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Abstract

We examine the welfare consequences of reallocating high-skilled labor across national borders. A labor demand shock in Norway—driven by a surge in oil prices—substantially increased physician wages and sharply raised the incentive for Swedish doctors to commute across the border. Leveraging linked administrative data across the two countries and a difference-in-differences design, we show that this shift doubled commuting rates and significantly reduced Sweden's domestic physician supply. The result was a persistent rise in mortality in Sweden, with no corresponding health gains in Norway. These effects were unevenly distributed, disproportionately harming certain places and populations. The underlying mechanism was a severe strain on Sweden's healthcare system: shortages of high-skilled generalists led to more hospitalizations, premature discharges, and higher readmission rates. Mortality effects were larger in low-density physician regions and concentrated in older individuals and acute conditions.

JEL Codes: J2, J6, H1

Keywords: Brain Drain, Worker Mobility, Mortality

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1 Introduction

Cross-border labor mobility has surged in recent decades, with millions of individuals now living in one country while working in another. This shift is redefining the boundaries of local labor markets and allowing firms and countries to access talent beyond national borders. The trend is especially pronounced among young, high-skilled workers, who are more mobile and tend to sort into occupations that offer higher returns to cross-border employment (e.g. European Commission, 2017).

The rapid rise in cross-border mobility raises important questions about its broader economic and social consequences. From a theoretical perspective, the welfare effects are ambiguous. On one hand, increased outflows of high-skilled workers can strain sending regions—particularly in sectors that rely on complex, specialized skills, where talent is costly to train and difficult to replace. On the other hand, inflows can ease shortages in receiving regions, serving as a remedy for places struggling to develop or retain high-skilled labor. The overall impact depends on how these opposing forces play out across origin and destination regions. Despite the growing scale of high-skilled mobility, we lack evidence on whether reallocation improves overall efficiency or simply redistributes talent in ways that impose large costs on the places and people left behind.

These theoretical ambiguities are compounded by the fact that high-skilled mobility is empirically difficult to study. First, rich longitudinal data that link individuals across borders remain scarce, limiting our ability to trace worker movements and assess their downstream effects. Second, identifying causal impacts is difficult in the absence of plausibly exogenous variation in mobility, which is rare. Third, even where flows can be credibly measured, constructing comparable outcome metrics across institutional contexts and countries constitutes a substantial challenge.

This paper studies how high-skilled labor mobility affects both origin and destination regions, leveraging a unique empirical setting in the Scandinavian healthcare sector to identify its economic and social consequences. We exploit a sharp, exogenous increase in physician commuting from Sweden to Norway, using linked administrative microdata across countries to track individual worker flows and estimate their downstream effects. We begin by estimating the impact of physician commuting outflows on local mortality. We then examine how these effects vary across places and people, allowing us to evaluate how physician labor dynamics shape inequality in population longevity and mortality. Next, we investigate the mechanisms underlying these patterns, focusing on access to care, service quality, and system-level costs. Finally, we quantify the social value of physician labor by combining our mortality estimates with standard valuation methods. This allows us to evaluate whether the losses associated with physician outflows exceed the fiscal cost of retention and to compare these estimates to existing benchmarks for other healthcare workers.

Our empirical strategy exploits a labor demand shock in Norway triggered by a sharp surge in oil prices and rapid expansion of the country's oil sector. The shock rippled through the economy, raising real wages across industries and occupations and nearly doubling the within-occupation wage differential between Sweden and Norway. This substantially increased the incentive for Swedes to commute across the border for work, drawing more Swedish workers into the Norwegian labor market.

For most occupations—including non-physician healthcare workers such as nurses—the commuting response was concentrated in border regions, where geographic proximity made it feasible to fully substitute a Swedish job with one in Norway (Dodini, Løken and Willén, 2022). Physicians responded differently. Their pre-existing scarcity in Sweden, combined with Norway's institutional flexibility in contracting short-term medical labor, allowed them to respond on the intensive margin. Rather than fully substituting and changing jobs, physicians could take on short, irregular stints in Norway—oftentimes far from the border—without leaving their primary Swedish positions. As a result, physician commuting patterns were driven not by geographic proximity but by relative earnings gains, which varied with local pre-shock wage levels across Sweden.[1]

We estimate the effects of physician outflows using a dose-response difference-in-differences framework that leverages variation in physician commuting incentives across Swedish municipalities. The key idea is that the Norwegian wage boom increased the return to cross-border work, but the size of that return varied by location. Municipalities with lower pre-shock physician wages had larger potential earnings gains and, as a result, experienced larger commuting outflows. In other words, lower pre-shock wage levels translated to a larger "dose" of commuting pressure when Norwegian wages began to rise. We use average physician pay in the municipality in the year before the shock as a proxy for baseline commuting incentives. This measure captures persistent local differences in earnings potential shaped by demand conditions, institutional pay-setting, and broader labor market structures, while abstracting from individual-level characteristics. Because the wage measure is predetermined and the shock was common across space, this variation provides a plausibly exogenous source of differential exposure to physician brain drain as external commuting pressures grew.

To avoid confounding our estimates with potential general equilibrium and non-physician labor market changes, we exclude municipalities along the Swedish–Norwegian border. While border areas experienced the largest commuting shocks among non-physician workers, those shocks affected entire communities through large-scale worker movements driven by geographic proximity. These flows triggered broader local adjustments—including firm closures, sectoral reallocation, and population shifts—that make it difficult to isolate the effects of physician mobility from other general equilibrium dynamics (Dodini, Løken and Willén, 2022).[2]

[1]This shock is particularly interesting because labor mobility and cross-border commuting were already well established before the boom. However, the surge in real wages in Norway increased the return to commuting from Swedish communities (despite stable macroeconomic conditions in Sweden). This pulled workers into Norway rather than pushing them out of Sweden.

[2]Including these areas does not substantively affect our results, but they are difficult to interpret.

Our key finding is that the economic boom in Norway triggered a large increase in cross-border commuting by physicians. This brain drain on the intensive margin strained hospitals in affected Swedish municipalities by reducing physician availability, disrupting continuity of care, and raising costs as regions attempted to respond. These pressures increased hospitalizations, premature discharges, and readmission rates, resulting in significantly higher mortality rates. These effects were larger among older individuals, in areas with low physician density, and were primarily driven by acute conditions related to infectious, circulatory, and respiratory conditions. We find no corresponding mortality improvements in Norway.

Our identification strategy rests on two core assumptions. First, municipalities with different baseline physician wages would have followed parallel trends in downstream outcomes absent the shock. Second, the differences in baseline physician wages across municipalities are not proxies for other structural characteristics that both influence physician mobility response to the shock and shape mortality outcomes over time. We provide a battery of robustness tests supporting these assumptions. Event studies show flat pre-trends across all outcomes examined, placebo tests using data from a decade before the shock demonstrate that the results are not driven by municipality-specific trends, and permutation tests confirm that the findings are unlikely to arise by chance. Moreover, pre-shock physician wages are not systematically correlated with observable confounders such as age structure, baseline mortality, hospital capacity, or physician supply. We also find no commuting responses among other occupations or industries as a function of baseline physician pay, and no evidence that physicians respond to wage variation in other occupations.

As an additional check, we estimate effects using data from the main summer months when the majority of physicians take statutory leave. During this period, any increase in commuting likely reflects physicians using vacation time to work in Norway, so any reduction in domestic physician supply should be minimal. The absence of mortality effects during these months provides further evidence that our estimates are not driven by unobserved differences across municipalities, but by shocks to physician availability. Finally, our results are robust to alternative specifications, including a synthetic difference-in-differences method; saturated controls for demographic and occupational composition; and controls for contemporaneous shocks such as the swine flu outbreak, local income changes, and exposure to the Great Recession. For the latter, we include a Bartik-style control that interacts local industry composition with national industry-level employment changes. None of these appreciably change our estimates. Taken together, these patterns are consistent with an interpretation in which the mortality effects reflect the consequences of physician reallocation.

Our key take-away is supported by four sets of core findings. First, in the years following the shock, the number of Swedish doctors working in Norway rose by more than 50 percent, resulting in a substantial outflow of high-skilled physicians from Sweden and a corresponding gain for Norway.[3] This shift was concentrated among young, highly productive generalists, who tend to be

[3] We use the terms "commuting" and "commuters" to describe physicians working in and earning income on the

more cash-constrained and less mobility-constrained.

We then show that the outflow of high-skilled doctors from Sweden led to significant and persistent increases in mortality in areas with higher rates of physician out-commuting. These effects were concentrated among individuals aged 55 and over, a group more likely to experience acute health issues requiring immediate attention. Additionally, we find that the mortality effects were larger in municipalities with lower baseline physician density, consistent with the idea that areas with more limited healthcare capacity are less able to absorb physician outflows. In contrast, the influx of commuting doctors into Norway did not result in measurable improvements in mortality rates. This aligns with the fact that Norway already has one of the highest physician-to-population ratios in the OECD, while Sweden's ratio is about 20 percent lower. The limited marginal impact of additional doctors in Norway stands in stark contrast to the severe consequences of physician shortages in Sweden.

We next examine the distributional consequences of physician outflows and show that the effects extend well beyond average mortality. At the country level, the widening gap between Sweden and Norway highlights the asymmetric impact of high-skilled labor flows on healthcare systems. Counterfactual estimates suggest that fully closing the cross-country physician wage gap would have reduced Swedish mortality by 1 per 1,000 by 2013. Our back-of-the-envelope calculations further indicate that the welfare gains from such a reduction would have far exceeded the fiscal cost of raising physician wages to Norwegian levels. Within Sweden, municipalities with lower baseline physician density experienced significantly larger increases in mortality than higher-density areas, despite similar rates of out-commuting. In a separate analysis, we show that equalizing physician wages across the highest- and lowest-paid municipalities would have meaningfully improved outcomes in underserved regions. At the individual level, mortality effects are concentrated among low-income and less-educated populations, who rely more heavily on public healthcare and emergency services.

Finally, we explore the mechanisms driving these effects. The mortality increase is primarily linked to the loss of high-quality young generalist physicians, who faced the strongest incentives to commute and whose departure disproportionately disrupts first-contact and emergency care. Acute conditions such as circulatory, respiratory, and infectious diseases—where immediate intervention is critical—account for the entire rise in mortality. Beyond mortality, we find that the physician shortage placed substantial strain on Sweden's healthcare system. Hospitalization rates increased, likely reflecting a rise in misdiagnoses or lower-quality care at the initial point of contact. At the same time, hospitals attempted to manage capacity by accelerating patient discharges, often at the expense of patient needs, leading to considerably higher readmission rates. In response to the

Norwegian side of the border while officially residing in Sweden. This includes daily commuters whose primary job is in Norway, those working in Norwegian hospitals and clinics for short stays (e.g., weekends, 1–2 weeks per month), and those taking longer continuous stays of up to two quarters.

growing competition for doctors, Swedish municipalities raised wages in an attempt to limit outflows, but these efforts fell short of reversing the trend. Instead, they increased hospital operational costs, further limiting resources and capacity. Taken together, fewer doctors, rising hospitalization rates, increased operational costs, and premature discharges overburdened the system and led to worse outcomes.

The overall mortality effect in our setting is negative. While the severity of this effect depends on factors such as the initial distribution of doctors, the baseline quality of healthcare services, and the ability of institutions to train and replace healthcare workers, our findings are especially relevant given the ongoing healthcare workforce crisis in Europe and the U.S. As of 2022, the EU faced a shortage of 1.2 million doctors, nurses, and midwives, while the U.S. is projected to experience a shortfall of up to 86,000 physicians by 2036 (OECD and European Commission, 2024; GlobalData Plc, 2024). These shortages have already strained healthcare systems and limited access to care. The fact that many countries are struggling to maintain adequate staffing suggests that the negative effects we document may have greater generalizability. Given Scandinavia's strong healthcare system and financial capacity, our results could be seen as conservative estimates.

This paper provides causal evidence on how cross-border reallocation of high-skilled labor affects welfare, inequality, and institutional capacity. Using rich administrative microdata and an exogenous shock to physician mobility between Sweden and Norway, we trace the downstream consequences of talent loss for public services and population outcomes. Our findings speak to economic questions about misallocation, institutional resilience, and the efficiency-equity tradeoffs of labor mobility.

First, we contribute to the literature on brain drain and high-skilled labor mobility by providing causal evidence on how outflows of critical talent affect mortality, inequality, and institutional performance in a developed, integrated labor market. Prior research has focused on individual returns, selection, and education system responses—especially in developing countries (e.g., Docquier and Rapoport, 2012; Khanna et al., 2022; Abarcar and Theoharides, 2024) or historical episodes (e.g., Waldinger, 2012; Becker and Ferrara, 2019). But despite growing concerns about high-skilled migration within the OECD, causal evidence on its population-level consequences remains scarce (Batista et al., 2025). We fill this gap by showing that even short-term, intensive-margin physician outflows can disrupt public service provision and widen health inequalities. Unlike most studies, our setting allows us to observe both sides of the border, enabling a rare analysis of how high-skilled mobility affects institutions and welfare in both sending and receiving regions. These effects are likely especially pronounced in sectors where skills are highly specialized and supply is slow to adjust. For example, in contrast to nurse migration from the Philippines, where educational systems rapidly scaled to meet global demand (Abarcar and Theoharides, 2024), physician training is lengthy, costly, and difficult to expand quickly. We also contribute to the smaller causal literature on how physicians respond to wage differentials across borders (e.g., Devlin and McCormack, 2023; Clemens and Gottlieb, 2014; Alexander, 2020; Alexander and Schnell, 2024).

Second, we show that labor market shocks in high-skilled, non-substitutable occupations can amplify inequality across both places and people. Research in economic geography and health economics has documented large place-based differences in health outcomes (e.g., Finkelstein, Gentzkow and Williams, 2021; Sanbonmatsu et al., 2012), poverty-related disparities (Lleras-Muney, Schwandt and Wherry, 2024), and the impact of macroeconomic conditions on mortality (Finkelstein et al., 2024). However, much of this work relies on observational correlations and stops short of identifying underlying mechanisms. We provide new causal evidence that reductions in healthcare capacity—driven by the loss of physician human capital—widen both geographic and socioeconomic disparities in mortality, revealing how shocks to local labor markets contribute to persistent place-based inequality.

Third, we highlight the underexplored consequences of migration for origin countries. While a vast literature studies how immigration affects wages, employment, and firm behavior in receiving countries (e.g. Johnson, 1980; Grossman, 1982; Borjas, 1987; Card, 1990; Friedberg, 2001; Borjas, 2003; Ottaviano and Peri, 2012; Foged and Peri, 2016; Friedberg and Hunt, 2018; Piyapromdee, 2021; Muñoz, 2024), far less is known about how sending regions absorb the loss of skilled labor. Existing work emphasizes individual labor market outcomes and firm responses (Hafner, 2021; Bütikofer, Løken and Willén, 2022; Dicarlo, 2022; Dodini, Løken and Willén, 2022), but we shift the focus to system-wide effects. We show that even temporary reallocations of essential labor can strain public services, degrade healthcare quality, and reduce institutional effectiveness.

Fourth, we advance the literature on the economic consequences of physician shortages by showing their direct population-level effects. Prior work links physician characteristics to patient outcomes (Badinski et al., 2023; Finkelstein, Gentzkow and Williams, 2021; Ginja et al., 2025) and connects physician time pressure or burnout to clinical decision-making (Chan, 2018; Silver, 2021; Currie, MacLeod and Musen, 2024). We help advance this literature by identifying how large-scale outflows affect system-wide mortality, particularly among older populations and in regions with low baseline capacity. This underscores the economic cost of losing generalist physicians, who provide first-contact care and help prevent downstream health deterioration.

Fifth, we reveal how shortages in critical skill areas affect institutional performance under constraints. Beyond lowering care quality, we show that physician shortages overload hospitals, raise hospitalization and readmission rates, and prompt premature discharges. While previous research has studied how hospitals respond to financial incentives, nurse shortages, and strikes (e.g., Gaynor, Moreno-Serra and Propper, 2013; Propper and Van Reenen, 2010; Gruber and Kleiner, 2012; Friedrich and Hackmann, 2021; Knutsson and Tyrefors, 2022), we identify how talent loss from international labor market shocks can destabilize systems. These results offer a new empirical channel through which labor supply affects state capacity and public sector performance.

Finally, we contribute to the study of labor market frictions and institutional rigidity in public systems. While economic logic suggests that employers should raise wages to retain scarce talent, public sector pay structures are often rigid, negotiated centrally, or slow to adapt. We show that even when municipalities raise physician wages, they may fail to prevent outflows or avoid system strain. Increasing pay for some workers from fixed or inflexible inputs necessarily requires a reallocation of resources. This extends prior work on rigid public pay and local conditions (Propper and Van Reenen, 2010; Britton and Propper, 2016; Willén, 2021) to a setting where wage pressure stems from international integration rather than domestic variation. Our findings suggest that public services are especially vulnerable in open labor markets, where wages are flexible but staffing systems might not be.

2 Background

2.1 Swedish Healthcare Sector

Sweden's healthcare system is based on a publicly funded, universal model primarily financed through taxes and copayments at the point of service.[4] The system is designed to ensure broad access to healthcare services for all citizens, with copayments being relatively low and capped annually to limit out-of-pocket expenses.[5] While the public system provides comprehensive coverage, private providers have become more common over the past few years. However, this was extremely rare during our analysis period.[6]

The healthcare system in Sweden operates within a multi-tier structure, with general practitioners, specialists, and hospitals forming the core of the system. Primary care is delivered by general practitioners, while more specialized treatments are referred to specialists and hospitals. During our sample period, emergency departments in Sweden were primarily staffed by generalists and primary care physicians. Larger hospitals sometimes involved specialists from internal medicine, surgery, orthopedics, and anesthesiology, but these settings—mainly university hospitals—are excluded from our analysis.[7] Eighty percent of physicians in Sweden are represented by the Swedish Medical Association (SMA), which helps doctors in contract negotiations, regulates licensing and training standards, offers legal assistance in disciplinary matters, and helps establish safety and patient care standards in the country. The number of medical school seats at university is primarily determined by the national government. While the SMA does not control admissions, it frequently weighs in on policy debates about training capacity and expansion, often emphasizing the need to

[4]Norway's health care system shares many core features with Sweden, so we focus on details relevant to our Swedish results. We highlight differences with Norway only when they matter for interpretation.

[5]The annual cap in 2003 was 900 SEK for out-patient visits and 1,800 SEK for prescription medication, which corresponds to approximately $135 and $270 in 2025 dollars, respectively, after adjusting for inflation. For reference, the exchange rate during our main sample period (2001-2014) was approximately $1:6.1 SEK.

[6]During our sample period, less than 5 percent of individuals held private health insurance.

[7]Since 2012 (one year before our analysis ends), Sweden has gradually transitioned toward employing Emergency Medicine specialists who work exclusively in emergency departments, aiming to improve continuity and expertise in acute care.

align medical education with available clinical training resources.

Since the mid-1990s, physician wages in Sweden have been set through individual negotiations between employers and employees, leading to variation by region and role. Regional wage differences reflect factors such as local budgets, healthcare demand, and staffing needs. Table A1 shows the correlation at the municipality level between baseline physician wages and a broad set of local characteristics. The table is organized into four panels. Panel A focuses on variables most directly relevant to our analysis—such as mortality rates, healthcare staffing levels, population size, and local (municipality) budgets. Panel B presents correlations with demographic characteristics, including age structure, education, immigrant share, and gender. Panels C and D report employment shares in 1-digit occupation and industry groups, respectively (defined in the table notes). Appendix Table A2 complements this by showing correlations between physician wages and wages in each 1-digit occupation (all measured in 2003; the year before the shock).

Overall, baseline physician wages are only weakly correlated with these observable characteristics. Some indicators of healthcare need—such as age composition, unemployment, education, and mortality—exhibit modest associations with wages, consistent with Sweden's decentralized wage-setting system, which partially aligns compensation with local demand. This generates useful spatial variation in pre-shock physician wages, which we exploit to identify the effects of physician out-commuting on local health outcomes.

