CESifo WORKING PAPERS

10747
2023

November 2023

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CESifo

Impressum:

CESifo Working Papers
ISSN 2364-1428 (electronic version)
Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH
The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute
Poschingerstr. 5, 81679 Munich, Germany
Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de
Editor: Clemens Fuest
https://www.cesifo.org/en/wp
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CESifo *Working Paper No. 10747*

Skilled Labour Migration and Firm Performance: Evidence from English Hospitals and Brexit

Abstract

How do skilled migrant workers affect firms' performance and output? I estimate the causal effect of EU nurse withdrawal after the Brexit referendum on the performance of English hospitals. Exploiting variation in the reliance on EU workers across hospital providers in pre-referendum years, I find that providers with a mean share of EU nurses before the referendum persistently face 2% more hospital-related deaths after the referendum. This translates to 5,900 additional hospital-related deaths p.a. in England. Unexpected readmissions of patients increase by 5% and reported incidents with harm to patients by 7% respectively. Providers respond to missing EU nurses by hiring UK nurses and fostering promotions in the short run, and by recruiting non-European nurses in the long run.

JEL-Codes: J240, J610, I180.

Keywords: skilled labour shortage, public healthcare, e-/immigration, Brexit.

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November 2023
I thank Anthony Edo, Benjamin Elsner, Peter Egger, Katharina Erhardt, Christina Gathmann, Daniel Kamhöfer, Andreas Lichter, Katrine Loken, Marco Mello, Dennis Novy, Andreas Peichl, Amelie Schiprowski, Jan Stuhler, Jens Südekum, Andrea Weber and Biliana Yontcheva for very helpful discussions about this paper. The paper has also been presented in the DICE Applied Micro Group in Spring 2022 and Summer 2023, the DICE PhD Workshop in Spring 2022, the DICE Brown Bag in Summer 2023, at the RGS PhD Conference 2023, at the Hohenheimer Oberseminar 2022, Düsseldorf, in the EuHEA Seminar Series Fall 2022, at the dggö Jahrestagung 2023, Hannover, and the at the CESifo Junior Economist Workshop on Migration Research 2023. I am very thankful for any comments, that I received there. This project is funded by the Deutsche Forschungsgemeinschaft (DFG, German Research Foundation) – 235577387/GRK1974. All remaining errors are my own.

1 Motivation

The shortage of skilled labour is one of the main bottlenecks which is expected to harm growth in Western economies in the upcoming decades. Even today, the US and EU member states are already lacking skilled workers in many professions such as construction, healthcare, or manufacturing, and face hiking job opening rates (U.S. Bureau of Labor Statistics, 2023; European Labour Authority, 2023; Eurostat, 2023a). In many countries, this pattern will persist and likely be reinforced in light of the current and future demographic trends. In the European Union, the working-age population is predicted to decline by 6% until 2040 (Eurostat, 2023b) and OECD countries are foreseen to lose on average 10% of their working-age population by 2060 (OECD, 2021).

Policymakers have been trying to tackle this challenge along various margins. Among others, by facilitating labour market participation, by incentivizing longer working hours or later retirement, and by improving the match between the labour demanded and supplied.[1] However, there is a broad consensus among labour economists that filling the gap of skilled labour in many Western industries is hardly possible without attracting migrant workers from other countries. To determine whether migration policies should be a central part of the policymakers' response to skilled labour shortages, their economic benefits and costs need to be quantified.

In this paper, I study how a change in the labour supply of skilled migrant workers affects firm performance and consumer rents. I examine the Brexit referendum as a persistent and large-scale, negative labour supply shock to an exceptionally tight labour market, the labour market for nurses in England. In this market, vacancy rates have been persistently at 10% and employment of foreign-trained nurses has been 2.5 times as high as the OECD average (OECD, 2017), mainly due to a lack of domestic workers.[2] I show that the reduction in skilled migrant nurses had a strong negative impact on hospital performance

[1] A recent example is Germany's 'Federal Government's skilled labour strategy' which combines up-to-date education, with targeted training, higher labour force participation, and a better working environment with migration policies (German Federal Government, 2023)

[2] The shortage of skilled nurses and healthcare workers is not unique to England. In the EU, the occupation of 'nursing professionals' is the number one field for which most EU member states report labour shortages (European Commission, 2020).

and their patients.

After the 2016 Brexit referendum, substantial uncertainty about visa requirements and the recognition of overseas qualifications led to a decrease in the number of EU nationality nurses in English hospitals by 5,000 (mainly Southern European) nurses in the five years post-referendum. This corresponds to 30% of all EU nurses or 2.5% of all nurses.

To identify the causal effect of this labour supply shock on firm behavior and consumers, I exploit quasi-experimental variation in how strongly hospitals were exposed to the withdrawal of EU nurses. Their pre-referendum share of EU nurses among all nurses ranged from 0 to more than 20%. While hospitals differ in their exposure to this shock, less and highly treated hospitals followed parallel trends in all employment and health outcomes before the Brexit referendum. As the Brexit vote was unanticipated, hospitals did not strategically adapt their workforce in advance. This allows me to compare outcomes of differently affected hospitals before and after the Brexit referendum in a difference-in-differences setup.

The institutional setting of English hospitals is ideal to answer the research question at hand. First, hospitals within the National Health Service (NHS) are subject to identical institutional guidelines (such as wage agreements and financial regulations) making them likely to follow similar outcome trends and, hence, suitable to compare. However, they are autonomous in hiring workers and, thus, might react heterogeneously to labour supply shortages. Second, the hospitals offer a labour-intensive product of substantial value to consumers as health is a basic need. Third, healthcare as the product allows me to rather objectively assess service quality and value which is hardly the case for other products like consumables where preferences and perceived quality can differ more strongly across consumers.

Building on administrative data on hospitals' employment structure and healthcare provision, I examine the impact of the withdrawal of EU nurses along various margins. First, I show that hospital performance is negatively impacted by worker withdrawal and substitution. For a hospital provider with a mean pre-referendum share of EU nurses hospital-related deaths increased by 2% after the referendum. Hospital-related deaths encompass around 60% of all registered deaths in England. The estimate translates to around 5,900 additional, hospital-related deaths p.a. in England implying 1.36 deaths p.a. for each net-lost EU nurse from 2016 to 2019. This, for example, exceeds the number of 4,000 non-COVID excess deaths during the first year of the COVID-19 pandemic in English hospitals (Fetzer and Rauh, 2022).[3]

Second, I show that the number of unexpected readmissions to a hospital within 30 days after a discharge increases by around 5% for a provider with mean exposure or 70,000 readmissions p.a. overall. In addition, the number of incidents with harm to patients reported increases by around 7% for providers with mean exposure or 30,500 additional incidents reported p.a. in total. Similar to the rise in mortality, more readmissions and incidents are a sign of decreasing healthcare quality (e.g., Gruber and Kleiner (2012)).

Third, I unveil the institutional responses to the withdrawal of EU nurses. I show that providers compensate for missing EU nurses by hiring UK nurses in the very short run and by attracting non-European nurses 2-3 years after the referendum. The hospitals immediately close the employment gap so that no absolute employment deficit arises. Newly hired, non-European nurses are employed in lower wage groups which could be interpreted as a sign of lower qualification (Cortes and Pan, 2015). Further, nurses employed by highly exposed providers are more likely to be promoted into a higher wage group after the referendum. As wages are fixed across providers, promotions likely reflect a wage channel and explain how additional UK nurses are attracted. This is supported by the fact that the number of promotions largely exceeds the mechanical increase of promotions after the EU nurse withdrawal. Hence, worker substitution affects hospitals' demand for incumbent workers (see, e.g., Jäger and Heining (2022) for a similar firm response in the replacement of German worker exits).

Fourth, I ensure that the drop in hospital quality was likely not induced by capacity constraints such as a lack of staff instead of a human capital loss. I show that the number of patients treated does not change with the intensity of the shock. This is in line with the

[3]By estimating the contribution of foreign healthcare workers to the English public healthcare system, I also inform the policymaker about the trade-off between educating domestic nurses or recruiting nurses from overseas. Educating a nurse costs the government more than twice the recruiting costs of studied nurses from overseas (Palmer et al., 2021) or up to 70,000 pounds in absolute terms (NHS, 2017).

fact that neither the infrastructural capacity (e.g., number of beds and operation theatres) nor the overall number of nurses changes with the referendum. These facts distinguish this paper from previous research studying a short-run lack of nurses such as Friedrich and Hackmann (2021) or Gruber and Kleiner (2012), who mainly exploit an actual deficit of nurses as a shock for identification.

Fifth, I ensure that the withdrawal of nurses is the main mechanism through which health-care performance is affected. Based on provider-level balance sheet data, I show that operative expenditures per patient as well as drug expenditures or the stock of health equipment were unaffected by the treatment. The latter reflects that there is no shock-induced increase in labour-saving technology (Acemoglu, 2010). I also provide evidence that the competition intensity among hospitals as well as the patient-nurse relationship did not change.

Related literature. This paper adds to several lines of research.

First, I provide evidence that public migration policies directly impact firm performance. Up to now, research on high-skilled migration policies and their impact on firm performance and innovation mainly focussed on H-1B visas in the US (Choudhury et al., 2022; Doran et al., 2022; Kerr and Lincoln, 2010; Peri et al., 2015) or accommodating policies for high-skilled workers (Hornung, 2014). Making use of non-policy, shift-share variation, papers also showed that immigrants can increase firm productivity (Mitaritonna et al., 2017; Ottaviani et al., 2018) and care home performance (Furtado and Ortega, 2023). Closest to me, Giesing and Laurentsyeva (2017) show that the emigration of workers due the opening of EU labour markets decreased firms' productivity. I am novel in studying the withdrawal of skilled migrants and quantifying its effects on consumer rents.

By studying the employment response and promotion of native nurses, I further add to a large literature on the labour market effects of migration on natives (Card, 2001; Choudhury et al., 2022; Dustmann et al., 2005, 2017; Foged and Peri, 2016; Friedberg and Hunt, 1995; Friedberg, 2001; Glitz, 2012) as well as the effects of emigration on stayers (Dustmann et al., 2015; Elsner, 2013a,b). Only little work exists on migrant workers' labour market effects in nursing markets (Furtado and Ortega, 2023). I also contribute to work on return migration which has mainly focussed on the individual incentives of migrant worker to leave the host country (Adda et al., 2022; Borjas and Bratsberg, 1996; Dustmann and Görlach, 2016). I extend this literature by explicitly studying the effect on firm performance in the host country.

Second, I quantify that leaving workers cannot be equally substituted by replacement workers. Jäger and Heining (2022) show that exogenous, permanent worker exits lead to increases in a firm's demand for the remaining incumbent workers. Being in line with their results, I find that worker exits result in wage increases for same-occupation workers. As their findings imply that incumbent workers, hence, are only imperfectly substitutable with outside workers, it is a natural question how worker exits affect firm performance. I contribute to this strand of research by identifying the effect of worker exits on firm performance, the output market and consumers. Other papers studying worker exit on firm or institutional performance either examine only short-run, transitional effects (Bertheau et al., 2022; Kuhn and Lizi, 2021), study temporary absences such as parental-leave programs (Brenoe et al., 2023; Gallen, 2019; Ginja et al., 2023; Huebener et al., 2022), look at small entities (Becker et al., 2017; Brenoe et al., 2023; Gallen, 2019), or explicitly deal with productivity spillovers between workers (Jones and Olken, 2005; Huber et al., 2021; Waldinger, 2010, 2012). I am novel in exploiting a large-scale employment shock in sizeable entities instead of using single exogenous layoffs or deaths for identification in small groups of workers.

Third, my paper is to my knowledge the first to quantify one channel via which the Brexit vote causally impacts public health. This adds to the literature on the (unintended) economic effects of recent deglobalization and nationalism tendencies and Brexit in particular. Born et al. (2019) and Hantzsche et al. (2019) argue that Brexit likely decreased the UK's GDP by about 2%. Fetzer and Wang (2020) show that these economic costs are unevenly spread across the different regions of the country. Davies and Studnicka (2018) add to this by showing substantial heterogeneity in the effect of Brexit on expected firm performance. Similar results are found by Breinlich et al. (2020). Breinlich et al. (2022), moreover, identify that Brexit has increased inflation. While these papers mainly focus on macroeconomic implications and expectations, I contribute microeconomic evidence on healthcare services, with possible effects on all UK citizens and their daily lives.

The strong effects of the Brexit referendum on public health are of particular interest from a political economy point of view. In his campaign in favor of Brexit, Prime Minister Boris Johnson even claimed that Brexit would allow additional money to flow into the publically funded healthcare system. Alabrese et al. (2019) and Becker et al. (2017) show that regional heterogeneity in NHS performance was a driver of 'Leave' votes. I provide the first evidence on a converse mechanism that Brexit deteriorated healthcare instead of improving it.

Fourth, I identify the economic value of nursing skills for patient health in a new setting, the withdrawal of EU nurses after Brexit. In comparison to other papers that quantify the value of nurses, I do not exploit an absolute deficit of workers in some hospitals or care homes. Instead, a change in the composition of the workforce along nationality and education dimensions is the underlying mechanism in my case study.[4] Gruber and Kleiner (2012) reveal a causal link between nurse strikes and mortality rates. Friedrich and Hackmann (2021) exploit a parental leave program in Denmark, which led to a short-run decline in nursing. They find deaths in retiree homes to increase. Foster and Lee (2015) exploit exogenous incentives for hospitals to staff up to show that more staff improves patient health. Fetzer and Rauh (2022) propose heterogeneity in absence rates across hospitals as a driver of deteriorating healthcare provision during the COVID-19 pandemic. Kelly et al. (2022) show that larger team sizes reduce patient mortality in the NHS. Moreover, the absence of EU employees is a persistent shock while the papers above discuss temporary interventions. Other papers exploit changes in the minimum staffing requirements to identify whether such a change in the number of nurses available per patient affects health outcomes (Lin, 2014).

Furthermore, this paper extends previous work on the different mechanisms that determine the quality and provision of healthcare. Bartel et al. (2014), Doyle et al. (2010) and

[4] Other exceptions are Propper and van Reenen (2010) who exploit regional differences in the outside-option wage to instrument nurse quality and Furtado and Ortega (2023) who use spatial heterogeneity in the share of immigrant nurses instead of employment deficits.

Okeke (2023) provide evidence that the occupation-specific human capital of workers is essential in healthcare provision. Other papers (Cook et al., 2012; Evans and Kim, 2006; Lin, 2014) investigate how the ratio of patients to medical staff might matter for health outcomes and find ambiguous results. I show that the provided health service deteriorates even though the staff-to-patient ratio remains the same. Huckman and Pisano (2006) and Kelly et al. (2022) show that surgeons and nurses perform better the longer they have been working at the same firm. I show that effects on the healthcare provision and quality are stable over time after the Brexit referendum. This indicates the limited role of firm-specific human capital which needs to be formed over time after a new worker entered a hospital in my case study. I further unveil that the employment change for nurses and not doctors drives the effects. Hence, I provide evidence on the importance of nurses also relative to doctors in a healthcare system delivering new results to the literature on the relative productivity of both professions (Chan Jr. and Chen, 2023).

The remainder of this paper is structured as follows. In Section 2, I explain the institutional setting, which underlies my analysis. In Section 3, I discuss the data used, before I outline my identification strategy in Section 4. Subsequently, I present institutional, patient and workforce responses in Section 5. In Section 6, I discuss my main results on health performance. Section 7 discusses alternative mechanisms and conducted robustness checks. Finally, Section 8 concludes.

2 Institutional Setting

Brexit & NHS. The UK electorate voted to leave the EU with a majority of 51.7% on June 23, 2016. Being the precedential case of leaving the EU, the UK faced substantial uncertainty about the consequences of the election. The EU and the UK took years to negotiate how to disentangle both parties and where to still let access for the UK to the European single market. The actual Brexit went into effect on February 01, 2020.

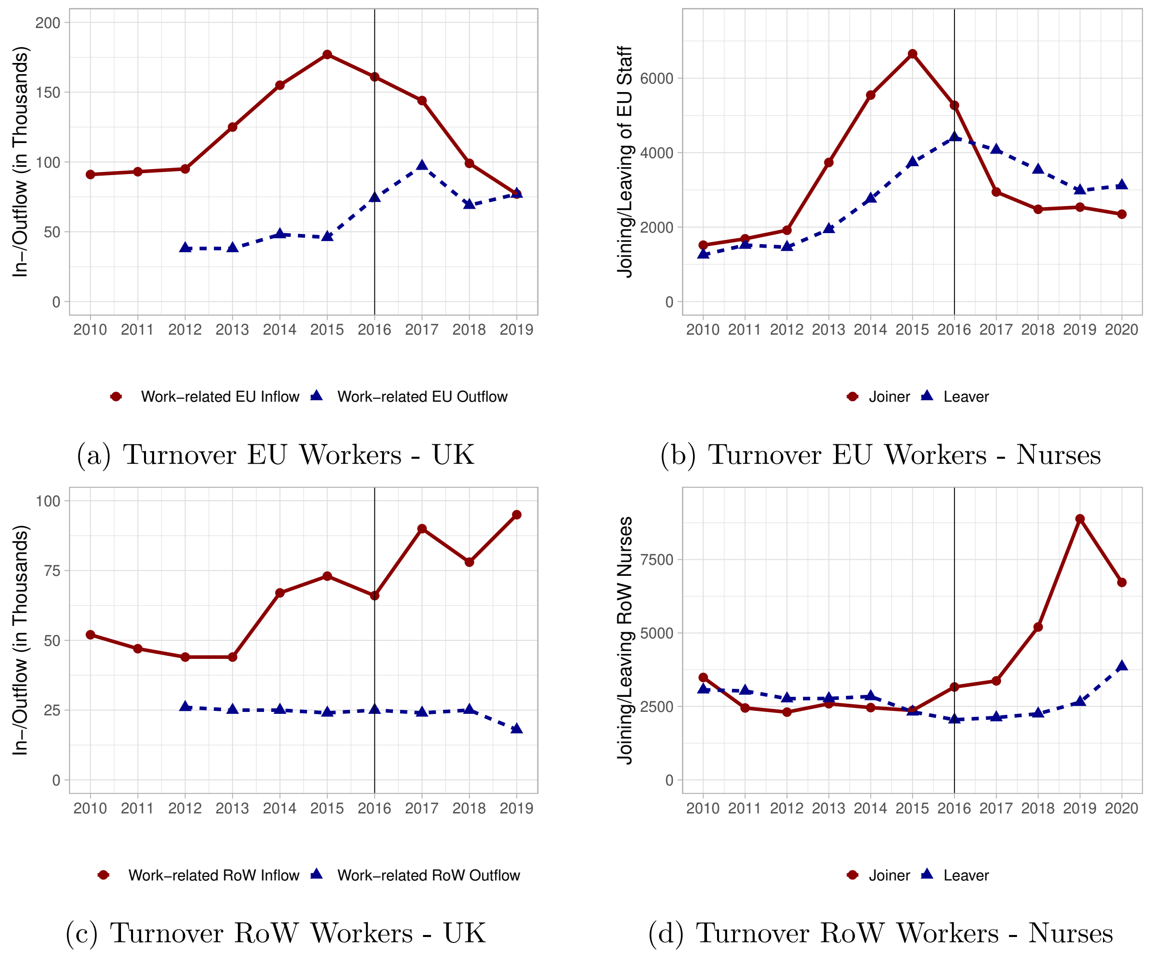
Admittedly, stricter immigration rules and visa requirements were not implemented before 2020. Nevertheless, the referendum induced uncertainty for NHS staff with EU citizenship, whose situation was unclear for years after the referendum. Even three years after the referendum, the parliament still debated about which fees EU nurses would have to pay to be allowed to work in the UK. Also, it was unclear whether newly arriving EU nurses would be treated similarly to applicants from non-EU countries.

I document migration trends in response to this policy uncertainty after the referendum in Figure 1. Panel 1a reports UK-wide numbers on work-related in- and outmigration. After the referendum in 2016, work-related inflow decreased substantially by up to 100,000 workers per year. At the same time, the work-related immigration of EU workers hiked up. Very similar trends are observable for the occupation of nurses within the NHS. While the number of EU nurses joining increased until 2016 up to more than 5,000 nurses p.a., this number dropped to slightly above 2,000 nurses p.a. right after the referendum. The number of leaving EU nurses peaked in the year of the referendum and remained above pre-referendum levels from then on.

