The Fatal Consequences of Brain Drain

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Abstract

This paper examines the welfare consequences of reallocating high-skilled labor across 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 and a dose-response 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, 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 young, high-skilled generalists led to more hospitalizations, premature discharges, higher readmission rates, and delayed care. Mortality effects were larger in low-density physician regions and concentrated in older individuals and acute conditions—circulatory, respiratory, and infectious diseases. Our findings show that even temporary, intensive-margin shifts in skilled labor can generate large and unequal welfare losses when public services are already capacity-constrained.

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1 Introduction

Cross-country employment has surged in recent decades, reshaping the geography of labor markets. Millions of workers now live in one country while working in another. These shifts are redefining the boundaries of local labor markets and enabling firms and countries to access talent in a much broader arena. The trend is particularly pronounced among young and high-skilled workers, who are more mobile and tend to sort into occupations that offer higher returns to cross-border employment (e.g. Dodini, Løken and Willén, 2022; European Commission, 2017).

The effects of skill-biased worker mobility are ambiguous. On one hand, "brain drain" presents a growing challenge for policymakers as the movement of cross-border workers accelerates, making countries more vulnerable to global and regional labor market fluctuations. This issue is particularly pronounced as work tasks are becoming increasingly complex and specialized, making it difficult to quickly attract, replace, and develop high-skilled talent. On the other hand, "brain gain" offers an opportunity for local areas struggling to cultivate their own talent, potentially acting as an antidote to specific or strategic labor shortages in certain areas. The overall impact, therefore, depends on the net benefits to the destination country or region relative to the costs borne by the country or region of origin. Currently, we do not have a comprehensive understanding of these shifts and their long-term implications for efficiency, equity, and overall welfare.

Understanding the broad welfare implications of brain drain is challenging due to three key obstacles. First, there is a lack of fine-grained longitudinal data that cuts across national borders, making it difficult to capture the true scope of cross-country labor supply changes. Second, there is a lack of exogenous variation in high-skilled labor mobility, which complicates efforts to isolate the impact of worker flows on welfare outcomes. Lastly, it is very difficult to identify objective welfare measures that are comparable across origin and destination countries. Due to these challenges, evidence on the causal effects of brain drain on welfare is scarce.

This paper overcomes both the data and identification challenges in the context of the Scandinavian health care sector, offering a thorough synthesis of the welfare implications of brain drain both in the sending and receiving areas. Specifically, we study how exogenous shifts in the cross-border mobility of highly skilled physicians shape welfare in both sending and receiving regions. First, we examine the impact of changes in physician supply on local mortality. We then analyze its implications for inequality in mortality outcomes across countries, across municipalities within countries, and across individuals within municipalities. Finally, we investigate the mechanisms driving these effects, focusing on healthcare access, quality, and costs.

To quantify these consequences, we combine our mortality estimates with economic benchmarks to estimate the social value of physician labor. This allows us to assess whether the welfare losses from physician outflows exceed the fiscal costs of retention, and to benchmark these estimates against existing evidence for other healthcare workers. In an era of increasing global and regional market integration, rising inequality, and shifting demographics, understanding the impact of worker mobility—both across and within countries—is essential. This is particularly relevant as international and regional institutions advance policies to further integrate labor markets and facilitate worker flows.

Our empirical strategy exploits a unique labor demand shock in Norway, driven by a sharp surge in oil prices and a rapid expansion of the country's oil sector. This shock rippled through the economy, supercharging real wages across industries and occupations and nearly doubling the within-occupation wage differential between Sweden and Norway. As a result, the incentive for Swedes to commute across the border for work increased substantially, drawing more Swedish workers into the Norwegian labor market. For most occupations, including non-doctor healthcare workers (e.g., nurses), the commuting response was driven by proximity to the border, as taking a job in Norway oftentimes required fully substituting their Swedish employment (Dodini, Løken and Willén, 2022). However, doctors–who were in short supply in Sweden and enjoyed highly flexible work arrangements in Norway–could adjust their labor supply on the intensive margin instead. Unlike other workers, their ability to take on short, irregular stints meant their response was dictated by relative gains to commuting based on the pre-shock wage levels in their home districts and was not constrained by distance to the border.[1]

This setting allows us to implement a dose-response difference-in-differences framework, leveraging municipality-level variation in pre-shock doctor pay as the intensity measure among non-border municipalities. The key idea is that municipalities with lower pre-shock wages experienced a stronger incentive to commute and a greater outflow of physicians, generating variation in exposure to physician brain drain. By exploiting this variation, we can identify the causal impact of physician mobility on health outcomes and inequality. A key identifying assumption is that only doctors respond to this variation, which we confirm by showing that no other occupations exhibit similar mobility patterns. This supports the exclusion restriction and reinforces the idea that broader commuting pressures remain constant across non-border municipalities. With access to rich register data, we track Swedish individuals commuting to Norway, offering novel insights into brain drain's consequences for both origin and destination countries. On the Norwegian side, we use multiple indicators of physician demand–baseline doctor wages, the share of physicians near retirement, baseline physician density, and existing commuter networks–to predict where incoming commuters might work and estimate their effects. Using all of these potential predictors, we find no evidence that the growth in new commuters had a measurable impact on the mortality outcomes of residents in Norwegian municipalities.

We present four core sets of findings. First, we confirm that the Norwegian labor demand shock prompted a significant response from Swedish physicians. In the years following the shock,

[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.

the number of Swedish physicians commuting to Norway increased by more than 50 percent, resulting in a substantial loss of high-skilled physicians for Sweden and a corresponding gain for Norway.[2] Notably, this shift in commuting behavior was primarily driven by young, highly productive generalists, who are 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+, a group more likely to experience acute health issues requiring immediate attention. Additionally, we find that the mortality effects were significantly 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, suggesting that the benefits of "brain gain" in receiving countries do not necessarily offset the costs borne by the sending countries. This aligns with the fact that Norway already has one of the highest physician-to-population ratios in the OECD, while Sweden's rate 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 turn to the distributional consequences of physician outflows and show that their effects extend well beyond average mortality rates, deepening existing inequalities in health outcomes. The loss of doctors disproportionately harms more vulnerable populations and exacerbates disparities across countries, regions, and individuals. We examine inequality across three dimensions. First, at the country level, the widening gap between Sweden and Norway underscores the asymmetric impact of brain drain on healthcare systems. Counterfactual estimates suggest that fully closing the Sweden-Norway physician wage gap would have reduced mortality by 1 per 1,000 in Sweden by 2013. Our back-of-the-envelope calculations further show that the welfare gains from such a reduction in mortality would have greatly exceeded the fiscal cost of raising physician wages to levels competitive with Norway. Second, within Sweden, municipalities with lower baseline physician density relative to the population size experienced significantly steeper increases in mortality than those with higher baseline physician density, despite experiencing similar rates of physician out-commuting. Using our estimates, we quantify how much equalizing physician wages across the highest- and lowest-paid municipalities would benefit the lower-paid municipalities. Third, at the individual level, low-income and less-educated populations bear the brunt of the physician shortage, facing higher mortality rates and reduced access to timely care. These groups are more reliant on public healthcare systems and emergency services, which are

[2] Throughout the paper, we use the terms "commuting" and "commuters" to describe physicians working in and earning income on 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. Commuting entails an *intensive* margin loss of physicians' human capital in addition to any *extensive* margin loss in this context.

particularly strained by the outflow of physicians. The inability to seek alternative care, such as private healthcare or treatment in better-staffed regions, further amplifies these disparities.

Finally, we explore the mechanisms driving these effects. The mortality increase is primarily linked to the loss of high-quality young generalist physicians, 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 most of the observed rise in mortality. Beyond mortality, we find that the physician shortage places substantial strain on Sweden's healthcare system. Hospitalization rates increase, likely reflecting a rise in misdiagnoses or lower-quality care at the initial point of contact. At the same time, hospitals attempt to manage capacity by accelerating patient discharges, often at the expense of patient needs, leading to considerably higher readmission rates. In response to the growing competition for doctors, Swedish municipalities raise wages in an attempt to limit outflows, but these efforts fall short of reversing the trend. Instead, they increase hospital operational costs, further limiting resources and capacity. Taken together, fewer doctors, rising hospitalization rates, increased operational costs, and premature discharges contribute to delays in patient access to care, overburdening the system and ultimately leading to worse treatment outcomes.

Our findings highlight how shocks to the human capital stock can significantly impact the stability of healthcare systems. This is a critical consideration for policymakers seeking to build more resilient healthcare systems. This includes the systems that set physician pay, those that train new physicians, and those that reallocate physicians across space. These shocks can occur for a range of reasons, from migration policies that influence the mobility of healthcare workers to broader changes in labor market dynamics and exchange rate dynamics. Therefore, policies that address the training, retention, and recruitment of healthcare professionals, especially in vulnerable sectors like emergency care, are essential for maintaining high standards of patient care and protecting public health. More broadly, our results highlight the need for the stability and development of human capital beyond healthcare; similar risks exist in other strategically critical sectors—such as nuclear energy, advanced aerospace engineering, and cybersecurity—where workers require extensive, specialized training and where talent shortages can undermine core societal functions.

The overall welfare (mortality) effect in our setting is negative, driven by the disruption to healthcare services and the reduced quantity and quality of care patients receive. While the severity of these effects 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, driven by demographic aging, physician retirements, difficult working conditions, and too few new professionals being trained, 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 therefore have greater generalizability. Given Scandinavia's strong healthcare system and financial capacity, our results may be seen as conservative estimates or lower bounds, reinforcing the broader policy relevance of our findings.

By providing a unified framework for understanding the welfare effects of brain drain in both sending and receiving countries using exogenous variation in mobility and combining it with detailed and objective welfare measures, we contribute to the existing literature in five distinct ways.

First, we contribute to the literature on brain drain by providing the first causal estimates of its short- and long-term welfare consequences for sending and receiving countries. As Batista et al. (2025) note in their review, "Despite fears of medical brain drain, causal evidence is absent on negative impacts..." Our study directly fills this gap by identifying the effects of physician outflows on mortality, inequality, and healthcare system strain in both origin and destination regions.

Prior research on this topic has focused on individual returns, determinants of high-skilled emigration, and educational and training responses, often in developing countries (e.g., Docquier and Rapoport, 2012; Khanna et al., 2022; Abarcar and Theoharides, 2024; Mobarak, Sharif and Shrestha, 2023). Historical cases, such as the dismissal of scientists in Nazi Germany (Waldinger, 2012), have also been studied in the context of forced migration (see Becker and Ferrara, 2019), but these settings differ fundamentally from voluntary migration. While concerns about high-skilled outflows are well-documented—particularly for countries facing shortages in critical sectors (see Lutz et al., 2019)—there is no empirical evidence quantifying the broader economic and welfare costs of large-scale out-migration. Existing studies rarely assess how brain drain affects service provision and long-term labor market adjustments. Our study fills this gap by identifying the mechanisms through which high-skilled human capital loss–even short-term and other intensive margin changes–disrupts local labor markets and public services. Our results highlight that the inability to quickly expand the supply of particularly high-skilled workers exacerbates the welfare losses of brain drain.[3] Our study also contributes to the relatively scarce causal literature on how physicians respond to changes in compensation, showing how wage shifts affect their labor supply and practice choices in a cross-border setting (e.g., Devlin and McCormack, 2023; Clemens and Gottlieb, 2014; Alexander, 2020; Alexander and Schnell, 2024).

Second, we contribute to the literature on place-based inequality and health disparities by providing causal evidence on how healthcare shocks and disruptions shape regional inequalities. Prior research has shown that location significantly influences health outcomes, but the underlying mechanisms remain difficult to disentangle. Finkelstein, Gentzkow and Williams (2021) finds that moving from a low-health to a high-health area in the U.S. increases life expectancy at age 65 by 1.1

[3]For example, in contrast to the out-migration of nurses in the Philippines, where nursing programs expanded in response to out-migration (Abarcar and Theoharides, 2024), the long training horizon and significant monetary cost to train physicians make it difficult to expand local supply.

years, explaining nearly half of the 90-10 mortality gap. Similarly, Lleras-Muney, Schwandt and Wherry (2024) highlights place as a key driver of poverty-related health disparities, while Finkelstein et al. (2024) shows that economic downturns, such as the Great Recession, affected both economic activity and elderly mortality, revealing complex interactions between economic and health outcomes.

While these studies provide valuable insights, they primarily rely on observational correlations, making it difficult to isolate the causal effects of healthcare supply constraints on regional health disparities and the mechanisms through which they operate. Our study addresses this gap by exploiting exogenous variation in physician outflows, offering a unique opportunity to identify how reductions in the supply of, and access to, healthcare expertise affect mortality and hospital strain across regions. By directly measuring the effects of physician shortages, we provide new evidence on how health system capacity shapes place-based disparities in health outcomes.

Third, we contribute to the literature on the consequences of migration for sending countries, an area that remains significantly understudied. While extensive research has examined the labor market effects of immigration and job posting 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), studies on how out-migration affects the countries left behind are comparatively scarce. The few existing studies focus primarily on individual labor market outcomes, community development, and firm behavior (Hafner, 2021; Bütikofer, Løken and Willén, 2022; Dicarlo, 2022; Dodini, Løken and Willén, 2022), leaving broader macroeconomic and institutional effects largely unexplored.

We advance this literature by shifting the focus from individual-level outcomes to system-wide consequences, offering new evidence on how shortages of skilled human capital impact public services, healthcare quality, and population-wide welfare. By exploiting exogenous variation in out-commuting, we provide causal estimates of the direct costs imposed on sending countries, identifying mechanisms through which even the temporary loss of expertise disrupts essential services and amplifies inequality. This moves beyond prior studies that largely focus on workers and firms, instead revealing how migration and other shocks to movement reverberate across entire economies.

Fourth, we deliver new, causal evidence on physician shortages and mortality, demonstrating that the loss of general physicians in Swedish hospitals directly increases elderly mortality. While prior studies link physician quality to patient outcomes (Badinski et al., 2023; Finkelstein, Gentzkow and Williams, 2021; Ginja et al., 2025) and physician working time and decision making to patient outcomes (Chan, 2018; Silver, 2021; Currie, MacLeod and Musen, 2024), we break new ground by identifying, for the first time, the population-wide effects of a large-scale physician outflow—isolating its impact from broader health system dynamics.

Finally, we push the frontier on healthcare capacity constraints by revealing that physician shortages do more than degrade care quality and inflate costs—they actively overload the system, triggering higher hospitalization and readmission rates. While existing research explores hospital competition (Gaynor, Moreno-Serra and Propper, 2013), regulated pay for nurses (Propper and Van Reenen, 2010), cost-cutting in emergency care (Knutsson and Tyrefors, 2022), and the interruption of treatments arising from nursing strikes (Gruber and Kleiner, 2012) and parental leave programs (Friedrich and Hackmann, 2021b), we provide the first causal evidence that physician-driven system strain has direct, quantifiable consequences for mortality.