These cross-sectional differences do not threaten our identification strategy. Our design relies on the assumption that, absent the shock, municipalities with different baseline physician wages would have followed similar trends in downstream outcomes. In Section 3.4, we show that this assumption holds: event study estimates reveal flat pre-trends, placebo tests using earlier periods find no anticipatory effects, and our results remain robust to a range of controls. Together, this evidence supports a causal interpretation of the post-shock divergence in health outcomes as a response to variation in physician outflows.

2.2 Working Across the Border

The Swedish and Norwegian labor markets are among the most integrated in Europe, shaped by long-standing institutional, linguistic, and economic ties. Since 1954, the Nordic Passport Union has allowed citizens to live and work freely across borders without residence or work permits—an arrangement later incorporated into the Schengen Agreement (1996). Importantly, Nordic citizens are allowed to work in another Nordic country without establishing legal residence there. Labor market institutions, education systems, and welfare structures are broadly similar across the two countries, and occupational licensing restrictions are minimal, including in healthcare. These conditions substantially reduce barriers to mobility.

Language and cultural proximity further facilitate workplace integration. Norwegian and Swedish are mutually intelligible, and workers routinely use their native language in professional settings. This is also codified in law; for instance, Norwegian higher education regulations explicitly permit instruction in Swedish, Norwegian, or Danish. Taxation and welfare agreements between the Nordic countries are designed to avoid double taxation and ensure coordination of social benefits. In general, individuals pay income taxes and accrue welfare entitlements in the country of employment.[8]

Cross-border labor mobility has been substantial for decades. While Norwegians historically commuted into Sweden, the flow has reversed in recent decades. Today, Swedish nationals account for roughly 80 percent of all Nordic cross-border commuters. Norway's higher wages and strong labor demand have made it an attractive destination for Swedish workers—especially younger, high-skilled individuals. Fewer than 2,000 Norwegians commute to other Nordic countries each year.

The cross-border commuting flow from Sweden to Norway was substantial in the late 1990s and early 2000s, with around 30,000 workers annually. It remained stable until 2004. It then more than doubled in the following years due to rising labor demand and wage growth in Norway. Doctors were among the first to increase commuting, shifting patterns nearly a year earlier than other occupations. Unlike other workers, they could adjust labor supply on the intensive margin by working short, irregular stints. Their flexible contracts and autonomy over working hours allowed them to respond more quickly to wage differentials than other professions. Instead of committing to permanent relocation, they could selectively increase commuting when Norwegian wages outpaced Swedish alternatives. On average, Swedish commuters spend 3.8 years working in Norway, while those who transition fully stay for an average of 2.3 years.

Swedish doctors can easily commute to Norway due to streamlined mobility rules. As discussed above, Swedish nationals have the right to live and work in Norway without the need to apply for a work or residence permit. To work as a doctor, however, Swedish citizens must still obtain a Norwegian Medical Certificate. This can be acquired through a simple online form where workers upload their relevant medical degree along with information about their citizenship. The process is quick (approximately 4 to 6 weeks), and the certificate is valid for life. There is no language barrier; Swedish doctors can work in Norway using Swedish. Many Swedish physicians find work in Norway through third-party staffing agencies that match Swedish labor supply with Norwegian demand. This system is institutionalized: the Norwegian government grants recruitment rights to a fixed number of private companies (currently eight) authorized to place Swedish doctors in Norwegian healthcare positions. In parallel, a substantial share of commuting physicians also secure positions through direct contact with Norwegian employers. These institutional arrangements closely mirror many of the *locum tenens* arrangements in, for example, the United States, where the Interstate Medical Licensure Compact allows physicians across 39 states to work across state lines, and a number of large private companies place these physicians in short-term

[8]An exception relates to workers who both reside and work in neighboring border municipalities, but those are excluded from our analysis.

contracts in hard-to-staff areas or areas undergoing manpower transitions.

2.3 The Norwegian Economic Boom

Before 2004, Sweden and Norway exhibited relatively stable economic conditions, with parallel per capita GDP growth trends, stable exchange rates, and consistent macroeconomic factors across both countries. However, between 2004 and 2009, Norway experienced a significant divergence in economic growth, driven primarily by the sharp rise in oil prices and a rapid expansion of the oil sector. In particular, the annual GDP per capita gap increased from approximately USD 11,000 in 2003 to USD 43,000 in 2013. Figure A1 shows the change in GDP per capita and exchange rate across the two countries over our analysis period.[9] Figure A2 shows the revenue from the oil sector to the Norwegian government over time - the primary driver behind these divergent macroeconomic trends. As shown in the figure, oil revenue remained relatively stable until 2003-2004, after which it increased substantially, doubling by 2006–2008.

This period of economic boom in Norway was characterized by an increase in GDP, a dramatic decline in unemployment, and a substantial rise in wages across nearly all occupations (Dodini, Løken and Willén, 2022). This wage growth was broadly distributed, affected both private and public sectors, and was largely driven by both broad-based increases in demand and Norway's national sectoral collective bargaining system. Wages in the export-oriented industrial sector, where the oil industry plays a central role, set the benchmark for wage negotiations in other sectors due to Norway's coordinated wage-setting system. In this system, the internationally exposed sectors negotiate first, establishing a wage norm (or benchmark) that other industries tend to follow. When the export sector secures strong wage growth, this norm pulls up wages in the rest of the economy, leading to significant spillover effects across all sectors (Bhuller et al., 2022; Dale-Olsen, 2024). As the oil sector, which accounts for 25% of Norway's GDP and 35% of state revenues, continued to perform well, the increased government spending and aggregate demand fueled substantial wage growth across various industries. The implication was a considerable increase in the within-occupation wage gap between Norway and Sweden – both overall as well as for physicians (Panels (a) and (b) of Figure 4).[10]

This aggregate wage growth translated into substantial gains for physicians in Norway relative to their Swedish counterparts across the entire wage distribution. Panels (c) and (d) of Figure 4 illustrate the wage distributions for physicians in Sweden and Norway in 2003 and 2013. Even at baseline, Norwegian physicians earned substantially more than their Swedish counterparts. Over the subsequent decade, the Norwegian distribution shifts markedly to the right, while the Swedish distribution remains relatively stable, despite being shown in nominal terms. This sharp divergence highlights the dramatic rise in the return to cross-border commuting for Swedish doctors, particu-

[9] We focus on the non-PPP adjusted GDP per capita as Swedish commuters would receive a wage from Norway but live and consume in Sweden.

[10] Figure A3 presents the same within-occupation wage gap over time, stratified by ISCO-88 occupation codes.

larly for those in lower-paying municipalities. The evolving wage gap between the two countries is a central driver of the commuting response we document.

2.4 Commuting Shock

The Norwegian labor demand shock propagated across the economy, generating a significant inflow of Swedish workers into various industries. This is illustrated descriptively in Panel (a) of Figure 1, which plots the raw number of Swedes with positive wage income from Norway (while still residing in Sweden) between 2001 and 2013. The figure shows a relatively steady number of Swedes crossing the border for work annually—approximately 30,000—before 2004. However, this number climbs rapidly as the labor demand shock takes hold in Norway, doubling to 60,000 workers in less than three years. This increase is substantial given the relatively small populations of the Scandinavian countries (4.5 million in Norway and 9 million in Sweden in 2003).

Panel (b) of Figure 1 focuses on Swedish doctors and reveals a similar commuting pattern as seen in the overall population, although Swedish doctors begin commuting about a year earlier. This may reflect the greater flexibility of Swedish physicians' contracts and their leverage at the negotiation table, enabling them to substitute portions of their Swedish positions for Norwegian equivalents relatively quickly (discussed in detail in Section 2).

Panel (c) of Figure 1 presents analogous information, measured as the growth rate in the share of commuters relative to the baseline (2003), across health workers, non-health workers, nurses, and doctors. Across all groups, there are notable increases in commuting growth rates, ranging from 50 percent for doctors to over 100 percent for nurses.

To examine where these commuters come from, Figure 2 provides heat maps showing the geographic variation in commuting responses for all workers (Panel a), nurses (Panel b), and doctors (Panel c). These maps highlight a key nuance: while most workers' responses correlate with their proximity to the border, doctors do not follow this pattern. We hypothesize that this is due to the relatively flexible contracts of Swedish doctors, which make it easier to substitute part of their Swedish work with Norwegian opportunities. Additionally, the absence of large hospitals right on the Norwegian side of the border necessitates air travel for Swedish commuting doctors regardless of where in Sweden they live.

If distance does not drive Swedish doctors' decisions to commute, what does? Figure 3 demonstrates that physicians are more likely to seize improved labor market opportunities in Norway when the return to commuting is higher. Specifically, there are significant regional differences in physician wages within Sweden, with the 25th percentile physician wage being 451000 SEK in 2003 and the 75th percentile being 573160 SEK. Panel C of Figure 4 provides the full distribution of average physician wages across Swedish municipalities in 2003 and 2013.

Figure 3 shows a strong relationship between average pre-shock physician wages in a municipality and the subsequent commuting response (measured as the percentage change relative to baseline). This pattern supports our identification approach, which exploits variation in baseline commuting incentives driven by local wage differentials. Notably, we find no such relationship for non-doctors—regardless of whether physician or non-physician wages are used as the baseline measure (Appendix Tables A11 and A12). This likely reflects institutional differences: non-doctors tend to hold less flexible contracts that require near-full-time, daily commuting, making distance a stronger constraint. In contrast, physicians can take on periodic and irregular shifts, allowing them to respond more directly to wage differentials rather than proximity.

The geographic variation in physician commuting is central to our identification strategy. While the Norwegian wage shock had the strongest direct effects near the border, prior research shows that Swedish border municipalities experienced broader general equilibrium changes, including shifts in firm behavior, population composition, and local economic activity (Dodini, Løken and Willén, 2022). In these areas, many workers were able to fully substitute their Swedish employment with full-time jobs in Norway, amplifying the local effects of the shock well beyond physician mobility. These overlapping adjustments make it difficult to isolate the causal impact of physician outflows on population health. Focusing on non-border municipalities allows us to hold these other margins constant. Crucially, physicians—unlike most other workers—can respond to cross-border wage differentials even when living far from the border, due to highly flexible contract structures and the ability to work short, irregular stints. We confirm that no other occupational group exhibits a similar response pattern, reinforcing our identification strategy.[11]

3 Data and Method

3.1 Data

Our primary data come from administrative registers at Statistics Sweden, covering all individuals aged 16 through 65 from 2001 to 2013. The demographic data include age, gender, marital status, family composition, education, and residence. Socioeconomic data include employment, occupation, earnings, and social welfare participation.

Using a database established by the Nordic Council of Ministers to track worker flows across the peninsula, we link our individual data to a Norwegian register detailing the employment history of all Swedes that have ever worked in the country. This data, also covering 2001-2013, provides individual-level information on Swedish residents' labor market activities in Norway, including employment, earnings, and municipality of work. These data are linked to our main data via social security numbers shared between the two statistical agencies. Together, these data enable us to identify all doctors in Sweden, their characteristics, and if they have ever worked in Norway.

We combine these data with health data extracted from administrative records held by Statistics Sweden. These data include information on mortality, cause of death, hospitalizations, length of

[11] In their work, Schlenker (2024) investigate hospital mortality rates near the German border, attributing increased mortality to a shortage of nurses who began commuting to Switzerland following the 2011 appreciation of the Swiss franc. However, because the shock affected other types of workers, firms, and economic conditions near the border, it is not possible to isolate the effects of nurse brain drain.

hospitalizations, time between hospitalizations, and surgeries, for each individual in the country. These data allow us to directly examine the potential welfare effect of brain drain from Sweden to Norway, as well as identify the underlying mechanisms.

Finally, to better understand whether any potential brain drain in Sweden is offset by brain gain in Norway, we supplement our data with rich register-based data from Statistics Norway. These data mirror the Swedish data described above (with the exception of the health mechanisms) and allow us to examine whether the inflow of high-skilled doctors into Norway had a positive health effect on the population as measured by mortality.

Table A3 presents summary statistics. Since we use a dose-response difference-in-differences design, we do not require treatment and control groups to be identical, only that they would have trended similarly in the absence of the shock, which we examine in detail in the results section.

3.2 Overview of Design

In theory, the rapid growth of the Norwegian economy generated improved labor market opportunities accessible to all Swedes willing to commute. This presents a central challenge for our analysis: identifying observational units within Sweden that experienced varying degrees of exposure to commuting pressure that only impacted the commuting patterns of physicians. Defining such variation is crucial for establishing a clear set of treatment and control units to disentangle the causal effects of brain drain.

To address this challenge, we leverage regional differences in physician pay within Sweden before the onset of the labor demand shock in Norway. Average physician salaries vary significantly across municipalities (Panels (c) and (d) of Figure 4). Importantly, physicians are more likely to pursue improved labor market opportunities in Norway when the financial return to commuting is higher, as it is when baseline physician pay in Sweden is lower. By capitalizing on these pre-shock differences, we implement a dose–response difference-in-differences approach, treating regional variation in baseline pay as a predictor of the commuting response. This design mirrors those used in prior work that leverage compensation differences to identify causal effects in a range of settings (Willén, 2021; Britton and Propper, 2016; Propper and Van Reenen, 2010).

We use average physician wages in each municipality in the year before the shock to proxy for baseline commuting incentives. This measure captures persistent local differences in earnings potential—driven by institutional pay-setting, staffing needs, and labor market conditions—while abstracting from individual characteristics such as age, specialization, or hours worked. Areas with lower wages prior to the shock received a larger "dose" of commuting pressure as wages in Norway grew. Since the Norwegian wage boom was national and unrelated to Swedish local labor market conditions, this variation provides a plausibly exogenous source of differential exposure to physician outflows. Crucially, our identification strategy does not rely on municipalities being similar in levels of physician wages or health outcomes. Instead, it requires that: (i) municipalities with different baseline wages would have followed parallel trends in the absence of the shock;

and (ii) no other shocks correlated with baseline wages jointly affect physician commuting and downstream outcomes. We assess both conditions empirically. Pre-trends are flat across the wage distribution for all key outcomes, and we find no evidence of differential changes in other potential confounders. Section 4 discusses these assumptions and tests in detail. Taken together, the evidence supports our interpretation of baseline wage differences as a valid and policy-relevant source of identifying variation.[12]

3.3 Visual Illustration of Design

Our analysis leverages pre-shock differences in physician pay across Swedish municipalities as a measure of treatment intensity in a difference-in-differences framework. To clarify our approach, we begin with a visual illustration of the causal pathway in the data.

We first examine whether variation in pre-shock physician base wages influenced post-shock cross-border commuting, serving as the first stage in our difference-in-differences framework. Panel (a) of Figure 5 presents a binned scatter plot of the relationship between changes in doctor commuting (2003–2013) and base physician earnings (2003) at the municipality level. The plot reveals a strong, negative, and nearly linear relationship: municipalities with lower base earnings in 2003 were significantly more likely to lose doctors to Norway after the positive wage shock. This provides clear evidence that financial incentives played a key role in shaping physician mobility.

Next, we assess whether pre-shock physician wages predict post-shock mortality rates, representing the reduced-form effect in our framework. Panel (b) of Figure 5 displays a binned scatter plot showing a negative relationship between base physician earnings and changes in mortality (2003–2013). Municipalities with lower wages in 2003 experienced sharper increases in mortality over the post-shock period, suggesting that physician outflows had measurable consequences for local health outcomes.

Finally, we bring these two pieces of evidence together by directly examining the relationship between physician mobility and mortality. Panel (c) of Figure 5 shows a positive, nearly linear relationship between changes in doctor commuting and changes in mortality rates. In other words, municipalities that lost more doctors to cross-border commuting also experienced relative increases in mortality. This pattern underscores the potential health consequences of physician shortages

[12] Some physicians may have commuted internally across Swedish municipalities prior to 2004. While we cannot observe such patterns in the data, if doctors in low-wage municipalities substituted internal commuting to higher-wage areas with cross-border work after the shock, our estimates would represent lower bounds on both commuting and mortality effects since the decline in physician supply would be smaller than expected in low-wage areas and larger than expected in high-wage areas. However, we find this unlikely: the new cross-border commuters were predominantly young generalists, for whom substantial internal commuting would have already reduced the marginal incentive to shift to Norway. Moreover, in the early 2000s, municipalities moved to restrict the use of temporary physician contracts (see, for example, reports on efforts to curb physician agency contracting during this period: https://www.dagensmedicin.se/alla-nyheter/nyheter/blandade-kanslor-nar-hyrlakare-forsvinner-fran-vardcentralerna/). The decline in temp work for physicians affected both within-municipality temporary contracts (desired by some physicians for their work flexibility) as well as cross-municipality commuting. Most, if not all, of this reduction in temp work was completed before the surge in wages and cross-border commuting in Norway began in 2004.

driven by these wage differentials.

To show preliminary support of a causal interpretation of the descriptive patterns, Figure 5 provides a placebo analysis of changes in mortality rates between 1994 and 2003 against base doctor wage earnings in 2003. Unlike the post-shock period, this pre-shock analysis reveals no evidence of a relationship between base wages and mortality.

3.4 Technical Implementation of Design

Our analysis employs a dose-response difference-in-differences framework, where we compare changes in outcomes across municipalities over time based on their average physician wage in 2003—the year before the Norwegian labor demand shock. This approach allows us to identify relative effects by comparing municipalities that were more or less exposed to the shock based on their initial wage levels.

The technical implementation of our approach consists of three steps. First, we calculate the average physician pay across all municipalities in Sweden in the year before the Norwegian labor demand shock began pulling Swedish doctors into the country. Second, we drop Swedish municipalities located on the border with Norway. This is motivated by the fact that these areas experienced substantial changes in response to the Norwegian labor demand shock across multiple dimensions, including a large amount of out-commuting among other occupations, a deteriorating business climate, and out-migration from the border municipalities to other areas in Sweden (Dodini, Løken and Willén, 2022). This makes it difficult to isolate the unique effect of the increase in the commuting behavior of physicians. The other areas of Sweden away from the border, however, saw no such changes (Dodini, Løken and Willén, 2022). Third, we exclude university hospital regions, as they follow fundamentally different growth trends across observable characteristics.[13]

After having constructed our sample, we estimate the following event study model:

$$Y_{mt} = \alpha + \sum_{t=2001}^{t=2013} [\delta_t(BasePay_m)] + \gamma_m + \rho_t + \varepsilon_{mt}, \tag{1}$$

where Y_{mt} represents an outcome of municipality m at time t. $BasePay$ is a continuous variable taking the value of the log average physician pay in the municipality in 2003 - the year before the Norwegian labor demand shock. For ease of interpretation, we estimate the models interacting with $-1 \times BasePay$ so that the coefficients capture the effect of *increasing* out-commuting and a greater incentive to commute because lower $BasePay$ values correspond to a larger increase in commuting. The δ_t coefficients trace out any pre-treatment relative trends (for δ_{2001} through δ_{2003}) as well as any time-varying treatment effects (for δ_{2004} through δ_{2013}). We omit δ_{2003} such that all coefficients are relative to the year prior to the onset of the shock. Standard errors are clustered at

[13] Specifically, when we include these municipalities, we observe similar effects in terms of commuting responses and mortality (Table A4). However, these municipalities were on a higher underlying population growth trajectory and are unique in Sweden given their large size relative to other municipalities.

the municipality level.

In terms of fixed effects, all specifications include year (ρ_t) and municipality (γ_m) fixed effects. The time fixed effects eliminate any macroeconomic shocks that affect all municipalities in the same year from biasing the results. The municipality fixed effects absorb any systematic differences across municipalities that are constant over time. Our coefficients, therefore, measure relative changes within municipalities over time.