In contrast to migration patterns of EU citizens, more workers from the rest of the world (non-EU, non-UK) came to the UK after the Brexit referendum (s. Panel 1c) while the number of work-related outmigrations did not change after 2016. The mirror image of this trend is also evident for NHS nurses. Panel 1d shows that the number of joining workers from the rest of the world tripled from 2016 to 2019 while the number of nurses leaving remained unaffected. Hence, there is a substitution from EU workers to workers from outside of the UK or the EU at the national level as well as in the NHS.

Figure 2 comprises the observations above and plots the share of EU nurses and 'Rest of World' nurses among all nurses within the NHS. While the share of EU nurses increased until the referendum, it decreased substantially after the referendum. Within five years post-referendum, the share of EU nurses dropped by 30% from 7.5% to slightly above 5%. The decrease in EU nurses did not purely take place immediately. The persistent uncertainty about the requirements for work in the UK likely induced the number of nurses to decrease for several years. 'Rest of World' nurses increased in response to the referendum making up for the missing EU nurses. I will use the exogenous leave of EU nurses later to identify the shock's effects on healthcare.[5]

[5]The Nursing and Midwifery Council, the institution which runs the official registry for nurses in the UK, reports that in a voluntary survey 50% of EU nurses leaving the registry state 'Brexit' as main



(a) Turnover EU Workers - UK

(b) Turnover EU Workers - Nurses

(c) Turnover RoW Workers - UK

(d) Turnover RoW Workers - Nurses

Figure 1: EU Im- and Emigration Trends to and from the UK and the NHS

Note: Panels (a) and (c) give the development of work-related in- and outflow of EU citizens and 'Rest of World' (non-EU, non-UK) citizens to the UK. Panels (b) and (d) give the development of the number of joiners and leavers among EU and 'Rest of World' nurses in the NHS. All statistics on the turnover of work-related immigrants to the UK are based on statistics by the Office for National Statistics (ONS). All information on nurses comes from the NHS Workforce Statistics provided.

In Panel A1a of Figure A1 in the Appendix, I show how the absolute employment of nurses changes over time by nationality. Among European countries, especially southern European nurses from Spain, Italy or Portugal leave the NHS making up the majority of net-lost nurses. Spanish nurses, for example, are leaving the NHS because after the referendum uncertainty evolved whether nurses are still able to get years worked in the NHS acknowledged after a potential return to Spanish hospitals. In Panel A1b, the number of Filipino and Indian nurses increased after the Brexit referendum.

In response to the risk of a withdrawal of foreign nurses, hospital providers as well as

reason (see e.g., Nursing and Midwifery Council (2019)).

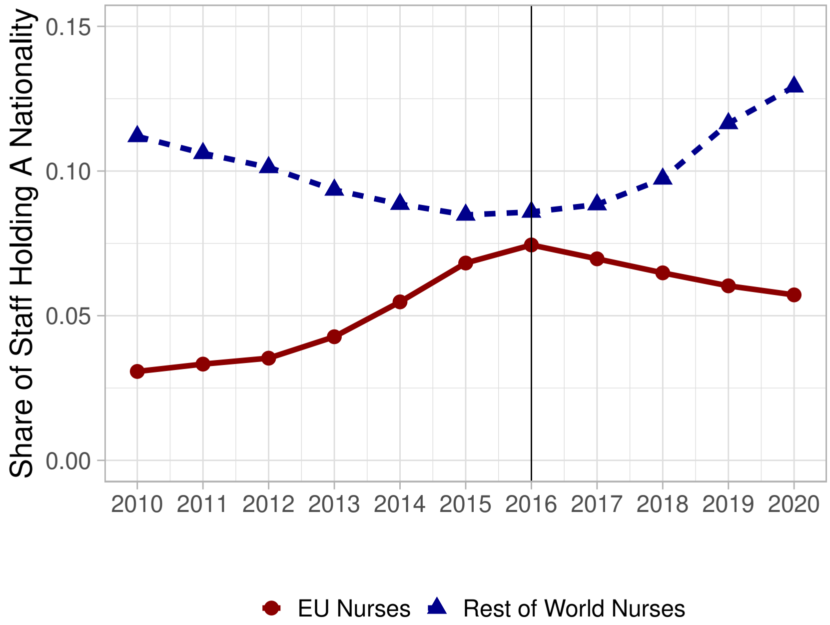


Figure 2: EU Im- and Emigration Trends to and from the UK and the NHS

Note: The figure gives the share of EU nurses and RoW nurses among the overall nurse workforce in the NHS. All information on nurses comes from the NHS Workforce Statistics provided.

the government became active. Some hospital providers, for example, started to pay visa fees for their EU employees to make them stay.[6] The parliament, though only by 2020, introduced a 'fast-track visa' for all foreigners who are offered a job in the NHS (health and care visa) with partly lower fees. In the NHS Long Term Plan of 2019, which describes the strategy for the upcoming decade, no specific policies in response to the Brexit outflow of nurses were proposed (NHS, 2019). In late 2019, the government, for the first time, announced that all nurses would be allowed to stay.[7] However, by then, many left already and fewer new EU nurses came to the UK. Nurses coming after the official Brexit need official recognition of their qualifications. Lastly, for many nurses, it was no option to become a UK citizen as this is only possible after five years in the UK. That was not the case for a large majority of nurses who entered from 2013 onwards (see Figure 1b). In general, the discussed shock has hit the NHS, a healthcare system that has been under pressure for decades, heavily. The NHS has been facing the challenge of increasing health-

[6]https://www.standard.co.uk/news/health/revealed-hospitals-paying-thousands-for-new-visas-to-keep-eu-staff-after-brexit-a3977661.html

[7]https://www.gov.uk/government/news/eu-workers-qualifications-will-be-recognised-after-eu-exit

care costs. As it is financed publicly, the NHS spends relatively little on public health per patient compared to other high-income, European countries and also has one of the lowest doctors and nurses to patient ratios (e.g., Nurse-to-patient ratio: UK 7.8, OECD Average 9.5) (Papanicolas et al., 2019; Simpkin and Mossialos, 2017).

NHS - Organisational Structure. The NHS offers a unique setting to study a reduction of skilled migrant workers and their substitution on firm performance. One reason for this is the organisational structure. The NHS is subdivided into seven health regions (as of 2019) with around 30 hospital providers, so-called 'Health Trusts', each. Each hospital provider runs a handful of hospitals. All providers act within the umbrella organisation NHS and hence are highly comparable concerning their governance, structure, and financial regulations. Nevertheless, they operate independently in staff employment and hiring. I, later on, show that the reliance on EU nationality workers varies strongly across providers. Moreover, I can rely on comparable data across providers at a micro level.

Hospital providers do not operate daily practices, which are typically run by general practitioners (GPs), but instead operate hospital accident and emergency (A&E) departments, conduct diagnostic tests, conduct operations, or are in charge of cancer patients. Hence, they are in charge of healthcare provision in many sensitive fields where most fatalities happen. I do not consider GPs in this paper because they have often worked in England for decades making it much more unlikely to leave. Five years after the referendum, the share of general practitioners with an EU medical degree has remained constant at around four percent.

3 Data

I combine data from several NHS data sources at the hospital provider level. I explain them subsequently. My analysis stretches over the years 2012 to 2019. I abstract from years before this time window due to a major restructuring and merger wave among NHS providers, which mainly took place until early 2012.[8] Years after 2019 are dropped due to the major COVID-19 shock on the healthcare system (Fetzer and Rauh, 2022), which started with the first COVID-19 wave in March 2020. To convincingly disentangle the pandemic's effect from Brexit's consequences seems very challenging. For example, the exposure to COVID-19 and foreign healthcare workers was highest in urban regions and, thus, might be spatially correlated. In general, I also account for mergers between NHS providers throughout my sample period by aggregating observations from the merging providers before the merger and by manually dropping critical provider-year observations in which employment data was not restructured yet. Finally, I end up with a panel of 216 providers as of 2019.

The number of providers, that report data on a certain subject and outcome variable, can vary as, for example, every provider reports employment data while not all providers have active intensive-care units. Fatality data is primarily reported by acute care providers. Table A1 in the Appendix gives the number of providers reporting data for each outcome variable. As data reporting is compulsory for the relevant providers, selection is not a concern.

NHS Workforce Statistics & Payroll Data. At the heart of my analysis is the measurement of an NHS provider's exposure to the referendum shock. For this, I need to know how many EU nurses have been employed by a provider over time. I obtain annual data on the staff composition of nurses and health visitors by nationality groups ('UK', 'EU&EEA'[9], 'Rest of World') for each provider from 2012 until 2019.[10]

Employment statistics do not only encompass absolute employment numbers but also inform about the number of 'joiners' and 'leavers' and their nationality for an annual turnover period.

[8] In particular, the NHS forbid private care providers from early 2013 onwards. This decision led to substantial employee shifts between providers until late 2011. Gaynor et al. (2012) and Tafti and Hoe (2022) explicitly show that mergers and restructuring among hospitals affect healthcare performance in the NHS.

[9] I subsequently refer to EU and EEA nurses jointly as EU nurses as only 0.6% of EU&EEA nurses come from EEA countries.

[10] A small share of 9% of all employee-year observations do not include information on the nationality status, since the reporting of nationality is voluntary for employees. I assume that the distribution of nationalities among nurses in a provider is identical for reporting and non-reporting nurses.

Finally, the NHS Workforce Statistics encompasses information about how many nurses are employed in the various wage bands for each year.

Table A1 in the Appendix includes descriptive statistics on the main provider-level information in 2015, the year before the treatment, split up into low- and high-exposure providers based on the average pre-referendum share of EU nurses. An average provider employs 1,465 nurses out of which 104 (7%) are EU citizens.

I, further, collect provider-level payroll data. Wage bands are uniform across providers. I later on use the data to track wage changes in response to the referendum.

NHS Performance Data. My main analysis studies how Brexit affects healthcare provision and performance. For this, I mainly use datasets based on the Health Episode Statistics (HES) aggregated to the provider level. First, I collect data on how many patients were treated by each hospital provider. This, later on, allows me to study whether the exposure to the Brexit shock *quantitatively* affects the overall provision of healthcare. Table A1 includes the summary statistics for providers' patient episodes, admissions, diagnostic tests and similar outcomes. An average provider has a catchment population of almost 0.4 million people and has 78,000 patients admitted to hospital per year. In the providers' A&E (accident and emergency) departments, more than 125,000 cases are handled per year. Further, hospital providers on average conduct more than 116,000 diagnostic tests per year and get almost 12,000 cancer referrals from GPs per year.

Second, I collect data on performance measures proxying healthcare *quality*. I gather annual, provider-level data on observed deaths from patients, who visited a provider throughout the last 30 days. Such fatalities represent about 60% of all registered deaths in England. Related to this, data on the number of (unexpected) readmissions as well as the number of incidents with harm to patient health is available. Even small changes in such outcomes can be highly relevant since an average provider counts 2,304 deaths, 7,539 unexpected readmissions, and 2,043 incidents with harm to patients per annum.

Lastly, I also use data on capacity measures because Brexit could affect the extensive margin of healthcare. In particular, I use data on the provider-level number of operation theatres, beds as well as absence rates.

Trust Accounts Data. To test how provider accounts and finances are affected by the Brexit referendum, I collect data on purpose-specific expenditures and income sources at the provider level. This allows me to test whether healthcare performance is influenced by different budget decisions and to rule out alternative mechanisms. In the pre-referendum year 2015, an average provider had operative expenditures of 336 million pounds and a deficit of 10.3 million pounds. Staff costs make up the largest part of expenditures averaging more than 200 million pounds per provider and year. Drug expenditures sum up to almost 30 million pounds per provider and year.

4 Empirical Strategy.

In this section, I, first, manifest the causal relation between the Brexit referendum and a decrease in EU nurse employment. Second, I then propose an empirical framework that allows me to estimate the direct effect of EU nurse withdrawal on healthcare outcomes.

The Referendum and EU Nurse Employment. In Section 2, I showed that the share of EU nurses among all NHS nurses dropped by 30% in the years after the referendum. In this section, I formally show that EU nurse employment decreased due to the referendum. To do so, I apply a difference-in-differences model at the provider level in which I compare the annual number of EU nurses leaving (joining) an NHS provider relative to the number of 'UK' and 'Rest of the World' nurses leaving (joining) the provider:

$$Y_{int} = \alpha_{in} + \gamma_{rt} + \sum_{\tau=2012, \tau\neq 2015}^{2019} \beta_\tau \times 1[EU]_n \times 1[Year = \tau]_t + X'_{it}\zeta + \epsilon_{it}$$

where Y_{int} is the logged number of nurses leaving (joining) provider i in year t and who are of nationality n. $1[EU]_n$ is the dummy for the national group of EU/EEA nurses with n giving the nationality. α_{in} is a provider-nationality fixed effect and γ_{rt} an NHS health region-year fixed effect (seven health regions as of 2020).

Figure 3 provides evidence for a simultaneous increase in the logged number of EU nurses leaving (by around 10%) as well as a decrease in the logged number of EU nurses joining (by around 50%). Both effects are persistent over the post-referendum period.

Measure of Exposure. Estimating the causal effect of foreign healthcare workers' withdrawal on healthcare performance is challenging because the distribution of withdrawals across providers is not random, as it would be the case in an ideal experiment. Withdrawal decisions are driven by unobserved factors which also correlate with outcomes of healthcare performance, so that an omitted variable bias arises. For example, EU nurses might especially leave hospitals where the majority of people voted to leave the EU. These hospitals are especially in low-income areas (Becker et al., 2017). Propper and van Reenen (2010) show that such hospitals generally perform well as nurses' real wage is higher so that better nurses are employed. Contrary, EU nurses could be more prone to leave badly performing hospitals that offer a low-quality working environment. Also, standard concerns of non-random sorting in the migration literature (e.g., Dustmann et al. (2005)) imply that EU nurses' im- and emigration decisions are not orthogonal to hospital quality.

Therefore, I need exogenous variation in the exposure to nurse withdrawal to identify causal effects on hospital performance. I take the heterogeneous pre-referendum dependence of providers on EU nurses as a measure of exposure to the decrease in EU nurses. This isolates the sensitivity to the shock based on providers' pure number of EU nurses and abstracts from unproportional changes in nurse withdrawal at the provider level for unobserved reasons. While the referendum implies identical legislative changes across all English regions and health providers in the subsequent years, the exposure to the shock varies across providers.

I create a time-invariant measure of exposure for each provider i by calculating the share of EU/EEA nurses among all nurses in the pre-shock years 2012 to 2015:

$$bite_i = \frac{\sum_{t=2012}^{2015} \#\ EU/EEA\ Nurses_{it}}{\sum_{t=2012}^{2015} \#\ All\ Nurses_{it}}$$

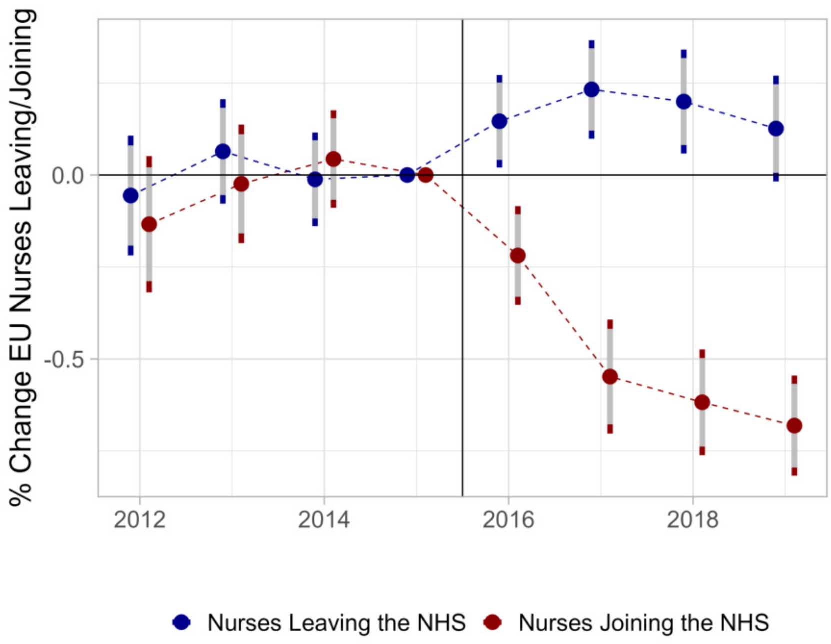


Figure 3: Effect on EU Nurse Turnover

Note: This plot reports the effect of the referendum on the annual, provider-level share of EU nurses who leave or join a provider relative to non-EU (UK, 'Rest of World') employment. I control for the logged number of nurses of the respective nationality and its lagged value. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.

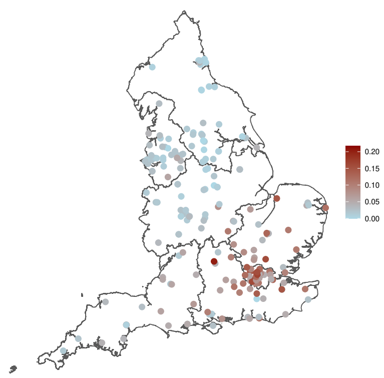
The provider-level exposure $bite_i$ ranges from 0 to 0.217 with a mean (median) of 0.049 (0.030) and a standard deviation of 0.045.

In panel 4a of Figure 4, I test whether the pre-referendum exposure to EU nurses has predictive power for the withdrawal of EU nurses. There is a clear relation between the calculated bite and the actual decrease in nurses. A linear fit suggests that a one percentage point increase in the pre-referendum share of EU nurses implies that the withdrawal of EU nurses increases by 0.35 percentage points. Also, Figure A2 in the Appendix shows that more nurses of a provider voluntarily leave for reasons of 'relocation' in high-bite providers after the referendum.

While providers are free to operate all over England, they geographically cluster their activity. This allows me to report the geographical distribution of the treatment exposure. Panel 4b gives the bite at each provider's headquarter location. There is substantial variation in the exposure in space. It is highest in the southeast of England. The variation in space is driven by demand- and supply-side factors in the labour market. On the one hand, immigrants' selection into areas where more EU citizens have been living might explain parts of the variation. On the other hand, many nurses join the NHS via agencies that match nurses to providers demanding workers. As wages are to a large extent fixed across hospitals, areas with high living costs offer lower real wages. Hence, domestic nurses are less likely to work there. Thus, in such regions, hospitals can demand more international workers which explains the distribution in space as well.

Difference-in-Differences. I rely on a difference-in-differences approach to identify the causal effects of the referendum on multiple provider-level outcomes. The treatment exposure is given by the bites described above. The timing of treatment is the referendum and not the official Brexit in 2020 as EU nurses started to withdraw from England right after the Brexit referendum due to rising uncertainty.

(a) Predictive Power of Bites



(b) Bite by Provider

Figure 4: Predictive Power and Spatial Variation of Bites

Note: Panel (a) proves the predictive power of the pre-referendum share of EU nurses among all nurses at the provider-level for the post-referendum decrease in EU nurses. Panel (b) documents the spatial variation of the constructed provider-level variable $bite_i$ within NHS region borders. Each point represents a provider's head office.

I set up a simple difference-in-differences model in the following form:

$$Y_{it} = \alpha_i + \gamma_{rt} + \beta \times bite_i \times 1[Post]_t + X_{it}'\zeta + \epsilon_{it} \tag{1}$$

where α_i are provider fixed effects and γ_{rt} time fixed effects interacted with dummies for the seven NHS regions (as in place in 2020, see borders in Panel 4b of Figure 4). Identification within NHS regions allows me to compare providers nearby that likely experience similar local shocks except for the heterogenous exposure to pre-Brexit EU nurse employment. Y_{it} are several provider-time-specific health outcomes. $bite_i \times 1[Post]_t$ gives the interaction terms characterizing the provider-specific treatment.

In most estimations later on, I also provide results from a dichotomous treatment. In particular, I run equation (1) with the interaction term $1[bite_i > p75(bite_i)]_i \times 1[Post]_t$ which pools all providers in the top quartile of the bite distribution as the treatment group. Studying the top quartile of the distribution explicitly in a binary difference-in-differences setup will be informative about whether the effect is non-linear in the bite. I further control for covariates, X_{it}.

