4)	(5)
	INFANT MORTALITY IV: PHYSICIAN EMIGRATION				
# of physicians	−19.24***	−19.18***	−16.83***	−16.07***	−18.70***
	(5.04)	(5.09)	(5.65)	(5.80)	(5.13)
Hospital type					
# of physicians x no public hospital	−0.09				
	(7.66)				
# of physicians x no university hospital		−1.57			
		(5.61)			
# of physicians x no infant hospital			−13.65*		
			(7.03)		
# of physicians x no maternity hospital				−17.88**	
				(7.30)	
Hospital capacity					
# of physicians x $\mathbb{1}\left\{\text{beds} \leq p(50)\right\}$					−10.66*
					(6.29)
Year fixed effects	✓	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓	✓
N municipalities	317	317	317	317	317
N	2853	2853	2853	2853	2853

Notes: Infant mortality variables are measured per 1,000 live births, stillbirths per 1,000 births. Registered physicians are measured per 1,000 of population. Included instruments are a full set of year and municipality dummies. Excluded instrument is yearly emigration of physicians. Results are based on a control function specification estimated using a GMM system of equations. Standard errors clustered at the municipality level given in parentheses. *, **, *** denote significance at the 0.1, 0.05, 0.01 level respectively.

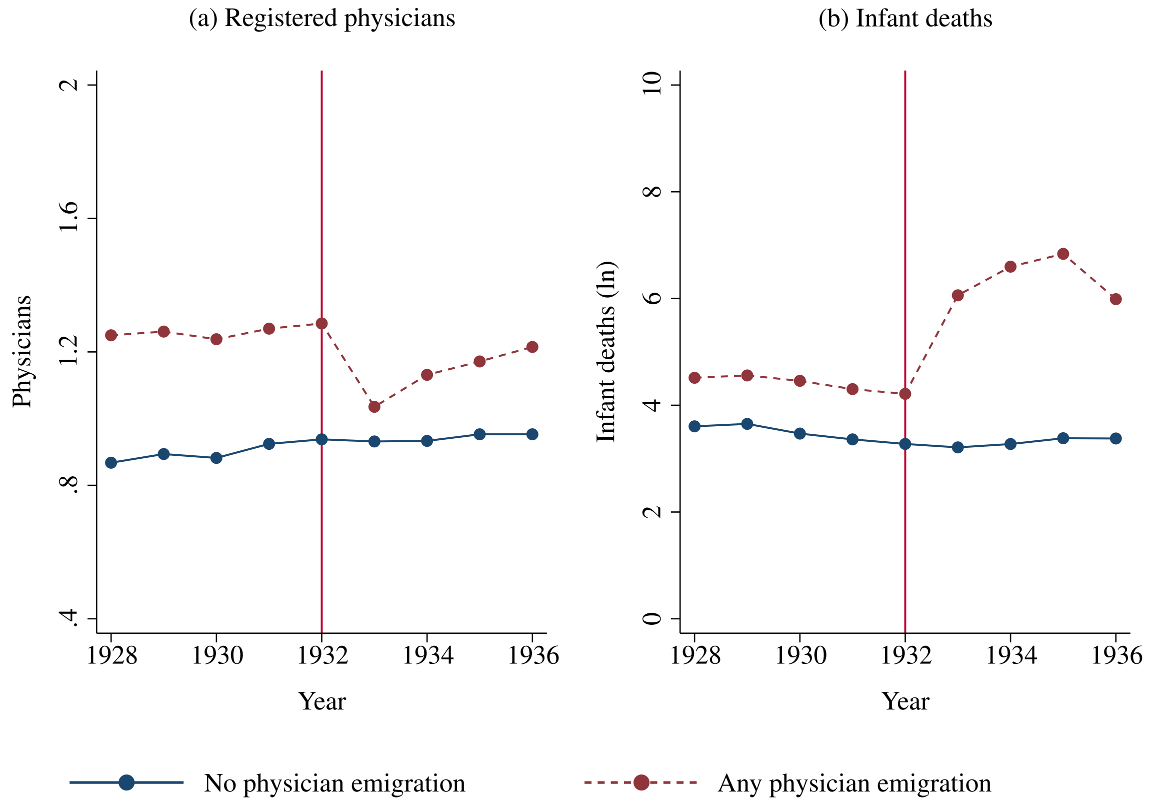
Figures

Figure 1: Infant mortality in Germany, 1826–2010



Notes: The graphs shows the historical development of infant mortality and physician density in German territories between 1826–2014. Infant mortality is measured as the number of children dying within the first year of life per 1,000 live births. Sources: Data is collected from Gehrmann (2012), Statistisches Reichsamt (1884–1940) and Statistisches Bundesamt (1944-2015) [Federal statistical office].

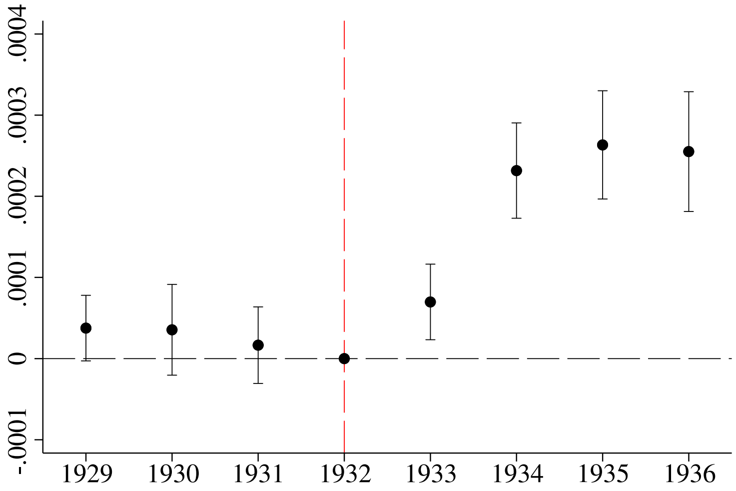
Figure 2: Trends in physician coverage and infant mortality



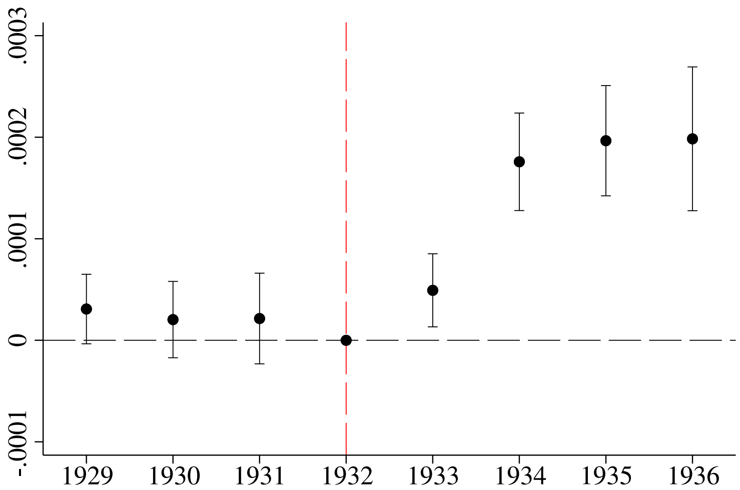
Note: The graphs shows the development of the number of registered physicians per 1,000 of population and log infant deaths over time. Plotted are yearly mean values. Municipalities are partitioned into groups according to whether there is any outmigration of physicians. To account for size, yearly means for the group with positive migration are weighted by migrant frequency.

Figure 3: Event Study Analysis

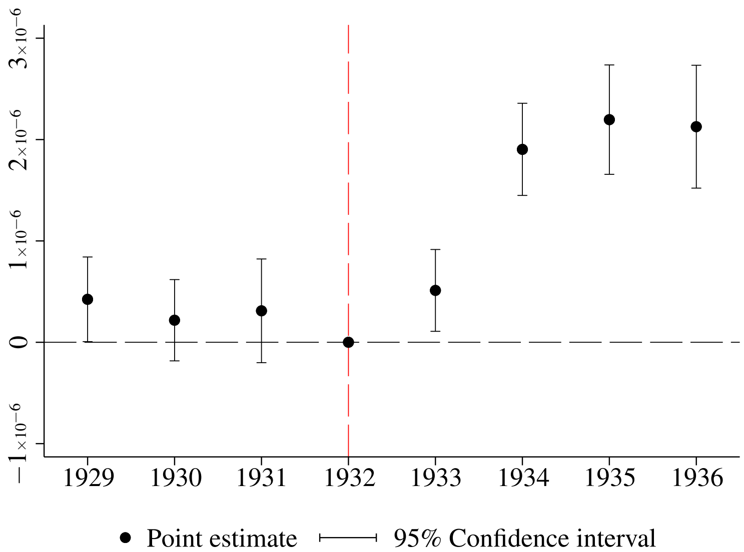
(a) Infant mortality (ln), exposure: year x emigration



(b) Infant mortality (ln), exposure: year x jewish physicians

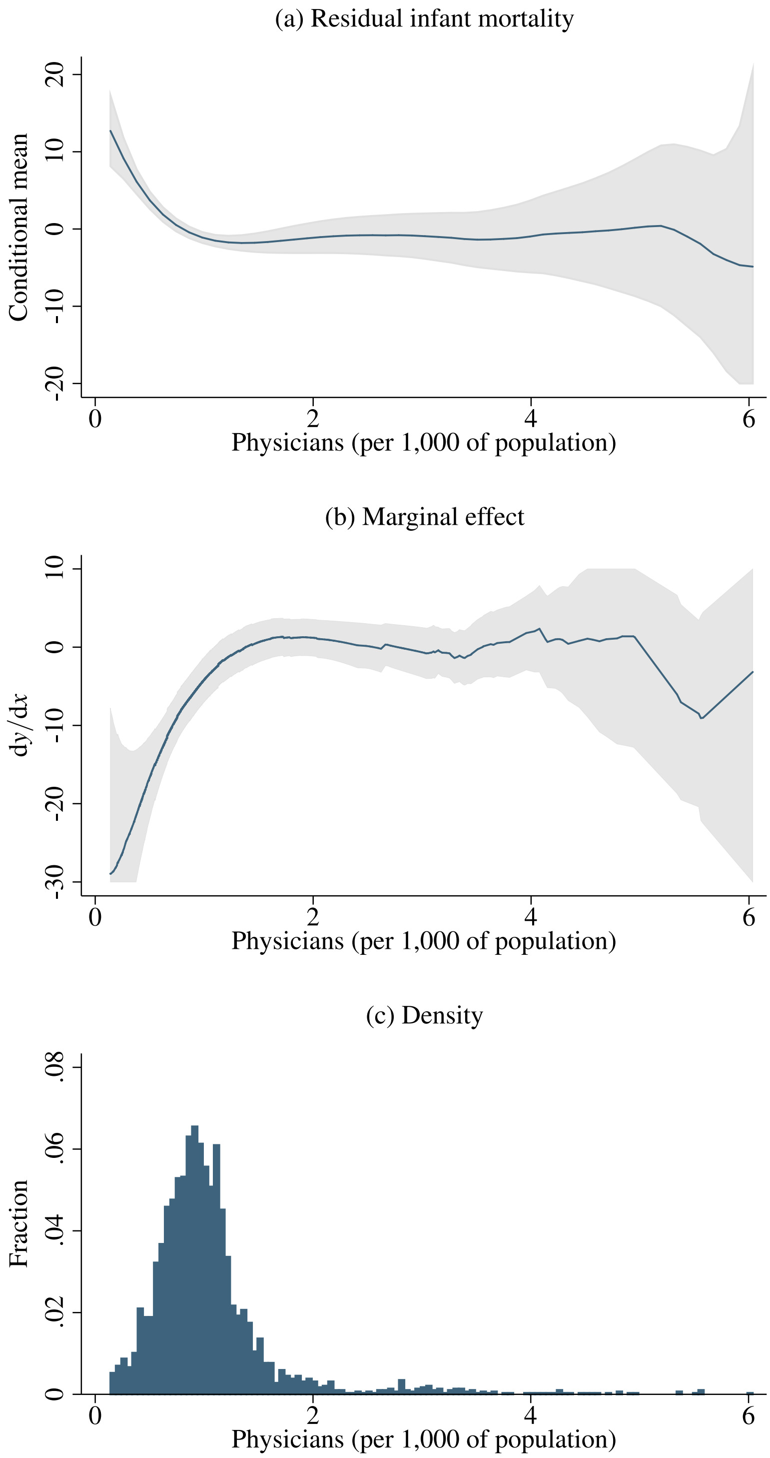


(c) Infant mortality (ln), exposure: year x 1933 jewish population



Notes: Estimates are based on a specification including year dummies interacted with the respective exposure measure. The excluded reference period is highlighted. The graph plots the coefficient point estimates and the corresponding 95% confidence intervals over time.

