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Impacts of Norway's extended free choice reform on waiting times and hospital visits

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Abstract

Norway's extended free choice (EFC) reform extends the patient's choice of publicly funded hospitals for treatment to authorized private institutions (EFC providers). We study the effects of the reform on waiting times, number of visits, and patients' Charlson Comorbidity Index scores in public hospitals. We use a difference-in-differences model to compare changes over time for public hospitals with and without EFC providers in the catchment area. Focusing on five prevalent somatic services, we find that the EFC reform did not exert pressure on public hospitals to stimulate shorter waiting times and more visits. Moreover, we do not find that the sum of public and private visits increased. When we compare patient comorbidity between public hospitals and EFC providers, we find that for non-invasive diagnostic services, patient comorbidity is lower in EFC providers. For surgical services, we detect no difference in patient comorbidities between public and EFC providers.

KEYWORDS
difference-in-differences, health care reform, patient choice, public-private mix

JEL CLASSIFICATION
H42, H44, I11, I18

1 | INTRODUCTION

In Norway, an extended free choice (EFC) reform was implemented in November 2015 (The Parliament, 2015).[1] Earlier, private healthcare providers needed a contractual agreement with the Regional Health Authorities (RHAs) to be included in the patients' choice set. The reform allows all private for-profit healthcare providers that fulfil minimum quality requirements to compete for patients in the hospital sector. Given the license to operate, private providers are paid a fixed price per treatment. The rationale behind the reform was that competition from private providers would stimulate public hospitals to become better and more efficient, and to reduce waiting times for elective treatments.

From a theoretical perspective, the impact of choice on the economic incentives facing hospitals is not straightforward and depends on institutional details. Chalkley and Malcomson (1998) analyze the incentives to increase quality for the provider when demand responds to quality, and when there is excess demand (as in Norway). They demonstrated that the provider never has an incentive to supply higher quality, which would result in lower waiting times, regardless of how responsive demand is to quality. The intuition behind this is that with excess demand, efforts to increase quality, which would reduce waiting times, have no implications for the revenues of the hospital. Siciliani (2005) shows in a duopolistic model that exogenous increases in supply reduce waiting time while increased substitutability of treatment, for example, due to patients' choice, among the two hospitals provides an incentive to reduce supply, which increases waiting time. The reason for the result is that a hospital attracts patients if waiting times are reduced through an increase in supply. The higher demand generates an upward pressure on waiting times and eliminates the benefit from higher supply. The result holds either when the hospital is remunerated with a fixed budget or with activity-based funding based on diagnosis related groups (DRGs). However, Siciliani (2005) also suggests an alternative financial arrangement. Each patient could be initially assigned to an administrative area and to a hospital that is responsible for providing elective care in that area. Patients are free to choose the hospital they prefer. Hospitals would have to pay from their own budget if patients from their own area demand treatment to a hospital in a different area. Siciliani (2005) suggests that more choice may then reduce waiting times, although a more rigorous formulation is left for future research. This alternative financial arrangement is similar to the institutional set-up of EFC. Payments made to private providers are deducted from the block grant allocated to public hospitals based on the catchment areas of the patients treated by private providers. Hence, the public hospitals have an economic incentive to increase effort to reduce waiting times to avoid losing patients and revenue.

We hence study the reform's effects on public hospital waiting times (*the competition effect*), the number of visits in public hospitals and the total number of visits, that is, the sum of public and private visits (*access*), and patients' comorbidity index scores (*composition of patients*) using data from the first 3 years of the reform.

We focus on five services that are among the most prevalent in EFC providers: two minimally invasive procedures often used for diagnostic purposes (stress ECG and endoscopy) and three invasive medical surgeries with potential complications and risks (carpal tunnel syndrome, ganglion, and hallux valgus and hammertoe). We have a six-year panel of visits to publicly funded hospitals. We use a difference-in-differences (DID) model to compare changes over time for public hospitals with and without EFC providers in the catchment area. The timing of exposure varied across hospitals; therefore, we use the methods described by Callaway and Sant'Anna (2021), which are robust to differential exposure timing.

We do not find that the EFC reform exerted pressure on public hospitals to stimulate shorter waiting times and more visits. Except for one surgical procedure (ganglion) we do not find that the total number of visits is higher in catchment areas with EFC providers. Furthermore, the reform did not affect the severity of patients in public hospitals measured by patient comorbidity. When we compare patient comorbidity between public and EFC providers, we find that for non-invasive diagnostic services, patient comorbidity was lower in EFC providers. For surgical services, we detect no difference in patient comorbidities between public and EFC providers.

We complement the literature on the impacts of free choice reforms in several ways. First, this study broadens the empirical literature by providing evidence from Norway. Second, we extend the services we examined from what has already been studied in other countries, mostly surgical procedures, to multiple service types. Concurrently, we assess outcomes similar to those examined in previous studies conducted in different contexts, thereby facilitating cross-country comparisons. Third, a rich individual-level panel dataset facilitates the DID method, which provides reliable causal estimates. In addition, EFC providers are required to report to the Norwegian Patient Registry (NPR), ensuring that the information registered is complete and precise. Finally, we contribute to the broader waiting time literature by providing results on how competition from for-profit providers affect public hospitals' waiting times.

The Norwegian EFC reform can be seen as part of a trend in which price-regulated markets are opened to competition. Providers compete with regulated prices in terms of location and quality. Markets are often associated with welfare services, such as health, education, schooling, and social services. Proponents of EFC reforms argue that a diversity of providers that compete on the same terms would stimulate cost efficiency, contribute to quality improvements, and improve access and consumer choice (Ministry of Health and Care Services, 2014). Opponents raise concerns that EFC reforms could impair equity, increase socioeconomic and geographical inequity (Bardsley & Dixon, 2011), hamper coordination between private and public organizations (Lindén & Ervik, 2022), and that new entrants may not have sufficient scale to affect the behavior of public providers (Goddard, 2015). Proponents are often on the right side of the political spectrum, whereas opponents are on the left.

Theoretical models of mixed markets with regulated prices are often placed in a spatial competition framework. Herr (2011) compares a mixed duopoly with fixed symmetric locations to both public and private profit-maximizing duopolies. She finds that a mixed duopoly is optimal if the public firm is sufficiently efficient, and competition is intense. Levaggi and Montefiori (2013) analyze patient selection in a mixed (hospital) oligopoly market when public providers face a soft budget constraint. They find that private providers benefit from soft budget constraints by providing a quality mix that is preferred by less severe (and less costly) patients. Hehenkamp and Kaarbøe (2020) analyze how private providers' location choices affect quality competition. They show that, although opening a public hospital market typically raises quality, private providers strategically locate themselves toward the corner of the market to avoid costly quality competition.

In hospital markets, free choice reforms were introduced in Denmark in 2002 (Danish Health Care Act § 87, 2002), in England through a series of policy reforms in the 2000s (Cooper et al., 2018), and in Norway and Sweden in 2015 (The Parliament, 2015; The Swedish Parliament, 2014). In primary care, free choice was gradually implemented in Sweden during 2007–2010 (Swedish Ministry of Health and Social Affairs, 2008), and in Spain, the regional government of the Community of Madrid extended the patients' right to choose any health care provider within the Community of Madrid from November 2009 (BOCM, 2009).

Dietrichson et al. (2020) investigate the Swedish free choice reform within primary care. This reform began in 2007 when some Swedish regions allowed for the free establishment of private providers. In 2010, the reform became mandatory for all regions in Sweden (Swedish Ministry of Health and Social Affairs, 2008). The authors exploit the differential timing of the reform in a DID analysis and find small effects on primary care quality, although the reform led to a substantial increase in the entry of new providers. Fernándes-Pérez et al. (2022) analyze the effects of the free choice reform in Madrid. Their empirical analysis was conducted by using Spanish microdata for the period 2002–2016 and by using synthetic control estimation techniques. The reform had a strong and long-lasting impact, reducing average waiting times and increasing patients' satisfaction with the specialist attention received.

In England, first, the governmental reforms allowed for-profit hospitals (Independent Sector Providers) to enter the market for specific publicly funded elective procedures; second, all patients should be given a choice of at least five providers when referred from primary care; and third, private for-profit providers could compete for government-funded patients. From 2012/13, these private for-profit providers were funded by the national healthcare resource group's tariffs.

Studies find different effects on whether competition from private providers improves public hospital performance in England. Cooper et al. (2018) find that the entry of private surgical centers led to a shorter pre-surgery length of stay at nearby public hospitals. However, private surgical centers selected healthier patients and left public hospitals to treat patients who were sicker. Kelly and Stoye (2020), find that new entrants exerted little competitive pressure on public hospitals. Specifically, these authors find that private hospital entry increased the number of publicly funded hip replacements by 12% but did not reduce volumes at incumbent public hospitals and had no impact on readmission rates. Cooper et al. (2012) study the impact of competition on the efficiency of public hospitals conducting four types of elective surgeries between 2002 and 2010.[2] They find that the presence of a private hospital in a hospital's market was associated with a small increase in length of stay, driven by the sorting of more severely ill patients to public hospitals.[3] Beckert and Kelly (2021) analyze how, and through what channels, the benefits of private provider competition are distributed. Their findings indicate that the inclusion of for-profit providers impairs equity. Specifically, they find that regarding the value of access, the entry of for-profit providers benefited richer patients twice as much as poorer, and white patients six times as much as ethnic minority patients.