In our baseline estimations, I cluster all regressions at the provider level. I test alternative clustering approaches in the robustness checks.

Identification Assumptions & Threats. To identify the causal effects of the referendum's shock on health outcomes, two main assumptions need to hold. First, the 'parallel trends' assumption implies that more and less heavily treated providers would have developed on similar trends if the referendum had not taken place. Second, the 'stable unit treatment variable assumption' (SUTVA) suggests that, among others, there should not be any spillovers of treatment between providers of different treatment statuses.

Admittedly, the first assumption cannot be explicitly tested, but I provide suggestive evidence for the assumption to not be violated in two ways. First, Table A2 in the Appendix regresses the continuous and binary measure of exposure on the provider-level percent changes in main outcome variables from 2012 to 2015. If providers were on different trends before the referendum in 2016, the change in outcome variables would be correlated with the treatment exposure. I show that identification within NHS regions satisfies the absence of a significant relation between outcome trends and treatment status. I take this as evidence for less and highly treated providers being on parallel trends.

Second, I can inspect pre-trends in the event study version of model (1). If pre-trends are statistically not distinguishable from zero, the parallel trends assumption should not be violated. The event study version is estimated as follows

$$Y_{it} = \alpha_i + \gamma_{rt} + \sum_{\tau=2012,\ \tau\neq 2015}^{2019} \beta_\tau \times bite_i \times 1[Year = \tau]_t + X'_{it}\zeta + \epsilon_{it}$$

Flat pre-tends imply that β_τ is not significantly different from zero for $\tau \in \{2012, ..., 2015\}$. As the voting outcome and, hence, the policy realization was unclear ex-ante (Fetzer, 2019), providers likely did not change behavior in advance (anticipatory effects).

When using the continuous bite as the measure of treatment exposure, identification further requires that high-bite providers would have the same treatment effect as low-bite providers at a given exposure level (Callaway et al., 2021). The binary treatment at the 75th percentile does not need this 'strong parallel trends assumption'.

Concerning the SUTVA, spillover effects of one provider's exposure on the treatment status or outcomes of another provider in the near vicinity could arise. For example, patients are free to choose their provider, so switching behavior between providers might smooth per se exposure across providers. This, however, would only imply that I am likely to underestimate most effects of the shock (in absolute terms). Potential population movement between less and highly treated areas in response to outmigration could also affect the patient composition in all providers (Giuntella et al., 2018). I later show that this is not the case. Lastly, it might be that after the Brexit referendum providers within the same NHS region react by adopting different policies which correlate with the staff shock. Then, I would pick up effects unrelated to and not caused by the actual labour supply shock. I address this by testing a large variety of alternative mechanisms such as heterogeneous reactions in finances and expenditures of trust later on.

Moreover, a threat to identification would be reverse causality - meaning that health outcomes might affect treatment status and timing. While the referendum's timing is exogenous, regional differences in health outcomes might affect the composition of a provider's workforce. For example, public health quality might attract more (inter)national workers. However, reverse causality would imply that the development of health outcomes significantly differs between more and less heavily treated providers before the referendum. A visual inspection of the pre-trends will be informative about this.

Lastly, let me note that I am not aware of regional policies made in response to the Brexit referendum, which might have correlated with the share of EU employees. The federal government is in charge of the NHS as well as the negotiations on Brexit.

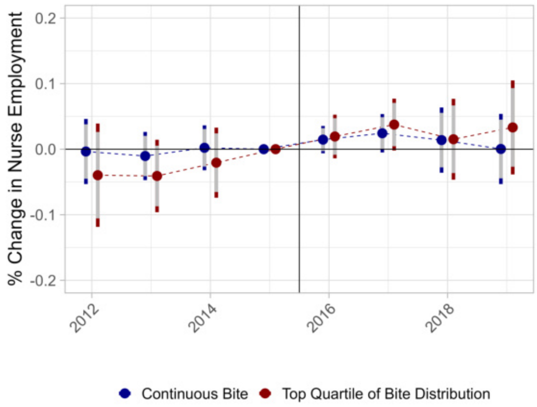
5 Institutional Responses

First, I look at how the providers changed hiring strategies after the referendum. This informs me about whether the EU nurse withdrawal led to an actual deficit of nurses or not. Starting out, I study how the staff composition changed. While fewer EU nurses came to and more left the NHS, it is unclear whether the deficit in nurses could be compensated and, if yes, through which channel.

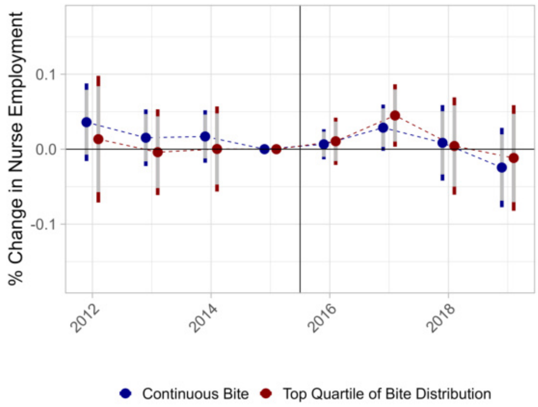
I examine how employment of different nationalities evolved over time. There was a change in the hiring strategy from 2018 onwards. Figure 5 presents the effect of the treatment on the logged overall number of nurses for all nationalities jointly (s. Panel 5a), for UK nationality (s. Panel 5b) and 'Rest of World' (s. Panel 5c and 5d). To make the estimates of both regressions (continuous exposure and binary treatment classification) comparable, I multiply the effect of the continuous exposure with the mean bite in the sample. There is no decline in the aggregate number of nurses for highly exposed providers. Hence, the reduction in EU nurses is outbalanced by hired nurses from other nationalities. The absence of a lack in the absolute number of nurses is different from other papers in the literature (Fetzer, 2019; Friedrich and Hackmann, 2021; Gruber and Kleiner, 2012). In the long run, especially employees from non-European countries were hired. For a provider of mean exposure, the employment of 'Rest of World' nurses increased by 25% until 2019. This is purely driven by an increase in Filipino and Indian nurses and not other 'Rest of World' nationalities. This translates to 6,900 'Rest of World' nurses which slightly over-compensates the reduction in EU nurses in the five post-referendum years (5,700 nurses). To compare Filipino and Indian nurses joining after 2015 with EU nurses leaving after 2015, I contrast both groups' wage and age distribution.[11] Figure 6 shows that EU nurses

[11] Figure A3 in the Appendix documents the distribution of nurse grades.

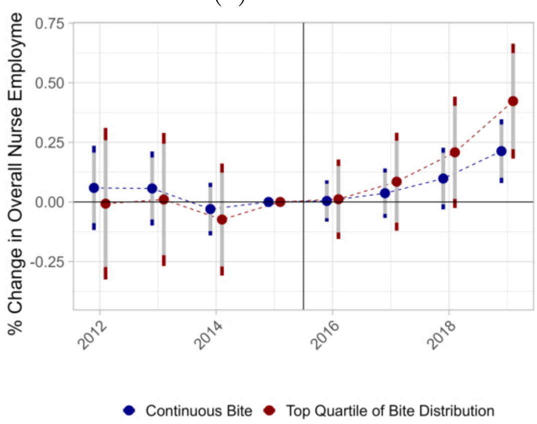
are on average older and are paid in higher wage bands. This indicates likely higher tenure and might reflect a higher education level mapping into salary (Cortes and Pan, 2015). However, the increase in 'Rest of World' nurses only evolved over time and did not compensate for the drop in EU nurses right after the referendum. In the short run, there instead was an increase in UK nurses in 2017.[12]



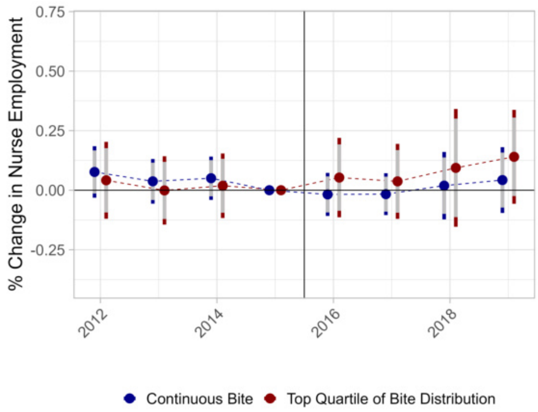
(a) All Nurses



(b) UK Nurses



(c) RoW Nurses - Indian/Filipino



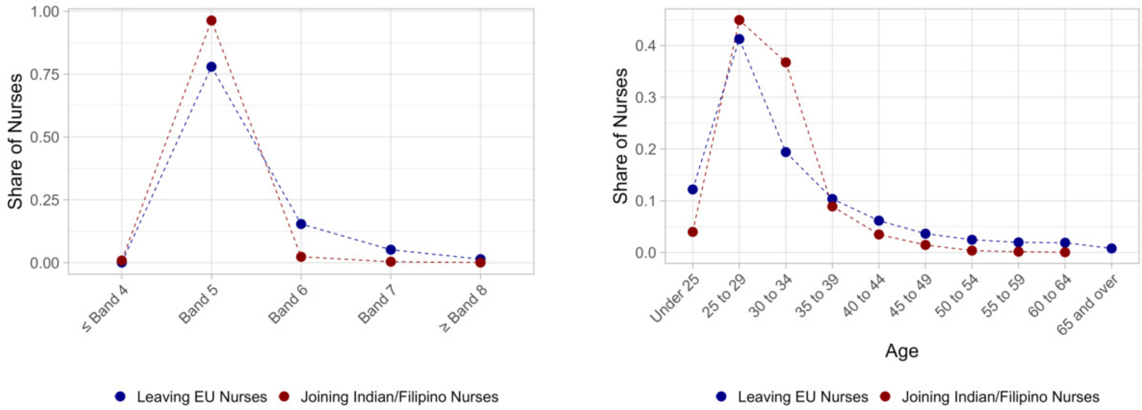
(d) RoW Nurses - Non-Indian/Filipino

Figure 5: Effect on Nurse Employment by Nationality

Note: Panel (a) documents the effect of the referendum on the logged annual, provider-level number of nurses employed. Panel (b) documents the effect of the referendum on the logged annual, provider-level number of UK nationality nurses. Panels (c) and (d) document the effect of the referendum on the logged annual, provider-level number of Indian/Filipino and non-Indian/Filipino nurses from the rest of the world. I control for employment changes in other occupations within the NHS such as managers, infrastructural employees and learning doctors. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.

A natural question, which arises from the observation of a short-run increase in UK nurses,

[12] Cortes and Pan (2014) also show a negative relation between foreign nurses' employment and native nurses' decisions to work in a hospital.



(a) Wage Distribution (b) Age Distribution

Figure 6: Differences in Joiner and Leaver Characteristics

Note: Panel (a) plots the wage distribution of RoW nurses joining and EU nurses leaving after 2015. Panel (b) plots the age distribution of RoW nurses joining and EU nurses leaving after 2015.

is where these nurses come from and how they get convinced to work. This is of particular interest as there has been a shortage of domestic nurses for years. This shortage only has led to the increasing demand for EU nurses after 2010. One reason could be an increasing salary. Salary contracts are very similar across NHS providers. They only differ in regional compensations for heterogeneity in living costs which, however, only make up less than 3% of the overall salary (Propper et al., 2021; Propper and van Reenen, 2010). I, therefore, can compare 'salaries' based on the so-called 'bands' of NHS nurses (wage groups).

Grade 5 and 6 of the wage distribution are the primarily relevant grades for my analysis as the large majority of nurses is employed there. Also stepping up from grade 5 to 6 only demands NHS-internal training while grade 7 mostly requires additional studies like a master's degree.

I study two questions. First, do I find evidence for a wage effect of the referendum by shifting more nurses to a higher wage band? Second, do joining and leaving nurses differ in their wage category and, by that, quality? I study the first question by looking at the share of nurses, which is promoted from wage band 5 to wage band 6. Figure 7 gives the effect of the treatment on the likelihood of getting promoted. Indeed, I find evidence in favor of more promotions occurring in highly treated providers. The likelihood of promotions increases by around one percentage point or 12% respectively or 900 nurses per annum. Of course, promotions could also be induced by nurses replacing EU nurses with higher responsibilities. Though, this should not lead to 900 promotions per year implying 3,600 promotions relative to a decrease of EU nurses by 4,350 from 2016 to 2019.

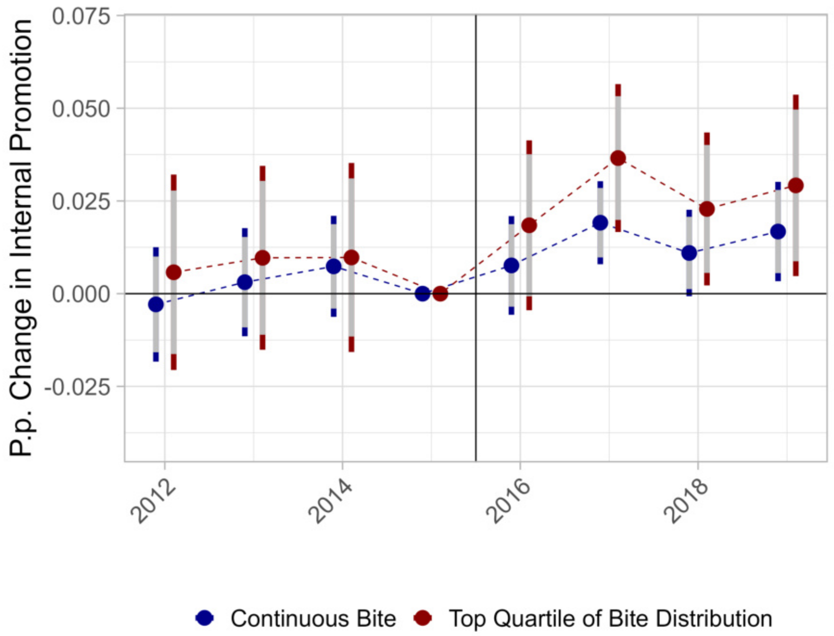


Figure 7: Effect on Internal Promotion

Note: The plot reports the effect of the referendum on the annual, provider-level share of promotion from wage band 5 to wage band 6. The outcome variable, the share of promoted nurses of a provider in a year, is winsorized at the 5th and 95th percentile to account for outliers and to allow for observations with zero nurses. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.

Secondly, I show new nurses are more likely to be hired in low wage bands likely reflecting lower levels of formal qualification than joiners in years before the referendum (Cortes and Pan, 2015). Figure A4 in the Appendix shows the effect on the number of joiners and leavers in each wage grade. It can be seen that joiners are hired in wage band five, the wage band for starting nurses, more often in highly treated providers right after the referendum. Fewer nurses joined in wage band 7 in 2016. In 2019, when most non-European substitution nurses join, the hiring in the low wage band 5 was significantly higher than in pre-referendum years again. I take this as suggestive evidence for substitution nurses being of relatively low quality.

I, further, study the full-time equivalents per worker, i.e. working hours of nurses. Figure A5 in the Appendix shows that there - if at all - is only a small change in the average working time of nurses in the long run. Hence, compensation for missing nurses did not take place through extending the working times of incumbent nurses.

Lastly, I examine whether treated hospitals invest in more long-run improvement of the employment situation. In Figure A6 in the Appendix, I analyze whether the likelihood of a provider having nursing learners, i.e. nurses in their training, changes with the referendum. I do not find evidence for such a sustainable response.

Overall, these results show that providers reacted to the personnel shock by implicitly offering higher wages and by recruiting people from other regions of the world.

6 Effect on Healthcare Performance

Deaths. As a natural starting point, I investigate the number of observed deaths as a public health outcome at the provider level. The NHS counts all deaths of people who have been in a provider's hospitals throughout the last 30 days for any reason. These deaths sum up to about 60% of all deaths in England's population. Figure 8 gives the dynamic results from the shock's impact on logged provider-level deaths. I flexibly control for the age structure of providers' patients as well as the number of expected deaths. The latter is calculated by the NHS based on patient diagnoses and characteristics. I observe flat pre-trends up to the referendum. Afterward, deaths persistently increase for both types of exposure, the continuous measure as well as the top quartile of the bite distribution relative to providers with a lower bite.

The effect translates to 2% more deaths for a provider of mean exposure. Providers from the top quartile of the bite distribution even face an increase of deaths by 4% relative to all less-treated providers. Extrapolating these findings, this implies around 5,900 additional hospital-related deaths per year in England.[13] This is, for example, more than the estimated number of non-COVID excess deaths during the early phases of the COVID-19

[13] Alternatively, the second estimate implies that the treatment causes 2,600 additional hospital-related deaths per year for only the providers in the top quartile of the bite distribution relative to all others.

pandemic (Fetzer and Rauh, 2022).

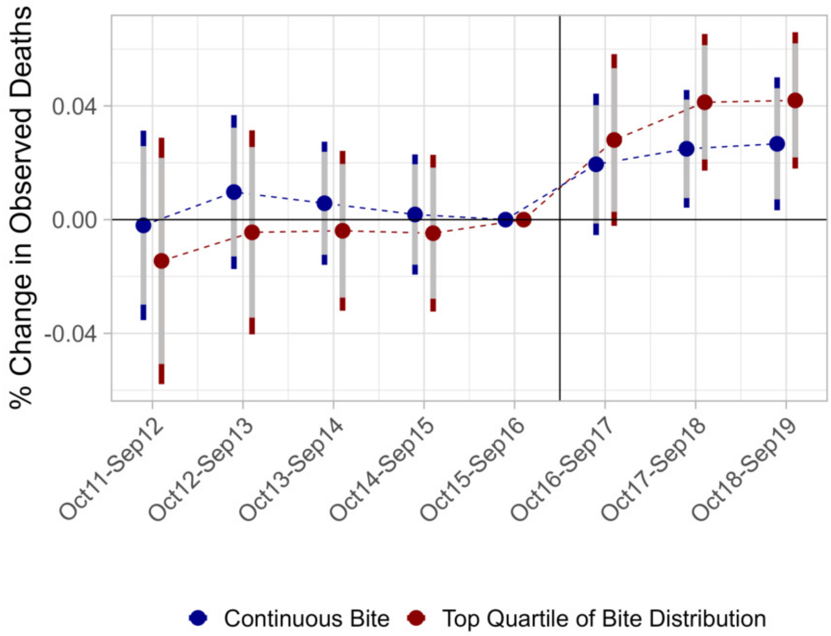


Figure 8: Effect on Hospital-Related Deaths

Note: The plot reports the effect of the referendum on the logged annual, provider-level deaths following regression equation (4). Deaths are counted in the statistic when a patient visited a provider throughout the last thirty days for any reason. I flexibly control for the number of expected deaths (as calculated by the NHS based on patient characteristics), the providers' overall number of elective and emergency admissions, and the provider's number of elective and emergency admissions in the age group of 80+ years. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.

As the effect is persistent over time, healthcare provision does not only change right after the Brexit referendum in response to the first, unexpected short-run decrease in EU nurses. This indicates that the effect, similar to Kelly et al. (2022), is driven by human-specific capital and not just institution-specific capital. The latter would imply that the treatment effect could shrink over time as healthcare worker performance can improve with gained experience within an entity (Kelly et al., 2022; Huckman and Pisano, 2006). I, further, use diagnosis-specific data on hospital-related deaths for the 15 diagnoses which are associated with the most deaths (representing more than 60% of all deaths in the sample). In the Appendix (see Figure A7), I show that deaths are mainly driven by severe diagnoses such as lung cancer, organic mental disorders and renal failure. Patients with such diagnoses are care-intense. However, additionally, these diagnoses demand a much more specific occupational knowledge and skills than treatments of, for example, pneumonia and bronchitis, which are unaffected by the shock.