How should we interpret the coefficients? The effects on downstream outcomes like mortality reflect more than just a reduction in physician headcount. When physicians commute abroad, the consequences can include disruptions to continuity of care, reduced access to timely and appropriate treatment, higher hospital costs, increased pressure on remaining staff, and broader institutional strain. These system-level effects may be particularly acute in contexts where generalist physicians play a critical role in triage, first-contact care, and emergency response. We examine these mechanisms directly in the sections that follow, but emphasize up front that any observed mortality effects capture the full set of healthcare system disruptions triggered by physician commuting—not just the reduction in personnel.

To parsimoniously summarize the large set of coefficients obtained through Equation 1, we also present results from a simplified difference-in-differences framework:

$$Y_{mt} = \alpha + \beta_1 Treat_m + \beta_2 (Treat_m \times Post_t) + \gamma_m + \rho_t + \varepsilon_{mt}, \tag{2}$$

where $Post_t$ is a dummy variable equal to one for observations in 2004 through 2013—the years following the onset of the Norwegian labor demand shock.

As discussed in Section 3.2, causal identification from Equations 1 and 2 requires that outcomes in treated and control municipalities would have trended similarly absent the shock (the common trends assumption). This assumption is important because the estimation framework leverages the evolution of the outcomes in the control group to infer what would have happened in the treatment group without the shock. Additionally, identification requires no other contemporaneous policies or shocks coinciding with the Norwegian boom that occurred in the Swedish municipalities with high pre-shock physician wages relative to Swedish municipalities with low pre-shock physician wages.

The results from Equation 1 provide strong support in favor of the first assumption. The δ_t coefficients trace out any pre-treatment relative trends (for δ_{2001} through δ_{2003}), allowing us to study to what extent trends in Swedish border municipalities prior to the boom matched those in the control municipalities. Complementing our register data with aggregate municipality mortality data from 1994 onward allows us to extend the pre-trend analysis and ensure that there are no differential trends in the ten years leading up to the shock with respect to our key outcome. Since treatment is continuous, the difference-in-differences design also requires parallel trends across levels of the exposure variable. In Appendix Table A5, we show that pre-trends in mortality from

1994 to 2003 are flat and statistically indistinguishable across wage quintiles (relative to quintile 3 as the omitted group). In Section 5.6, we provide additional support for this assumption using a rich set of robustness checks, including placebo tests, permutation inference, and alternative specifications.

In addition to estimating the effects on Swedish municipalities, we also investigate potential consequences in Norway. To assess the effects in Norway, we use the main pre-shock predictor of Swedish commuters' destinations in Norway—the presence of a major hospital in the municipality. We then examine whether municipalities with major hospitals and the larger inflows of Swedish doctors in those areas experienced different mortality outcomes than municipalities without major hospitals. We also carefully examine pre-trends to verify that these municipalities had similar trends in mortality prior to the shock. Finally, we look into mechanisms by studying different measures of labor supply of Norwegian physicians, including physicians per capita, sickness absence, and retention rates.

4 Brain Drain

In this section, we demonstrate a substantial commuting response among doctors as a function of the pre-shock average physician wage in their municipality of residence. We also show that these wages are not associated with factors that could confound our results, ensuring that the observed effects are attributable to the loss of doctors rather than other municipality-specific shocks or trends.

4.1 Commuting Response

We begin by establishing the commuting response at the individual level. Panel A of Table 1 presents a difference-in-differences specification estimating the probability of commuting to Norway before and after the shock for physicians and, separately, for nurses. The results show a large and statistically significant increase in commuting among physicians: the probability of commuting rises by 6.9 percentage points on a 2003 baseline of just 4.4 percent—more than a 150 percent increase. In contrast, we find no commuting response among nurses, despite their similar affiliation with the healthcare sector. This sharp and occupation-specific adjustment supports the interpretation that the Norwegian wage boom triggered a selective labor supply response among physicians.

To study the broader implications of this response—particularly for population health—we aggregate to the municipality level. Downstream outcomes such as mortality, hospitalization, and readmission are measured at the municipal level, so our treatment variable must reflect variation in commuting at the same level of aggregation. Panels (a) and (b) of Figure 6 plot the proportion of physicians commuting to Norway by municipality as a function of baseline physician wages, using Equation 1. Panel (a) presents unweighted results; Panel (b) weights by baseline municipal population to account for heterogeneity in municipality size.

Three observations are worth noting. First, before 2004, commuting patterns among doctors showed no significant differences between municipalities with higher and lower average physician base wages. This suggests that the trends were similar across these groups prior to the shock, lending credibility to the common trends assumption. Second, starting in 2004, a clear divergence emerges, which becomes more pronounced in the following years. Specifically, doctors who stood to gain the most from commuting based on the wage gap between their municipality of residence and opportunities in Norway began commuting more frequently as the external opportunities in Norway became increasingly attractive. This pattern underscores the role of economic incentives in driving commuting behavior following the shock. Third, four years after the shock began, the effect appears to have stabilized, with a coefficient estimate of between 0.05 (unweighted) and 0.08 (weighted), and a p-value of less than 0.01. This represents a sizable effect: on average, lower-wage municipalities experienced a 5–8 percentage point increase in the share of doctors commuting to Norway. To benchmark the scale, this is equivalent to partially removing the domestic labor supply of one full cohort of newly educated physicians—or nearly two full cohorts of generalist physicians trained in Sweden—across the affected municipalities. In practical terms, it is as if an entire new generation of physicians sharply reduced their domestic availability through intensive-margin adjustments, placing sudden pressure on the local health system.

In addition to presenting the time-varying treatment effects based on Equation (1), we also provide municipality-level results from the simplified difference-in-difference specification using Equation (2). These coefficients represent the weighted average treatment effect across all post-treatment years (2004–2013) and offer a more straightforward interpretation of the effect magnitudes and their relative sizes across different outcomes.

In Column (1) of Panel B of Table 1, we present the average post-treatment commuting effect. Consistent with the event study findings, the result shows a significant commuting response: a 100 log-point reduction in the base physician wage in 2003 generates a 3.4 percentage-point increase in the share of commuting doctors. This represents more than a doubling of the commuting rate compared to the pre-treatment mean (measured in 2003). Note that one log-point in our setting is approximately 5,000 SEK (about USD 800), which is very close to 1 percent of the average physician wage in 2003.

In Column (2) of Panel B, we present the average post-treatment effect on the total number of doctors in the municipality. The result reveals no significant change on this dimension, suggesting that doctors substitute some of their time working in Sweden with work in Norwegian hospitals, but that they do not leave their positions in Sweden entirely. In other words, the shock generates an intensive-margin effect on labor supply among Swedish doctors, but little extensive-margin effect on physicians living in more exposed municipalities. Another possible explanation for the lack of a significant effect in Column (2) is that commuting doctors are replaced by other incoming doctors (from other regions, from unemployment, or from abroad). However, as shown in Table A6, our estimates of doctor inflow (including moves from other Swedish municipalities and new trainees) and the number of immigrant doctors reveal no significant effects on these margins either. This is important because the loss of physicians' human capital on the intensive margin would, presumably, need to be met with an increase in supply to make up the difference. On the extensive margin, new physicians are not arriving in the more intensely exposed municipalities. Therefore, there is essentially no measurable supply response, either through reallocation across space, recruiting from abroad, or in expanding the training of new physicians.[14]

Table 1, Panel B, Columns (3)–(8), examine whether the treatment generated other changes in the municipalities that could bias the interpretation of our findings. The results reveal no significant effects on other demographic characteristics of the municipalities. There are no changes in the number or commuting propensity of other types of healthcare workers or nurses. Additionally, neither the total population nor the elderly population (65+) shows differences based on the 2003 base physician wage. These findings suggest that other municipality-level confounders are uncorrelated with baseline physician wages, supporting the exclusion restriction.

Did Swedish municipalities respond to the increased competition for their physicians? In Table A7, we show that average physician wages rose in response to the outflow, closing approximately 20–25 percent of the cross-country wage gap. While this is a sizable adjustment, it is not sufficient to prevent a continued outflow of doctors. Crucially, this wage response leads to a substantial increase in the total cost of employing physicians in affected regions (column 2). This matters for two reasons. First, it reinforces that the consequences of the shock go beyond headcount: even where physician availability is maintained, it comes at significantly higher cost. Second, rising wages require municipalities to reallocate already limited health budgets by diverting resources from other areas, increasing financial strain, and potentially worsening care quality. This broader cost-based disruption is essential for interpreting the mortality effects we observe: the deterioration in outcomes reflects not only the loss of physician time but also the ripple effects of strained health system capacity.

5 Mortality Effects

This section establishes a clear relationship between lower pre-shock average physician wages in a municipality and higher post-shock increases in mortality, an effect concentrated among individuals aged 55 and older. In Section 7.2, we show that this pattern is driven by deaths from respiratory, infectious, and circulatory diseases—conditions typically requiring immediate attention—highlighting the critical role of timely emergency care.

[14]Additionally, we observe no sizable increase in the number of new doctors graduating from medical school during this time period, suggesting that the loss of physicians is not compensated through an increased supply of newly trained doctors (going from 0.09 per 1,000 people in 2001 to 0.10 per 1,000 people in 2013). Limits on medical school funding and capacity—set by the national government—along with frequent concerns raised by the Swedish Medical Association about training expansions, may play a role in the inability to quickly expand physician training.

5.1 Mortality Effects

Panels (c) and (d) of Figure 6 illustrate the relationship between the mortality rate and the average physician base wage in a municipality for a given year, based on the event study specification outlined in Equation 1. For ease of interpretation, we use $-1 \times BasePay$ as the treatment intensity variable so that the estimates can be interpreted as the effect of an *increase* in physician outcommuting. To strengthen the validity of our approach, we extend the event study back to 1994, a full decade before the shock occurred. While cross-border commuting data are only available from 2001 onward, historical mortality data at the municipality level allow us to examine trends long before the shock. This extended timeline provides a stronger foundation for verifying the required parallel trends assumption, confirming that municipalities with higher and lower average physician wages followed similar mortality trajectories before the increase in physician commuting.[15] Panel (a) shows results using unweighted data in which each municipality represents a single observation, while Panel (b) shows results in which the estimates have been weighted by the underlying baseline population in the municipality (to account for size differences across municipalities).

We see no observable differences in mortality between municipalities with higher and lower average physician base wages prior to 2004. However, a clear divergence emerges starting in 2004, with significantly higher mortality rates in municipalities with more doctors commuting across the border. This divergence underscores the mortality impact of the shock to physicians' labor supply to local healthcare institutions.

To better understand the nature of the mortality response, we disaggregate outcomes by cause of death. We separate conditions requiring urgent care—respiratory, infectious diseases, and circulatory conditions (RIC)—from all other causes. The results are shown in Figure A5. The effects on RIC mortality begin immediately in 2004 and scale up over time. In contrast, we observe no changes in mortality from other causes. These patterns are consistent with the idea that physician commuting strains emergency and acute care capacity, leading to worse outcomes in conditions where timely intervention is critical. The fact that other causes remain flat rules out broader shifts in health trends, composition, or reporting. Note that we also estimate the effects for each RIC category separately (Appendix Figure A6). While the timing, precision, and magnitude vary somewhat across conditions, we find evidence of mortality effects for all three.

Column (1) of Table 2 presents the average treatment effect using the simplified difference-in-difference specification (Equation 2). The results indicate that a 100 log-point decrease in the base physician wage in 2003 is associated with an increase in mortality of 0.8 per 1,000 inhabitants, equivalent to 7 percent of the mean mortality rate. As shown in Table 2, Columns (2)–(5), this effect

[15] As another test of the parallel trends assumption, we estimate our model while replacing the 2003 *level* of the average physician wage (the "Treat" interaction) with the 2001-2003 *change* in the average physician wage in the municipality. This specification in Table A8 reveals if differential trends in physician wages before 2003 predict commuting behavior after 2003. The result is a tightly estimated zero, meaning different trends in doctor wages do not predict later commuting behavior and cannot, therefore, explain our results.

is driven almost entirely by the elderly population aged 65 and above, with no significant impact observed for individuals under 55. This pattern highlights how the shock disproportionately affects the most vulnerable populations, reinforcing the critical importance of local healthcare access in mitigating adverse health outcomes.

5.2 Inequality Effects

Sections 4.1 and 5.1 show that lower pre-shock physician wages led to greater doctor outflows and, in turn, higher mortality in affected regions, exacerbating regional health disparities. Building on this, we examine the three distinct types of inequality generated by the shock—across countries, municipalities, and individuals—through counterfactual exercises to assess the costs and implications of mitigating these effects.

5.3 Across Country Inequality

The primary dimension of health inequality generated by this shock is the disparity between Sweden and its neighboring country, Norway. To quantify the magnitude of this impact, we conduct a series of counterfactual exercises in Figure 7 that estimate how mortality rates in Sweden would have evolved if physician wages had kept pace with those in Norway.

Panel (a) examines the scenario where Swedish physician wages in 2004 matched those in Norway in the same year. This counterfactual isolates the effect of the initial wage gap under the assumption that wages for Swedish physicians did not lag behind their Norwegian counterparts. Our findings suggest that, in the absence of this baseline wage gap, mortality rates in Sweden would have been approximately 0.4 deaths per 1,000 lower by 2013. The cost of this policy, as calculated by the total increase in wages required for this to come into play and fixing the number of physicians in 2004, is $3.1 billion.[16] This exercise highlights the significant role of the initial wage differential in shaping mortality outcomes, with a reduction in the wage gap translating to tangible improvements in public health.

Panel (b) explores the scenario in which the wage gap between Sweden and Norway remains constant at its 2004 level, but Swedish wages grow at the same rate as Norwegian wages from 2004 to 2013. This scenario evaluates the impact of wage growth disparities over time. Our results show that if Swedish physician wages had grown in tandem with those in Norway, mortality rates in Sweden would have been approximately 0.7 deaths per 1,000 lower by 2013. The cost of this policy, as calculated by the total increase in wages required for this to come into play and fixing the number of physicians in 2004, is $2.7 billion.[17] This suggests that the failure to match wage growth contributed not only to the outflow of skilled physicians but also to higher mortality rates,

[16] 154000 (gap) * 86 (average number of physicians in analysis municipalities) * 235 (number of municipalities in analysis) = 3.1 billion SEK = 311 million USD. 10-year total: $3.1 billion.

[17] We multiply the amount required to keep the SWE-NOR physician wage gap at the 2004 level for each year by 86 (the average number of physicians per municipality) and 235 (the number of municipalities). This yields annual costs in billion SEK of 1.1, 1.6, 2.1, 2.7, 4.0, 4.0, 3.8, 4.2, and 3.6. The total over nine years is 27.1 billion SEK, corresponding to approximately $2.7 billion.

underscoring the broader health consequences of unequal wage trajectories.

Finally, Panel C presents a counterfactual scenario in which Swedish physician wages close the baseline gap with Norway and also keep pace with the wage growth observed in Norway after the wage shock. In this scenario, total mortality rates in Sweden would have been approximately 1 death per 1,000 lower by 2013. The cost of this policy, as calculated by the total increase in wages required for this to come into play and fixing the number of physicians in 2004, is $5.9 billion.[18] This exercise underscores the cumulative impact of both addressing the initial wage gap and aligning wage growth between the two countries, emphasizing the long-term health benefits of more equitable labor market conditions.

Using our estimates of the average age at death to calculate the effects on life expectancy over the population, the three counterfactual scenarios above would have resulted in increases in life expectancy relative to the observed outcomes of approximately 0.5, 0.6, and 1.1 years in these municipalities by the end of 2013.

The valuation of a statistical life year (VSLY) is a widely accepted metric for translating improvements in health or longevity into monetary terms. For this analysis, we adopt a conservative estimate of USD 35,000, consistent with EU-level guidelines.[19] We calculate per-person benefits by multiplying the VSLY by estimated life expectancy losses and then aggregate these across the 3.5 million residents in our sample areas, yielding total social gains of $61, $73, and $134 billion over ten years.[20] Comparing these figures to the estimated policy costs implies a cost-benefit ratio of roughly 1:23, meaning each dollar spent yields approximately $23 in social value.

5.4 Inequality Across Municipalities

The second dimension of health inequality generated by the shock is across municipalities within Sweden. We consider two forms of inequality across space: (1) places with low baseline levels of physician density that are more vulnerable to shortages if physicians leave; and (2) areas with relatively low physician wages.

Places with different doctor densities. Municipalities with lower baseline levels of physician density may have been more vulnerable to shortages and worsening care once physicians began commuting to Norway in larger numbers. This dynamic might exacerbate inequality across places within Sweden by drawing away human capital resources from places in which that human capital has the highest marginal returns. We test this in Table A9 by estimating Equation 2 with an added interaction for whether the municipality was below the sample median of baseline (2003) physicians per capita. Despite having the exact same physician commuting response to the shock,

[18]Following the same procedure as above, this yields annual costs in billion SEK of 3.2, 4.3, 4.8, 5.3, 5.8, 7.1, 7.1, 7.0, 7.6, and 6.8. The total over ten years is 58 billion SEK, or approximately $5.9 billion.

[19]This provides a lower-bound estimate of the economic cost of reduced life expectancy. For comparison, the VSLY in the US is USD 369,000.

[20]We multiply the VSLY by the three alternative estimates of life expectancy loss—0.5, 0.6, and 1.1 years—yielding per-person values of $17,500, $21,000, and $38,500.

the effects of that commuting response on mortality in municipalities with lower physician density are between 5 and 8 percent larger than in areas with higher baseline physician density. This dynamic increases inequality in longevity across Swedish municipalities by increasing the relative deprivation of areas already struggling to retain physicians.

Places with different doctor salaries. Prior to the commuting shock, municipalities with lower physician wages (Quartile 1) exhibited lower mortality rates than those with higher wages (Quartile 4). Specifically, pre-shock mortality was 11.7 per 1,000 in Q1 municipalities, compared to 13.0 per 1,000 in Q4, creating a mortality gap of 1.3 per 1,000. However, following the shock to physician wages in Norway, the larger wage differential in Q1 municipalities led to stronger commuting pressures on Swedish physicians. This induced a significant outflow of healthcare workers from these areas, exacerbating the shortage of medical personnel in Q1 municipalities and driving up mortality rates.

To quantify the effect, we leverage the relationship between wages and mortality from our difference-in-differences regression (Equation 2). A one-unit decrease in log wages is associated with a 0.774 per 1,000 increase in post-shock mortality. Given a baseline log wage difference between Q1 and Q4 municipalities of 0.50, we predict that mortality in Q1 rose by $0.774 \times 0.50 = 0.387$ per 1,000 relative to Q4, representing a significant reduction relative to the baseline gap of 1.3. However, this convergence does not signal an improvement in health outcomes. The narrowing gap reflects increased mortality in lower-wage municipalities rather than improvements in higher-wage municipalities. In this sense, the shock resulted in regressive inequality, where the health outcomes in higher-wage areas remained stable, but those in lower-wage areas deteriorated.

5.5 Inequality Across People

The third dimension of inequality is across individuals. As prior research shows, there is a strong socioeconomic gradient in health (e.g., Case, Lubotsky and Paxson (2002); Currie and Schwandt (2016)). Regional disparities in commuting may, therefore, shape inequality between localities and exacerbate individual-level disparities in healthcare access and outcomes within regions. We have already shown that the negative health implications load on the older and more vulnerable populations. Here, we examine the mortality effects across different education and income levels.

Table A10 presents the mortality effects by key demographic groups: education (more or less than a high school degree) and income (above or below the mean annual income). The estimates reveal heterogeneous effects of physician out-commuting across these groups, indicating that the shock's impact on mortality was not uniform. The table shows that low-income individuals experienced a significant increase in over-65 mortality, with an increase of 4.9 per 1,000 following the shock. In contrast, high-income individuals saw no significant change in mortality (65+), with a negligible change of -0.2 per 1,000.

Low-income individuals were more likely than more advantaged groups to suffer negative health impacts from the shock. The higher mortality rate in this group likely reflects their greater vulnerability, as low-income individuals are typically at higher risk for adverse health outcomes and have more limited access to healthcare resources. High-income individuals, on the other hand, are more likely to have better access to healthcare, including private care options, which may have mitigated the adverse effects of the shock.