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. 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.[4] While the public system provides comprehensive coverage, private providers have become more common over the past few years among high-income individuals who seek quicker or more specialized care. This shift reflects an increasing trend toward private healthcare access for those with the financial means to opt for it. However, this was extremely rare during our analysis period.[5]

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.[6] Eighty percent of physicians in Sweden are represented by the Swedish Medical Association, 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. Unlike most university programs in Sweden, the number of medical school seats is 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 align medical education with available clinical training resources.

Since the mid-1990s, physician wages in Sweden have been determined through individual wage setting between employers and employees, leading to regional and role-specific variation in compensation. Factors such as experience, education, specialization, local budgets, and healthcare

[4] 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.

[5] During our sample period, less than 5 percent of individuals held private health insurance. Since then, the number has risen sharply, and currently stands at 15 percent.

[6] Since 2012, Sweden has gradually transitioned toward employing full-time Emergency Medicine (EM) specialists who work exclusively in hospital emergency departments, aiming to improve continuity and expertise in acute care.

demand contribute to wage differences across health regions. This decentralized salary system aligns compensation with regional healthcare needs and market conditions, resulting in substantial spatial wage variation. We exploit this regional wage variation before the labor demand shock in Norway to identify the effects of physician out-commuting on local health outcomes.

2.2 The Sweden-Norway Border

The Sweden-Norway border is the longest cross-country land border in Europe, spanning 1619 kilometers. This is equivalent to approximately 60 percent of the US - Mexico border.

The Norwegian and Swedish labor markets are among the most integrated in Europe, thanks to strong institutional and historical ties. In 1954, predating both the European Union's single market and the Schengen Agreement, the Nordic Passport Union was established as a pioneering move to strengthen regional cooperation across country borders.

The Nordic Passport Union is an agreement that allows citizens of the Nordic countries—Denmark, Finland, Iceland, Norway, and Sweden—to travel, live, and work freely across their borders without the need for a passport or residence permits. It is one of the earliest and most comprehensive regional agreements of its kind, emphasizing the deep cooperation among these nations.

There are three features of this agreement that are particularly interesting for our study. First, citizens of the Nordic countries can cross borders without passports and reside in each other's countries without residence permits. This applies to all Nordic citizens, regardless of the purpose of their travel or stay (work, study, or leisure). Second, Nordic citizens enjoy similar rights as nationals when residing in another Nordic country. This includes access to social security, healthcare, and education, making it easier to integrate. Third, Nordic citizens can work in any Nordic country without a work permit and are not required to reside in the country where they are employed. The arrangement supports the seamless movement of labor across borders, fostering a well-integrated regional labor market.[7] In contrast to labor markets, the product market is much less integrated since Norway is outside the EU customs union.

In terms of institutional barriers to employment integration, Sweden and Norway are very similar with respect to labor market design, education systems, and welfare policies, and there are very few prohibitive occupational licensing restrictions across the border. The cultural proximity and mutually intelligible languages further reduce barriers to workplace integration. Specifically, Norwegian and Swedish are closely related North Germanic languages with high mutual intelligibility. While some lexical, phonetic, and grammatical differences exist, their structural similarities greatly facilitate cross-border communication. As a result, individuals working in either country can use their native language in professional settings without significant barriers, and this is often incorporated into law.[8] Shared norms and values also make it easier for workers to adapt to

[7] With the implementation of the Schengen Agreement in the Nordic countries in 1996, the Passport Union became part of the broader European system of free movement, allowing non-Nordic Schengen citizens to also move freely in the region.

[8] For example, the laws governing the Higher Education Sector in Norway explicitly states that the language of

different work environments.

Finally, Nordic countries have agreements to prevent double taxation and to coordinate social security systems. This ensures that cross-border workers receive equitable treatment in terms of benefits, pensions, and healthcare. In particular, tax and welfare systems in the Nordic region require workers to pay taxes and receive welfare benefits—such as pensions, unemployment benefits, parental leave, and sick leave—in the country where they are employed. This applies to all cross-border commuters, with one exception: workers who live in a border municipality on one side of the border and work in a border municipality on the other side pay income taxes in their country of residence. These individuals are omitted from our analysis.

2.3 Working Across the Border

Labor mobility across the Sweden-Norway border has been a significant feature of the Nordic labor market over the past several decades, with Swedish citizens being particularly mobile within the region. Specifically, Swedish nationals account for around 80% of all cross-border commuting in the Nordic countries, primarily commuting to Denmark and Norway. The proximity of these countries, combined with their large labor markets and high wages near smaller Swedish cities, makes them attractive destinations for Swedes living near the borders. Commuters tend to be young, highly educated, higher-income, single, and men, often motivated by the prospect of higher wages and better job opportunities. These workers can be found across all industries. Among the high-skilled, doctors, nurses, and economists make up the largest groups. In contrast, very few Norwegians commute to other Nordic countries, with fewer than 2,000 making the move each year.

The cross-border commuting flow from Sweden to Norway was substantial in the late 90s and early 00s, with around 30,000 workers annually. It remained stable until 2004, 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 quickly to wage differentials more quickly 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

instruction has to be Swedish, Norwegian, or Danish.

process is quick (approximately 4 to 6 weeks), and the certificate is valid for life. There is no language barrier, as Swedish and Norwegian are closely related North Germanic languages with high mutual intelligibility. While some differences in vocabulary, pronunciation, and grammar exist, these do not impede communication. Swedish doctors can work in Norway using Swedish, as it is widely understood in the Norwegian healthcare system. 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 contracts in hard-to-staff areas or areas undergoing manpower transitions. These arrangements entail a significant pay premium.

2.4 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 1 shows the change in GDP per capita and exchange rate across the two countries over our analysis period.[9] Figure A1 shows the revenue from the oil sector to the Norwegian government over time - the primary driver behind this 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 not only characterized by an increase in GDP but also by 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, affecting both private and public sectors, and was largely driven by both broad-based increases in demand and Norway's national sectoral collective bargaining system. Specifically, 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,

[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.

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 (Figure 5).[10]

The unprecedented rise in wages and labor demand in Norway had significant spillover effects on neighboring Sweden, especially in regions near the Norwegian border. The demand for labor in Norway intensified competition for workers, which, in turn, spurred a surge in cross-border commuting from Sweden. The number of Swedish workers commuting to Norway doubled between 2004 and 2009, reflecting the growing disparity in economic conditions between the two countries.

2.5 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 2, 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—between 2001 and 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 (approximately 4.5 million in Norway and 9 million in Sweden in 2003).

Panel (b) of Figure 2 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 2 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 3 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

[10]Figure A2 presents the same within-occupation wage gap over time, stratified by ISCO-88 occupation codes.

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 4 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.[11] Figure A7 provides the full density distribution of physician wages across Swedish municipalities in 2003.

Figure 4 shows a strong correlation between average physician wages in a municipality prior to the shock and the subsequent commuting response (percentage change relative to baseline), supporting this hypothesis. Interestingly, this relationship does not hold for non-doctors, whether based on non-doctor base pay or doctor base pay. This difference is likely due to the less flexible contracts held by non-doctors, who typically need to commute daily on a near-full-time basis, making distance a more significant factor. In contrast, doctors can more easily take advantage of periodic, infrequent opportunities, allowing them to adjust their commuting based on wage differentials rather than proximity.

The geographic variation in commuting responses for doctors is crucial for our analysis. Previous research has shown that Swedish border municipalities experienced significant changes due to the Norwegian labor demand shock, from individual commuting decisions to firm behavior, population shifts, and local economic development near the border (Dodini, Løken and Willén, 2022). Consequently, focusing on these areas would prevent us from isolating the causal effects of brain drain—specifically, the outflow of physicians—on population health outcomes. Therefore, we exclude border municipalities from our analysis. [12]

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

[11] Wages for doctors in Sweden are individually negotiated between the worker and the employer, leading to regional variation driven by factors such as supply, demand, experience, and competition.

[12] 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 currency shock likely affected other types of workers, firms, and local economic conditions near the border, it is not possible to isolate the mortality effects to nurse brain drain alone.

employment, earnings, industry, and municipality of work. These data are linked to our main data via social security numbers shared between the two national statistical agencies. Together, these data enable us to identify all doctors in Sweden, what they look like in terms of socioeconomic and demographic characteristics, and if they have ever worked in Norway.

We combine these data with detailed health data extracted from rich administrative records held by Statistics Sweden. These data include information on mortality, cause of death, hospitalizations, length of 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 effects in Sweden are offset by brain gain effects 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. While these results are not as well identified as those on the Swedish side, they provide clear suggestive evidence that helps us assess the overall welfare impact of the brain drain phenomenon. Our data enable us to analyze how the initial shock to the cross-country earnings gap and the subsequent labor supply shock affect doctor flows across the border and how that shaped mortality and health-related outcomes in both Sweden and Norway. Table A6 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 (something we examine in Section 3).

3.2 Empirical Design

3.3 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 widespread exposure to the Norwegian labor market shock 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, enabling us to disentangle the causal effects of brain drain on citizen well-being.

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 (Figure A7). 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 pay 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 allows us to isolate the causal effect of physician brain drain on key outcomes, such as changes in mortality rates. This design mirrors those used in prior work that have used regional differences in pay to identify causal effects in a range of different settings (Willén, 2021; Britton and Propper, 2016; Propper and Van Reenen, 2010).

In addition to the parallel trends assumption inherent to the difference-in-differences framework, our approach requires that variation in physician commuting responses across municipalities is not confounded by simultaneous outflows of other workers or other concurrent shocks that differentially shape the outcomes of interest across areas with higher versus lower baseline physician pay. In Section 4, we demonstrate that these areas experienced no other negative effects from the Norwegian labor demand shock, that only Swedish doctors in these regions (and no other groups of workers) responded to the increased external opportunities, and that variation in baseline pay does not predict changes in pre-shock outcomes. Together, these findings provide robust support for the identification assumptions necessary for causal inference in our setting.

3.4 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 empirical approach, we begin with a visual illustration of the causal pathway using raw data. We then provide a detailed explanation of our empirical design before presenting regression-based analyses to isolate causal effects and address potential identification concerns.

We begin by examining 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 6 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 6 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 6 shows a positive, nearly linear relationship between changes in doctor commuting and changes in mortality rates. In other words, municipalities that lost more doctors due to stronger financial incentives for cross-border commuting also experienced relative increases in mortality. This pattern underscores the potential health consequences of physician shortages driven by these wage differentials.

The findings presented above provide descriptive evidence of brain drain, which generates negative welfare effects on the Swedish population. However, in order to interpret these correlations as causal effects, a set of core assumptions must hold. First, it must be the case that municipalities with varying levels of exposure to the labor demand shock would have followed similar trends in outcomes absent the shock. Second, there must be no changes concurrent with the Norwegian labor demand shock that are correlated with base pay differences across municipalities in Sweden and that independently influence both physician mobility and mortality during the same time period. Finally, physician base pay should not directly affect other municipality-level outcomes during the shock in ways that have direct impacts on the outcomes of interest; these might include commuting in other professions or population growth.

In the next section, we outline how we examine and address these assumptions within our causal framework. Importantly, however, we note that even simple placebo tests based on raw data provide strong initial support for these assumptions. For example, Figure 7 shows 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, suggesting that differential trends are not driving our results.

3.5 Technical Implementation of Design

Our analysis employs a conventional 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 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 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 all observable characteristics.[13]

[13]Specifically, when we include these municipalities, we observe similar effects in terms of commuting responses

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 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.

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. The coefficient of interest in Equation 2 is thus β_2, providing us with the average relative effect of the commuting shock during the post period. All other variables are defined as above.

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 help us examine if our data are consistent with the first assump-

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.

tion. Specifically, 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.

In Section 5.6, we subject our analysis to a rich set of robustness checks to provide additional support for our required assumptions. We present sensitivity analyses where we include border municipalities, the university hospital regions, and when we trim outliers in the sample. In addition, we show that the pre-shock physician wage cannot predict mobility responses in any other occupation groups; cannot predict demographic changes, immigration, or population effects; and that there is no indication of our intensity measure picking up effects that are driven by other factors than differential out-commuting of physicians. Further, we show that differences in other pre-shock occupation wages cannot explain the commuting response of physicians that we uncover. Finally, we binarize our continuous exposure variable (splitting at the sample median) and apply a synthetic difference-in-difference approach (Arkhangelsky et al., 2021). This approach extends the logic of the synthetic control method (Abadie, Diamond and Hainmueller, 2010) to a difference-in-differences setting. In a two-way fixed effects model with unit and time fixed effects, this method creates a synthetic weighted average of the pre-treatment outcomes from control units to match the trends of treated units in the pre-treatment period and then examines changes in these weighted averages after treatment. We also conduct permutation tests in which we randomize the distribution of baseline physician wages across municipalities and re-estimate our difference-in-differences model 300 times and present the distribution of the β_2 coefficients. This exercise helps us eliminate concerns over potential spurious correlations driving our results and provides empirical p-values as a secondary inference check.

In addition to estimating the effects on Swedish municipalities, we also investigate potential consequences in Norway. To assess the effects in Norway, we use pre-shock indicators of physician demand—such as baseline physician wages, age composition, physician density, and commuter networks—to predict where Swedish doctors are most likely to work. We then examine whether these inflows affected local mortality or the labor supply of Norwegian physicians. While this exercise reveals that these predictors of commuters' destinations within Norway are fairly weak, these results are essential for understanding the aggregate welfare effects of cross-border physician mobility. We discuss the empirical approach and findings in greater detail in Section 6.

4 First-Stage 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. Furthermore, we confirm that pre-shock physician wages are not associated with factors that could confound our results and lead to biased estimates, ensuring that the observed effects are specifically attributable to the loss of doctors rather than other potential municipality-specific shocks or trends.

4.1 Commuting Response

Figure 8 illustrates the proportion of doctors in a municipality who commute across the border for work, plotted as a function of the average pre-shock physician wage in the municipality using Equation 1. 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 better account for size differences across places).

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 parallel trends assumption, which is crucial for causal inference. 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, indicating that physicians are relatively responsive (elastic) to increases in outside options in terms of mobility and commuting behavior.

In addition to presenting the time-varying treatment effects based on Equation (1), we provide 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 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 (USD 500), which is very close to 1 percent of the average physician wage in 2003.

In Column (2) of Table 1, 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 A5, 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, Columns (3)–(8), examine whether the treatment generated other changes in the municipalities that could confound the results and bias the interpretation of our findings. The results reveal no significant effects on other demographic characteristics of the municipalities. Specifically, there are no changes in the number or commuting propensity of other types of healthcare workers (Columns (3) and (4)) or nurses (Columns (5) and (6)). Additionally, neither the total population nor the elderly population (65+) shows differences based on the 2003 base physician wage. These findings suggest that other potential confounding factors at the municipality level are uncorrelated with base physician wages, supporting the validity of the exclusion restriction in this context.

Are Swedish municipalities responding to the increased competition for their healthcare workers? In Table A8, we show that the average Swedish wage is increasing in response to the outflow of physicians, eliminating approximately 20-25 percent of the cross-country physician wage gap (column 1). This is a relatively sizable response, though not sufficient to prevent a large outflow of doctors to Norway. The wage increase generates an increase in the overall cost of physicians in the affected regions (column 2). There are two particularly noteworthy implications of this finding. First, the reallocation of already scarce resources to cover rising physician wages implies that regions may need to divert funds from other essential spending categories, plausibly increasing financial strain and risking further reduction in patient care quality. We explore this in detail below. Second, in later sections we will also show that the physicians most likely to commute to Norway are positively selected from the ability distribution, implying an increase in the cost of physicians who, on average, are of lower quality.[15]

[14]Additionally, we observe no sizeable 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. As a result, many Swedish physicians are trained in other European countries and regions such as Denmark and the Baltics.