Figure 4: Semiparametric analysis



Notes: Figure (a) plots the partialled-out residuals of the infant mortality rate by physician density; accounting for the control function of the physician coverage ratio, individual and time fixed effects following the framework outlined by Baltagi and Li (2002). The nonparametric fit was generated using local polynomial regression with a polynomial of degree 4, an Epanechnikov kernel function and a bandwidth of 1.3 chosen by Silverman's rule-of-thumb. Figure (b) plots the marginal effects by physician density, i.e. the first derivative of the above function for each level of the independent variable. Shaded in grey in both plots are the 95% confidence intervals. Figure (c) plots the distribution of the physician density to better illustrate sparse regions in the support.

Online Appendix

A.1 Tables

Table A1: Pre-treatment covariate balance statistics

	Jewish pshare$_{1933} < Q_2$	Jewish pshare$_{1933} \geq Q_2$	Difference
	(1)	(2)	(2) - (1)
(A) MORTALITY OUTCOMES			
Infant mortality	70.822	71.345	0.523
	(22.284)	(21.645)	(2.467)
Infl. bowel diseases	3.945	4.050	0.105
	(4.861)	(4.191)	(0.509)
Premat. birth/congen. debility	32.948	32.899	–0.049
	(15.903)	(13.154)	(1.636)
Stillbirths	30.846	30.027	–0.819
	(12.756)	(10.505)	(1.310)
Measles	0.010	0.016	0.006
	(0.024)	(0.040)	(0.004)
Scarlet fever	0.007	0.009	0.001
	(0.028)	(0.018)	(0.003)
Diphtheria	0.064	0.061	–0.003
	(0.108)	(0.075)	(0.010)
Influenza	0.107	0.117	0.010
	(0.105)	(0.101)	(0.012)
Bronchitis	0.172	0.172	–0.001
	(0.148)	(0.100)	(0.014)
Pneumonia	0.653	0.742	0.088***
	(0.240)	(0.243)	(0.027)
(B) MUNICIPALITY CHARACTERISTICS: VOTE SHARES			
Vote share: NSDAP	0.311	0.319	0.008
	(0.083)	(0.090)	(0.013)
Vote share: SPD	0.248	0.206	–0.042***
	(0.092)	(0.082)	(0.013)
Vote share: KPD	0.171	0.165	–0.006
	(0.074)	(0.069)	(0.010)
Vote share: Zentrum	0.095	0.153	0.058***
	(0.130)	(0.150)	(0.020)
Vote share: DNVP	0.097	0.088	–0.008
	(0.057)	(0.041)	(0.007)
Vote share: DVP	0.027	0.025	–0.002
	(0.017)	(0.017)	(0.002)
(C) MUNICIPALITY CHARACTERISTICS: POPULATION AND EMPLOYMENT			
Population (ln)	10.378	10.961	0.583***
	(0.637)	(1.113)	(0.102)
Population growth	0.616	0.691	0.076
	(1.477)	(1.656)	(0.176)
Labor force participation	0.764	0.771	0.006
	(0.073)	(0.058)	(0.007)
Unemployment rate	0.148	0.139	–0.009
	(0.046)	(0.043)	(0.006)
Social assistance	0.040	0.042	0.001
	(0.020)	(0.020)	(0.003)

Notes: All statistics are based on a 1931 cross-section of municipalities prior to treatment. Vote shares are based on the general election November 1932. Sources: *Reichsgesundheitsblatt* [Health bulletin] (eds. 1928–1936), Reichsgesundheitsamt [Federal health ministry]. *Volks-, Berufs- und Betriebszählung* [Census], Statistisches Reichsamt [Federal statistical office].

Table A2: Robustness: Population dynamics

	(1)	(2)	(3)	(4)
	INFANT MORTALITY			
	TOTAL	INFL. BOWEL DISEASES	PREM. BIRTH, CONG. DEBILITY	STILLBIRTHS
	IV: PHYSICIAN EMIGRATION			
# of registered physicians	−18.34***	−6.75***	−4.26	−8.85***
	(3.54)	(0.99)	(5.02)	(2.22)
First stage F-stat.	32.62	32.62	39.70	39.70
	IV: JEWISH PHYSICIANS × YEAR ≥ 1933			
# of registered physicians	−12.58**	−3.84***	−2.42	−6.20***
	(5.63)	(1.11)	(4.04)	(1.65)
First stage F-stat.	16.77	16.77	18.97	18.97
	IV: JEWISH POPULATION × YEAR ≥ 1933			
# of registered physicians	−12.40**	−4.10***	−1.91	−6.96***
	(5.51)	(1.08)	(4.25)	(1.78)
First stage F-stat.	16.08	16.08	18.02	18.02
Add. controls for pop. size and growth	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
N municipalities	317	317	317	317
N	2853	2853	1902	1902

Notes: Infant mortality variables are measured per 1,000 live births, stillbirths per 1,000 births. Registered physicians are measured per 1,000 of population. Included instruments are a full set of year and municipality dummies, log population and the population growth rate. Excluded instruments are given in the respective paragraph header if applicable. Standard errors clustered at the municipality level given in parentheses. *, **, *** denote significance at the 0.1, 0.05, 0.01 level respectively.

Table A3: Robustness: Including state x year fixed effects

	INFANT MORTALITY			
	TOTAL	INFL. BOWEL DISEASES	PREM. BIRTH, CONG. DEBILITY	STILLBIRTHS
	IV: PHYSICIAN EMIGRATION			
# of registered physicians	−20.63***	−7.49***	−2.94	−7.95***
	(4.28)	(1.02)	(5.78)	(2.24)
	IV: JEWISH PHYSICIANS × YEAR ≥ 1933			
# of registered physicians	−16.01**	−4.53***	−1.88	−5.26***
	(6.31)	(1.22)	(4.05)	(1.82)
	IV: JEWISH POPULATION × YEAR ≥ 1933			
# of registered physicians	−15.29**	−4.77***	−1.32	−6.12***
	(6.22)	(1.18)	(4.24)	(1.86)
State x year fixed effects	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
N municipalities	317	317	317	317
N	2853	2853	1902	1902

Notes: Infant mortality variables are measured per 1,000 live births, stillbirths per 1,000 births. Registered physicians are measured per 1,000 of population. Included instruments are a full set of year effects and linear time trends on the state-/province-/district-level. Excluded instruments are given in the respective paragraph header. Standard errors clustered at the municipality level given in parentheses. *, **, *** denote significance at the 0.1, 0.05, 0.01 level respectively.

Table A4: Robustness: Including linear regional trends

(A) STATE-LEVEL LINEAR TRENDS

	Infant mortality			
	Total	Infl. bowel diseases	Prem. birth, cong. debility	Stillbirths
	IV: Physician emigration			
# of registered physicians	−20.36***	−7.67***	−2.71	−8.31***
	(4.06)	(1.05)	(5.50)	(2.17)
	IV: Jewish physicians × year ≥ 1933			
# of registered physicians	−15.44**	−4.64***	−1.19	−6.01***
	(6.36)	(1.23)	(4.40)	(1.75)
	IV: Jewish population × year ≥ 1933			
# of registered physicians	−14.84**	−4.91***	−0.75	−6.83***
	(6.26)	(1.18)	(4.55)	(1.80)
State-level linear trend	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
N municipalities	317	317	317	317
N	2853	2853	1902	1902

(B) PROVINCE-LEVEL LINEAR TRENDS

	Infant mortality			
	Total	Infl. bowel diseases	Prem. birth, cong. debility	Stillbirths
	IV: Physician emigration			
# of registered physicians	−18.71**	−10.50***	−1.11	−10.47***
	(7.84)	(2.02)	(8.97)	(2.91)
	IV: Jewish physicians × year ≥ 1933			
# of registered physicians	−10.64	−5.11***	2.37	−7.55***
	(9.33)	(1.82)	(7.02)	(2.46)
	IV: Jewish population × year ≥ 1933			
# of registered physicians	−7.69	−4.93***	3.48	−8.49***
	(9.44)	(1.87)	(7.20)	(2.62)
Province-level linear trend	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
N municipalities	317	317	317	317
N	2853	2853	1902	1902

(C) VOTE DISTRICT-LEVEL LINEAR TRENDS

	Infant mortality			
	Total	Infl. bowel diseases	Prem. birth, cong. debility	Stillbirths
	IV: Physician emigration			
# of registered physicians	−18.46**	−9.77***	−1.11	−10.39***
	(7.64)	(2.22)	(8.85)	(2.81)
	IV: Jewish physicians × year ≥ 1933			
# of registered physicians	−12.30*	−4.65**	2.13	−8.49***
	(7.43)	(1.93)	(6.68)	(1.87)
	IV: Jewish population × year ≥ 1933			
# of registered physicians	−8.94	−4.56**	3.53	−9.01***
	(8.02)	(2.06)	(7.07)	(2.21)
District-level linear trend	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
N municipalities	317	317	317	317
N	2853	2853	1902	1902

Notes: Infant mortality variables are measured per 1,000 live births, stillbirths per 1,000 births. Registered physicians are measured per 1,000 of population. Included instruments are a full set of year effects and linear time trends on the state-/province-/district-level. Excluded instruments are given in the respective paragraph header if applicable. Standard errors clustered at the municipality level given in parentheses. *, **, *** denote significance at the 0.1, 0.05, 0.01 level respectively.

Table A5: Robustness: IV placebo checks

(A) PREDICTING PAST OUTCOMES WITH FUTURE IV

	Infant mortality in 1929		
Physician emigration	–0.015		
	(0.011)		
Jewish population		0.000	
		(0.000)	
Jewish physicians			–0.003
			(0.003)

(B) PLACEBO INSTRUMENT: MAIN INFANT MORTALITY OUTCOMES
(IV: PSYCHIATRIST EMIGRATION)

	Infant mortality	Infl. bowel dis.	Pre-term birth
# of registered physicians	6.869	0.604	8.420
	(14.771)	(2.322)	(14.553)

(C) PLACEBO OUTCOME: NON-MITIGABLE DISEASES
(IV: PHYSICIAN EMIGRATION)

	Stroke	Old age	Unknown
# of registered physicians	–0.219	–0.020	–0.063
	(0.203)	(0.134)	(0.118)

Notes: Results in panel (a) are based on a linear regression of infant mortality in 1929, pre-dating the occupational restrictions, on the instruments in 1934, post-dating the restrictions. Panel (b) repeats the main specification from Table 3 for all infant mortality outcomes, using an indicator for emigration of psychiatrists instead of emigration of physicians as the instrument. Panel (c) repeat the main specification for outcomes which are unlikely to be affected by changes in physicians. Infant mortality is measured per 1,000 live births, incidence for other conditions per 1,000 of population. Registered physicians are measured per 1,000 of population. In panels (b) and (c), included instruments are a full set of year and municipality dummies. Excluded instruments are given in the respective paragraph header. Standard errors clustered at the municipality level given in parentheses.