For education, the low-education group experienced an increase in over 65 mortality of 3.1 per 1,000, significant at the 5% level. However, the difference between the two groups in terms of the mortality response is not significant. This implies that low-education individuals might have been more affected by the shock, but this evidence is merely suggestive.

Our results align with the broader literature on socioeconomic gradients in health, which demonstrates that disadvantaged groups often experience a disproportionate burden from economic and policy shocks. In particular, the increase in mortality for lower-income individuals underscores the importance of addressing inequality within regions to avoid exacerbating existing health disparities.

5.6 Robustness

In this section, we conduct a series of tests to further assess the validity of the identification strategy and rule out concerns about unobserved confounders or spurious correlations.

Placebo Tests for Commuting Behavior. A potential concern is that our treatment measure–pre-shock physician wages–may proxy for broader labor market conditions rather than physician-specific incentives. To test this, Table A11 examines whether pre-shock wages in non-healthcare occupations predict physician commuting. Column (2) shows they do not. The remaining columns include physician, non-healthcare, and non-doctor healthcare wages in a single specification, confirming that only physician wages significantly predict physician commuting to Norway.

As a complementary test, Table A12 assesses whether physician wages predict commuting in other occupation groups. We find no such relationship for non-physician workers, nurses, or other healthcare workers. This supports our identification strategy: commuting responses as a function of pre-shock physician wages are uniquely concentrated among physicians. These findings also align with the spatial patterns in Figures 2 and 3.

We conduct additional tests to rule out alternative explanations via other placebo tests. First, Appendix Table A13 examines whether baseline physician wages predict commuting responses among other workers. We estimate commuting changes by 1-digit occupation and 1-digit industry and find no systematic patterns, suggesting that broader labor market re-sorting is unlikely to confound our results or drive general equilibrium effects. Second, Appendix Table A8 presents a placebo test using wage trends: we replace the 2003 physician wage level with the change in physician wages from 2001 to 2003 and find no evidence that pre-trends in physician pay predict mortality after the shock.

We also rule out a variety of other factors correlated with baseline demographic, industry, and occupation composition, as well as baseline income levels across municipalities in Table A14. In this table, we add interactions between baseline composition values and year dummies to control for any differential time trends across these values. All of our estimates remain robust despite this high level of saturation, and none of the estimates is statistically different from our baseline estimate.

Placebo Test for Mortality Effects in July. An ideal placebo test for the mortality effects is a setting where physician commuting does not reduce Sweden's domestic physician supply. July provides such a case. Under Sweden's Annual Leave Act (Semesterlagen, 1977:480), most employees—including physicians—take three to four consecutive weeks of vacation, typically in July. Many commuting physicians use this time to work temporarily in Norway as sommarvikar (summer temps), rather than reducing their Swedish hours. Since these absences are planned and accounted for in staffing, July commuting should not generate additional strain on the Swedish healthcare system.

Mortality increasing in July suggests that our main results might be driven by broader health trends correlated with our treatment measure. Conversely, a null result would strengthen the interpretation that mortality only increases when commuting reduces physician availability. Re-estimating our model using July mortality yields a coefficient of -0.016 (SE = 0.298, p = 0.955), a precisely estimated zero. This reinforces the causal interpretation: physician outflows raise mortality only when they generate true supply-side disruptions.[21]

Permutation Test for Treatment Assignment. We conduct a permutation test to assess whether our treatment assignment could spuriously generate the observed effects. If the treatment intensity measure were simply capturing random variation in mortality or commuting, similar results should emerge under random reassignment.

To test this, we randomly shuffle baseline physician wages across municipalities and re-estimate our model 300 times using these permuted values. This generates a placebo distribution of effect estimates under the null of random assignment. Figure A7 shows the results: Panel (a) for physician commuting and Panel (b) for mortality. In both cases, our main estimates lie in the extreme tail of the distribution, with a p-value of 0.02, indicating that the observed effects are highly unlikely to arise by chance. This strengthens confidence in our treatment strategy and supports a causal interpretation.

Sensitivity to Sample Selection and Treatment Definitions. The following robustness tests examine whether the results are sensitive to alternative sample selection criteria and treatment definitions. Table A4 presents estimates using different specifications.

First, we assess whether including university hospital regions and border municipalities affects the results. The main analysis excludes these regions because they differ systematically from other municipalities in ways that could confound the estimates. University hospital regions are much

[21] We find no corresponding mortality effect in Norway in July.

larger, have distinct population growth trends, and feature highly specialized healthcare facilities with different patient referral patterns, making them less comparable to other municipalities. Border municipalities, in contrast, are more exposed to cross-border economic activity and labor market fluctuations that could independently influence both physician mobility and health outcomes. Prior work has also shown that the labor demand shock in Norway had substantial effects on these municipalities across multiple dimensions, complicating the interpretation of the treatment effects. For completeness, Table A4 reports estimates when including these regions. The estimated coefficients remain similar, suggesting that the main findings are not driven by their exclusion.

Second, we test whether extreme observations influence the results by trimming the top and bottom percentiles of the mortality distribution. This adjustment removes potential outliers and ensures that the findings are not driven by extreme values. The results, presented in Panel C, remain consistent with the main analysis.

Third, we examine whether the results hold when replacing the continuous treatment variable with a binary indicator. Panel D defines municipalities with below-median baseline physician wages as treated, providing a simpler treatment assignment mechanism. The results are similar to those in the main specification, though naturally smaller in magnitude, indicating that the findings are not sensitive to the choice of treatment definition.

Fourth, we refine this binary treatment approach using the synthetic difference-in-differences method developed by Arkhangelsky et al. (2021), which extends the logic of the synthetic control approach (Abadie, Diamond and Hainmueller, 2010) to a difference-in-differences setting. This method constructs a synthetic weighted average of pre-treatment outcomes from the donor pool of control units to match the pre-trends of treated units. The results, presented in Panel E, are even stronger than those in Panel D, providing further evidence that the observed mortality effects are not sensitive to the choice of control group.

Finally, we examine if baseline physician wages predict subsequent population growth to ensure that the treatment intensity variable is not proxying for broader regional economic conditions or any differential influx of immigrants during the expansion of the European Union. Table A4, column (2), shows no relationship between pre-shock physician wages and later population trends. Furthermore, Table A15 also shows that areas with lower baseline physician wages did not experience any differential changes to their immigrant makeup, either in levels or in the share of the foreign-born population.

Potential Confounders. The final set of robustness tests examines sensitivity to potential confounders. Table A16 presents estimates controlling for possible effects of the swine flu and local economic conditions on mortality.

The swine flu (A(H1N1) virus) outbreak reached Sweden in late 2009 and persisted into 2010. In that year, the virus caused a total of 35 deaths nationwide, making it unlikely to serve as a direct confounder in our mortality estimates.[22] Nevertheless, it may have had indirect effects on other causes of death by placing additional strain on the healthcare system, through increased hospitalizations and the diversion of resources to support a widespread vaccination campaign. To account for this, we include county-level swine flu cases interacted with the post-reform indicator in our analysis. As shown in Table A16, our mortality estimates remain robust when controlling for swine flu incidence.

The financial crisis of 2009–2010 occurred during our post-treatment period and may have affected municipalities differentially through variation in industry exposure to the recession or through reductions in local government revenues used to fund public healthcare. To account for possible industry-specific exposure to the Financial Crisis, we construct a Bartik shift-share measure of local industry exposure in which we interact baseline employment shares for each industry in each municipality in 2004 with the total shift in industry employment nationally. Our results are robust to this control (Table A16), implying that differential exposure to industry-specific shocks from the Great Recession do not affect our conclusions.

To control for any local government revenue effects of the recession, we also control for time-varying revenue and revenue-cost measures at the municipality level. As shown in Table A16, our estimated effects on mortality remain very similar to our baseline results, suggesting that differential economic hardships via lower government revenues associated with the financial crisis that hit municipalities differently as a function of the 2003 physician wage in the region are unlikely to be driving our findings.

6 Effects in Norway

We turn to the Norwegian side to examine if the inflow of commuting physicians aligned with local physician demand and if it affected patient outcomes or the retention of Norwegian physicians. Do receiving regions benefit from inflows of skilled labor to the same extent that sending regions may be harmed? Understanding this asymmetry is central to evaluating the broader welfare consequences of cross-border professional mobility.

At the beginning of and throughout our sample period, Norway had among the highest numbers of physicians per capita in the OECD. However, considering the aging population and a wave of expected physician retirements, various forecasters projected and raised concerns about potential shortages.[23] Attracting Swedish commuting physicians was, in part, a response to concerns about these projected shortfalls.

Were commuters from Sweden allocated to municipalities with signs of potential physician

[22]See, for example, https://www.folkhalsomyndigheten.se/contentassets/1d7096c2b65d45b499c924d76333272c/influenza-in-sweden-2009-2010.pdf, (Accessed July 8, 2025).

[23]For example, the Nordic Medical Association projected that Norway would need a physician growth rate of over 2.7% per year to keep up with projected demand, or else they would face a shortfall of 400 to 1,100 physicians by 2015 and 2020. See https://shorturl.at/6xb8i (Accessed July 1, 2025).

shortages? Figure A8 presents four binned scatter plots showing the relationship between commuter growth from 2005 to 2013 and baseline municipality characteristics. Panel (a) plots 2005 log physician earnings, Panel (b) shows the share of physicians aged 55 and older, and Panel (c) captures baseline physician density. None of these indicators is strongly correlated with commuter inflows. Panel (d) shows no systematic relationship between actual commuter inflows and predicted inflows from a shift-share instrument as in Card (2001), constructed by interacting national commuting growth with the 2005 distribution of commuters across municipalities. These patterns suggest that the allocation of commuting physicians was not strongly shaped by wages, expected retirements, shortages, or past commuter flows.

One factor does, however, predict the inflow of commuter physicians from Sweden: the presence of a major or university hospital in 2004. Table A17 shows that areas with a university hospital received approximately 17 more physician commuters from Sweden per year than areas without a university hospital, while areas with a major (non-university) hospital received 8 more commuters than areas without a major hospital.

Using a simple event study framework in which we interact time dummies with an indicator for whether the municipality had a major or university hospital in 2004, we test whether these municipalities experienced different changes in over-65 mortality and other outcomes in Norway. Figure A9 presents the result of this exercise for mortality and measures of physician labor supply. Panel (a) shows no meaningful change in over-65 mortality in municipalities with major hospitals– the areas with larger commuter inflows. This suggests that the arrival of Swedish physicians did not translate into improvements in population health outcomes.

Why might there be no significant effect of this commuting inflow on mortality outcomes? Panel (b) shows that areas with major or university hospitals were the areas in which physician supply was growing at a faster pace from 2004-2014. Areas with major hospitals had approximately 10 percent more physicians per capita by 2014 than areas without a major hospital. Panel (c) examines whether Norwegian physician separation rates responded to the arrival of commuters and again finds no clear effects, implying that the inflow did not displace incumbent physicians or necessarily improve their retention. Furthermore, in Panel (d), we show that there was no differential change in measures of workplace quality or burnout as captured by sick days taken by physicians in areas with a major hospital.

These findings suggest that commuting physicians were not directed to underserved areas in Norway and had no detectable effects on patient outcomes or physician retention. While Sweden experienced measurable losses, there is little evidence of offsetting gains in Norway, in part because the destination areas were increasing the supply of "home-grown" physicians at the same time commuters were arriving in larger numbers. This asymmetry highlights that when health systems are already relatively well-staffed, the marginal benefit of skilled inflows may be limited, especially if not targeted to areas of high need. Norway, for instance, has about 20% more general practitioners per capita than Sweden, and diminishing returns to physician density may explain the muted effects. This mirrors our Swedish findings, where physician losses had the largest impact in low-density areas.

7 Mechanisms

This section disentangles the mechanisms behind the observed mortality effects in Sweden. We begin by identifying which types of doctors are most likely to commute, shedding light on which healthcare units are most affected by the brain drain. We then analyze cause-of-death patterns to assess whether physician shortages disproportionately impact conditions like cardiovascular disease, infections, or emergencies. Finally, we examine changes in hospitalization rates, visit timing and duration, and surgical outcomes to assess how commuting-driven shortages affect hospital performance and patient care quality.

7.1 Commuter Types

To better understand the mechanisms behind the observed effects, we examine heterogeneity in commuting responses among doctors. We identify which types of physicians are most likely to leave and, consequently, which hospital areas and departments may be most affected by the resulting brain drain, particularly those critical to patient care.

Our analysis is conducted at the individual level, using a modified and disaggregated version of Equation 1, estimated separately for each demographic group. This approach enables us to examine heterogeneity in responses across various doctor characteristics such as age, gender, family situation, and specialization. However, because the estimates are derived from individual-level regressions, the point estimates are not directly comparable to the municipality-specific effects presented in Table 1. This is because the municipality-level estimates are not weighted by the population.[24]

Table A18 summarizes the results of this analysis. Column (1) highlights the overall effect: the share of doctors commuting to Norway increased substantially from 4 percent before 2004 to over 12 percent during the 2005–2014 period. This increase is particularly pronounced among younger doctors, males, and those without children, who appear more responsive to the enhanced incentives for commuting. These groups likely have fewer constraints related to family or long-term career commitments, making them more adaptable to the opportunities presented by the wage differential (similar to Dodini, Løken and Willén (2022); Le Barbanchon, Rathelot and Roulet (2021)).

When examining productivity using pre-shock earnings as a proxy, we find strong evidence of positive selection: higher-paid doctors are significantly more likely to commute. This may initially seem at odds with the fact that municipalities with lower average physician wages experience the largest outflows. But the logic operates on two levels. Across municipalities, those with lower pre-shock wages experience larger outflows because the relative wage gain from commuting to

[24] We can recover the individual effects using population-weighted estimates at the municipality level.

Norway is greatest. Within municipalities, however, it is the higher-paid—and likely more productive—physicians who are most responsive to that opportunity. In other words, we observe positive selection on productivity within municipalities, layered on top of a dose–response gradient across municipalities. This pattern is consistent with models of labor mobility in which high-ability workers respond first to wage differentials, especially when there are frictions in adjusting local supply.

Generalists, rather than specialists, drive the commuting response. Appendix Figure A10 helps explain this pattern. It shows the 2003 wage distributions for generalists and specialists across Swedish municipalities. Specialists earn more on average, and the generalist distribution has a much longer and thicker left tail. This implies that many generalists earned quite low wages relative to specialists at baseline. When outside options improve, as they did following the Norwegian wage shock, it is these lower-paid generalists who face the steepest relative returns to commuting. In addition, in Norway, general practitioners, who typically play an outsized role in allocating care in the Norwegian system, earn approximately 20% more on average than specialists, further helping us explain this observed effect pattern (e.g. 640,000 vs 800,000 NOK in 2003 in our sample).

Many of these generalists are younger physicians working in hospital wards and emergency departments. As frontline providers, they serve as the first point of contact for patients presenting with acute illnesses, including circulatory, respiratory, and infectious diseases. Their departure puts direct pressure on hospitals' ability to deliver timely and continuous care, particularly in emergency settings.

These findings align closely with our cause-of-death results below, which show that the observed mortality increases are driven by conditions often requiring immediate medical attention, such as circulatory, respiratory, and infectious diseases. These conditions are typically managed in emergency departments and require prompt interventions by skilled physicians. The heterogeneity analysis reinforces this mechanism: the doctors most likely to commute are precisely those working in these critical roles. This consistency underscores the broader implications of cross-border commuting on the healthcare system's ability to respond to urgent patient needs, particularly in vulnerable municipalities.

7.2 Cause of Death

A critical question arises from our analysis: what types of illnesses underlie the observed changes in mortality that we identify? Understanding this is important for unpacking the mechanisms driving our findings and for understanding the broader implication of brain drain in the health care sector. To address this question, we leverage detailed cause-of-death data from the death registry and categorize causes into two broad groups. The first group consists of conditions traditionally regarded as urgent and requiring immediate medical intervention: circulatory diseases, respiratory diseases, and infectious diseases. The second group includes all other causes, including chronic illnesses, such as cancer, mental health conditions, endocrine disorders, digestive diseases, musculoskeletal conditions, neurological disorders, accidents, and other miscellaneous causes.

The results are shown in Panel A of Table 3 and demonstrate that the urgent conditions predominantly drive the observed mortality changes. These conditions often require immediate interventions and access to emergency healthcare services, highlighting the critical role of local physician availability in mitigating adverse outcomes. In addition, these are likely to require close observation after treatment to mitigate risks of complications. In contrast, we find no significant effects for non-emergency causes, which typically involve illnesses with longer time horizons such as cancer, mental health conditions, and musculoskeletal disorders. These conditions are generally managed by highly specialized physicians over extended periods, suggesting that the mortality increase is primarily linked to urgent conditions rather than chronic or less time-sensitive illnesses.

Panel B presents the effects for each cause category. This disaggregated analysis confirms that circulatory diseases, respiratory diseases, and infectious diseases are the primary drivers of the mortality increase. However, the disaggregation introduces slightly noisier estimates, particularly for circulatory diseases, due to smaller sample sizes and greater variability across municipalities. Despite this, the overall pattern remains consistent with the aggregate results. The pattern is also clearly visible in the disaggregated event studies, which show sharp post-shock divergences for these causes and no evidence of differential pre-trends (Appendix Figure A6).

7.3 Hospitalization

To further understand the mechanisms behind the mortality effects we find, we analyze how the commuting shock affected hospital utilization. Reduced physician availability may lead to delays in treatment, inadequate management of health conditions, greater reliance on hospital services, and less continuity of care. If so, patients may experience worsening conditions that necessitate hospitalization.[25]

Using the simplified difference-in-differences design, we estimate the effect of the commuting shock on hospitalizations per 1,000 people. Column (1) of Table 4 shows a significant increase in hospitalization rates following the shock: a 1 log-point lower pre-shock physician wage (a 0.01 log change) led to an increase of 0.142 hospitalizations per 1,000 individuals. These findings are consistent with reduced access to skilled generalist physicians at the first point of contact, which increases the risk of misdiagnoses and delays in treatment, ultimately leading to higher hospitalization rates. The positive selection of skilled doctors into commuting likely amplifies these effects. This pattern aligns with the broader framework that physician shortages contribute to systemic healthcare strain. The event study results are shown in Figure A12.

[25]To further examine the operational costs as a function of the physician out commuting, we also digitize net cost information (costs net of revenues) for the 21 aggregated healthcare regions in Sweden. We then plot the change in net cost over the treatment period as a function of the base wage. The result is shown in Figure A11 and illustrates a strong negative correlation. This provides further suggestive evidence of the strong impact of the out-commuting on operational costs and the financial strain on the healthcare system.

7.4 Length of Hospital Stay

A sharp rise in hospitalizations coupled with physician shortages may force hospitals to shorten inpatient stays to manage capacity constraints. If hospitals are unable to accommodate the patients, they may need to discharge individuals sooner than under normal conditions, potentially affecting recovery and increasing the risk of readmission.

Column 2 of Table 4 shows a statistically significant reduction in the average length of hospital stays. This finding suggests that hospitals actively managed capacity by discharging patients earlier, possibly prioritizing acute care cases over extended inpatient monitoring. These adjustments are consistent with a system under strain, where hospitals must balance rising admissions with limited physician availability. These findings highlight how hospitals adapted to workforce constraints by altering discharge patterns, potentially shifting more responsibility to outpatient and follow-up care while forgoing care at critical moments shortly after treatment.

7.5 Time Between Hospitalizations

To further assess the strain on healthcare systems, we examine changes in the time between hospitalizations. A reduction in time between visits implies more frequent admissions, which may indicate worsened health outcomes or inadequate care at earlier stages.