This paper also contributes to the literature on the effect of patient choice and provider competition on waiting time in public hospitals. Most of this literature stems from England where hospital waiting times are considerable and patient choice has been introduced to encourage hospitals to reduce waiting times to attract patients. Empirical results have been mixed. Propper et al. (2008) use data from England 1991–1999 to examine the impact of competition in an environment with limited quality information. They find that competition reduces waiting times. Using AMI mortality as a measure of hospital quality, they also find a positive relationship between competition and AMI mortality. The authors conclude that the results indicate that hospitals in competitive markets reduce unmeasured and unobserved quality to improve measured and observed waiting times. Siciliani and Martin (2007) test if patient choice induces more competition among hospitals and reduced waiting times using 120 English NHS hospitals over the period 1999–2001. They find that more choice is significantly associated with lower waiting times at the sample mean (five hospitals) although the quantitative effect is modest: an extra hospital in a catchment area will only reduce waiting by at most a few days (or 1%–2% reduction in waiting). Moscelli et al. (2021) examine the effect of the 2006 patient choice reform in England on the quality of three high-volume non-emergency treatments. The reform relaxed restrictions on patient choice of hospital. They find that public hospitals facing more rivals increased waiting times for hip and knee replacements and find no effects for coronary artery bypass grafts. Moscelli et al. (2023) follow up by examining whether the 2006 reform affected waiting times inequalities for three common elective treatments (coronary bypass, hip replacement and knee replacement) in hospitals which faced more potential competition (number of rivals) before the choice reform was introduced relative to those which faced less competition. Their results suggest that the choice reform led to longer and more dispersed waiting times in areas that are more competitive, while deprivation related inequity was smaller in the post-choice period. In Denmark, the free choice reform aimed to avoid long waiting times (Danish Health Care Act § 87, 2002). The reform gave patients an extended free choice if the regional authority could not provide treatment at a public hospital within a certain time limit (e.g., 1 month if the patient was considered seriously ill). Simonsen et al. (2020) demonstrate that patients who use their right to extended free choice can obtain significantly lower waiting times, but patients must themselves initiate the extended choice. This contributes to a socioeconomic gradient in the waiting time.

Holmås and Kaarbøe (2021) investigate which factors influencing the likelihood of a patient in need of somatic patient care opting for an EFC provider. Using data from 2017 to 2018 they find that women, people under 40, and people with higher education are more likely to use an EFC provider. They also find that better accessibility, measured by distance in kilometers to the nearest EFC provider, increases the probability that the provider is chosen. Another important factor is the general practitioner's referral practice. Their analysis shows that somatic patients, who belong to lists where a relatively large proportion of patients received treatment in private hospitals in 2017, had a significantly higher probability of using the EFC scheme in 2018. We complement this analysis by providing results on how the EFC reform affected waiting time, total access and the composition of patients between public hospitals and EFC providers.

The remainder of this paper is organized as follows: in Section 2, we outline the institutional background; Section 3 presents the hypotheses; Section 4 describes the data; Section 5 outlines the empirical framework; the results are presented in Section 6; followed by a robustness analysis in Section 7; and finally, Section 8 concludes the paper.

2 | INSTITUTIONAL BACKGROUND

In Norway, most healthcare services are publicly funded and organized into primary and specialist healthcare. To access specialist care, patients need referral from a general practitioner (GP). That is, a GP must examine the patient and write a letter of referral if specialist care is required. The referral letter is assessed by a specialist typically employed by a public hospital. If the conclusion of the referral letter is confirmed, the patient is granted the right to specialist treatment within a specified period (waiting time guarantee).

Specialist care in the healthcare system is administered through four Regional Health Authorities (RHAs) and is primarily funded by public sources. These publicly financed services are offered by the following entities: (i) the RHA's hospitals with an assigned catchment area, herein referred to as "public hospitals"; ii) private hospitals that have tender contracts with an RHA; iii) specialists contracted by the RHA; and iv) EFC providers.

The financing of specialist care involves a combination of block grants and activity-based funding, which is determined by diagnosis-related groups (DRGs). Notably, in the years 2014–2022, activity-based funding has accounted for 50% of the total hospital budget allocated for somatic care.

The budgetary allocation for healthcare services originates from the central government and is disbursed to the RHAs. The RHAs bear the responsibility of financing all publicly funded hospital services provided to residents within their respective regions, regardless of the institutional source of such services. Activity-based funding is channeled by the RHAs to public hospitals based on their level of activity, while block grants are allocated to these hospitals in accordance with certain characteristics of the inhabitants within their catchment areas, such as age and socioeconomic status. RHA-contracted specialists receive a fixed practice allowance in addition to fee-for-service compensation.[4]

For private hospitals with tender contracts with an RHA, their funding is determined through a competitive tendering process that encompasses both framework agreements and performance agreements (NOU, 2020, p. 13). The duration of the tendering contracts is typically 2 years, with a unilateral option for the RHA to extend the contracts for an additional 1 + 1 year, thus allowing for a maximum contract duration of 4 years. Additionally, the RHAs can make supplementary acquisitions, for example, specifying the treatment of referrals or the requirement to provide evening services to adhere to waiting time targets, accommodate urgent cases, and the competence and experience of key personnel. The agreements may also include clauses governing the delivery of the agreed-upon service volume, such as uniform distribution over a specified period or suspension of the agreement upon fulfillment of the agreed-upon volume until a new performance agreement is reached. Both quality and price are essential parameters in the tender competitions (The Norwegian Competition Authority, 2015).

As of 2012, information shared during the tendering process has been classified as confidential, limiting public access to details regarding prices, quantities, and specific contractual terms.[5] Since the introduction of the EFC reform, selected private hospitals with tender contracts with an RHA may be given permission to consider GP referrals and grant patients the right to specialist care (Ringard et al., 2016). Activity data and budgeted financial limits for procurement agreements per RHF for the period 2015–2019 indicate that the publicly funded admissions at private hospitals with tender contracts have been relatively stable from 2015 to 2019 (Norwegian Directorate of Health, 2017, 2019).

The EFC reform was instituted in November 2015. Subsequently, during the fiscal years 2016 and 2017, the Norwegian Government allocated a budget of NOK 550 million (equivalent to €48.5 million). Most of these funds were earmarked for the enhancement of mental health services and substance abuse services. From 2017, no earmarked budget was granted.

The EFC reform allows all private for-profit providers that fulfil minimum quality requirements to compete for patients in the hospital sector. These providers are paid a fixed price per treatment but are not given permission to consider GP referrals and grant patients the right to specialist care.

In the treatment of the EFC reform, amending the Patient and User Rights Act and the Specialist Health Service, the Norwegian Parliament (The Parliament, 2015) agreed that the following principles should guide the pricing in the EFC treatment scheme:

- The prices within the EFC treatment scheme should not result in an increase in state expenditures when compared to the scenario of enhanced utilization of regular tendering processes.
- Prices should be set in a way that upholds the legitimacy of the EFC reform. This entails, among other things, that private providers should not achieve unreasonably high profits or offer unreasonably high salaries.

Because of these principles, prices are believed to be set lower than the lowest relevant bid price obtained through tendering. It is the responsibility of the Norwegian Directorate of Health to determine annually the prices in the EFC treatment scheme within the framework set by the Norwegian Parliament.

For services where tendering is not carried out or bid prices are not known, the prices are based on cost calculations conducted by the Norwegian Directorate of Health. This approach establishes a connection between the prices offered to EFC providers and the expenses associated with delivering equivalent services in public hospitals, as these costs are obtainable through activity-based financing. However, due to variations in costs (cost heterogeneity), a comparison of prices should be made at the procedural level, rather than at the DRG level. Additionally, to mitigate potential patient selection bias, prices for EFC providers should be set lower than the procedural costs incurred by public hospitals (NOU 2020:13).

As an illustration, in 2022, EFC providers claim NOK 3715 (€328) for colonoscopies, whereas the reimbursement to public hospitals, as indicated in the activity-based financing price list, amounts to NOK 3342 (€295). Given that the price provided to public hospitals is estimated to cover approximately 50% of the hospital's cost for rendering these services, the reimbursement granted to an EFC provider is considerably lower than the estimated cost incurred by public hospitals.

The remuneration for EFC services is determined by the government and covered through the budget to the RHAs. This arrangement is directly linked to the objective of ensuring that patients are entitled to have their treatment needs evaluated and, if granted the right to specialist treatment, to receive healthcare services from the public specialist health service. Consequently, the RHAs are responsible for guaranteeing the provision of specialized health services to the eligible population.

Significantly, the payments made to private providers are deducted from the block grant allocated to public hospitals based on the catchment areas of the patients treated by private providers. This mechanism effectively results in a reduction of funding for public hospitals, as privately treated patients indirectly impinge upon their financial resources.

3 | HYPOTHESES

The underlying rationale behind the EFC reform was to foster competition among healthcare providers, with the expectation that the participation of private entities would incentivize public hospitals to improve their services and operational efficiency, ultimately leading to reduced waiting times for elective treatments. This leads to the following hypothesis:

H1 (*competition*): Public hospitals, when exposed to EFC providers, will demonstrate a reduction in waiting times and an increase in patient volume. This strategic response is expected to counter the allure of private providers, which may otherwise draw patients away from public hospitals and undermine their budgetary stability.

A greater private sector presence should increase total access to care. Our second hypothesis is thus:

H2 (*access*): In catchment areas where public hospitals are exposed to EFC providers, the total treatment volume, encompassing both public and private treatments, is significantly higher compared to areas without EFC exposure.

Increased access raises the question of who receives these additional services. As capacity expands, we expect GPs to refer patients with lower levels of need. This leads to the final hypothesis:

H3 (*composition of patients*): Exposed catchment areas will demonstrate a higher influx of patients seeking treatment, predominantly comprising individuals with milder severity, resulting in lower patient comorbidity compared to catchment areas without exposure to EFC providers.