[15]Even though the commuting is characterized as an intensive margin response on the hours dimension, the fact

5 Second-Stage Mortality Effects

In this section, we establish a clear relationship between lower pre-shock average physician wages in a municipality and higher post-shock changes in mortality rates. The analysis shows that this effect is concentrated among individuals aged 55 and older. In Section 7.2, we identify three specific causes of death driving these results: respiratory diseases, infectious diseases, and circulatory diseases. These causes, often linked to sudden and urgent health conditions requiring immediate attention in emergency settings, highlight the critical importance of timely medical care in reducing adverse health outcomes.

5.1 Mortality Effects

Figure 9 illustrates 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 out-commuting. To strengthen the validity of our approach, we extend the event study to begin in 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.[16]

The results from this exercise, shown in Figure 9, reveal 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.

Column (1) of Table 2 presents the average post-treatment effect using the simplified difference-in-difference specification from Equation 2. The results indicate that a 1 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 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.

that higher-skilled physician reduce their time in Sweden implies that the gain in average wage must, to an extent, be driven by higher compensation for lower-quality physicians. This is consistent with prior work on cross-border migration and commuting (e.g. Dodini, Løken and Willén, 2022).

[16] 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 A9 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 can, therefore, explain our results.

5.2 Inequality Effects

As demonstrated in Section 4.1, regions with lower pre-shock physician wages experienced higher out-migration of doctors. Section 5.1 shows that this commuting response led to a significant increase in mortality rates in these affected regions. This suggests that uneven exposure to the labor demand shock, and the resulting brain drain in the healthcare sector, exacerbated regional health disparities in Sweden. While prior research has emphasized the importance of location in shaping health outcomes, disentangling the mechanisms driving these disparities remains a challenge (e.g., Finkelstein, Gentzkow and Williams (2021), Lleras-Muney, Schwandt and Wherry (2024), Finkelstein et al. (2024)).

This shock generates three distinct types of inequality: across countries, across municipalities, and across individuals. In this subsection, we examine each type of inequality separately. We conduct counterfactual exercises to explore how these disparities would have evolved in the absence of the shock, the potential costs of averting the mortality effects of the shock, and whether such interventions would be cost-effective. Through this analysis, we aim to understand the broader implications of labor market shocks on regional health inequalities and individual-level disparities.

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 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 scenario 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 billon.[17] 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

[17] 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.

the number of physicians in 2004, is \$2.7 billion.[18] This suggests that the failure to match wage growth contributed not only to the outflow of skilled physicians but also to higher mortality rates, 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.[19] 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.

Through these counterfactual exercises, we quantify the extent to which the wage gap between Sweden and Norway exacerbated mortality disparities, providing a clearer picture of the role of labor market inequalities in shaping health outcomes.

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 used measure to assign a monetary value to improvements in health outcomes or extensions in life expectancy. VSLY estimates vary substantially across regions, reflecting differences in socioeconomic factors. For this analysis, we use a conservative estimate of VSLY at USD 35,000, as reported for the European Union (EU).[20]

$$\begin{aligned}\text{Cost per person} &= \text{VSLY} \times \text{Life expectancy reduction}\\ &= 35,000 \times \{0.5,\ 0.6,\ 1.1\} = \{17,500;\ 21,000;\ 38,500\}\ \text{USD}\end{aligned}$$

For the total population in our sample areas of 3.5 million, this amounts to total gains of \$61, \$73, and \$134 billion over ten years. This implies a cost-benefit ratio of roughly 1:23 across all three scenarios, meaning that every dollar spent yields approximately \$23 in social value.

[18] 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 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.

[19] 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.

[20] This provides a lower-bound estimate of the economic cost of reduced life expectancy. For comparison, the VSLY in the United States is significantly higher at an estimated USD 369,000.

5.4 Inequality Across Municipalities

The second dimension of health inequality generated by the shock is within Sweden, across municipalities. 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, therefore, 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 3 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 (Column 4), 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. Areas with ex ante higher probabilities of physician shortages experienced the largest increases in mortality. This dynamic significantly increases inequality in longevity across Swedish municipalities by increasing the relative deprivation of areas already struggling to retain physician expertise.

Places with different doctor salaries. Prior to the commuting shock, municipalities with lower physician wages (Q1) exhibited lower mortality rates than those with higher wages (Q4). 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 that the baseline log wage difference between Q1 and Q4 municipalities was 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 that across individuals. As extensive 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 responses may therefore not only shape inequality between localities but also exacerbate individual-level disparities in healthcare access and outcomes within regions. As discussed earlier, age-specific effects have already been uncovered, with negative health implications loading on the older and more vulnerable population. In this section, we examine the mortality effects across different demographic groups and assess the differential impacts by education and income levels.

Table 4 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 results in Table 4 show 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.

This result across the income gradient suggests that low-income individuals are more likely to suffer negative health impacts from the shock compared to more advantageous groups. 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, statistically significant at the five percent level. However, the difference between the low- and high-education groups in terms of the mortality response is not statistically significant. This suggests that while low-education individuals might have been more affected by the shock, 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 additional tests to assess the validity of the identification strategy and rule out concerns about unobserved confounders or spurious correlations. We examine whether commuting responses are specific to physician wages, whether wages in other professions predict physician commuting, whether mortality effects disappear during a placebo period with no impact on physician supply, whether the treatment holds up under permutation tests, and whether alternative sample and treatment definitions yield consistent results.

Placebo Tests for Commuting Behavior. An important concern in our identification strategy is whether the relationship between pre-shock physician wages and subsequent commuting behavior is unique to physicians or whether it reflects broader labor market dynamics that influence workers in other occupations. If wages in non-healthcare occupations also predicted physician commuting, this would suggest that the treatment intensity measure, which is based on pre-shock physician wages, might be capturing more general economic conditions rather than physician-specific incentives.

To test this, Table A2 examines whether pre-shock wages in non-healthcare occupations predict physician commuting behavior as a function of the Norwegian labor demand shock. Column (2) shows that non-healthcare wages have no predictive power for physician commuting, indicating that the observed physician outflows are not driven by general labor market conditions. The remaining columns extend this analysis by including physician wages, non-healthcare wages, and non-doctor healthcare wages in a single specification. The results confirm that only physician wages significantly predict physician commuting to Norway, with non-healthcare wages and non-doctor healthcare wages having no explanatory power.

A complementary test reverses this approach by examining whether pre-shock physician wages predict commuting in other occupation groups. Table A3 reports these estimates, showing that baseline physician wages do not predict commuting for non-physician workers, non-physician healthcare workers, nurses, or any healthcare workers who are not physicians or nurses. If physician wages are simply capturing broader economic conditions or reflecting labor demand shifts that affected multiple sectors, we would expect to see some degree of mobility among other occupations in response to the Norwegian labor market shock. However, the fact that no such pattern emerges suggests that the observed outflows are uniquely concentrated among physicians. These results are also consistent with the spatial commuting patterns shown in Figures 3 and 4 based on raw data.

This finding strengthens the validity of our identification strategy in two key ways. First, it confirms that the treatment intensity measure is not proxying for general labor market conditions or sector-wide employment shocks but is instead isolating the specific response of physicians to wage incentives in Norway. Second, it demonstrates that the municipalities experiencing physician outflows do not simultaneously experience shifts in the composition of other occupations, ruling out the possibility that these areas were subject to broader labor market disruptions that could confound the estimated effects. These results reinforce that the estimated physician outflows are not simply part of a larger trend in worker mobility but represent a distinct and isolated supply shock affecting healthcare professionals.

Placebo Test for Mortality Effects in July. The ideal placebo test for the mortality effects uncovered in the main specification is to examine a period in which the commuting response does not necessarily represent a reduction in physician labor supply in Sweden. This would allow us to assess whether the observed mortality increases are truly driven by physician shortages or whether they reflect broader patterns of deteriorating health that happen to be correlated with the treatment measure. If areas with lower pre-shock physician wages systematically experienced worsening health outcomes for reasons unrelated to a reduction in labor supply, this could lead to a misattribution of mortality increases to physician shortages rather than underlying health trends.

Seasonal mortality in July provides a natural setting for this test. The Swedish labor market is structured around a statutory right to summer vacation, as codified in the Annual Leave Act (Semesterlagen, 1977:480), which grants employees, including healthcare professionals, four to five weeks of continuous leave, most often taken in July. This structure means that even if physician commuting to Norway is large during this period, it is unlikely to significantly reduce physician labor supply in Sweden. Many commuting physicians take advantage of their scheduled leave to work temporarily in Norway as *sommarvikar*(Summer temp) rather than by cutting their regular hours in Sweden.

While not all commuting physicians fall into this category, the overwhelming majority do, and the Swedish healthcare system anticipates these planned absences in its staffing decisions. If the mortality effects in the main analysis were driven by physician shortages, no effect should be observed in July, when the system is already structured to accommodate reduced staffing and when commuting itself does not generate an additional supply drop, as physicians are substituting vacation time for temporary work in Norway. Conversely, if mortality were to increase in July as well, this would suggest that the estimated effects in other months could be driven by unrelated health trends rather than the causal impact of physician shortages.

To test this, we re-estimate the main dose-response difference-in-differences model using July mortality as the dependent variable. The treatment intensity measure remains the inverse of log pre-shock physician wages. The specification includes all baseline controls and clusters standard errors at the municipality level.

The result yields a coefficient estimate of -0.016 with a standard error of 0.298 and a p-value of 0.955 in terms of per capita mortality (per 1000 people). This is a relatively precisely estimated zero with confidence intervals that rule out any meaningful effect size, indicating that increased physician commuting during this period has no impact on mortality.[21] These findings strengthen the causal interpretation of the main results, demonstrating that mortality only increases when physician commuting leads to an actual reduction in healthcare availability.

Permutation Test for Treatment Assignment. We conduct a permutation test to assess whether the treatment assignment mechanism introduces any bias into the estimates. The key

[21] On the Norwegian side, we see no evidence of a mortality effect in July either.

idea behind this test is to check whether the effects we observe in the main analysis could have appeared by chance rather than reflecting a true causal relationship. If our treatment intensity measure—based on pre-shock physician wages—were simply picking up random variation in mortality or commuting, we would expect to see similar results even when the treatment is randomly reassigned.

To test this, we randomly shuffle baseline physician wages across municipalities and re-estimate the difference-in-differences model using these permuted treatment intensity values. By repeating this process 300 times, we create a distribution of placebo estimates that shows what the results would look like if treatment were assigned randomly. Comparing this distribution to the actual estimates allows us to determine whether the observed effects are larger than what would be expected under random assignment.

Figure A8 presents the distribution of placebo estimates. Panel A reports the placebo estimates for physician commuting, while Panel B shows the placebo estimates for mortality. The estimates from the main analysis lie in the extreme tail of the simulated distribution, with a p-value of 0.02, confirming that the observed effects are highly unlikely to be driven by spurious correlations. These results strengthen the robustness of the treatment assignment strategy and further validate the causal interpretation of the findings.

Sensitivity to Sample Selection and Treatment Definitions. The final set of robustness tests examines 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 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 also has 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 excluded regions. Panel A adds university hospital regions to the sample, while Panel B incorporates border municipalities. The estimated coefficients remain similar, suggesting that the main findings are not driven by their exclusion. However, given the structural differences between these areas and the rest of the sample, these results should be interpreted with caution.

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 whether 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, reinforcing that baseline physician wages do not reflect later shocks or developments after 2005 beyond their role in predicting physician commuting. Furthermore, Table A10 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. This helps us rule out the possible health effects of changing the composition of the local population to include more immigrants, particularly from the lower-income countries of Eastern Europe that joined the EU.

6 Effects in Norway

We turn next to the Norwegian side of the border to examine whether the inflow of commuting physicians from Sweden aligned with local physician demand and whether it coincided with changes in patient outcomes or the retention of Norwegian physicians. While the empirical setup here holds less statistical power for predicting commuters' destination than predicting out-commuting on the Swedish side, the analysis offers evidence on a key and often overlooked question: 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.

We begin by examining whether new commuters were disproportionately allocated to municipalities with signs of physician shortages. Figure A4 presents four correlations 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. If anything, Panel C suggests that municipalities with more physicians per capita and a lower share of commuters at baseline saw slightly larger increases in commuting physicians–opposite the pattern we might expect if new inflows responded to local need. Panel D shows no systematic relationship between actual commuter inflows and predicted exposure 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.

We then explore whether municipalities that received more commuting physicians experienced changes in over-65 mortality rates or physician separation. Panel A of Figure A5 interacts the total 2005-2013 change in commuters with year indicators and shows no meaningful change in mortality in municipalities with larger commuter inflows. This suggests that the arrival of Swedish physicians did not translate into improvements in population health outcomes. Given that commuters are more likely to work in municipalities with a major or university hospital (Appendix Table A7), Panel B splits municipalities by major hospital presence and again finds no differential mortality trends, reinforcing the absence of observable health impacts. Finally, 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 improve their retention.

Taken together, these results suggest that the inflow of Swedish commuting physicians was not targeted to underserved areas and did not produce detectable effects on patient outcomes or physician retention. While Sweden experienced measurable losses following the outflow of physicians, there is little evidence of corresponding gains in Norway. This asymmetry reinforces the broader point: when health systems are already well-resourced, the marginal benefit of skilled labor inflows may be limited, especially when those inflows are not directed to areas with high need. For example, Norway has approximately 20 percent more general practitioners per capita than Sweden, and if the marginal return to an additional physician diminishes with density, this may help explain the limited impact observed. These findings are consistent with our results on the Swedish side, where physician outflows had the largest mortality effects in areas with low baseline physician density.

7 Mechanisms

In this section, we aim to disentangle the mechanisms through which our observed mortality effects in Sweden operate. We start by identifying the types of doctors who are most likely to commute, providing insight into which hospital departments are likely most affected by the brain drain.

In this analysis, we focus on effect heterogeneity across physicians of varying demographic characteristics known from prior literature to be more or less mobile in response to changes in outside options. These include age, gender, family structure, and specialization status. We also analyze whether commuting is associated with positive selection, focusing on pre-shock earnings distribution to determine if higher-productivity doctors are disproportionately commuting. Understanding these patterns is crucial for identifying which segments of hospitals—such as the emergency department or general wards—are most likely to face challenges in maintaining adequate staffing levels.

Next, we study detailed cause-of-death effects to better understand which aspects of the hospital and healthcare system drive the overall mortality patterns we observe. By analyzing specific causes of death, we can identify whether physician shortages disproportionately affect certain conditions, such as cardiovascular diseases, infections, or emergency cases, shedding light on where gaps in care emerge.