Column (3) of Table 4 shows a significant decline in the time between hospitalization events. This effect indicates that hospitals faced sustained increases in demand. The increase in hospitalization frequency suggests that physician out-commuting contributed to lower-quality care at earlier stages, resulting in more cases requiring follow-up hospitalizations. Together with the hospitalization and length-of-stay findings, this result indicates that physician shortages not only increased overall hospital utilization but also affected patient management within the healthcare system, leading to more frequent readmissions.[26]

7.6 Surgical Procedures

Despite the broad disruptions in hospital admissions, hospitalization length, and readmission frequency, Column (4) of Table 4 shows no significant change in the likelihood of patients undergoing surgical procedures. However, the event study in Figure A12 hints at a short-term increase following the shock. This may reflect shifts in hospital treatment strategies under strain: shorter stays, delayed care, and reduced access to early intervention could raise the need for surgery. However, given that commuting primarily affected generalists rather than surgical specialists (Section 7.1), it is plausible that surgical capacity remained stable. Overall, we interpret the lack of a clear effect as consistent with the pattern of disruption concentrated in diagnostics and inpatient care rather than surgical services.

[26]Appendix Figure A12 provides additional evidence of an increase in the number of hospitalization events per person.

7.7 Benchmarking the Social Value of Physicians

To benchmark the social value of physician labor, we adapt the back-of-the-envelope method from Friedrich and Hackmann (2021), who estimate the marginal product of nurses by linking mortality effects to life-years lost and the monetary value of a statistical life year (VSLY). In their setting, a parental-leave reform in Denmark caused a sharp drop in nurse employment. A 10% staffing decline (1,100 nurses) raised mortality among 100,000 elderly nursing home residents by 1.5 percentage points. Assuming each excess death reflects one lost life-year, and adjusting the standard VSLY of $100,000 by a quality-of-life factor of 0.35 for the frail and institutionalized population, they estimate a welfare loss of $52.5 million. Dividing this by the reduction in staffing yields a social value of $47,700 per nurse per year—roughly double the average nurse salary of $24,000.

We apply this framework to the Swedish side of our setting. A 3.4 percentage point increase in commuting corresponds to 3.74 additional physicians working in Norway, out of an average stock of 110. Since we do not observe hours worked directly, we estimate labor supply effects in full-time equivalents (FTEs) using earnings data. We use the ratio of average income earned by Swedish commuters in Norway to the base wage of a full-time Norwegian physician, yielding an estimate of 0.35 FTEs per commuting physician. This implies a total labor reduction of 1.31 FTEs (3.74×0.35).

Using our estimate of 0.88 excess deaths per 1,000 residents, a municipality of 20,000 would experience 17.6 additional deaths. As in Friedrich and Hackmann (2021), we conservatively treat each death as one lost life-year. Even though the affected population is relatively younger and healthier than that in Friedrich and Hackmann (2021), we apply the 0.35 quality-of-life adjustment to provide a conservative estimate that is easier comparable to their findings. This results in a total welfare loss of $0.62 million. Dividing by the labor reduction yields a marginal product of $0.47 million per physician per year. Compared to an average physician wage of $150,000, this implies a value-to-wage ratio of 3.1.[27]

These estimates imply that physicians are more underpaid relative to their social value than nurses. A value-to-wage ratio of 3.1, compared to 2 for nurses, highlights the pivotal role physicians play in preserving life. That said, this figure may overstate the true marginal product, as it does not account for the potential longer-run effects of care disruptions—possibly inflating the mortality response relative to the labor reduction. In addition, physician expertise likely complements the work of other healthcare providers - more so than nurses - so their absence may reduce the overall effectiveness of the broader care team. Beyond headcount, the loss of physicians also strains continuity of care, raises staffing costs, and forces hospitals to reallocate limited resources—all of which may degrade system performance

[27]Without applying the 0.35 quality-of-life adjustment, our estimates would yield a total welfare loss of $1.76 million, a marginal product per physician per year of $1.34 million, and a value-to-wage ratio of 8.9.

8 Discussion

The acceleration of cross-border worker mobility has reshaped local labor markets, particularly for high-skilled workers. While migration or commuting offers numerous opportunities, it also introduces significant challenges, especially for countries facing high outflows of skilled professionals.

This paper studies the welfare consequences of high-skilled labor mobility by examining how a labor demand shock in Norway triggered a sharp increase in cross-border commuting among Swedish physicians. We show that even temporary, intensive-margin outflows of doctors led to persistent increases in mortality in Sweden—with no measurable gains in Norway. These effects were concentrated in low-capacity health systems, among older individuals, and for acute conditions. We then trace how the shock amplified inequality across countries, municipalities, and individuals. Together, the findings reveal how brain drain in public services can generate large and unequal welfare losses, even in settings with strong institutions.

Our results underscore the need for policymakers to grapple with the increasingly complex consequences of brain drain, particularly in sectors where public services rely on scarce, high-skilled professionals. For sending countries, the challenge lies in preserving the benefits of labor mobility while maintaining the resilience of essential systems. This calls for strategies that mitigate exposure to staffing shocks without curbing flexibility. More broadly, it highlights the importance of integrating the welfare implications of skilled worker movement into economic planning and the long-run design of public systems.

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Tables and Figures

Table 1: Commuting and Other Demographic Variables

Panel A: Individual Commuting Responses

	Physicians	**Nurses**
Post × Treat	0.069***	0.005
	(0.021)	(0.006)
Baseline commuting (2003)	0.044	0.024
Observations	262,333	977,860

Panel B: Municipality Level Commuting Responses and Demographics

	Doctors		Other Healthcare		Nurses		Population	
	Commute	Number	Commute	Number	Commute	Number	Total	65+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treat	0.034**	1.619	-0.003	7.413	-0.001	-4.971	329.38	-12.942
	(0.013)	(9.439)	(0.002)	(24.398)	(0.005)	(7.703)	(213.180)	(15.866)
Mean	0.031	86	0.013	1008	0.032	319	15,250	2,930

Source: Authors' calculations using Swedish registry data from 2001 to 2013. Panel B sample includes 3,060 observations (municipalities × 14 years).
Notes: The "treat" variable is $-1\times$ the log of average physician base pay in the municipality (year 2003). Standard errors in parentheses (clustered at the municipality level). ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table 2: Mortality Effects

	Mortality Rate (per 1,000)				
	Total	Age 65+	Age 55+	Age <35	Age 35-55
	(1)	(2)	(3)	(4)	(5)
Post × Treat	0.774***	3.116**	1.963**	0.064	0.584
	(0.286)	(1.478)	(0.815)	(0.074)	(0.399)
Mean	11.801	51.993	32.376	0.387	4.687

Source: Authors' calculations using Swedish registry data from 2001 to 2013. Sample includes 3,060 observations (municipalities × 14 years).

Notes: The "treat" variable is $-1\times$ the log of average physician base pay in the municipality (year 2003). Standard errors in parentheses (clustered at the municipality level). ***, **, and * indicate significance at the 1%, 5%, and 10% levels

Table 3: Causes of Death

Panel A: Aggregate Categories

	RIC	non-RIC
	(1)	(2)
Post × Treat	2.377**	0.040
	(1.054)	(0.595)
Mean	24.95	14.12

Panel B: Detailed Categories

	Respiratory	Infectious	Circulatory	Cancer	Mental	Endo + Dig	Musc + Nerve	Accident	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Post × Treat	0.949***	0.300*	1.127	0.240	0.019	-0.158	-0.041	0.067	0.207
	(0.302)	(0.155)	(0.894)	(0.439)	(0.356)	(0.267)	(0.229)	(0.178)	(0.297)
Mean	3.01	0.56	21.40	10.21	1.92	2.59	1.32	1.30	2.03

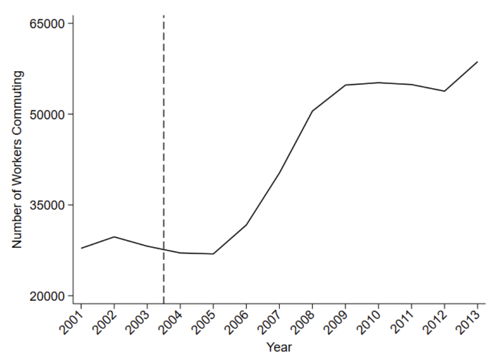
Source: Authors' calculations using Swedish registry data from 2001 to 2013. Sample includes 3,060 observations (municipalities × 14 years).
Notes: RIC aggregates respiratory, infectious, and circulatory causes of death. non-RIC aggregates all other causes. The "treat" variable is $-1\times$ the log of average physician base pay in the municipality (year 2003).
Standard errors in parentheses (clustered at the municipality level). ***, **, and * indicate significance at the 1%, 5%, and 10% levels

Table 4: Mechanisms: Hospitalization Responses

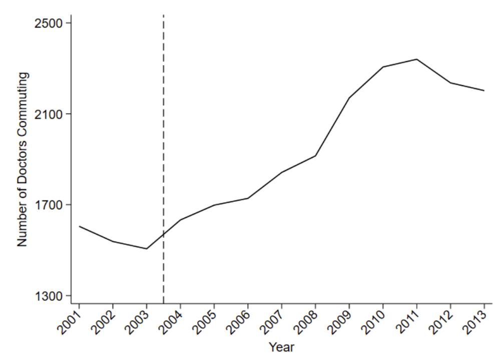
	Hosp.	Hosp. length	Time between	Surgeries
	(1)	(2)	(3)	(4)
Post × Treat	14.183***	-0.429**	-3.036*	0.005
	(5.025)	(0.178)	(1.730)	(0.006)
Mean	203.19	6.532	62.885	0.072
Observations	3,059	3,059	3,059	3,059

Source: Authors' calculations using Swedish registry data from 2001 to 2013. Sample includes 3,060 observations (municipalities × 14 years).
Notes: Hosp: (Number of hospitalizations * 1000 / base pop). Hosp. length: hospitalization spell length (days). Time between: days between hospitalization events (same year). Standard errors in parentheses (clustered at the municipality level). ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

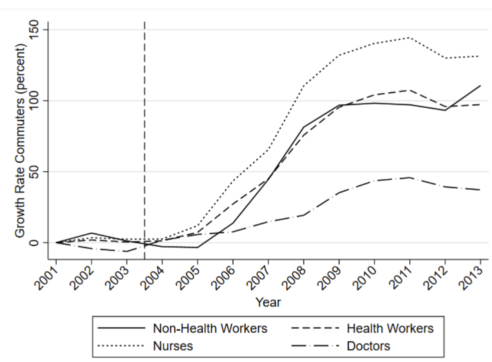
Figure 1: Swedish Commuting Response



(a) All workers



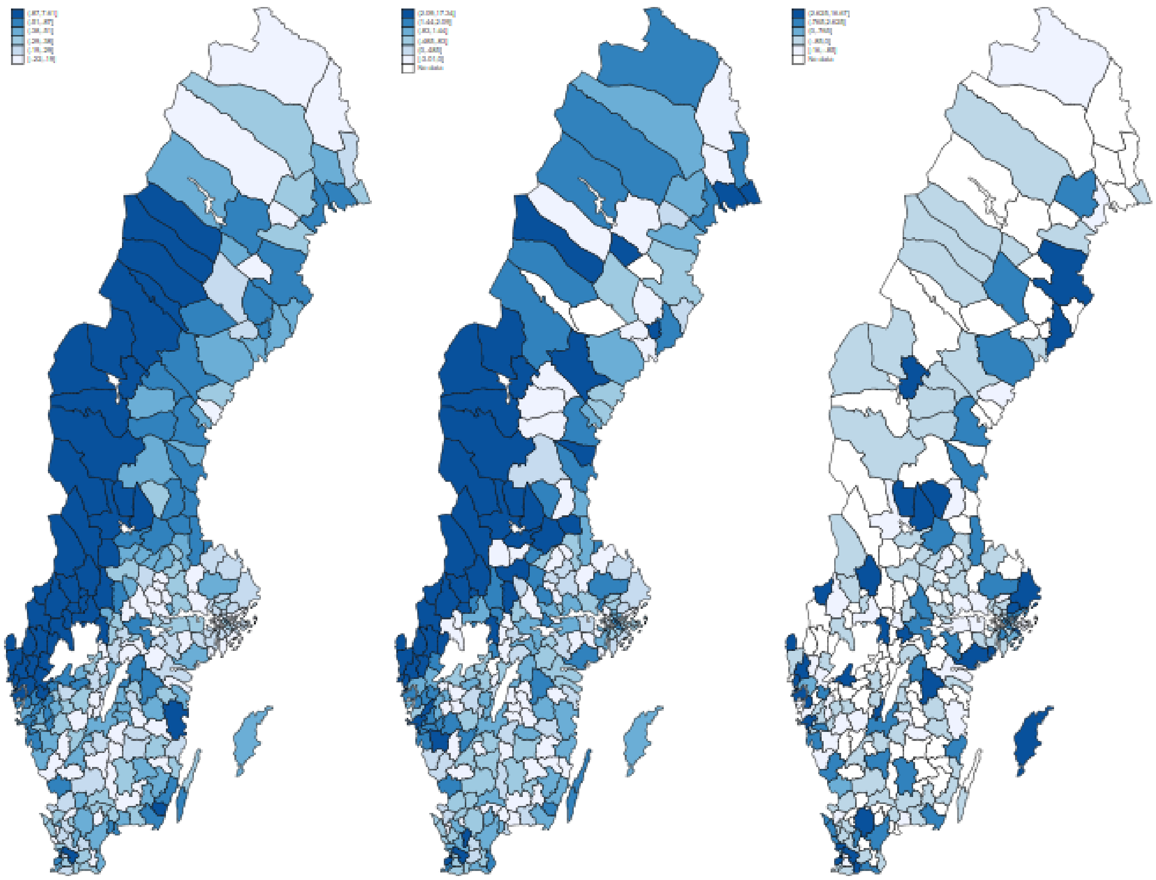
(b) Doctors



(c) Growth rate by sector

Source: Authors' calculations of Swedish register data from 2001 to 2013.
Notes: Panels (a) and (b) show the commuting response for all workers and doctors, respectively. Panel (c) presents the growth rate of commuting across different occupational groups.

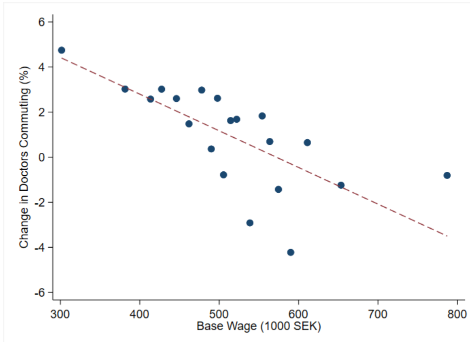
Figure 2: Swedish Commuting Response by Occupation



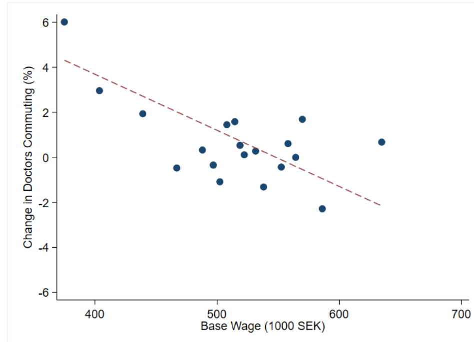
(a) All Workers (b) Nurses (c) Doctors

Source: Authors' calculations of Swedish register data from 2001 to 2013.
Notes: Panels (a), (b), and (c) illustrate the commuting response for all workers, nurses, and doctors, respectively. The geographic nature of the response varies across groups.

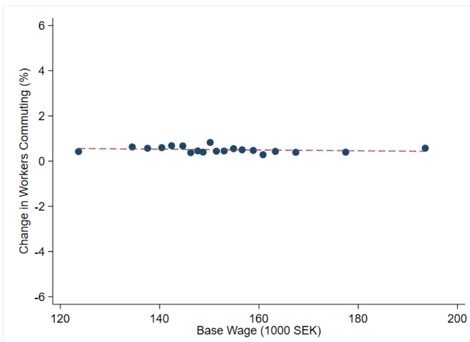
Figure 3: Swedish Commuting Response: Wage and Distance Effects



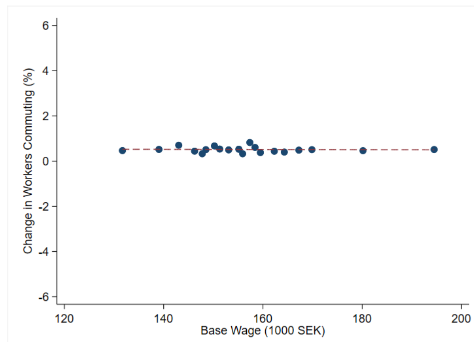
(a) Doctors



(b) Doctors (weighted)



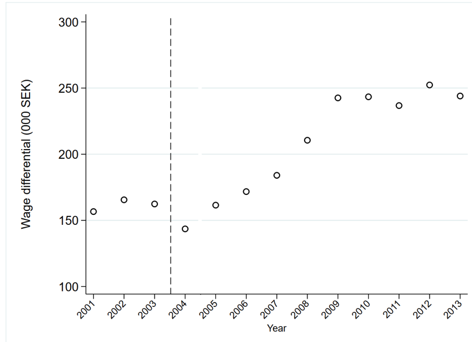
(c) Non-Doctors



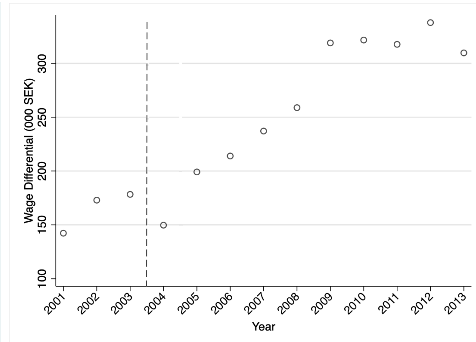
(d) Non-Doctors (weighted)

Source: Authors' calculations of Swedish register data from 2001 to 2013.
Notes: Panels (a) and (b) show the commuting response for doctors with and without frequency weights for the size of the municipality by average baseline wage earnings for doctors. Panels (c) and (d) show the response for non-doctors. Doctors respond to wage returns, while non-doctors do not.

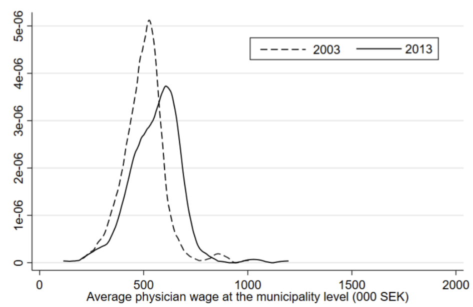
Figure 4: Shock to Labor Market Competition: Occupation Earnings Differential



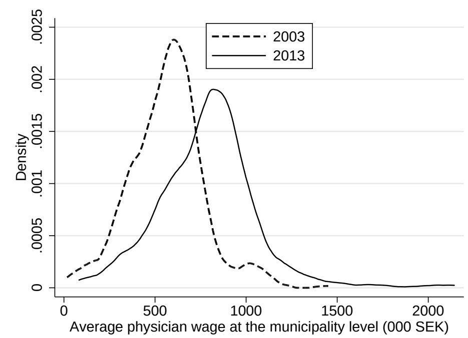
(a) Within-Occupation Earnings Difference, All Occupations



(b) Within-Occupation Earnings Difference, Doctors



(c) Density of Doctor Pay, Sweden, 2003 and 2013

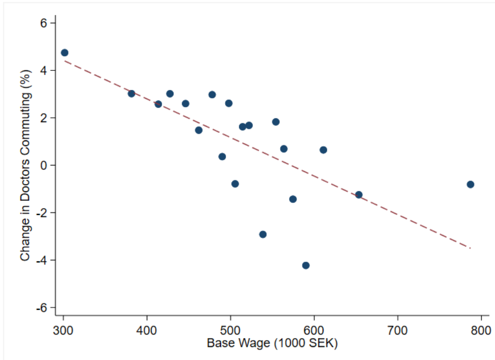


(d) Density of Doctor Pay, Norway, 2003 and 2013

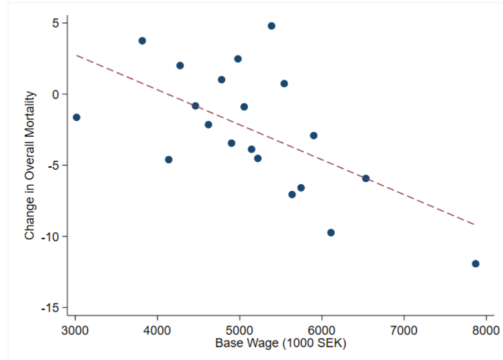
Source: Authors' calculations of Swedish register data from 2001 to 2013.

