What Divides Us Can Kill Us: Political Polarization, Human Capital Allocation and Hospital Outcomes

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Abstract

This paper provides empirical evidence on the negative effects of political polarization on human capital allocation and hospital outcomes. We leverage Spain's centralized rank-based system for allocating recently graduated doctors to hospitals. We obtain (i) a precise measure of each doctor's quality and (ii) their work-location choices. Exploiting a plausibly exogenous increase in political polarization in one Spanish region, we find that polarization reduces the average quality of incoming doctors, as higher-ranked candidates become less likely to select positions in the affected region. Consistent with this decrease in doctors' quality, we document an increase in in-hospital mortality rates and length of stay. Our findings highlight the broader economic and social costs of political polarization, particularly its impact on the distribution of skilled professionals and public health.

JEL Codes: D72, I12, J24

Key words: Health, Human Capital, Political Polarization

1 Introduction

In recent decades, political polarization, marked by deepening ideological divisions and increasing partisan hostility, has become a defining feature of contemporary society (Gentzkow 2016; Autor et al. 2020).[12] As a result, the study of its consequences has emerged as a central concern in economics (Iyengar et al. 2019). While some studies have begun to examine the effects of political polarization on individuals' residential choices (Kaplan et al. 2022), there is no analysis on whether it affects the regional distribution of human capital, a key driver of economic growth (Mankiw et al. 1992).[3] In a context where regions may be unevenly affected by political polarization, this unexplored dimension can be a crucial determinant of economic outcomes, especially in human capital intensive sectors.

The main objective of this paper is to empirically assess the effects of political polarization on human capital allocation and its associated real effects. Achieving this, however, requires overcoming two fundamental challenges that to the best of our knowledge remain unresolved in the existing literature. First, the lack of granular data linking individual human capital to revealed allocation preferences, which hinders even the identification of correlational patterns. Second, the absence of plausible exogenous variation in regional polarization, which constrains the use of credible identification strategies.

We overcome the highlighted challenges and document that an increase in political polarization in a region leads to a relative decrease in the quality of human capital in such region. We link this finding to a change in the revealed preferences of workers' allocations (medical residents' location choices). Next, we document how this increase in political polarization reduces productivity (hospital outcomes). Consistent with the observed decrease in human capital, we find that the region suffering an increase in political polarization experiences a differential decline in productivity, specifically in hospital quality measured as higher in-hospital death rates and longer hospital stays.

1. Throughout this article, we follow the definition of political polarization provided by Kempf and Tsoutsoura (2024): "both the partisan differences between individuals and the political uniformity within groups, which contributes to a landscape of politically segregated environments."
2. Recent literature, while showing heterogeneity across countries (Gidron 2020; Boxell et al. 2024; Draca and Schwarz 2024), tends to document a rise in political polarization over time, with the increase being particularly pronounced in the United States (Baker et al. 2014; Boxell 2020; Dimock and Wike 2020; Boxell et al. 2024), but also significant in Europe (Müller and Schnabl 2021).
3. Recent literature on human capital and economic growth includes studies that highlight its role in development across countries, within countries, and in the long run (Hanushek et al. 2017; Hendricks and Schoellman 2018; Hsieh et al. 2019; Toews and Vézina 2020; Deming 2022; Hendricks and Schoellman 2023).

To address the challenges of measuring human capital and allocation decisions at a granular level, we leverage Spain's centralized placement system for medical residents, known as MIR (Spanish acronym for intern resident physician).[4] In this system, all candidates take a standardized national exam and, based primarily on their score on such exam, sequentially select a specialty and training hospital until all positions are filled. This institutional setting provides standardized measures of individual human capital, as well as revealed preferences over hospital-specialty pairs. Building on these features, we construct a novel dataset covering the universe of MIR decisions from 2012 to 2024, enabling to measure the human capital of residents at the hospital-specialty level with high precision.[5]

Our empirical strategy exploits the sharp and plausibly unexpected increase in polarization triggered by the events surrounding the October 2017 secessionist crisis in Catalonia, a Spanish region.[6] We argue that these events provide a natural laboratory for our analysis for the following reasons. First, we document how the outbreak of tensions in October was largely unexpected, with no prior indication in public opinion or media coverage until just a month before the crisis unfolded. This allows us to precisely identify the moment of increase in political polarization. Second, the crisis was geographically concentrated in Catalonia. This allows us to identify a well-defined region where political polarization intensified, potentially affecting its relationship with the rest of Spain while leaving the dynamics among other Spanish regions plausibly unchanged. Finally, while the social conflict was intense by its own standards, it did not result in fatalities. This helps to isolate the effects of an increase in political polarization from the broader range of potential outcomes that could arise from a more prolonged and intense conflict.[7]

This setup allows us to implement a difference-in-differences framework, with Catalonia as the treatment region and the rest of Spanish regions as the control group. Due to

4. For example, Machado et al. (2012) leverage the Spanish placement system for medical residents to infer hospital quality from revealed preferences. Similarly, Gottlieb et al. (2025) use USMLE scores as a proxy for talent.
5. Other resident matching markets, such as the National Residency Matching Program in the United States, also exhibit similar features (Agarwal 2015, 2017). However, unlike the MIR system, which operates as a one-sided market in which all decision-making power is on the candidates' side, these systems function as two-sided markets, meaning that both the resident and the training program participate in the selection process.
6. Balcells and Kuo (2023) document high and persistent levels of political polarization surrounding such events.
7. One person's death has been linked indirectly to the street protests by the spanish police. Specifically, a French tourist suffered a fatal heart attack and was unable to receive timely medical attention due to disruptions blocking access to the airport. See: https://www.rtve.es/noticias/20240229/guardia-civil-concluye-protestas-tsunami-retrasaron-atencion-turista-frances-muerto-prat/15991798.shtml.

the closed-economy nature of the MIR setting, any change in the treatment region is mechanically reflected in the rest of the country, enabling us to capture relative effects across regions rather than average treatment effects.[8] In our analysis we compare the MIR exam scores of residents who chose hospitals in Catalonia with those of residents who chose hospitals in other parts of Spain, both before and after 2018.[9] For causal inference to be valid under this identification strategy, the parallel trends assumption must hold. That is, in the absence of the late 2017 events, the average scores in Catalonia and the rest of Spain would have followed similar trends. We provide evidence supporting this assumption by using event-study versions of our baseline specification. To strengthen our analysis, we aggregate the individual score data at the hospital-specialty level and apply a synthetic difference-in-differences approach (Arkhangelsky et al. 2021), which combines the two-way fixed effects model of the difference-in-differences framework with the synthetic control method (Abadie and Gardeazabal 2003) into a single estimator.

Our core finding of this analysis is that the late-2017 events led to a relative decrease in the average quality of human capital in Catalonia, the region directly affected by the increase in political polarization. According to our estimates, the shock resulted in a 0.12σ decrease in the average score of residents in Catalonia, equivalent to a drop of 3.84 percentiles or 7.2% relative to pre-shock levels. Year-by-year estimates show no evidence of pretrends and show that the relative effect documented persists at least until 2024, seven years after the shock and the last year in our sample. Notably, the decline in human capital was more pronounced in municipalities with higher levels of polarization, proxied by support for secessionist parties, and was primarily driven by individuals with higher outside options, captured by higher MIR scores.

We then explore the mechanism underlying our first main finding. In particular, we investigate whether the decline in exam scores reflects ideological sorting —that is, a spatial clustering of individuals with similar views on the conflict— or whether it was primarily driven by disutility from increased polarization among individuals who are averse to being exposed to conflict. To this end, we examine whether the decline in scores differed between candidates graduated in Catalan universities, whom we assume are more likely to support the secessionist cause, and those from universities in the rest of Spain, whom we assume are less sympathetic to it. We find no evidence that the drop in scores was statistically different

8. A prominent example of this approach is Dix-Carneiro and Kovak (2017).

9. While the shock occurred in October 2017, medical graduates had already taken the MIR exam on January 27 of that year. Thus, we assume that the 2018 cohort of candidates was the first to be affected by the shock.

across these two groups. We interpret this result as consistent with a mechanism primarily driven by the decisions of individuals who are averse to being exposed to conflict.

A natural question that arises from these findings is where the human capital displaced from Catalonia was reallocated. As noted above, the MIR exam functions as a zero-sum game: if one region loses high-scoring candidates, others must necessarily gain them.[10] Our estimates reveal that several regions experienced statistically significant differential gains, with the largest positive effects observed in Murcia, Valencia, Galicia, and Andalusia. Interestingly, we also document a negative spillover effect in the Basque Country, the other Spanish region with a prominent secessionist movement.

We conclude this part of the analysis by conducting a battery of robustness checks to validate our core findings. These exercises address concerns related to sample composition, potential confounders, and clustering choices. We also consider an alternative explanation for the observed effects: a decline in the quality of education received by students at Catalan universities following the unrest of October 2017.[11] If educational disruptions were the driving force behind the decline in MIR scores in Catalonia, we would expect a differential drop in performance among students from Catalan universities. However, we find no statistically significant change in their scores relative to students from other Spanish universities after the shock, leading us to reject this alternative channel.

It is important to clarify the scope of our results. We interpret the observed decline in residents' human capital as a proxy for broader effects on the healthcare workforce. Due to data limitations, we are unable to measure human capital or allocation decisions for other key healthcare providers —such as senior doctors or nurses— who may also have been affected by the increase in political polarization. For this reason, we refrain from attributing our findings on health outcomes solely to changes in the behavior of newly trained doctors. Even so, the potential relevance of medical residents for hospital performance should not be understated, as they represent a substantial share of the physician workforce, with a resident-to-doctor ratio around 0.3 during our study period.[12]

Having documented a decrease in hospitals' human capital in the region affected by the increase in political polarization, we next examine whether it resulted in worse hospital

10. To identify whether any region from the pool of control regions disproportionately benefited from the decline in scores in Catalonia, we iteratively re-estimate our baseline specification by assigning treatment status to each control region in turn, excluding Catalonia from the sample.

11. Prior research has consistently documented the strong negative effects of educational interruptions on learning. See, for example, Betthäuser et al. (2023) for the case of COVID-19 pandemic.

12. To construct the ratio, we only consider doctors with permanent contracts. However, if we also include those with temporary contracts, the ratio drops to around 0.25.

outcomes. To explore the relationship between political polarization and healthcare performance, we analyze individual-level data on over two million patients and implement a difference-in-differences strategy that mirrors our earlier approach. Specifically, we compare health outcomes of patients treated in Catalonia to those treated in the rest of Spain, before and after 2018. The granularity of our data enables us to perform risk-adjusted comparisons.[13] We further validate our results by aggregating the data at the province level and applying a synthetic difference-in-differences approach. Reassuringly, event-study designs at both the individual and aggregate levels show no evidence of differential pre-trends in health outcomes prior to the onset of political polarization.

We document a significant relative decrease in hospital performance in the treatment region after the polarization increase. Something consistent with the documented decrease in hospitals' human capital. Our primary measures of hospital performance are in-hospital mortality events and patients length of stay, two widely adopted metrics in the literature (Lingsma et al. 2018; Doyle et al. 2019; Chandra et al. 2023). We focus our analysis on those patients diagnosed with heart attack (acute myocardial infarction), heart failure, and pneumonia, as the non-deferrable nature of these conditions makes them standard benchmarks for assessing hospital quality (Ryan et al. 2017; Gupta 2021; Aghamolla et al. 2024). Baseline estimates indicate that individuals directly exposed to the increase in political polarization suffered a 0.6 percentage point increase in mortality, implying an 8% rise relative to the pre-treatment mean, whereas the length of stay increased by approximately 3.7%. Back-of-the-envelope calculations suggest that this corresponds to 306 additional deaths in the affected region annually.

Next, we investigate whether the observed decrease in hospital performance could be explained by alternative mechanisms rather than reductions in human capital. To do so, we use annual hospital-level data and implement synthetic difference-in-differences regressions comparing hospitals in Catalonia to those in the rest of Spain before and after 2018. On the demand side, one might be concerned that the late 2017 events in Catalonia triggered either an increase in healthcare demand or compositional shifts in the patient population. We find no evidence supporting either channel. On the supply side, we rule out changes in the extensive margin of labor as well as variations in hospital investments and purchases. Taken

13. We include a comprehensive set of fixed effects: year-month fixed effects to capture seasonality in health outcomes; province-diagnosis fixed effects to account for geographic variation in medical practice and resource availability; year-diagnosis fixed effects to capture evolving treatment protocols; and age-gender fixed effects to absorb systematic demographic differences in patient outcomes.

together, these findings reinforce our proposed mechanism.

We conclude the analysis with a series of complementary exercises that reinforce the robustness of our findings. In particular, we address concerns about results being driven by a single condition, as well as other potential issues regarding patient selection, sample restrictions and clustering decisions.

1.1 Literature Review

By exploiting a plausibly exogenous increase in political polarization and using it to trace its real effects on the healthcare sector through changes in human capital allocation, we contribute to at least four strands of the literature

First, we contribute to the growing literature on the consequences of political polarization (see Iyengar et al. (2019), for a review). Within this body of research, several studies have examined the relationship between political polarization and geographic sorting, particularly whether ideologically similar individuals are becoming increasingly segregated across space (Bishop and Cushing 2009; Abrams and Fiorina 2012; Brown and Enos 2021) and whether this sorting is intensifying over time (Glaeser and Ward 2006; Kaplan et al. 2022; Brown et al. 2023). However, to the best of our knowledge, no study has documented the potential economic consequences of this polarization-driven reallocation of human resources.[14] Specifically, there is no empirical evidence on whether such sorting distorts the regional distribution of human capital, a key driver of economic growth. We directly address this gap by showing that an exogenous increase in polarization shifted residents' location preferences, leading to a persistent decline in the average level of human capital in the affected region's healthcare sector. This shift, in turn, affected hospital performance, as reflected by increased in-hospital mortality and longer lengths of stay. Furthermore, our paper extends existing research by providing suggestive evidence that the geographic sorting may also be driven by neutral individuals avoiding polarized environments and not only by ideologically motivated clustering.

Our paper is also linked to the literature on brain drain. Previous studies have identified multiple drivers of high-skilled labor outflows, including economic development (Beine

14. Although related, it is important to distinguish between partisan sorting—the movement of individuals across geographic space based on political preferences—and partisan assortative matching, which refers to individuals forming social or economic ties (e.g., friendships, marriages, or professional relationships) with others who share their political views. Regarding the latter, recent evidence shows that partisan assortative matching within firms is associated with lower organizational performance. See, for example, Lee et al. (2014), Colonnelli et al. (2022) and Evans et al. (2024).

et al. 2001; Docquier and Rapoport 2012), forced migration (Waldinger 2012; Toews and Vézina 2020), immigration policy (Abarcar and Theoharides 2024; Khanna and Morales 2025), tax incentives (Prato 2025), and wage differentials (Dodini et al. 2025). We provide the first causal evidence that political polarization can also act as a trigger of brain drain. Unlike most of the related literature, which focuses on cross-border migration from developing countries (Batista et al. 2012; Dinkelman and Mariotti 2016), we examine a specific high-skilled sector, healthcare, within a developed country, Spain. Additionally, this setting enables us to contribute to the limited body of work assessing the impact of brain drain on service provision. In this respect, our paper is closely related to Dodini et al. (2025), who examine the outflow of physicians from Sweden in response to a labor demand shock in Norway. Beyond identifying a novel source of brain drain, our study departs from theirs by leveraging a granular proxy of human capital, allowing us to capture intensive-margin adjustments directly, rather than inferring them from commuting patterns.

Third, we deliver new, causal evidence on human capital and quality of care in the healthcare sector. Prior research has consistently documented the critical importance of human capital in this sector. For instance, in the case of radiologists, Chan et al. (2022) report that up to 78% of the variation in outcomes can be explained by differences in physicians' skills. Within this body of research, approaches to measuring human capital are diverse, ranging from metrics developed at the individual level, such as the application of value-added methods (Stoye 2022; Ginja et al. 2022) and other performance-based indicators (Currie and MacLeod 2017, 2020; Chan et al. 2022), to more indirect measures such as the prestige of the training institution (Doyle Jr et al. 2010), year of graduation (Hollingsworth et al. 2024), or time spent working as part of a team when human capital is assessed at the team level (Chen 2021). Within the strand of literature that uses national licensing examination scores as proxies for human capital, although most studies find evidence of exam scores as predictors of various quality-of-practice indicators (Tamblyn et al. 1998, 2007; Wenghofer et al. 2009; Doyle Jr et al. 2010; Norcini et al. 2014), causal evidence remains scarce. A notable exception is the contemporaneous work of Posso et al. (2024), which, leveraging a policy experiment in Colombia, shows how higher scores improve health outcomes at birth. Our paper adds to this literature by providing novel causal evidence on the impact of examination scores on quality of care across a broad set of medical specialties.

Finally, we contribute to the literature on healthcare capacity constraints. While prior work has shown that cost cuts, physician shortages, and labor disruptions strain health systems and increase mortality (Propper and Van Reenen 2010; Gaynor et al. 2013; Friedrich and Hackmann 2021; Knutsson and Tyrefors 2022), few studies examine the role of the labor intensive margin (Otero and Munoz 2022; Dodini et al. 2025). We advance this literature by showing that system strain can arise even in the absence of staffing gaps. Using administrative data from the Spanish residency assignment system, we provide causal evidence that lower human capital among newly assigned physicians—holding staffing levels constant—leads to worse patient outcomes. Compared to the limited existing work on the labor intensive margin, our analysis relies on a direct, objective measure of human capital at the individual level. In an international context where physician scarcity is an increasing concern (Adovor et al. 2021), our findings underscore the importance of physician quality for strengthening healthcare capacity.

The remainder of the paper is organized as follows. The next section provides background on both the Catalan secessionist conflict and the MIR system. Section 3 describes the data sources and presents descriptive statistics. Section 4 outlines and discusses the identification strategy. Section 5 and Section 6 present the results, including heterogeneity analyses and robustness tests, for human capital and hospital performance, respectively. First for the effects on human capital and then for the effects on hospital performance. Finally, Section 7 presents our concluding remarks.

2 Institutional Setting

This section provides a description of the MIR system, the institutional setting from which we derive our measure of human capital and our revealed-preference based allocation decisions. We also provide a brief historical context surrounding the 2017 secessionist events in Catalonia and the increase in political polarization.

2.1 The MIR System

In Spain, nearly all medical graduates complete a residency training period of four to five years, during which they specialize in a specific medical field (e.g., digestive system, oncology, cardiology).[15] Residency positions, defined as hospital-specialty pairs, are allocated

15. For medical graduates in Spain, the range of career opportunities is limited if they do not go through the MIR process. Some of the available options include working as military doctors, in vaccination centers, or as medical inspectors. As a result of this limited number of options, more than 92% of graduates choose to take the MIR exam. See: `https://www.cesm.org/2023/02/20/informe-del-numero-de-egres`

through a centralized matching process known as MIR system.

The selection process is based on two merit-based components. First, candidates' undergraduate academic records are considered. Second, all participants take a standardized exam, known as the MIR exam, which features the same questions and grading scheme for everyone. The total score is a weighted average of these two components, with the academic record accounting for 10% and the MIR exam for 90%.[16] Candidates are then ranked by their total score, and allocation follows a serial dictatorship mechanism: the highest-ranked candidate selects first, then so does the second-best, and so on, until all offered positions are filled. Importantly, candidates are aware of their entire choice set. That is, they know the choices made by higher-ranked candidates and, consequently, the positions still available.

The key dates of this process, summarized in Figure 1, unfold as follows. Every year, the Ministry of Health determines the number of positions offered for each specialty at hospitals accredited to train medical graduates. This decision, which considers both the local population's needs and the training capacity of the institutions, is publicly announced in September. In this moment, final-year medical students decide whether to take the MIR exam, which is mandatory in order to be eligible for the positions offered. In case of applying for any of the positions offered, students are required to complete an institutional form to submit their application for the exam. Then, in December, the Ministry of Health announces the total number of candidates taking the exam. The MIR exam takes place in January or February of the following year. It consists of multiple-choice questions, ranging from 170 to 250, and lasts between 4 to 5 hours. The MIR exam is considered a high-stakes test due to its format and the pool of skilled candidates that take it.[17]

Once the total score is known, around March-April, candidates, as decribed above, follow a first-ranked first-served scheme and select from the available positions sequentially, ranked by their total score. Finally, by early summer, once the positions for specialist doctors in training have been allocated, the candidates who have obtained a position sign an on-the-job training contract and begin their training as residents.[18] This period lasts between four

ados-de-las-facultades-de-medicina-que-se-han-presentado-al-examen-mir-desde-2017/

16. Until 2009, the MIR exam accounted for 75% of the final score. From 2010 onwards, its weight increased to 90%. For further details about changes in the MIR test, see Díez-Rituerto et al. (2022).
17. Between 2018 and 2023, the average admission score across all degree programs was 7.9, whereas for Medicine, it reached 12.9, making it the program with the highest average admission score (Ministerio de Universidades de España 2024).
18. By law, the terms of this contract are established by regional health authorities, leaving no room for hospitals to set their own conditions

and five years, and once it finishes residents are authorized to work as specialized doctors within Spain's National Health System (NHS). It is relevant to note that during their period as residents, and although they are not formally doctors, residents have full-time work schedules, often combined with an overload of responsibilities.[19]

2.2 Political Polarization in Catalonia

The surge in secessionist intensity in late 2017 is part of the Catalan independence process, commonly knowns as *El Procés*. This movement, led by the *Generalitat* (Catalonia regional government) since 2012, aimed to achieve self-determination and independence from Spain, culminating in the unilateral declaration of independence in October 2017.