Finally, we examine changes in key healthcare system outcomes to further explore how these shifts impact hospital performance. We analyze hospitalization rates, time between patient visits, average visit durations, and surgical outcomes to assess potential declines in quality and capacity at local hospitals. Together, these analyses shed light on how physician shortages, driven by cross-border commuting, may contribute to reduced healthcare system responsiveness and overall patient care quality.

7.1 Commuter Types

To better understand the mechanisms behind the observed effects, we examine heterogeneity in commuting responses among doctors. This allows us to identify which types of physicians are more likely to leave and, consequently, which areas of hospitals may be disproportionately affected by the resulting brain drain. By analyzing these patterns, we aim to shed light on how cross-border commuting impacts the distribution of medical labor across hospital departments, 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.[22]

Appendix Table A1 summarizes the results of this analysis. Column (1) highlights the overall effect: the share of doctors commuting to Norway increased substantially, rising 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 pre-

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

sented by the wage differential (e.g., 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, with higher-paid doctors being more likely to commute. This suggests that the doctors leaving are not only more mobile but also among the most productive within their municipalities. The data further reveal that generalists, rather than specialists, are driving this effect. Many of these generalists are younger doctors who have recently completed medical school and are working in hospital wards in general and emergency departments in particular. As frontline workers, they often serve as the first point of contact for patients presenting with acute illnesses, including circulatory, respiratory, and infectious diseases. Their departure poses substantial challenges for hospitals in maintaining capacity to deliver timely care, particularly for emergencies.

These findings align closely with our cause-of-death results show in the next subsection, 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

After examining the commuting and overall mortality effects of the Norwegian labor demand shock, a critical question arises: what types of illnesses underlie the observed changes in mortality that we identify? Understanding this is essential 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 in emergency settings: 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.

In Panel A of Table 5, we focus on the aggregate categories of death causes: emergency and non-emergency illnesses. The results show that urgent conditions predominantly drive the observed mortality changes. These conditions, such as circulatory, respiratory, and infectious diseases, 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 acute and sudden health crises rather than chronic or less time-sensitive illnesses.

For full transparency, Panel B presents the effects for each individual 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. These findings underscore the critical importance of a responsive healthcare system, particularly in delivering urgent care for acute conditions. They also highlight how disruptions in physician availability can disproportionately impact vulnerable populations and individuals facing sudden medical emergencies.

7.3 Hospitalization

To further understand and disentangle the mechanisms driving the observed mortality effects, 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. This is particularly the case in light of the increased wage bill documented in prior sections (Appendix Table A8). If so, patients may experience worsening conditions that necessitate hospitalization.[23]

Using the simplified difference-in-differences estimator, we estimate the effect of the commuting shock on hospitalizations per 1,000 people. Column (1) of Table 6 shows a significant increase in hospitalization rates following the shock, with 1 log-point lower pre-shock physician wage (a 0.01 log change) leading to an increase of 0.142 hospitalizations per 1,000 individuals. These findings are consistent with the expectation that lower access to skilled generalist physicians at the first point of contact raises the likelihood of misdiagnoses or conditions worsening before appropriate care is received, resulting in hospitalization. The positive selection of skilled doctors into commuting likely amplifies these effects, as their absence disproportionately affects the capacity of local healthcare services.

This pattern aligns with the broader framework that physician shortages contribute to systemic healthcare strain. Higher hospitalization rates suggest that disruptions in the quality and quantity of care at the first point of contact led to a greater need for hospital-based treatment, increasing the overall demand for acute medical services.

[23] 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 aggregate healthcare regions in Sweden (21). 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 A9 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.

The event study results in the appendix (Figure A10) confirm that these hospitalization effects track closely with the timing of the commuting shock and do not violate the common trends assumption required for causal inference in our setting. The increase in hospital admissions appears persistent, reinforcing the interpretation that physician shortages cause a lasting strain on the system rather than a temporary adjustment.

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 rising number of 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 6 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.

The reduction in hospitalization length provides further evidence that physician shortages affected not only hospital entry but also the duration of inpatient care. 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 successive hospitalizations. A reduction in time between visits suggests that patients require more frequent hospital admissions, which may indicate worsened health outcomes or inadequate care at earlier stages.

Column (3) of Table 6 shows a statistically 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 shortages 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.[24]

7.6 Surgical Procedures

Despite the broad disruptions observed in hospital admissions, hospitalization length, and readmission frequency, Column (4) of Table 6 shows no significant change in the likelihood of patients

[24] Appendix Figure A10 provides additional evidence of an increase in the number of hospitalization events per person, reinforcing the conclusion that hospital utilization rose as a result of the commuting shock.

undergoing surgical procedures. However, the event study in the online appendix (Figure A10) suggests a short-term increase in surgical procedures following the commuting shock. While this estimate is noisy, it aligns with the other observed effects documented in this section—rising hospitalization rates, worsening care quality, and greater care pressure—potentially leading to a higher need for more serious interventions.

One potential explanation is that physician shortages primarily affected generalists rather than specialists. As shown in Section 7.1, the doctors most likely to commute were generalists working in emergency departments and general hospital wards, while specialists—who are responsible for performing surgeries—were less affected. This distinction is critical: while generalists play a central role in diagnostics and inpatient management and first point of contact, specialists may have been relatively insulated from physician outflows, helping maintain surgical capacity despite disruptions elsewhere in the system.

At the same time, the short-term increase in surgical procedures suggests that the strain on hospital workflows may have led to a shift in treatment strategies. As indicated by the findings in Sections 7.3–7.5, hospitals adapted to increased patient loads by shortening stays and managing higher hospitalization frequencies, likely affecting treatment pathways. The rise in surgical procedures could reflect a reallocation of hospital resources in response to these pressures, where delayed diagnoses or reduced access to earlier-stage treatment resulted in a higher need for surgical interventions. Additionally, the commuting shock may have disrupted continuity of care, increasing the risk of complications that necessitate surgery.

These results highlight the differential effects of physician shortages across medical specializations. However, given the noise in the event study estimates and the baseline difference-in-difference specification, we remain cautious in interpreting the short-term increase in surgical procedures as a definitive response to the shock. We believe that additional research on this topic would be highly beneficial.

7.7 Summary of Mechanisms

Our analysis of the commuting shock's mechanisms provides a clearer understanding of how physician shortages translate into increased mortality. First, we document that commuting is concentrated among younger, high-skilled generalist physicians, who are more mobile and responsive to outside labor market opportunities. These doctors are disproportionately responsible for staffing emergency departments and general hospital wards, where immediate access to care is critical. Their departure reduces frontline hospital capacity, particularly for acute and urgent conditions.

Second, we find clear evidence of strain on the healthcare system following the commuting shock. Hospitalization rates increase, hospital stays become shorter, and patients require more frequent readmissions. These effects are consistent with reduced continuity of care and lower treatment quality at earlier stages, which may lead to worsened health outcomes. Importantly, we observe no significant change in surgical procedures, suggesting that specialist-driven care remained largely unaffected. This distinction reinforces the idea that disruptions primarily affected general and emergency care rather than specialized treatment units.

Finally, the mortality effects align closely with these healthcare system disruptions. The observed increase in deaths is concentrated among older patients and those suffering from time-sensitive conditions, including circulatory, respiratory, and infectious diseases. These conditions require immediate and continuous medical intervention, making them particularly vulnerable to shortages of skilled generalist physicians. The fact that high-skilled doctors were the most likely to commute further exacerbates this issue, as their departure disproportionately impacts the quality of emergency and inpatient care. Overall, these findings illustrate how even moderate reductions in the local physician workforce—particularly among key frontline providers—can generate system-wide consequences, increasing strain on hospitals and raising mortality risks for the most vulnerable patient populations. The results underscore the importance of maintaining physician availability, particularly in regions where access to emergency care is critical for patient outcomes.

7.8 Benchmarking the Social Value of Physicians

To benchmark the social value of physician labor, we replicate the back-of-the-envelope approach developed by Friedrich and Hackmann (2021a), who estimate the marginal product of nurses by combining mortality effects with assumptions about life-years lost and the monetary value of a statistical life year (VSLY). In their setting, a parental-leave reform in Denmark led to a sharp, policy-induced reduction in nurse employment across hospitals and nursing homes. A 10 percent decline in nurse staffing (1,100 nurses) increased mortality among elderly nursing home residents by 1.5 percentage points, based on a reference population of 100,000 residents. To translate this effect into economic value, they assume that each excess death corresponds to one lost life-year. Recognizing that the affected population is frail and institutionalized, they adjust the standard VSLY of \$100,000 by a quality-of-life factor of 0.35—implying a value of \$35,000 per life-year. This yields a total patient welfare loss of \$52.5 million. Dividing this by the staffing reduction implies a social value of approximately \$47,700 per nurse per year. Compared to an average nurse salary of \$24,000, this corresponds to a value-to-wage ratio of roughly 2.

We adapt this framework to estimate the social value of physicians in our setting on the Swedish side. A 3.4 percentage point increase in cross-border commuting corresponds to 3.74 additional physicians leaving the municipality for work in Norway, out of an average stock of 110 physicians. Since we do not observe hours worked directly, we estimate labor supply effects in full-time equivalents (FTEs) using earnings data. Specifically, 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).

To estimate the corresponding mortality effect, we use our baseline estimate of 0.88 excess deaths per 1,000 residents. In a municipality of 20,000 people, this corresponds to 17.6 additional deaths. Following the conservative assumption in Friedrich and Hackmann (2021a), we treat each death as representing one lost life-year. However, because the affected population in our setting is younger and healthier, we do not apply a quality-of-life adjustment. We value each life-year at the full VSLY of $100,000. This results in a total social value of $1.76 million. Dividing by the estimated labor loss of 1.31 FTEs yields a marginal product of approximately $1.34 million per physician per year. Relative to an average physician wage of $150,000 toward the end of the period, this implies a value-to-wage ratio of 8.9.[25]

Inverting the logic of our back-of-the-envelope calculation, we estimate that each physician FTE annually saves approximately 13.4 life years, implying a cost of $11,200 per life year saved. This is substantially lower than the VSLY of $100,000 and also below the corresponding estimate of $17,600 per life year saved for nurses in Friedrich and Hackmann (2021a). These findings reinforce the conclusion that clinical staffing levels—particularly for physicians—may be inefficiently low from a social perspective.

These estimates imply that physicians are considerably more underpaid relative to their social value than nurses.[26] The value-to-wage ratio for physicians in our setting is 8.9, compared to a ratio of roughly 2 for nurses in Friedrich and Hackmann (2021a). This highlights the central role of physicians in preserving life and suggests that the marginal social product of physician labor far exceeds both their compensation and that of other core healthcare workers. That said, this estimate may still overstate the true marginal product of physicians, as it does not account for longer-term health effects associated with disruptions to continuity of care (such that the commuting effects is greater than the reduction in physician supply). Moreover, physicians' expertise is often complementary to the work of other healthcare workers, such that their absence could reduce the productivity or effectiveness of the broader care team.

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. The effects of this "brain drain" are not just limited to the loss of talent; they can ripple through public services, healthcare systems, and overall welfare. This is particularly crucial in sectors like healthcare, where shortages of skilled workers can directly impact service delivery and health outcomes. As labor mobility increases, understanding these dynamics becomes essential to gauge the broader implications for both sending and receiving countries.

In this study, we examine the welfare consequences of brain drain in the healthcare sector, focusing on the commuting behavior of physicians between Sweden and Norway. By leveraging an exogenous wage shock in Norway triggered by a surge in oil prices, we explore how shifts

[25]The exchange rate at the time was approximately $1:6.1 SEK.

[26]If anything, our estimates may be conservative, as they reflect a relatively healthier population at baseline.

in physician commuting influenced health outcomes and hospital capacity in Sweden. Using a dose-response difference-in-differences framework, we isolate the effects of physician mobility on mortality rates, healthcare access, and quality of care, offering new insights into the consequences of moving the human capital and expertise for both the origin and destination countries.

Our key findings reveal significant and uneven consequences of brain drain. While Sweden experienced a notable increase in mortality rates due to the outflow of physicians, particularly among elderly patients and those needing urgent care, Norway did not see similar improvements despite an influx of physicians. We also observe that the negative effects on healthcare quality were most pronounced for low-income and less-educated individuals, whose reliance on public healthcare systems likely made them more vulnerable to disruptions in service provision. These findings point to the complex dynamics of brain drain, highlighting how the loss of particularly skilled workers can exacerbate regional inequalities and disproportionately impact disadvantaged groups.

In terms of mechanisms, we find that the increased commuting is driven by young, high-productivity generalists, who are more likely than specialists to work the floor in emergency departments. This results in a shortage of experienced physicians in these critical areas, consistent with the mortality effects being driven by time-sensitive respiratory, circulatory, and infectious diseases. As a consequence, we observe increased hospitalization rates and earlier discharges to manage capacity. These discharges are premature, leading to a notable rise in re-admissions as patients are sent home before fully recovering or fully resolving their illness. This sequence of events underscores the impact of physician shortages on patient care and health outcomes, and highlights how the exit of critical personnel can generate a snowball effect across entire organizations.

This paper contributes to the literature by offering causal evidence of the health and welfare costs of brain drain, particularly in the healthcare sector. It moves beyond individual-level migration effects to provide a comprehensive understanding of how skilled migration disrupts public services and amplifies inequality, particularly when public systems are rigid in how they can respond to wage competition. The study also highlights the need for a broader consideration of the systemic impacts of commuting and migration behavior, offering a more nuanced perspective on the consequences of labor mobility across borders.

In terms of policy implications, our results emphasize the importance of policymakers recognizing the complex effects of brain drain, particularly in critical sectors like healthcare. For sending countries, strategies that mitigate the loss of healthcare professionals while still allowing for mobility can help balance the economic benefits of migration and cross-border work with the need to maintain robust public services. This study adds urgency to the need for policies that better integrate the welfare implications of skilled worker movement into broader economic and healthcare planning as well as the management of human capital in strategically critical sectors.

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Tables and Figures

Table 1: Commuting and Other Demographic Variables

	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 2014. 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. Standard errors in parentheses. ** denotes significance at the 5% level.

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 2014. 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. Standard errors in parentheses. ***, ** denote significance at the 1% and 5% levels, respectively.

Table 3: 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 2014. 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. "Low density" is defined as having physician density below the sample median in 2003. Standard errors in parentheses. ***, ** denote significance at the 1% and 5% levels, respectively.