Table A6: Robustness: Do political composition and trends predict physician emigration?

Specification	Level-level			Level-changes		Changes-level			Changes-changes	
Dependent variable Independent variables	Emigration 1934 Votes for party (ln)			Emigration 1934 Growth rate votes for party (ln diff)		Growth rate emigration 1934-1933 (ln diff) Votes for party (ln)			Growth rate emigration 1934-1933 (ln diff) Growth rate votes for party (ln diff)	
	Election May 1928	Election Sep. 1930	Election Mar. 1933	Δ Elections Mar. 1933 –May 1928	Δ Elections Mar. 1933 –Sep. 1930	Election May 1928	Election Sep. 1930	Election Mar. 1933	Δ Elections Mar. 1933 –May 1928	Δ Elections Mar. 1933 –Sep. 1930
Population (ln)	0.01	−0.00	−0.03	0.00	0.01	−1.27	1.29	4.41	2.95	3.39
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(4.08)	(5.93)	(5.35)	(5.88)	(5.66)
Population growth	0.00	−0.00	0.00	0.00	0.00	−0.14	0.04	0.07	0.07	0.06
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.09)	(0.08)	(0.10)	(0.10)	(0.10)
Votes cast	0.01	0.02	0.04	0.01	0.01	−1.32	1.24	−2.43	−2.55	−2.65
	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(4.00)	(7.42)	(6.69)	(6.03)	(5.39)
Zentrum	−0.00	−0.00	0.00	0.01	0.01	0.48	−0.20	−0.15	0.74	−0.97
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.40)	(0.37)	(0.43)	(1.63)	(1.67)
SPD	0.00	0.00	0.00	−0.00	0.00	0.86	−0.41	0.19	0.43	1.89
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.86)	(0.79)	(0.70)	(1.33)	(1.94)
KPD	0.00	0.00	0.00	0.00	0.00	−0.17	−0.75	−0.57	0.52	0.10
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.55)	(0.69)	(0.62)	(0.69)	(1.55)
NSDAP	−0.00	−0.00	−0.00	0.00	0.00	0.25	−0.29	−0.96	−0.18	0.29
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.30)	(0.65)	(1.77)	(0.25)	(0.84)
DNVP	0.00	0.00	0.01	−0.00	−0.00	0.10	−0.38	0.08	−0.14	0.27
	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.46)	(0.37)	(0.73)	(0.35)	(0.39)
DVP	−0.01	0.00	−0.00	0.00	−0.00	0.94	−0.01	0.01	−0.39	−0.02
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.62)	(0.13)	(0.30)	(0.60)	(0.12)
DSTP/DDP	0.00	0.00	0.00	−0.00	−0.00	0.16	−0.12	0.01	−0.07	0.16
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.47)	(0.74)	(0.36)	(0.63)	(0.43)
Other	−0.00	−0.00	0.00	0.00	0.00	0.52	0.15	0.00	−0.02	0.05
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.37)	(0.28)	(0.08)	(0.12)	(0.16)
F-stat (polit. vars = 0)	0.35	0.34	0.33	0.35	0.25	0.99	0.48	0.41	0.50	0.17
p-value	0.95	0.95	0.95	0.93	0.97	0.46	0.86	0.91	0.83	0.99

Note: The table reports results for a regression of emigration on the number of votes cast for different parties in major elections. The elections considered are May 1928, September 1930 and March 1933. Independent and dependent variables are specified in both levels and changes in different columns. The bottom row reports the test statistic and the *p*-value of an F-test for joint significance of all political variables. Standard errors clustered at the municipality level are given in parentheses. *, ** and *** denote $p<0.1$, $p<0.05$ and $p<0.01$ respectively.

Table A7: Robustness: Controlling for violent deaths

	IV: Physician emigration											
	Infant mortality			Infl. bowel diseases			Cong. deb./prem. birth			Stillbirths		
# of reg. physicians	−18.51***	−19.04***	−25.19***	−6.88***	−7.10***	−8.83***	−4.68	−4.72	−4.71	−9.09***	−9.26***	−9.26***
	(4.04)	(4.26)	(3.72)	(0.98)	(0.97)	(1.18)	(5.13)	(5.16)	(5.18)	(2.11)	(2.01)	(2.01)
Murders	0.04	0.04	0.09	0.09**	0.09**	0.06	0.02	0.02	0.02	−0.20	−0.19	−0.19
	(0.14)	(0.14)	(0.16)	(0.04)	(0.04)	(0.04)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)
Suicides	−0.00	−0.00	0.01	0.00	0.00	0.00	0.02	0.02	0.02	0.00	0.00	0.00
	(0.04)	(0.04)	(0.05)	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Accidents		0.06*	0.02		0.03***	0.01		0.01	0.01		0.06**	0.06**
		(0.04)	(0.04)		(0.01)	(0.01)		(0.04)	(0.04)		(0.03)	(0.03)
Unspecified			−0.01			0.00			−0.00			−0.00
			(0.01)			(0.00)			(0.01)			(0.01)
N municipalities	317	317	317	317	317	317	317	317	317	317	317	317
N	2853	2853	1902	2853	2853	1902	1902	1902	1902	1902	1902	1902
	IV: Jewish physicians × year ≥ 1933											
	Infant mortality			Infl. bowel diseases			Cong. deb./prem. birth			Stillbirths		
# of reg. physicians	−13.53**	−14.12**	−19.26***	−4.05***	−4.28***	−4.96***	−2.98	−3.04	−3.01	−6.81***	−7.12***	−7.11***
	(6.19)	(6.42)	(4.85)	(1.15)	(1.19)	(1.11)	(4.14)	(4.17)	(4.18)	(1.65)	(1.69)	(1.70)
Murders	0.04	0.04	0.09	0.09**	0.09**	0.06	0.02	0.02	0.02	−0.20	−0.20	−0.20
	(0.14)	(0.14)	(0.16)	(0.04)	(0.04)	(0.04)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)
Suicides	−0.00	−0.01	0.01	0.00	0.00	0.00	0.02	0.02	0.01	0.00	0.00	0.00
	(0.04)	(0.04)	(0.05)	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Accidents		0.06*	0.02		0.02***	0.01		0.01	0.01		0.06**	0.06**
		(0.04)	(0.04)		(0.01)	(0.01)		(0.04)	(0.04)		(0.03)	(0.03)
Unspecified			−0.01			0.00			−0.00			−0.00
			(0.01)			(0.00)			(0.01)			(0.01)
N municipalities	317	317	317	317	317	317	317	317	317	317	317	317
N	2853	2853	1902	2853	2853	1902	1902	1902	1902	1902	1902	1902
	IV: Jewish population × year ≥ 1933											
	Infant mortality			Infl. bowel diseases			Cong. deb./prem. birth			Stillbirths		
# of reg. physicians	−13.03**	−13.65**	−19.13***	−4.29***	−4.53***	−5.11***	−2.44	−2.50	−2.47	−7.58***	−7.88***	−7.86***
	(6.16)	(6.37)	(4.74)	(1.11)	(1.13)	(1.04)	(4.35)	(4.39)	(4.40)	(1.73)	(1.70)	(1.70)
Murders	0.04	0.04	0.09	0.09**	0.09**	0.06	0.02	0.02	0.02	−0.20	−0.20	−0.20
	(0.14)	(0.14)	(0.16)	(0.04)	(0.04)	(0.04)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)
Suicides	−0.00	−0.01	0.01	0.00	0.00	0.00	0.02	0.02	0.01	0.00	0.00	0.00
	(0.04)	(0.04)	(0.05)	(0.01)	(0.01)	(0.01)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Accidents		0.06*	0.02		0.02***	0.01		0.01	0.01		0.06**	0.06**
		(0.04)	(0.04)		(0.01)	(0.01)		(0.04)	(0.04)		(0.03)	(0.03)
Unspecified			−0.01			0.00			−0.00			−0.00
			(0.01)			(0.00)			(0.01)			(0.01)
N municipalities	317	317	317	317	317	317	317	317	317	317	317	317
N	2853	2853	1902	2853	2853	1902	1902	1902	1902	1902	1902	1902

Note: The table reports results for the main specification while controlling for measures of violent deaths. Each panel is dedicated to one of three instrumental variable measures. Standard errors clustered at the municipality level given in parentheses. *, ** and *** denote p<0.1, p<0.05 and p<0.01 respectively.

Table A8: Robustness: Controlling for crime

	(1)	(2)	(3)	(4)
	INFANT MORTALITY			
	TOTAL	INFL. BOWEL DISEASES	PREM. BIRTH, CONG. DEBILITY	STILLBIRTHS
	IV: PHYSICIAN EMIGRATION			
# of registered physicians	−25.86***	−8.36***	−2.43	−9.74***
	(4.98)	(1.32)	(5.77)	(2.46)
First stage F-stat.	34.04	34.04	34.04	34.04
	IV: JEWISH PHYSICIANS $\times$ YEAR $\geq$ 1933			
# of registered physicians	−20.04***	−4.84***	−0.72	−7.59***
	(5.76)	(1.08)	(4.47)	(2.23)
First stage F-stat.	17.58	17.58	17.58	17.58
	IV: JEWISH POPULATION $\times$ YEAR $\geq$ 1933			
# of registered physicians	−19.76***	−4.90***	−0.01	−8.26***
	(5.72)	(1.04)	(4.73)	(2.25)
First stage F-stat.	16.45	16.45	16.45	16.45
Controls for criminal sentences	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
N municipalities	317	317	317	317
N	1902	1902	1902	1902

Notes: Infant mortality variables are measured per 1,000 live births, stillbirths per 1,000 births. Registered physicians are measured per 1,000 of population. Included instruments are a full set of year and municipality dummies, and a set of controls for criminal convictions. Excluded instruments are given in the respective paragraph header if applicable. Standard errors clustered at the municipality level given in parentheses. *, **, *** denote significance at the 0.1, 0.05, 0.01 level respectively.

Table A9: Robustness: Instrumenting for physician emigration net of Jewish population

(A) CONTROLLING FOR OVERALL JEWISH POPULATION

	Infant mortality	Infl. bowel diseases	Cong. deb./prem. birth	Stillbirths
# of reg. physicians	−15.85*	−8.56***	−5.53	−2.34
	(9.61)	(2.35)	(8.62)	(4.45)
Controls for pop. and Jewish pop. x post 1932	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
N municipalities	317	317	317	317
N	2853	2853	1902	1902

(B) CONTROLLING FOR OVERALL JEWISH POPULATION AND POLITICAL ATTITUDES

	Infant mortality	Infl. bowel diseases	Cong. deb./prem. birth	Stillbirths
# of reg. physicians	−16.33*	−7.52***	−3.48	−5.23
	(9.00)	(2.19)	(8.19)	(4.54)
Controls for political parties	✓	✓	✓	✓
Controls for pop. and Jewish pop. x post 1932	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
N municipalities	317	317	317	317
N	2853	2853	1902	1902

(C) CONTROLLING FOR OVERALL JEWISH POPULATION, POLITICAL ATTITUDES AND VIOLENCE

	Infant mortality	Infl. bowel diseases	Cong. deb./prem. birth	Stillbirths
# of reg. physicians	−16.25*	−7.43***	−3.61	−5.27
	(9.02)	(2.18)	(8.20)	(4.50)
Controls for violent deaths	✓	✓	✓	✓
Controls for political parties	✓	✓	✓	✓
Controls for pop. and Jewish pop. x post 1932	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
N municipalities	317	317	317	317
N	2853	2853	1902	1902

(D) CONTROLLING FOR OVERALL JEWISH POPULATION, POLITICAL ATTITUDES, VIOLENCE AND CRIME

	Infant mortality	Infl. bowel diseases	Cong. deb./prem. birth	Stillbirths
# of reg. physicians	−22.49**	−6.99***	−4.63	−9.53*
	(10.30)	(2.37)	(9.43)	(5.16)
Controls for crime	✓	✓	✓	✓
Controls for violent deaths	✓	✓	✓	✓
Controls for political parties	✓	✓	✓	✓
Controls for pop. and Jewish pop. x post 1932	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
N municipalities	317	317	317	317
N	1902	1902	1902	1902

Note: The table reports results for the main specification using physician emigration as the instrumental variable while controlling for log total population and log Jewish population interacted with a dummy for the years 1933 and after. Standard errors clustered at the municipality level given in parentheses. *, ** and *** denote p<0.1, p<0.05 and p<0.01 respectively.