4 | DATA

4.1 | Data sources and sample selection

Our main source of data was NPR, which covers all publicly funded somatic hospital visits from 2013 to 2018.[6] We used a subsample of these visits for our analysis. Detailed attrition tables for referrals to public hospitals and EFC providers are given in Tables A1 and A2 in Appendix A. Referral to a hospital may prompt many visits. For each referral, we kept the first visit that involved a medical procedure. The EFC reform covers a list of procedures, most of which are used to treat specific diagnoses. Moreover, similar procedures used to treat similar diagnoses are grouped together as "services." Table B1 in Appendix B shows all 14 services performed by EFC providers during our observation period. Two services, stress ECG and endoscopy, are diagnostic procedures with no specific diagnoses. The remaining services are for specific diagnoses, and each consists of one or several surgical procedures.

We focused on five services, as shown in Table 1: stress ECG, endoscopy, carpal tunnel syndrome, ganglion, and hallux valgus and hammertoe.[7] A full list of the procedures for each service is given in Table C1 in Appendix C. These five services were selected for several reasons. First, they are among the most prevalent procedures performed in EFC. Second, the number of visits to exposed public hospitals is sufficiently large. Third, the EFC share is sufficiently large to expect a detectable impact on exposed public hospitals. Fourth, the services were provided in many exposed public

TABLE 1 Descriptives of the five services included in the analyses.

Service	1	3	5	8	9
When EFC started	2016Q1	2016Q2	2016Q2	2017Q2	2017Q1
EFC median waiting time (days)	23	38	34	70	17
Exposed public median waiting time (days)	56	102	190	186	42
EFC #	2507	278	105	108	3426
Exposed public #	10589	2519	805	762	31223
EFC share (%)	19	10	12	12	10
# exposed public hospitals	18	23	23	19	17

Note: 1. Stress ECG; 3. Carpal tunnel syndrome; 5. Ganglion; 8. Hallux valgus and hammertoe; 9. Endoscopy (gastroscopy, colonoscopy, sigmoidoscopy, rectoscopy).

hospitals, ensuring enough clusters for robust inference. Finally, EFC providers that provided these services were approved before 2018, which ensured at least 1 year of follow-up. These five services can be categorized into two types: "diagnostic services" (stress ECG and endoscopy), that is, minimally invasive and often used for diagnostic purposes; and "surgical services" (carpal tunnel syndrome, ganglion, and hallux valgus and hammertoe), that is, invasive surgeries with potential complications and risks.

We conducted the analyses at the hospital level and described the hospitals in terms of the patients they served. We obtained patient information on age, sex, educational attainment, immigration status, and marital status from the Statistics Norway database. We also computed the patients' Charlson Comorbidity Index scores (henceforth Charlson scores). The Charlson score is based on the patients' diagnostic information from the NPR records in the 6 months prior to the first referral of interest.

4.2 | Descriptives of the reform

We have data until the end of 2018, 3 years after the reform. At this point, 26 EFC providers were approved: 22 in southeast Norway and four in southwest Norway, surrounding the large cities of Oslo and Bergen, respectively. During the sample period, EFC providers were gradually approved. While they had a slow start taking in publicly funded patients, the reform saw an increase in the number of patients opting for EFC during our sample period, especially in later years (Norwegian Directorate of Health, 2018). Table D1 in Appendix D summarizes the number of approved EFC providers and patient visits by EFC providers in each quarter from 2015Q4 to 2018Q4.

Figures 1 and 2 show the mean waiting time and total number of publicly funded hospital visits stratified by type of provider. We show the mean Charlson score by type of provider in Figure 1e in Appendix E. The exposed hospitals are public hospitals within whose catchment area at least one EFC provider provides the service of interest. Unexposed hospitals are public hospitals within whose catchment area no EFC provider has ever provided any service.

Figure 1 shows that waiting times in public hospitals are shorter for diagnostic services than for surgical services. Comparing the different types of providers within a service, we see that the waiting times follow similar trends between exposed and unexposed public hospitals and are much longer in public hospitals than EFC providers. Because the number of EFC visits is minor compared to public visits, the waiting times of exposed public hospitals are similar to the average waiting time in the affected area (i.e., the average waiting time for public exposed plus EFC).

Figure 2 shows the number of publicly funded visits. This is different from the analysis, which is based on the number of visits per hospital. More visits were observed for diagnostic services than for surgical services. Specifically, endoscopy has the highest prevalence of approximately 50,000 visits per year in public hospitals as opposed to ganglion surgery, whose number is only around 1400. For diagnostic services, the volume is lower in exposed hospitals than in unexposed hospitals. However, for surgical services, the exposed group was larger than the unexposed group. This is mainly because approved EFC providers that provide diagnostic services are located closer to each other than those providing surgical services; thus, fewer public hospitals are affected by the reform for diagnostic services than for surgical services. EFC visits increase the total number of publicly funded visits in exposed areas; however, the magnitudes are small for all services except stress ECG.

Table 2 shows patient characteristics across providers by service. On average, patients who received the diagnostic procedures had a slightly different profile than those who received surgery. The diagnostic procedure group of patients are older, more likely to be male, and have higher comorbidity. Comparing patients who went to public and EFC providers within services, those who went to EFC providers for diagnostic services are younger, more likely to be a single immigrant with a higher educational background and have lower comorbidity as opposed to public hospital patients. For surgical services, EFC and public hospital patients are similar in many aspects, but EFC patients are younger and more likely to be immigrants.

5 | EMPIRICAL FRAMEWORK

To estimate the impact of the EFC reform, we compare trends in outcomes between exposed and unexposed hospitals. This difference-in-differences (DID) approach removes bias from permanent differences between groups and from common time trends unrelated to the reform (Angrist & Pischke, 2009). The main identifying assumption is that trends in outcomes would run parallel between the groups in the absence of the reform.

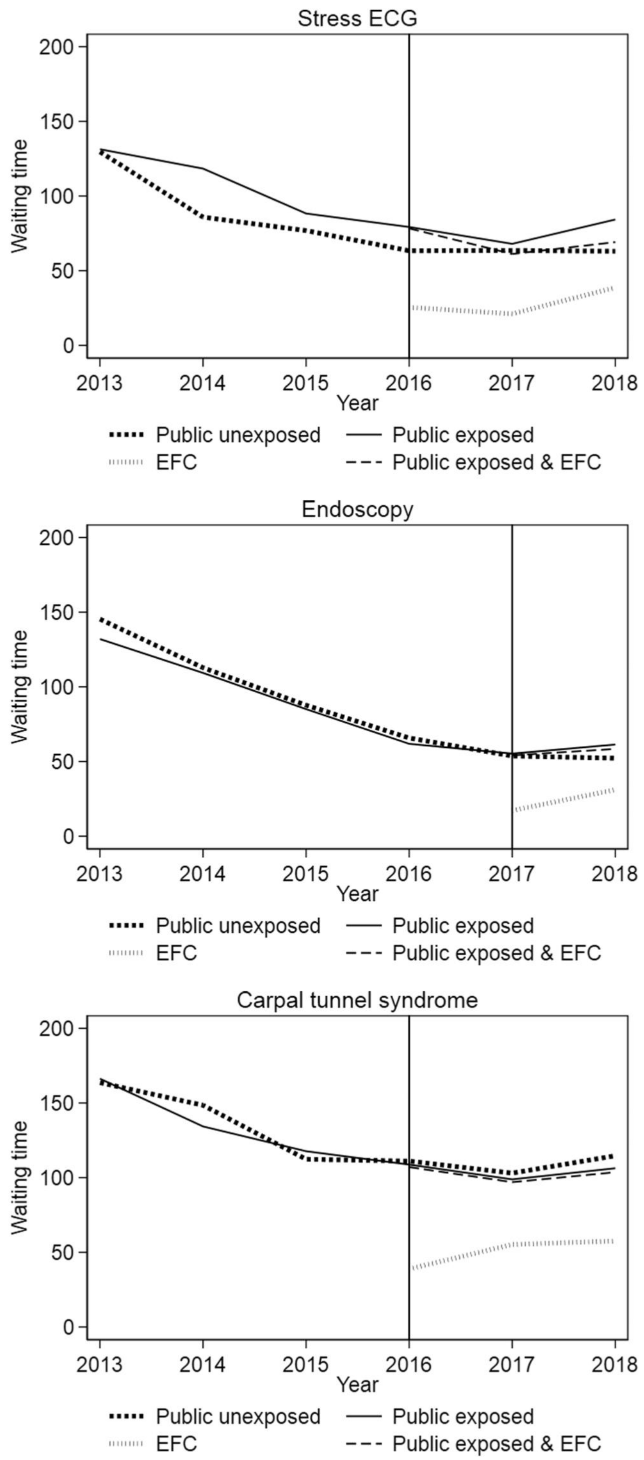

FIGURE 1 Mean waiting time of publicly funded visits by EFC exposure. Waiting time was measured in days. Exposed hospitals are public hospitals located in health trusts within whose catchment area at least one EFC provider delivers the service of interest. Unexposed hospitals are public hospitals in health trusts within whose catchment area no EFC provider has delivered any service. The vertical line indicates when EFC providers began delivering the given service to publicly funded patients.