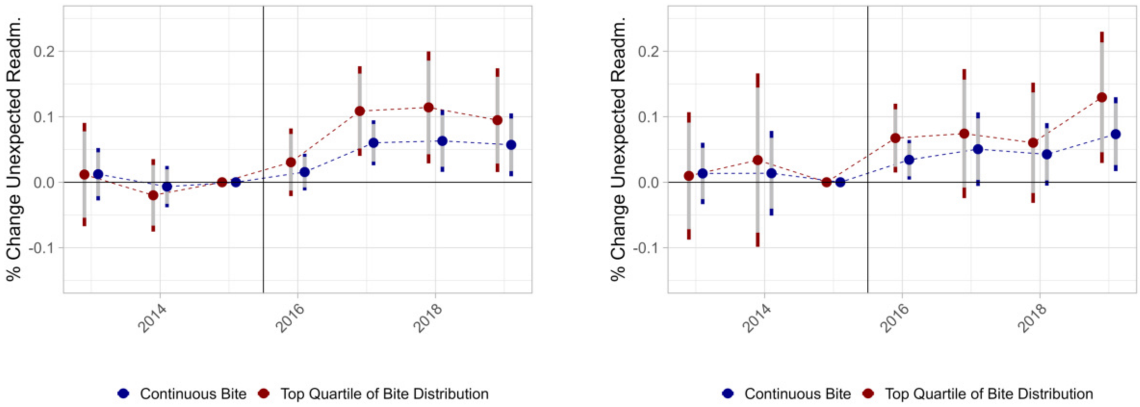
Given the unintended effect of the EU nurses' withdrawal on hospital-related fatalities, it is necessary to study the underlying reasons for the effect. A pure lack of employees as in Friedrich and Hackmann (2021) or Gruber and Kleiner (2012) is not the driver as shown in Section 5. Alternatively, the missing EU nurses could be lost human capital, so that healthcare quality instead of healthcare quantity could be the mechanism.

Unexpected Readmissions. I start the examination of what drives the additional deaths by looking at the logged annual, provider-level unexpected readmissions within 30 days of treatment first. Unexpected readmissions might capture scenarios, in which the actual reason for the medical problem was not found or treatment was insufficient. Figure 9 presents results for the overall population of patients and for the subgroup of the elderly population. In both cases, I observe an increase in the share of readmissions linked to the referendum. Readmissions rise substantially by 5% evaluated at the mean bite. The effect for providers from the top quartile is even larger with almost 10% for the pooled post-referendum years. Slightly smaller effects are found for the subsample of 75+ year-old patients.

Again, these effects are persistent over time. This hints at an underlying mechanism, which is fundamentally related to the workforce substituting leaving EU nurses. Short-run replacement effects would be more likely right after the referendum.

As I show later on that the withdrawal of EU nurses did not lead to an overall deficit of nurses employed by highly treated providers, the increase in unexpected readmissions likely originates from human capital loss instead of deteriorating diagnoses due to a lack of workers.

Incidents. The observed increase in deaths might also be caused by worse treatment. I can measure this by analyzing the change in the logged number of incidents of harm to patients. Figure 10 shows that, using the continuous bite as a measure of exposure, an increase in the number of harmful incidents is evident especially right after the referen-



(a) All Patients

(b) Age 75+

Figure 9: Effect on Unexpected Readmissions

Note: The left plot reports the effect of the referendum on the logged annual, provider-level number of patients, who get readmitted to a hospital within 30 days after they left the hospital. The right plot reports the same effect for the subsample of elderly patients (75+ years). Data collection only started in 2013, so data for 2012 is unavailable. I control for the logged number of spells. Both regressions follow regression equation (4). Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.

dum (pooled treatment effect of 7.5%, p-value = 0.09). When comparing the providers from the top quartile of the bite distribution, I find the effect to be significant over the complete post-referendum period. The pooled effect is 18%.

Diagnostics. I further estimate effects of the treatment on the logged number of diagnostic tests conducted. Less diagnostic activity could explain why people are unexpectedly resubmitted or even die. Panel 11a of Figure 11 presents the effect on the number of conducted diagnostic tests. I find that the number of diagnostic tests conducted fell after the referendum. While not every bin is significantly different from zero, the pooled effects are different from zero at the 5% significance level for both exposure variants. The pooled effect for the continuous bite is 6% evaluated at the mean bite and providers from the top quartile of the bite distribution experience 8% less diagnostic tests after the referendum. A loss in human capital of nurses as well as a lack of workforce being able to conduct such diagnostic tests could both explain the found effects. While I, later on, show that there is no lack of nurses in highly treated providers after the referendum, I can also study providers' general ability to conduct specific tests. Therefore, I analyze the logged num-

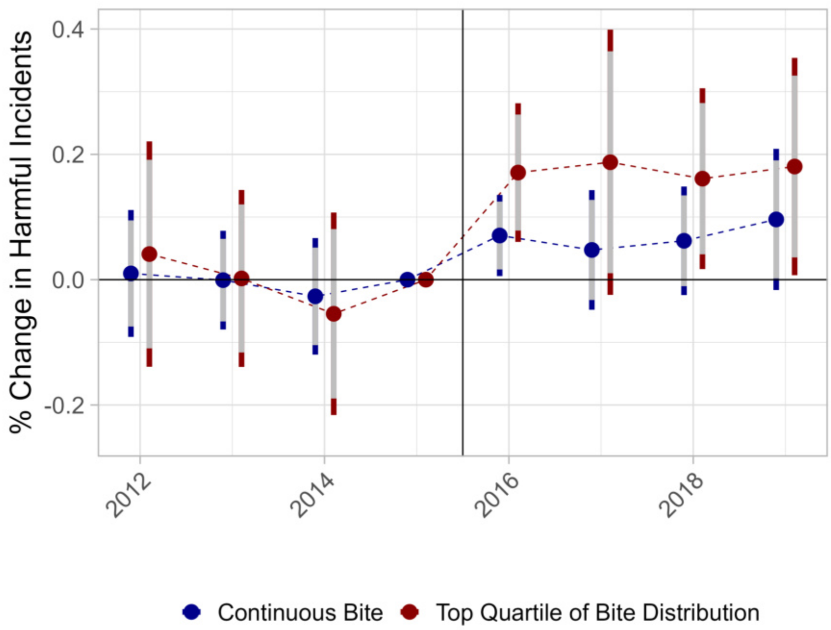
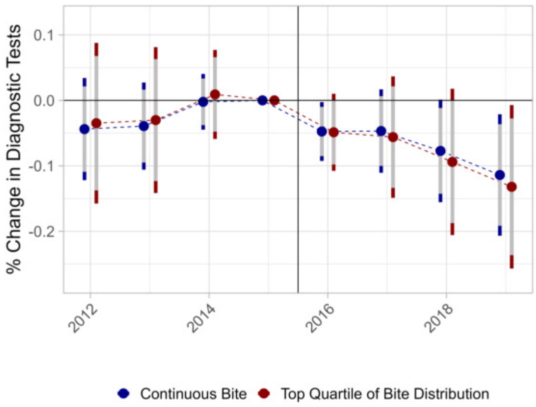


Figure 10: Effect on Incidents of Harm to Patient

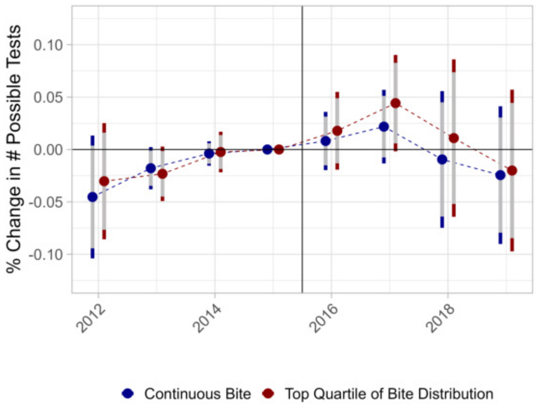
Note: The plot reports the effect of the referendum on the logged semiannual, provider-level number of incidents following regression equation (4). Only observations of providers with incidents from the full six months reported are included. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.

ber of different diagnostic tests out of fifteen tests reported in the data, which a provider conducts or offers throughout a month in Panel 11b of Figure 11. There is no change in the variety of tests a provider can conduct indicating that providers do not lack the ability to perform tests, so that capacity issues are not the driver of fewer diagnostics. Further, I show that capacity limitations due to the referendum shock are not an issue. For this, I analyze the effect on the waiting list for diagnostic tests. Figure A8 in the Appendix shows the effect on the waiting list length and the likelihood that at least one patient has to wait for more than 13 weeks for a test. I neither find an effect on the waiting list length nor on the likelihood that a provider has at least one patient waiting for a test for more than 13 weeks.

Patients. The increase in deaths, unexpected readmissions and incidents can stem from either a loss in human capital or increasing pressure (e.g., an insufficient supply of nurses) on the highly treated providers. Both explanations would need to be persistent to explain the stable treatment effects for the named outcomes over the period 2016 to 2019. If the



(a) Diagnostic Tests



(b) Test Capacities

Figure 11: Effect on Diagnostics

Note: The left plot reports the effect of the referendum on the logged monthly, provider-level number of diagnostic tests conducted for fifteen subcategories of diagnostic tests. The right plot reports the effect of the referendum on the logged number of different tests a provider conducted in one month. This is a measure of test capacities and capability. In contrast to equation (4), I include provider-test and test-NHS region-month fixed effects. I control for the logged number of nurses and doctors. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.

effects are purely driven by a lack of workers or an insufficient treatment capacity, this should lead to stricter regulations on who should be treated or who can be admitted to hospitals. Given the unaffected level of nurses employed, this would imply a decrease in the number of patients treated by an average nurse. Hence, I subsequently study measures proxying the number of treated patients per nurse to unveil potential treatment reductions, pressure on nurses and increasing work for nurses. First, I will look at the logged number of patient episodes and admissions per nurse as documented in the Health Episode Statistics including all patient journeys in hospitals. Second, I then look at logged A&E cases (accident & emergency department) per nurse separately as emergency cases might reflect patients being admitted too late or results from wrong diagnoses.

Figure A9 in the Appendix summarizes the effect on the logged overall number of patient episodes and admissions as well as A&E attendance per nurse. Panels A9a and A9b reveal the treatment's effect on the logged number of patient episodes and admissions per nurse. Episodes also include inspected patients who do not get admitted. Out of all patient episodes more than 80% get admitted to hospital. I neither find an effect on the logged number of patient episodes nor admissions per nurse which indicates that there is not a lack of healthcare workers able to treat all patients. As the share of patients getting admitted neither changes with the treatment, there also is no strategic shift in admissions requirements. Hence, the overall workload of nurses measured in the number of patients they treat does not change with the treatment.

In Panels A9c to A9f, I provide results on A&E outcomes. Accidents and emergency (A&E) units are especially used by patients who unexpectedly visit the hospital. This might be due to an accident or due to unexpected symptoms of a wrong diagnosis. Patients in A&E units usually need immediate medical advice and treatment. First, I neither find a reduction nor an increase in the logged total number of A&E cases nor in the logged number of major cases per nurse or the share of cases that lead to an emergency admissions. Lastly, I show that the share of people waiting for more than four hours in A&E departments (internal goal threshold by the NHS) for a major A&E visit does not robustly increase after the Brexit shock. I take these results as evidence that A&E departments do not lack capacity as they, if at all, increase the number of A&E cases treated and do not change the share of emergency admission.

I further take a look at cancer patient pathways. Cancer patients usually are forwarded by their GPs to hospital provider specialists. The latter decide whether a patient should be treated or not. In Figure A10 in the Appendix, I document the shock's effect on the logged number of GP referrals to hospital providers, the logged number of treatment decisions, and the logged number of first cancer treatments. I neither find the GP referrals, treatment decisions, nor the number of patients getting their first treatment to change. Further, the NHS sets goals concerning until when the large majority of patients should be seen by provider specialists and has the first treatment. Within two weeks after the urgent referral by a GP, a patient should have been checked by a specialist. 31 days after the decision to treat, the first treatment should have taken place and 62 days after the referral, treatment should have started. I show that there is no effect on cancer pathways, i.e. the number of patients being referred to a specialist, getting the decision to treat, or starting the first treatment within the NHS standard. This is indicative of no capacity constraints in cancer diagnostics and treatment. Fetzer (2019) already found that pressure on the workforce during the COVID-19 pandemic reduced the number of patients getting treated for cancer. Hence, I take this as evidence that EU nurse outmigration did not result in a similar lack of workers.

Capacity Measures. To further prove that highly treated providers do not reduce their service as a potential reaction to pressure on the workforce, I study the effect on logged bed occupation, the logged number of available operation theatres as well as the logged number of cancelled operations as indicators for potential bottlenecks. Finally, I examine whether absence rates among staff members change with the referendum. Figure A11 in the Appendix comprises the effect on all of these outcomes. In Panels A11a and A11b, the logged number of occupied general-purpose beds is unaffected by the shock. As admitted patients are mostly care-intensive relative to ambulant health episodes, this is an indicator of sufficient capacities to treat a similar number of patients relative to before the referendum. Further, operation theatres are unaffected by the treatment.

In Panels A11c and A11d, I show that the logged number of cancelled elective as well as the rare occurrence of urgent operations does not alter with the referendum. Cancellations of especially urgent cancellations due to a lack of workforce could have explained deteriorating health outcomes.

Lastly, in Panel A11e, there is no significant change in absence rates. Absence rates would have indicated a lack of workers which can have detrimental effects on healthcare provision (Fetzer and Rauh, 2022).

All results indicate that there is no effect on capacity proxies. Neither capacity constraints due to missing personnel such as the number of hospital beds or cancelled operations as well as physical constraints, like the number of operation theatres or the number of possible diagnostic tests (s. above), are more binding or restrictive after the prohibition in highly-exposed providers.

7 Alternative Mechanisms and Robustness Checks

Alternative Mechanisms. Healthcare performance of hospitals may not just be driven by the ability and the human capital of nurses. Other determinants could, for example, be hospital budget, management quality, or competition among hospital providers. Also, changes in the patient-nurse relationship can affect health outcomes. I, therefore, test such alternative mechanisms subsequently.

First, to ensure that heavily treated providers' performance does not change because of providers having fewer financial capacities (Shen, 2003), I analyse the balance sheet information of providers.[14] In doing so, I can test whether providers' treatment status is related to worse budgets and fewer expenditures per patient. Figure A12 in the Appendix reports the effect of the treatment on different balance sheet outcomes. Importantly, Panels A12a and A12b show that neither per-patient logged operative income nor per-patient logged operative expenditures decrease with the treatment. Also, per-patient logged staff costs, per-patient logged drug costs and per-patient logged plant and equipment assets are unaffected by the referendum (see Panels A12c to A12e). Hence, I can exclude that the alternative channel of financial constraints drives the healthcare effects found above.

Secondly, I can further use the balance sheet data to estimate the effect of the referendum on provider-level surplus as a proxy of management quality. This outcome remains unaffected by the treatment in the short and long run. Management quality also depends on the capacity of workers in administrative occupations within the hospitals. Therefore, I test whether the logged employment of workers in central functions (e.g., human resources, finances, etc.) or in hotel, property, and estates occupations (craftsmen, electricians, etc.) changes with the treatment (see Figure A13 in the Appendix). I again find no evidence for this.

Thirdly, I test whether competition among hospital providers changes with the treatment. Several papers have found competition to improve hospital performance (Bloom et al., 2015; Gaynor et al., 2012, 2016, 2013; Propper et al., 2008; Tafti and Hoe, 2022). As treat-

[14] This analysis is only based on balance sheet data of around 130 NHS Foundation trusts, a subsample of all trusts. Foundation trusts are more independent in decision-taking from the main NHS body than other trusts and had to mandatory report balance sheet data over the entire sample period.

ment prices are regulated, hospitals mainly compete on quality components (Camarda, 2022). Hospital providers in the NHS operate in narrow geographical areas. Hence, competition could decrease and reduce performance if exposed hospitals need to adjust to the withdrawal of nurses. Using patient data at the MSOA level (small census areas of around 7,000 people), I analyze how many MSOAs a provider supplies over time. Additionally, using the evolvement of Hirshman-Herfindahl-Index within MSOAs, I do not find differences in spatial competition in response to the exposure (see Figure A14 in the Appendix).

Lastly, to test whether the patient-nurse relationship changes with the influx of non-European nurses after the referendum, I make use of data from the NHS staff survey. Nurses can report whether they are physically violated by patients or feel bullied and harassed. In the Appendix, Figure A15 shows that there is no clear effect on these proxies of how well a patient-nurse relationship is.

Withdrawal of Doctors. As a quasi-placebo test, I analyze the impact of the Brexit referendum on EU doctor employment and providers' healthcare performance. Panel A16a of Figure A16 in the Appendix documents that doctors did not withdraw in a similar fashion as nurses. If at all, the growth in EU doctors decelerated, but the absolute number of EU doctors did not decrease after the Brexit referendum. Panel A16b of Figure A16 supports the picture as the bite calculated for doctors does not predict post-referendum changes in the share of EU doctors among all doctors.

Reasons for the different reactions to Brexit for EU doctors and EU nurses are multifold. A reason is that more than half of all EU nurses came to England in just the three years before the Brexit referendum (s. Panel A16a of Figure A16). This implies that doctors likely have been in the UK for a longer time and have settled.

As the employment of EU doctors is only barely affected by the referendum, I would not expect strong effects of the doctor bite in a difference-in-differences regression on healthcare outcomes. Figure A17 in the Appendix exemplarily shows that hospital-related deaths are unaffected by the doctor bite treatment.

Patient Pool & Composition. An alternative explanation for the found effects could be that patient composition changes differently in highly in comparison to weakly treated providers. For example, highly treated providers are in regions where generally the EU population is larger. Hence, the shock of the referendum might also affect the health distribution among patients of different providers heterogeneously. To show that this is not the case, I analyze patient characteristics. Table A3 in the Appendix shows that the treatment does not induce a change in the likelihood of admissions to be emergency admissions (see column (1)). In columns (2) to (4), I provide evidence that the age distribution among providers' patients is not sensitive to the shock. Columns (5) and (6) finally reveal no change in the gender distribution as well as no change in the average length of stay by patients.

I, further, check whether the overall population in the vicinity and catchment area of highly-treated providers changed over time to check for potential migration responses of patients. This would change the patient composition, too. Figure A18 in the Appendix shows that this is not the case.

Heterogeneity. The effects of nurse withdrawal might differ across providers. First, EU withdrawal might be non-homogenous across providers. Second, effects might vary with how stressed hospitals have been already before the referendum.

I study how the referendum's regional voting outcome predicts how strong the withdrawal of EU nurses is. More 'Leave' votes might induce workers to feel less welcome. I match 'Leave' vote shares to providers based on in which local authority district a provider's headquarter lies. Figure A19 in the Appendix shows the distribution and substantial variation of 'Leave' shares across providers. Figure A20 in the Appendix shows that providers with an above-median share of 'Leave' votes reveal a stronger decline in the number of EU nurses after the Brexit referendum. Panel A20a unveils that the share of nurses lost per percentage point exposure is larger for areas with more 'Leave' votes. Panel A20b presents the dynamic difference-in-differences effect of a one percentage point higher share of 'Leave' votes on EU nurse employment. The pooled estimate implies that shifting a provider from the 25th to the 75th quartile of the 'Leave' share distribution (by

14%) means that the provider will lose 6% more of its EU nurses. As the EU nurse withdrawal is stronger in providers with a high 'Leave' share, health effects might be stronger there, too. Panel A20c of Figure A20 shows that, indeed, the effects on observed deaths are significantly higher for such high-'Leave' areas.[15] These results show that pro-Brexit areas suffer from more detrimental Brexit effects. Becker et al. (2017) show that local NHS performance was one reason why people voted to Leave. But instead leaving the EU especially backfired for those areas which explicitly voted for it.

In Figure A21 in the Appendix, I further show that the stronger decrease in nurses in high 'Leave' areas is driven by fewer nurses joining the respective providers while the number of leavers is not higher than in other providers. These areas become less attractive to be joined due to their revealed perception of migrant workers. Admittedly, fewer joining nurses could be a result of a changing hiring policy of providers, but it seems unlikely that providers voluntarily deteriorate healthcare provision. Also, different budget changes between high and low 'Leave' areas are not the driver of the result since I showed above that income and expenditures are unaffected by the referendum.

Further, for policymakers, it is interesting to understand whether the effects especially arise in formerly well-performing or badly-performing providers. The latter would be the case if the exposure leverages other already existing deficits in a provider. I estimate the effect of the treatment on the distribution of observed deaths per expected deaths as a crucial performance measure in Figure A22 in the Appendix. Whether treatment effects occur at a particular tail of the distribution or along the complete distribution will be insightful about which kind of providers are prone to performance deteriorations. To estimate the counterfactual distribution, i.e. observed deaths per expected deaths observations in a world without EU nurse withdrawal, I follow Chernozhukov et al. (2013). They propose to estimate 'distribution regressions' which is the same regression as in model (1) with an outcome dummy turning one if an observed-to-expected deaths ratio of a provider in a year lies above a threshold p. The treatment effect then gives the effect on the ECDF at the threshold p. Repeating this for several p allows to elicit the full counterfactual dis-

[15] Pooled difference-in-differences effects significantly different with a p-value of 0.003.

tribution. I show that treatment significantly shifts the performance distribution in the middle to upper quantiles of the distribution. There is no effect on very well-performing providers. Hence, the treatment effect is not uniform across providers with providers of a very low bite experiencing no performance change.