Although it is difficult to formally pinpoint the beginning of *El Procés*, a reasonable starting point is September 2012, when Catalonia's president, Artur Mas, from CiU (the main nationalist right-wing party), failed to negotiate the so-called fiscal pact, a specific regional funding model for Catalonia, with the central government of Spain. Following this breakdown in negotiations, Mas called for early elections, with the self-determination referendum as the main point of his electoral program. CiU won the elections and signed a governance pact with ERC (the main nationalist left wing party), committing to hold a sovereignty referendum before the end of the legislature. Initially framed as 'the right to decide', the movement soon adopted a more radical tone when Mas declared, 'First, we have to try to do it in accordance with the laws, and if we can't, do it anyway'.[20]

In January 2013, the Catalan Parliament approved the Declaration of Sovereignty and the Right to Decide of the People of Catalonia. A year later, in March 2014, the Constitutional Court declared this declaration unconstitutional. Despite this, the *Generalitat* proceeded with *El Procés* and, in November of that year, held a non-binding self-determination consultation. One third of the electorate voted, with 80% in favor of independence. Although the results were non-binding, Mas once again called for early elections in September 2015, now interpreting them as a plebiscite on independence. CiU and ERC, two parties that had previously competed with each other, ran as a single list united by the goal of independence. Their two leaders, Artur Mas and Oriol Junqueras, respectively, promised to declare Catalonia's independence within 18 months if they won. When the elections took

19. For instance, in 2024, 71.4% of MIR residents reported performing emergency on-call duties without the minimum required supervision from senior doctors. See: https://drive.google.com/file/d/1FfLwu_oPqRgZvd5co-QDwM042aOlZFk3/view

20. https://elpais.com/ccaa/2012/09/26/catalunya/1348651881_550302.html

place, although the coalition won in seats, it did not win in votes.

After such elections, Mas and Junqueras felt legitimized to pursue their primary electoral promise, and in November 2015, the Catalan Parliament passed a secessionist resolution, initiating the process to establish an independent Catalan state. To secure the majority needed for this vote, the governing coalition of CiU and ERC (known as *Junts pel Sí*) had to negotiate with the CUP, a left-wing pro-independence party. As a condition for reaching an agreement, the CUP imposed the dismissal of Artur Mas in exchange for appointing Carles Puigdemont as president of the *Generalitat*.

In early 2017, the Catalan government accelerated *El Procés*. In March, budget funds were allocated for the referendum scheduled for October. Next, in June, it officially and unilaterally announced the self-determination referendum, bypassing the rest of the Spanish state. In September, the Catalan Parliament passed the law authorizing the referendum and outlining the consequences of its potential outcome. Although being declared unconstitutional, the referendum took place on October 1, 2017. That day was marked by clashes between police forces, who attempted to halt the voting, and some of the citizens determined to vote. Despite these efforts, the referendum continued, with 90% of voters favoring independence, though turnout was at 43%.

In the aftermath, the King (head of state in Spain) delivered a public speech opposing the referendum, while large-scale protests both for and against it took place. On October 10, at the peak of the crisis, Carles Puigdemont declared Catalonia's independence in the Catalan Parliament. In response, Spain's government, led by Mariano Rajoy (PP, the primary right-wing constitutionalist party), with the support of the main opposition party (PSOE, the primary left-wing constitutionalist party), invoked Article 155 of the Constitution. This measure allowed the central government to assume control of Catalonia's regional administration. Regional elections were subsequently scheduled for December 21, characterized by high turnout (80%) and a victory for pro-independence parties. Although tensions have persisted since then, the secessionist movement has not returned to the intensity reached in late 2017.

Importantly for our empirical strategy, the events leading up to late 2017 did not generate widespread concern or social unrest within Spanish society. We illustrate this lack of anticipation of the secessionist crisis using two pieces of evidence presented in Figure 2. Panel A draws on data from the CIS Barometer (the Spanish acronym for the Center for Sociological Research), a monthly nationally representative survey that asks respondents to identify the main problems facing Spain. The figure shows the share of respondents who cite "the independence of Catalonia" as one of the country's main issues. This share remained low, averaging 2.8%, through September 2017, but rose sharply to 29% in October, coinciding with the referendum. Following this surge, and in line with a persistent rise in political polarization, concern over Catalonia's independence did not return to prior levels. Instead, it stabilized, with some fluctuations, at an average of 10.8%. Panel B presents complementary evidence using the Reported Social Unrest Index developed by Barrett et al. (2022), which provides a monthly measure of media-reported social unrest across 130 countries. To place the 2017 events in historical perspective, we plot the index for Spain from 1990 to 2022. While the index shows a modest increase beginning in 2011—likely associated with the financial crisis—it spikes dramatically at the end of 2017, reaching an unprecedented peak over the three-decade period.[21] Moreover, event study estimates comparing Spain to other countries confirm that the increase in social unrest in late 2017 was specific to Spain and not part of a broader global trend (Figure A1).[22] Taken together, these figures suggest that while the events of late 2017 triggered a lasting shift in political polarization (Panel A), the unrest that initiated this shift was both sudden and short-lived (Panel B).

3 Data

In this section, we briefly summarize the data sources that we use in our analysis. We also present our main outcome variables and provide a description of the data we use.[23]

3.1 Data Sources and Main Variables

Human capital. For our human capital proxy, we primarily rely on administrative data on the MIR performance of nearly the entire population of exam takers over the period 2012–2024. Crucially, we link each exam taker's score to their final allocation decision (the

21. These dynamics are corroborated by related indicators of political and economic uncertainty (Baker et al. 2016; Ghirelli et al. 2019).

22. Specifically, we estimate:

$$SUI_{it} = \eta_i + \alpha_t + \sum_{k=07/2017}^{01/2023} \beta_k Spain_i + \epsilon_{it},$$

where the outcome variable SUI_{it} is the social unrest index observed in country i in month t, $Spain_i$ is an indicator that takes the value one if the country is Spain and zero otherwise, and η_i and α_t denote country and year fixed effects, respectively. Standard errors are clustered at the country level.

23. Table A1 of the Appendix provides a description of all the variables used throughout the analysis.

hospital-specialty pair), as well as other relevant characteristics such as their university of origin, nationality and gender.

To construct this dataset, we rely on two data sources. First, the Spanish Ministry of Health provided us with data on the universe of candidates who took the MIR exam during the 2012–2024 period. Specifically, this dataset includes information on candidates' academic record, their MIR exam score, their total score (90% MIR exam score and 10% academic record), their position in the ranking, as well as their university of origin, gender, nationality and specialty of choice. From our second source of information, we obtain data on the hospital for each candidate who successfully secured a position. This data is sourced from MIR Asturias[24], a preparatory academy for the exam which annually publishes all MIR allocation decisions except those reserved under the disability quota.[25] The merging of these two datasets yields an individual-level dataset comprising 89,897 observations, covering 96.4% of the universe.

Our proxy for human capital is the total score, *Score*, which represents a weighted average of the MIR exam score (90%) and the academic record (10%), as detailed in subsection 2.1. Higher scores correspond to better ranks, with rank position being the sole criterion for sequentially filling available positions. To account for potential differences between MIR calls across years, in our main analysis we standardize *Score* by year, such that each year the variable has a mean of zero and a standard deviation of one. Table A2 reports the correlation matrix between *Score* and its two components: Academic Record and MIR Score. Reassuringly, the correlations among all these measures are high and significant. This suggests that *Score*, although largely determined by performance in the MIR Exam, is not merely a matter of chance but rather a meaningful reflection of the human capital accumulated during the medical degree.

Health outcomes. We construct our measures of hospital performance using information from the EMS (acronym in Spanish for Hospital Morbidity Survey). This database, produced by the National Institute of Statistics (INE), collects information from all hospitals

24. https://www.curso-mir.com/consultaMIR/
25. The regulation mandates that 7% of the available positions be reserved for individuals with disabilities. Since these candidates do not compete directly with the rest due to the quota, previous studies exclude these positions from the analysis (Conde-Ruiz et al. 2020). Moreover, in practice, the share of disabled candidates is far below the 7% threshold—for instance, between 2018 and 2024, only 0.8% of positions were allocated to individuals with disabilities.

in Spain, both public and private, and provides detailed clinical information on patients, including age, gender, admission diagnosis (whether ordinary or urgent), date of admission, date of discharge, reason for discharge and main diagnosis according to the International Classification of Diseases. Geographical information, such as province of residence and province of hospitalization, is also available.[26]

Throughout the analysis, we focus on patients diagnosed with acute myocardial infarction (AMI), heart failure, and pneumonia, given that the undeferability of these conditions turn them in standard quality benchmarks, including those used by the U.S. Center for Medicare and Medicaid Services (Ryan et al. 2017; Gupta 2021; Aghamolla et al. 2024).[27] As hospital performance proxies, we use risk-standardized length of stay (*LoS*) and mortality events (*Death*), two widely adopted metrics in the literature (Doyle et al. 2019; Chandra et al. 2023).[28] Following standard practice (Bloom et al. 2015; Gaynor et al. 2013; Duggan et al. 2023), we focus on short-term outcomes and exclude patients hospitalized for over 28 days (2.8% of the original sample). Yet our findings are unaffected if we relax this sample restriction. The final patient-level dataset comprises 2,174,314 observations.

Hospital-level data. The SIAE database (acronym in Spanish for Specialized Care Information System) contains information on all hospitals, both public and private, in Spain. Specifically, it includes data on endowments, services offered, hospital activity, as well as financial and expenditure information, reported annually. Importantly, hospital identifiers are anonymized, and only the region in which each hospital is located is disclosed. In subsection 6.2, we use SIAE data from 2010 to 2019 to rule out alternative mechanisms that could be driving our results. To be included in the analysis sample, hospitals must be observed throughout the entire period and must report at least a mortality event per year. After applying these filters, we are left with a balanced panel of 4,728 hospital-year observations.

26. Unfortunately, the available data does not allow us to identify hospitals. Moreover, we acknowledge that not all hospitals host MIR residents. However, these training hospitals account for approximately 75% of all patients, so a substantial portion of the effects documented in our analysis is driven by this type of facility. It is also important to note that the effects we document need not be solely attributable to residents. Other healthcare professionals, such as senior doctors and nurses, may also have been affected by the increase in political polarization.

27. According to the International Classification of Diseases, acute myocardial infarction is coded as 410 under ICD-9 (used until 2015) and as I12-I22 under ICD-10 (from 2016 onward); heart failure is coded as 428 under ICD-9 and as I50 under ICD-10; pneumonia is coded as 480-486 under ICD-9 and as J12-J18 under ICD-10.

28. Another common quality measure is the death-adjusted readmission rate. However, due to data limitations, we do not include it in our analysis.

Additional data. In some of the results, we complement our analysis relying on data obtained from other sources. Specifically, in the heterogeneity analysis in subsection 5.2, we use data from the September 2015 and December 2017 regional elections in Catalonia at the municipal level, available on the website of the Generalitat.[29] In the robustness checks section, we incorporate several variables from external sources, as the net regional salary, sourced from the Andalusian Medical Union website.[30] One of these checks includes the rental price index at the municipal level as a control variable, obtained from the National Institute for Statistics (INE). Another additional variable used in section 6, includes the number of doctors by age group and region, also sourced from the INE.

3.2 Descriptive Statistics

Table A3 summarizes MIR data by treatment status, before and after the shock. Prior to 2018, Catalan residents scored 0.08σ above the national average, equivalent to the 53rd percentile (compared to just below the 50th percentile for the rest of Spain). After 2018, this reversed: scores in Catalonia fell to 0.06σ below the mean, while the rest of Spain rose to 0.01σ above it, corresponding to the 49.27th and 50.72nd percentiles, respectively. Consistent with this pattern, academic records and MIR scores declined for residents that chose Catalonia while improving for those choosing the rest of Spain. Figure 3 complements this static description of the data and previews our main result by showing the yearly evolution of average scores by treatment status. Before 2018, Catalan residents scored higher but followed a similar trajectory to the rest of Spain. After the shock, while scores in the rest of Spain remained stable, Catalan scores dropped sharply, falling below the national average. With respect to individual characteristics, we find no gender differences. However, the proportion of foreign residents in Catalonia is twice as high as in the rest of Spain, partly due to its greater share of medical graduates from abroad. Figure A2 shows the steady rise in MIR positions per call, from 6,509 in 2012 to 8,522 in 2024, with Catalonia's share slightly increasing to an average of 15.3% over the period.

Table A4 presents summary statistics at the patient level, distinguishing between geographic location (Catalonia or the rest of Spain) and periods (before or after 2018). Regarding our first health outcome, while mortality rates decline over the study period for AMI and pneumonia in both Catalonia and the control regions, the decline is more pronounced in the

29. See: https://gencat.cat/economia/resultats-parlament2017/09pdf/FTOP.htm?lang=es
30. See: https://simeg.org/

control regions. In contrast, the mortality rate for heart failure increases significantly more in Catalonia (16%) than in the rest of Spain (3%). A similar pattern emerges for our second health outcome, length of stay. While the average length of stay decreases for all three diagnoses in the rest of Spain, in Catalonia it either declines less (AMI and pneumonia) or increases (heart failure). In terms of patients' demographic characteristics, Catalonia is, on average, quite similar to the rest of Spain.

We end this section exploring the relationship at the regional level between our measure of human capital, *Score*, and our measures of hospital quality, *Death rate* and *LoS*, for the period 2012–2017. Figure 4 plots these relationships, showing a negative correlation between both measures and *Score*: regions with higher average levels of human capital tend to exhibit lower death rates and shorter hospital stays. This relationship holds after adjusting for different risk components (Figure A3).[31]

4 Empirical Identification

In this section, we first introduce a difference-in-differences model to estimate the relative effect of the political polarization shock on the regional distribution of human capital in Catalonia compared to the rest of Spain. We then assess whether the political polarization shock affected hospital performance outcomes. To do so we present a similar difference-in-differences approach that leverages regional variation in exposure to the shock with patient-level data. For both sets of regressions, on human capital and hospital outcomes, we extend our analysis by aggregating the data at the most granular geographic units available and re-estimating our models using synthetic difference-in-differences estimators.

4.1 Human Capital

4.1.1 Individual-level Specifications

In the first stage of the analysis, our empirical strategy aims to identify the impact of political polarization on human capital allocation of residents. To estimate this effect, we adopt a difference-in-differences approach, treating the late 2017 outbreak of political polarization as an unexpected shock to individuals' preferences. That is, we compare levels of human capital between Catalonia and the rest of Spain, before and after 2017. The primary identify-

31. Using microdata from the HMS for AMI, heart failure, and pneumonia, we adjust for age, gender, and diagnosis. Details on the risk-adjustment procedure are provided in subsection 6.1.

ing assumption of this design is that, in the absence of the late 2017 events, human capital in Catalonia would have followed parallel trends compared to the rest of Spain. Additionally, identification relies on the absence of confounders coinciding with the timing of the shock, a point we address in subsection 5.5. Specifically, we consider the following model:

$$y_{i(hrsj)t} = \beta CAT_r \times Post_t + \delta_{sh} + \mu_{st} + \theta_j + \epsilon_{i(hrsj)t}, \tag{1}$$

where the outcome variable, $y_{i(hrsj)t}$, is the MIR score, our proxy of human capital, obtained by individual i, doing the residence in hospital h (within region r), specializing in specialty s, and who takes the MIR exam in year t. Subindex j denotes the university in which individual i obtained the medical degree. We construct our treatment variable as the product of the dummies CAT_r and $Post_t$, where CAT_r takes the value one for all individuals doing their residence in hospitals located in Catalonia and 0 otherwise, and $Post_t$ refers to years from 2018 onwards.[32] Hence, the control group is composed of all those individuals that take the MIR exam and start doing their residence in regions different from Catalonia.

To ensure that our regressions capture residual variation unexplained by time-invariant characteristics of hospitals and specialties, our most saturated specification incorporates a comprehensive set of fixed effects. Specifically, specialty-hospital fixed effects (δ_{sh}) capture hospitals' reputation within specialties[33], while specialty-year fixed effects (μ_{st}) account for specialty-specific time trends (for example, the rise in popularity of dermatology). Additionally, university fixed effects (θ_j) absorb time-invariant university characteristics. As a robustness check, some specifications also include individual-level controls and regional variation in positions offered. This set of control variables includes dummies for gender and foreign status, as well as the log number of positions available at the province and regional levels.[34]

The inclusion of supply variables as covariates is less prone to simultaneity bias in the MIR system since the number of available positions (supply) is determined before the exam, ensuring that demand is independently established afterward. To address potential concerns about bad controls (Angrist and Pischke 2009), we verify that the Catalan government

32. The reason for choosing 2018 as the first year under treatment lies in the fact that while the MIR exam was held on January 28, 2017, the main secessionist events did not take place until the fourth quarter of that year. Hence, we do not consider that students who took the MIR in early 2017 were affected by the shock.

33. Within the same hospital, there can be substantial variation in the levels of human capital across specialties. For instance, at Hospital 12 de Octubre, in Madrid, the Oncology specialty receives an average standardized Score (standardized by year and specialty) of 1.2, while Allergy receives an average Score of -0.44, notably below the mean, which is zero by design.

34. With province, we refer to *provincias*, that is, NUTS 3 regions according to the European Union standards. With region, we refer to *comunidades autónomas*, that is, NUTS 2 regions.

did not dynamically adjust the supply of positions in response to the 2017 events. Any unobserved variation is captured by the error term $\epsilon_{i(hrsj)t}$. We employ cluster-robust standard errors at region level, which is the treatment-level unit (Bertrand et al. 2004), but given the low number of clusters, we also report the p-value obtained using wild bootstrap procedures (Cameron et al. 2008) as a robustness check.

Importantly, the MIR system operates as a closed economy, meaning that any change in one region is fully transmitted to the rest. As a consequence, our empirical strategy systematically violates the stable unit treatment value assumption (SUTVA) (Rubin 1980) due to equilibrium effects driven by the re-sorting of individuals across treatment and control regions. This violation implies that our estimates do not capture the average treatment effect but rather reflect the relative effect between treated and control regions (Sobel 2006).[35] That is, they capture both the impact of the shock on Catalonia and its effects on the rest of Spain, leading to an upward bias relative to the average treatment effect. Arguably, in our specific setting, the economically relevant parameter is the relative effect, rather than the average treatment effect. As long as MIR score patterns in Catalonia and the rest of Spain would have followed the same time trend in the absence of the 2017 events, our coefficient of interest, β, captures the relative effect of political polarization on average MIR scores, our measure of human capital, between Catalonia and the rest of Spanish regions.

In addition to our difference-in-differences specification, we employ the following event study model to partially validate the parallel trends assumption and to examine the dynamic evolution of relative outcomes. For this part of the analysis, the model we estimate is specified as follows:

$$y_{i(hrsj)t} = CAT_r \times \sum_{\substack{y=2012 \\ y\neq 2017}}^{2024} \beta_y \mathbb{I}[t = y] + \delta_{sh} + \mu_{st} + \theta_j + \epsilon_{i(hrsj)t} \tag{2}$$

where $\mathbb{I}[t = y]$ are year dummies, with 2017, that is, the year prior to the shock, as the baseline (omitted) year. Thus, each estimate of β_y quantifies the differential change in the outcome variable between treatment and control groups, relative to 2017. All other variables are defined as above.

35. Given the substantial size of the treatment group (around 15% of the sample), spillovers are likely to affect all control regions, making strategies aimed at eliminating them, such as the "ring approach", ineffective (Alves et al. 2023).