Table 4: 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
Observations	3,475	3,475	3,475	3,059

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 5: Causes of Death

Panel A: Aggregate Categories

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

Panel B: Detailed Categories

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

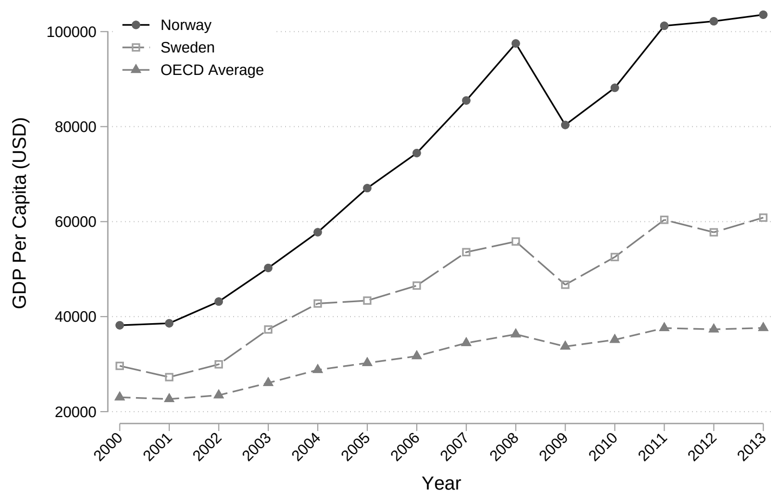
Source: Authors' calculations using Swedish registry data from 2001 to 2014. Sample includes 3,059 observations (municipalities × 14 years).
Notes: RIC aggregates respiratory, infectious, and circulatory causes of death. CEDMN aggregates cancer, endocrine, digestive, muscular, and nerve-related causes of death. 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 6: Mechanisms: Hospitalization Responses

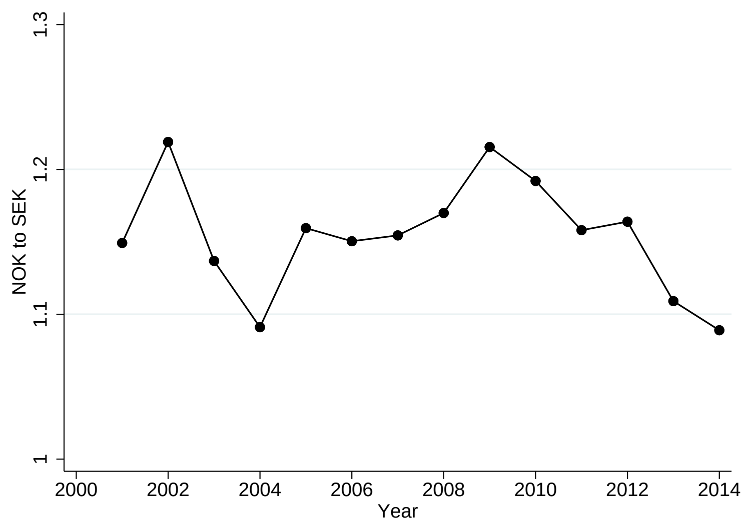
	Hosp.	Hosp. length	Time between	Surgeries
	(1)	(2)	(3)	(4)
Post $\times$ Treat	14.183***	-0.429**	-3.036*	0.005
	(5.025)	(0.178)	(1.730)	(0.006)
Mean	203.19	6.532	62.885	0.072
Observations	3,059	3,059	3,059	3,059

Source: Authors' calculations using Swedish registry data from 2001 to 2014. Sample includes 3,059 observations (municipalities $\times$ 14 years).
Notes: Hosp: (Number of hospitalizations * 1000 / base pop). Hosp. length: hospitalization spell length. Time between: days between hospitalization events (same year).
Standard errors in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Figure 1: Macroeconomic Development Over Time Across Sweden and Norway



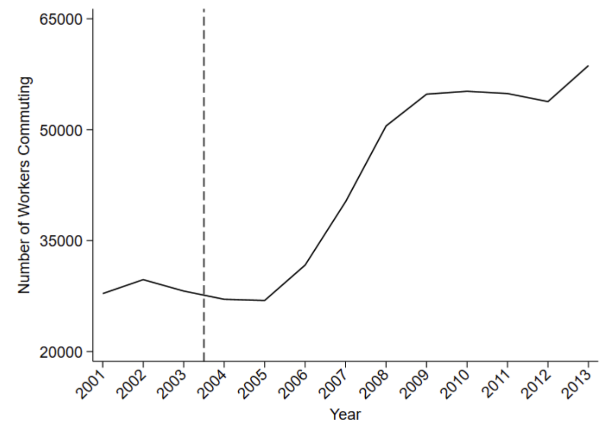
(a) GDP Growth



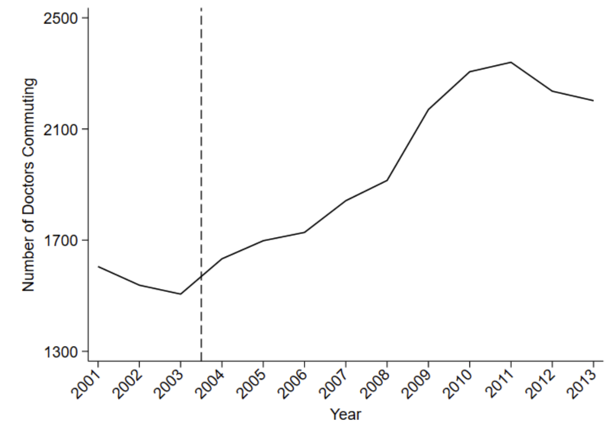
(b) Exchange Rate

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

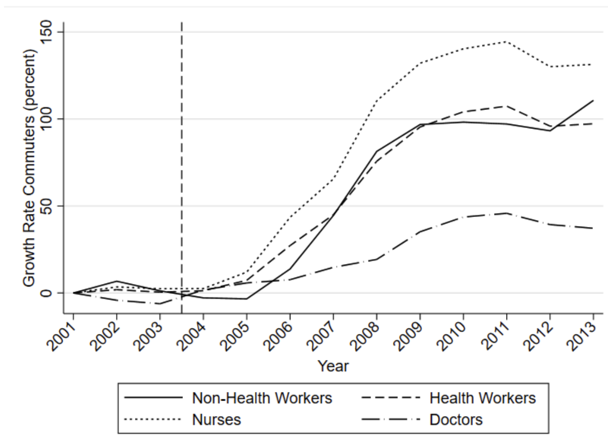
Figure 2: Swedish Commuting Response



(a) All workers



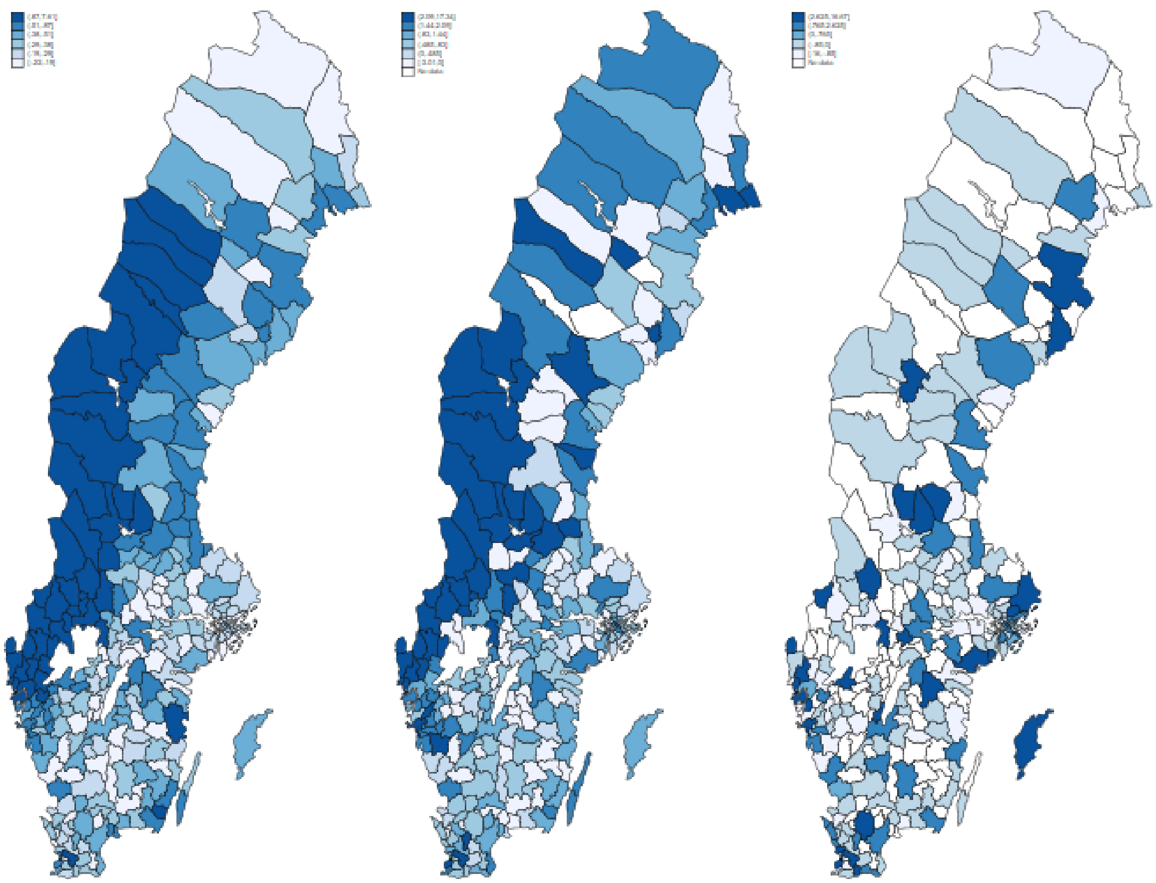
(b) Doctors



(c) Growth rate by sector

Source: Authors' calculations of Swedish register data.
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 sectors.

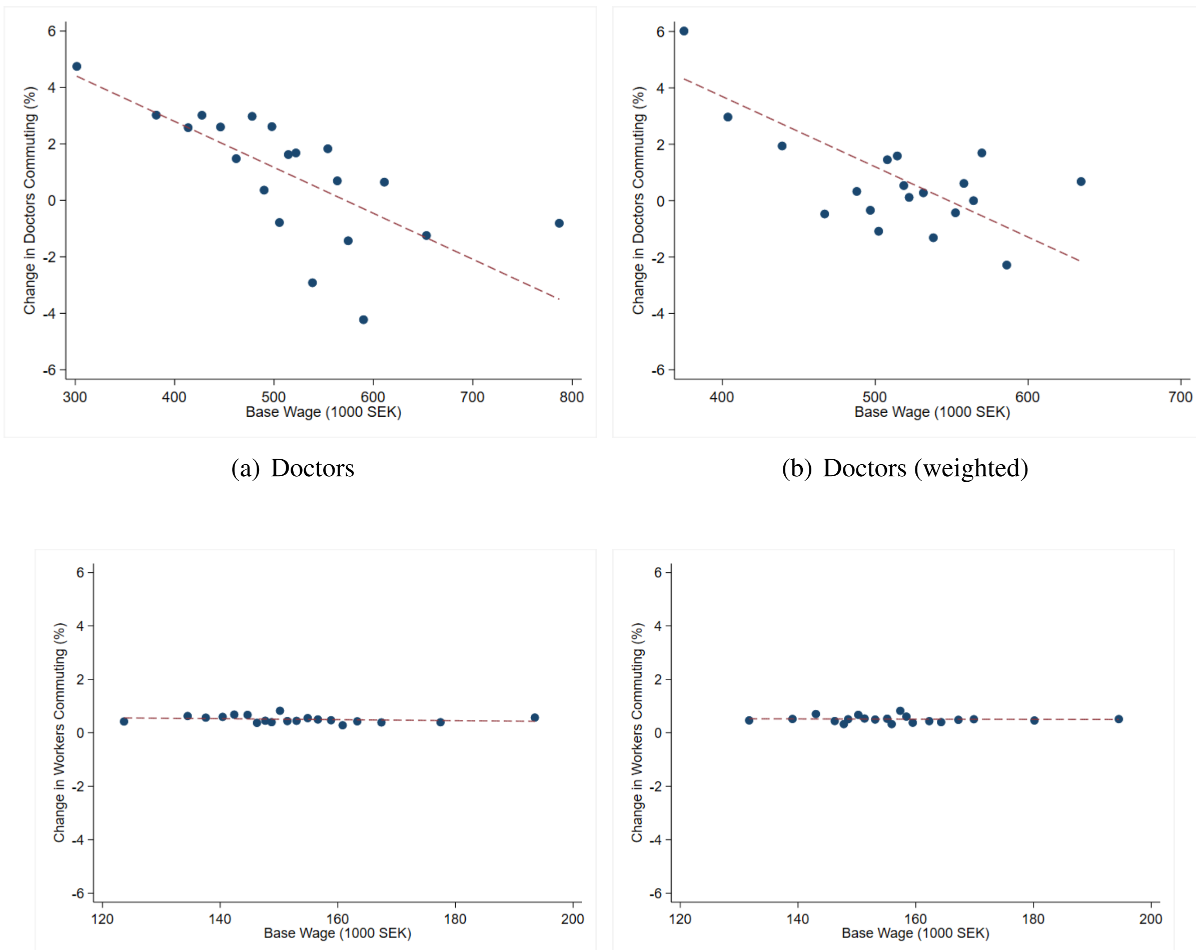
Figure 3: Swedish Commuting Response by Occupation



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

Source: Authors' calculations of Swedish register data.
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 4: Swedish Commuting Response: Wage and Distance Effects



(a) Doctors

(b) Doctors (weighted)

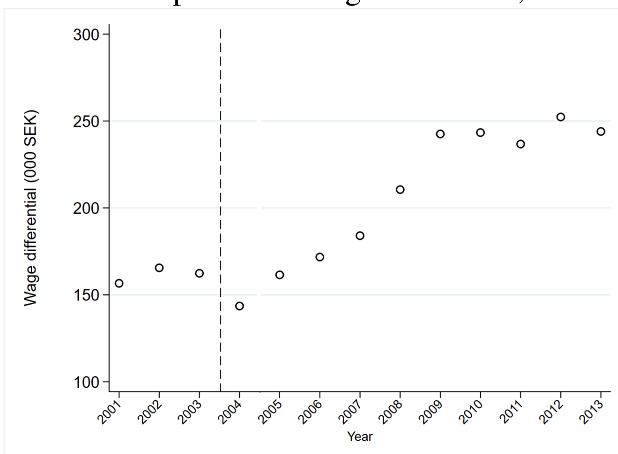
(c) Non-Doctors

(d) Non-Doctors (weighted)

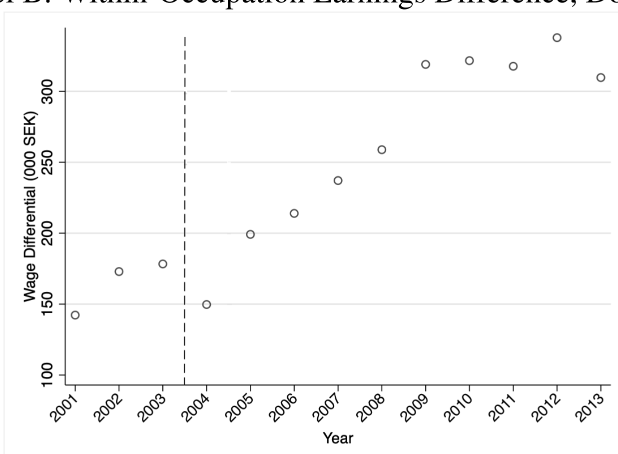
Source: Authors' calculations of Swedish register data.
Notes: Panels (a) and (b) show the commuting response for doctors with and without frequency weights for the size of the municipality. Panels (c) and (d) show the response for non-doctors. Doctors respond to wage returns, while non-doctors do not.