Notes: The figure shows the occupation earnings differential adjusted for purchasing power, highlighting the impact of labor market competition on wages. Panel (a) is the average across all occupations, while Panel (b) is for doctors only. Panels (c) and (d) show the kernel densities of physician earnings in Sweden and Norway in 2003 and 2013, respectively.

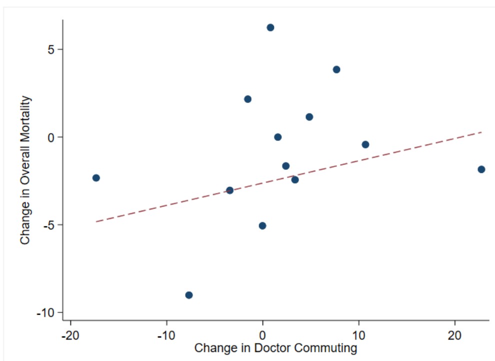
Figure 5: Base Pay, Commuting, Mortality and pre-Mortality



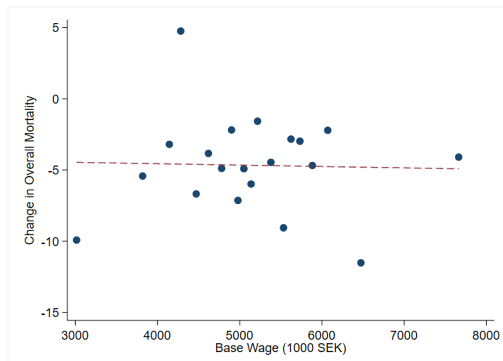
(a) Commuting vs Base Pay



(b) Mortality vs Base Pay



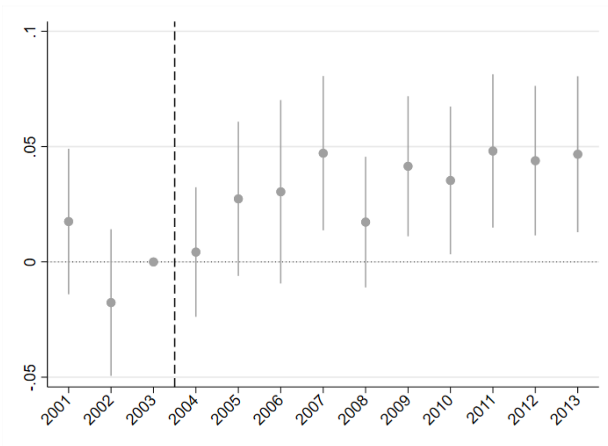
(c) Mortality vs Commuting



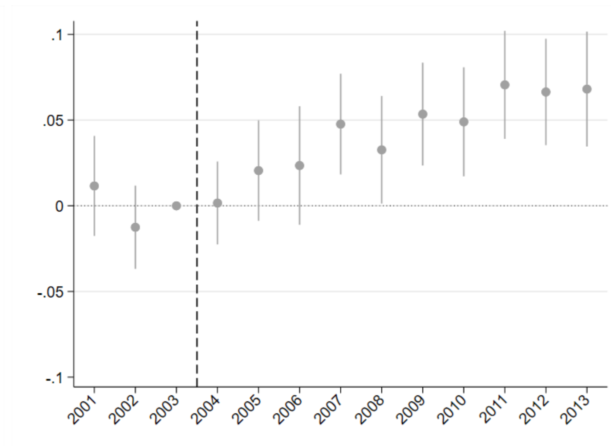
(d) Pre-mortality vs Base Pay (placebo)

Source: Authors' calculations of Swedish register data from 2001 to 2013.
Notes: For Panels (a) and (b): Each data point represents the change in the outcome variable for a municipality between 2003 and 2013, plotted against the base doctor wage in the municipality in 2003. The dotted line represents an OLS regression of the outcome variable on the base doctor wage in 2003, weighted by population size. For Panel (c): Each data point represents the change in mortality rates for a municipality between 2003 and 2013, plotted against the change in doctor commuting over the same period. The dotted line represents an OLS regression of the change in mortality on the change in commuting, weighted by population size. For Panel (d): Each data point represents the change in the outcome variable for a municipality between 1994 and 2003, plotted against the base doctor wage in the municipality in 2003. The dotted line represents an OLS regression of the outcome variable on the base doctor wage in 2003, weighted by population size.

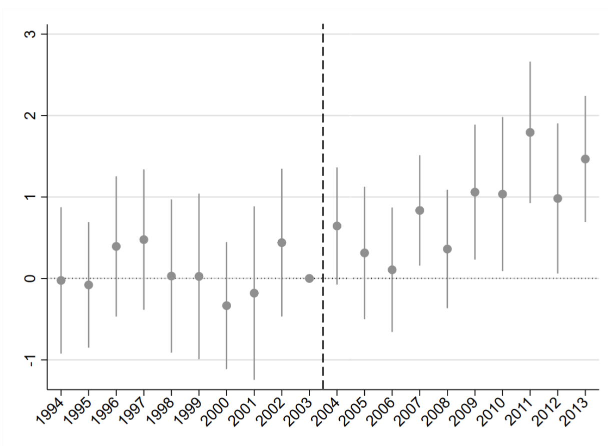
Figure 6: Share of Doctors Commuting and Mortality Rates per 1,000



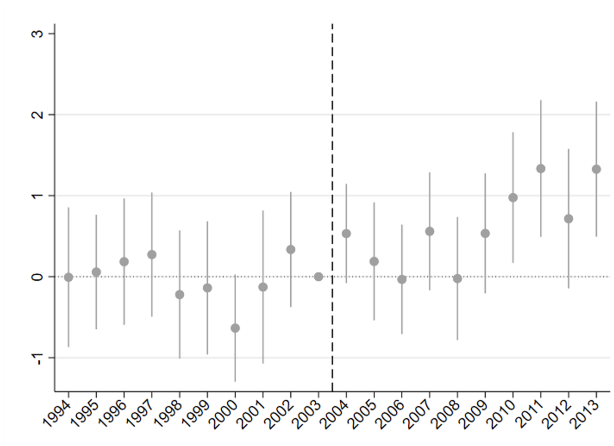
(a) Share doctors commuting - unweighted



(b) Share doctors commuting - weighted



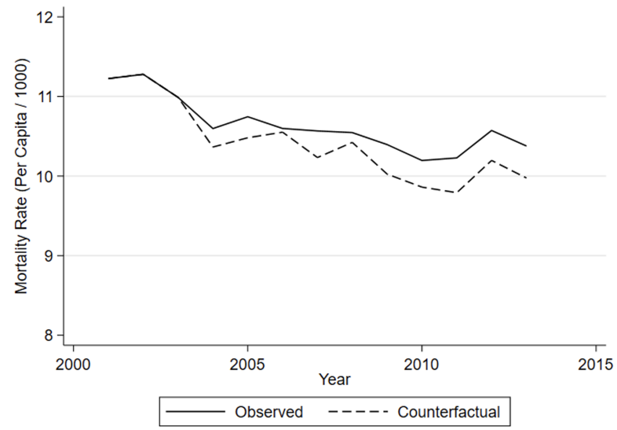
(c) Mortality- unweighted



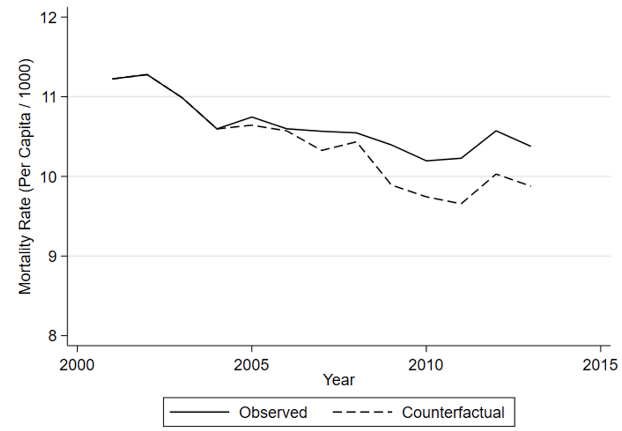
(d) Mortality - weighted

Source: Authors' calculations of Swedish register data from 1994 to 2013.
Notes: Each point represents the coefficients from Equation 1. The "treat" variable is $-1\times$ the log of average physician base pay in the municipality (year 2003). The lines represent 95% confidence intervals, with standard errors clustered at the municipality level. The dashed line is the year physician wages began growing quickly in Norway. The estimates in panels (b) and (d) are weighted by total population in 2003.

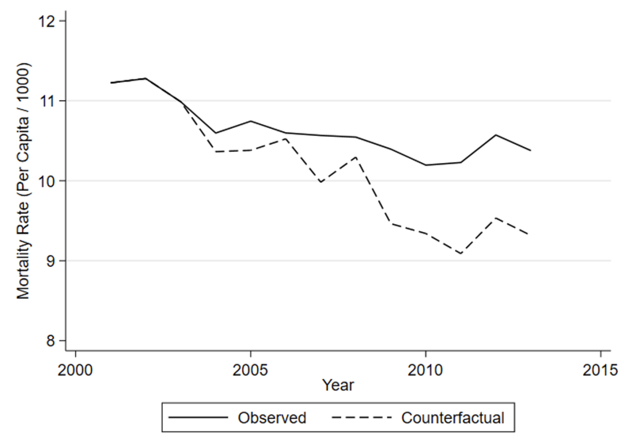
Figure 7: Counterfactual Total Mortality Rates in Sweden



(a) Counterfactual if Swedish Wages Fixed at 2004 Norwegian Levels



(b) Counterfactual if Sweden-Norway Wage Gap Fixed at 2004 Level



(c) Counterfactual if Swedish Wages Equaled Norwegian Wages from 2004-2013

Source: Authors' calculations of Swedish register data from 2001 to 2013.
Notes: We first estimate Equation 1. Then we set the counterfactual earnings in Sweden to different levels and plot the evolution of counterfactual mortality rates given each scenario described in the text.

Online Appendix (not for publication)

Table A1: Correlation Between Base Physician Wage and Observable Characteristics

Panel A: Municipality Characteristics

Population	Population 65+	Deaths 65+ pc	Deaths all pc	Doctors pc	Nurses pc	Revenue	Costs
−0.086	−0.068	0.138	0.239	−0.056	0.213	0.056	−0.063

Panel B: Demographic Characteristics

Age	Unemp.	Child < 18	Female	Immigrant	High education
0.233	0.236	−0.072	−0.101	−0.251	−0.108

Panel C: Occupation Shares

Occ. 1	Occ. 2	Occ. 3	Occ. 4	Occ. 5	Occ. 6	Occ. 7	Occ. 8	Occ. 9
−0.017	−0.069	−0.102	−0.102	0.109	0.009	0.050	0.082	−0.058

Panel D: Industry Shares

Ind. 1	Ind. 2	Ind. 3	Ind. 4	Ind. 5	Ind. 6	Ind. 7	Ind. 8	Ind. 9	Ind. 10
0.096	0.017	−0.096	−0.039	−0.201	−0.114	0.035	0.045	0.215	0.072

Source: Authors' calculations using Swedish registry data in 2003.

Notes: The table shows correlations between the treatment variable (log base physician wage in 2003) and key demographic characteristics in 2003). Occupations are grouped into nine SSYK 1-digit categories (excluding physicians): (1) managers; (2) professionals (ex. physicians); (3) technicians and associate professionals; (4) clerical support workers; (5) service and sales workers; (6) agricultural, forestry, and fishery workers; (7) craft and related trades workers; (8) plant and machine operators and assemblers; (9) elementary occupations. Industries are grouped into ten categories: (1) agriculture, fishing, and mining; (2) manufacturing; (3) utilities (electricity, water, waste); (4) construction; (5) wholesale, retail, hotels, transport, and communication; (6) finance and real estate; (7) public administration; (8) education; (9) health and social work; (10) other services, including households and extraterritorial organizations.

Table A2: Correlation Between Base Physician Wage and Wages in 1-Digit Occupations

Occ. 1	Occ. 2	Occ. 3	Occ. 4	Occ. 5	Occ. 6	Occ. 7	Occ. 8	Occ. 9
-0.155	-0.160	-0.185	-0.118	-0.033	-0.156	-0.205	0.009	-0.100

Source: Authors' calculations using Swedish registry data from 2003.
Notes: The table shows correlations between the treatment variable (log base physician wage in 2003) and wages in other 1-digit occupations also measured in 2003. Occupations are grouped into nine SSYK 1-digit categories (excluding physicians): (1) managers; (2) professionals (ex. physicians); (3) technicians and associate professionals; (4) clerical support workers; (5) service and sales workers; (6) agricultural, forestry, and fishery workers; (7) craft and related trades workers; (8) plant and machine operators and assemblers; (9) elementary occupations.

Table A3: Descriptive Statistics by Group (Averages Across Entire Sample Period)

Panel A: Swedish Commuter and Non-Commuter Characteristics				
Variable	All Commuters	All Non-Commuters	Doctor Commuters	Doctor Non-Commuters
	(1)	(2)	(3)	(4)
Age	34.89 (12.99)	40.39 (14.30)	43.82 (12.61)	43.89 (12.34)
Female	0.38 (0.49)	0.49 (0.50)	0.27 (0.44)	0.49 (0.50)
Married	0.01 (0.11)	0.03 (0.17)	0.04 (0.20)	0.04 (0.19)
Immigrant	0.31 (0.46)	0.19 (0.39)	0.36 (0.48)	0.30 (0.46)
Child Under 18	0.27 (0.44)	0.40 (0.49)	0.38 (0.48)	0.43 (0.49)
Border to Norway	0.15 (0.35)	0.02 (0.15)	0.02 (0.15)	0.01 (0.09)
Total Wage	319,563.6 (329,091.9)	190,318.4 (204,906.7)	736,681.6 (517,222.4)	484,614.3 (355,838.9)
Less than High School	0.09 (0.28)	0.21 (0.40)	0.00 (0.00)	0.00 (0.00)
High School	0.54 (0.50)	0.45 (0.50)	0.00 (0.00)	0.00 (0.03)
More than High School	0.38 (0.48)	0.34 (0.47)	1 (0)	1 (0.03)
Observations (N)	539,532	77,564,767	24,719	605,426

Panel B: Norwegian Municipality Characteristics	Mean	SD
Mortality Rate per 1,000	10.64	3.40
Over 65 Mortality Rate	48.32	11.82
Over 55 Mortality Rate	32.24	8.63
Under 35 Mortality Rate	0.83	0.72
Age 35-55 Mortality Rate	2.38	1.61
Population	17,769	145,767
Average Earnings (NOK)	308,830	69,266
Average Physician Earnings (NOK)	575,735	229,567
Average Nurse Earnings (NOK)	303,389	63,114
Commuting Physician Pct of Workers (2005)	0.11 %	0.56 %

Source: Authors' calculations using Swedish and Norwegian registry data from 2001 to 2013.
Notes: Standard deviations in parentheses in Panel (a).

Table A4: Effect of Norwegian Labor Demand Shock; Sensitivity Checks

	Doctors (Commute)	Population (Total)	Mortality (Total)	Mortality (65+)
	(1)	(2)	(3)	(4)
Panel A: Including University Hospital Regions				
Post × Treat	0.032***	2595.651**	0.941***	4.444***
	(0.012)	(1003.256)	(-0.274)	(-1.433)
Panel B: Including University Hospital Regions, Border				
Post × Treat	0.039***	2461.623***	0.854***	3.998***
	(0.012)	(927.824)	(0.273)	(1.402)
Panel C: Trimming (top and bottom percent)				
Post × Treat	0.039**	267.52	0.906***	3.561**
	(0.016)	(260.232)	(0.306)	(1.607)
Panel D: Binary				
Post × Treat	0.012**	-85.774	0.243*	1.112*
	(0.005)	(156.895)	(0.127)	(0.664)
Panel E: SDID Binary (entire country)				
Post × Treat	0.012***	827.493	0.332***	1.734**
	(0.005)	(589.203)	(0.120)	(0.761)

Source: Authors' calculations using Swedish registry data from 2001 to 2013. Sample includes municipality-level observations across years.
Notes: The baseline "treat" variable is $-1\times$ the log of average physician base pay in the municipality (year 2003) in 2003. This is altered in Panel D and E as specified in the table and the text. Standard errors in parentheses (clustered at the municipality level). ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A5: Pre-trend Assessment Across Different Treatment Levels

Treatment Quintile	Pre-trend Estimate
Quintile 1	-0.047
	(0.030)
Quintile 2	-0.003
	(0.026)
Quintile 4	0.007
	(0.024)
Quintile 5	-0.041
	(0.029)

Source: Authors' calculations using Swedish registry data from 1994 to 2003. Sample includes 2,350 observations (municipalities $\times$ 12 years).
Notes: The model estimates linear trends in the pre-exposure period for different quantiles of the treatment levels. Quantile 3 is the omitted group. Standard errors in parentheses (clustered at the municipality level). ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A6: Effect of Norwegian Labor Demand Shock on New and Immigrant Doctors

	Number of New Doctors	Number of New Immigrant Doctors
	(1)	(2)
Post $\times$ Treat	-0.520	-0.451
	(0.605)	(0.431)
Mean	3.980	2.520
Observations (N)	2824	2824

Source: Authors' calculations using Swedish registry data from 2001 to 2014. Sample includes municipality-level observations across years.
Notes: The "treat" variable is $-1\times$ the log of average physician base pay in the municipality. Standard errors in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Table A7: Effects on Swedish Wages and Wage Bill

	Log(Mean Swedish Wage)	Log(Total Wage Bill)
	(1)	(2)
Post × Treat	0.256***	0.283**
	(0.070)	(0.122)
Mean	13.15	16.38

Source: Authors' calculations using Swedish registry data. Sample includes 3,060 observations (municipalities × 14 years).
Notes: The "treat" variable is $-1\times$ the log of average physician base pay in the municipality (year 2003). Standard errors in parentheses (clustered at the municipality level). ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A8: Placebo: Commuting Effect based on wage trends

	Share of Doctors Commuting	Share of Doctors Commuting
	(1)	(2)
Post × $[Wage_{Doc,2003} - Wage_{Doc,2001}]$	0.0006	
	(0.00004)	
Post × $[\left(\frac{Wage_{Doc,2003} - Wage_{Doc,2001}}{Wage_{Doc,2001}}\right) \times 100]$		-5.28e-07
		(1.86e-06)
Mean	0.031	0.031

Source: Authors' calculations using Swedish registry data from 2001 to 2013. Sample includes 3,060 observations (municipalities × 14 years).
Notes: The "treat" variable is $-1\times$ the change in the average municipality physician wage between 2001 and 2003 as specified in the the table row. Standard errors in parentheses (clustered at the municipality level). ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A9: Mortality Effects by High vs Low Baseline Physician Density

	Mortality Rate (per 1,000)			
	Total (1)	Age 65+ (2)	Age 55+ (3)	Commuting (4)
Post × Treat	0.822***	3.352**	2.095**	0.034**
	(0.300)	(1.567)	(0.856)	(0.013)
Post × Treat × Low Density	0.037***	0.181***	0.101***	-0.000
	(0.009)	(0.047)	(0.027)	(0.000)
Mean	11.801	51.993	32.376	0.031

Source: Authors' calculations using Swedish registry data from 2001 to 2013. Sample includes 3,060 observations (municipalities × 14 years).
Notes: The "treat" variable is $-1\times$ the log of average physician base pay in the municipality (year 2003). "Low density" is defined as having physician density below the sample median in 2003. Standard errors in parentheses (clustered at the municipality level). ***, ** denote significance at the 1% and 5% levels.