I also perform further heterogeneity analysis in Figure A23 in the Appendix. There, I examine heterogeneity across providers of different sizes. I also test whether effects are different for providers with higher/lower bites than their nearest neighbors or providers whose nearest neighbor provider is far away. For example, Costinot et al. (2022) show that workers' labour market outcomes do not just depend on the employer's bite but also on how high the exposure of other firms in the local labour market is. These tests of scale effects, spillovers and competition as mitigating factors do not show statistically significant differences across providers.

Robustness. To ensure that my main finding - the mortality effect induced by a higher exposure to EU nurses pre-referendum - is robust to variation in the regression design, outliers in the treatment or outcome variable, or inference variations, I conduct several robustness checks in Figure A24 in the Appendix. Among others, I change the used regional fixed effect, winsorize and trim the bite distribution and the outcome variable and implement different methods of clustering. All these robustness checks, do not change the qualitative findings.

In addition, I run placebo tests of my main analysis on fatalities to test whether my results arise by pure chance. For this, I randomly sort all providers into a treatment group with a 25% probability and a control group with a 75% probability and rerun the dichotomous treatment difference-in-differences from Figure 8. I repeat this exercise 5000 times. Figure A25 in the Appendix shows that my baseline estimate lies outside of the placebo estimates.

8 Conclusion

This paper analyses the effects of the withdrawal of foreign nurses on firm performance. I exploit the heterogeneity of different English healthcare providers' exposure to employed EU nurses and compare the providers' performance before and after the Brexit referendum. Causally linked to the withdrawal of EU nurses, I find hospital-related deaths to increase. Also, other measures of health service such as the number of patients in accident and emergency units or the number of conducted diagnostic tests decrease after the shock. I interpret these results as clear evidence of the sizeable contribution of foreign skilled workers to firm performance. As mechanisms, I unveil a composition change in the workforce. My results have important implications for the upcoming decades of demographic change and the ever-increasing demand for health workers in highly developed countries.

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Appendix

8.1 Tables

Table A1: Descriptive Statistics

Statistic	# Providers	All Providers Mean (1)	All Providers SD (2)	% Pre-Referendum EU Nurses ≤p50 (3)	>p50 (4)	≤p75 (5)	>p75 (6)
Staff (Composition)							
Nurses	216	1,465	878	1,373	1,557	1,388	1,697
of which EU Nurses		104	146	30	178	49	269
Doctors	216	515	474	377	653	420	800
of which EU Doctors		50	54	27	73	34	97
Other Staff	216	3,314	1,877	3,261	3,367	3,282	3,410
of which EU Staff		129	127	60	198	87	255
Patients							
Catchment Population	140	391,323	229,385	351,506	431,140	375,029	440,205
Patient Episodes	200	93,312	85,775	74,761	111,863	79,854	133,686
Patient Admissions	200	78,429	71,494	62,847	94,012	66,988	112,753
Patient Emergency Admissions	200	28,815	26,738	23,435	34,185	25,060	40,080
A&E Cases	164	125,983	87,126	114,201	137,765	113,162	164,456
Diagnostic Tests	165	116,068	84,163	100,820	131,602	105,964	146,627
Cancer Referrals	141	11,848	6,911	10,831	12,880	11,519	12,846
Cancer Treatments	150	895	607	824	967	891	909
Performance Indicators							
Deaths	123	2,304	1,088	2,400	2,208	2,377	2,086
Unexpected Readmissions	199	7,540	7,232	6,170	8,925	6,501	10,638
Incidents of Harm to Patient	215	2,051	1,552	2,142	1,960	2,092	1,929
MRSA Cases	141	2.113	2.490	2.070	2.157	2.028	2.371
Bacteria Cases	141	36.610	26.158	38.028	35.171	36.811	36.000
Capacity Indicators							
Operation Theatres	147	21.510	14.096	20.419	22.616	21.048	22.885
Beds	200	514	466	426	603	444	725
Absence Rate	214	0.043	0.008	0.047	0.038	0.045	0.035
Cancelled Elective Operations	147	486	389	469	503	493	464
1[Cancelled Urgent Operation]	140	0.521	0.501	0.457	0.586	0.514	0.543
Accounts							
Surplus (million £)	135	-10.300	19,723	-6.542	-14.205	-7.678	-18.089
Operative Income (million £)	135	331.66	255.53	267.01	397.28	300.29	424.87
Operative Expenditures (million £)	135	336.07	359.35	268.57	404.79	302.36	436.20
Drug Expenditures (million £)	135	29.958	38.232	19.620	40.450	24.060	47.477
Staff Costs (million £)	135	210.31	142.94	177.17	243.95	195.79	253.46

Note: This table compares low to high exposure providers in the last full pre-treatment year 2015. For each variable, I document the mean for providers below and above the median bite.

Table A2: Smoothness Test

	$bite_i$		$1[bite_i > p75(bite_i)]$	
	(1)	(2)	(3)	(4)
Employed Doctors	0.046**	0.031	0.532**	0.453*
	(0.023)	(0.022)	(0.241)	(0.235)
Employed Nurses	−0.002	−0.002	0.026	0.024
	(0.006)	(0.003)	(0.057)	(0.032)
Employed Other Staff	0.059***	0.018	0.567***	0.259
	(0.020)	(0.018)	(0.190)	(0.157)
Deaths per Expected Deaths	0.047	−0.001	0.177	−0.158
	(0.052)	(0.032)	(0.544)	(0.551)
Incidents	0.003	−0.000	0.016	−0.004
	(0.006)	(0.005)	(0.051)	(0.048)
Patient Episodes per Nurse	0.023*	0.019	0.250*	0.165
	(0.014)	(0.012)	(0.149)	(0.136)
Patient Admissions per Nurse	0.016	0.016	−0.028	−0.073
	(0.013)	(0.017)	(0.140)	(0.176)
Emergency Admissions per Nurse	−0.002	0.008	−0.069	0.005
	(0.008)	(0.008)	(0.080)	(0.091)
A&E Cases per Nurse	0.006	0.009	−0.016	−0.007
	(0.011)	(0.009)	(0.096)	(0.079)
Diagnostic Tests	0.015	−0.005	0.121	−0.041
	(0.010)	(0.010)	(0.105)	(0.121)
Waiting List Length Diagnostic Tests	0.005	0.001	0.020	−0.003
	(0.009)	(0.007)	(0.076)	(0.073)
Cancer Referrals	0.004	−0.004	0.028	−0.037
	(0.007)	(0.003)	(0.054)	(0.035)
NHS Region FE	×	✓	×	✓

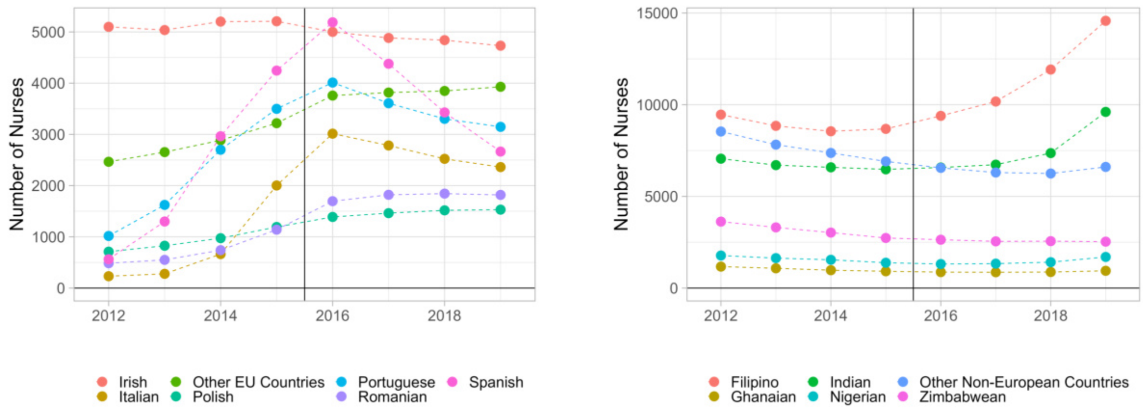
Note: This table compares how high-exposure providers developed in terms of outcome variables in comparison to low-exposure providers over the pre-treatment time period 2012 to 2015 for variables which were available from 2012 onwards. The reported coefficients report the estimate of a linear regression of the measure of exposure ($bite_i$ or $1[bite_i > p75(bite_i)]$) on provider-level change between 2012 and 2015 of the variable reported in the most-left column. Providers with zero values in 2012 excluded. Standard errors are heteroskedasticity-robust. Significance levels are given by $***$, $**$, $*$ for $p < 0.01$, $p < 0.05$, and $p < 0.1$.

Table A3: Patient Composition

	ln(Patients)					
	Emergency Admissions	00-44 years	45-74 years	75+ years	Female Admissions	Length of Stay
	(1)	(2)	(3)	(4)	(5)	(6)
$bite_i \times 1[Post]_t$	0.016	0.008	0.007	0.017	-0.004	0.001
	(0.022)	(0.015)	(0.008)	(0.019)	(0.005)	(0.022)
$1[bite_i > p75(bite_i)]_i \times 1[Post]_t$	0.059*	0.009	0.021	0.024	-0.008	0.023
	(0.033)	(0.029)	(0.015)	(0.039)	(0.008)	(0.046)

Note: Regressions (1) and (5) control for the overall number of admissions at the provider-year level and regressions (2), (3), (4) and (6) control for the overall number of episodes at the provider-year level to capture general developments in the outcomes' respective umbrella measures of hospital visits. Regressions are run separately for the continuous difference-in-differences and the binary treatment difference-in-differences. Standard errors are clustered at the provider level. All outcome variables are logged. Significance levels are given by $***$, $**$, $*$ for $p < 0.01$, $p < 0.05$, and $p < 0.1$.

8.2 Figures



(a) EU Nurse Employment (b) Rest of World Employment

Figure A1: Nurse Employment by Nationality

Note: Panel (a) reports the absolute number of EU nurses by nationality over time in the NHS. Panel (b) reports the absolute number of 'Rest of World' nurses by nationality over time in the NHS.

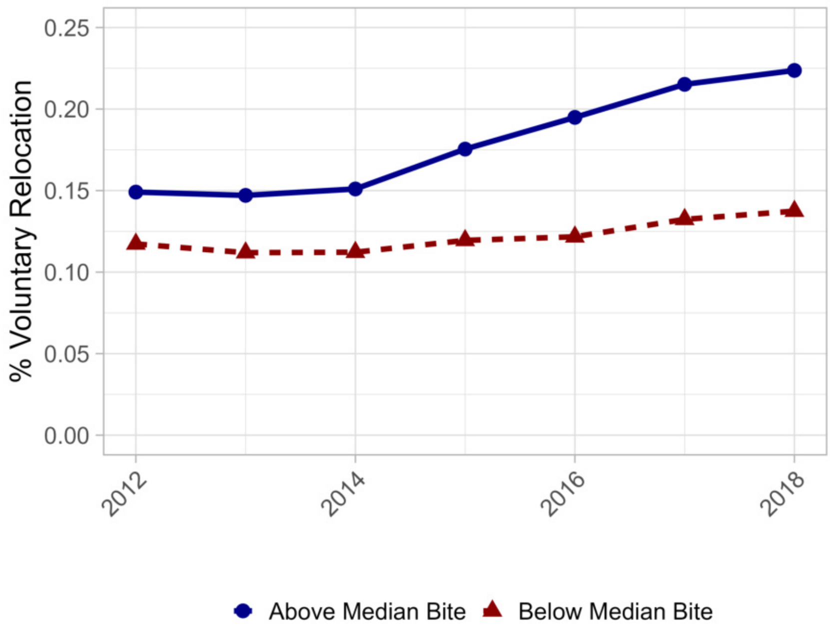


Figure A2: Share of Nurses Leaving with Reason 'Relocation'

Note: The plot reports the share of nurses that leave with the reasons 'relocation' for the subgroups of health education regions ($N = 13$) with below and above median bite. As providers all belong to one health education region, the bite for health education regions is a weighted average of the provider bites, weight by the number of patients.

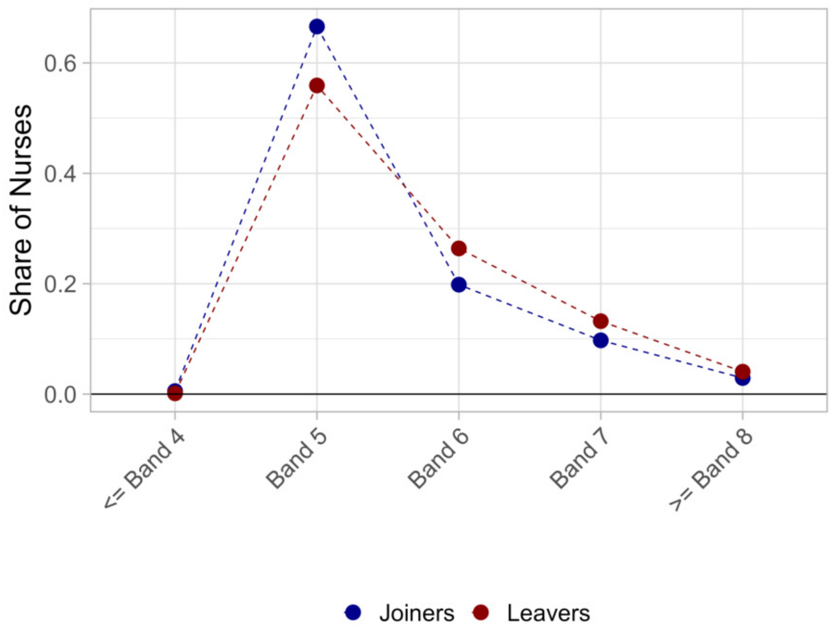
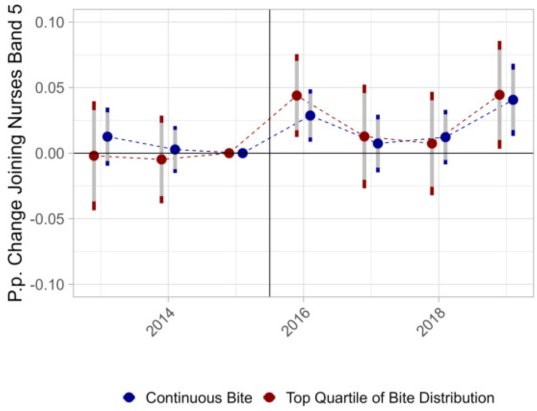
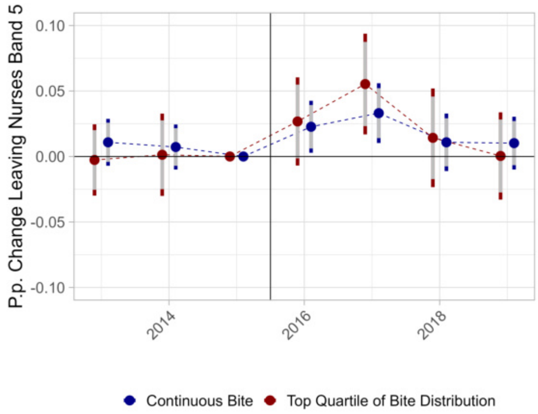


Figure A3: Nurse Grade Distribution

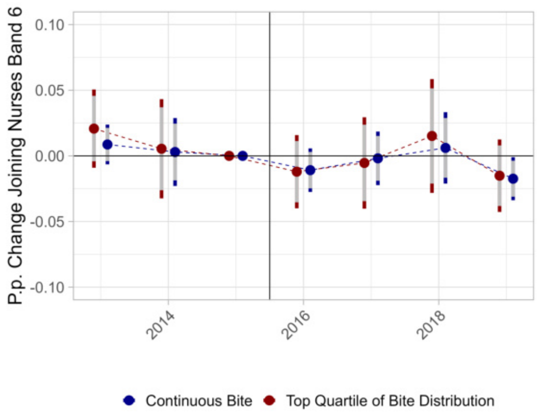
Note: The plots report the density of grade distribution among joiners and leavers in the staff group of nurses.



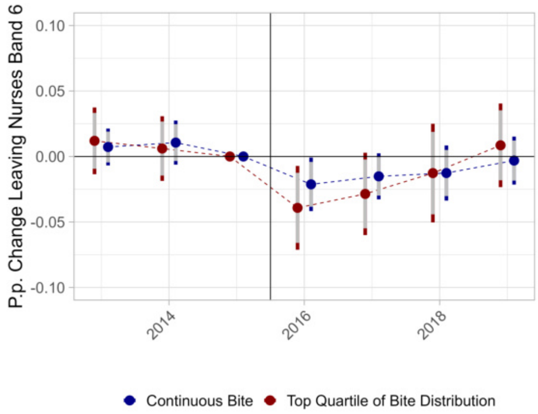
(a) Band 5 - Joiners



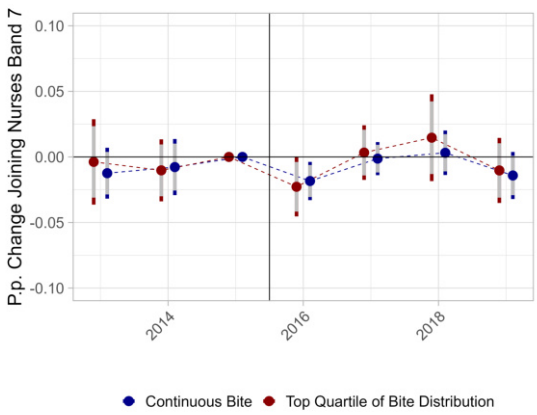
(b) Band 5 - Leavers



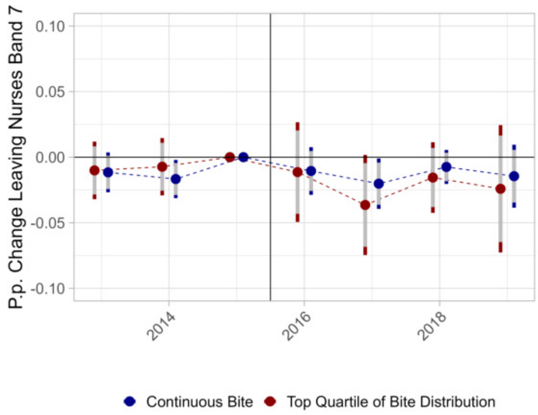
(c) Band 6 - Joiners



(d) Band 6 - Leavers



(e) Band 7 - Joiners



(f) Band 7 - Leavers

Figure A4: Effect on Joiners and Leavers by Wage Band

Note: Panel (a) documents the effect of the referendum on the logged annual, provider-level number of nurses joining wage band 5. Panel (b) documents the effect of the referendum on the logged annual, provider-level number of nurses leaving wage band 5. Panel (c) documents the effect of the referendum on the logged annual, provider-level number of nurses joining wage band 6. Panel (d) documents the effect of the referendum on the logged annual, provider-level number of nurses joining wage band 6. Panel (e) documents the effect of the referendum on the logged annual, provider-level number of nurses joining wage band 7. Panel (f) documents the effect of the referendum on the logged annual, provider-level number of nurses joining wage band 7. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.