4.1.2 Hospital-level Specifications

Our previous specifications rely on cross-sectional data on individual choices. To complement this analysis, we construct a balanced panel and aggregate the data at the hospital-specialty level, the most granular geographic unit available. We then estimate the causal effect of increased political polarization on hospital performance using a synthetic difference-in-differences (SDID) research design (Arkhangelsky et al. 2021).

This estimator, which combines features of the difference-in-differences (DID) and synthetic control (SC) methods within a unified least-squares framework, offers several advantages over either approach in isolation. First, relative to DID, SDID relaxes the reliance on the parallel trends assumption by constructing weights that optimally align pretreatment trends, in the spirit of the SC method. Second, compared to SC, SDID minimizes differences in pretreatment trends rather than levels, thereby mitigating bias under imperfect fit (Ferman and Pinto 2021). Moreover, the inclusion of time and unit fixed effects, along with the structured weighting procedure, enhances statistical power while simultaneously reducing researcher degrees of freedom in selecting control variables.

Specifically, the estimator proceeds in two steps. First, it determines those unit weights ($\hat{\omega}_r^{sdid}$) that optimally align pre-exposure outcome trends between unexposed and exposed units. Simultaneously, it selects time weights ($\hat{\lambda}_t^{sdid}$) to balance pre- and post-exposure periods. Intuitively, these time weights downweight pre-treatment periods that exhibit unusual patterns relative to the post-treatment period in control regions. Second, once computed these weights, the estimator runs a weighted two-way fixed effects regression to estimate the average causal effect of political polarization on hospital performance, captured by τ:

$$\left(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}\right) = \arg \min_{\tau,\mu,\alpha,\beta} \left\{ \sum_{r=1}^{N} \sum_{t=1}^{T} (Y_{rt} - \mu - \alpha_r - \beta_t - \tau CAT_r \times Post_t)^2 \, \hat{\omega}_r^{sdid} \hat{\lambda}_t^{sdid} \right\} \tag{3}$$

For statistical inference, we compute standard errors using the block boostrap method derived in Arkhangelsky et al. (2021) and implemented by Clarke et al. (2023).

4.2 Hospital Performance

4.2.1 Patient-level Specifications

In the next stage of our analysis, we examine whether the impact of political polarization on the regional distribution of human capital translates into effects on hospital performance. To estimate this effect, we employ a difference-in-differences strategy similar to that developed above. That is, we compare hospital performance in Catalonia with the rest of Spain, before and after 2017. Specifically, we consider the following model:

$$y_{irt} = \beta CAT_r \times Post_t + \mu_{irt} + \epsilon_{irt}, \tag{4}$$

where y_{irt} denotes a hospital performance variable, registered for individual i, hospitalized in region r in year t. CAT_r and $Post_t$, as in Equation 1, are dummy variables that refer to individuals located in Catalonia, and to years from 2018 onwards, respectively. The parameter μ_{irt} represents a vector of fixed effects designed to isolate the impact of political polarization on hospital performance. We include time fixed effects at the year-month level to account for nationwide temporal patterns, such as seasonal fluctuations in health outcomes. Diagnostic-province fixed effects capture persistent regional differences in diagnostic practices, reflecting variation in local medical norms and resource availability. Diagnostic-year fixed effects control for temporal shifts in treatment protocols and medical technology specific to each diagnosis. Age-gender fixed effects absorb individual characteristics that may systematically influence health outcomes. ϵ_{irt} denotes the error term. We weight each observation by its survey expansion factor, which adjusts the sample to represent the full population. Standard errors are clustered at region level, which is the treatment-level unit.

To partially validate the parallel trends assumption, we expand the above specification as follows:

$$y_{irt} = \beta CAT_r \times \sum_{\substack{y=2010 \\ y \neq 2017}}^{2019} \beta_y \mathbb{I}[t = y] + \mu_{irt} + \epsilon_{irt}, \tag{5}$$

where $\mathbb{I}[t = y]$ are year dummies, with 2017 as the baseline (omitted) year. Thus, each estimate of β_y quantifies the differential change in the outcome variable between individuals hospitalized in Catalonia and in the rest of Spain, relative to 2017. The remaining variables are the same as in Equation 4.

4.2.2 Province-level Specifications

We complement our analysis by constructing a balanced panel and aggregating the data at the province level, our most granular geographic unit available. We then estimate the causal effect of increased political polarization on hospital performance using a synthetic difference-in-differences (SDID) research design. Importantly, in these province-level regressions, it becomes necessary to apply risk adjustments to our aggregated health outcome variables, *Death(A)* and *LoS(A)*. To do so, we follow the UK's National Health Service (Health and Social Care Information Centre 2015). Specifically, we construct the risk-adjusted mortality rate (*Death(RA)*) as follows. First, using patient-level data and restricting the sample to the pre-treatment period, we estimate mortality risk by fitting a logit model on patient characteristics, including diagnosis, age, and gender. Second, we predict individual mortality probabilities and aggregate these predictions at the province level. Finally, we define the risk-adjusted mortality rate as the ratio of the actual province-level death rate to the predicted death rate. Under this definition, an increase (decrease) in the ratio above 1 indicates higher (lower) mortality than expected. Likewise, the risk-adjusted length of stay (*LoS(RA)*) is constructed using the same procedure, but with an OLS model instead of a logit model.

5 Human Capital Effects

In this section, we estimate the relative effect of rising political polarization on human capital. We then explore heterogeneous effects and regional spillovers, along with the mechanism behind these effects. Finally, we assess the robustness of our findings through a battery of complementary exercises.

5.1 Main Results

We begin our analysis by estimating the difference-in-differences specification described in Equation 1, which aims to determine whether human capital in Catalonia decreased relative to the rest of Spain following the political polarization events of late 2017. Table 1 presents the baseline estimates at the individual level. We proceed with a step-by-step analysis where each subsequent specification adds progressively more controls, ending with our most saturated specification. In all specifications, the estimated interaction coefficient, β, remains negative, statistically significant at the 1% level and economically relevant.

Starting with the simplest specification, without controls, column (1) reports an estimated coefficient of -0.162. As our dependent variable is standardized by year, adding year fixed effects in column (2) does not change the coefficient. Column (3) introduces specialty fixed effects, causing the coefficient to decrease to -0.127 and substantially increasing the R^2 from 0.001 to 0.652, showing the relevance that choosing certain specialties have for MIR students. Columns (4)-(6) progressively add location fixed effects of increasing granularity to control for some inherent and time independent local factor that might affect choices. In columns (7) and (8), we fully saturate the specification with Hospital×Specialty and Year×Specialty fixed effects, to control for reputation of some specialties in some hospitals or changes in the attractiveness of certain specialties through time, yielding an estimated coefficient of -0.125, very close to the one in column (3). Finally, column (9), our preferred specification, includes university fixed effects, leading to an estimated coefficient of -0.118. It is worth noting that as the R^2 increases by 83 percentage points across specifications, the coefficient of interest remains relatively stable, shifting from -0.162 in column (1), to -0.118 in column (9). This stability suggests that the correlation between the treatment and unobserved time-varying characteristics is low, reducing concerns about omitted variable bias (Altonji et al. 2005; Oster 2019). Table A5 follows the same structure as Table 1, but uses *Percentile* as outcome variable. In this specification, our estimated coefficient indicates a decline of -3.845 percentiles relative to a pre-treatment average of 53.415, implying a 7.2% reduction in the seven years after the shock.[36] We finish this first set of results by constructing balanced panels with different levels of granularity, ranging from region-specialty to hospital-specialty. Table 2 presents the results. In columns (1)-(4), the estimator used is OLS; in columns (5)-(8), the estimator used is the synthetic difference-in-differences.[37] Regardless of the unit of observation chosen, the effect reported is negative and significant across all columns. In our preferred specifications, columns (4) and (8), the relative effect reported is -0.14.

36. To contextualize the magnitude of our findings, the overall learning deficit due to school closures during the COVID-19 pandemic is estimated at approximately -0.14σ (Betthäuser et al. 2023). Likewise, Chetty et al. (2014) documents that a one standard deviation increase in teacher value added raises student scores by 0.14σ in mathematics and 0.1σ in English.

37. By construction, the synthetic difference-in-differences estimator assigns a weight to each observational unit. We aggregate these weights at the province-specialty level and plot their distribution. Figure A4 presents the resulting weight matrix, with weights expressed as percentages of the total. While no specialty receives markedly more weight than others, this is not the case for provinces, with only Madrid accounting for 22.7% of the total. Nevertheless, the distribution remains relatively dispersed, in comparison to the more concentrated weights typically produced by the synthetic control method (Abadie and Gardeazabal 2003).

To provide evidence that the effects presented so far are due to the increase in political polarization driven by the secessionist shock, in Figure 5 we report our year-by-year β estimates, both at the individual and hospital-specialty levels. Between 2012 and 2016, the estimated coefficients tend to be insignificant and centered around zero. That is, Catalonia and the rest of Spain did not show systematically different trends in terms of *Score* with respect to 2017, thus providing support for the parallel trends assumption. Following the political polarization shock, we document a significant decrease in 2018 in the average *Score* in Catalonia compared to the rest of Spain. This effect stabilizes around -0.15 up to 2024, seven years after the shock. The persistence of this effect is in line with a sustained change in the population's perception of the level of political polarization in Catalonia. Taken together, these findings suggest that the shock led to a relative decline in the human capital of MIR residents in Catalan hospitals and that this effect is long lasting.

5.2 Heterogeneity

Next, we exploit two sources of variation to explore whether the effect exhibits heterogeneity, either based on the characteristics of the population where the residency hospitals are located or on the characteristics of the MIR candidates themselves.

Location characteristics.— We explore whether the intensity of the effect changes with regional variation in the intensity of secessionism, our source of political polarization. Arguably, if political polarization negatively affects individuals' utility, we expect the effect to be greater in areas with higher secessionist sentiment. In order to capture regional variation in secessionist intensity, we use as a proxy of political polarization the percentage of votes for secessionist parties (CUP, ERC, Junts x Cat and Diàleg) in the December 2017 regional elections at the town level. Figure A5 shows that the variation in the percentage of pro-independence votes across towns is considerable, ranging from 18% to 97%. When focusing uniquely on towns with MIR hospitals (in the map, identified with red dots), the variation remains substantial, with a mean of 0.44 and a standard deviation of 0.14. We examine whether the late-2017 events had a greater impact in municipalities with higher levels of pro-independence voting by estimating the following triple difference-in-differences model:

$$y_{i(hrsj)t} = \beta_1 CAT_r \times Post_t \times Votes_h + \beta_2 CAT_r \times Post_t + \delta_{sh} + \mu_{st} + \epsilon_{i(hrsj)t}, \quad (6)$$

where all variables are defined as in Equation 1, with the addition of *Votes*, a continuous variable capturing the percentage of pro-independence votes at the town level. The baseline effect of the late-2017 shock is reflected in the estimate of β_2, while β_3, our triple interaction term, identifies whether this effect changes with the percentage of pro-independence votes. The remaining variables and interactions in the model are absorbed by the fixed effects.

The results of this exercise are presented in Table 3.[38] As in Table 1, we gradually add fixed effects, ending with our most saturated specification. Column 9 reveals that the average effect of the shock on Score in Catalonia (β_2) remains negative and significant at the 1% level. More notably, the estimate of β_3 is also negative and highly significant, indicating that the effect intensifies in areas with a higher share of pro-independence votes. Specifically, with an estimate of -0.074, moving from a town at the 25th percentile of pro-independence votes (32.8%) to one at the 75th percentile (55.5%) is associated with an absolute increase in the effect close to 0.02 ($0.074 \times (0.555 - 0.328)$). This finding suggests that the impact of the shock is sensitive to the intensity of local secessionist sentiment. Although this finding does not entirely rule out the alternative explanation that individuals avoid Catalonia as a place of residence solely due to fears of potential independence, it does indicate that this factor alone cannot fully account for the observed effect. Otherwise, if the results were driven by such a mechanism, and assuming that the prospect of independence would affect all Catalan localities equally, the effect should manifest uniformly across municipalities. However, the geographical heterogeneity in the results contradicts this interpretation.

Individual characteristics.— In terms of heterogeneity of the effect according to individual characteristics, we explore whether the results vary over the distribution of the outcome variable, *Score*. To do so, we re-estimate Equation 1 using quantile regressions with the method of moments developed by Machado and Silva (2019). Table 4 presents the estimates for the quantiles 0.05, 0.25, 0.5, 20.75 and 0.95, and Figure 6 displays the estimates for the whole distribution. While the effect of the shock is negative and statistically significant across the entire *Score* distribution, it intensifies monotonically as we move from the bottom performers to the top performers. The estimated coefficient at the 5th percentile is -0.09 and increases to -0.159 among the top 5% performers. This result suggests that the MIR candidates whose choices were most affected were those with the highest scores, which by design are those with the greatest outside options.

38. Given that judicial decisions may have an impact on political attitudes (Casas et al. 2024), in Table A6 we replicate the same exercise using pre-treatment voting data from the September 2015 regional elections in Catalonia. The results, however, remain virtually unchanged.

5.3 Mechanism

So far, we have documented a decrease in the average level of human capital allocated to Catalonia after the shock. This result indicates that students with higher levels of *Score*, our proxy for human capital, were replaced by those with lower *Score*. A natural question that arises is whether this shift is driven by sorting based on ideological affinity, or if it is instead the result of neutral individuals moving away from a polarized environment, —what we refer to hereafter as the disutility channel. In this subsection, we aim to disentangle the mechanism behind the observed decrease in human capital.

To this end, ideally, we would access a variable capturing individual-level ideological polarization around secessionism. However, since we do not have this variable, we use the geographic location of the university where individuals graduated as a proxy for their likelihood of supporting secessionism. Specifically, for individuals graduated from universities located in Catalonia, we assume they are more likely to be Catalan and, consequently, more likely to support secessionism than students from other regions. Following this reasoning, we divide the dataset into three mutually exclusive groups: (i) individuals coming from foreign universities, which we identify with the dummy variable *International*; (ii) those candidates doing their residency in the same region as their university, identified by the variable *Local*; and (iii) those completing their residency in a region different from their university of graduation, identified by *Other Region*.

Under these definitions, we analyze two distinct margins of impact. First, we assess whether political polarization affected differently MIR scores across groups, which we refer to as the intensive margin. Second, we examine whether the shock altered the composition of these groups in terms of their propensity to select Catalonia for their residency, capturing what we define as the extensive margin. Regarding the intensive margin, through which we aim to disentangle the mechanism driving our results, the specification is as follows:

$$\begin{aligned} y_{i(hrsj)t} = {} & \beta_1 CAT_r \times Post_t \times Local_j + \beta_2 CAT_r \times Post_t \times OtherRegion_j \\ & + \beta_3 CAT_r \times Post_t + \delta_{sh} + \mu_{st} + \theta_j + \epsilon_{i(hrsj)t}, \end{aligned} \tag{7}$$

If ideological affinity sorting is the dominant mechanism, the decline in *Score* should be primarily driven by individuals from outside Cataloni. That is, we should observe $\beta_1 < \beta_2$. Conversely, if the decrease is similar for both Catalan and non-Catalan students, we interpret this as suggestive evidence in favor of the disutility channel as main driver of the effect. Table 5 summarizes our results. In columns (1)-(4), we estimate the specification from Equation 1, first for each subgroup separately and then for the entire dataset, which corresponds to Equation 7. For all subgroups, the shock was negative and statistically significant at the 1% level. However, our results indicate that the shock affected the different subgroups heterogeneously. Specifically, the least affected group was that of international candidates (-0.064σ). This finding is consistent with a utility function less responsive to local political issues compared to that of national candidates. The remaining candidates, those from local universities and those from universities located in other regions, experienced estimated effects of -0.136σ and -0.124σ, respectively, although the difference between these coefficients is not statistically significant (column (4)). We interpret this result as supportive evidence of the disutility channel.

Next, in columns (5)-(7), we further examine the extensive margin by regressing each location-based dummy on our treatment variable.[39] Consistent with our findings on the intensive margin, the results suggest that, following the shock, the proportion of MIR residents trained at foreign universities increased, primarily at the expense of interregional students. Specifically, we estimate a 1.8 percentage point increase in the probability that a MIR resident in Catalonia graduated from a foreign university, representing a 7.6% rise relative to the pre-treatment mean. Taken together, these findings suggest that the surge in political polarization led to compositional shifts not only within groups (intensive margin) but also across them (extensive margin).

5.4 Regional Spillovers

We now examine which regions may have indirectly benefited from the political polarization shock. To this end, we re-estimate our baseline specification at the hospital-specialty level, sequentially assigning treatment status to each region in the control group, while continuing to exclude Catalonia from the sample. To minimize the risk of capturing unrelated shocks, we restrict the analysis to the two years following the polarization shock. Figure 7 presents event study estimates for the only region where we detect a statistically significant positive effect, Valencia, with a relative increase of 0.096σ (p-value$<$0.01). Figure A6 displays the

39. Specifically, in columns (5)-(7) we estimate:

$$y_{i(hrsj)t} = \beta CAT_r \times Post_t + \delta_{sh} + \mu_{st} + \epsilon_{i(hrsj)t},$$

where $y_{i(hrsj)t}$ is *International*, *Local* and *Other Region*, respectively.

estimated effects for all regions

5.5 Robustness

Having established the effect of an increase in political polarization on the distribution of human capital, we next examine the robustness of this relationship. We include a battery of exercises that take into account a host of other possible concerns related to (i) alternative explanations driving the results (ii) sample decisions; (iii) confounding variables driving the effects; (iv) the level at which standard errors are clustered; (v) violation of the parallel trends assumption; (vi) effects due to other events happening before or after october 2017.

Alternative explanation.— So far, we have argued that the decline in scores following the late 2017 shock is due to a decrease in the preferences for students to do their residence in the treated region. However, an alternative explanation for this narrative is that the drop in scores may reflect the impact of social unrest in Catalonia in 2017 on the academic performance of students located in the region. This disruption in their studies may have subsequently resulted in poorer performance on the MIR exam. By leveraging data on the university of study for each candidate, we can explore this alternative explanation. In Table A7, using a differences-in-differences specification, we compare the performance of students from Catalan universities with those from the rest of Spain, before and after the shock, regardless of whether they ultimately pursue their residency in Catalonia.[40] Whether we use the MIR exam score or the total score as the outcome variable, our estimated coefficient, while negative, is far from being statistically significant at conventional level. Moreover, its economic magnitude is much smaller that the observed results in final choices. Therefore, we find no evidence consistent with this alternative explanation.

Sample decisions.— As previously highlighted, given the closed-economy nature of the MIR system, declines in scores in Catalonia must mechanically translate into increases in other regions. This inherent feature raises concerns about potential violations of the SUTVA assumption. While addressing this bias is not strictly necessary as long as we interpret our estimates as relative effects, it is nonetheless informative to assess how far this relative effect deviates from the average treatment effect that would be identified in the absence of SUTVA

40. Specifically, we estimate:

$$y_{ijt} = \beta Cat\_Uni_r \times Post_t + \delta_j + \mu_t + \epsilon_{ijt},$$

where Cat_Uni_j is a dummy variable that takes the value one for MIR candidates from universities located in Catalonia. δ_j and μ_t denote university and year fixed effects. Standard errors are clustered at university level.

violations. To do so, we exploit the fact that regions differ in their indirect exposure to the shock through MIR resident flows. As illustrated in Figure A7, during the pre-treatment period, the five regions receiving the largest inflows from Catalonia accounted for 71.8% of all such movements, while the five regions sending the largest outflows to Catalonia represented 67.6%. To account for this uneven cross-regional elasticity, we re-estimate Equation 1, progressively excluding from the analysis the regions most likely to absorb the shock. Table A8 presents the estimated coefficients after sequentially removing the five regions with the largest MIR inflows from and outflows to Catalonia. Despite the substantial reduction in sample size, the estimated effect remains remarkably stable, ranging between -0.103 and -0.115. Furthermore, event-study analyses based on these alternative control group definitions continue to support the parallel trends assumption (Figure A8 and Figure A9). Similarly, iteratively excluding individual regions from the control group does not alter the magnitude or direction of the estimated treatment effect (Figure A10). This consistency suggests that the relative effect we document may arguably be a close approximation of the average treatment effect that would be identified in the absence of SUTVA violations.

Next, we show that our baseline results are not driven by particular specialties. Figure A11 plots the estimated coefficients and their associated 95% confidence intervals as we progressively exclude from the sample those specialties offering at least 100 positions per year. The estimates remain remarkably stable, ranging between -0.121 and -0.110. An exception arises when the specialty of Family Medicine is excluded, where the estimated coefficient increases (in absolute terms) to -0.148. This increase in the coefficient likely reflects the introduction of a monetary incentive program for Family Medicine residents by the *Generalitat* in 2022, which provided bonuses of up to 47% of residents' wages.