Figure 5: Shock to Labor Market Competition: Occupation Earnings Differential

Panel A: Within-Occupation Earnings Difference, All Occupations



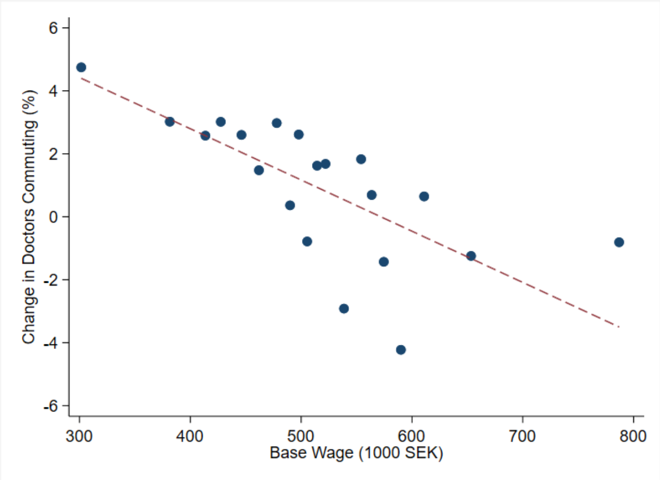
Panel B: Within-Occupation Earnings Difference, Doctors



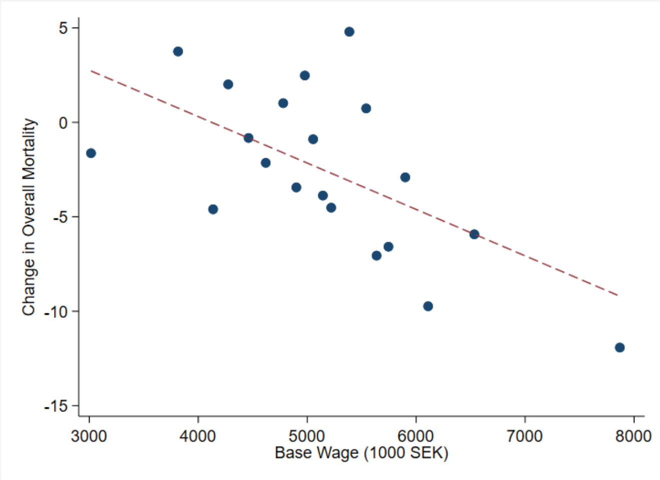
Source: Authors' calculations of Swedish register data.

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.

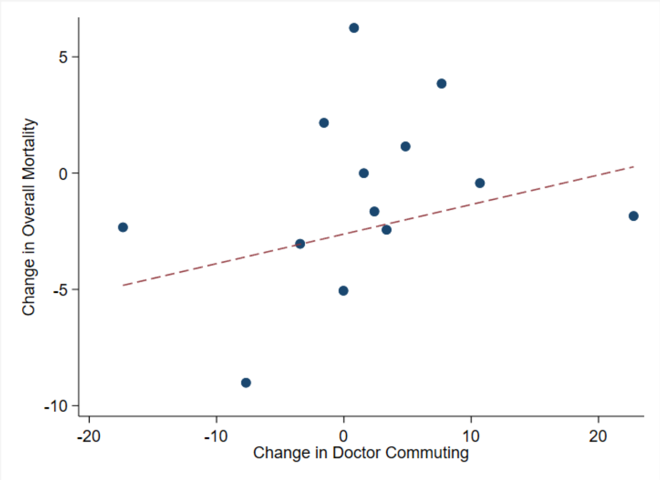
Figure 6: Base Pay, Commuting, and Mortality



(a) Commuting vs Base Pay



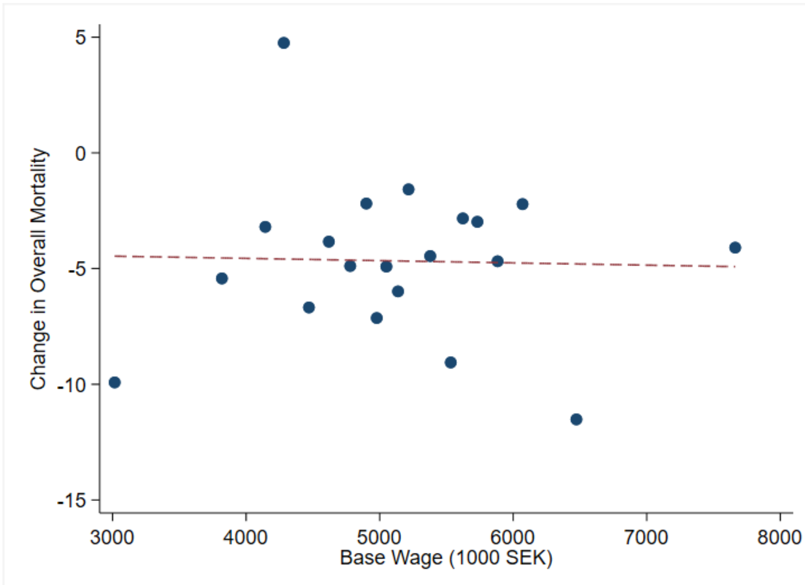
(b) Mortality vs Base Pay



(c) Mortality vs Commuting

Source: Authors' calculations of Swedish register data.
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.

Figure 7: Placebo: Base Pay, and pre-Mortality

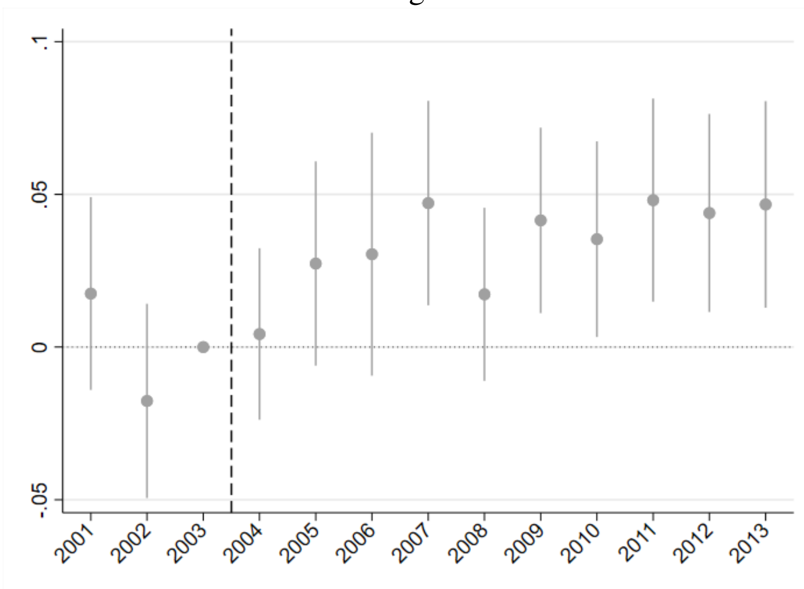


(a) Commuting vs Base Pay

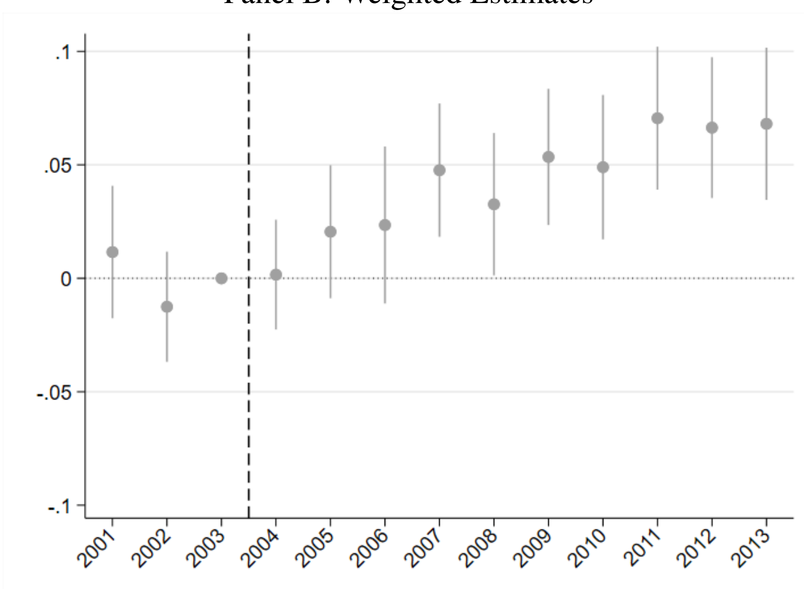
Source: Authors' calculations of Swedish register data.
Notes: Each data point represents the change in the outcome variable for a municipality between 1994 and 2003, plotted against the base doctor wage in the municipality in 2003. The dotted line represents an OLS regression of the outcome variable on the base doctor wage in 2003, weighted by population size.

Figure 8: Share of Doctors Commuting

Panel A: Unweighted Estimates



Panel B: Weighted Estimates

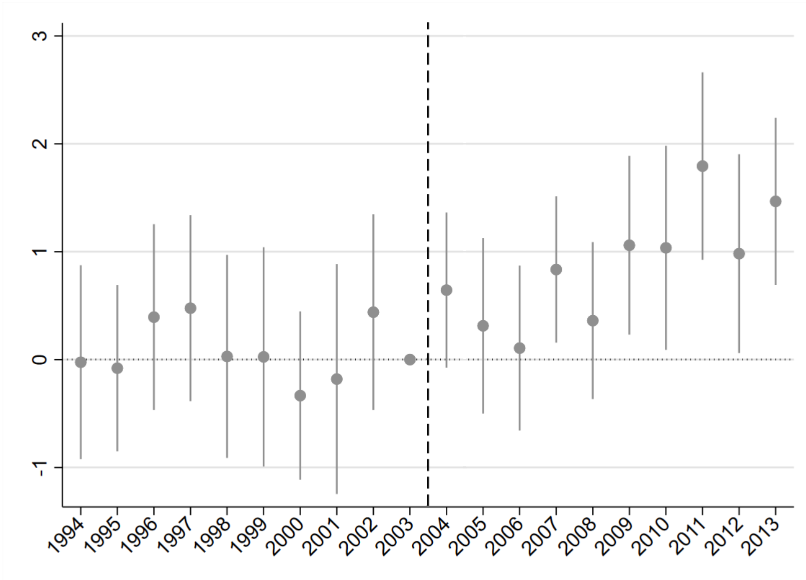


Source: Authors' calculations of Swedish register data.

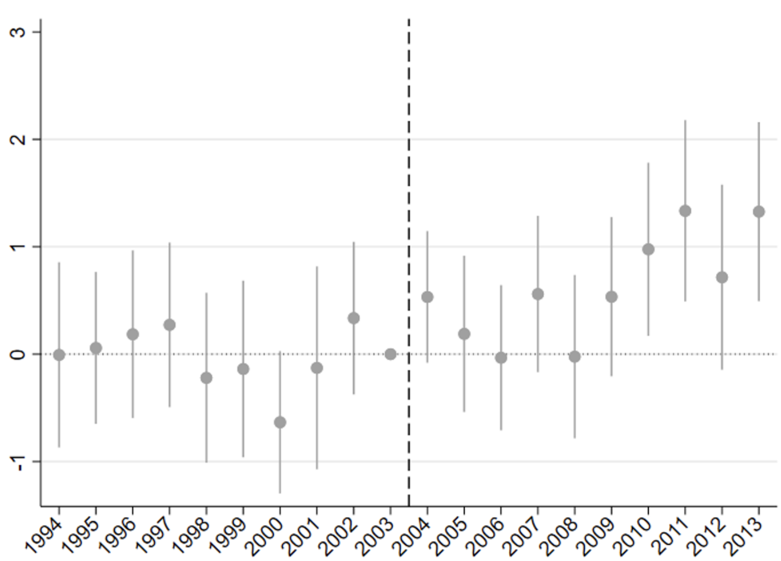
Notes: Each point represents the coefficients from Equation 1. The "treat" variable is $-1\times$ the log of average physician base pay in the municipality. The lines represent 95% confidence intervals. The dashed line is the year physician wages began growing quickly in Norway. Panel B estimates are weighted by total population in 2003. The estimates in Panel B are a closer approximation of the individual commuting responses estimated in Table A1.

Figure 9: Mortality Rates per 1,000

Panel A: Unweighted Estimates



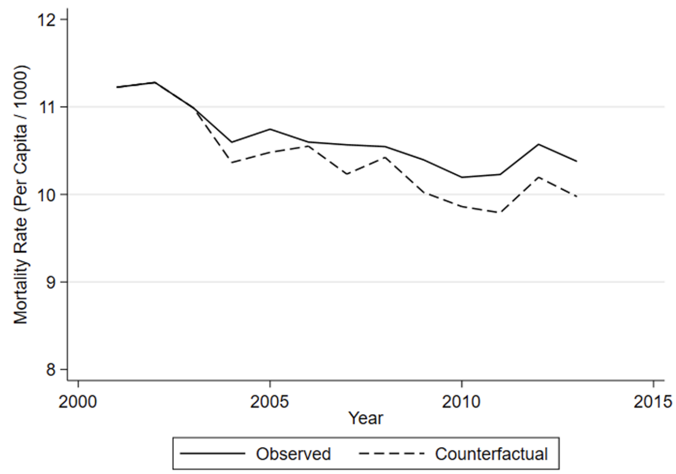
Panel B: Weighted Estimates



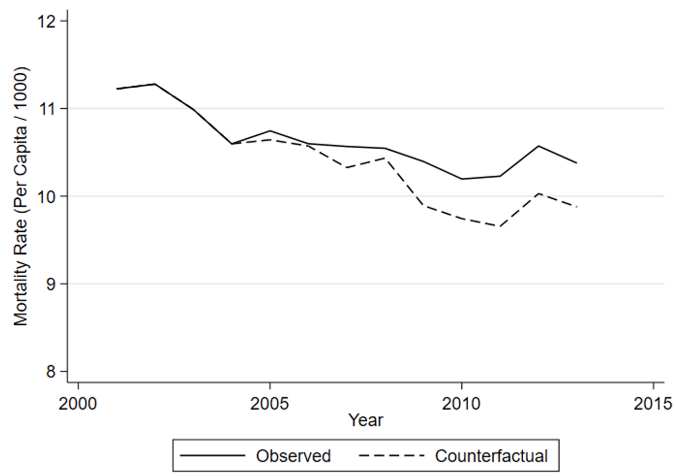
Source: Authors' calculations of Swedish register data.
Notes: Each point represents the coefficients from Equation 1. The "treat" variable is $-1\times$ the log of average physician base pay in the municipality. The lines represent 95% confidence intervals. The dashed line is the year physician wages began growing quickly in Norway. Right figure shows results from a version of Equation 1 using base year population as weights. Left figure is unweighted.