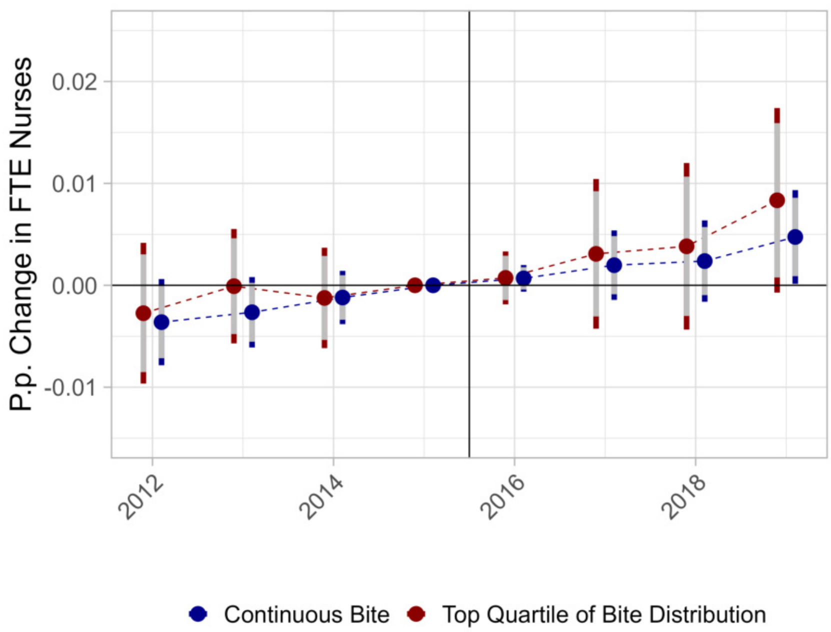


Figure A5: Effect on Full-Time Employment Nurses

Note: The plot reports the effect of the referendum on the annual, provider-level share of full-time equivalents per nurse employed (FTE/Headcount at provider level). I winsorize the variable at the 5th and 95th percentile to account for outliers. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.

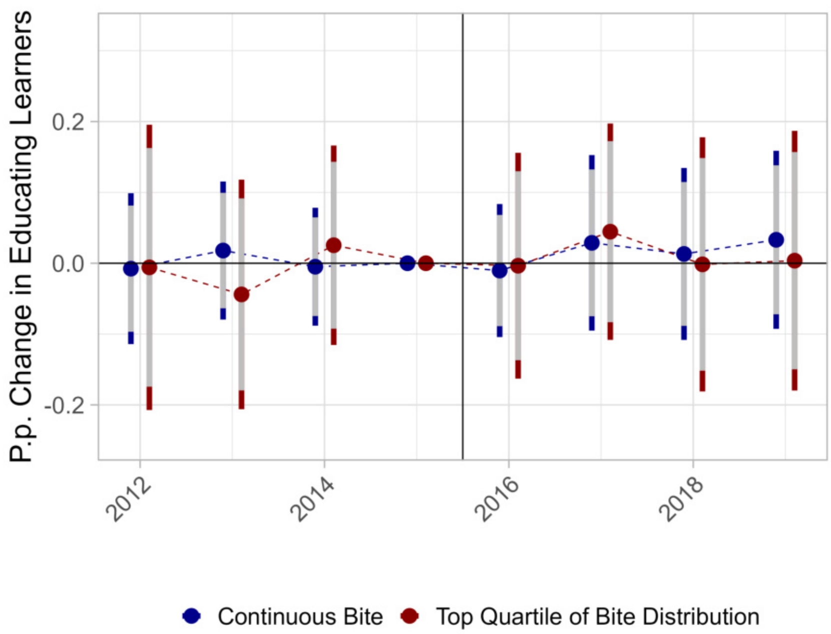


Figure A6: Effect on Educating Nurses

Note: The plot reports the effect of the referendum on the annual, provider-level likelihood to have nursing learners, i.e. nurses in their training, in the staff. Standard errors are clustered at the provider level. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.

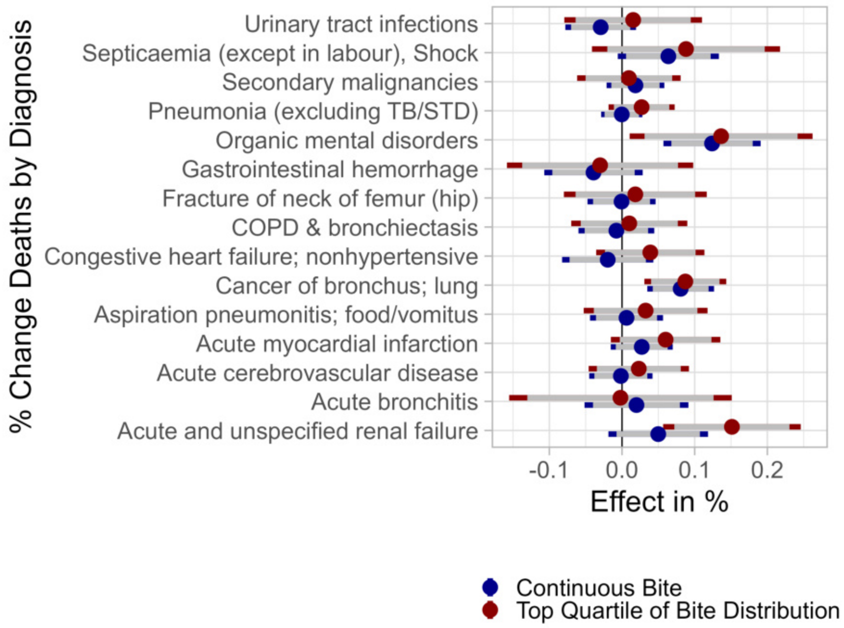
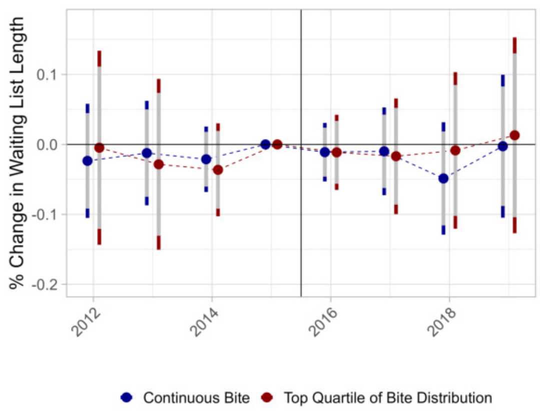
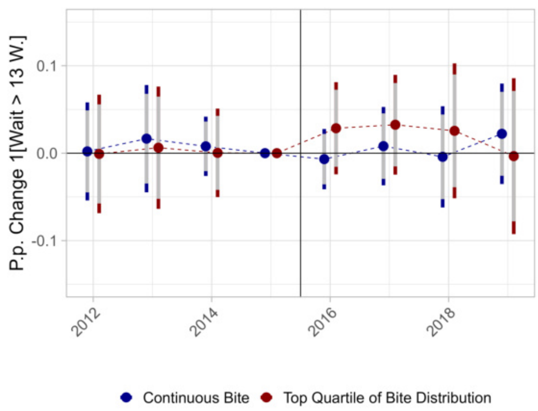


Figure A7: Effect on Diagnosis-Specific, Hospital-Related Deaths

Note: The plot reports the effect of the referendum on the logged annual, provider-level number of observed deaths by diagnosis. I flexibly control for the number of expected deaths (as calculated by the NHS based on patient characteristics), the providers' overall number of elective and emergency admissions, and the provider's number of elective and emergency admissions in the age group of 80+ years. I only include those providers in the regressions for which data is reported and calculated for all years in the sample (2013-2019). Standard errors are clustered at the provider level. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.



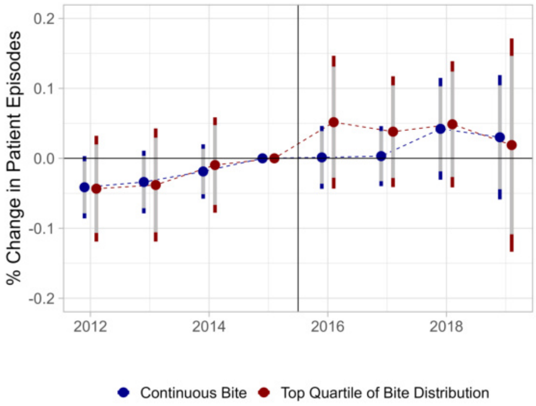
(a) Waiting List Length



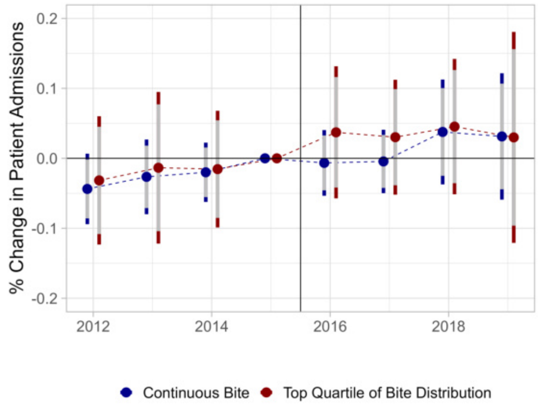
(b) 1[Waiting at least 13 Weeks > 0]

Figure A8: Effect on Diagnostics Waiting List

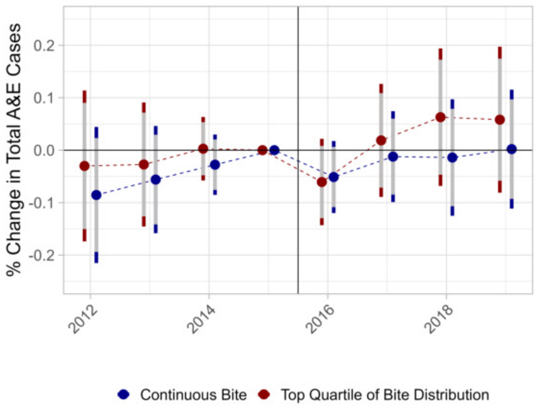
Note: The left plot reports the effect of the referendum on the logged monthly, provider-level waiting list length. The right plot reports the effect of the referendum on the likelihood that at least one patient has to wait for more than 13 weeks for a specific test. In contrast to equation (4), I include provider-test and test-NHS region-month fixed effects. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.



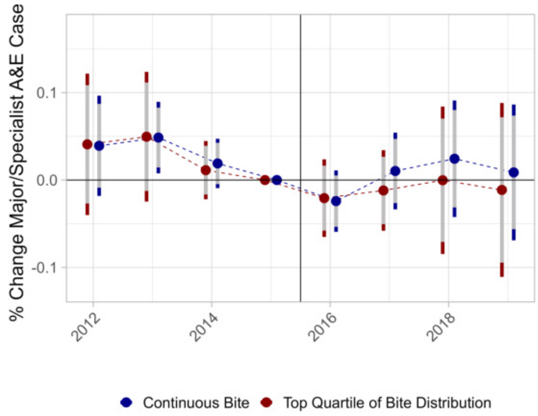
(a) Patient Episodes per Nurse



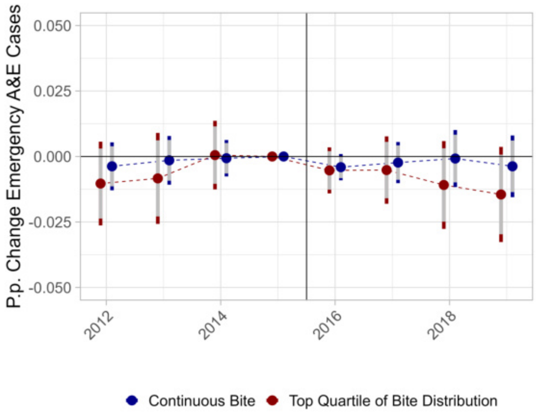
(b) Patient Admissions per Nurse



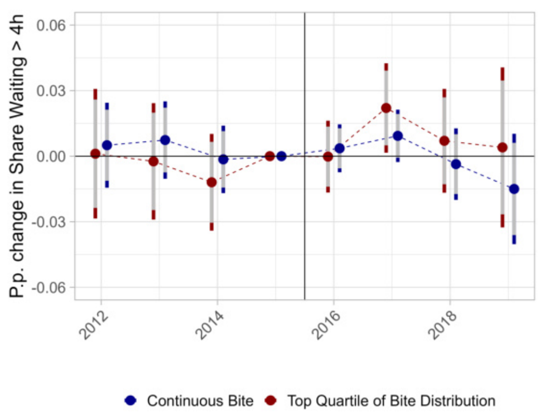
(c) A&E Cases (Total) per Nurse



(d) A&E Cases (Major) per Nurse



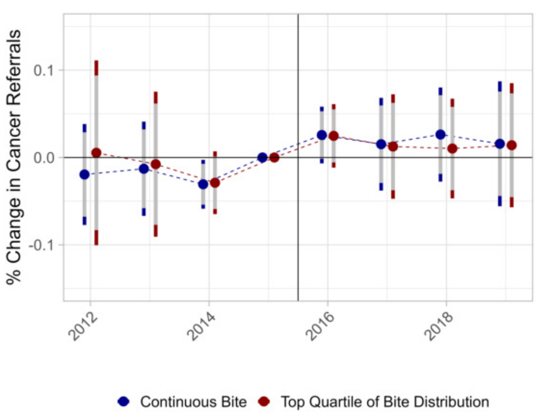
(e) % A&E Cases (Emergency)



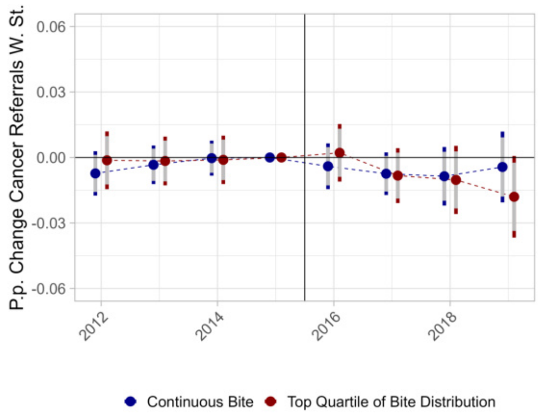
(f) Major A&E Cases (% Waiting >4h)

Figure A9: Effect on Number of Patients

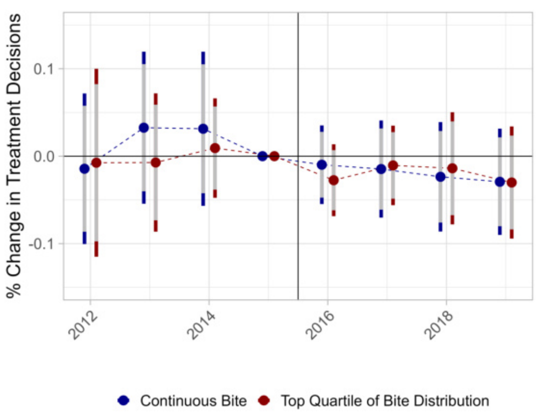
Note: Plot (a) documents the effect of the referendum on the logged annual, provider-level number of patient episodes per nurse. Plot (b) documents the effect of the referendum on the logged annual, provider-level number of patient admissions per nurse. Plot (c) documents the effect of the referendum on the logged monthly, provider-level total number of A&E cases per nurse. Plot (d) documents the effect of the referendum on the logged monthly, provider-level number of major A&E cases per nurse. Plot (e) documents the effect of the referendum on the logged monthly, provider-level share of A&E cases leading to an emergency admission. Plot (f) documents the effect of the referendum on the monthly, provider-level share of A&E cases which have waited for more than four hours. Regressions follow regression equation (4). Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.



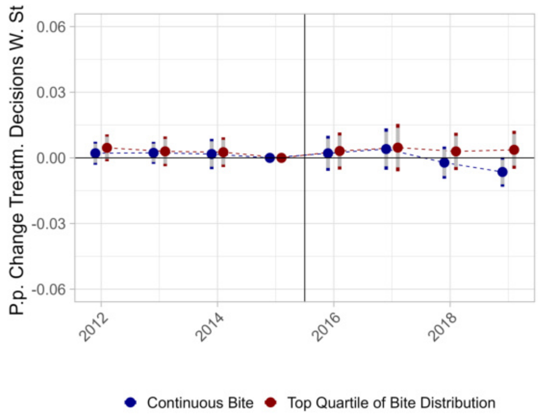
(a) Cancer Referrals



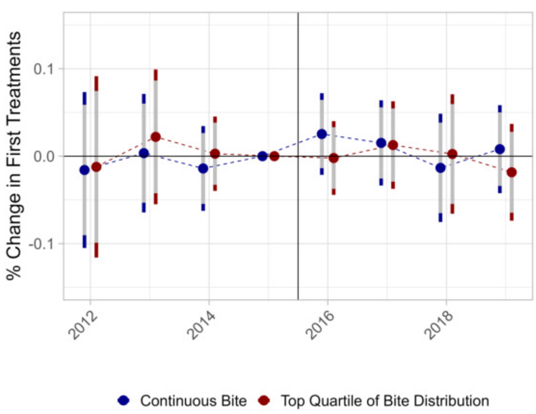
(b) % Cancer Referrals in Time (14 d.)



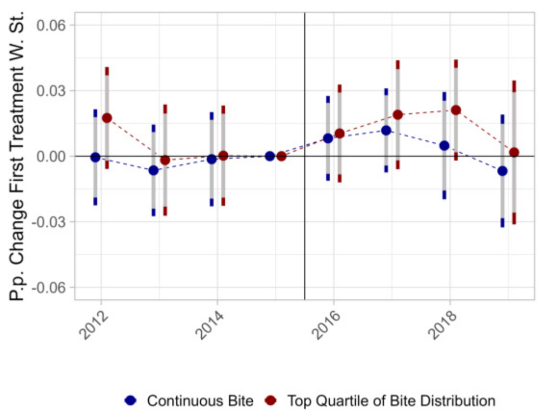
(c) Cancer Decision to Treat



(d) % Cancer Decision to Treat in Time (31 d.)



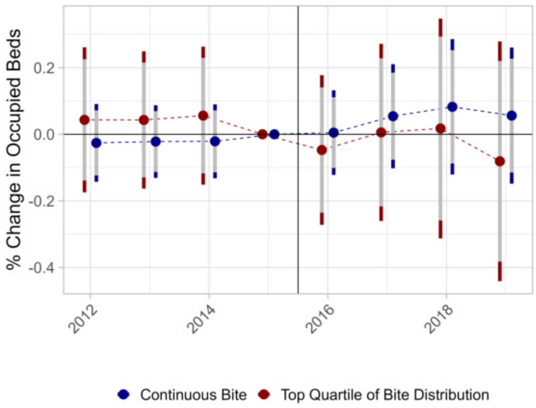
(e) Cancer First Treatments



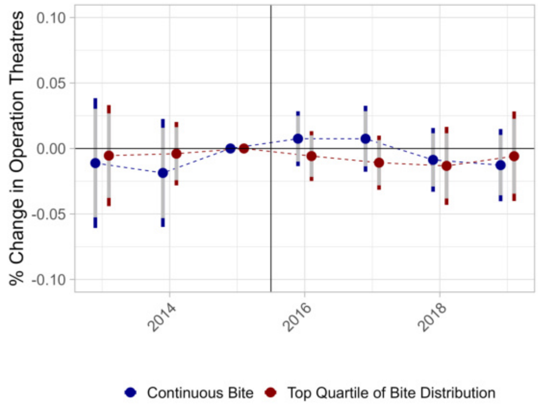
(f) % Cancer First Treatments in Time (62 d.)

Figure A10: Effect on Cancer Patients

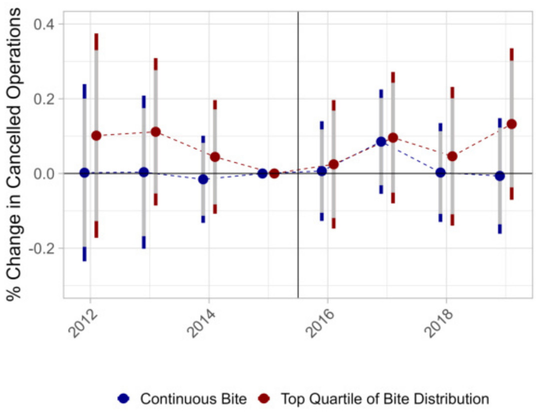
Note: Plot (a) documents the effect of the referendum on the logged monthly, provider-level total number of cancer patient referrals by GPs. Plot (b) documents the effect of the referendum on the logged monthly, provider-level total number of cancer patient referrals by GPs in time (14 days). Plot (c) documents the effect of the referendum on the logged monthly, provider-level total number of treatment decisions by provider specialists. Plot (d) documents the effect of the referendum on the logged monthly, provider-level total number of treatment decisions by provider specialists within time (31 days). Plot (e) documents the effect of the referendum on the logged monthly, provider-level total number of first treatments. Plot (f) documents the effect of the referendum on the logged monthly, provider-level total number of first treatments within time (61 days after referral). Regressions follow regression equation (4). Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.