Table A10: Robustness: Health personnel

	INFANT MORTALITY IV: PHYSICIAN EMIGRATION							
	Infant mortality		Infl. bowel diseases		Cong. deb./prem. birth		Stillbirths	
# of reg. physicians	−19.63***	−19.61***	−6.71***	−6.73***	−4.90	−4.98	−9.51***	−9.56***
	(3.98)	(3.99)	(0.96)	(0.97)	(5.10)	(5.18)	(2.20)	(2.18)
# of reg. midwives	−6.66**	−6.91**	0.30	0.56	−3.70	−4.91	−1.55	−2.25
	(2.88)	(3.07)	(0.81)	(0.85)	(3.20)	(3.67)	(2.14)	(2.55)
# of reg. pharmacists		2.32		−2.38		8.83		5.16
		(8.48)		(2.51)		(11.12)		(8.17)
Year fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
N municipalities	317	317	317	317	317	317	317	317
N	2853	2853	2853	2853	1902	1902	1902	1902

The table reports coefficient estimates from the main specification for all major outcomes with added controls for health personnel. Standard errors clustered at the municipality level reported in parentheses. *, **, *** denote significance at the 0.1, 0.05, 0.01 level respectively.

Table A11: Robustness: Controlling for physician supply in neighboring municipalities

	Dependent variable: Infant mortality								
Distance threshold	5km	10km	20km	50km	100km	150km	200km	300km	500km
# of registered physicians	−18.80***	−18.81***	−18.77***	−18.67***	−18.64***	−19.27***	−19.14***	−19.63***	−19.11***
(IV: Physician emigration)	(4.17)	(4.17)	(4.18)	(3.96)	(4.19)	(4.17)	(4.25)	(4.33)	(4.16)
# of registered physicians	−13.63**	−13.64**	−13.46**	−13.31**	−13.21**	−13.50**	−13.74**	−14.23**	−13.97**
(IV: Jewish physicians)	(6.41)	(6.42)	(6.48)	(6.33)	(6.65)	(6.82)	(6.78)	(6.81)	(6.40)
# of registered physicians	−13.10**	−13.12**	−12.92**	−12.77**	−12.73*	−13.04*	−13.26**	−13.71**	−13.53**
(IV: Jewish population)	(6.39)	(6.40)	(6.44)	(6.30)	(6.56)	(6.72)	(6.70)	(6.74)	(6.37)
	Dependent variable: Infl. bowel diseases								
Distance threshold	5km	10km	20km	50km	100km	150km	200km	300km	500km
# of registered physicians	−7.11***	−7.10***	−7.10***	−7.13***	−7.16***	−7.02***	−7.03***	−7.11***	−7.08***
(IV: Physician emigration)	(0.99)	(0.99)	(1.00)	(0.97)	(0.99)	(0.97)	(1.00)	(1.03)	(0.98)
# of registered physicians	−4.15***	−4.13***	−4.12***	−4.21***	−4.20***	−4.19***	−4.18***	−4.19***	−4.12***
(IV: Jewish physicians)	(1.20)	(1.21)	(1.21)	(1.22)	(1.23)	(1.18)	(1.20)	(1.24)	(1.20)
# of registered physicians	−4.38***	−4.34***	−4.35***	−4.44***	−4.43***	−4.42***	−4.40***	−4.42***	−4.34***
(IV: Jewish population)	(1.14)	(1.15)	(1.16)	(1.16)	(1.16)	(1.13)	(1.15)	(1.19)	(1.14)
	Dependent variable: Congenital debility/premature birth								
Distance threshold	5km	10km	20km	50km	100km	150km	200km	300km	500km
# of registered physicians	−2.38	−2.55	−2.49	−2.48	−2.79	−2.39	−2.51	−2.58	−2.56
(IV: Physician emigration)	(5.13)	(4.93)	(5.12)	(5.10)	(5.37)	(5.07)	(5.06)	(5.07)	(5.02)
# of registered physicians	−0.98	−1.25	−0.88	−0.92	−1.33	−0.98	−0.90	−0.95	−1.02
(IV: Jewish physicians)	(3.96)	(3.79)	(4.06)	(4.02)	(4.07)	(4.00)	(4.10)	(4.09)	(3.98)
# of registered physicians	−0.38	−0.78	−0.23	−0.29	−0.64	−0.33	−0.29	−0.35	−0.46
(IV: Jewish population)	(4.24)	(4.01)	(4.36)	(4.31)	(4.39)	(4.30)	(4.37)	(4.36)	(4.25)
	Dependent variable: Stillbirths								
Distance threshold	5km	10km	20km	50km	100km	150km	200km	300km	500km
# of registered physicians	−8.51***	−8.32***	−8.44***	−8.44***	−8.82***	−8.19***	−8.30***	−8.53***	−8.44***
(IV: Physician emigration)	(2.27)	(2.10)	(2.20)	(2.17)	(2.14)	(2.18)	(2.25)	(2.28)	(2.27)
# of registered physicians	−6.05***	−5.68***	−6.03***	−6.14***	−6.65***	−6.29***	−6.15***	−6.15***	−6.14***
(IV: Jewish physicians)	(1.63)	(1.68)	(1.63)	(1.69)	(1.62)	(1.56)	(1.58)	(1.71)	(1.66)
# of registered physicians	−6.79***	−6.24***	−6.73***	−6.93***	−7.35***	−7.01***	−6.89***	−6.94***	−6.93***
(IV: Jewish population)	(1.74)	(1.66)	(1.71)	(1.82)	(1.73)	(1.75)	(1.76)	(1.81)	(1.80)

Note: The table reports coefficient estimates from linear regression. and standard errors standard errors clustered at the municpality level in parentheses. Regressions are based on the main specification including an additional control variable for the average physician level in neighboring municipalities within the distance threshold listed for each column. Each cell reports the coefficient of interest and standard error from a single regression. Each panel corresponds to one major outcome. Rows signify different measures of the instrumental variable. Columns correspond to different choices for the distance cutoff in kilometers beyond which the correlation between the error term of two observations is assumed to be zero. *, **, *** denote significance at the 0.1, 0.05, 0.01 level respectively.

Table A12: Robustness: Accounting for spatial correlation

	Dependent variable: Infant mortality								
Spatial corr. cut-off	5km	10km	20km	50km	100km	150km	200km	300km	500km
# of registered physicians	−18.80***	−18.80***	−18.80***	−18.80***	−18.80***	−18.80***	−18.80***	−18.80***	−18.80***
(IV: Physician emigration)	(5.01)	(4.98)	(4.99)	(5.18)	(5.04)	(5.06)	(4.39)	(5.46)	(4.77)
# of registered physicians	−13.63***	−13.63***	−13.63***	−13.63***	−13.63***	−13.63***	−13.63***	−13.63***	−13.63***
(IV: Jewish physicians)	(4.07)	(4.07)	(4.09)	(4.16)	(4.08)	(4.09)	(3.92)	(4.11)	(3.66)
# of registered physicians	−13.10***	−13.10***	−13.10***	−13.10***	−13.10***	−13.10***	−13.10***	−13.10***	−13.10***
(IV: Jewish population)	(4.08)	(4.08)	(4.09)	(4.18)	(4.09)	(4.10)	(3.86)	(4.14)	(3.67)
	Dependent variable: Infl. bowel diseases								
Spatial corr. cut-off	5km	10km	20km	50km	100km	150km	200km	300km	500km
# of registered physicians	−7.11***	−7.11***	−7.11***	−7.11***	−7.11***	−7.11***	−7.11***	−7.11***	−7.11***
(IV: Physician emigration)	(1.47)	(1.47)	(1.46)	(1.42)	(1.37)	(1.27)	(1.26)	(1.22)	(1.12)
# of registered physicians	−4.18***	−4.18***	−4.18***	−4.18***	−4.18***	−4.18***	−4.18***	−4.18***	−4.18***
(IV: Jewish physicians)	(1.35)	(1.35)	(1.34)	(1.32)	(1.30)	(1.27)	(1.23)	(1.27)	(1.33)
# of registered physicians	−4.41***	−4.41***	−4.41***	−4.41***	−4.41***	−4.41***	−4.41***	−4.41***	−4.41***
(IV: Jewish population)	(1.35)	(1.35)	(1.34)	(1.31)	(1.30)	(1.29)	(1.26)	(1.25)	(1.25)
	Dependent variable: Congenital debility/premature birth								
Spatial corr. cut-off	5km	10km	20km	50km	100km	150km	200km	300km	500km
# of registered physicians	−2.48	−2.48	−2.48	−2.48	−2.48	−2.48	−2.48	−2.48	−2.48
(IV: Physician emigration)	(4.81)	(4.79)	(4.80)	(5.08)	(4.59)	(5.04)	(4.61)	(4.79)	(4.99)
# of registered physicians	−0.92	−0.92	−0.92	−0.92	−0.92	−0.92	−0.92	−0.92	−0.92
(IV: Jewish physicians)	(3.27)	(3.27)	(3.28)	(3.38)	(3.07)	(3.35)	(3.41)	(3.05)	(3.44)
# of registered physicians	−0.29	−0.29	−0.29	−0.29	−0.29	−0.29	−0.29	−0.29	−0.29
(IV: Jewish population)	(3.39)	(3.40)	(3.41)	(3.53)	(3.18)	(3.46)	(3.55)	(3.26)	(3.71)
	Dependent variable: Stillbirths								
Spatial corr. cut-off	5km	10km	20km	50km	100km	150km	200km	300km	500km
# of registered physicians	−8.42*	−8.42*	−8.42*	−8.42**	−8.42**	−8.42**	−8.42**	−8.42	−8.42
(IV: Physician emigration)	(4.46)	(4.46)	(4.43)	(4.24)	(4.23)	(4.11)	(4.14)	(5.17)	(6.24)
# of registered physicians	−6.11***	−6.11***	−6.11***	−6.11***	−6.11***	−6.11***	−6.11***	−6.11**	−6.11**
(IV: Jewish physicians)	(2.28)	(2.29)	(2.25)	(2.21)	(2.19)	(2.08)	(2.07)	(2.66)	(2.82)
# of registered physicians	−6.88***	−6.88***	−6.88***	−6.88***	−6.88***	−6.88***	−6.88***	−6.88**	−6.88**
(IV: Jewish population)	(2.38)	(2.40)	(2.33)	(2.29)	(2.27)	(2.11)	(2.10)	(2.85)	(3.06)

Note: The table reports coefficient estimates from linear regression and standard errors corrected for spatial correlation based on Conley (1999) in parentheses. Regressions are based on the main specification and each cell reports the coefficient of interest and standard error from a single regression. Each panel corresponds to one major outcome. Rows signify different measures of the instrumental variable. Columns correspond to different choices for the distance cutoff in kilometers beyond which the correlation between the error term of two observations is assumed to be zero. *, **, *** denote significance at the 0.1, 0.05, 0.01 level respectively.