Table A10: Mortality Effects by Demographic Groups

	Over 65 Mortality Rate (per 1,000)			
	Low Education	High Education	Low Income	High Income
Panel A: Overall				
Post × Treat	3.099**	2.872	4.918**	-0.203
	(1.446)	(4.224)	(2.500)	(0.698)
Panel B: RIC Conditions				
Post × Treat	2.785**	2.388	4.439**	0.416
	(1.250)	(3.385)	(1.930)	(0.778)
Mean (overall)	46.26	53.54	74.056	10.881
Mean (RIC)	34.13	37.68	50.985	8.956

Source: Authors' calculations using Swedish registry data from 2001 to 2013. Sample includes 3,060 observations (municipalities × 14 years).
Notes: The "treat" variable is $-1\times$ the log of average physician base pay in the municipality (year 2003). Standard errors in parentheses (clustered at the municipality level). ***, **, and * indicate significance at the 1%, 5%, and 10% levels. "RIC conditions" aggregates respiratory, infectious, and circulatory causes of death.

Table A11: Effect of Norwegian Labor Demand Shock - Alternative Treatment Measures

	Share of Doctors Commuting			
	(1)	(2)	(3)	(4)
Post × Treat (Doctor Wage)	0.034**			0.033**
	(0.013)			(0.014)
Post × Treat 2 (Non-Health Wage)		0.002		-0.006
		(0.021)		(0.032)
Post × Treat 3 (Non-Doctor Health Wage)			0.029	0.024
			(0.033)	(0.049)
Mean	0.031	0.031	0.031	0.031

Source: Authors' calculations using Swedish registry data from 2001 to 2013. Sample includes 3,060 observations (municipalities × 14 years).
Notes: The "treat" variable is $-1\times$ the log of average wage in the municipality for the group specified in the table row in 2003. Standard errors in parentheses (clustered at the municipality level). ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A12: Effect of Norwegian Labor Demand Shock on Commuting Shares

	Share of Doctors Commuting	Share of Non-doc Commuting	Share of Non-doc Health Commuting	Share of Nurses Commuting	Share of Other Health Commuting
	(1)	(2)	(3)	(4)	(5)
Post × Treat	0.034**	-0.002	-0.001	-0.001	-0.003
	(0.013)	(0.002)	(0.003)	(0.003)	(0.002)

Source: Authors' calculations using Swedish registry data from 2001 to 2013. Sample includes 3,060 observations (municipalities × 14 years).
Notes: The "treat" variable is $-1\times$ the log of average physician base pay in the municipality (year 2003). Standard errors in parentheses (clustered at the municipality level). ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A13: Commuting Effect in Other Industries and Occupations

Panel A: By Industry	Industry									
	1	2	3	4	5	6	7	8	9	10
Post × Treat	-0.001	-0.001	-0.000	-0.000	-0.001	-0.000	-0.000	0.000	-0.001	-0.001
	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
N	1,000,785	5,938,371	479,704	2,314,891	6,606,451	4,046,303	1,252,713	3,426,978	6,318,672	1,286,222

Panel B: By Occupation	Occupation								
	1	2	3	4	5	6	7	8	9
Post × Treat	-0.000	-0.001	0.000	-0.001	-0.001	-0.004**	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
N	1,875,770	4,477,638	5,752,608	2,989,555	7,484,128	624,440	3,674,465	4,325,593	2,463,648

Notes: Occupations are grouped into nine SSYK 1-digit categories (excluding physicians): (1) managers; (2) professionals (ex. physicians); (3) technicians and associate professionals; (4) clerical support workers; (5) service and sales workers; (6) agricultural, forestry, and fishery workers; (7) craft and related trades workers; (8) plant and machine operators and assemblers; (9) elementary occupations. Industries are grouped into ten categories: (1) agriculture, fishing, and mining; (2) manufacturing; (3) utilities (electricity, water, waste); (4) construction; (5) wholesale, retail, hotels, transport, and communication; (6) finance and real estate; (7) public administration; (8) education; (9) health and social work; (10) other services, including households and extraterritorial organizations. Standard errors in parentheses.

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

Table A14: Robustness to Additional Controls

Specification	Total Mortality (per capita)
Baseline	0.774^{***}
	(0.286)
Baseline + Demographics-by-Year FEs	0.601^{**}
	(0.298)
Baseline + Industry-by-Year FEs	0.613^{***}
	(0.291)
Baseline + Occupation-by-Year FEs	0.606^{**}
	(0.277)

Source: Authors' calculations using Swedish registry data from 2001 to 2013. Sample includes 3,055 observations (municipalities $\times$ 12 years.

Notes: Standard errors in parentheses. ${}^{*}p < 0.10$, ${}^{**}p < 0.05$, ${}^{***}p < 0.01$. Occupation and industry makeup are measured as the baseline shares of employment in each 1-digit occupation or industry. Occupations are grouped into nine SSYK 1-digit categories (excluding physicians): (1) managers; (2) professionals (ex. physicians); (3) technicians and associate professionals; (4) clerical support workers; (5) service and sales workers; (6) agricultural, forestry, and fishery workers; (7) craft and related trades workers; (8) plant and machine operators and assemblers; (9) elementary occupations. Industries are grouped into ten categories: (1) agriculture, fishing, and mining; (2) manufacturing; (3) utilities (electricity, water, waste); (4) construction; (5) wholesale, retail, hotels, transport, and communication; (6) finance and real estate; (7) public administration; (8) education; (9) health and social work; (10) other services, including households and extraterritorial organizations. Demographic shares are defined as baseline shares of the population by gender, share of the population born outside Sweden, the share of the adult population with a Bachelor's degree or more education, and average municipality age.

Table A15: Effects on Immigrant Population

	Number Immigrants	Share Immigrants
	(1)	(2)
Post × Treat	34.591	0.004
	(141.150)	(0.003)
Mean	1871.085	0.069

Source: Authors' calculations using Swedish registry data from 2001 to 2013. Sample includes 3,060 observations (municipalities × 14 years).
Notes: The "treat" variable is $-1\times$ the log of average physician base pay in the municipality (year 2003). Standard errors in parentheses (clustered at the municipality level). ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Table A16: Controlling for Potential Confounders

	Commuting	Total Mortality
Swine Flu	0.034***	0.802***
	(0.013)	(0.287)
Bartik Shift-Share	0.035***	0.700***
	(0.014)	(0.273)
Income	0.043***	0.617**
	(0.016)	(0.302)
Income Net Costs	0.032***	0.831***
	(0.013)	(0.305)

Source: Authors' calculations using Swedish registry data from 2001 to 2013. Sample includes 3,055 observations (municipalities × 12 years).
Notes: Swine flue controls for the number of affected individuals on the county level (overall) interacted with the post indicator. Income controls for time varying revenue at the municipality level. Income Net Costs controls for time varying revenue at the municipality levle minus municipality costs.

Table A17: Changes in Commuting Physicians from 2005-2013 in Norway, by Major or University Hospital Status

	No (1)	Yes (2)	Difference (3)
University Hospital	1.008	18.250	17.24**
	(11.165)	(87.073)	(2.95)
University or Major Hospital	0.366	11.029	10.66***
	(10.019)	(43.106)	(3.69)
Major Hospital	0.788	8.889	8.101*
	(16.199)	(19.180)	(2.47)

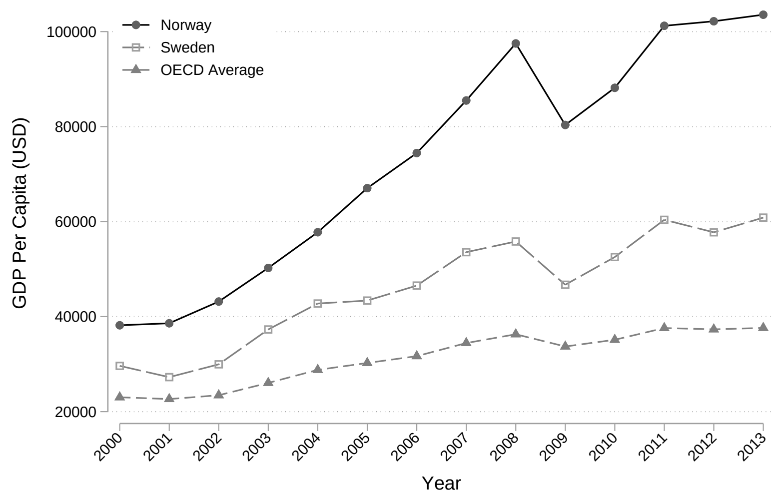
Source: Authors' calculations of Norwegian register data from 2001 to 2013.
Notes: Standard deviations in parentheses in columns (1) through (3). Standard errors of a t-test in parentheses in column (3).

Table A18: Commuting by Group Characteristics

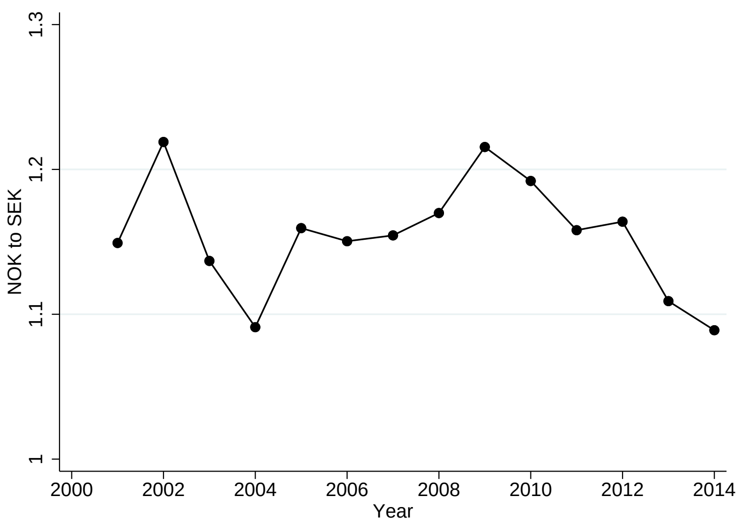
		Age Group			Gender	
	All	Young	Mid-career	Old	Male	Female
Post × Treat	0.081***	0.128***	0.020	0.033**	0.088***	0.054***
	(0.016)	(0.031)	(0.014)	(0.015)	(0.022)	(0.011)
Mean	0.044	0.022	0.061	0.042	0.063	0.016
Observations	615,138	238,726	203,486	156,896	322,508	292,630
	Nativity		Parental Status		Wage Level	
	Immigrants	Natives	Children	No Children	High Pay	Low Pay
Post × Treat	0.080***	0.089***	0.024*	0.109***	0.085***	0.007
	(0.019)	(0.018)	(0.013)	(0.024)	(0.021)	(0.009)
Mean	0.071	0.036	0.049	0.040	0.059	0.023
Observations	184,631	430,507	264,105	351,033	357,554	229,373
	Occupation Type					
	Generalist	Specialist				
Post × Treat	0.112***	0.002				
	(0.021)	(0.015)				
Mean	0.036	0.057				
Observations	414,377	200,761				

Source: Authors' calculations using Swedish registry data from 2001 to 2013. Regressions are estimated at the individual physician level.
Notes: The "treat" variable is $-1\times$ the log of average physician base pay in the municipality (year 2003). Standard errors in parentheses (clustered at the municipality level). ***, **, and * indicate significance at the 1%, 5%, and 10% levels.

Figure A1: Macroeconomic Development Over Time Across Sweden and Norway



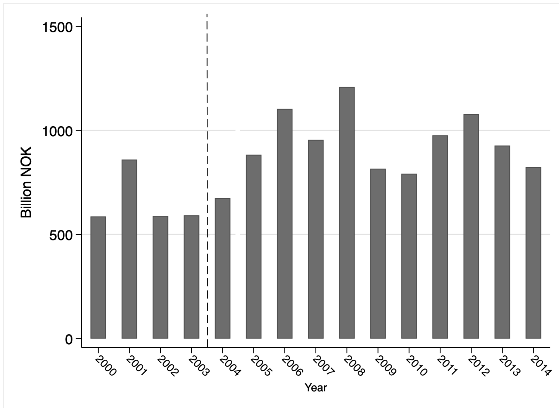
(a) GDP Growth



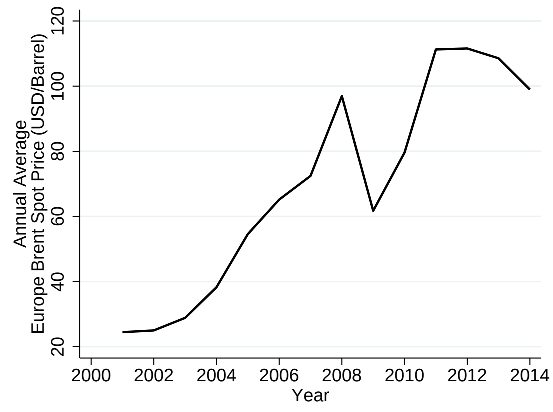
(b) Exchange Rate

Source: Authors' calculations of aggregate OECD data.
Notes: Panel (a) shows the change in real GDP per capita over time. Panel (b) shows the NOK-SEK exchange rate over time.

Figure A2: Norwegian Government Oil Revenue and Oil Prices Over Time



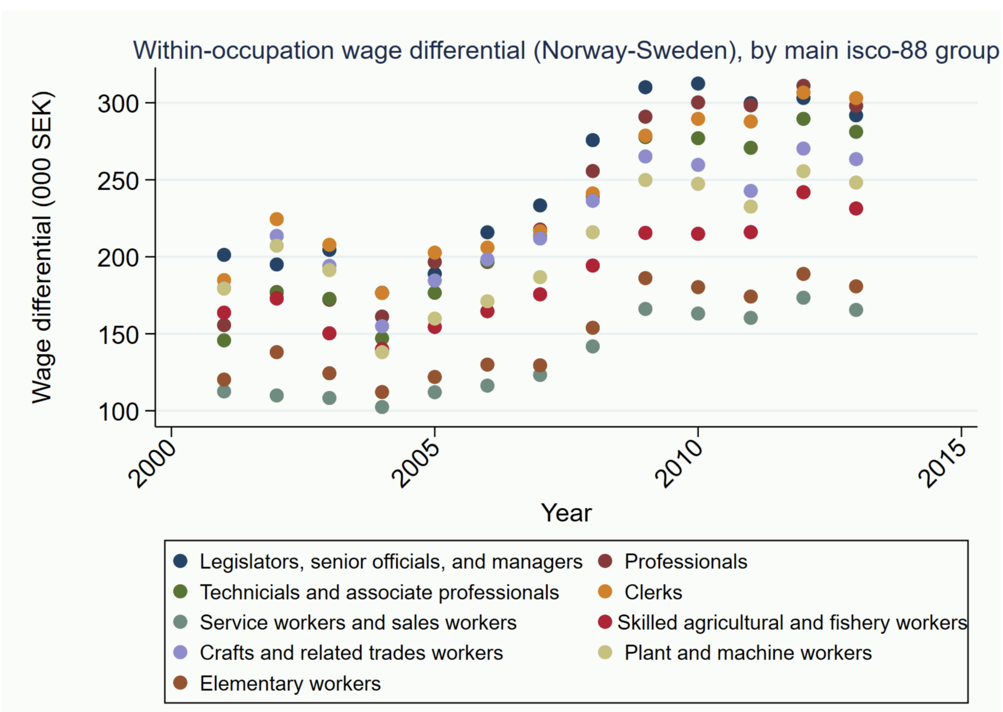
(a) Norwegian Oil Revenue Over Time



(b) Europe Brent Spot Oil Prices

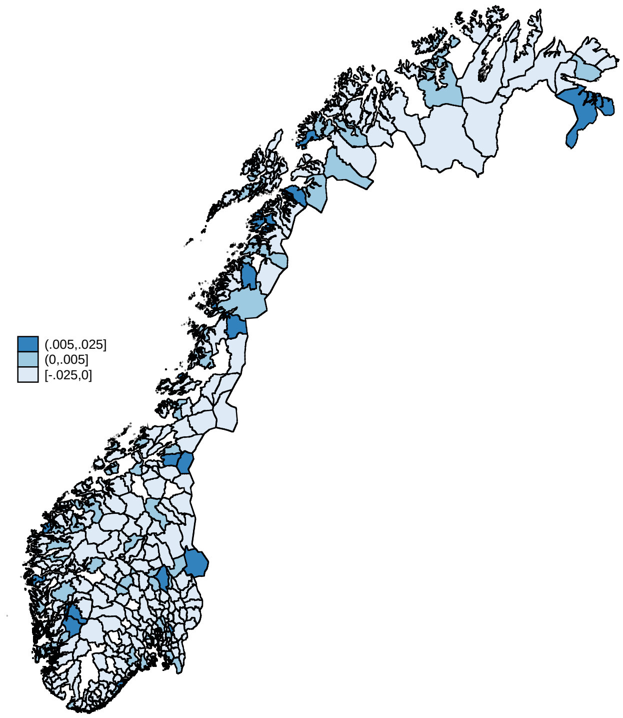
Source: Authors' calculations based on Norwegian government data.
Notes: Panel (a) presents the oil revenue collected by the Norwegian government over time, illustrating trends in resource-based income. Panel (b) shows the price of European Brent over time.

Figure A3: Shock to Labor Market Competition: Occupation Earnings Differential by ISCO88 Group



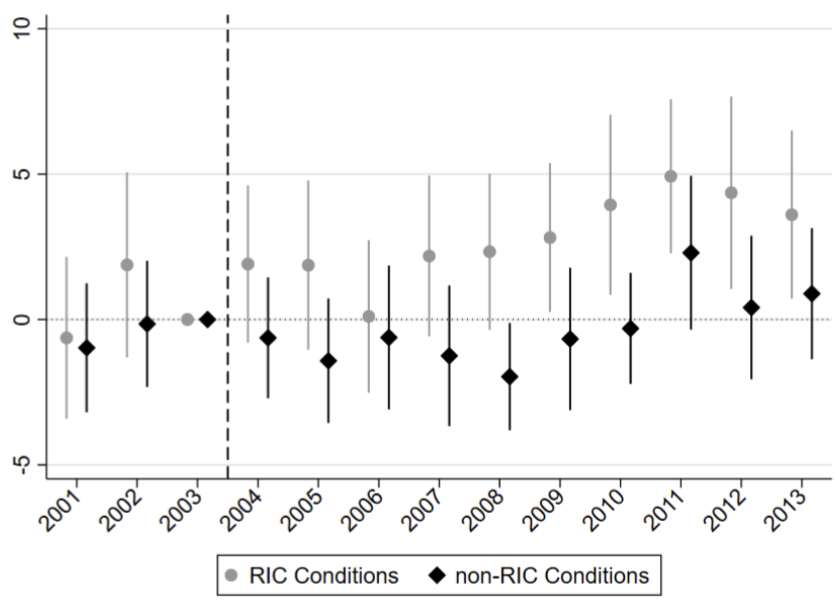
Source: Authors' calculations of Swedish and Norwegian register data from 2001 to 2013.
Notes: The figure presents occupation earnings differentials between Norway and Sweden over time, broken down by ISCO88 occupational groups.