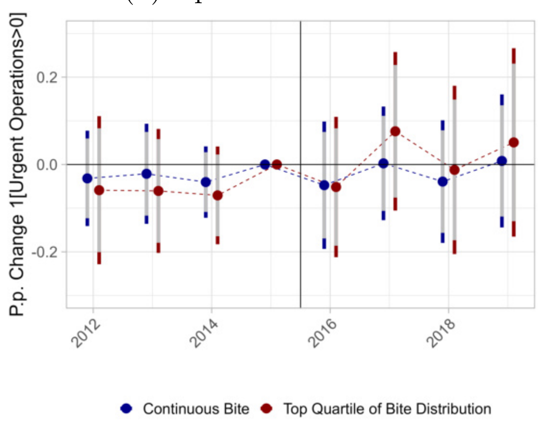
(a) Occupied Beds



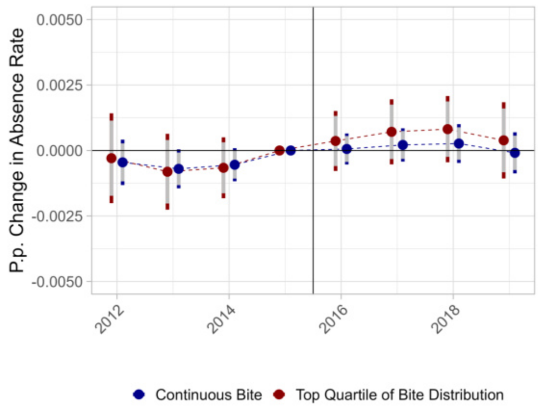
(b) Operation Theatres



(c) Cancelled Elective Operations



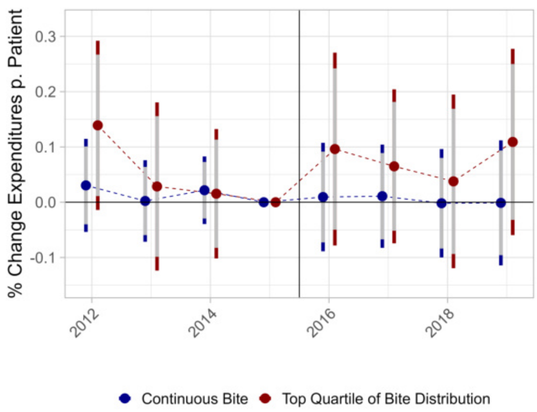
(d) 1[Cancelled Urgent Operations>0]



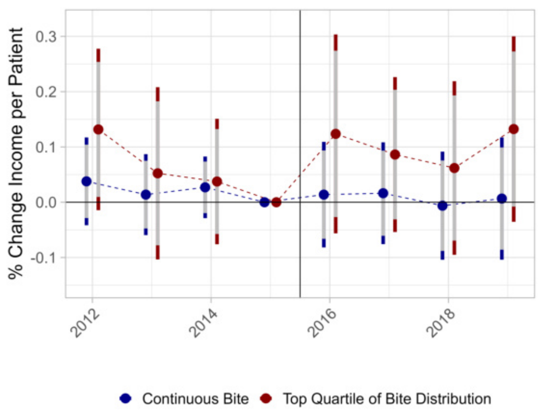
(e) Absence Rates

Figure A11: Effect on Capacity Measures

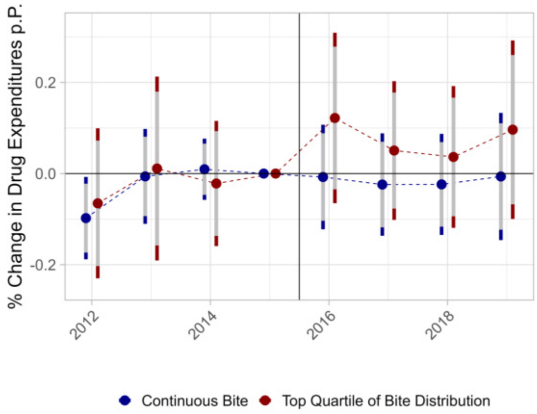
Note: Plot (a) documents the effect of the referendum on the logged quarterly, provider-level number of occupied, general purpose beds. Plot (b) documents the effect of the referendum on the logged quarterly, provider-level number of operation theatres operated. Plot (c) documents the effects of the referendum on the logged quarterly, provider-level number of cancelled elective operations. Plot (d) documents the effects of the referendum on a monthly, provider-level dummy indicating that at least one urgent operation was cancelled. Plot (e) documents the effect of the referendum on the annual, provider-level absence rate. All regressions are estimated following equation (4). Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.



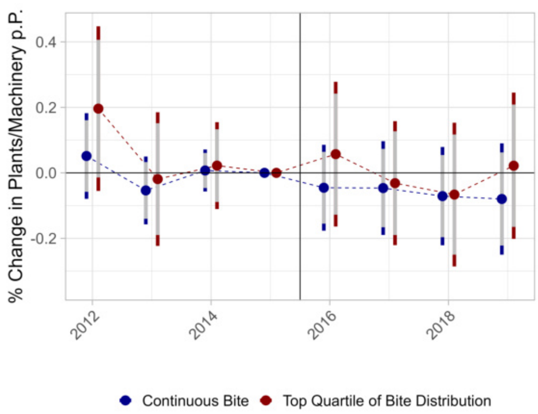
(a) Per-Patient Operative Expenditures



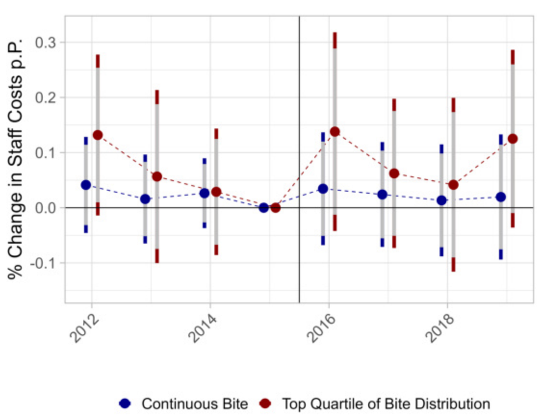
(b) Per-Patient Operative Income



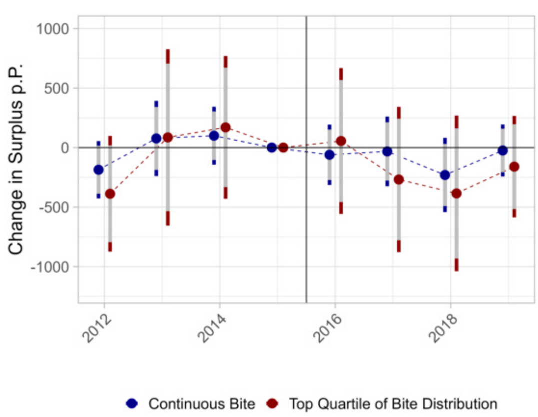
(c) Per-Patient Drug Expenditures



(d) Per-Patient Plants/Equipment Assets



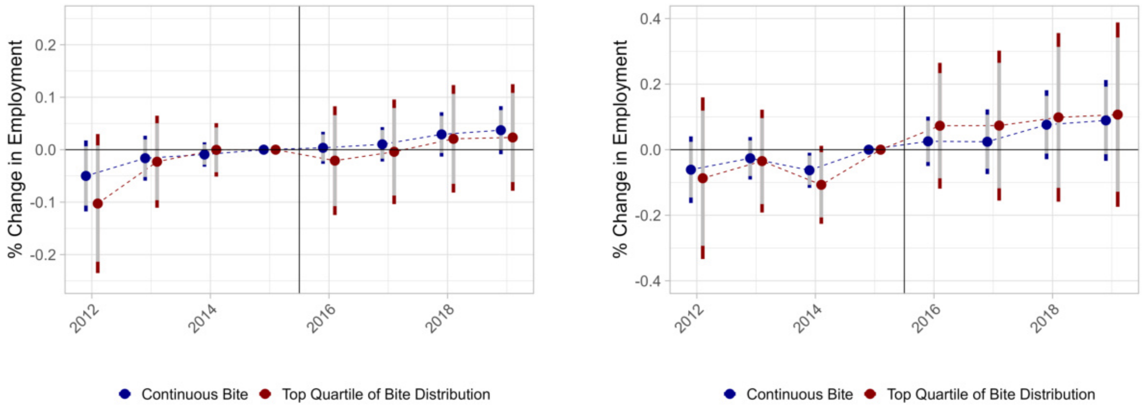
(e) Per-Patient Staff Costs



(f) Per-Patient Surplus

Figure A12: Effect on Provider-Level Balance Sheet Information

Note: All plots give the dynamic difference-in-differences estimates for the effect on an balance sheet outcome. The outcome variable for the analysis on 'per-patient surplus' is winsorized at the 5th and 95th percentile to account for large outliers as the outcome variable is in levels due to negative levels. Outcomes of Panels (a) - (e) are logged. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.

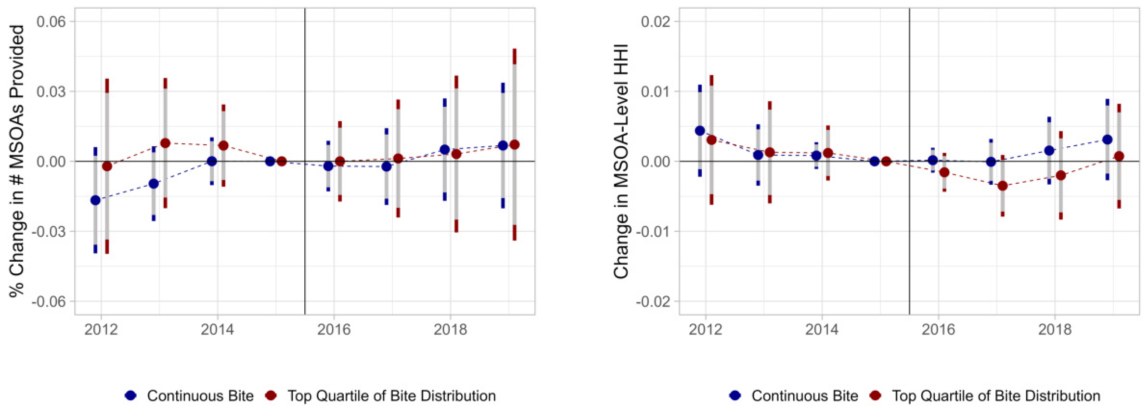


(a) Central Functions

(b) Hotel, Property & Estates

Figure A13: Effect on Provider-Level Administrative Employment

Note: All plots give the dynamic difference-in-differences estimates for the effect on logged occupation-specific employment. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.

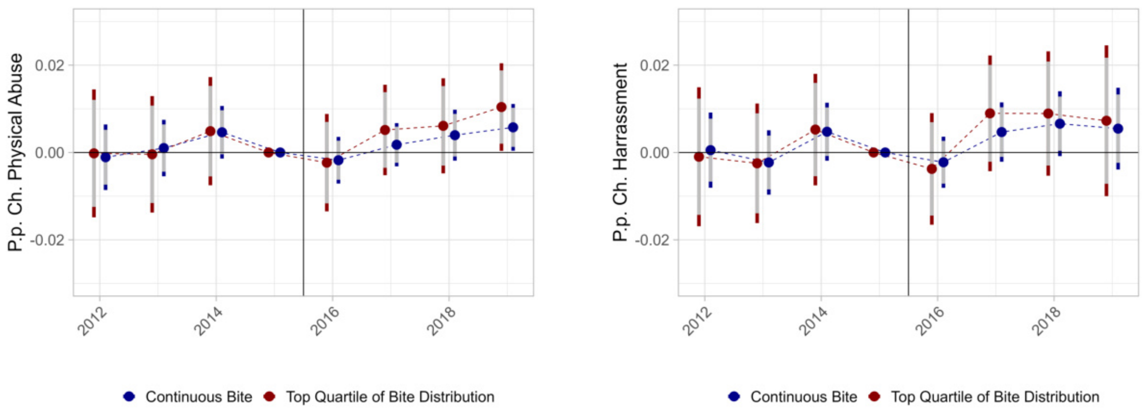


(a) # MSOAs Provided

(b) MSOA-Level HHI

Figure A14: Effect on Spatial Competition

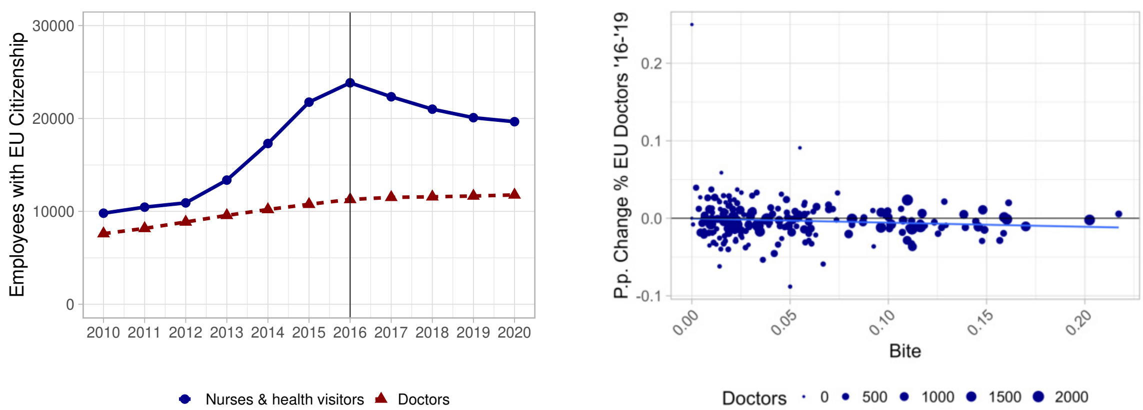
Note: Panel (a) reports the effect of the referendum on the logged annual, provider-level number of MSOAs provided. MSOA-provider-specific data on patients for acute providers comes from Public Health England. Panel (b) reports the effect of the referendum on the annual, MSOA-level Hirshman-Herfindahl-Index based on the number of patients each provider supplied in an MSOA in a year. In (a), I regress the outcome on the MSOA-level bite interacted with year fixed effects as well as MSOA fixed effects and health region-year fixed effects in a difference-in-differences model. In (b), I regress the outcome on the provider-level bite interacted with year fixed effects as well as provider fixed effects and health region-year fixed effects in a difference-in-differences model. There are 6,791 MSOA regions in England as of 2019. To account for differences in MSOA size, I control for the logged number of patients overall per provider and year in (a). For (a), I match the bite of the largest provider in an MSOA before the referendum to an MSOA. This is similar to Fetzer and Rauh (2022). I do the same to match the NHS region to MSOAs. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.



(a) Physical Abuse (b) Harassment & Bullying

Figure A15: Effect on Provider-Level Patient-Nurse Relationship

Note: All plots give the dynamic difference-in-differences estimates for the effect on staff survey outcomes (share of workers reporting physical abuse in the last 12 months, share of workers reporting harassment and bullying in the last 12 months). Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.



(a) Absolute Employment (b) Bite and Actual Change

Figure A16: Robustness Checks: EU Doctors' Reaction to Brexit Referendum

Note: Panel (a) gives the absolute number of EU nurses and health visitors as well as the absolute number of EU doctors over time. Panel (b) gives the relation between the calculated bite for doctors and the actual change in the share of EU doctors among all doctors.

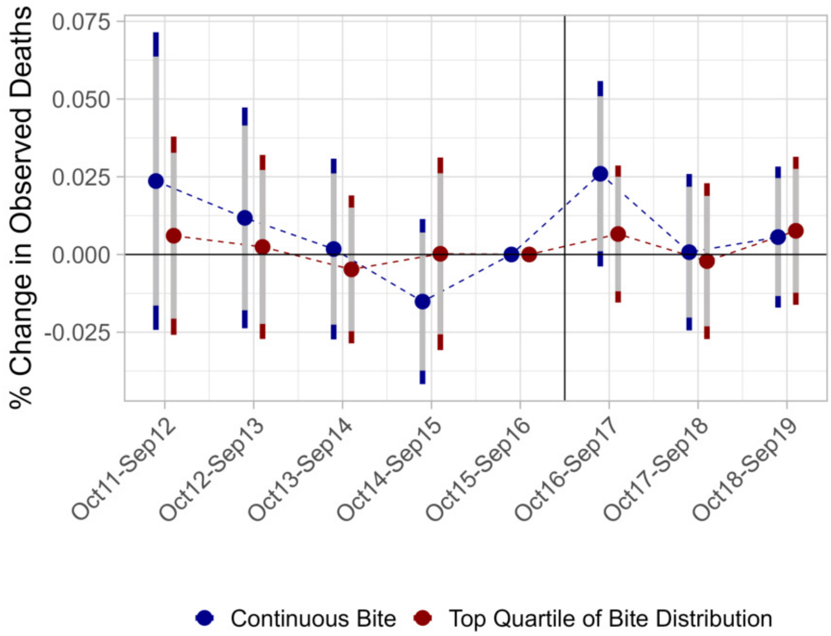


Figure A17: Effect on Hospital-Related Deaths (Doctor Bite)

Note: The plot reports the effect of the referendum on the logged annual, provider-level deaths following regression equation (4). Deaths are counted in the statistic when a patient visited a provider throughout the last thirty days for any reason. I flexibly control for the number of expected deaths (as calculated by the NHS based on patient characteristics), the providers' overall number of elective and emergency admissions and the provider's number of elective and emergency admissions in the age group of 80+ years. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.

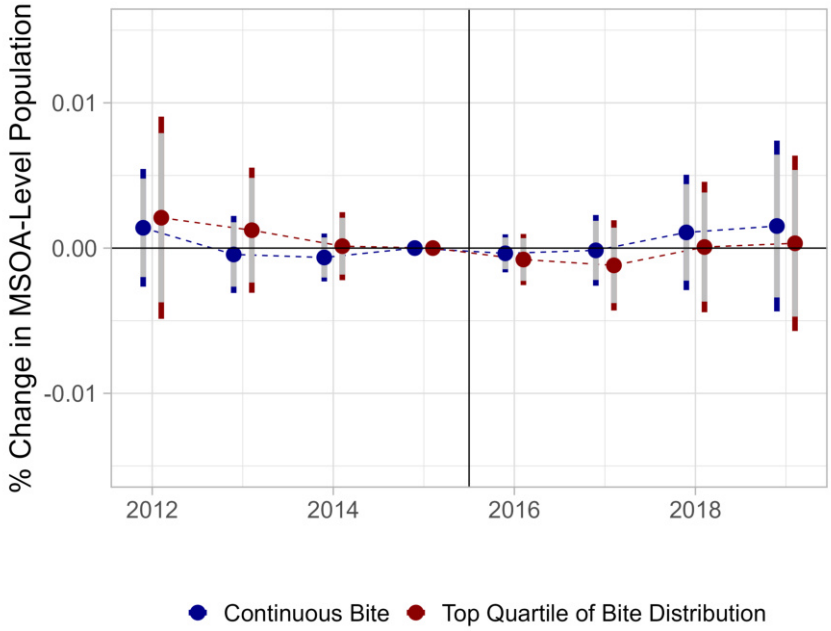


Figure A18: MSOA-Level Population

Note: This plot reports the effect of the referendum on the logged annual, MSOA-level population. Population data comes from the Office for National Statistics (ONS), patient data comes from Public Health England. I regress the outcome on the MSOA-level bite interacted with year fixed effects, MSOA fixed effects and health region-year fixed effects. There are 6,791 MSOA regions in England as of 2019. I match the bite of the largest provider in an MSOA before the referendum to an MSOA. This is similar to Fetzer and Rauh (2022). I do the same to match the NHS region to MSOAs. Standard errors are clustered at the MSOA level. 90% and 95% confidence intervals are reported.