Table A13: Robustness: Linear model using log-transformed infant mortality counts

	(1)	(2)	(3)	(4)
	LOG TOTAL INFANT MORTALITY			
	TOTAL	INFL. BOWEL DISEASES	PREM. BIRTH, CONG. DEBILITY	STILLBIRTHS
	IV: PHYSICIAN EMIGRATION			
# of registered physicians	–0.24***	–0.77***	–0.03	–0.34***
	(0.06)	(0.19)	(0.16)	(0.09)
First stage F-stat.	28.51	33.65	35.10	35.21
	IV: JEWISH PHYSICIANS × YEAR ≥ 1933			
# of registered physicians	–0.18**	–0.51***	0.03	–0.24***
	(0.07)	(0.17)	(0.13)	(0.08)
First stage F-stat.	15.56	17.07	17.84	17.82
	IV: JEWISH POPULATION × YEAR ≥ 1933			
# of registered physicians	–0.17**	–0.52***	0.06	–0.26***
	(0.07)	(0.17)	(0.14)	(0.08)
First stage F-stat.	14.63	15.89	16.85	16.84
Control: ln population	✓	✓	✓	✓
Control: ln (live) births	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
N municipalities	317	317	317	317
N	2853	1958	1894	1901

Notes: All dependent variables are logged counts. Registered physicians are measured per 1,000 of population. Included instruments are a full set of year and municipality dummies. Excluded instruments are given in the respective paragraph header if applicable. Standard errors clustered at the municipality level given in parentheses. *, **, *** denote significance at the 0.1, 0.05, 0.01 level respectively.

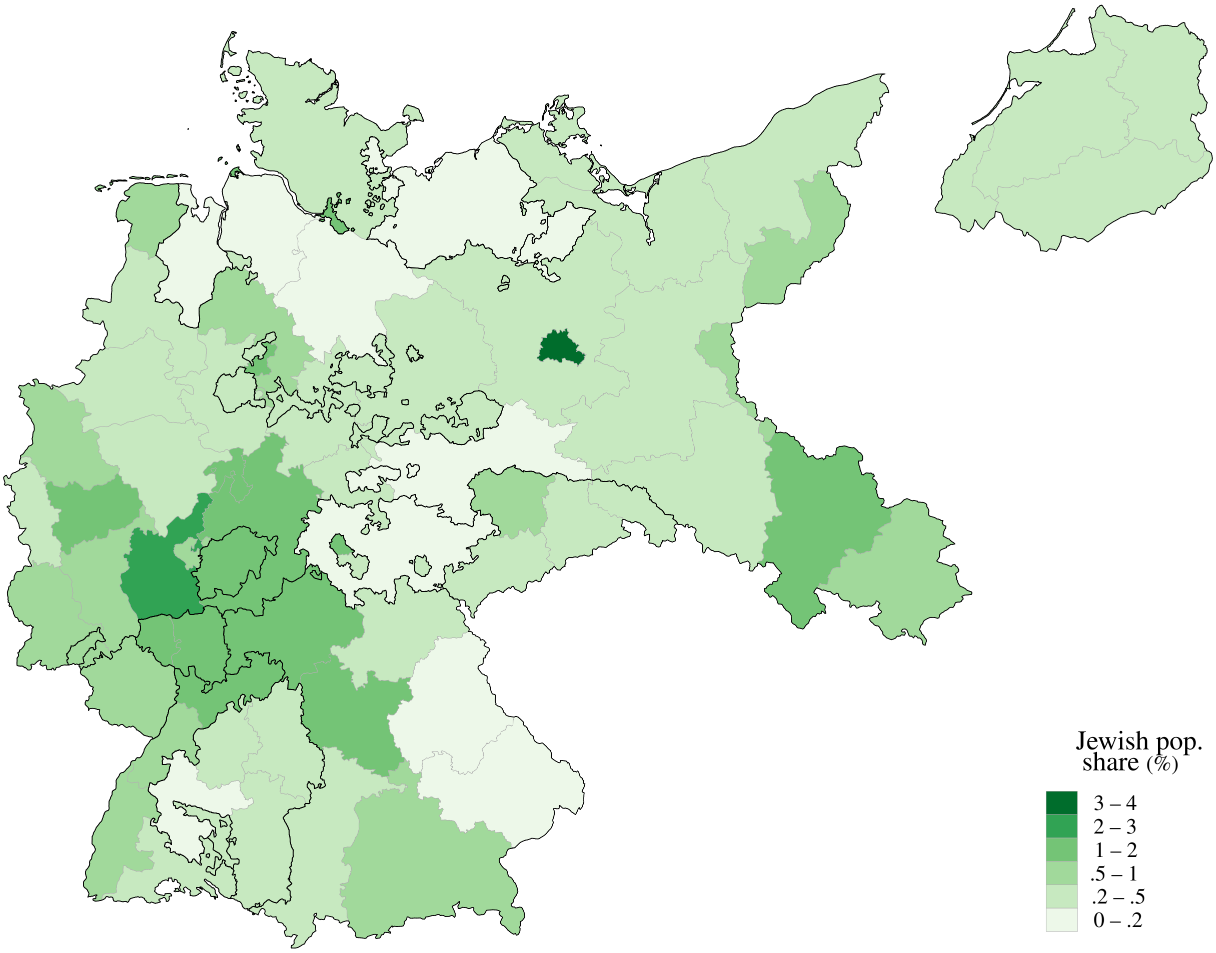
Table A14: Robustness: Exponential model mortality estimates

	(1)	(2)	(3)	(4)
	INFANT MORTALITY			
	TOTAL	INFL. BOWEL DISEASES	PREM. BIRTH, CONG. DEBILITY	STILLBIRTHS
	POISSON			
# of registered physicians	1.02	0.83	0.96	0.90
	(0.05)	(0.17)	(0.10)	(0.07)
	IV: PHYSICIAN EMIGRATION			
# of registered physicians	0.77***	0.32***	0.92	0.78***
	(0.04)	(0.09)	(0.12)	(0.06)
	IV: JEWISH PHYSICIANS $\times$ YEAR $\geq$ 1933			
# of registered physicians	0.83**	0.39***	1.01	0.84***
	(0.06)	(0.10)	(0.13)	(0.05)
	IV: JEWISH POPULATION $\times$ YEAR $\geq$ 1933			
# of registered physicians	0.84**	0.38***	1.03	0.82***
	(0.06)	(0.10)	(0.14)	(0.05)
Year fixed effects	✓	✓	✓	✓
Municipality fixed effects	✓	✓	✓	✓
N municipalities	317	317	317	317
N	2853	2853	1902	1902

Notes: Results from exponential model instrumental variable estimation using GMM. Estimates are reported as incidence rate ratios, indicating the multiplicative change in the dependent variable given a unit increase in the physician coverage ratio. The null hypothesis in all tests is that the coefficient takes value one. Infant mortality variables are measured per 1,000 live births, stillbirths per 1,000 births. Registered physicians are measured per 1,000 of population. Included instruments are a full set of year and municipality dummies. Excluded instruments are given in the respective paragraph header if applicable. Standard errors clustered at the municipality level given in parentheses. *, **, *** denote significance at the 0.1, 0.05, 0.01 level respectively.