Confounding variables.— To rule out concerns about potential confounding factors driving the reported effects, we augment our preferred specification with a vector of time-varying control variables. Specifically, we include, as supply-side proxies, the (log) number of positions offered at the province and regional levels. Additionally, we control for individual characteristics such as gender and foreign status. Furthermore, we incorporate two key factors that influence the spatial sorting of workers, which were omitted from the baseline model: resident wages and rental prices. Importantly, net wages for MIR residents are determined at the regional level and include estimated compensation for on-call shifts. Rental prices are defined at the local level. Table A9 presents the results of progressively adding these control variables to our baseline specification. It is important to note that as wage data is only available for the years 2012 and 2018–2022, we restrict the sample to these years to maintain consistency across specifications. Column (1) replicates our baseline specification without covariates. Interestingly, despite the significantly reduced sample size, the estimate remains nearly identical to that in Column (9) of Table 1. As for the inclusion of covariates, the estimated coefficient remains highly stable across specifications, reaching -0.127 in Column (7) when all covariates are included in the model.

Clustering decisions.— Next, to address concerns about the robustness of our baseline results to clustering standard errors at different levels, Table A10 presents the baseline estimates with standard errors clustered at the region, province, and municipality levels. Furthermore, given that in our baseline specification the number of observations per cluster is uneven and the number of clusters is relatively small, we report the p-value obtained using wild bootstrap procedures (Cameron et al. 2008). While the size of the standard errors increases as we cluster at more granular levels, the estimated coefficient remains statistically significant at the 99% confidence level. The p-value from the wild bootstrap procedure also reaffirms the statistical significance of our baseline effect. To assess the likelihood that our estimates are false positives, we conduct randomization inference. Specifically, we randomly assign treatment groups of the same size as the original treatment group (N=14,119) and repeat this process 500 times. Figure A12 plots all resulting estimates along with their confidence intervals. Compared to our baseline estimate, only 7% of the random allocations are statistically significant at the 5% level and the magnitude of the estimates obtained is substantially smaller.

Parallel trends violation.— Given the critical importance of the parallel trends assumption in our empirical strategy, we evaluate the sensitivity of our results to potential pre-treatment trend deviations. Following the methodology proposed by Rambachan and Roth (2023), we present the results of this check in Figure A13. Reassuringly, the effects remain statistically significant at the 95% confidence level even when allowing for deviations from linear trends up to 0.8 times the largest deviations observed in the pre-treatment period.

Placebo starting years.— Finally, it is unlikely that our effect is driven by other events occurring before or after October 2017, as our baseline difference-in-differences estimate is larger than all possible placebo estimates generated by assigning fake event starting years (Figure A14).

6 Hospital Performance Effects

In this section, we first present our results regarding the effect of political polarization on health care outcomes. We then perform a set of robustness analyses of our main result, as well as study different alternative explanations to our proposed explanation: the decline in human capital documented in the previous section.

6.1 Main Results

We now examine whether, consistent with the human capital drop documented in section 5, the increase in political polarization had an impact on patient health outcomes. As highlighted in prior research, human capital in the healthcare sector plays a critical role in determining the quality of care (Doyle Jr et al. 2010; Currie and MacLeod 2017; Chen 2021; Chan et al. 2022). Consequently, we expect poorer health outcomes in Catalonia after the shock, relative to the rest of Spain.

Table 6 presents the results of this analysis at the patient level. In Panel A, the outcome variable is *Death*, a binary indicator equal to 1 if the individual dies and 0 otherwise. Across specifications, from column (1) with no fixed effects to column (5), our most saturated model, the estimated interaction coefficient remains stable. It is positive, statistically significant, and economically meaningful. Using the point estimates, individuals exposed to the shock face a 0.6 percentage point increase in mortality, an 8% rise relative to the pre-treatment mean. Given approximately 52,000 patients annually in Catalonia, the effect size we obtain implies an average of 306 additional deaths per year. Using a Spain-specific value of statistical life of €2 million (Abellán et al. 2024), our back-of-the-envelope calculation suggests annual mortality costs of €612 million. Table 6, Panel B presents the results of a Poisson model where the outcome variable is *LoS*, measured as the number of days spent in the hospital before discharge. Along similar lines, the results are consistently positive, statistically significant, and highly stable across specifications. In our most saturated model (column 5), the estimated coefficient suggests that the shock increased the length of stay by approximately 3.7% (exp($\hat{\beta}$)-1, where exp($\hat{\beta}$) corresponds to the incidence rate ratio of *LoS*).[41]

41. Recall that to account for the count nature of *LoS*, Equation 4 is modified as follows:

$$y_{irt} = exp(\beta CAT_r \times Post_t + \mu_{irt})\epsilon_{irt},$$

For further details on how to implement the Poisson estimator in a difference-in-differences framework, see Wooldridge (2023).

Next, in Table 7, we present the results using patient data aggregated at the province level. Columns (1) and (4) estimate the effect of the shock on the death rate and average length of stay using OLS. To relax the parallel trends assumption, columns (2)–(6) implement the synthetic difference-in-differences estimator. Specifically, columns (2) and (5) estimate the effect on *Death(A)* and *LoS(A)*, while columns (3) and (6) use the risk-adjusted mortality rate (*Death(RA)*) and risk-adjusted length of stay (*LoS(RA)*) as outcomes. Across all specifications, Table 7 indicates a deterioration in health outcomes in Catalonia following the political polarization shock. Our preferred estimates (columns (2) and (6)) suggest that the risk-adjusted mortality rate and the average risk-adjusted length of stay rose by 8.7 and 2.2 percentage points, respectively, corresponding to increases of 9.4% and 2.4% relative to the pre-treatment mean. These estimates closely align with our patient-level findings.

The validity of our approach relies on the parallel trends assumption, which we assess by plotting the dynamic effects for the synthetic difference-in-differences regressions estimated in Table 7.[42] Panels A and C of Figure 8 examine the parallel trends when *Death(A)* and *Death(RA)* are the outcome variables, respectively. Before the shock, estimates remain close to zero and statistically insignificant, but they gradually turn positive and significant afterward. Panels B and D show a similar pattern for *LoS(A)* and *LoS(RA)*. Again, we detect no pre-trends, while length of stay increases following the increase in political polarization.[43] Taken together, these findings suggest that the parallel trends assumption holds in our setting.

Thus far, we have consistently documented the impact of the human capital shock on patient health outcomes in Catalan MIR hospitals. To contextualize our findings, we conclude this section by benchmarking the magnitude of our estimates against results from related studies in the literature. First, we compare our findings to the effects of improving the human capital of hospital top managers. Otero and Munoz (2022) analyze the introduction of a competitive selection system for hospital top managers in Chile, reporting an average decrease in hospital mortality of 7.4%. Other studies focus on specific diagnoses using in-hospital death rates as well as outcome variable. For example, Propper and Van Reenen (2010) find that a 10% increase in outside wages leads to a 7% rise in the heart attack death rate. Similarly, Bloom et al. (2015) document that the presence of one additional hospital

42. Figure A18 presents the parallel trends graphs for the patient-level specifications reported in Table 6, along with the difference-in-differences estimates at the province level.

43. In Figure A17, we assess whether the rise in *Death(RA)* and *LoS(RA)* stems from changes in the predicted score (the denominator of the ratio). Reassuringly, we find no such evidence.

in the neighborhood reduces heart attack death rates by 9.7%. Finally, in a recent study, Aghamolla et al. (2024) show that bank stress tests, acting as a credit supply shock to connected hospitals, result in a 9.8% increase in pneumonia death rates. While none of these studies is directly comparable to ours, the similarity in the magnitude of their estimates provides additional confidence in our results, suggesting that our findings fall within the typical range observed in the broader literature.

6.2 Alternative Explanations

Having established our main results, it should be noted that, while the findings on health outcomes are consistent with the proposed human capital reallocation mechanism, alternative explanations could also drive the observed increase in mortality rates. In this subsection we explore some alternative explanations classifying them into two broad groups: those derived from the demand for healthcare services and those originating from the supply of healthcare services. We do so by leveraging hospital-level data and estimating the following specification with a synthetic difference-in-differences estimator:

$$y_{h(r)t} = \beta_1 CAT_r \times Post_t + \beta_2 Beds_{h(r)t} + \delta_r + \mu_t + \epsilon_{h(r)t}, \tag{8}$$

where $y_{h(r)t}$ denotes an outcome, registered for hospital h, located in region r in year t. CAT_r and $Post_t$ are dummy variables that refer to hospitals located in Catalonia, and to years from 2018 onwards, respectively. $Beds_{h(r)t}$ is the logarithm of the number of available beds, which controls for hospital capacity (Finkelstein 2007; Acemoglu and Finkelstein 2008).[44]

Demand mechanisms.— One could argue that the social unrest generated at the end of 2017 in Catalonia may have led to both an increase in the demand for healthcare services and compositional changes in the patient profile. As a result, our findings might not indicate poorer hospital performance but rather the consequences of these demand shifts. Although our individual-level differences-in-differences approach partially mitigates this concern by comparing individuals diagnosed with the same conditions, one could still argue that the shock had a heterogeneous impact on hospital demand in Catalonia relative to hospitals in the control regions. In that case, the underlying mechanism driving our results might not be human capital reallocation but rather this alternative explanation. In Table A13, we examine

44. See Table A12 for descriptive statistics of the hospital-level variables used in this subsection and Figure A16 for the geographic distribution of hospitals.

whether there were any significant demand shifts around the event in both the total patient volume and the share of patients across different hospital areas. To do so, we compare hospitals in Catalonia with those in the rest of Spain, before and after the shock, estimating Equation 8. In column (1), the outcome variable is the logarithm of total admissions, while in columns (2) through (6), the outcomes are the logarithm of one plus the percentage of admissions in traumatology, surgery, internal medicine, palliative care, and intensive care, respectively. All reported coefficients are very close to zero and statistically insignificant at conventional levels, except for traumatology, which shows a negative effect. Taken together, these results suggest that changes in demand are unlikely to be the driving force behind our results, as we do not identify any clear effect across any of the specifications.

Supply mechanisms.— Another potential concern is that the supply of healthcare services, whether in terms of labor or physical capital, may have changed as a consequence of the shock. In particular, it is plausible that not only did residents with lower human capital move to Catalonia, but also that doctors already working in Catalonia might have decided to relocate to other regions due to the increase in social unrest. Under such circumstances, the effect we document on hospital performance would be driven not only by changes in the labor-intensive margin (human capital) but also by changes in the extensive margin (number of doctors). To assess the explanatory power of this alternative mechanism, we first obtain data from Spain's National Statistics Institute (INE) on the population of doctors at the provincial and regional levels, disaggregated by age groups. We then analyze whether there were significant changes in the number of doctors before and after the shock in Catalonia compared to the rest of Spain.[45] We conduct this exercise both for Catalonia as a whole and for each of its provinces. The resulting estimates from this test are presented in Table A14. For Catalonia overall, the number of doctors across different age groups does not evolve differently compared to the rest of Spain, except for the 55–64 age group, which increases. When disaggregated at the province level, out of 16 estimates, only one is negative, in Girona, for the 45–54 age group. Overall, the data does not suggest a significantly different trend in the doctor population, either in Catalonia as a whole or within its provinces,

45. Specifically, for each age group (below 35 years, 35–44, 45–54, and 55–64), we estimate the following model:

$$y_{pt} = \beta_1 CAT_p \times Post_t + \delta_p + \mu_t + \epsilon_t, \tag{9}$$

where the outcome variable y_{pt} is the logarithm of the number of doctors working in province p in year t. CAT_p is a dummy variable equal to one for the Catalonian province included in the subsample and zero otherwise. $Post_t$ refers to years from 2018 onwards. δ_p and μ_t are province and year fixed effects, respectively.

relative to the rest of Spain.

Next, we examine whether changes in the labor force at the extensive margin occurred at the hospital level. To this end, we estimate Equation 8 using as outcome variables the logarithm of the number of professionals in different categories (doctors, nurses and technicians). The results are presented in Table A15 and, once again, we find no evidence that Catalonia's hospitals experienced a net aggregate loss of professionals following the shock. We conclude exploring whether the events of late 2017 could have led to a reallocation of expenditures and investments, effectively triggering a shock in hospital spending and investments. In Table A16, we test this financial explanation. However, neither when comparing investment levels nor when analyzing expenditures on medical equipment do we obtain significant estimates, thus suggesting that financial mechanisms are also unlikely to explain our results.

Throughout this subsection, we have explored the plausibility of alternative mechanisms that could contribute to explain the documented increase in mortality rates in Catalan MIR hospitals since 2018, beyond the human capital allocation channel. We considered both demand-side and supply-side factors. On the demand side, we examined increases in admissions, as well as compositional changes in patient profiles. On the supply side, we evaluated changes in the extensive labor margin, as well as variations in investment and purchase levels. In none of our tests do we find evidence that these alternative mechanisms are able to compete with the explanatory power of the human capital channel.

6.3 Robustness

In addition to the alternative explanations ruled out above, we include a battery of exercises that take into account a host of other possible concerns related to (i) effects driven by a single diagnostic condition; (ii) patient selection; (iii) sample restrictions; (iv) inference; (v) frequency of data in the synthetic differences-in-differences design.

First, to ensure that the effect reported in our baseline regressions is not driven by a single diagnosis, we repeat the analysis separately for each condition. Table A17 presents the results of this exercise, showing that following the political polarization shock, health outcomes deteriorated across all diagnoses. Thus, the effects documented in Table 6 do not stem from a particular condition but rather reflect an overall decline in hospital quality.

Another challenge to our identification strategy is the possibility that our estimates reflect changes in patient composition. Although this concern is unlikely, both due to the characteristics of the diagnostic conditions analyzed and the built-in risk adjustment in our analysis, we leverage specific features of our data in Table A18 to further mitigate this issue. Thus we find that our baseline results (columns (1) and (4)) remain robust when: (i) we restrict the sample to emergency admissions (columns (2) and (5)); (ii) we limit the sample to individuals whose province of residence matches their province of hospitalization (columns (3) and (6)); and (iii) when using *LoS* as the outcome variable, we exclude patients who ultimately die from the sample (column (7)). Another potential concern regarding patient selection is that, due to the increase in political polarization, healthier individuals may have left the region, and this unobserved heterogeneity may not be fully captured by our fixed effects. To address this potential issue, in Table A19, columns (1) and (3), we re-estimate our baseline regressions using the subsample of patients aged 65 and older. This age group, which coincides with retired individuals, is the least likely to migrate across regions and is therefore less prone to experiencing a substantial compositional shift. Similarly, in columns (2) and (4), we restrict the sample to patients who stayed in the hospital for at least two days. This restriction aims to mitigate concerns that the effect may be driven by patients arriving in critical condition, where outcomes are determined by factors unrelated to hospital quality. Reassuringly, our results remain robust to these two sample restrictions.

We further examine whether our results hold after including the years affected by the COVID-19 pandemic (2020 and 2021), as well as 2022, the last year for which data is available. As reported in Table A20, the results remain consistent and, if anything, the estimated effect size increases for *LoS*. Additionally, in Table A21, we relax the short-term stay restriction by including patients hospitalized for more than 28 days, confirming the robustness of our results.

Next, in Table A22, we report the results clustering standard errors at the province level, as well as using wild bootstrap procedures. While clustering at the province level leaves standard errors largely unchanged for *Death* and increases them substantially for *LoS*, the results remain significant at the 99% level. In contrast, the p-values obtained with the wild bootstrap procedure are marginally above conventional significance thresholds. [46]

Finally, while synthetic difference-in-differences reduces researcher degrees of freedom, some discretion remains in selecting the temporal aggregation of the data. In our main analysis, we aggregate data at the yearly level to reduce variance and mitigate seasonality con-

46. For computational reasons, to estimate the p-value in column (4), we use OLS instead of a Poisson model.

cerns. However, in Table A23 we explore alternative temporal frequencies by aggregating results at the quarterly, semiannual, and monthly levels. For *Death Ratio* (columns (1)–(4)), although the effect size gradually decreases as frequency increases—eventually becoming insignificant at the monthly level—the coefficient remains positive across specifications. In contrast, when the outcome variable is *LoS Ratio* (columns (5)–(8)), both the effect size and statistical significance remain stable across all temporal frequencies.

7 Conclusion

The rise in political polarization is reshaping interactions among individuals with differing ideological views and may have far-reaching and multifaceted implications for the economy. One such consequence is the potential to influence individuals' mobility decisions. This phenomenon, known as spatial sorting, can alter the geographic distribution of human capital, with possible consequences for the productivity of human capital–intensive sectors. As polarization intensifies, understanding these dynamics becomes essential to assess its broader impact on economic performance.

This paper examines the consequences of political polarization for the geographic distribution of human capital in the healthcare sector. Exploiting an exogenous surge in polarization in Catalonia, a region of Spain, we first analyze how this shock affected the allocation decisions of newly graduated doctors, and then study its downstream impact on hospital performance in the affected region. Using a difference-in-differences framework, we isolate the effects of polarization on both the regional distribution of human capital and hospital outcomes, shedding light on the broader economic consequences of rising political polarization.

Our core findings highlight the significant consequences of rising political polarization. The region affected by the shock experienced a sustained decline in human capital that lasted at least seven years, with the most pronounced negative effects occurring in municipalities most exposed to the shock and for individuals with the best outside options. In terms of mechanisms, the spatial sorting implied from our results seems to be driven by neutral individuals avoiding polarized environments rather than by ideological affinity clustering. Consistent with the decline in human capital, we find that the affected region saw increases in both death rates and hospital length of stay. After ruling out a battery of alternative explanations, we interpret these results as evidence that the decline in human capital directly contributed to the reduced performance of hospitals.

Our findings may have far-reaching implications for the ongoing debate on the economic consequences of rising political polarization and highlight the important effects of human capital reallocation on welfare.