Given panel data, a common way to estimate DID is via two-way fixed effects (TWFE):

$$y_{it} = \mu_i + \lambda_t + \gamma D_{it} + \beta \boldsymbol{x}_{it} + \varepsilon_{it}. \tag{1}$$

Here, y_{it} is the outcome in hospital i at time t, μ_i are hospital fixed effects, λ_t are time fixed effects and $\boldsymbol{x}_{it}$ is a vector of patient characteristics (see Table 2). D_{it} is a binary variable equal to 1 if hospital i is treated at time t and 0 otherwise. γ is

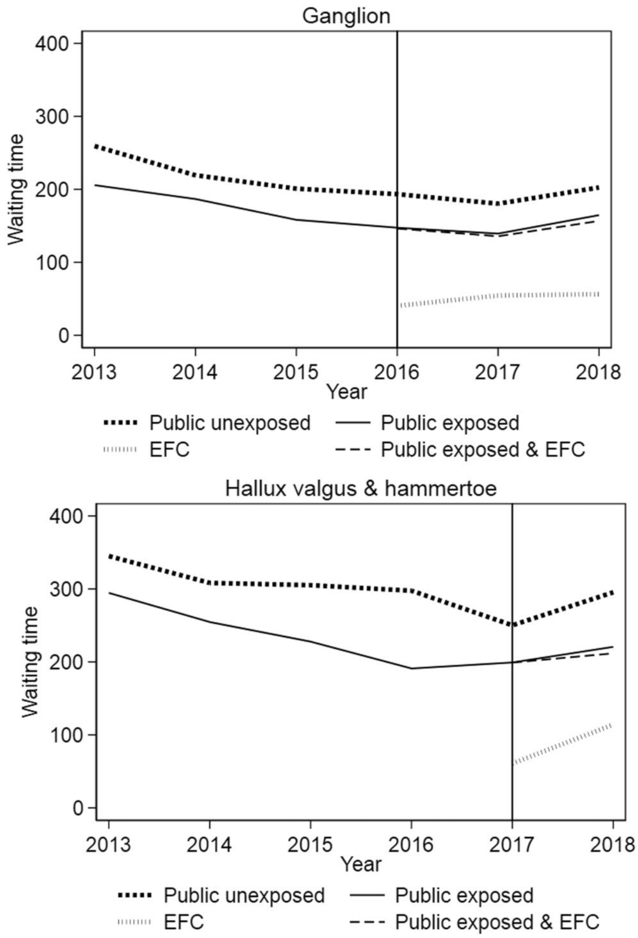

FIGURE 1 (Continued)

the parameter of interest, it seeks to capture the impact of the EFC reform on exposed hospitals aka the average treatment effect on the treated (ATT). Recently, Goodman-Bacon (2021) showed that γ is a weighted combination of all possible group-time ATT's (henceforth ATT(g, t)). These are ATT's for a particular group—where group is defined by treatment timing—in a particular period. The problem is that some ATT(g, t)'s use as controls hospitals that are already treated. This implies that for γ to capture the ATT one must assume—in addition to parallel trends—that the impact of the EFC reform is the same irrespective of the treatment timing. To avoid this problem, we use the DID approach proposed by Callaway and Sant'Anna (2021), henceforth CSDID. CSDID solves the problem by only estimating ATT(g, t)'s that use as controls never-treated or not-yet-treated hospitals. Equation (2) shows how we implement CSDID to recover ATT(g, t)'s.[8]

$$\text{ATT}(g,t) = E\left[\frac{G_g}{E\left[G_g\right]}\left(y_t - y_{g-1} - E\left[y_t - y_{g-1}\middle|\boldsymbol{x}, D_t = 0, G_g = 0\right]\right)\right]. \tag{2}$$

In words, the ATT of participating in the treatment for hospitals in timing group g is identified by adjusting the outcome path actually experienced by that group, by the conditional expectation of the outcome path of the comparison groups. Under the conditional parallel trends assumption, this latter path is the path of outcomes that hospitals in group g would have experienced if they had not participated in treatment (Callaway & Sant'Anna, 2021).

To aid interpretation, we present overall aggregates of all ATT(g, t)'s, as well as partial aggregates of ATT(g, t)'s by time from the reform. The latter is useful because post-reform effect estimates allow us to study how the exposure effect

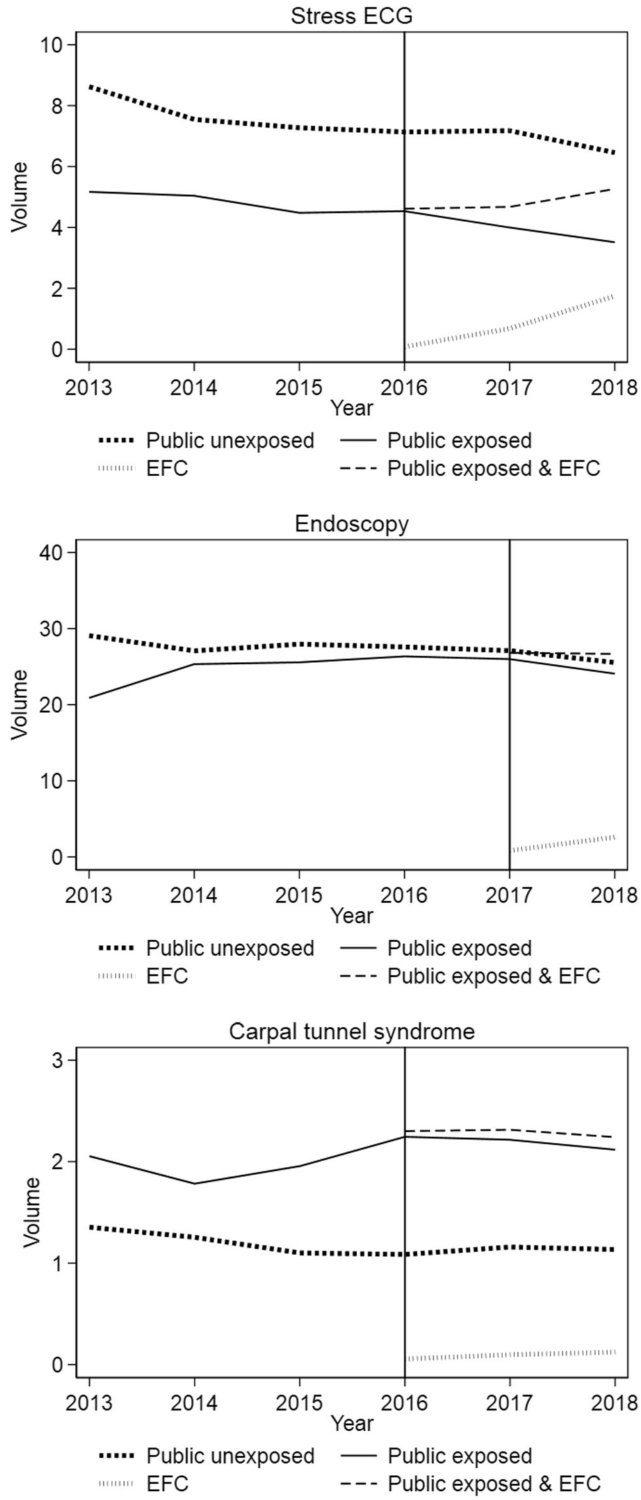

FIGURE 2 Number (in thousands) of publicly funded visits by EFC exposure. Exposed hospitals are public hospitals located in health trusts within whose catchment area at least one EFC provider delivers the service of interest. Unexposed hospitals are public hospitals in health trusts within whose catchment area no EFC provider has delivered any service. The vertical line indicates when EFC providers began delivering the given service to publicly funded patients.

changes over time, while pre-reform effect estimates allow us to visually inspect and formally test whether conditional pre-trends in outcomes are parallel. Reassuringly, Figures 3–5 show that there are virtually no differential trends across differentially exposed hospitals in the pre-period for all outcomes that we study.

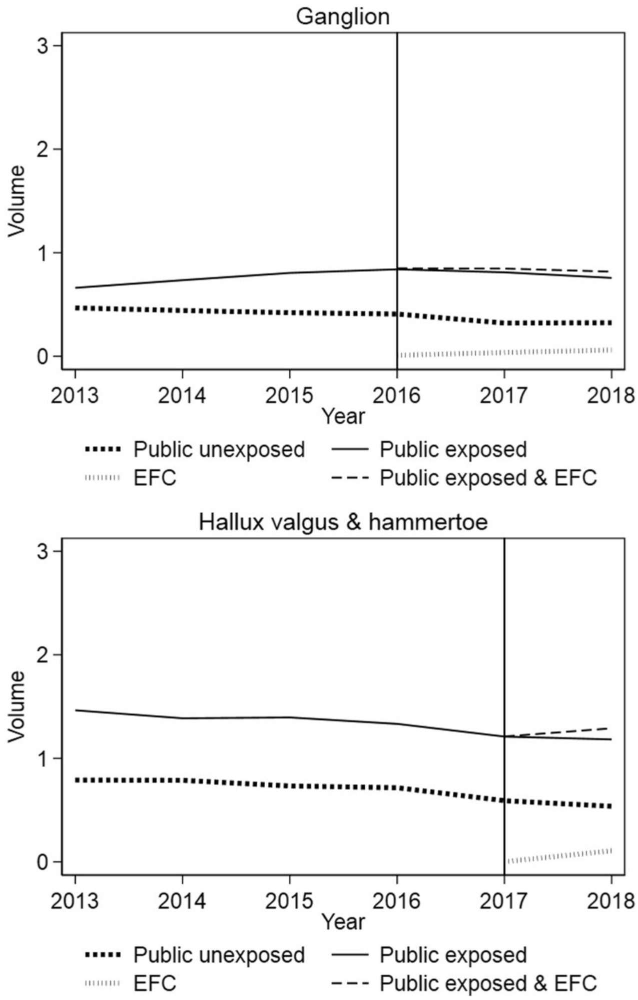

FIGURE 2 (Continued)

Our primary objective is to estimate interpretable, causal parameters. Therefore, we use the CSDID method as the main estimation method. However, CSDID has some drawbacks: unlike the TWFE estimator, CSDID cannot be estimated at the patient level, which would allow us to better control for patient severity. Moreover, CSDID does not produce interpretable coefficients on covariates. For these reasons, and as a robustness check, we return to Equation (1) in Section 7 Robustness analysis.