Figure A4: Destination of Commuting Doctors



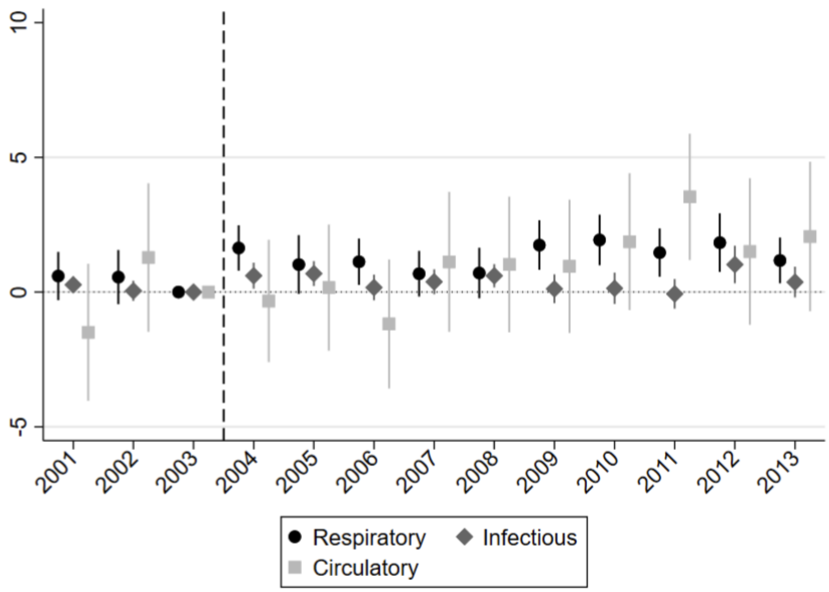
Source: Authors' calculations of Norwegian register data from 2001 to 2013.
Notes: The map shows the change in the number of incoming commuting doctors as a share of total physicians in the municipality.

Figure A5: Disaggregated Mortality Event Studies



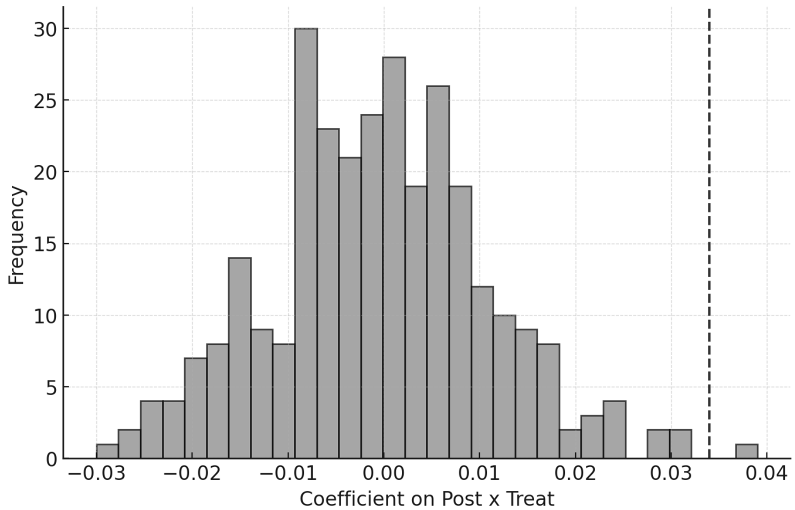
Source: Authors' calculations of administrative data from Statistics Sweden from 2001 to 2013.
Notes: This figure shows the change in mortality for RIC conditions and non-RIC conditions as a function of the physician base wage in 2003. "RIC" conditions are respiratory, infectious disease, and circulatory conditions.

Figure A6: Disaggregated Mortality Event Studies RIC Conditions

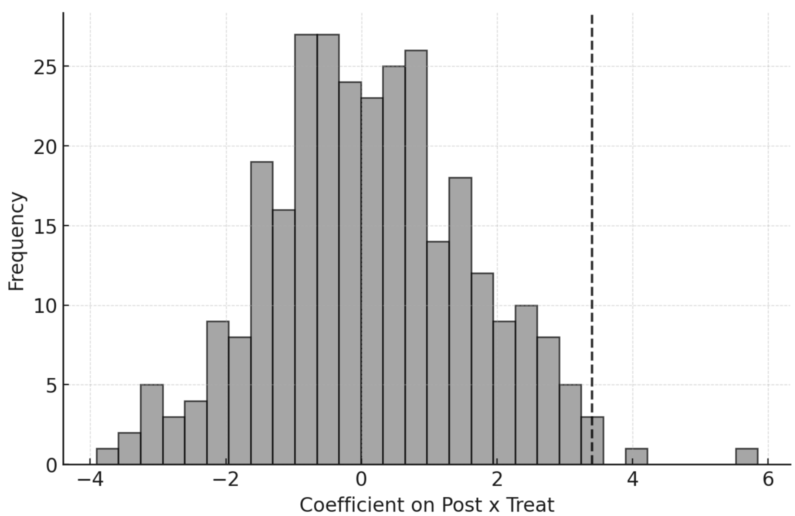


Source: Authors' calculations of administrative data from Statistics Sweden from 2001 to 2013.
Notes: This figure shows the change in mortality for infectious, respiratory, and circulatory conditions as a function of the physician base wage in 2003. "RIC" conditions are respiratory, infectious disease, and circulatory conditions.

Figure A7: Permutation Checks, 300 iterations



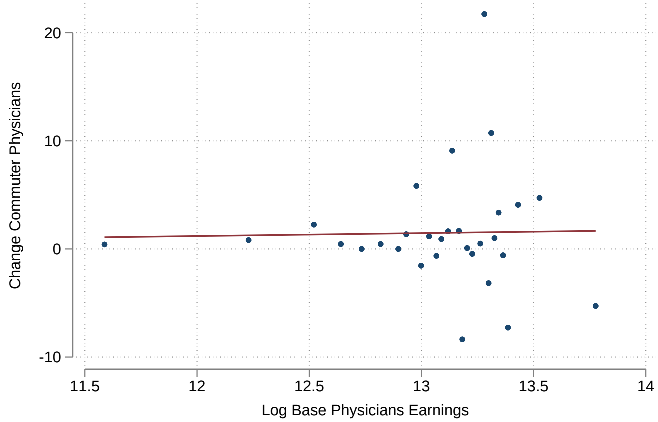
(a) Doctor Commuting



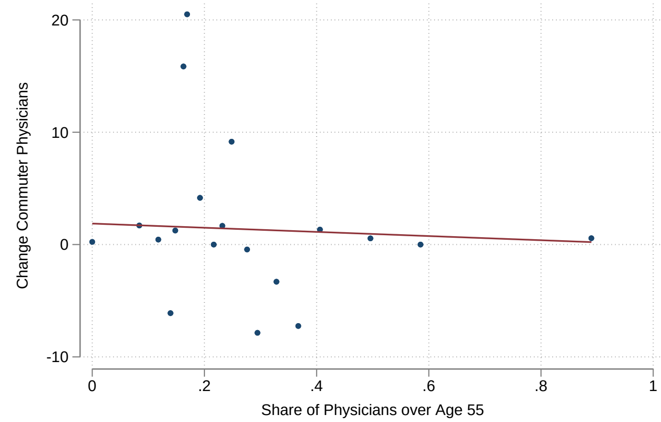
(b) Mortality

Source: Authors' calculations of Swedish registry data from 2001 to 2013.
Notes: These figures show the distribution of the β_2 coefficient from Equation 2 after randomly shuffling the value of the "treat" variable across municipalities. The "treat" variable is $-1\times$ the log of average physician base pay in the municipality (year 2003). In both cases, 2 percent of permutations return values slightly higher than the core estimate, generating a randomization inference p-value of 0.02.

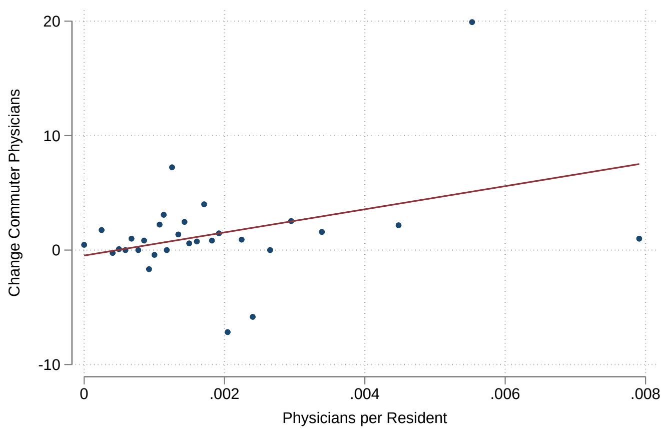
Figure A8: Factors Predicting Commuter Destination in Norway



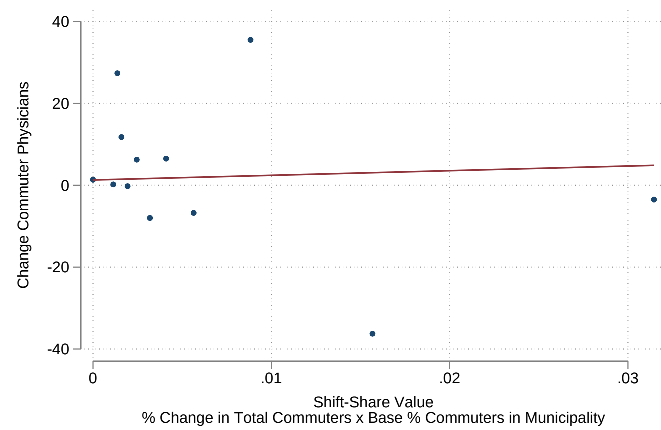
(a) Baseline Physician Log Wage



(b) Baseline Share of Physicians Over Age 55



(c) Baseline Physician Density

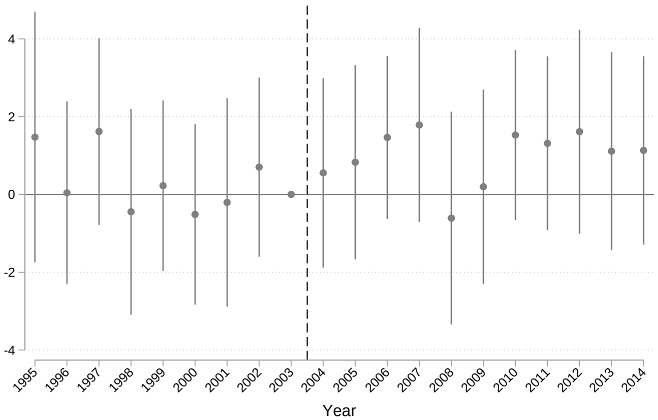


(d) Shift-Share Instrument

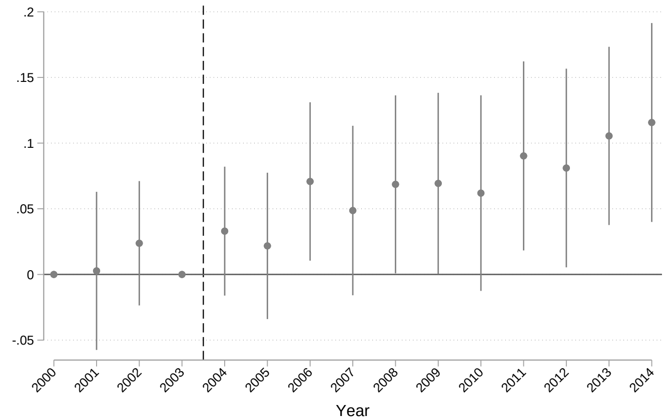
Source: Authors' calculations of Norwegian registry data from 2001 to 2013.

Notes: The shift-share instrument in Panel (d) is the predicted number of physician commuters based on interacting the baseline share of total commuters in municipality X by the total percent increase in commuters from 2005 to 2013 in the whole country.

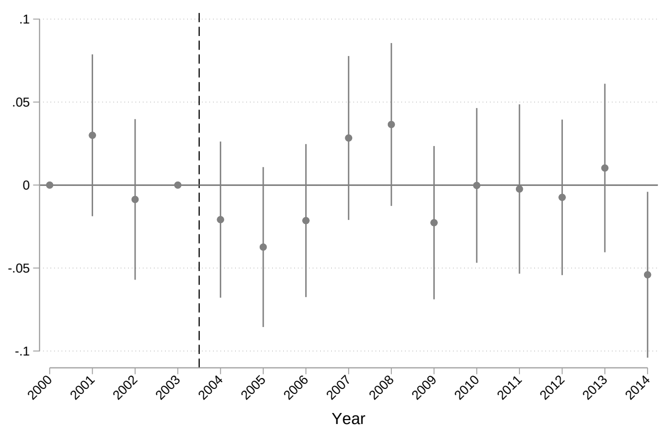
Figure A9: Effects in Norway, Mortality and Physician Labor Supply, Major or University Hospital



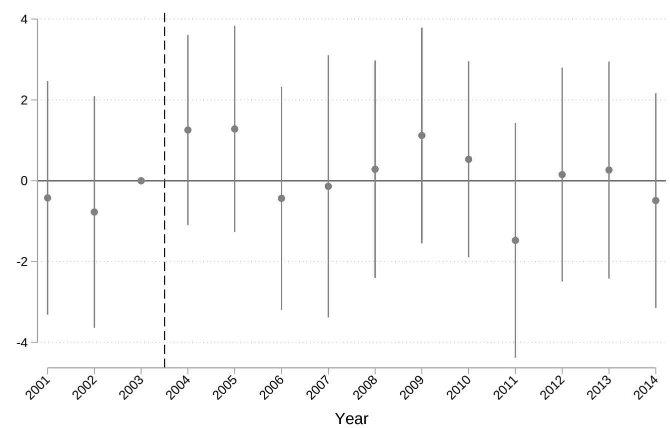
(a) Over 65 Mortality per 1,000



(b) Log Norwegian Physicians per Capita



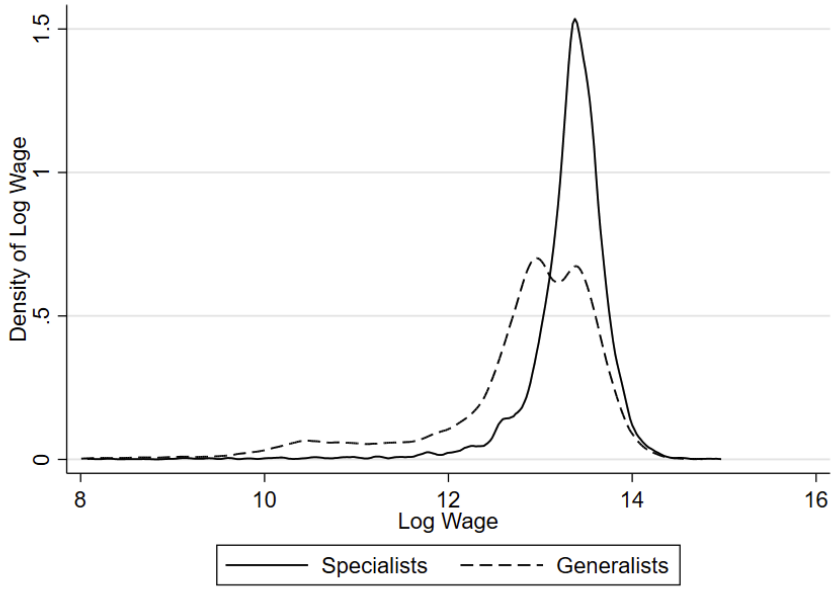
(c) Norwegian Physician Separation Rates



(d) Average Sick Days Taken by Doctors

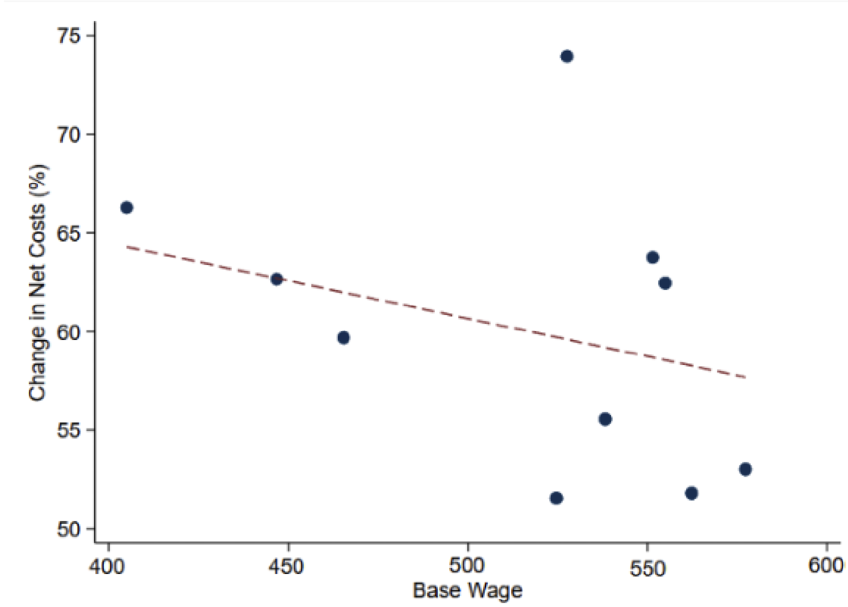
Source: Authors' calculations of Norwegian registry data from 2001 to 2013.
Notes: Each panel estimates the interaction between year dummies and the presence of a major or university hospital in the municipality, which is the strongest predictor of the destination of Swedish commuting physicians to the municipality. 95% confidence intervals are based on clustering at the municipality level.

Figure A10: Wages of Specialists and Generalists



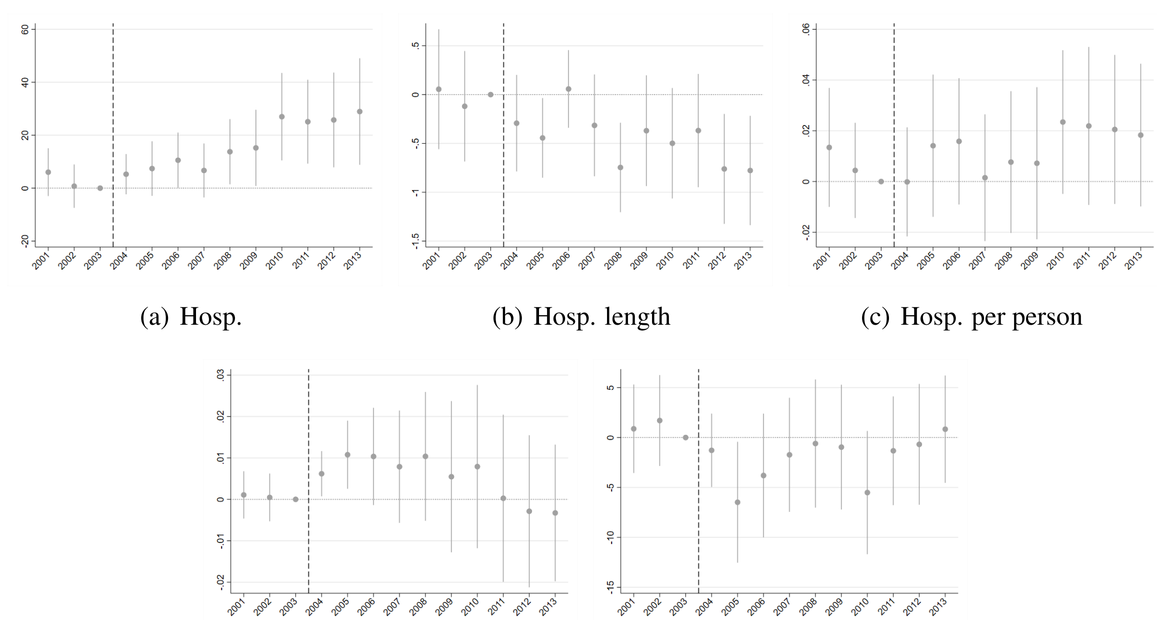
Source: Authors' calculations of administrative data from Statistics Sweden in 2003
Notes: This figure shows the log wage of specialists and generalists in 2003.

Figure A11: Net Cost at the Health Region Level



Source: Authors' calculations of digitalized data on health care costs from SKL (Swedish Association of Local Authorities and Regions) from 2001 to 2013.
Notes: This figure shows the change in the net cost of healthcare (costs net of revenues) between 2013 and 2003 as a function of the physician base wage in 2003. There are 21 health regions in the country.

Figure A12: Event Studies - Hospitalization outcomes



(a) Hosp. (b) Hosp. length (c) Hosp. per person

(d) Surgeries (e) Time between hospitalizations

Source: Authors' calculations of Swedish register data from 2001 to 2013.
Notes: The coefficients correspond to the event study specification in Equation 1 for each outcome. Standard errors are clustered at the municipality level, and the vertical bars show the 95 percent confidence interval.