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Figures and Tables

Figure 1. Timeline of the MIR Selection Process

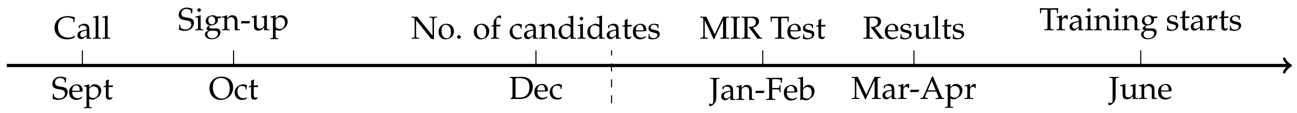
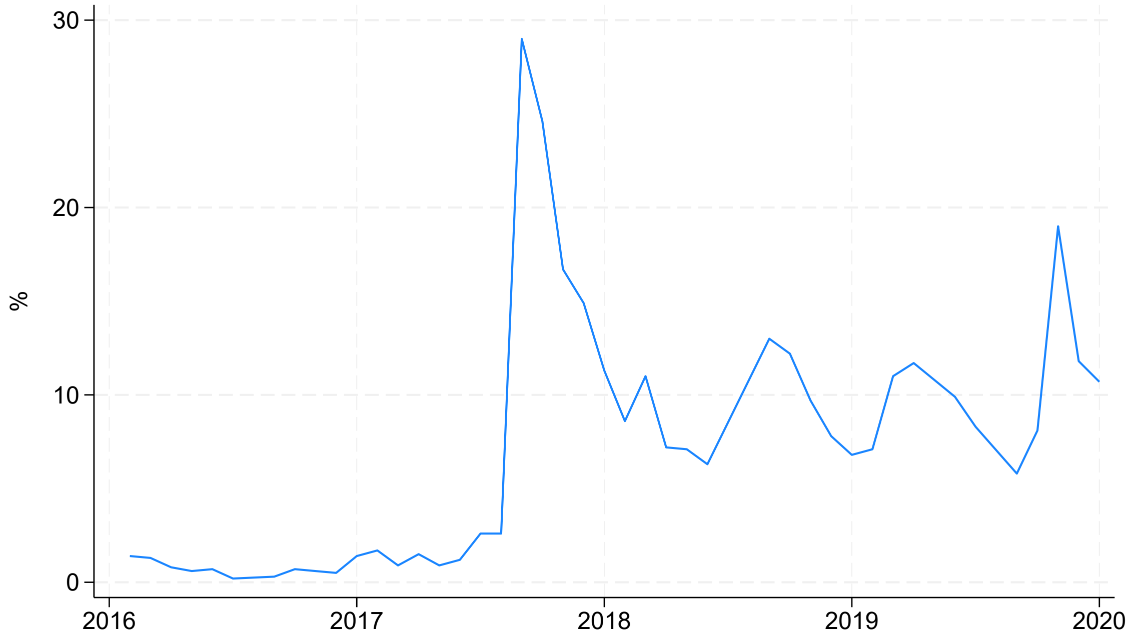
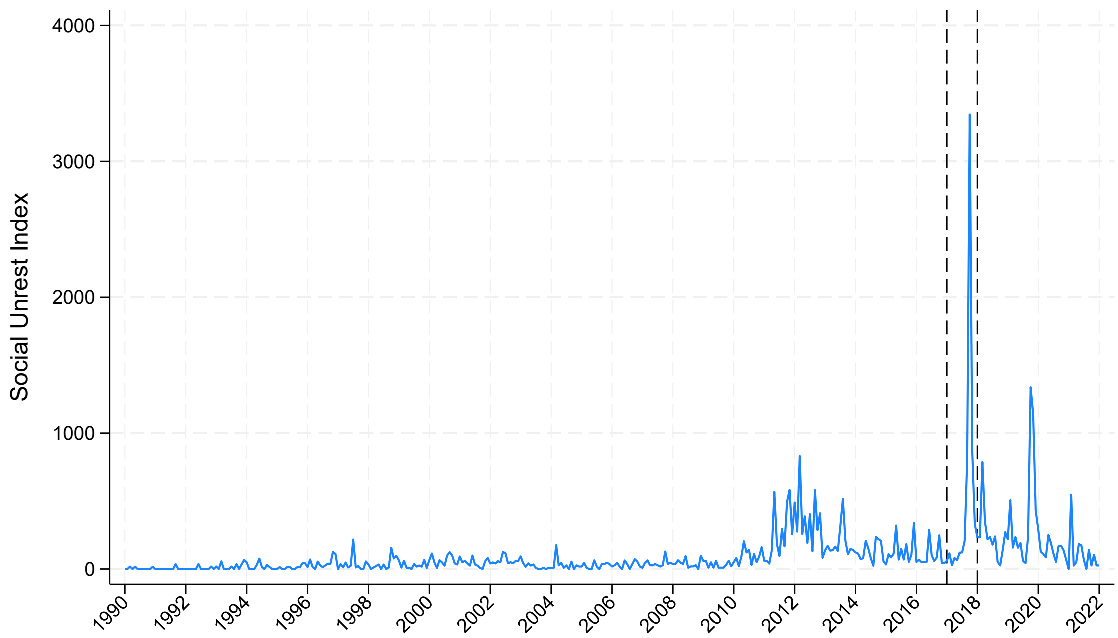


Figure 2. Time-series dynamics of social concern around the shock



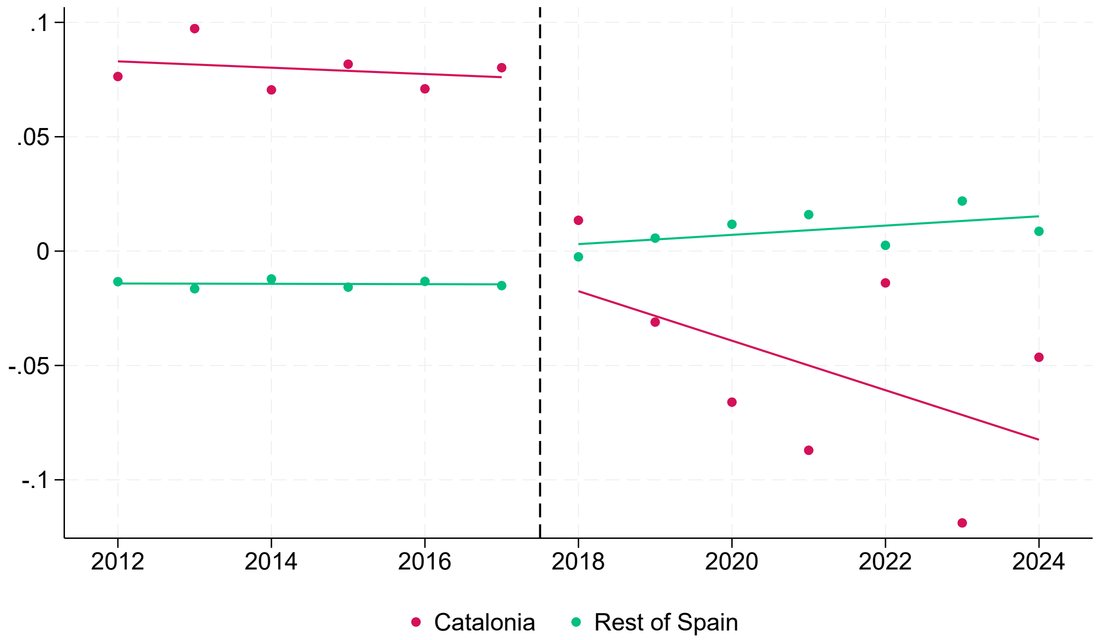
(a) Percentage of the population that considers Catalonia's independence one of the main problems in Spain



(b) Social unrest index in Spain

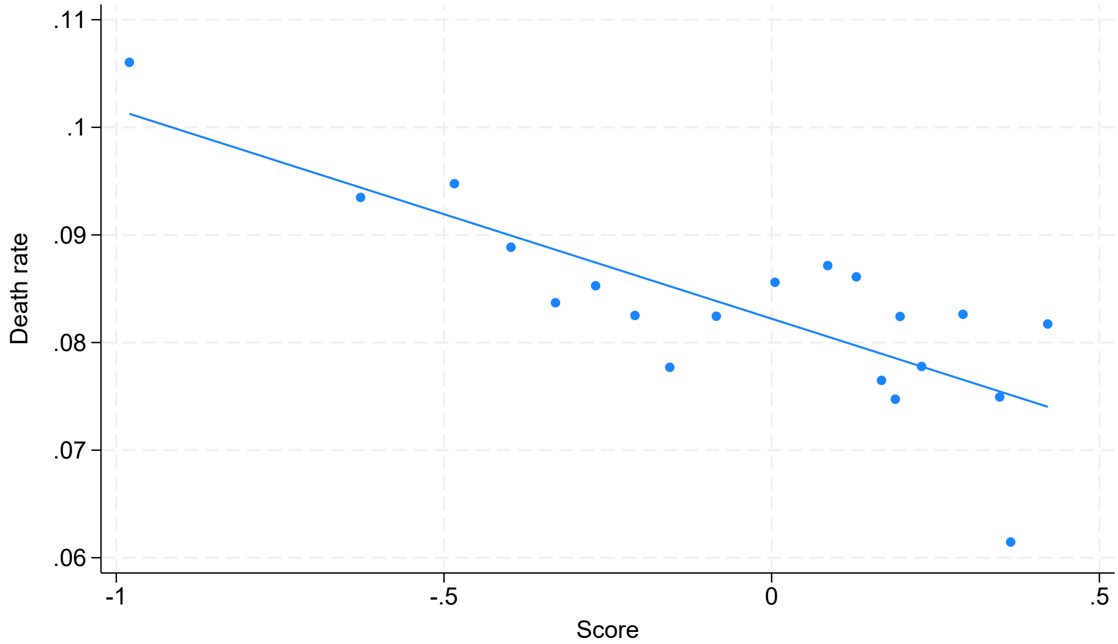
Notes: This figure presents the evolution of public concern and social unrest surrounding the 2017 Catalan secession crisis. Panel A plots the share of respondents identifying "The independence of Catalonia" as the main problem facing Spain, based on 33 nationally representative surveys conducted by the Center for Sociological Research. Panel B displays the Social Unrest Index for Spain, as developed by Barrett et al. (2022), over the period 1990–2022.

Figure 3. Average doctors' score, Catalonia vs Rest of Spain

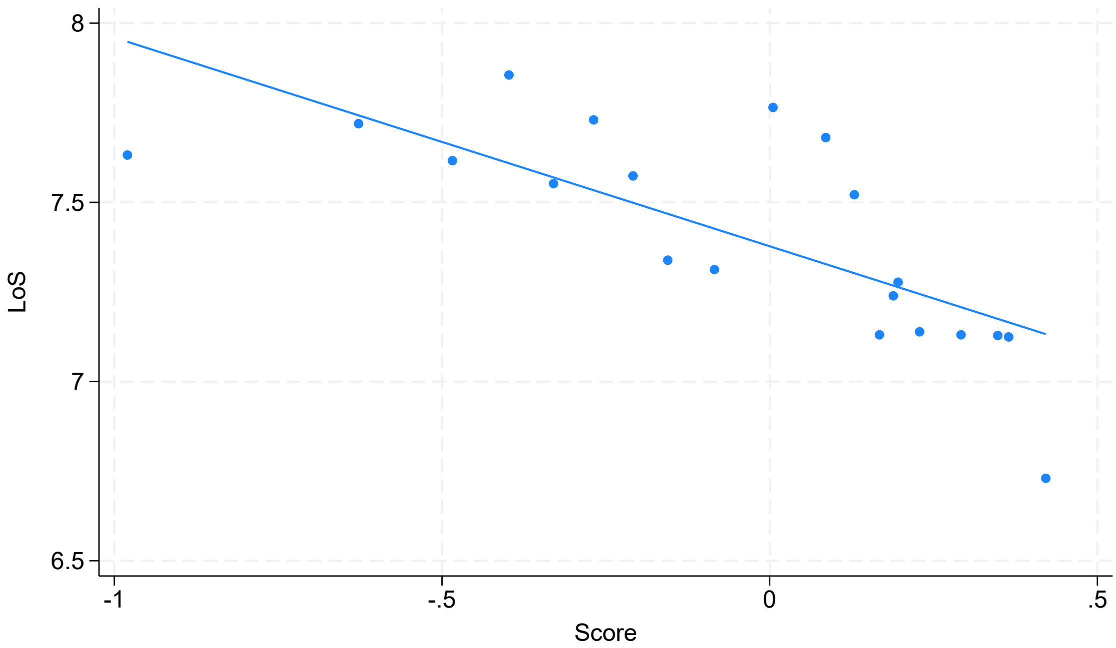


Notes: The plotted data represent yearly mean residuals from an individual-level regression of *Score* on year, province and specialty fixed effects, disaggregated by treatment status. Lines represent the linear fit to the scatterplots, separately estimated before and after the shock.

Figure 4. Human capital score and hospital outcomes



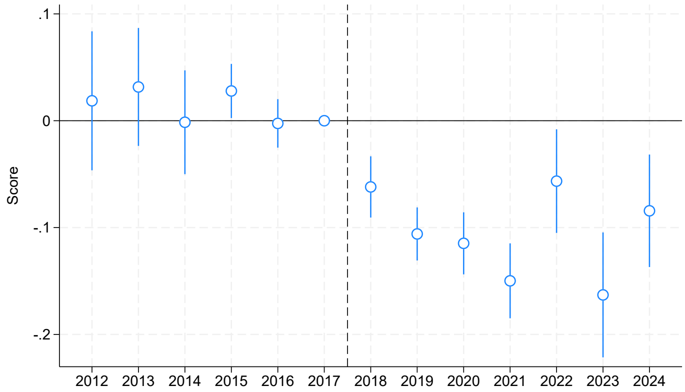
(a) Human capital score and death rate



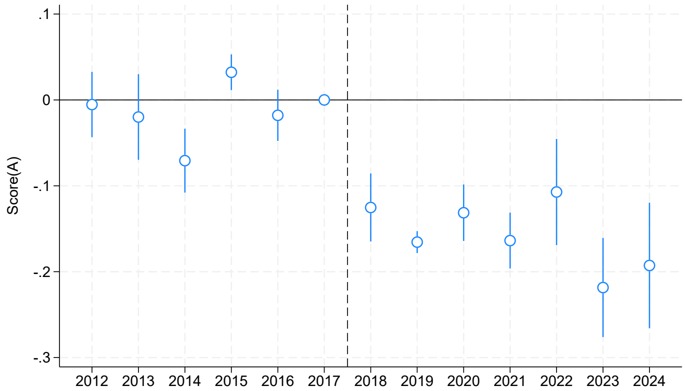
(b) Human capital score and length of stay

Notes: This figure shows the relationship between the regional mean of our human capital proxy, *Score*, and the regional mean of our hospital quality outcomes, *Death rate* and *LoS*. For visual clarity, *Score* is grouped into equal-sized bins, such that each bin contains the average *Score* and the average value of either *Death rate* or *LoS*. The underlying data are at the region level and corresponds to the period 2012-2017.

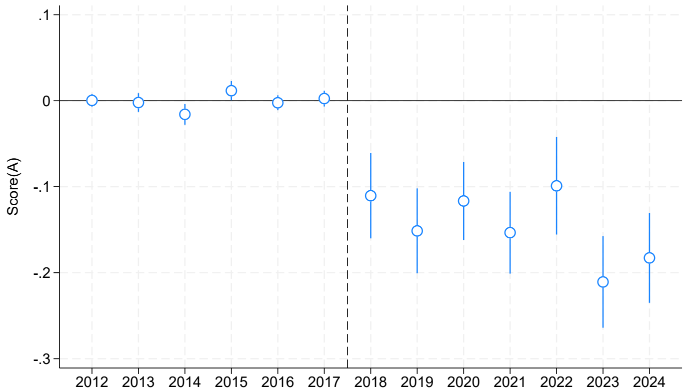
Figure 5. Dynamic effects of the political polarization shock on human capital score: Individual and aggregated evidence



(a) DID (individual level)



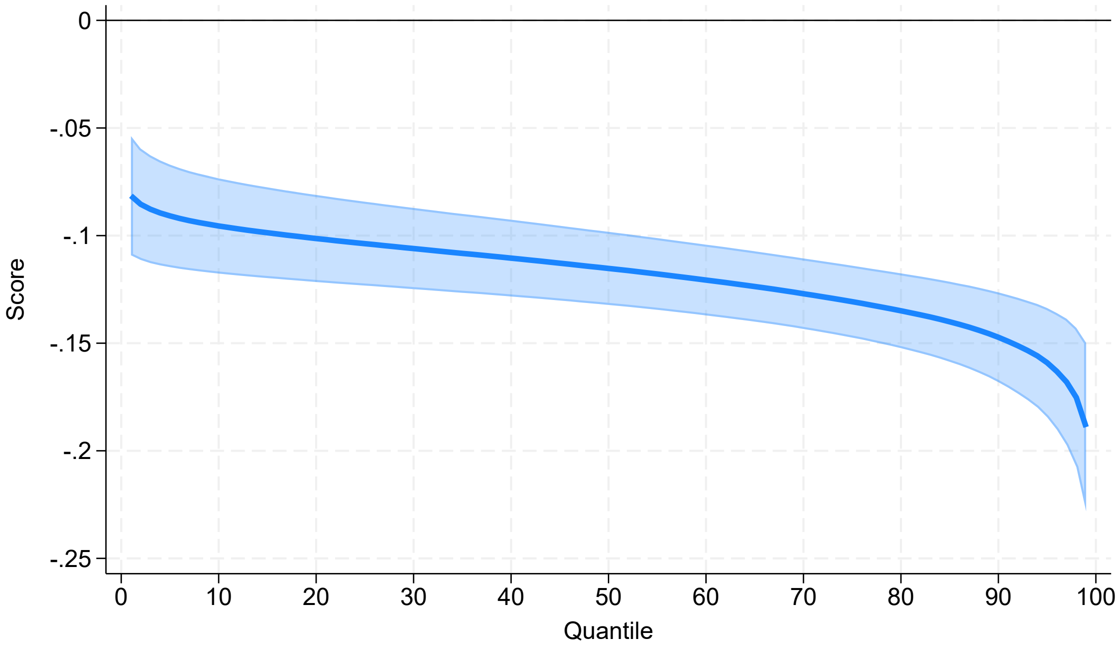
(b) DID (hospital-specialty level)



(c) SDID (hospital-specialty level)

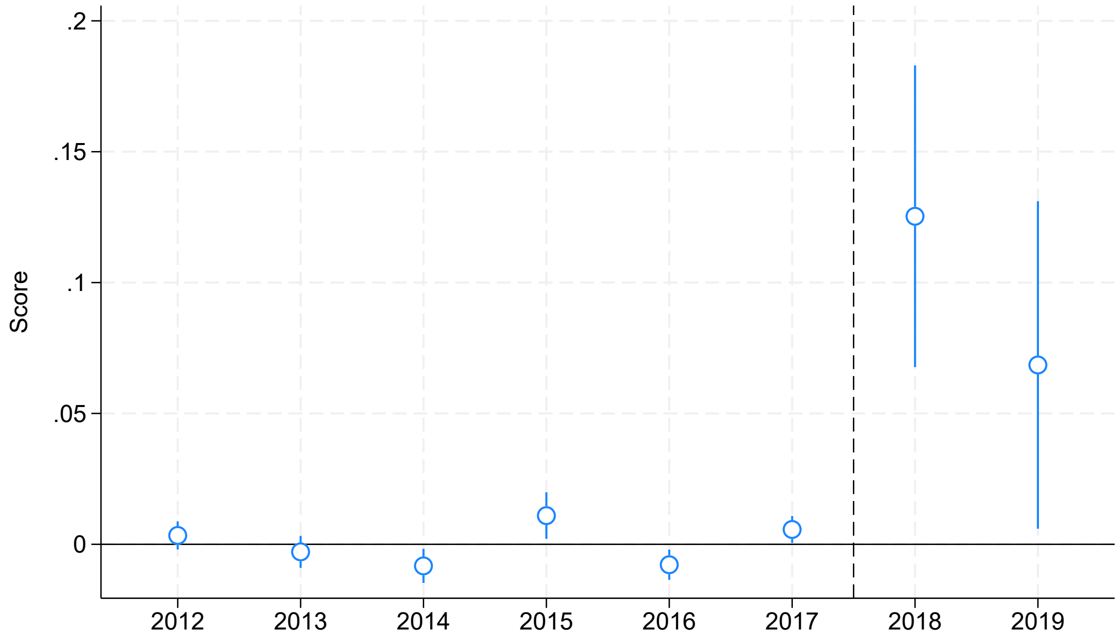
Notes: This figure presents event study evidence of the late 2017 events effects on human capital at the individual and hospital-specialty levels. Panel A's outcome, *Score*, is the total score standardized by year, while Panels B and C use *Score*(*A*), the median total score by hospital-specialty. Panels A and B report difference-in-differences estimates, and Panel C uses synthetic difference-in-differences. Each dot corresponds to an estimated coefficient, and vertical lines indicate corresponding 95% confidence intervals. Standard errors are clustered at region level in Panels A and B, and obtained using bootstrapping procedures in Panel C.

Figure 6. Score by quantiles



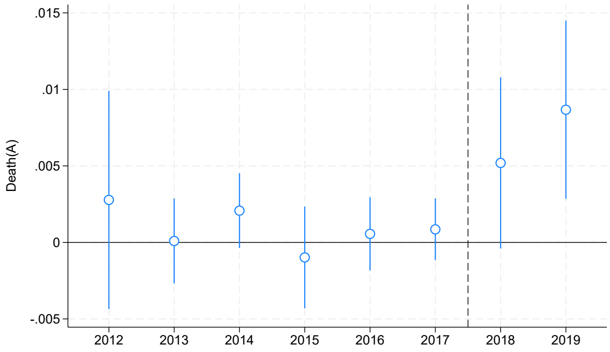
Notes: This figure presents the intensity of the effects of late 2017 events on human capital across the human capital distribution, following Equation 1. We use quantile regressions with the method of moments proposed by Machado and Silva (2019). The line corresponds to estimated coefficients, and shaded areas indicate corresponding 95% confidence intervals. Standard errors are clustered at region level.

Figure 7. Regional spillovers: Valencia

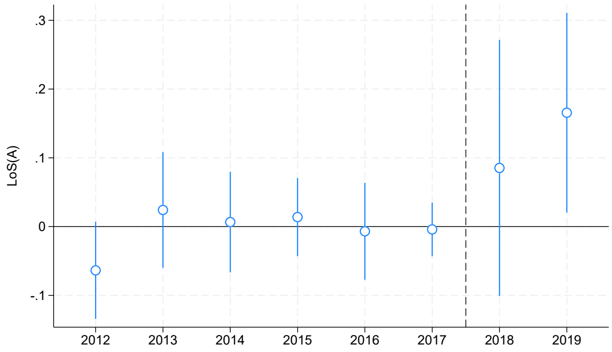


Notes: This figure presents event study evidence of the regional spillovers in Valencia at the hospital-specialty level, employing the synthetic difference-in-differences estimator developed in Arkhangelsky et al. (2021). Valencia serves as the treated region, with the rest of Spain, excluding Catalonia, acting as the control group. The outcome, *Score(A)*, is the median *Score* by hospital-specialty. Each dot corresponds to an estimated coefficient, and vertical lines indicate corresponding 95% confidence intervals. Standard errors are obtained using bootstrapping procedures.