6 | RESULTS

In this section, we present our results showing how the entry of EFC affects waiting time, number of visits, and patients' comorbidity index scores in exposed public hospitals. We also showed how EFC usage correlates with patients' comorbidity index scores. This exercise aims to shed light on whether patients who use EFC are marginal.

6.1 | Impacts of EFC on waiting time and number of visits

Table 3 summarizes the impact of EFC on log-transformed waiting time and the number of visits for each of the five services considered. The estimates given in the table suggest that EFC did not have a statistically significant impact on waiting time or the number of visits. The number in parenthesis in the second column shows the impact on waiting time in days.[9] The estimates in Table 3 are simple averages of the post-event study estimates. Figures 3 and 4 plot the

TABLE 2 Patient characteristics across providers by service.

	Stress ECG				Endoscopy			
	Public		EFC		Public		EFC	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Age group (years) (%)								
0–19	1	11	1	10	3	16	1	10
20–59	44	50	47	50	54	50	69	46
60–74	41	49	39	49	29	46	25	43
75+	14	35	13	34	14	34	5	22
Female (%)	44	50	43	50	57	49	58	49
Immigrant (%)	10	30	19	39	11	32	17	37
Married (%)	58	49	55	50	49	50	46	50
Education (%)								
No high school	28	45	22	41	29	45	22	42
High school	47	50	38	49	43	49	40	49
University	25	43	40	49	28	45	38	49
Charlson score	0.17	0.60	0.14	0.54	0.14	0.68	0.04	0.28
N	65910		2316		262903		2762	
	Carpal tunnel syndrome				**Ganglion**			
	Public		**EFC**		**Public**		**EFC**	
	Mean	**SD**	**Mean**	**SD**	**Mean**	**SD**	**Mean**	**SD**
Age group (years) (%)								
0–19	0	4	0	0	11	31	13	34
20–59	53	50	56	50	68	46	73	44
60–74	29	45	29	45	17	38	12	33
75+	18	38	15	35	4	19	2	14
Female (%)	65	48	64	48	66	47	67	47
Immigrant (%)	12	32	15	36	13	34	16	37
Married (%)	55	50	57	50	42	49	34	47
Education (%)								
No high school	30	46	28	45	33	47	34	47
High school	48	50	48	50	40	49	33	47
University	22	41	24	43	27	45	33	47
Charlson score	0.07	0.42	0.10	0.43	0.02	0.23	0.00	0.00
N	18085		261		6740		101	

	Hallux valgus and hammertoe			
	Public		EFC	
	Mean	SD	Mean	SD
Age group (years) (%)				
0–19	2	14	4	19
20–59	57	49	59	49

TABLE 2 (Continued)

	Hallux valgus and hammertoe			
	Public		EFC	
	Mean	SD	Mean	SD
60–74	33	47	29	46
75+	8	27	8	27
Female (%)	87	34	86	34
Immigrant (%)	10	30	12	32
Married (%)	52	50	50	50
Education (%)				
No high school	22	42	19	40
High school	44	50	45	50
University	34	47	36	48
Charlson score	0.03	0.28	0.04	0.24
N	11567		103	

pre- and post-event estimates. These figures make two contributions: first, they show whether exposed public hospitals exhibit different pre-trends in outcomes; and second, they reveal how EFC affects outcomes over time. The point estimates of the parameter values are small (at most 0.14% waiting time reduction), and the confidence intervals are wide relative to the point estimates. Also, the dynamic paths displayed in Figure 3 point in the same direction. Table 3 also implies that the point estimates of the relative effects on the number of services in exposed public hospitals are of the magnitudes of 0.02–0.4%.

While the reform was designed to bring about change in the public hospitals, it is important to show how it affected total access for patients, and therefore how total volume is affected. The challenge with analyzing this outcome is that a hospital level analysis lacks observations of EFC providers pre-reform and using larger geographical areas as the unit of analysis results in too few clusters for accurate inference. Our solution is to attribute EFC observations to a treated hospital in the same area. Results from this analysis, shown in Table 4, do not suggest an increase in the total volume.

Figure 3 plots event study estimates for log-transformed waiting time. For all services, the pretrends are similar across exposed and unexposed hospitals. The figure does not suggest that waiting time has changed differentially in exposed hospitals following the entry of EFC. Figure 4 shows event study estimates for the number of visits. All services have parallel pre-trends, and in the first post-treatment quarters no treatment effects are evident. Longer-term effects should be interpreted with caution, as only the earliest exposed hospitals contribute to the final post-quarters. This is indicated by the wide confidence intervals. Wide confidence intervals suggest variation in the change in the number of visits between the earliest exposed hospitals. Such variation is not indicative of a clear treatment effect. The wide confidence intervals also remind us that many EFC providers did not receive authorization until near the end of our sample period. However, if the proposed mechanisms of the reform were accurate, one would expect effects to arise in a relatively short period of time.

These results show that the entry of EFC providers has little impact on waiting times and volume in public hospitals. Meanwhile, we observed an increased number of publicly funded visits to EFC providers. This suggests that the EFC reform facilitated expansion in publicly funded supply, but it did not exert competition pressure on public hospitals to stimulate efficiency in terms of shorter waiting times and more visits. We conclude that we arrive at a null result. Even if we would be able to reject the null hypotheses, the small point estimates rule out economically significant effects.

6.2 | Impacts on patient severity

Next, we investigated whether the composition of patients in exposed public hospitals changed owing to EFC and compared comorbidities between patients in exposed public hospitals and EFC providers. This is particularly interesting

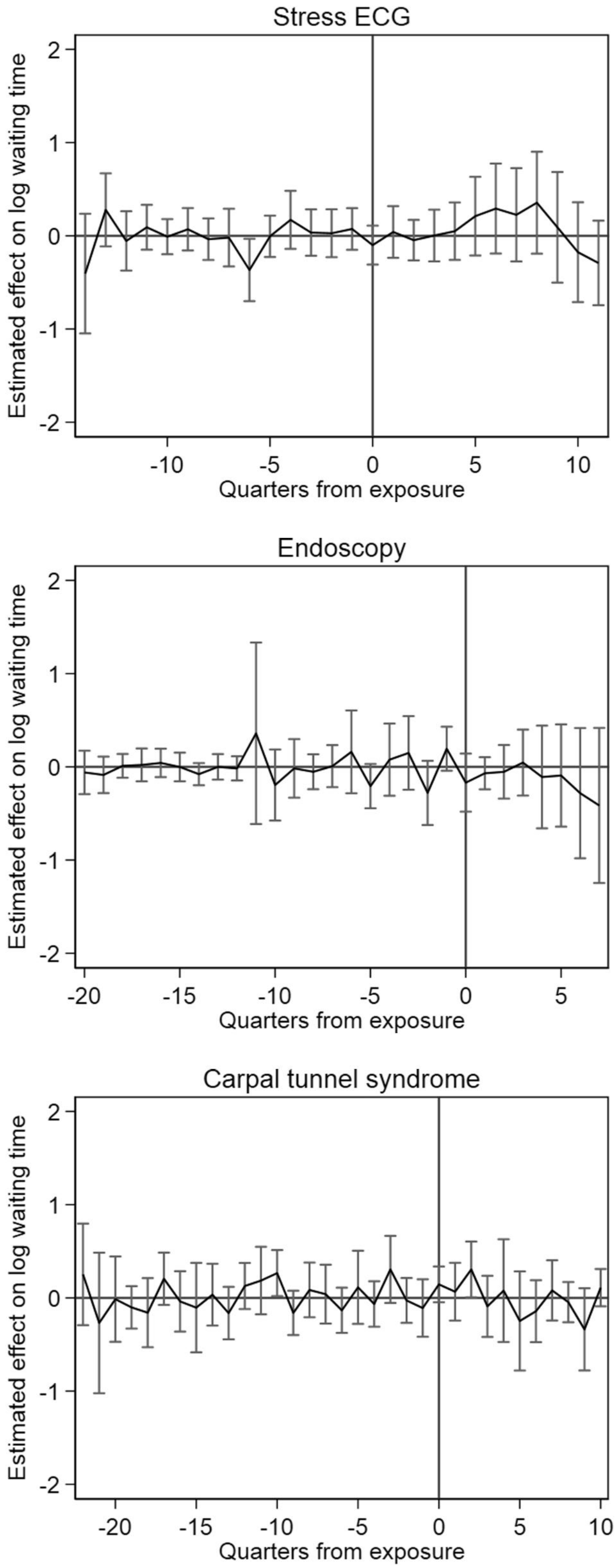

FIGURE 3 Dynamic effects of EFC on log waiting time in exposed public hospitals. Each panel shows estimates of the effects of EFC on log waiting time by quarter from exposure using the difference-in-differences method described by Callaway and Sant'Anna (2021). The capped spikes plot 95% confidence intervals constructed from standard errors clustered at the hospital level. The lengths of the pre- and post-periods depend on when the first EFC provider delivered the given service.