Figure 10: Counterfactual Total Mortality Rates in Sweden

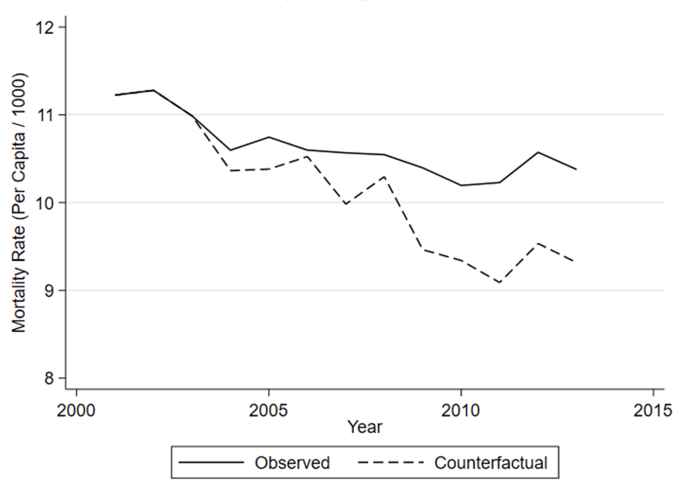
Panel A: Counterfactual if Swedish Wages Fixed at 2004 Norwegian Levels



Panel B: Counterfactual if Sweden-Norway Wage Gap Fixed at 2004 Level



Panel C: Counterfactual if Swedish Wages Equaled Norwegian Wages from 2004-2013



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

A Online Appendix (not for publication)

Table A1: 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				

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 A2: 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
Observations (N)	3057	3057	3057	3057

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 A3: 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)
Observations (N)	3761	3761	3761	3761	3761

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 A4: Effect of Norwegian Labor Demand Shock Across Panels

	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 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 A5: Effect of Norwegian Labor Demand Shock on New and Immigrant Doctors

	Number of New Doctors	Number of New Immigrant Doctors
	(1)	(2)
Post × 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 A6: Descriptive Statistics by Group (Averages Across Entire Sample Period)

Panel A: Swedish Commuter and Non-Commuter Characteristics				
Variable	All Commuters (1)	All Non-Commuters (2)	Doctor Commuters (3)	Doctor Non-Commuters (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 2014.
Notes: Standard deviations in parentheses in Panel A.

Table A7: 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.
Notes: Standard deviations in parentheses in columns 1-3. Standard errors of a t-test in parentheses in column 3.

Table A8: 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
Observations (N)	3057	3057

Source: Authors' calculations using Swedish registry data.
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 A9: Placebo: Commuting Effect based on wage trends

	Share of Doctors Commuting	Share of Doctors Commuting
	(1)	(2)
Post $\times$ $[Wage_{Doc,2003} - Wage_{Doc,2001}]$	0.0006	
	(0.00004)	
Post $\times$ $[\frac{Wage_{Doc,2003} - Wage_{Doc,2001}}{Wage_{Doc,2001}} \times 100]$		-5.28e-07
		(1.86e-06)
Mean	0.031	0.031
Observations (N)	3057	3057

Source: Authors' calculations using Swedish registry data.
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 A10: Effects on Immigrant Population

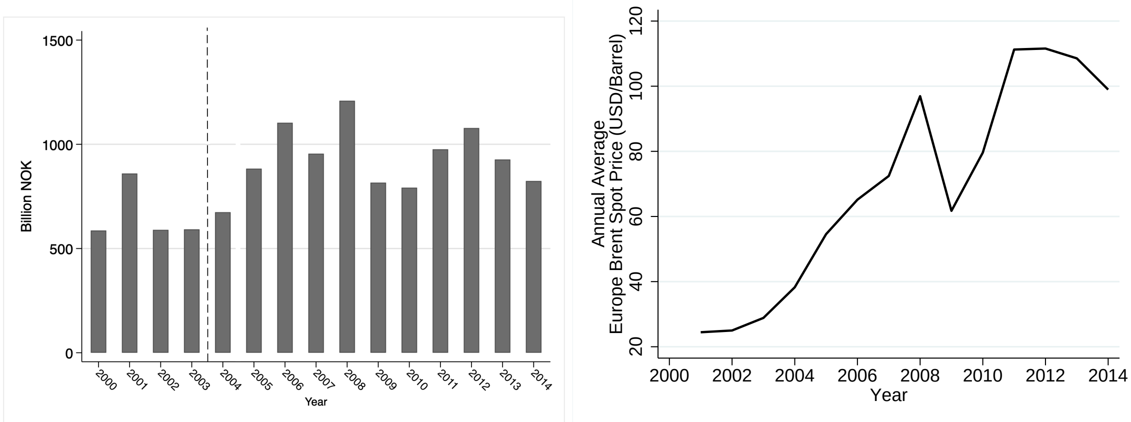
	Number Immigrants	Share Immigrants
	(1)	(2)
Post × Treat	34.591	0.004
	(141.150)	(0.003)
Mean	1871.085	0.069

Source: Authors' calculations using Swedish registry data.
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.

Figure A1: Norwegian Government Oil Revenue and Oil Prices Over Time

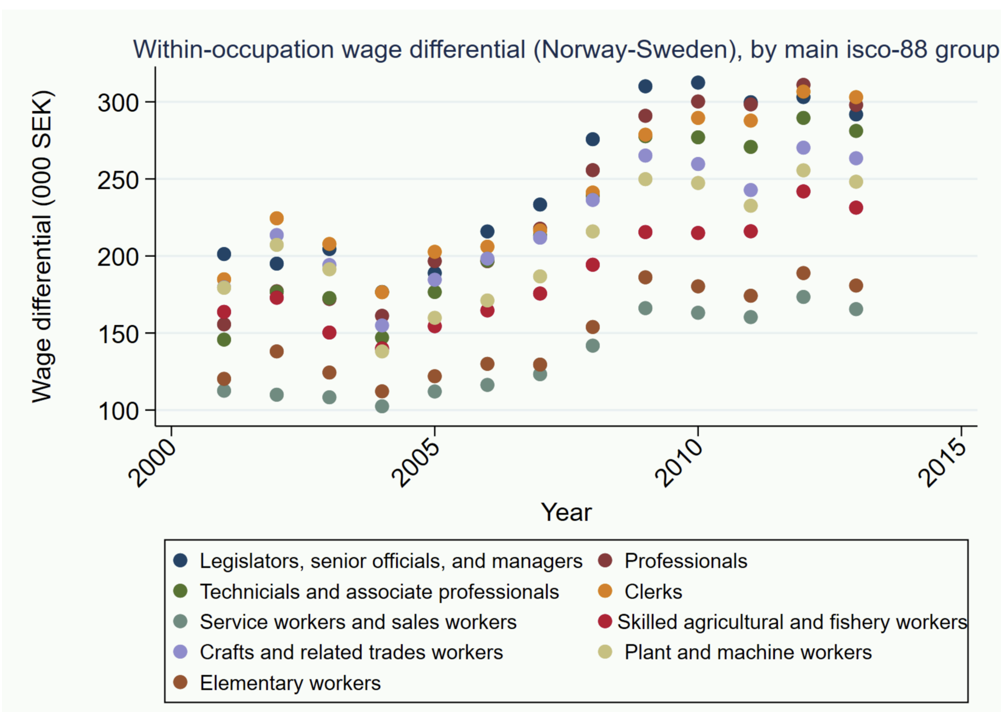
Panel A: Norwegian Oil Revenue Over Time

Panel B: Europe Brent Spot Oil Prices



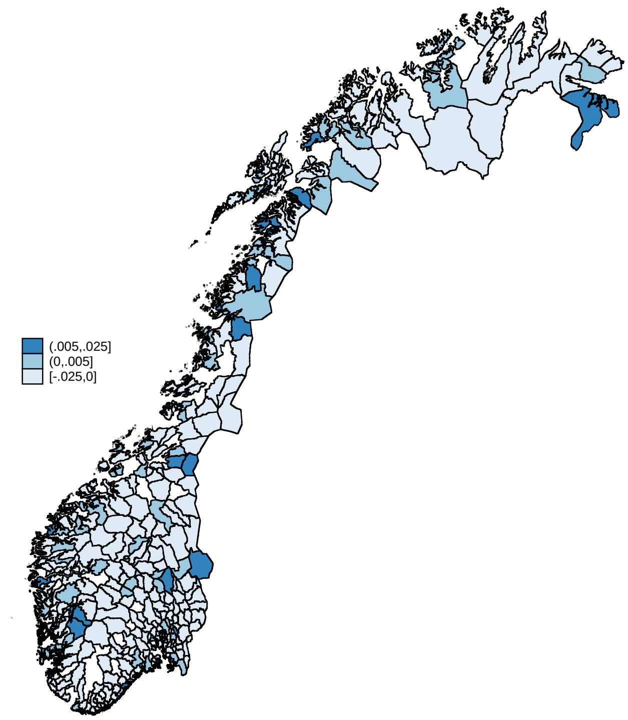
Source: Authors' calculations based on Norwegian government data.
Notes: The figure presents the oil revenue collected by the Norwegian government over time, illustrating trends in resource-based income.

Figure A2: Shock to Labor Market Competition: Occupation Earnings Differential by ISCO88 Group



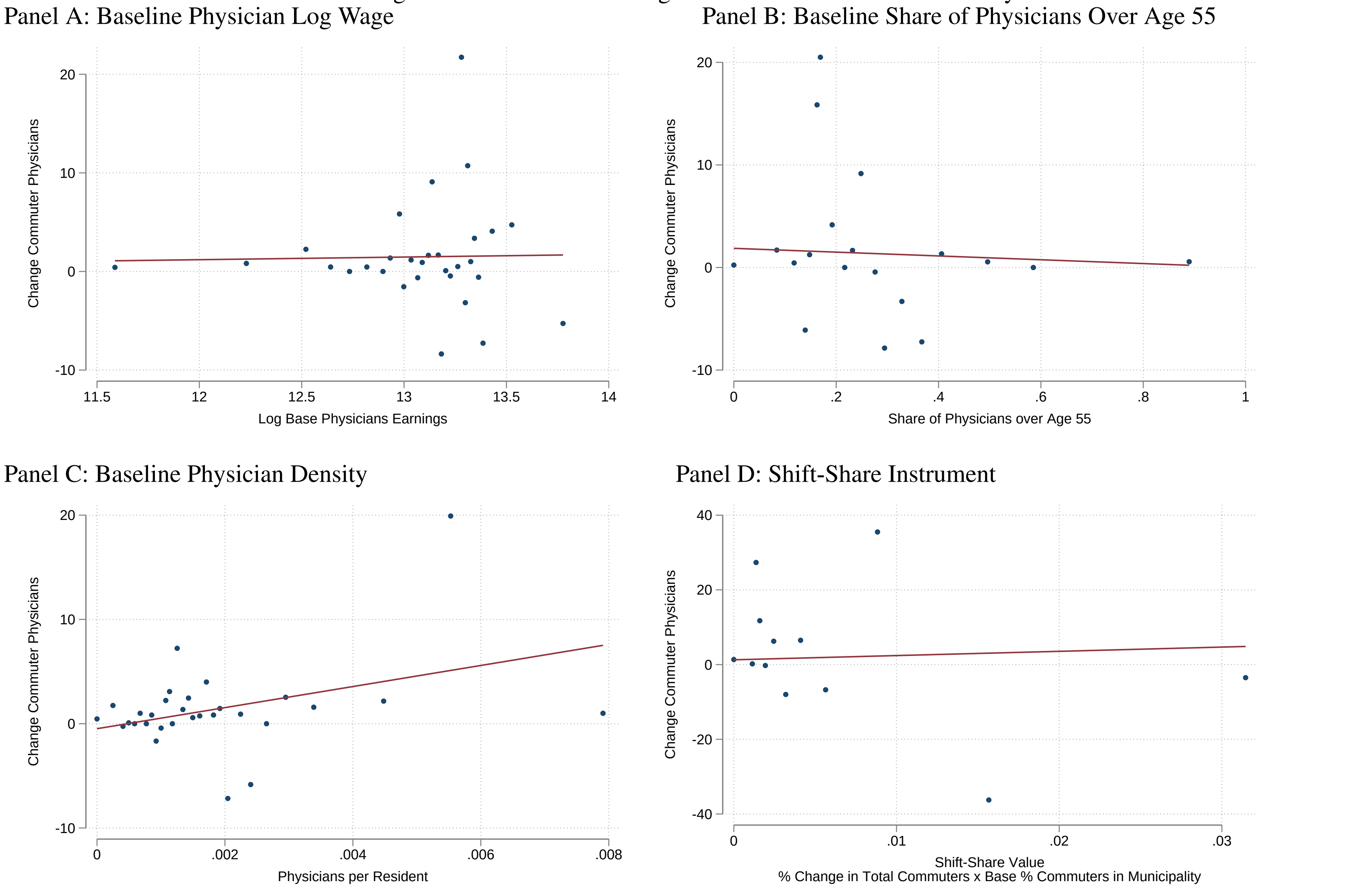
Source: Authors' calculations of Swedish register data.
Notes: The figure presents occupation earnings differentials adjusted for purchasing power, broken down by ISCO88 occupational groups. This highlights how different skill-based job categories respond to labor market competition.

Figure A3: Destination of Commuting Doctors



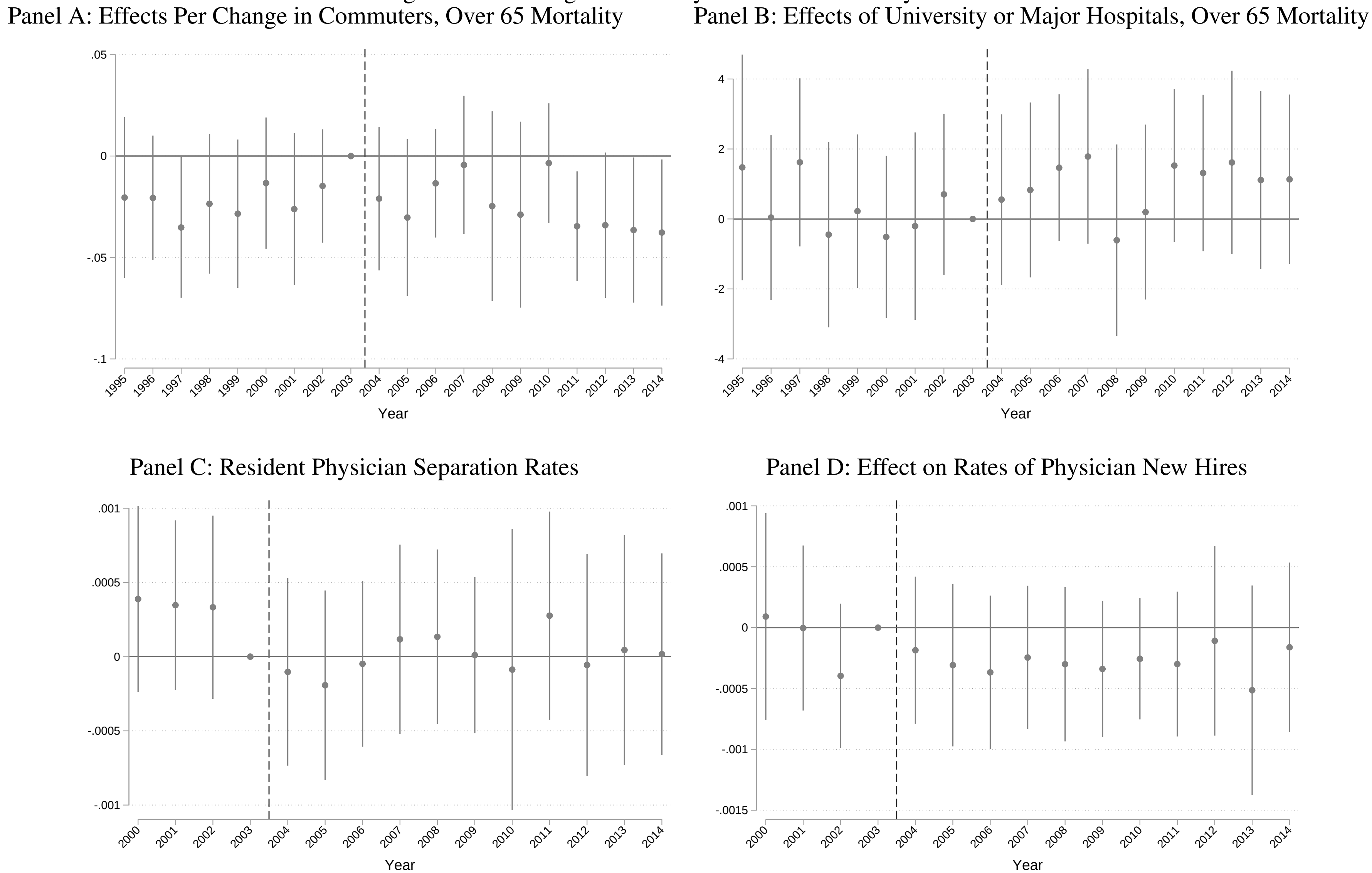
Source: Authors' calculations of Norwegian register data.
Notes: Panels (a), (b) illustrate the commuting response for nurses, and doctors, respectively, as a share of baseline workers. The geographic nature of the response varies across groups.