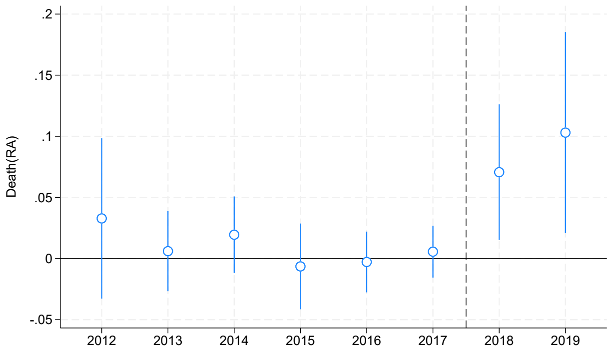
Figure 8. Dynamic effects of the political polarization shock on health outcomes



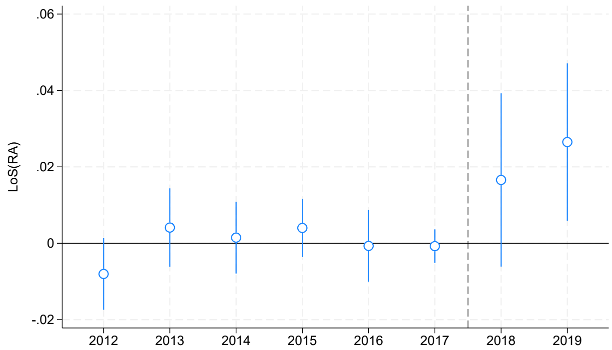
(a) Death(A)



(b) LoS(A)



(c) Death(RA)



(d) LoS(RA)

Notes: This figure presents event study evidence of the late 2017 events effects on health outcomes, following Equation 5 and employing the synthetic difference-in-differences estimator developed in Arkhangelsky et al. (2021). All regressions are at the province-level. In panels A and B the outcome variables are *Death(A)* and *LoS(A)*, respectively. In panels C and D the outcome variables are *Death(RA)* and *LoS(RA)*, respectively. For risk adjustment, we first use pre-treatment patient-level data to estimate a model of the hospital outcome, either *Death* or *LoS*, based on patients' demographics and diagnoses. We then predict the probability of death or the expected length of stay for each patient and aggregate these predictions at the province level. Finally, *Death(RA)* and *LoS(RA)* are defined as the ratio of the observed average death rate and length of stay to their respective predicted values at the province level. Estimates are produced using the procedure outlined in Clarke et al. (2023). Each dot corresponds to an estimated coefficient, and vertical lines indicate corresponding 95% confidence intervals. Standard errors are obtained using bootstrapping procedures.

Table 1. Impact of political polarization on human capital: individual level evidence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CAT x Post	-0.162***	-0.162***	-0.127***	-0.141***	-0.136***	-0.142***	-0.122***	-0.125***	-0.118***
	[0.015]	[0.015]	[0.015]	[0.011]	[0.012]	[0.011]	[0.010]	[0.010]	[0.009]
CAT	0.093	0.093	0.050						
	[0.085]	[0.086]	[0.074]						
Post	0.025								
	[0.015]								
Adjusted R^2	0.001	0.001	0.651	0.684	0.707	0.747	0.804	0.815	0.824
Observations	89,897	89,897	89,897	89,897	89,897	89,897	89,771	89,771	89,750
Year FE	×	✓	✓	✓	✓	✓	✓	✓	✓
Specialty FE	×	×	✓	✓	✓	✓	✓	✓	✓
Region FE	×	×	×	✓	✓	✓	✓	✓	✓
Province FE	×	×	×	×	✓	✓	✓	✓	✓
Town FE	×	×	×	×	×	✓	✓	✓	✓
Hospital x Specialty FE	×	×	×	×	×	×	✓	✓	✓
Year x Specialty FE	×	×	×	×	×	×	×	✓	✓
University FE	×	×	×	×	×	×	×	×	✓

Notes: This table presents the impact of late 2017 events on human capital. In all columns, the outcome variable, *Score*, measures the total score, standardized by year. Estimates are from the difference-in-differences specification in Equation 1. The underlying data are at the individual level and cover the period 2012-2024. Fixed effects are denoted at the bottom of the table. Standard errors are displayed in brackets and are clustered at region level. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table 2. Impact of political polarization on human capital: hospital-specialty level evidence

	Difference-in-differences				Synthetic difference-in-differences			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CAT x Post	-0.149***	-0.100***	-0.133***	-0.144***	-0.159***	-0.099***	-0.131***	-0.146***
	[0.013]	[0.011]	[0.017]	[0.015]	[0.030]	[0.022]	[0.016]	[0.015]
Observations	7,254	13,247	19,968	26,624	7,254	13,247	19,968	26,624
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Region x Specialty FE	✓	✓	✓	✓	✓	✓	✓	✓
Province x Specialty FE	✗	✓	✓	✓	✗	✓	✓	✓
Town x Specialty FE	✗	✗	✓	✓	✗	✗	✓	✓
Hospital x Specialty FE	✗	✗	✗	✓	✗	✗	✗	✓

Notes: This table presents aggregate evidence on the impact of late 2017 events on human capital. Across all columns, the outcome variable, *Score(A)*, represents the median *Score* at the respective unit of analysis. Columns (1) and (5) aggregate data at the region-specialty level, columns (2) and (6) at the province-specialty level, columns (3) and (7) at the town-specialty level, and columns (4) and (8) at the hospital-specialty level. Estimates are from the difference-in-differences specification in Equation 1. The underlying data are at the individual level and cover the period 2012-2024. Fixed effects are denoted at the bottom of the table. Standard errors are reported in brackets and clustered at the region level in columns (1)-(4), while in columns (5)–(8), they are obtained using bootstrapping procedures. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table 3. Impact of political polarization on human capital by intensity of the treatment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CAT x Post x Votes	-0.345***	-0.345***	-0.306***	-0.307***	-0.298***	-0.155***	-0.131***	-0.133***	-0.074***
	[0.000]	[0.002]	[0.005]	[0.006]	[0.006]	[0.004]	[0.001]	[0.006]	[0.017]
CAT x Post	-0.011	-0.011	0.006	-0.007	-0.007	-0.075***	-0.066***	-0.069***	-0.087***
	[0.015]	[0.015]	[0.014]	[0.010]	[0.011]	[0.010]	[0.010]	[0.011]	[0.009]
CAT x Votes	-0.475***	-0.475***	-0.155***	-0.162***	0.233***				
	[0.000]	[0.002]	[0.016]	[0.017]	[0.016]				
CAT	0.296***	0.296***	0.117						
	[0.085]	[0.086]	[0.073]						
Post	0.025								
	[0.015]								
Adjusted R^2	0.002	0.002	0.652	0.684	0.707	0.747	0.804	0.815	0.824
Observations	89,897	89,897	89,897	89,897	89,897	89,897	89,771	89,771	89,750
Year FE	×	✓	✓	✓	✓	✓	✓	✓	✓
Specialty FE	×	×	✓	✓	✓	✓	✓	✓	✓
Region FE	×	×	×	✓	✓	✓	✓	✓	✓
Province FE	×	×	×	×	✓	✓	✓	✓	✓
Town FE	×	×	×	×	×	✓	✓	✓	✓
Hospital x Specialty FE	×	×	×	×	×	×	✓	✓	✓
Year x Specialty FE	×	×	×	×	×	×	×	✓	✓
University FE	×	×	×	×	×	×	×	×	✓

Notes: This table presents the heterogeneous impact of late 2017 events on human capital by intensity of the treatment. In all columns, the outcome variable, *Score*, measures the total score, standardized by year. *Votes* corresponds to the percentage of votes at the town level for secessionist parties in the December 2017 regional elections. Estimates are from the difference-in-differences specification in Equation 6. The underlying data are at the individual level and cover the period 2012-2024. Fixed effects are denoted at the bottom of the table. Standard errors are displayed in brackets and are clustered at region level. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table 4. Impact of political polarization on human capital by quantiles

	(1) τ = 0.05	(2) τ = 0.25	(3) τ = 0.50	(4) τ = 0.75	(5) τ = 0.95
CAT x Post	-0.091***	-0.104***	-0.115***	-0.131***	-0.159***
	[0.012]	[0.010]	[0.009]	[0.009]	[0.013]
Observations	89,897	89,897	89,897	89,897	89,897
Year FE	✓	✓	✓	✓	✓
Specialty FE	✓	✓	✓	✓	✓
Region FE	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓
Town FE	✓	✓	✓	✓	✓
Hospital x Specialty FE	✓	✓	✓	✓	✓
Year x Specialty FE	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓

Notes: This table presents the impact of late 2017 secessionist events on human capital by quantiles using the method of moments proposed by Machado and Silva (2019). In all columns, the outcome variable, *Score*, measures the total score, standardized by year. Estimates are from the difference-in-differences specification in Equation 1. In each column, τ denotes the quantile of the *Score* distribution analyzed. The underlying data are at the individual level and cover the period 2012-2024. Fixed effects are denoted at the bottom of the table. Standard errors are displayed in brackets and are clustered at region level. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table 5. Heterogeneous effects by location of the university of graduation

	Intensive margin				Extensive margin		
	(1) Score	(2) Score	(3) Score	(4) Score	(5) International	(6) Local	(7) Other Region
CAT x Post	-0.064***	-0.136***	-0.124***	-0.059**	0.018**	0.001	-0.019
	[0.020]	[0.011]	[0.014]	[0.022]	[0.008]	[0.027]	[0.021]
CAT x Post x Local				-0.072**			
				[0.027]			
CAT x Post x Other Region				-0.071***			
				[0.020]			
Adjusted R^2	0.879	0.791	0.842	0.828	0.162	0.151	0.130
Observations	12,916	44,289	31,086	89,750	89,771	89,771	89,771
Specialty x Year FE	✓	✓	✓	✓	✓	✓	✓
Specialty x Hospital FE	✓	✓	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✗	✗	✗
Sample	International	Local	Other Region	All	All	All	All

Notes: This table presents the heterogeneous impact of late 2017 events on human capital by the location of the university of graduation of candidates. In columns (1)-(4), the outcome variable, *Score*, measures the total score, standardized by year. Estimates are from variations of the difference-in-differences specification defined in Equation 1. Column (1) restricts the sample to international candidates. Column (2) focuses on candidates from local universities, while column (3) narrows the sample to interregional candidates. Finally, column (4) utilizes the entire dataset. In columns (5)-(7), the outcome variables, *International*, *Local* and *Other Region*, are regressed on $Cat \times Post$. For a detailed description of each of the variables, see Table A1. The underlying data are at the individual level and cover the period 2012-2024. Fixed effects are denoted at the bottom of the table. Standard errors are displayed in brackets and are clustered at region level. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table 6. Impact of political polarization on hospital performance: individual-level evidence

	(1)	(2)	(3)	(4)	(5)
Panel A: Death					
CAT x Post	0.006***	0.007***	0.007***	0.006***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
CAT	-0.012**	-0.012**			
	(0.005)	(0.005)			
Post	-0.007***				
	(0.001)				
Panel B: LoS					
CAT x Post	0.038***	0.038***	0.040***	0.036***	0.037***
	(0.008)	(0.008)	(0.007)	(0.007)	(0.006)
CAT	-0.088***	-0.087***			
	(0.025)	(0.025)			
Post	-0.052***				
	(0.008)				
Observations	2,174,314	2,174,314	2,174,314	2,174,314	2,174,312
Year x Month FE	✗	✓	✓	✓	✓
Diagnostic x Province FE	✗	✗	✓	✓	✓
Diagnostic x Year FE	✗	✗	✗	✓	✓
Age x Gender FE	✗	✗	✗	✗	✓

Notes: This table presents the impact of late 2017 events on hospital performance. In Panel A, the outcome, *Death*, is a dummy variable that takes the value of one if the individual dies and zero otherwise. In Panel B, the outcome variable, *LoS*, measures in days the length of stay at the hospital. Estimates are from the difference-in-differences specification in Equation 4. The underlying data are at the individual level and cover the period 2012-2019. Fixed effects are denoted at the bottom of the table. Standard errors are displayed in brackets and are clustered at region level. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

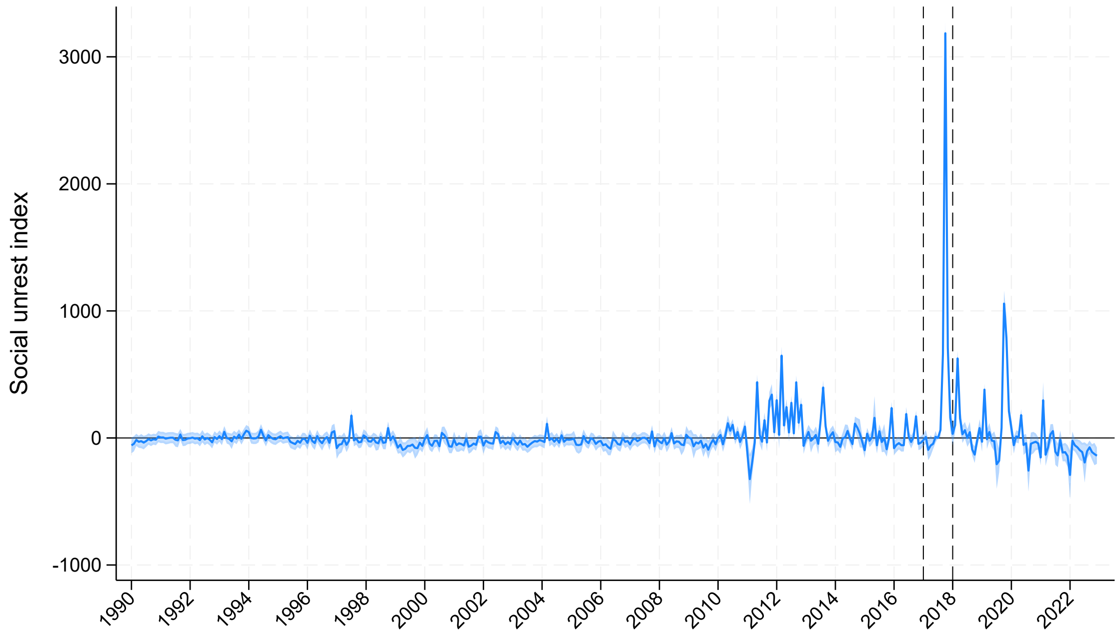
Table 7. Impact of political polarization on hospital performance: province-level evidence

	(1) Death(A)	(2) Death(A)	(3) Death(RA)	(4) LoS(A)	(5) LoS(A)	(6) LoS(RA)
CAT x Post	0.005***	0.007***	0.087***	0.202***	0.125*	0.022**
	[0.001]	[0.002]	[0.034]	[0.044]	[0.072]	[0.010]
Observations	416	416	416	416	416	416
Year FE	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Mean. Dep. Variable	0.080	0.080	0.926	6.975	6.975	0.931
Empirical strategy	DID	SDID	SDID	DID	SDID	SDID

Notes: This table presents the impact of late 2017 events on hospital performance. In columns (1) and (2) the outcome variable, *Death(A)*, measures the death rate. In column (3), the outcome variable, *Death(RA)*, measures the risk-adjusted death rate. In columns (1) and (2) the outcome variable, *LoS(A)*, measures the average length of stay. In column (3), the outcome variable, *LoS(RA)*, measures the risk-adjusted length of stay. For risk adjustment, we first use pre-treatment patient-level data to estimate a model of the hospital outcome, either *Death* or *LoS*, based on patients' demographics and diagnoses. We then predict the probability of death or the expected length of stay for each patient and aggregate these predictions at the province level. Finally, *Death(RA)* and *LoS(RA)* are defined as the ratio of the observed average death rate and length of stay to their respective predicted values at the province level. Estimates are from the difference-in-differences specification in Equation 4. The underlying data are at the province level and cover the period 2012-2019. Fixed effects are denoted at the bottom of the table. Standard errors are reported in brackets and clustered at the region level in columns (1) and (4), while in columns (2)–(3) and (5)–(6), they are obtained using bootstrapping procedures. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

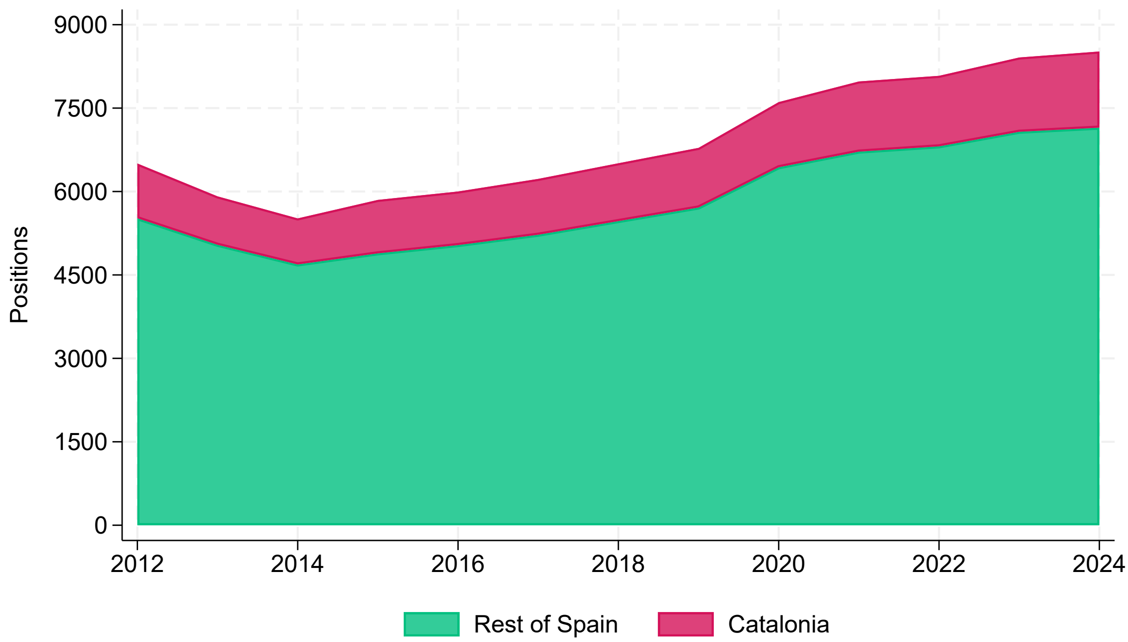
A Online Appendix

Figure A1. Social unrest index in Spain compared to other countries



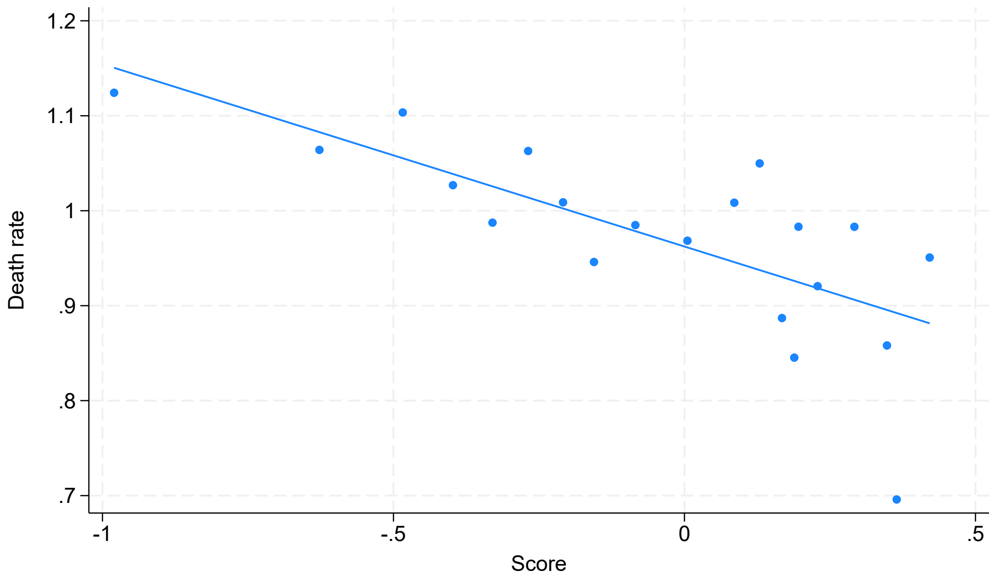
Notes: This figure presents event study evidence of the late 2017 events effects on social unrest. The line connects the estimated coefficients, and shaded areas indicate corresponding 95% confidence intervals. Social unrest is proxied with the index developed by Barrett et al. (2022). The regression compares Spain's social unrest index with other 130 countries, taking september 2017 as the base month. Standard errors are clustered at country level.