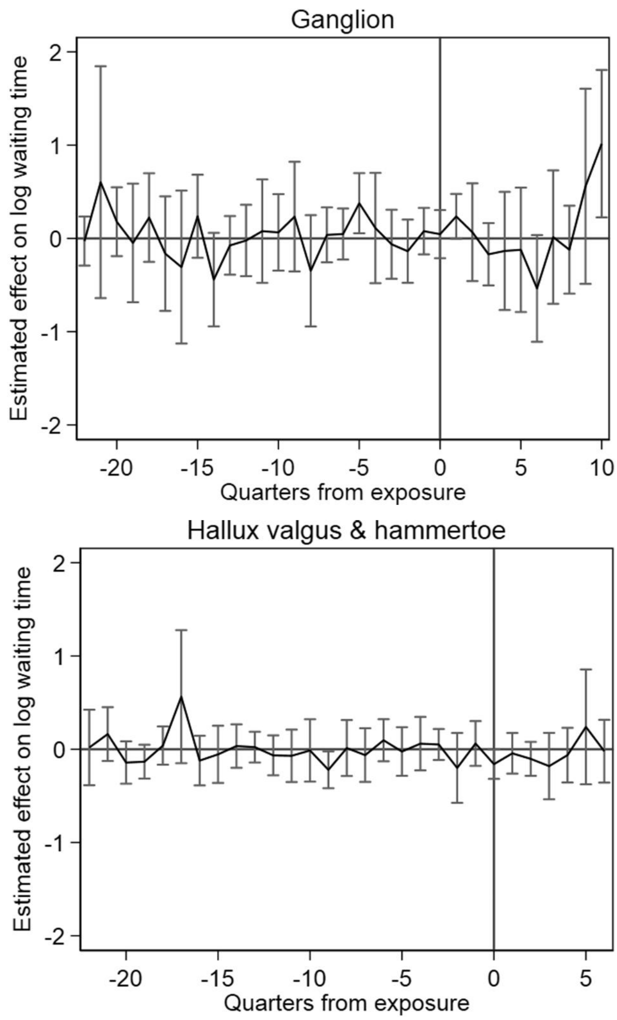

FIGURE 3 (Continued)

because the above results indicate that the entry of EFC providers did not reduce volumes at public hospitals, which raises the question of who receives these additional services. As capacity expands, one might expect GPs to refer patients with lower levels of need. Therefore, increases in supply could lead to marginal or even average patients being less severe. The Charlson score was used to measure patient severity. We computed the Charlson score using the patients' up to six-month history of diagnoses prior to the hospital visit of interest.[10]

First, we investigated whether the EFC affects the distribution of patient comorbidity in public hospitals. We used the same methodological approach as described above, with the Charlson score as the outcome variable. Figure 5 shows the dynamic effects of the EFC on patients' average Charlson scores. For all services, the reform had no impact on the distribution of patient comorbidity in public hospitals. The estimates are presented in Table 5.

Table 6 shows the comorbidities comparison between patients in exposed public hospitals and EFC providers. The results differed according to the type of procedure used. For diagnostic services, the comorbidity score is lower for patients in EFC providers than for those in public hospitals. Combined with the results above, this suggests (as patient composition in public hospitals was not affected by the reform) that less severely ill patients, who might not have been treated, had there been no reform, are now referred to EFC providers. This might be because stress ECG and most cases of endoscopy (for diagnostic purposes) are noninvasive and pose minimal adverse effects on patient health; thus, GPs and patients are more willing to undergo such procedures. However, for invasive surgical services, no significant difference (although positive point estimates) in comorbidity was detected between patients in public and EFC providers.

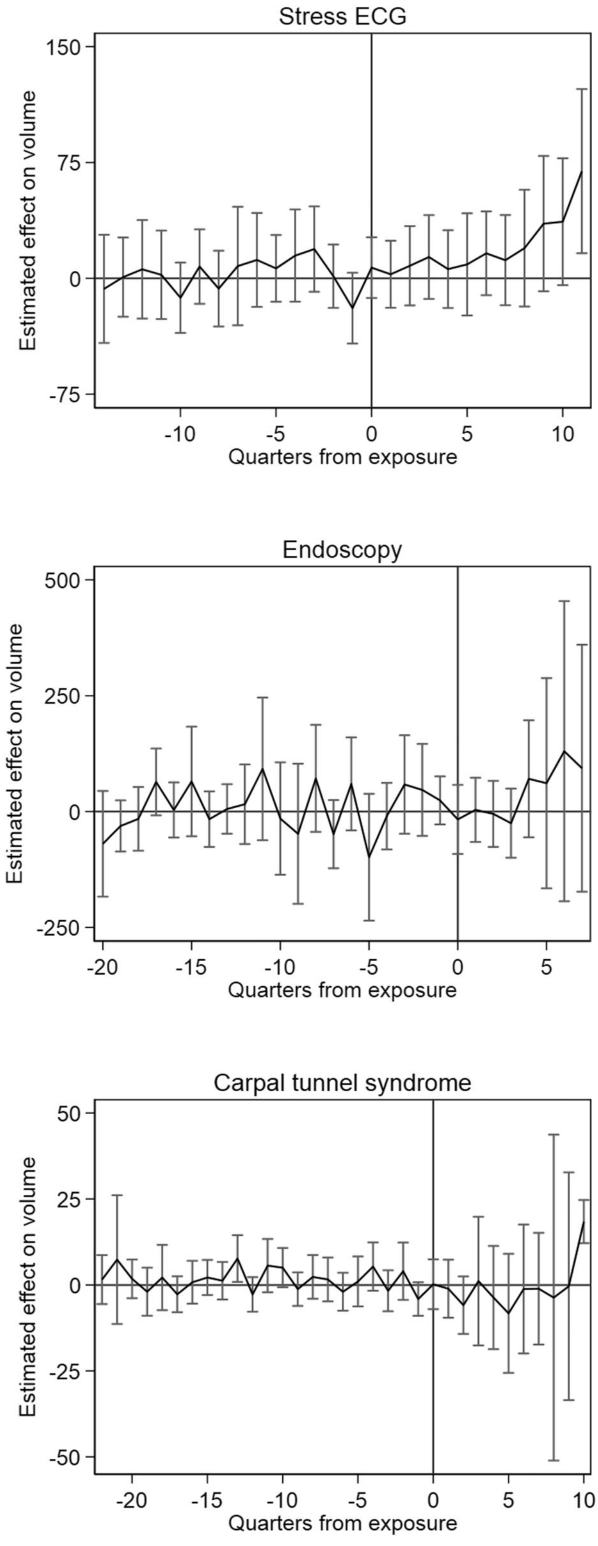

FIGURE 4 Dynamic effects of EFC on the number of visits in exposed public hospitals. Each panel shows estimates of the effects of EFC on the number of visits by quarter from exposure using the difference-in-differences method described by Callaway and Sant'Anna (2021). The capped spikes plot 95% confidence intervals constructed from standard errors clustered at the hospital level. The lengths of the pre- and post-periods depend on when the first EFC provider delivered the given service.

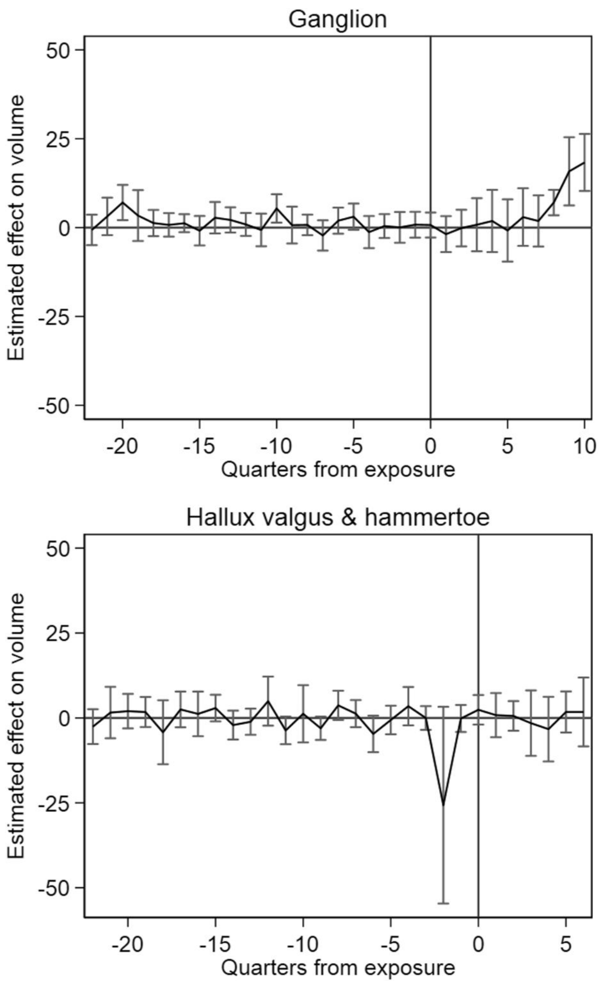

FIGURE 4 (Continued)

7 | ROBUSTNESS ANALYSES

In this section, we describe the results of our robustness checks. Overall, the robustness checks support the results from the main analysis that EFC does not affect the waiting time or volume of services examined. The finding that patients who receive stress ECG or endoscopy in EFC providers have lower comorbidity also appears robust. Robustness checks can be grouped into three categories: sample selection, outcome specifications, and regression model specifications. In turn, we discuss each category. Most of the results from the robustness checks are given in Appendix F.

We demonstrate the sample selection in Appendix A. In the last step, we keep referrals with a waiting time between 2 and 1800 days. In a robustness check, we demonstrate that keeping referrals with waiting times between 2 and 730 days or 2 and 365 days has little impact on the results. Moreover, we defined waiting time as the difference between the date of referral and the date of procedure. The results do not change if waiting time is defined as the difference between the date of referral and the date of first contact with the hospital. We also show that the results are robust to excluding hospitals in Northern Norway from the comparison group (due to large geographical distance).