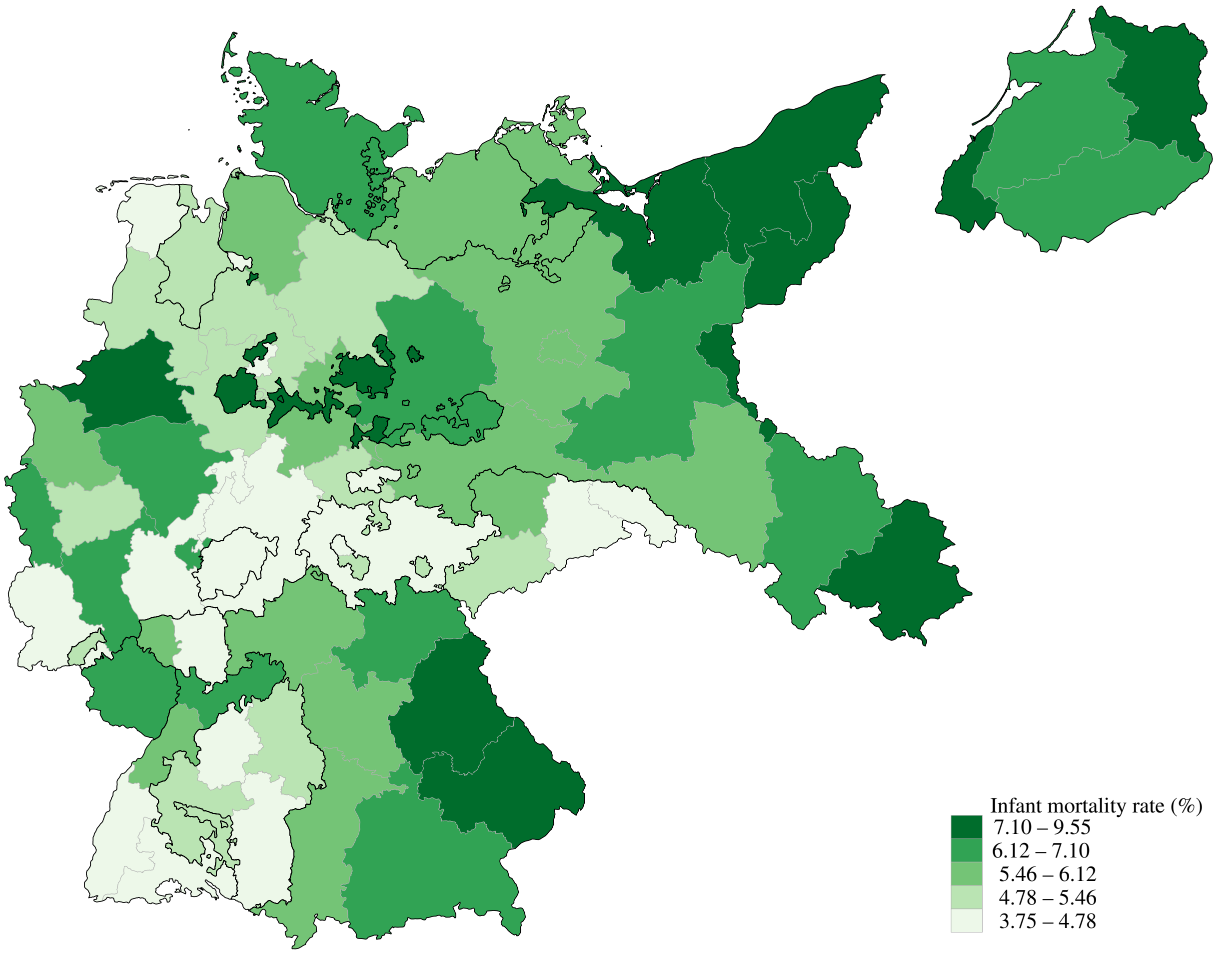
A.2 Figures

Figure A1: Jewish population share in German regions in 1933



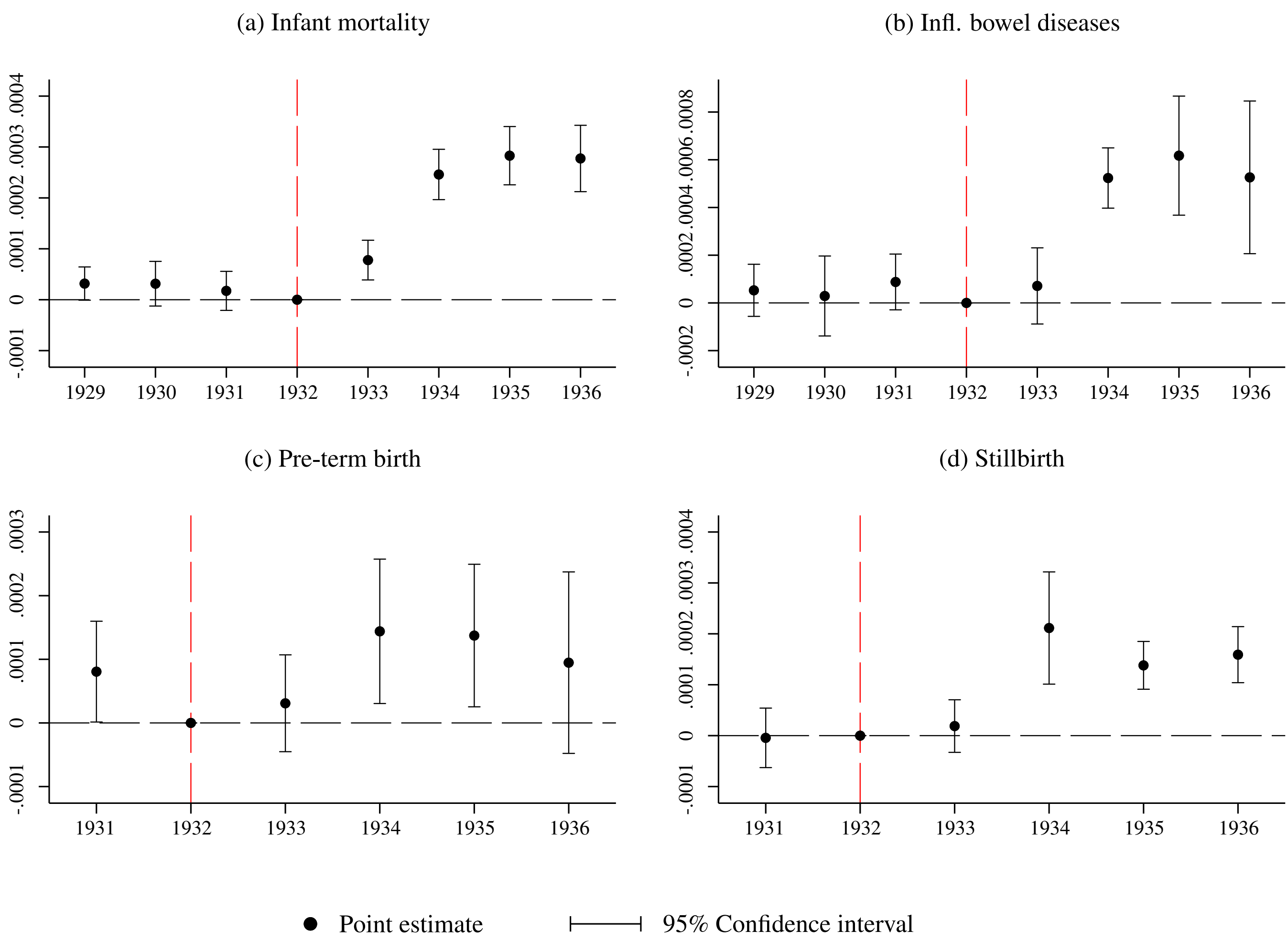
Notes: The graph shows the Jewish population share in 1933 across German administrative districts (*Regierungsbezirke*). District borders are shaded grey. State borders (*Länder*) are colored in black.

Figure A2: Infant mortality rate in German regions in 1933



Notes: The graph shows the infant mortality rate in 1933 across German administrative districts (*Regierungsbezirke*). District borders are shaded grey. State borders (*Länder*) are colored in black. Categories are based on quintiles.

Figure A3: Event study analysis for major infant mortality outcomes

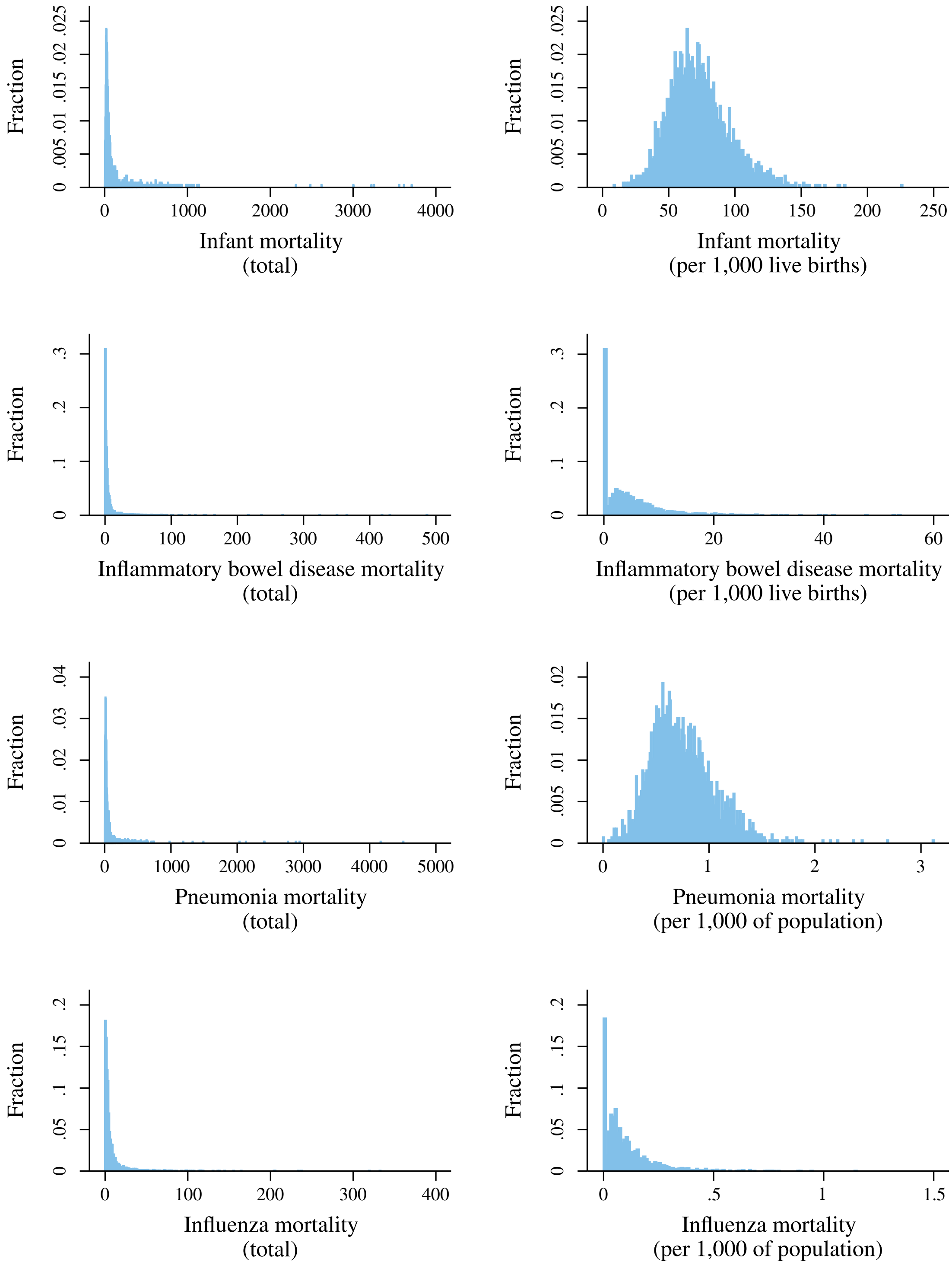


Notes: Estimates are based on a specification including year dummies interacted with emigration as the exposure measure. The excluded reference period is highlighted. The graph plots the coefficient point estimates and the corresponding 95% confidence intervals over time.

Figure A4: Comparison of count and reference-scaled distributions

(a) Raw counts

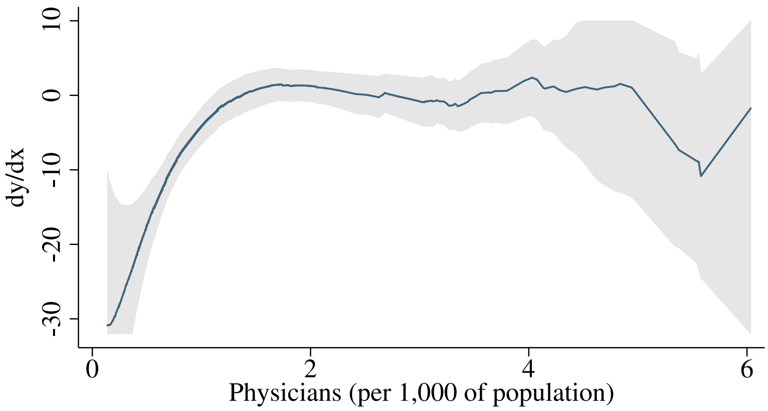
(b) Per reference population



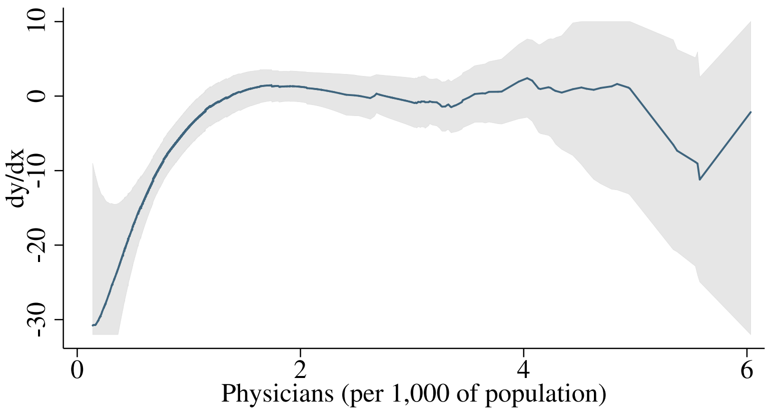
Notes: Column (a) shows the distribution of the original unscaled counts, column (b) the distribution of the same variables scaled per reference population. Each row depicts the same base variable. All plots are based on the (largest) main estimation sample.

Figure A5: Robustness: Control function and semiparametric analysis

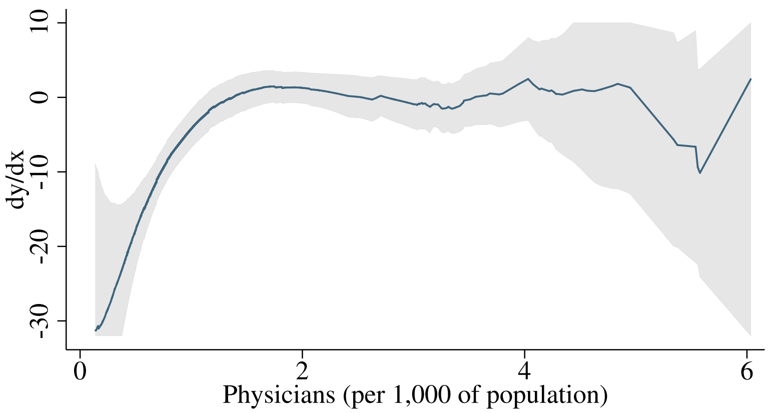
(a) Marginal effect (control function: linear)



(b) Marginal effect (control function: second order polynomial)



(c) Marginal effect (control function: third order polynomial)



Notes: Figures plot the marginal effect of physician density on infant mortality. Shaded in grey are the 95% confidence intervals. Panel (a) is based on an specification including only the linear control function term, panels (b) and (c) on a specification including a second/third order polynomial of the control function.