Figure A4: Factors Predicting Commuter Destination in Norway



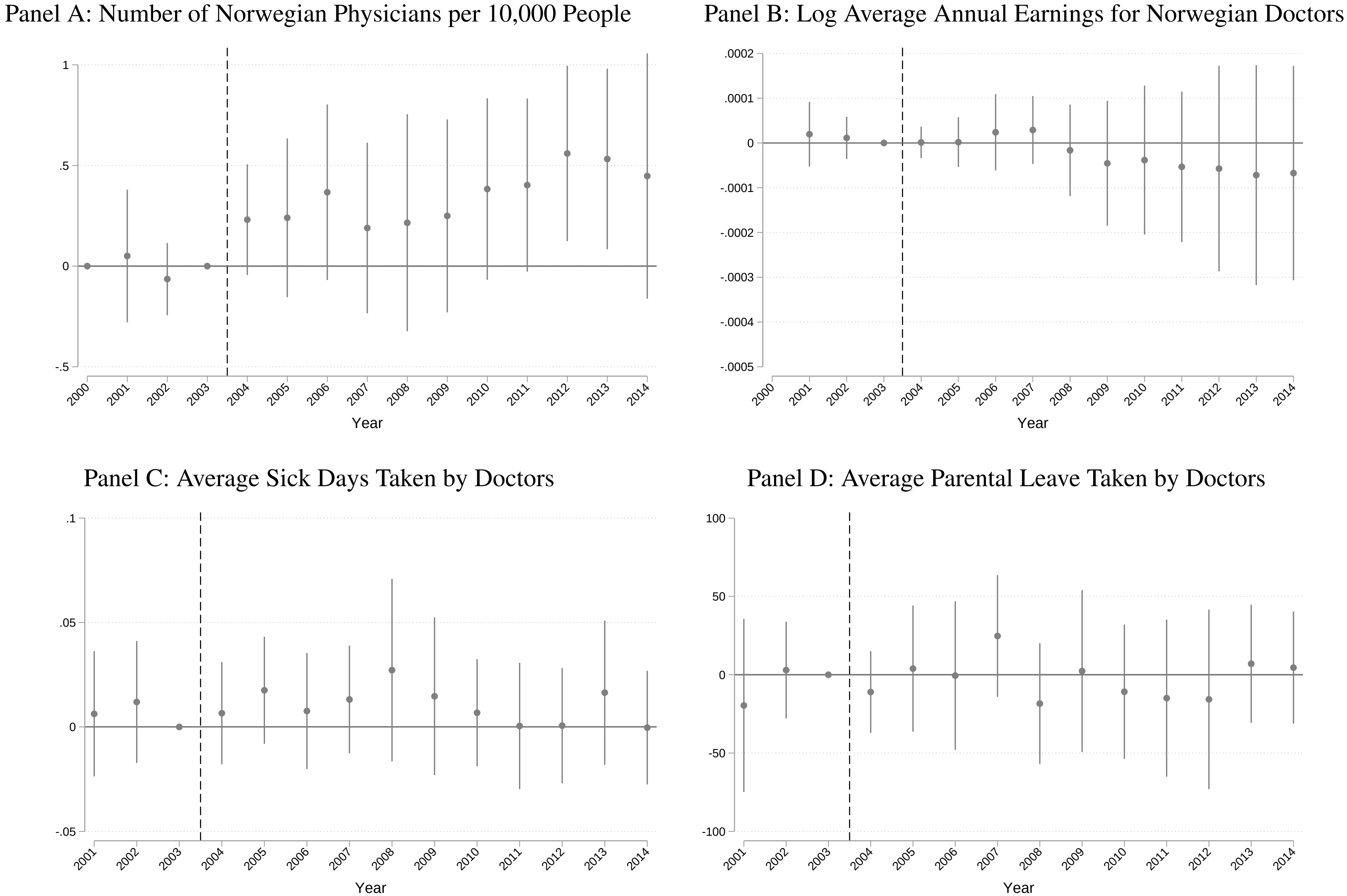
Source: Authors' calculations of Norwegian registry data.
Notes: The shift-share instrument in Panel D is the predicted number of physician commuters base on interacting the baseline share of total commuters in municipality X by the total percent increase in commuters from 2005 to 2013.

Figure A5: Norwegian Mortality Effects: Event Study Results



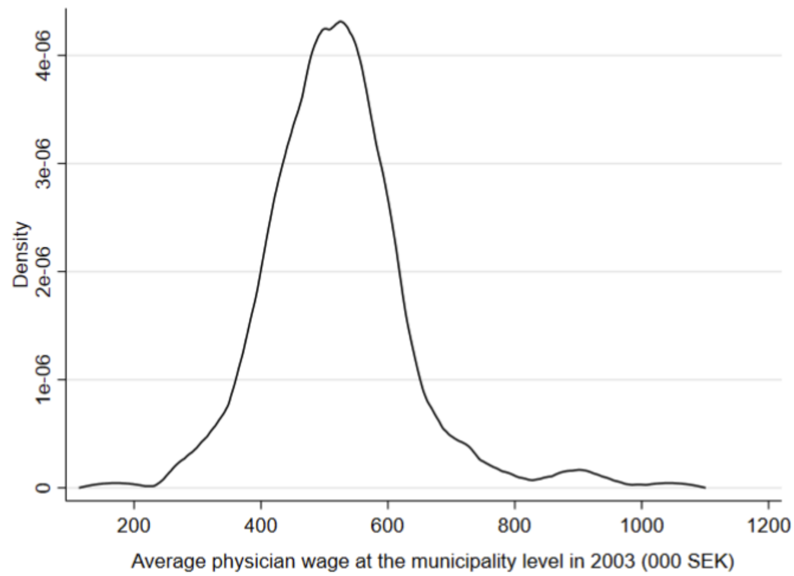
Source: Authors' calculations of Norwegian registry data.
Notes: Panel A estimates the interaction between year dummies and the 2005-2013 change in the number of Swedish commuting physicians to the municipality. Panel B interacts year dummies with an indicator for having a university or major hospital in the municipality as these were disproportionately likely to attract new commuters. Panels C and D follow the specification in Panel A, but replacing physician separation and hiring rates as the outcome variable. 95% confidence intervals are based on clustering at the municipality level.

Figure A6: Effects on Work Effort by Norwegian Physicians: Event Study Results
Effects Per Change in Commuters



Source: Authors' calculations of Norwegian registry data.
Notes: Each panel estimates the interaction between year dummies and the 2005-2013 change in the number of Swedish commuting physicians to the municipality. 95% confidence intervals are based on clustering at the municipality level.

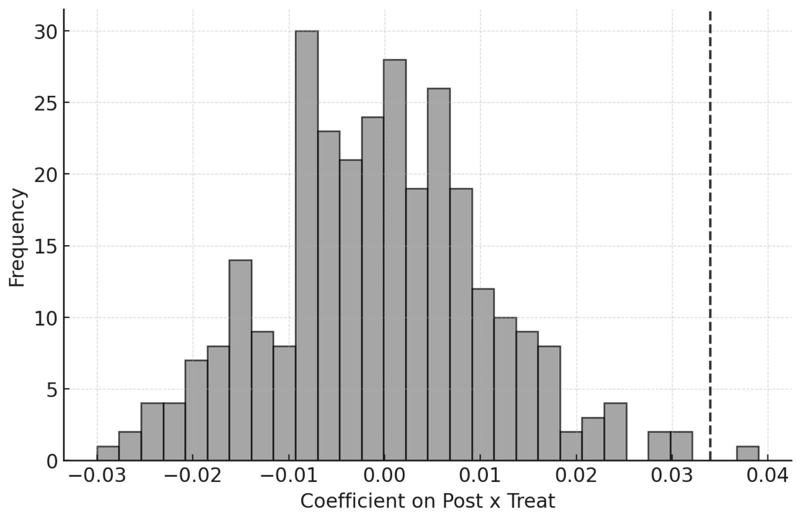
Figure A7: Density of Physician Pay in 2003



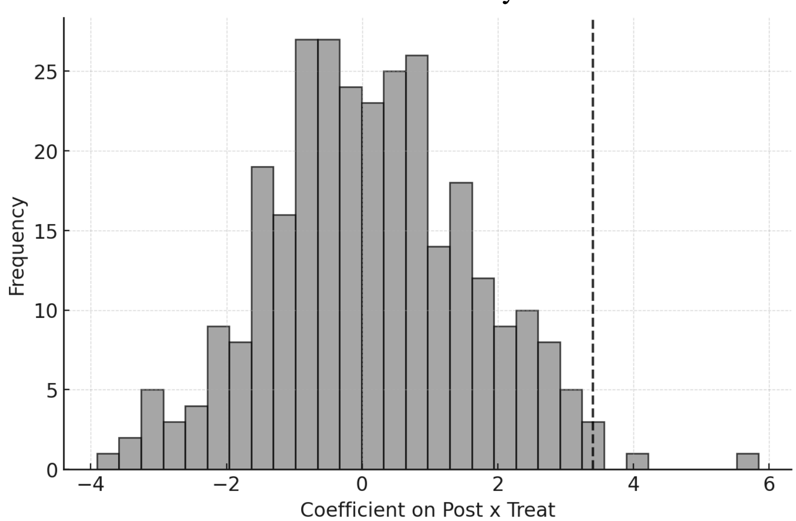
Source: Authors' calculations of Swedish registry data.
Notes:

Figure A8: Permutation Checks, 300 iterations

Panel A: Doctor Commuting

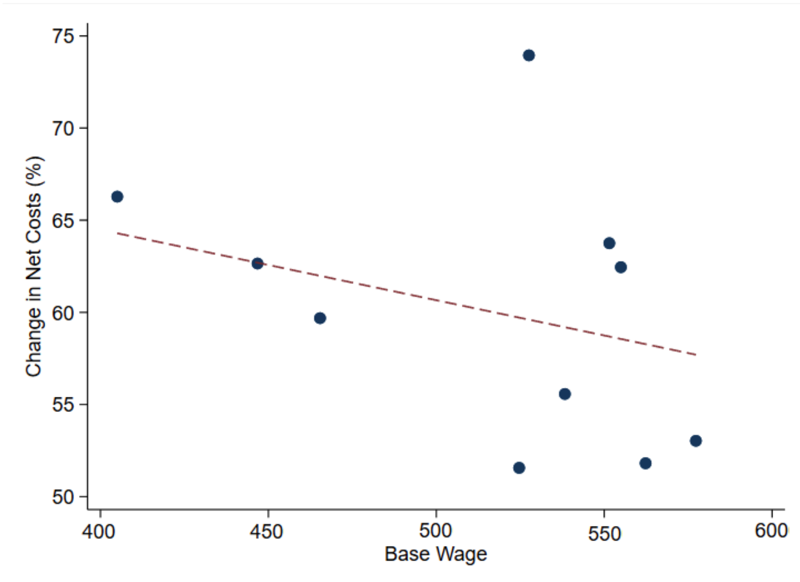


Panel B: Mortality



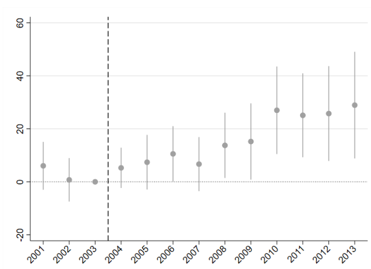
Source: Authors' calculations of Swedish registry data.
Notes: These show the distribution of the β_2 coefficient from Equation 2 after randomly shuffling the value of the "treat" variable across municipalities. The "treat" variable is $-1\times$ the log of average physician base pay in the municipality. 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 A9: Net Cost at the Health Region Level

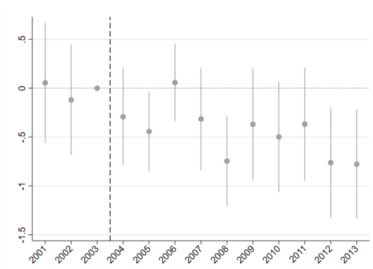


Source: Authors' calculations of digitalized data on health care costs from SKL.
Notes: This figure shows the change in the net cost of healthcare (costs net of revenues) between 2013 and 2003 as a function of the physician base wage. There are 21 health regions in the country.

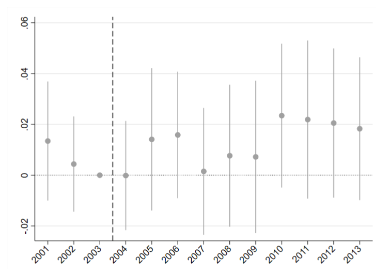
Figure A10: Event Studies - Hospitalization outcomes



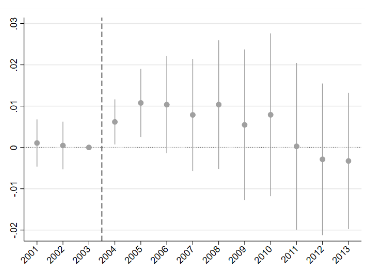
(a) Hosp.



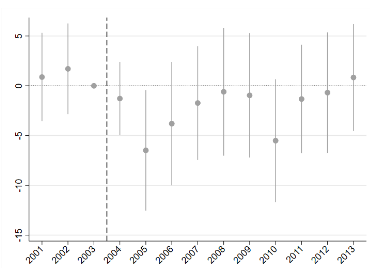
(b) Hosp. length



(c) Hosp. per person



(d) Surgeries



(e) Time between hospitalizations

Source: Authors' calculations of Swedish register data.
Notes: The coefficients correspond to the event study specification in Equation 1 for each outcome.