Next, we consider the robustness to the specification of the outcomes. In the main analysis, we used the log of waiting time. This is because the DID analysis assumes linearity in the effect of the EFC, and taking logs of waiting time is one way to improve the linearity between waiting time and the EFC. In addition, the coefficient of EFC in the log-linear model is simply the percentage change in waiting time. The results barely change if we do not take the log of the waiting time. In the main analysis, we included the Charlson score as a continuous variable. However, the results are very similar when we use a binary variable that indicates whether a patient has a positive Charlson score. We did not change the specification for the number of visits outcome variable.

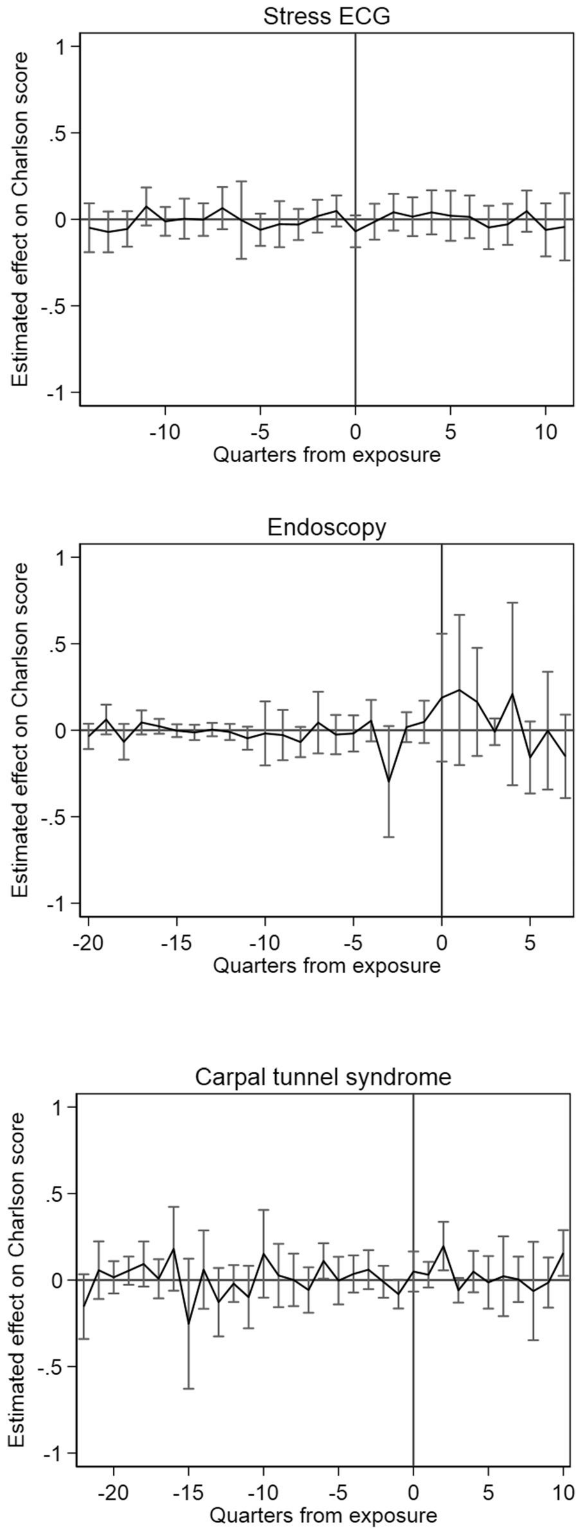

FIGURE 5 Dynamic effects of EFC on patient Charlson scores in exposed public hospitals. Each panel shows estimates of the effects of EFC on patient Charlson scores by quarter from exposure using the difference-in-differences method described by Callaway and Sant'Anna (2021). The capped spikes plot 95% confidence intervals constructed from standard errors clustered at the hospital level. The lengths of the pre- and post-periods depend on when the first EFC provider delivered the given service.

Finally, we consider the robustness of the specification of the regression model. In the main analysis, we used both never-treated and not-yet-treated as controls. Using only those that were never treated as a control did not affect the results. This could be due to the small variation in exposure timing. We also considered introducing a grace period, giving the exposed hospitals the time to respond to the advent of EFC providers. However, the results do not change

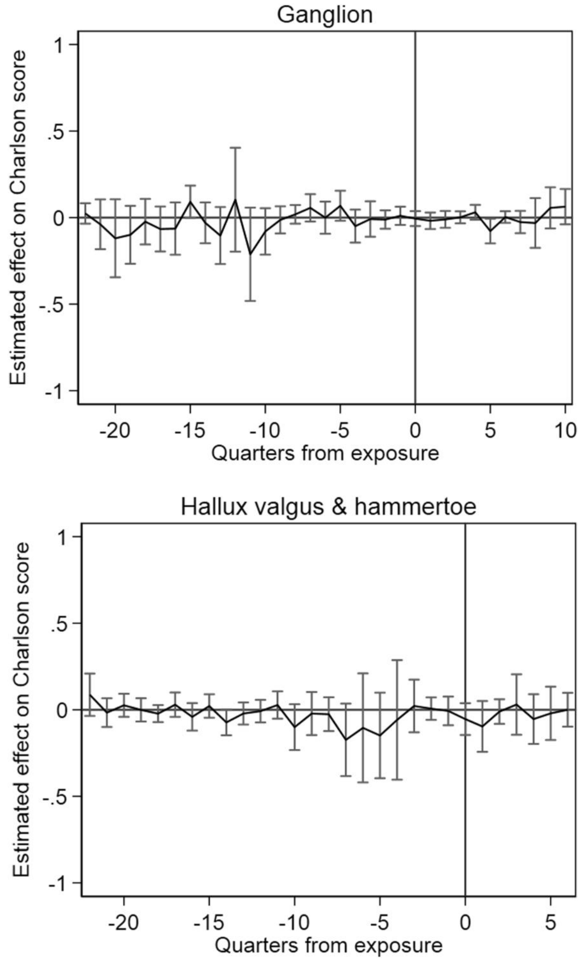

FIGURE 5 (Continued)

TABLE 3 Average effects of EFC on waiting time and number of visits in exposed public hospitals.

	Log waiting time		Number of visits		
	DID (days)	**95% CI**	**DID**	**95% CI**	***N***
Stress ECG	0.05 (0.06)	−0.24, 0.35	19.64	−6.01, 45.29	991
Endoscopy	−0.14 (−0.12)	−0.48, 0.20	39.02	−74.89, 152.94	969
Carpal tunnel syndrome	−0.01 (0.01)	−0.16, 0.15	−0.48	−15.70, 14.75	1004
Ganglion	0.08 (0.13)	−0.22, 0.37	4.25	−0.34, 8.84	936
Hallux valgus and hammertoe	−0.05 (−0.12)	−0.22, 0.12	0.36	−5.42, 6.14	957

Note: This table shows simple averages of post-event study estimates of the effects of EFC on log waiting time and the number of visits using the difference-in-differences method described by Callaway and Sant'Anna (2021). The 95% confidence intervals were constructed from standard errors clustered at the hospital level and are in brackets. N is the number of hospitals × quarters.

*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

appreciably if we delay the timing of the exposure by 1 quarter. In the main analysis, we use CSDID to guard against exposure effect heterogeneity. However, as we note in Section 5 Empirical framework, a common way to estimate DID is via two-way fixed effects (TWFE). Table 7 shows that the results from estimating Equation (1) are similar to the main CSDID results. This is likely in part because there is slight variation in the exposure timing, but also because hospitals that are exposed at different times are separated geographically. An advantage of the TWFE estimator is that it can be estimated at the patient level. This allows us to better control for patient severity. Reassuringly the results are quite similar when estimating Equation (1) on patient level data.

TABLE 4 Average effects of EFC on total publicly funded volume.

	Total volume		
	DID	95% CI	N
Stress ECG	38.82	−1.74, 79.37	991
Endoscopy	85.65	−45.39, 216.68	969
Carpal tunnel syndrome	3.34	−15.03, 21.70	1004
Ganglion	6.84	1.79, 11.89	936
Hallux valgus and hammertoe	2.46	−4.82, 9.73	957

Note: This table shows simple averages of post-event study estimates of the effects of EFC on total publicly funded volume using the difference-in-differences method described by Callaway and Sant'Anna (2021). The 95% confidence intervals were constructed from standard errors clustered at the hospital level and are in brackets. N is the number of hospitals × quarters.

*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

TABLE 5 Average effects of EFC on patient Charlson score in exposed public hospitals.

	Charlson score		
	DID	95% CI	N
Stress ECG	−0.01	−0.08, 0.07	991
Endoscopy	0.06	−0.16, 0.28	969
Carpal tunnel syndrome	0.03	−0.04, 0.10	1004
Ganglion	−0.00	−0.03, 0.03	936
Hallux valgus and hammertoe	−0.03	−0.11, 0.06	957

Note: This table shows simple averages of post-event study estimates of the effects of EFC on patient Charlson scores using the difference-in-differences method described by Callaway and Sant'Anna (2021). The 95% confidence intervals were constructed from standard errors clustered at the hospital level and are in brackets. N is the number of hospitals × quarters.

*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

TABLE 6 OLS estimates of the association between EFC providers and patients' Charlson score.

	OLS	95% CI	N public exposed	N EFC
Stress ECG	−0.04**	−0.07, −0.01	10366	2405
Endoscopy	−0.08***	−0.11, −0.06	29983	3291
Carpal tunnel syndrome	0.03	−0.03, 0.09	2449	265
Ganglion	0.01	−0.01, 0.02	799	104
Hallux valgus and hammertoe	0.05	−0.06, 0.15	760	107

Note: This table shows the OLS estimates of the association between EFC providers and patients' Charlson scores. The sample included patients from all the exposed public hospitals and EFC providers. Patient characteristics such as age, sex, educational attainment, immigrant status, and marital status were used as control variables. We included the GP fixed effects. The 95% confidence intervals in brackets. N public exposed and N EFC are the number of patients in public exposed and EFC providers, respectively.