A.3 Exponential model

Because of the non-negative nature of the dependent variable in our analysis, the linear model specified in equations (2) and (3) may be inadequate. Although rescaling reduces the long right tail in the original count distribution for many variables, this does not work equally well in all cases (cf. Figure A4). Especially when the original variable has generally low incidence that is spread over a comparatively large reference population, the transformed data still resembles typical count data distributions, but is not discrete anymore.

We propose an alternative model specification to address this issue as a robustness check. In addition to the triangular model outlined in section 4.1, we estimate an exponential conditional mean model with multiplicative error structure (e.g. Mullahy 1997, Windmeijer and Santos Silva 1997)

$$\begin{aligned} y_{it} &= \exp(\beta s_{it} + \eta_i + \delta_t + \varepsilon_{it}) \\ &= \exp(\beta s_{it} + \eta_i + \delta_t) \nu_{it} \\ &= \mu_{it} \nu_{it} \; . \end{aligned} \tag{10}$$

For $\mathrm{E}[y_{it} \mid s_{it}] = \mu_{it}$, the error term in (10) must have a conditional unit mean, i.e. it must be that $\mathrm{E}[\nu_{it} \mid s_{it}] = 1$. This implies that

$$\mathrm{E}\left[\frac{y_{it} - \mu_{it}}{\mu_{it}} \,\middle|\, s_{it} \right] = 0 \; , \tag{11}$$

which will typically be violated due to endogeneity in the number of physicians s_{it}. Instead, we assume a conditional moment condition based on the orthogonal instrument z_i which satisfies

$$\mathrm{E}\left[\frac{y_{it} - \mu_{it}}{\mu_{it}} \,\middle|\, z_{it} \right] = 0 \; . \tag{12}$$

Using moment conditions based on (12), estimation by GMM is straightforward. Even though non-linear, the model features constant relative effects that are intuitive to interpret as incidence rate ratios. The exponentiated coefficients express the multiplicative change in the dependent variable given a unit increase in the independent variable. Since we only specify one conditional moment, the recursive interpretation of equations (2) and (3) is lost. In fact, this limited-information approach requires less assumptions than two-stage least-squares, as we make no assumptions about the distribution of s_{it} given z_{it} (cf. A. C. Cameron and Trivedi 2013). The model is also preferable to the simple method of log-transforming the dependent variable, as it avoids the log-of-zero problem and other issues associated with log-transformations (e.g. Silva and Tenreyro 2006). Like quasi-maximum likelihood models, it is consistent if the mean is correctly specified.

A.4 Infant mortality in historical context

This section draws a comparison between developing countries today and Germany in the first half of the 20th century. We focus on infant mortality rates, causes of death and the disease environment, health care supply and treatment technology. We show that there are both pronounced differences and important similarities within these dimensions. We do not want to argue that our results generalize to present-day developing countries. Instead, we want to be clear about the dimensions in which the situation in these countries differs from that during the time period we study.

Since the 1950s, infant mortality in many low and middle income countries has decreased substantially. Infant mortality in South Asia, the Middle East and Sub-Saharan Africa has decreased from around 160 infant deaths per 1,000 live births in the 1960s to between 40 and 60 in 2015 (see Figure A6). These changes resemble the development of infant mortality rates in industrialized economies a century ago (e.g. Cutler et al. 2006). In Germany, 207 infant deaths per 1,000 live births were registered in 1990. By 1933, this number had fallen markedly to 77 cases (cf. Figure A6).

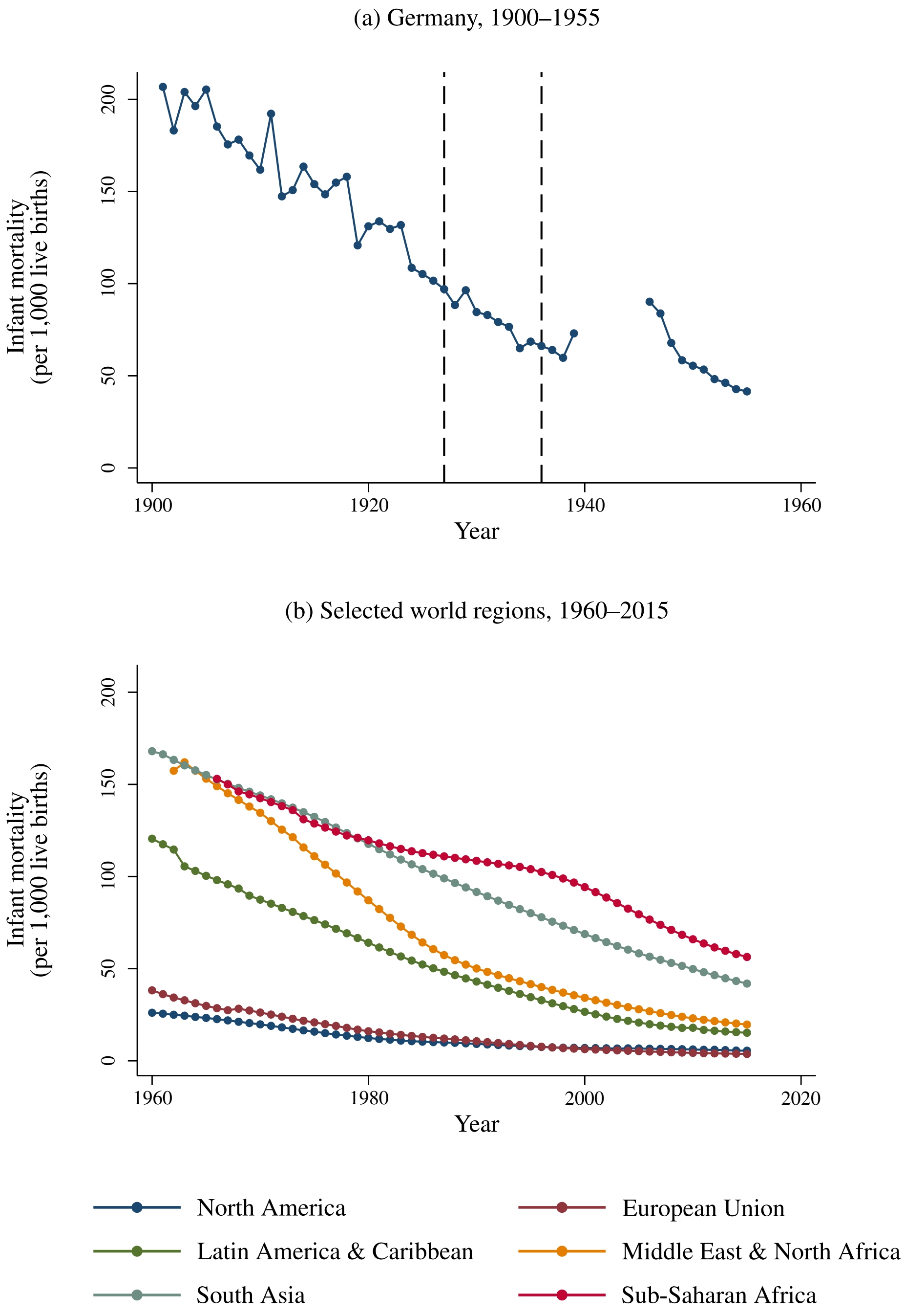
Both in developing countries today as well as in 20th Germany, this decrease was mainly driven by a reduction in mortality of infants which had survived the first month. Consequently, the share of neonates in infant deaths has increased by around 10% in middle and low-income countries since 1990 (The World Bank 2016).[11] Similarly, in Germany this share rose from 32% in 1890 to 52% in 1932 (Statistisches Bundesamt 1951).

Similarities are not only present with respect to infant morality rates but extend to the state of health care supply. In 1930, Germany's physician coverage ratio was approximately equal to 0.75 physicians per 1,000 population. This supply density is comparable to many developing countries today. Figure A7 displays physician coverage ratios for major world regions over the last 20 years. The lowest coverage ratio can be observed in Sub-Saharan Africa with less than 0.3 physicians per 1,000 population. Average coverage ratios in the Middle East & Northern Africa as well as South Asia are closer to our sample average of one physician per 1,000 of population.[12]

[11] Neonatal mortality refers to infants dying within the first 28 days of life (Andrews et al. 2008).

[12] Our sample average is slightly higher than physician density in the whole of Germany as our sample is selected

Figure A6: Infant mortality: Historical comparison



Notes: Panel (a) depicts historical infant mortality per 1,000 live births in Germany for the years 1900–1955, panel (b) shows infant mortality estimates per 1,000 live births for selected world regions over the time frame 1960–2015. Source: Statistisches Reichsamt (1900–1940), Statistisches Bundesamt (1945–1955) , World Development Indicators (2016).

Similar coverage ratios can be observed in many richer African or poorer Asian or Latin American countries, e.g. Bolivia, Pakistan or regions of India. However, these countries typically feature somewhat lower mortality rates. On the other hand, countries which have comparable mortality ratios today (e.g. those in Sub-Saharan Africa), typically also have lower physician supply ratios compared to Germany in 1930. This may reflect in part increases in medical technology which make treatments more effective.

Important parallels also exist regarding the main causes of infant mortality. Neonatal deaths in developing countries today are usually connected to inadequate access to basic medical care at and immediately following birth; the leading causes of neonatal death being infections (36%) such as sepsis, pneumonia, tetanus and diarrhoea, complications surrounding birth (27%) and asphyxia (23%).[13] Low birth weight is often a contributing factor (Lawn et al. 2005, Andrews et al. 2008). Postneonatal mortality can be attributed to malnutrition, infectious diseases and home environment (Andrews et al. 2008). These causes are similar to those common in developed countries in the early 20th century. The majority of infant deaths then were attributable to deficient pre- and post-natal care, infections and water- and food borne diseases, most commonly respiratory infections and gastrointestinal illnesses (Reichsgesundheitsamt 1926–1945, Cutler et al. 2006). Much like today, these diseases disproportionately affected the poor due to their living and working conditions and insufficient nutrition (Frohman and Brook 2006).