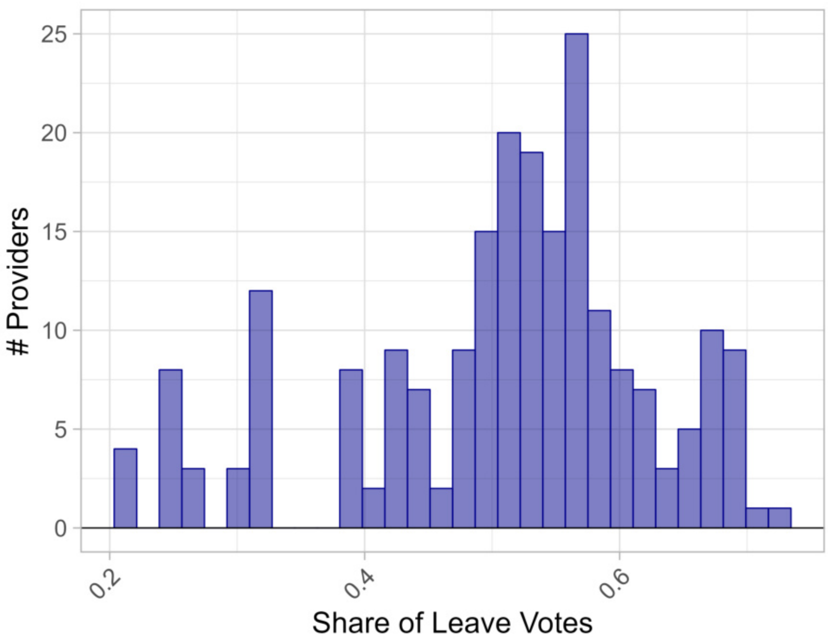
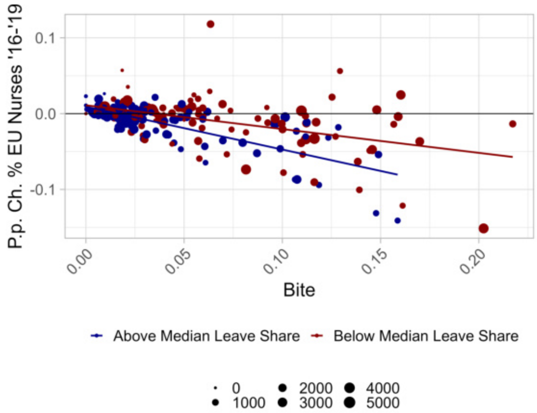
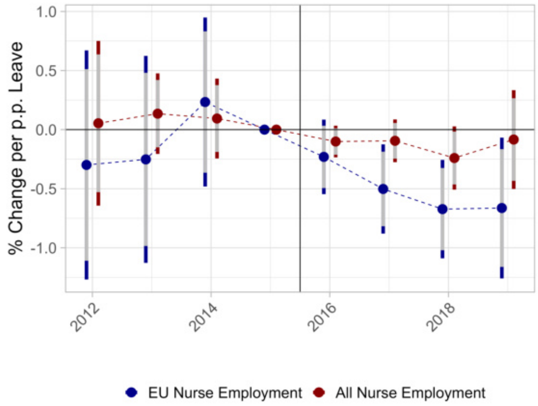


Figure A19: Provider-Level Distribution of ‘Leave’ Vote Share

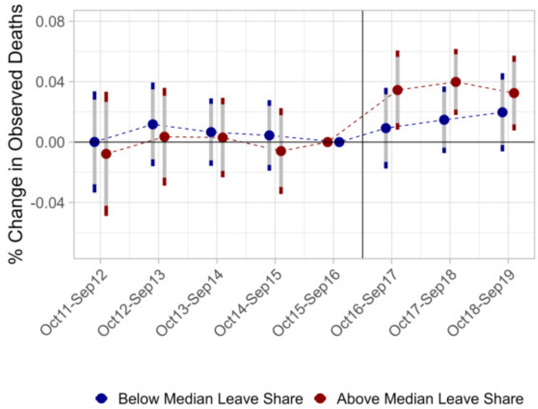
Note: The plot gives the distribution of the vote share for ‘Leaving the EU’ in the Brexit referendum in 2016. I match ‘Leave’ vote shares to providers based on in which local authority district a provider’s headquarter lies.



(a) Predictive Power of Bites



(b) EU Nurse Employment



(c) Deaths by Vote Share

Figure A20: Effect of 'Leave' Vote Share on EU Nurse Withdrawal

Note: Panel (a) proves the heterogeneous predictive power of the pre-referendum share of EU nurses among all nurses at the provider level for the post-referendum decrease in EU nurses. Panel (b) documents the effect of the local 'Leave' vote share on the logged number of EU nurses in a provider. The underlying difference-in-differences regression regresses the outcome on the provider-level, local 'Leave' share interacted with year fixed effects, provider fixed effects and health region-year fixed effects. I match 'Leave' vote shares to providers based on in which local authority district a provider's headquarter lies. Panel (c) reports the effect of the referendum on the logged annual, provider-level deaths following regression equation (4). Deaths are counted in the statistic when a patient visited a provider throughout the last thirty days for any reason. I flexibly control for the number of expected deaths (as calculated by the NHS based on patient characteristics), the providers' overall number of elective and emergency admissions and the provider's number of elective and emergency admissions in the age group of 80+ years. Controls are as in the baseline regressions above. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.

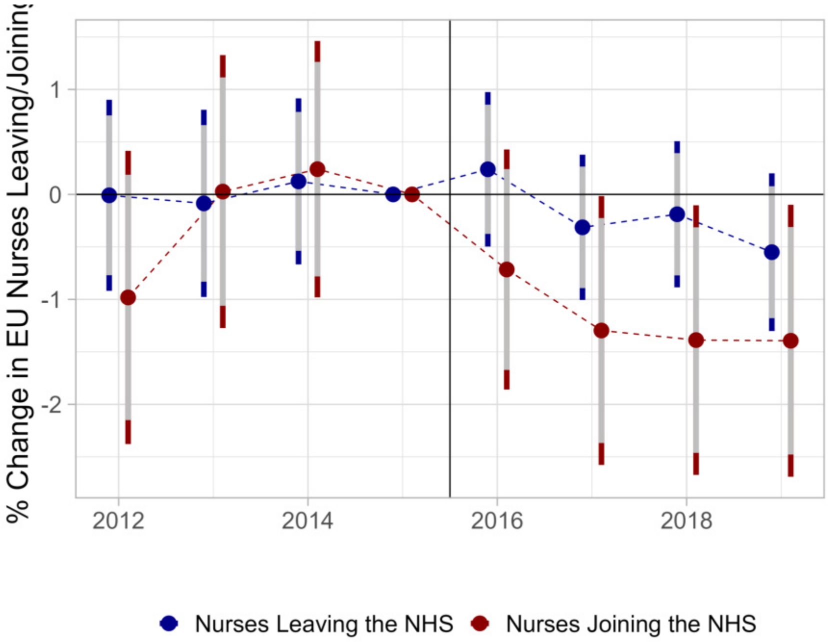


Figure A21: Effect of 'Leave' Vote Share on EU Nurse Turnover

Note: The plot gives the dynamic difference-in-differences effect of the share of 'Leave' votes on the logged annual, provider-level number of joining and leaving EU nurses. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.

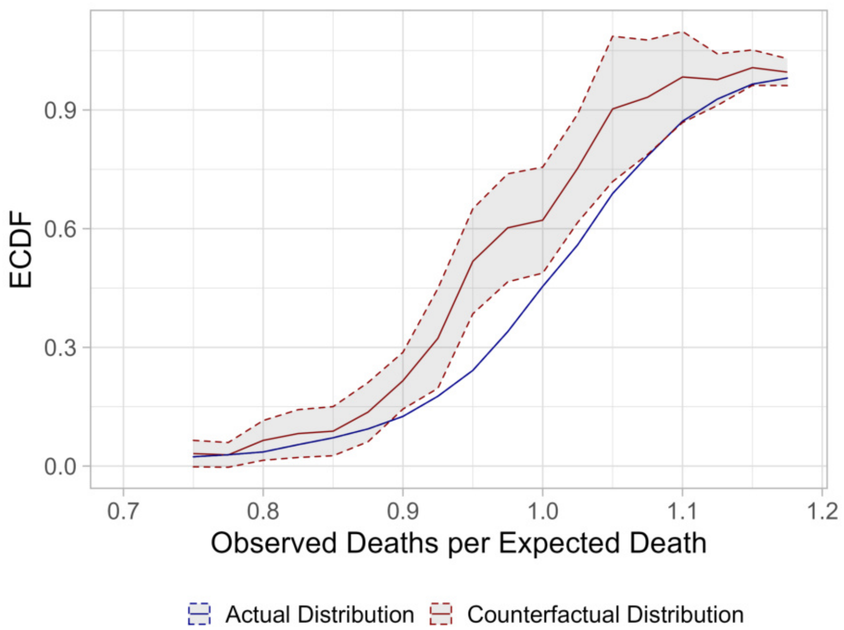
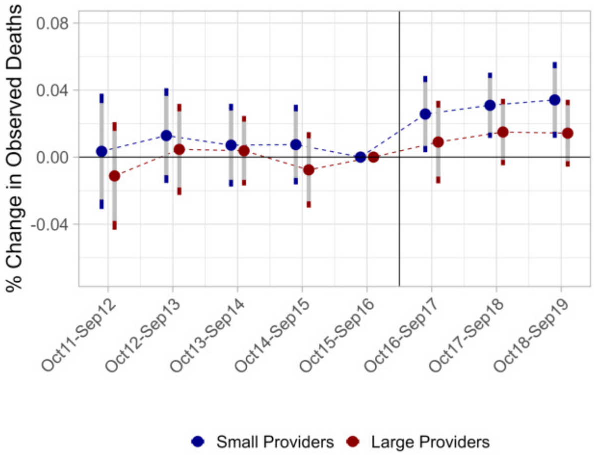
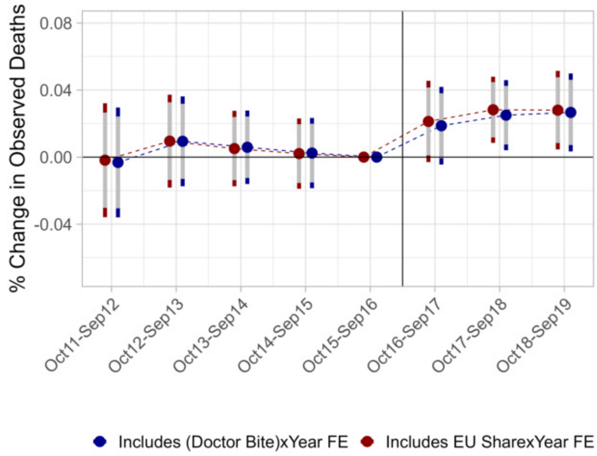


Figure A22: Effect Along the Observed/Expected Deaths Distribution

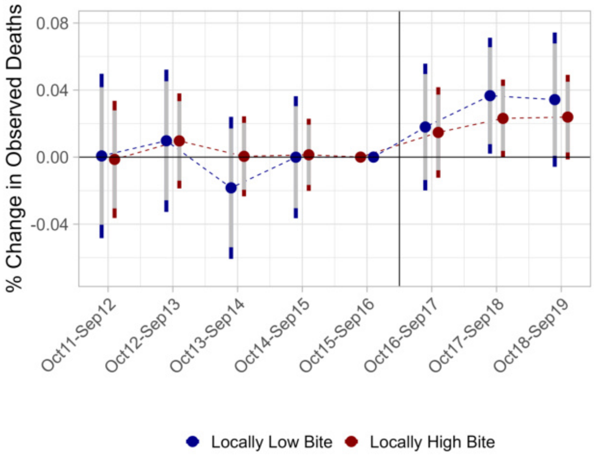
Note: The plot gives the observed distribution of observed-to-expected deaths ratios at the annual, provider-level. The counterfactual distribution is estimated through distribution regressions as in Chernozhukov et al. (2013) at equi-distant ratios of 0.025. A distribution regressions is estimated as in model (1) with an outcome variable indicating wether the observed-to-expected deaths ratio is above a threshold value q. Repeating this for many thresholds q allows the elicitation of the complete counterfactual distribution. Standard errors are clustered at the provider level. 90% confidence intervals are reported.



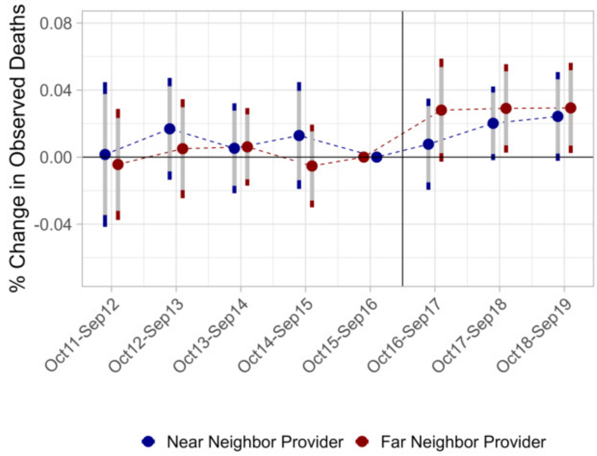
(a) Provider Size



(b) Doctor & EU Exposure



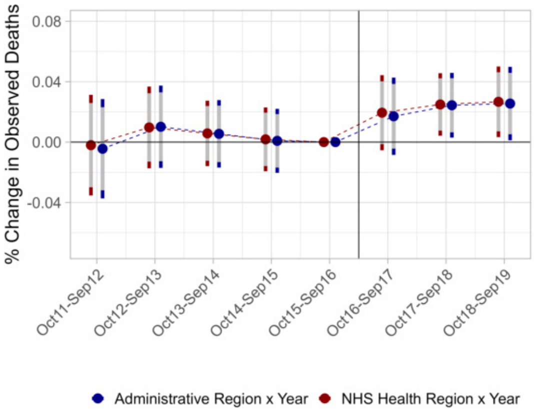
(c) Regional Bite Level



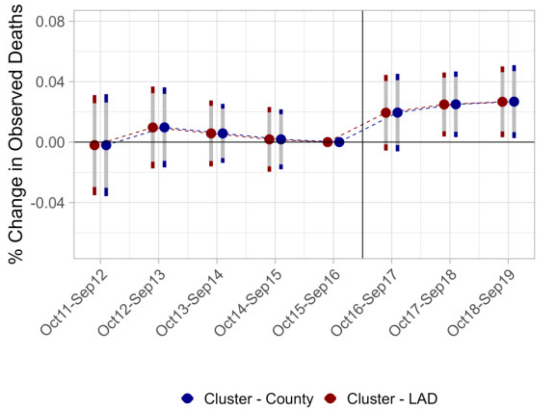
(d) Distance to Nearest Competitor

Figure A23: Robustness Checks/ Heterogeneity: Provider Size, Exposure to EU Doctors and Citizens, Brexit Voting Outcome, Treatment Group Allocation, Spillover Effects

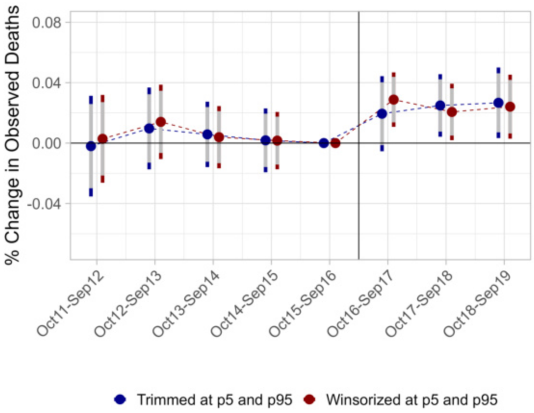
Note: All plots give the effect on the logged annual, provider-level number of observed deaths. Panel (a) distinguishes between large and small providers measured as the overall number of expected deaths. Panel (b) controls for the provider-level bite of doctors and the geographical exposure to EU workers in the working age population (16-64 years) at the county level. County level means counties and unitary authority districts ($N = 151$). I match providers to counties based on in which county the provider's headquarter lies. Panel (c) gives the heterogenous effects over time for providers with higher or lower bites than their nearest neighbor based on the minimum linear distance between head offices. Panel (d) gives estimates separately for providers, which have an above or below median distance to the nearest acute provider (minimum linear distance between head offices). Controls as in the baseline regressions. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.



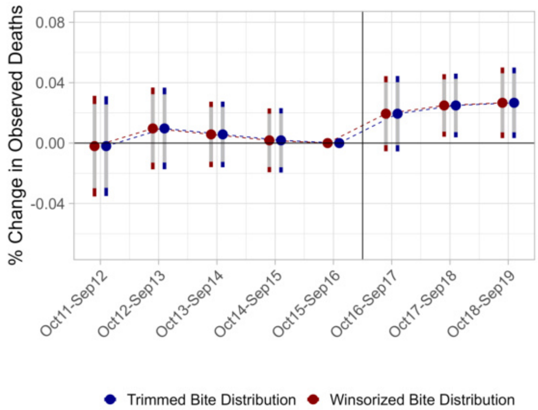
(a) Fixed Effects Variation



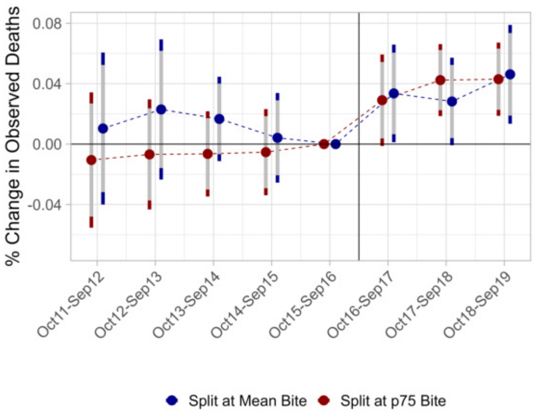
(b) Inference Variation



(c) Trimmed/Winsorized Outcome



(d) Trimmed/Winsorized Bites



(e) Binary Treatment

Figure A24: Empirical Robustness Checks: Variation of Region-Time Fixed Effects, Level of Clustering and Trimming and Winsorizing of Outcome and Bite Distribution

Note: All plots give the dynamic difference-in-differences estimates for the effect on the logged annual, provider-level number of observed deaths. Panel (a) distinguishes between the baseline approach of NHS region - year fixed effects and administrative region - year fixed effects. Panel (b) distinguishes between clustering at the provider level (baseline) and at the local authority district level or county level. County level means metropolitan and non-metropolitan counties, Greater London and unitary authority districts. I match providers to the different administrative regions based on in which region a provider's headquarter lies. Panel (c) trims and winsorizes the outcome variable at the 5th and 95th percentile. Panel (d) trims and winsorizes the bite distribution at the 5th and 95th percentile. Panel (e) presents results for two variants of a dichotomous treatment based on the mean bite or the 75th percentile of the bite distribution. Controls as in the baseline regressions. Standard errors are clustered at the provider level. 90% and 95% confidence intervals are reported.

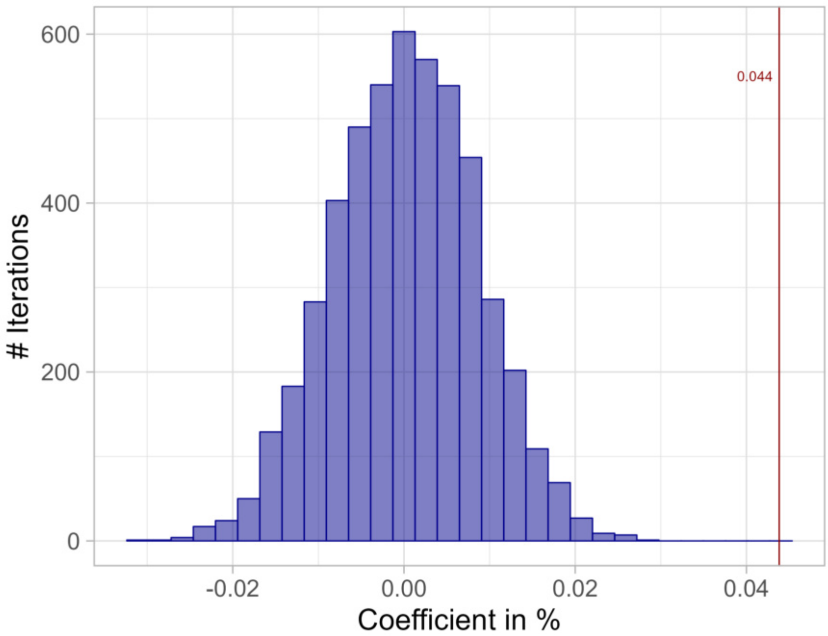


Figure A25: Placebo Test

Note: The plot gives the distribution of placebo treatment effects of 5000 regressions where I randomly sort providers into treatment (1/4) and control group (3/4). The red line gives the baseline estimate from the dichotomous treatment in my baseline estimation.