Figure A2. MIR positions by treatment status

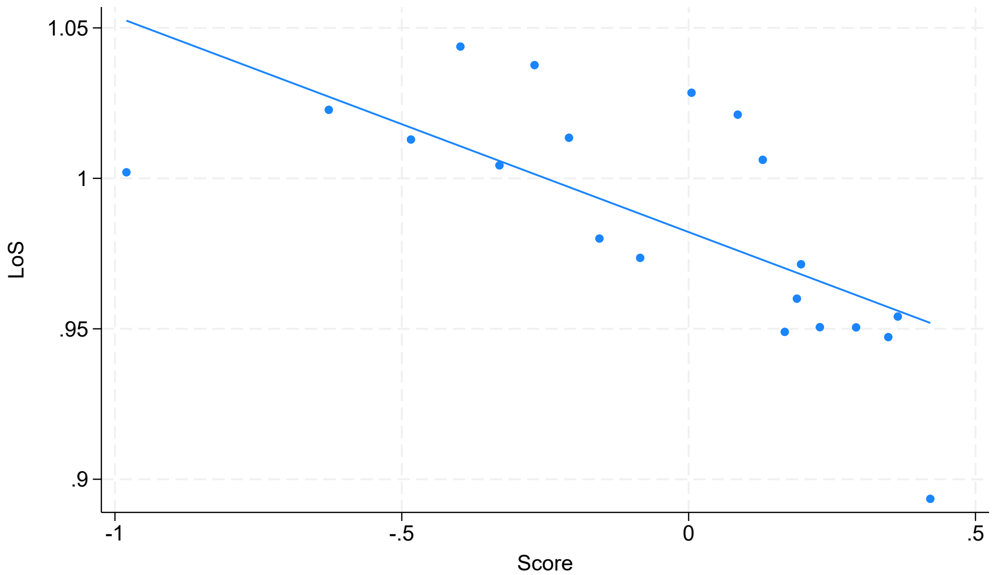


Notes: This figure plots the number of MIR positions offered by Catalonia (red area) and the rest of Spain (green area) over the study period.

Figure A3. Human capital score and risk-adjusted hospital outcomes



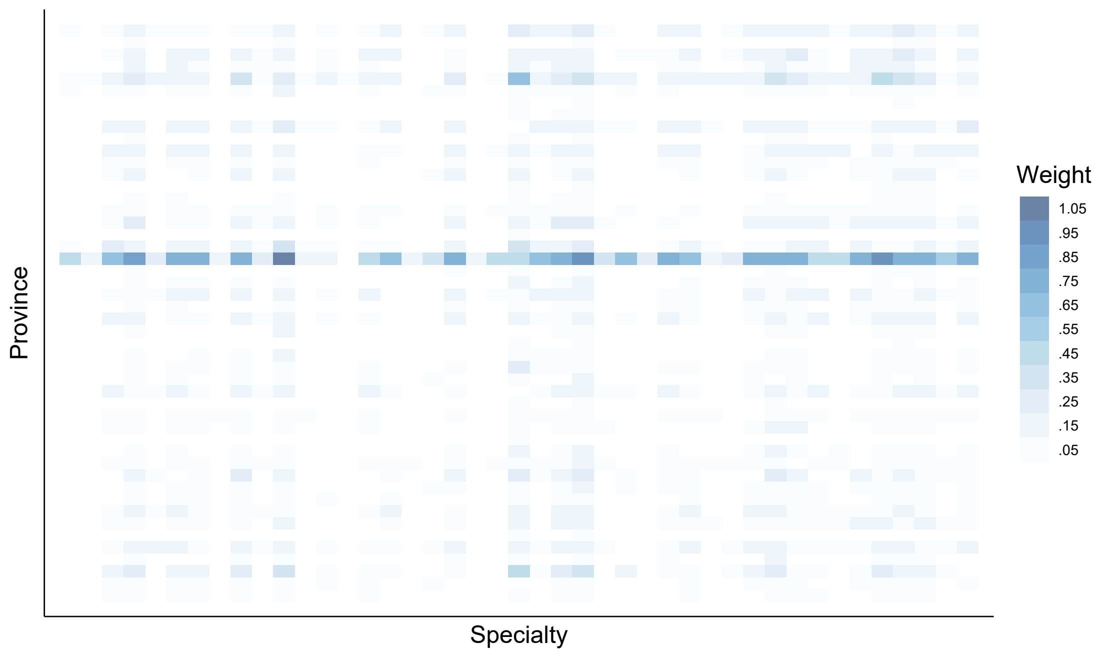
(a) Human capital score and risk-adjusted death rate



(b) Human capital score and risk-adjusted length of stay

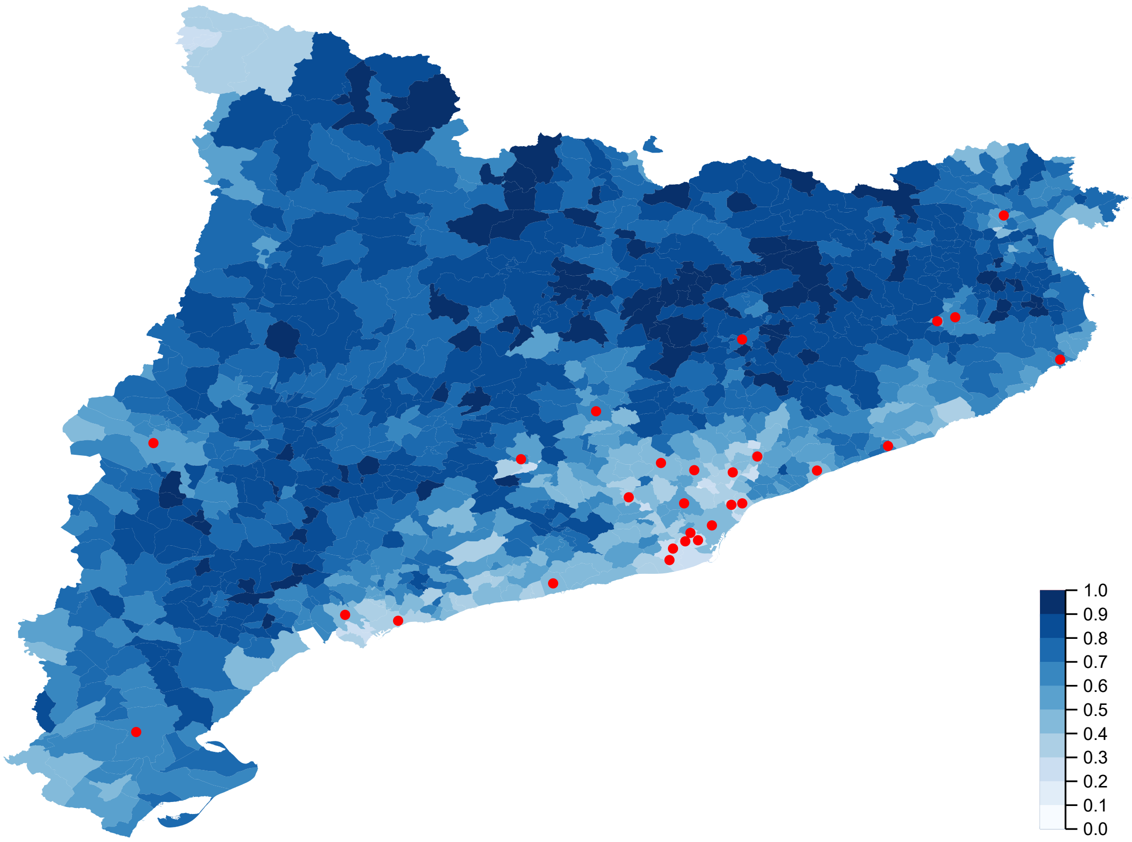
Notes: This figure shows the relationship between the regional mean of our human capital proxy, *Score*, and the regional mean of our risk-adjusted hospital quality outcomes, *Death rate(RA)* and *LoS(RA)*. For risk adjustment, we first use pre-treatment patient-level data to estimate a model of the hospital outcome, either *Death* or *LoS*, based on patients' demographics and diagnoses. We then predict the probability of death or the expected length of stay for each patient and aggregate these predictions at the province level. Finally, *Death(RA)* and *LoS(RA)* are defined as the ratio of the observed average death rate and length of stay to their respective predicted values at the province level. For visual clarity, *Score* is grouped into equal-sized bins, such that each bin contains the average *Score* and the average health outcome. The underlying data are at the region level and corresponds to the period 2012-2017.

Figure A4. Unit weights



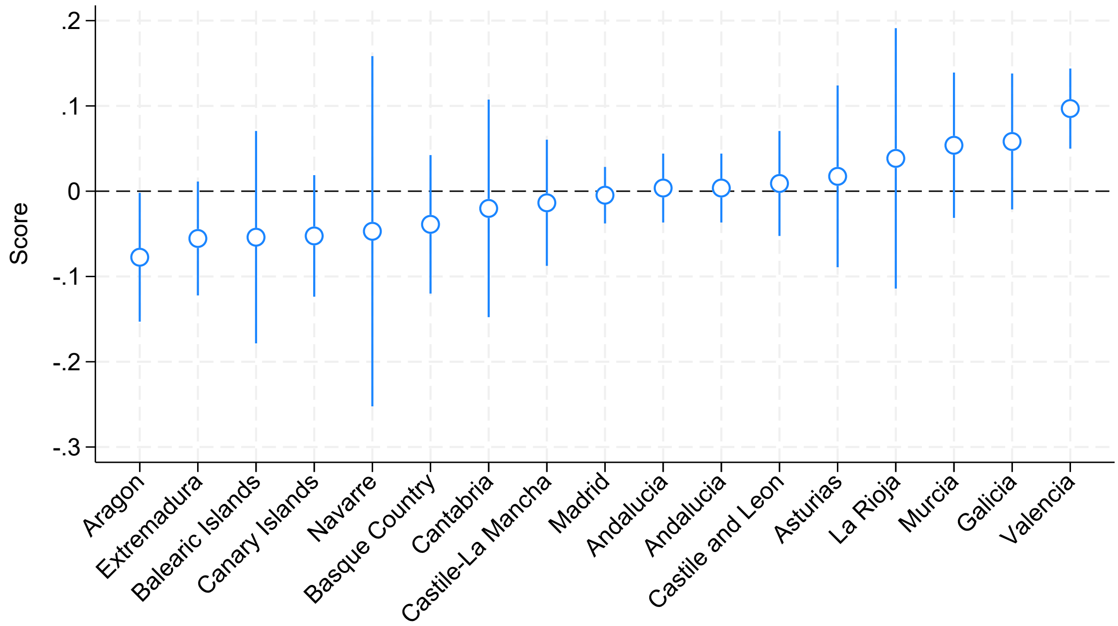
Notes: This figure plots the unit weights from the synthetic difference-in-differences estimator, using *Score (A)* as the outcome variable

Figure A5. Local exposure to political polarization



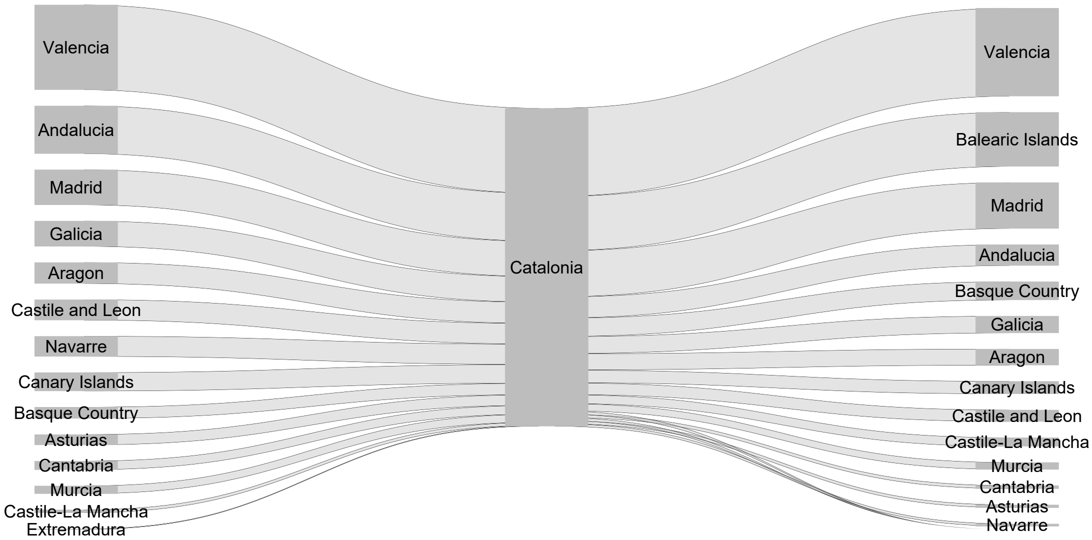
Notes: This map shows the percentage of votes to secessionist parties (CUP, ERC, Junts x Cat and Diàleg) in december 2017 regional elections. Red dots correspond to towns with hospitals with MIR residents over the period 2012-2024.

Figure A6. Spillovers



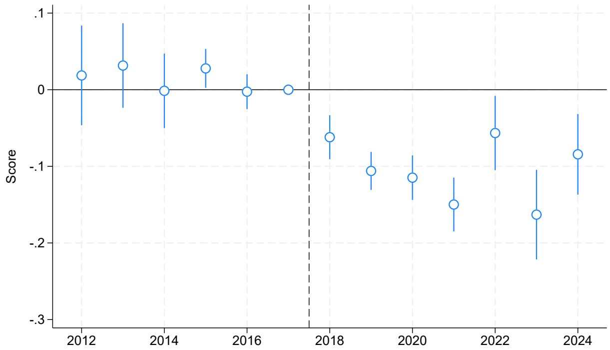
Notes: This figure presents the spillover effects of late 2017 events by region. Estimates are obtained employing the synthetic difference-in-differences estimator developed in Arkhangelsky et al. (2021). Catalonia is excluded from the sample. The outcome variable, Score(A), is the median Score by hospital-specialty. Each dot corresponds to an estimated coefficient, and vertical lines indicate corresponding 95% confidence intervals. Standard errors are obtained using bootstrapping procedures.

Figure A7. Inflows and outflows of MIR residents to and from Catalonia

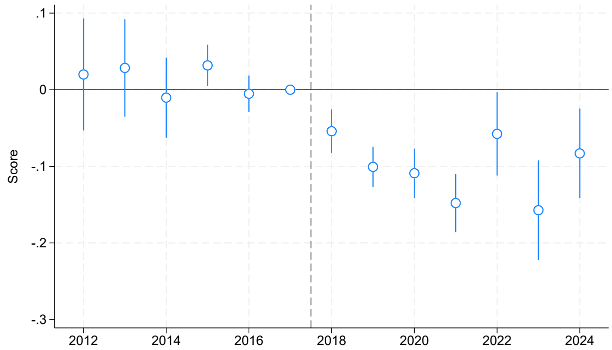


Notes: Inflows and outflows of MIR residents to and from Catalonia. The thickness of each flow corresponds to the relative number of residents arriving from or departing to the respective region. Candidates' origin is proxied by the location of the university of graduation. The underlying data cover the period 2012-2017.

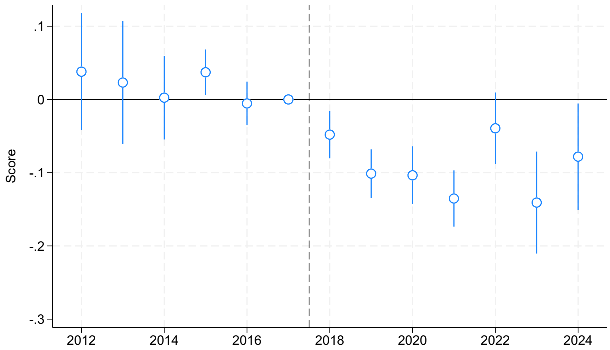
Figure A8. Impact of political polarization on human capital: excluding regions receiving the largest outflows of residents to Catalonia



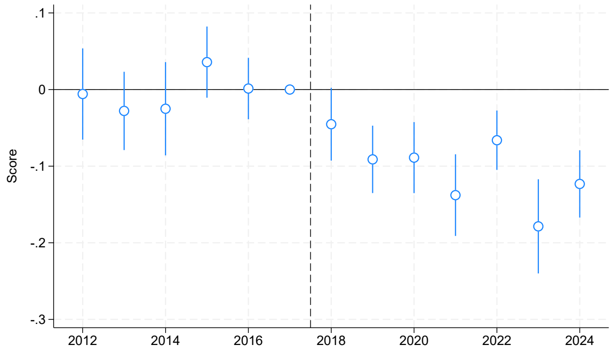
(a) Baseline



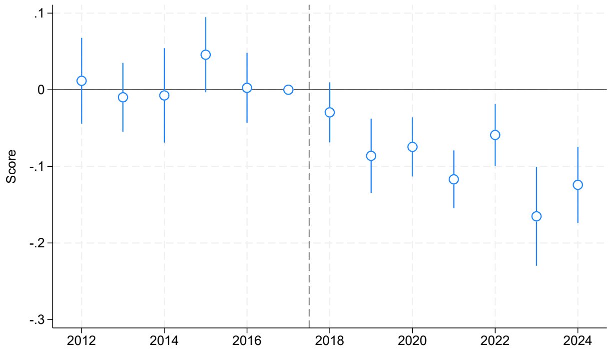
(b) Excluding 1 region



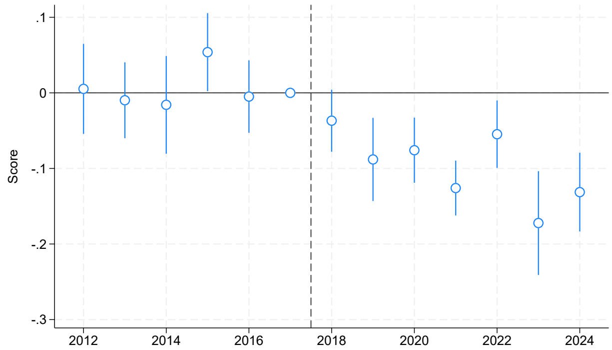
(c) Excluding 2 regions



(d) Excluding 3 regions



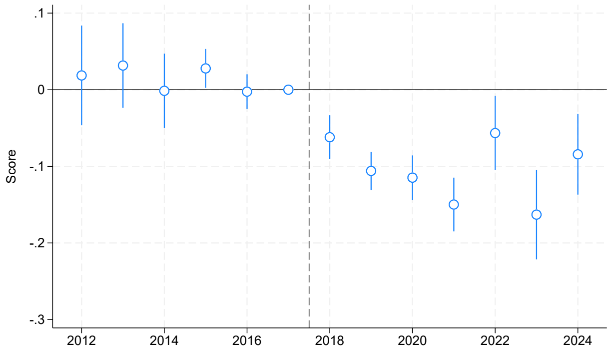
(e) Excluding 4 regions



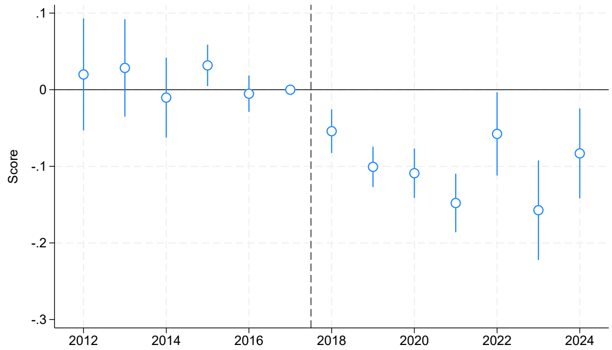
(f) Excluding 5 regions

Notes: This figure presents event study evidence of the late 2017 secessionist events effects on human capital, following Equation 2, and excluding cumulatively those regions sending the largest outflows of residents to Catalonia. The regions excluded, in order, are: Valencia, Andalucia, Madrid, Galicia and Aragon. Each dot corresponds to an estimated coefficient, and shaded areas indicate corresponding 95% confidence intervals. Standard errors are clustered at region level.