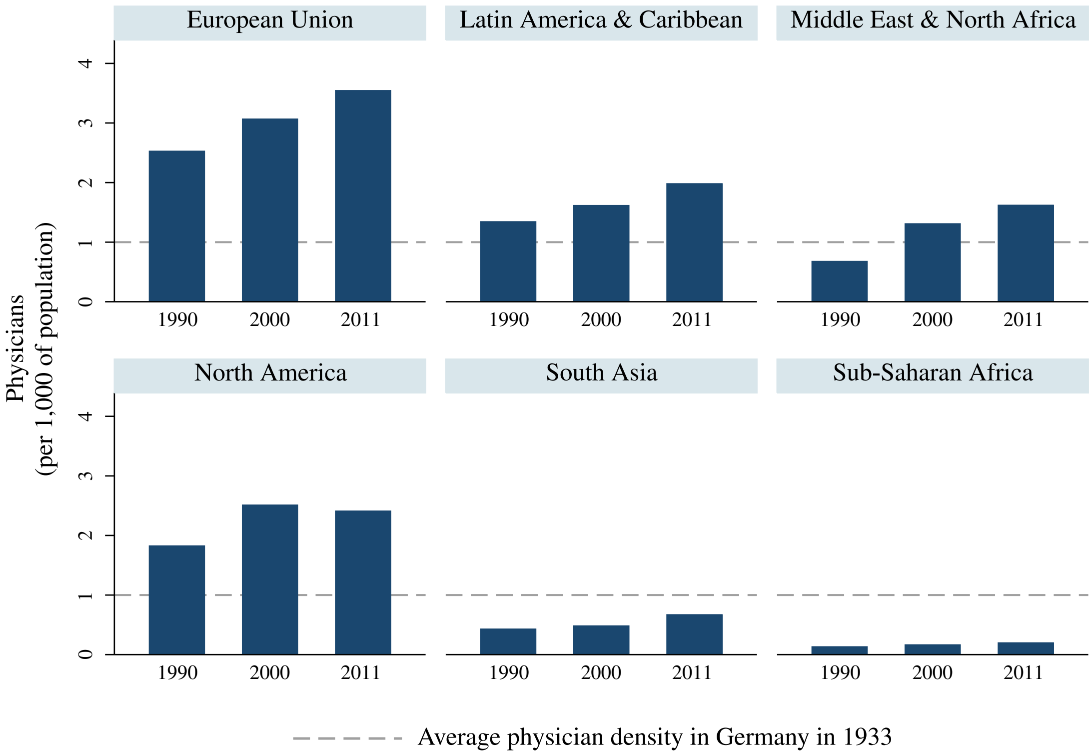
While infant mortality rates, causes of death and physician supply ratios are comparable to a certain degree between developing regions today and developed countries a century ago, the quality of health care has changed. Medical technology has advanced substantially since 1930. Still, a limiting factor is that medical technology diffusion from developed to developing countries is limited. Figure A8 shows prevalence rates of selected medical technologies for which comparable data is available (magnetic resonance imaging and computed tomography units) across world regions.[14] Especially in countries in South Asia and Sub-Saharan Africa, access to advanced medical technology is very limited. This lack of access is particularly pronounced for

on more populous areas where physician coverage ratios are generally higher.

[13] Around two-thirds of all births in developing countries occur at home and skilled-care is only available in about half of all cases. Postpartum visitation for the newborn is uncommon (Moss et al. 2002).

[14] In the context of infant mortality, data on ultrasound and incubator prevalence would be preferable. However, such data is unavailable. The available evidence suggests that supply is equally poor (Lawn et al. 2010, Ruiz-Peláez et al. 2004).

Figure A7: Physician density across world regions



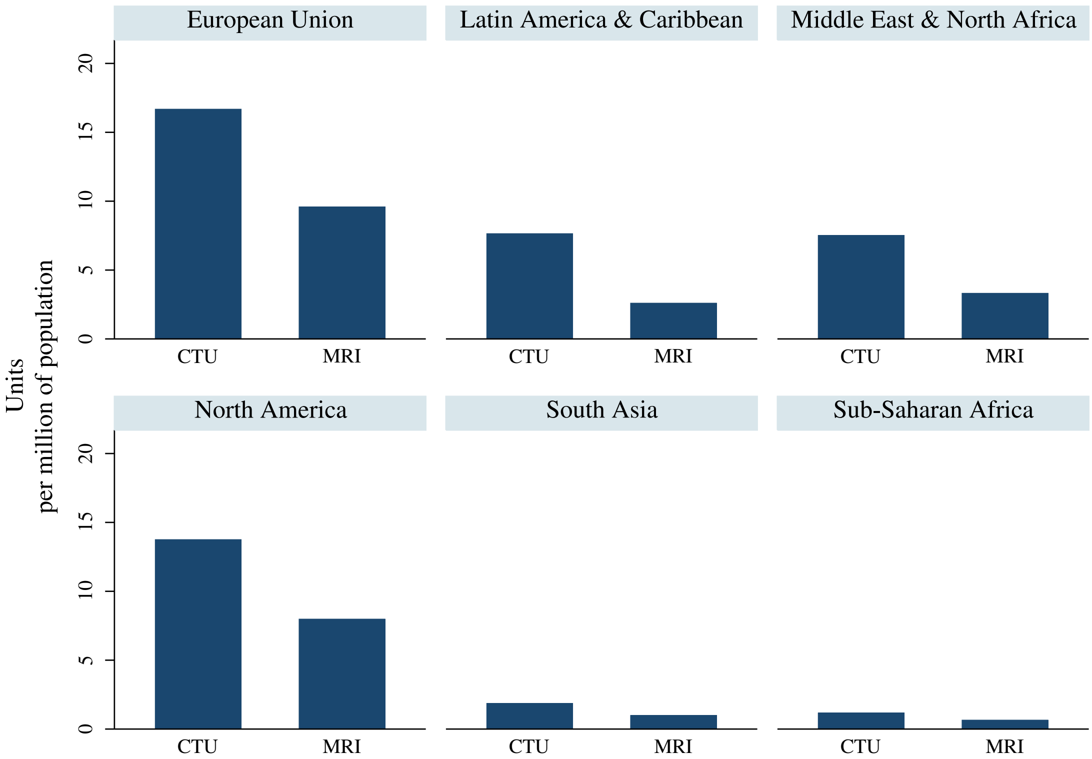
Notes: Bars depict the average physician density in a region for each available year. The horizontal line is the reference density in Germany in 1933. Source: World Development Indicators (2016).

rural and poor population segments as medical technology in developing countries is usually concentrated in cities and private hospitals (R. A. Malkin 2007, Peters et al. 2008). Moreover, Perry and R. Malkin (2011) estimate that around 40% of health-care equipment in developing countries is out of service compared to less than 1% in developed countries.[15] Access problems also extend to drugs, many of which are not available in low and middle income countries, especially in the public health sector. Even if certain medicines are available in the private sector, their price often substantially exceeds the international reference price which renders them prohibitively costly to large parts of the population (A. Cameron et al. 2009).

Another consideration is that modern medical technology is of limited relevance for addressing the main causes of infant mortality (e.g. Cutler et al. 2006, Andrews et al. 2008). Simple treatment measures are often sufficient to address many health issues. For example, gastrointestinal diseases are typically treated by ensuring sufficient rest, fluids, nutrition and possibly drugs like

[15]The lack of a reliable energy supply also constitutes a major hindrance to the employment of medical technology in developing countries. This is especially relevant for therapeutic devices such as neonatal incubators which need to be powered constantly. It is also of consequence for drugs and vaccines who need to be stored at low temperature to remain viable (Howitt et al. 2012).

Figure A8: Medical technology across world regions in 2013



Notes: Bars depict the average prevalence of computed tomography (CTU) and magnetic resonance imaging (MRI) units in a region. Source: World Health Organization, WHO (2016).

Acetaminophen to reduce fever and pain. In the case of birth asphyxia, resuscitation by tactile stimulation or the clearing of upper airway secretions using a covered finger or oral mucus trap is normally sufficient. A need for external ventilation is only given in exceptional circumstances (Moss et al. 2002).

Early diagnosis, disease prevention and health practices are relatively more important than treatment after a health problem has fully developed. Physicians play an important role in disease prevention by ensuring health and sanitary practices. Especially for perinatal deaths, pre- and post-natal care practices are a crucial factor (Cutler et al. 2006). For example, proper umbilical core care by physicians using antibacterial agents after birth has been shown to reduce infection of the cord and neonatal sepsis (Moss et al. 2002). Late-onset sepsis can be prevented by ensuring a clean caregiving environment. Similarly, a frequent problem in developing countries is hypothermia which affects more than half of all newborns and is associated with increased risk of neonatal infections, acidosis, coagulation defects, respiratory distress syndrome and brain haemorrhage. Neonatal incubators are only needed in extreme cases and hypothermia can generally be prevented by simple measures such as ensuring a warm environment during delivery, early breastfeeding, proper bathing, drying/swaddling and skin-to-skin contact with the mother (Moss et al. 2002).

The importance of health practices is mirrored in modern development economics (e.g. Dupas 2011). A large body of research focuses on how physicians can influence their patients' well-being by encouraging health-related behavior and compliance with hygienic standards (cf. Stanton and Clemens 1987, Fewtrell et al. 2005, Terza et al. 2008). Similarly, propagation of sanitary practices was also the leading sentiment in health policy in the early 20th century (Frohman and Brook 2006). The germ theory of disease was already well established at the time and the importance of a sanitary environment for the prevention of diseases was understood. In fact, many practices recommended by development organizations today are similar to those of physician organizations in Germany during the early 20th century. Examples are the recommendation of exclusive and immediate breastfeeding, in particular for low birth weight newborns and the dissemination of sanitary procedures such as sterilization of water and milk, among others (Moss et al. 2002, Frohman and Brook 2006). In the Weimar Republic propagation of such practices was primarily carried out through infant welfare centers staffed with physicians who provided free medical examinations to both mothers and their infants. Similar to development research today, policy debates focused on how to establish physicians as a recognized authority and improve compliance with their recommendations (Frohman and Brook 2006).

One field where technological progress has been very influential are vaccinations. Although many vaccines were invented during the first half of the 20th century (e.g. rabies, plague, diphtheria, pertussis, tuberculosis, tetanus and yellow fever) vaccination rates were still relatively low. Immunization rates increased sharply after 1960, and vaccinations for many important childhood diseases were invented afterwards (e.g. polio, measles, mumps, rubella and hepatitis B). However, even though morbidity consequences from these diseases are high, historical data suggests that direct mortality due to them was rare even prior to the availability of vaccines (e.g. Cutler et al. 2006). The exception to this is tuberculosis, which we exclude from the empirical analysis. To illustrate, the measles mortality rate in Germany in 1930 amounted to about 2 cases per 100,000 of population. This figure is comparatively low compared to mortality from pneumonia (approx. 80 cases per 100,000 of population) or total infant mortality (7,500 per 100,000 live births). Furthermore, even though immunization rates have increased steadily in developing countries over the last 20 years, vaccination is still far from universal especially among poorer population segments (WHO 2016). While vaccines for measles and diphtheria have become more widespread, for many of other diseases we consider in the analysis, this is not the case. Vaccines are either unavailable (gastrointestinal diseases, bronchitis, scarlet fever) or uncommon and do not offer long-term protection (influenza, pneumonia, typhoid fever). While vaccines matter for reductions in *child* mortality, they matter less so for *infant* mortality, where most deaths occur in the first month after birth.

Another important development was the discovery of antibiotics. Although penicillin was discovered in 1928, antibiotics were not commercially available for civilians before 1945. Similarly, the first sulfonamide drug Prontosil was first developed in 1935 but only gained widespread use during the 1940s. A limitation is that antibiotics and antimicrobial drugs are only effective against bacterial diseases, they are ineffective against viral diseases such as influenza or bronchitis. Other commonly prescribed drugs were already available and prescribed by physicians in 1930. Simple nonsteroidal anti-inflammatory drugs (e.g. acetylsalicylic acid, introduced as Aspirin by Bayer in 1899) and common pain and fever medications (e.g. Phenacetin, which metabolizes to Paracetamol (Acetaminophen), introduced in 1887) were readily sold in pharmacies (Jeffreys 2008).

In the paper, we analyze historical data from Germany, one of today's developed economies, and document the diminishing returns to health care provision during the later stages of the mortality transition. We do not want to argue that the historical context of our study provides a control case for present day developing countries. However, we provide this comparison to be clear about the dimensions in which the historical situation in today's developed countries compares to the situation in other developing countries, and those in which it does not. Undisputably, medical technology has advanced since the period of our study.

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