*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

8 | CONCLUDING REMARKS

The EFC reform was introduced in November 2015 and discontinued from January 2023 owing to a new majority in the Parliament and a new government from October 2021. The reform extended patients' choice of providers to approved private EFC providers. We investigated the impact of the reform on waiting times, number of visits, and patients' Charlson Comorbidity Index scores in public hospitals for five prevalent somatic services of both the diagnostic and

TABLE 7 Average effects of EFC on waiting time, number of visits and patients' Charlson score in exposed public hospitals estimated using two-way fixed effects.

	Log waiting time	Number of visits	Charlson score	*N*
Stress ECG				995
DID coefficient	0.05	−7.00	−0.00	
Age groups (years)				
0–19 (baseline)				
20–59	0.17	95.91*	0.32	
60–74	0.01	108.47**	0.43	
75+	−0.11	94.59*	0.43	
Female	0.25*	13.57*	−0.03	
Immigrant	−0.11	24.35	−0.01	
Partner	−0.03	15.34	0.04	
Education				
No high school (baseline)				
High school	−0.19	8.90	−0.05	
University	−0.44	8.40	−0.08	
Charlson score	0.25	−6.68		
Endoscopy				976
DID coefficient	0.08	5.89	0.08	
Age groups (years)				
0–19 (baseline)				
20–59	0.06	523.43*	−0.12	
60–74	−0.64	442.98	−0.04	
75+	−0.92**	356.95	−0.22	
Female	0.86***	60.38	0.21	
Immigrant	0.36	79.25	−0.58	
Partner	0.20	−41.66	0.31	
Education				
No high school (baseline)				
High school	−0.05	43.92	−0.40	
University	0.03	−59.37	0.36	
Charlson score	−0.11***	2.03		
Carpal tunnel syndrome				986
DID coefficient	−0.11	−1.97	0.06	
Age groups (years)				
0–19 (baseline)				
20–59	−0.49	−30.16*	−0.49	
60–74	−0.75	−29.76*	−0.34	
75+	−0.79	−30.14*	−0.35	
Female	−0.02	−0.93	−0.07	

(Continues)

TABLE 7 (Continued)

	Log waiting time	Number of visits	Charlson score	*N*
Immigrant	−0.09	2.36	0.06	
Partner	−0.00	0.06	−0.01	
Education				
No high school (baseline)				
High school	0.12	−1.16	0.02	
University	−0.09	1.63	−0.01	
Charlson score	0.07	−0.11		
Ganglion				932
DID coefficient	−0.01	−1.11	0.01	
Age groups (years)				
0–19 (baseline)				
20–59	0.06	0.83	0.03	
60–74	0.02	0.87	0.10**	
75+	−0.29	−0.38	0.10*	
Female	0.01	0.38	0.03*	
Immigrant	0.03	0.52	0.04	
Partner	−0.06	0.00	−0.03	
Education				
No high school (baseline)				
High school	0.09	−0.73	0.01	
University	0.08	−0.94	0.00	
Charlson score	0.41**	1.87		
Hallux valgus and hammertoe				930
DID coefficient	0.01	−0.97	0.01	
Age groups (years)				
0–19 (baseline)				
20–59	−0.44	6.79	0.06	
60–74	−0.29	7.14	0.09	
75+	−0.61*	6.96	0.15	
Female	0.08	0.26	0.08	
Immigrant	−0.04	3.79*	−0.05	
Partner	0.16	−0.12	0.00	
Education				
No high school (baseline)				
High school	−0.09	−2.83*	0.04	
University	−0.02	−3.62**	−0.02	
Charlson score	−0.10	0.27		

Note: This table shows estimates for γ from Equation (1) on waiting time, number of visits and patients' Charlson score. The standard errors were clustered at the hospital level. N is the number of hospitals × quarters.

*$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

curative types. We used a DID model to compare changes over time for public hospitals with and without EFC providers in the catchment area. Except for one surgical procedure, Ganglion, we find that the EFC reform did not facilitate an expansion in the total publicly funded supply nor exert pressure on public hospitals to stimulate shorter waiting times and more visits. In addition, the reform did not affect the comorbidity burden of patients in public hospitals; for minimally invasive diagnostic services, patients with fewer comorbidities were treated in EFC providers; for surgical services, we did not detect any difference in patient comorbidity between public and EFC providers.

The results imply that the intended competition pressure to stimulate efficiency in terms of shorter waiting times and more services did not occur. Since the government's aims were not fulfilled, we suggest that the mechanism supposed to create increased social efficiency did not work. Although the volume among EFC providers was too small to create a statistically significant increase in the total volume of private and public providers, the EFC services represented a benefit for the patients who experienced a shorter waiting time than they would have experienced in public hospitals. The question is then whether this reduction in waiting time is sufficient to claim that the reform is socially efficient. According to the policy makers' aims, we suggest a 'no'.

The five services we study are selected for reasons that are expected to be favorable to finding an effect of the reform. Both the absolute numbers of the services and the numbers at EFC providers relative to public providers pull in that direction. Hence, we would risk a bias of results if we had found effects and had concluded in favor of the reform more generally. In our case, we did not find many effects even for the favorably selected services. It is then unlikely that we would find effects for the less favorable services, and we conclude that the reform does not seem to affect waiting times and number of services.

Our study contributes to the empirical literature that examines the effects of opening price-regulated markets to competition by providing evidence from another country (Norway), and from minimally invasive diagnostic services and other surgical procedures than what are already studied. Our study supports the findings of Cooper et al. (2018) and Kelly and Stoye (2020) in that new entrants exerted little pressure on public hospitals and that the new entrants had less severe patients. However, we also found differences, including that the patient comorbidity burden in public hospitals was not affected. The government's motivation for introducing EFC was to encourage increased capacity utilization among both private and public providers. Private providers applied to HELFO for approval as providers with the right to send claims to the public health insurance administration. The HELFO forwarded bills from approved private providers to the public hospital responsible for the catchment area covering a patient's residential address. As we were unable to identify more procedures or reduced waiting times in public hospitals, the mechanism supposed to create increased social efficiency did not work. An additional concern is that private providers may contribute to inequity in the utilization of health services. Most private providers are in a narrow geographical area around the capital, Oslo, and the second-largest city of Norway, Bergen. Most of the patient population did not benefit from the reform because the waiting times and capacity in public hospitals were not affected.

One lesson from the reform is that opening a price-regulated market to competition is not sufficient to increase efficiency in the public sector. This is not surprising, as the theoretical literature has learned that private providers may act strategically to dampen quality competition (e.g., Hehenkamp & Kaarbøe, 2020). Another lesson from the reform is that policymakers should carefully consider how the various competition mechanisms affect the incentives of public hospitals and whether expansion of the tender mechanism may be more appropriate than an EFC reform. Finally, although the aim of improving efficiency of the public hospitals was clearly stated, the reform was implemented in such a way that doing business as usual had few financial consequences for public hospitals. We believe this is one main explanation for why this aim was not achieved.

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CONFLICT OF INTEREST STATEMENT

None of the authors have a conflict of interest to disclose.

DATA AVAILABILITY STATEMENT

Research data are not shared.

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ENDNOTES

[1] The reform was heavily debated, and after a change in government in 2021, the newly elected government decided to abandon the EFC reform, starting from January 2023 (The Parliament, 2022).

[2] Elective hip replacements, knee replacements, hernia repairs, and arthroscopies.

[3] To quantify the public and private hospital market structure, the authors measure counts and Herfindahl-Hirschman index within three separate market definitions: one market of a fixed geographic size and two market definitions based on a size that captures a fixed population.

[4] The remuneration system for contracted specialists is three-fold, encompassing financial support from regional health authorities, reimbursements from insurance funds, and patient co-payments.

[5] Hagen et al. (2018) analyses the price effect of competitive tendering of day surgeries in Norway. The study revealed a significant disparity in pricing between private and public hospitals, with private institutions offering day surgeries at considerably lower costs. It was observed that the introduction of competitive tendering mechanisms served as a catalyst for the observed price reduction. Their analyses demonstrated an average price difference of 26 percentage points between private and public hospitals during the study period encompassing 2010 to 2011. These years represented the final phase of investigation in the study.

[6] In this study, we do not include visits to private hospitals with tender contracts with an RHA nor visits to RHA-contracted specialists. First, these providers are regulated according to separate instructions from the Ministry of Health and Care Services, meaning that visits are plausibly exogenous to the reform. These instructions have not changed during the sample period. Second, the number of services in RHA-contracted specialists and private hospitals with tender contracts is likely small. We confirm that this is the case for private hospitals with tender contracts, which accounted for the following percentage shares of publicly funded visits: stress ECG 0%, endoscopy 0.3%, carpal tunnel syndrome 6.4%, ganglion 9.3%, and hallux valgus and hammertoe 0.9%.

[7] Endoscopy includes colonoscopy, gastroscopy, rectoscopy, and sigmoidoscopy.

[8] For periods after the treatment. For periods before the treatment, replace $g-1$ with $t-1$.

[9] For example, the pre-period average waiting time for stress ECG in the exposed group was 110 days, and a DID coefficient of 0.05 on log waiting time means 0.05% of waiting time. Thus, the impact on waiting time is (0.05*110)/100 = 0.06 days.

[10] In a robustness check, we show that results change little when we increase the look-back window for ascertaining comorbidity to 1 year. The reason for a six-month look-back window is to limit the number of patients whose history of diagnoses prior the hospital visit of interest is shorter than 6 months. We use the Stata module charlson to compute Charlson score.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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