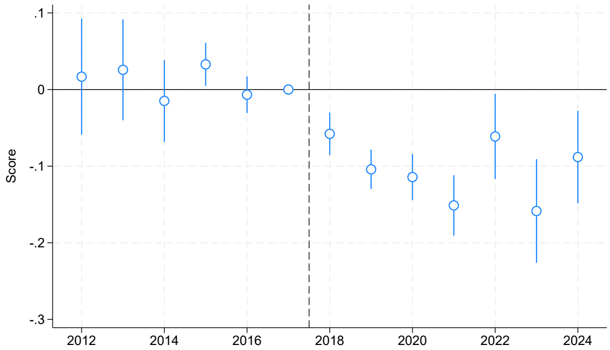
Figure A9. Impact of political polarization on human capital: excluding regions receiving the largest inflows of residents from Catalonia



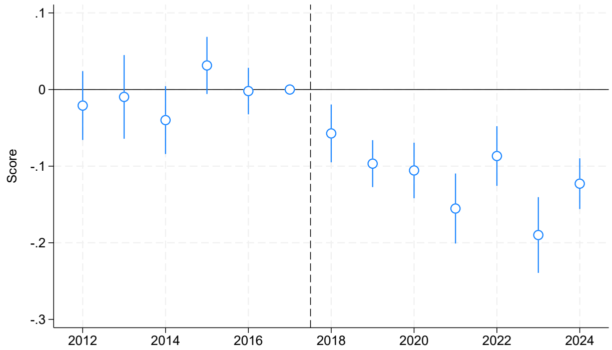
(a) Baseline



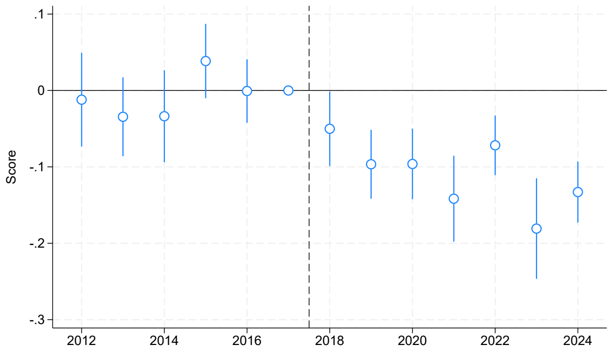
(b) Excluding 1 region



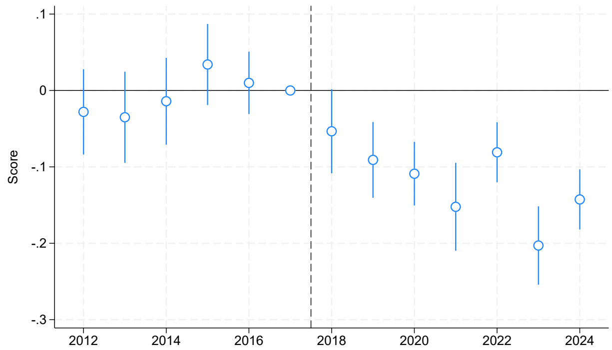
(c) Excluding 2 regions



(d) Excluding 3 regions



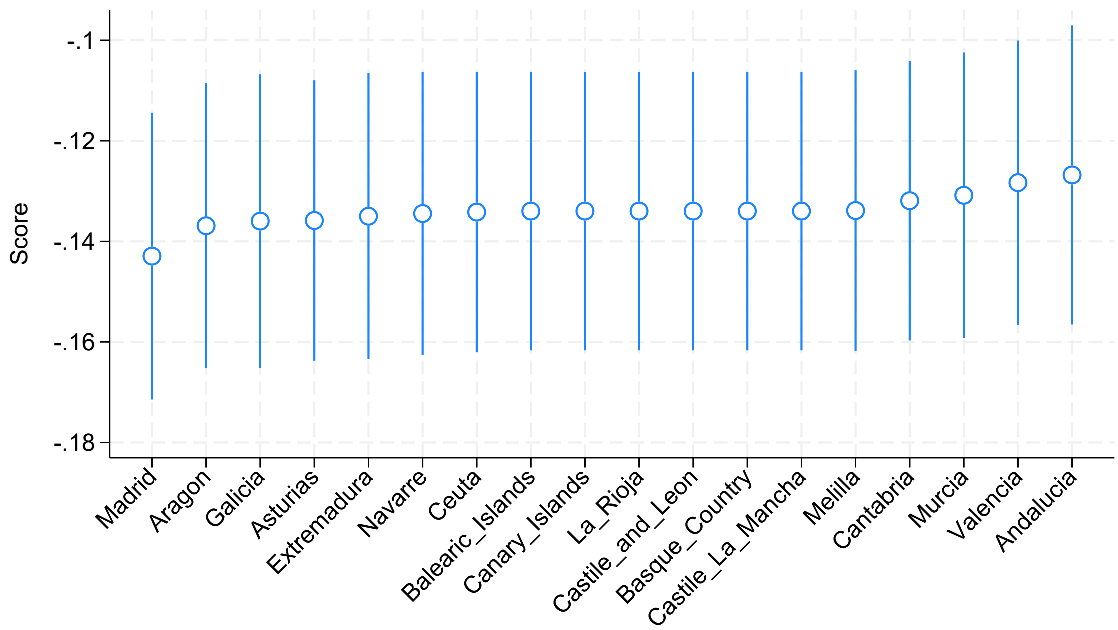
(e) Excluding 4 regions



(f) Excluding 5 regions

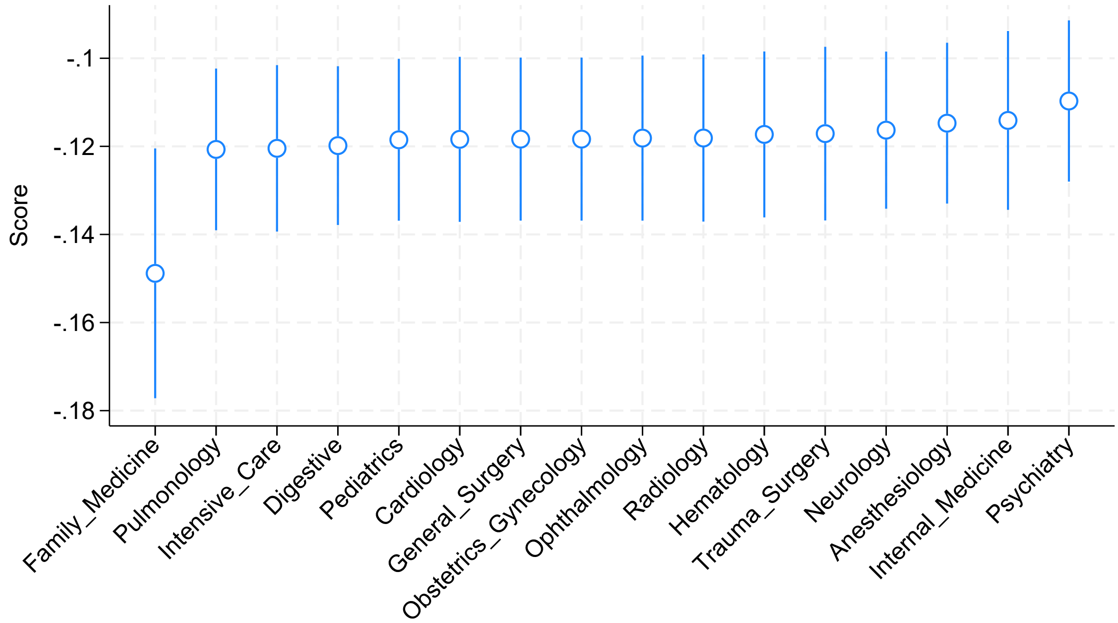
Notes: This figure presents event study evidence of the late 2017 secessionist events effects on human capital, following Equation 2, and excluding cumulatively those regions receiving the largest inflows of residents from Catalonia. The regions excluded, in order, are: Valencia, Balearic Islands, Madrid, Andalucia and the Basque Country. Each dot corresponds to an estimated coefficient, and shaded areas indicate corresponding 95% confidence intervals. Standard errors are clustered at region level.

Figure A10. Baseline results excluding regions one at a time



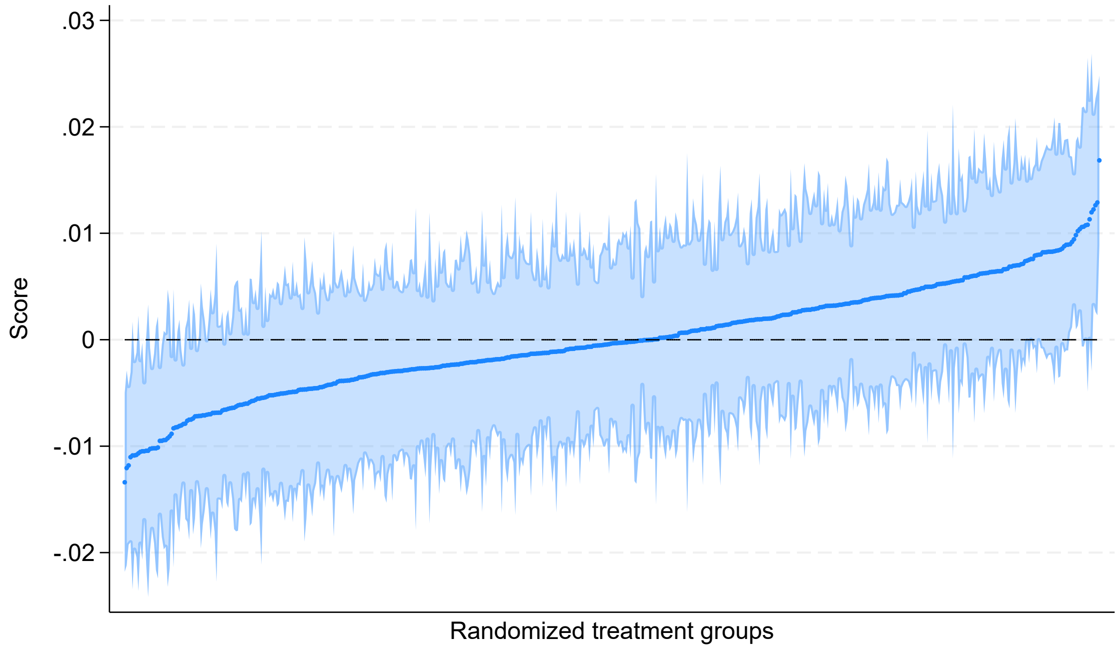
Notes: This figure presents the impact of late 2017 events on human capital (measured as *Score*), following Equation 1, for 18 different subsamples, excluding Spanish regions one at a time. Each dot corresponds to an estimated coefficient, and vertical lines indicate corresponding 95% confidence intervals. Standard errors are clustered at region level.

Figure A11. Baseline results excluding specialties one at a time



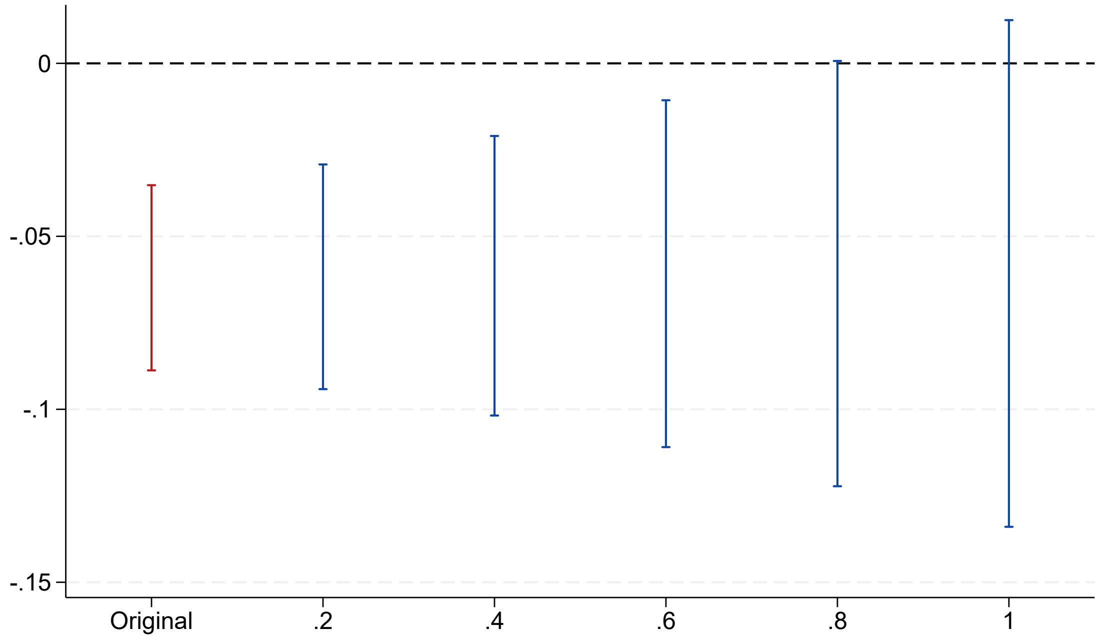
Notes: This figure presents the impact of late 2017 events on human capital (measured as *Score*), following Equation 1, for 16 different subsamples, excluding specialties one at a time. In order to be considered for exclusion, specialties must have at least 100 positions each year. Each dot corresponds to an estimated coefficient, and vertical lines indicate corresponding 95% confidence intervals. Standard errors are clustered at region level.

Figure A12. Randomization inference



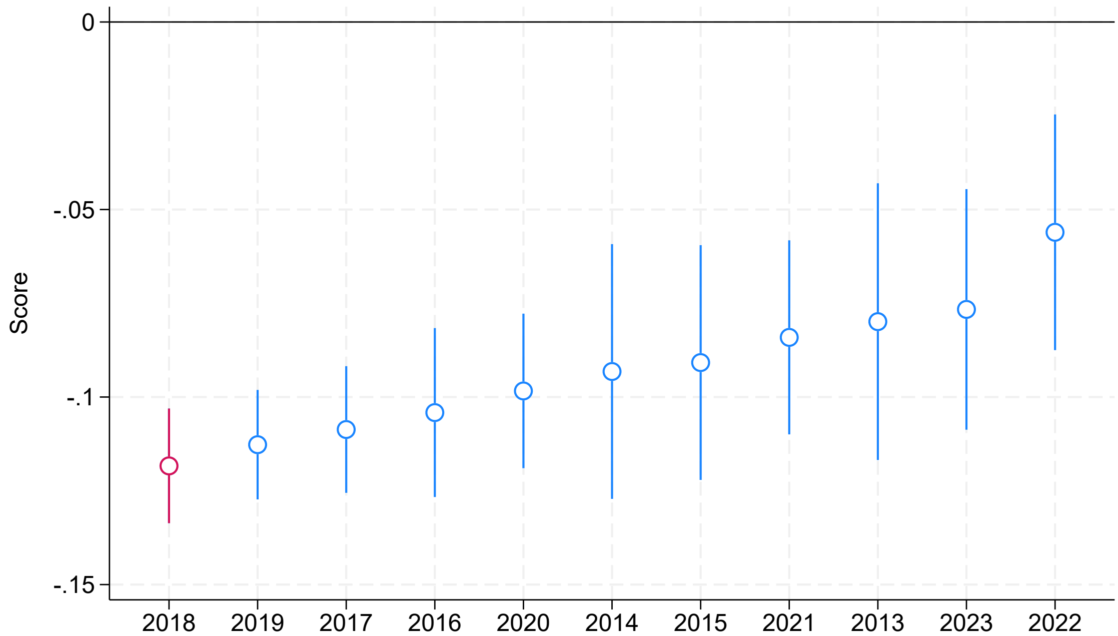
Notes: This figure presents the results of the randomization inference test for the impact of late 2017 events on human capital (measured as *Score*), following Equation 1. Each dot corresponds to an estimated coefficient, and shaded areas indicate corresponding 95% confidence intervals. This analysis involves 500 iterations in which the treatment status is randomly reassigned. Standard errors are clustered at region level.

Figure A13. Impact of political polarization on human capital, allowing pre-trends up to different $\bar{M}$ levels



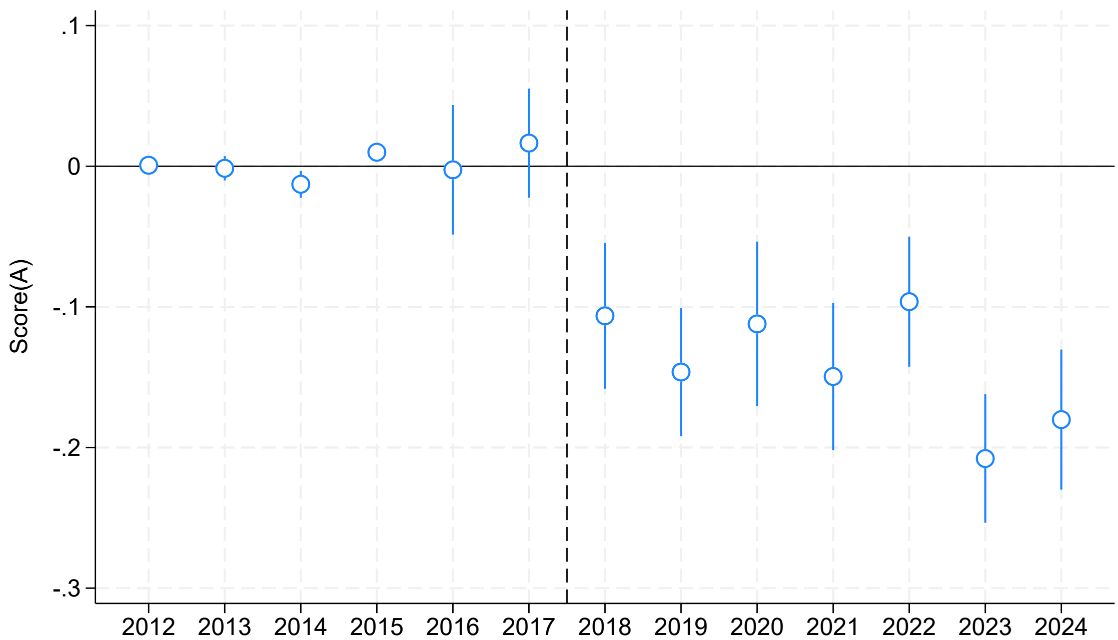
Notes: This figure presents the impact of late 2017 events on human capital (measured as *Score*), following Equation 1. We allow pre-trends differing from zero by different $\bar{M}$ levels following the procedure described in Rambachan and Roth (2023). Each dot corresponds to an estimated coefficient, and vertical lines indicate corresponding 95% confidence intervals. Standard errors are clustered at region level.

Figure A14. Placebo starting years



Notes: This figure presents the impact of late 2017 events on human capital (measured as *Score*), following Equation 1 for the true starting year of the political polarization event (2018), and for 10 plalcebo starting years. Each dot corresponds to an estimated coefficient, and vertical lines indicate corresponding 95% confidence intervals. In red, the effect for the real starting year; in blue, the effect for placebo starting years. Standard errors are clustered at region level.

Figure A15. ROBUSTNESS

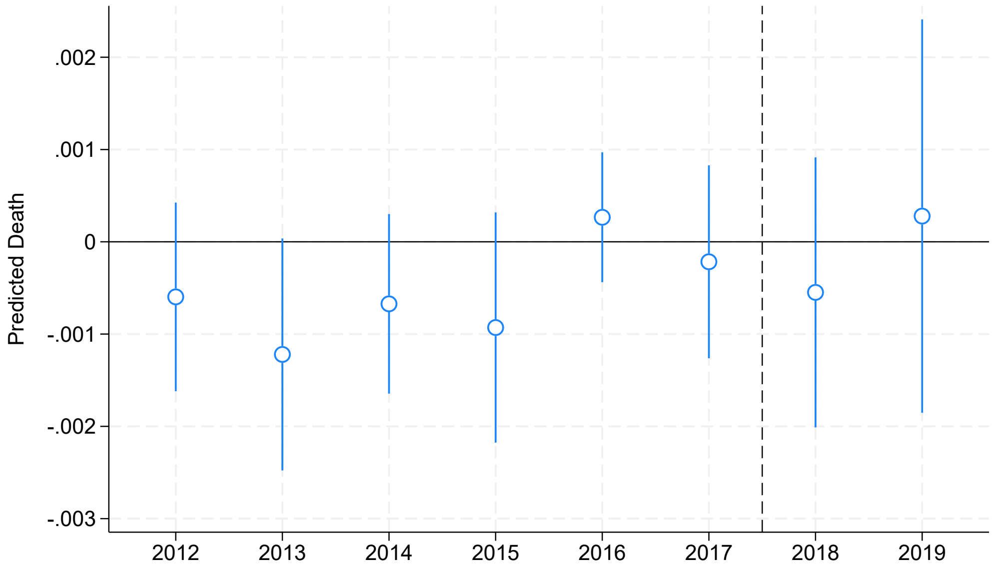


Notes:

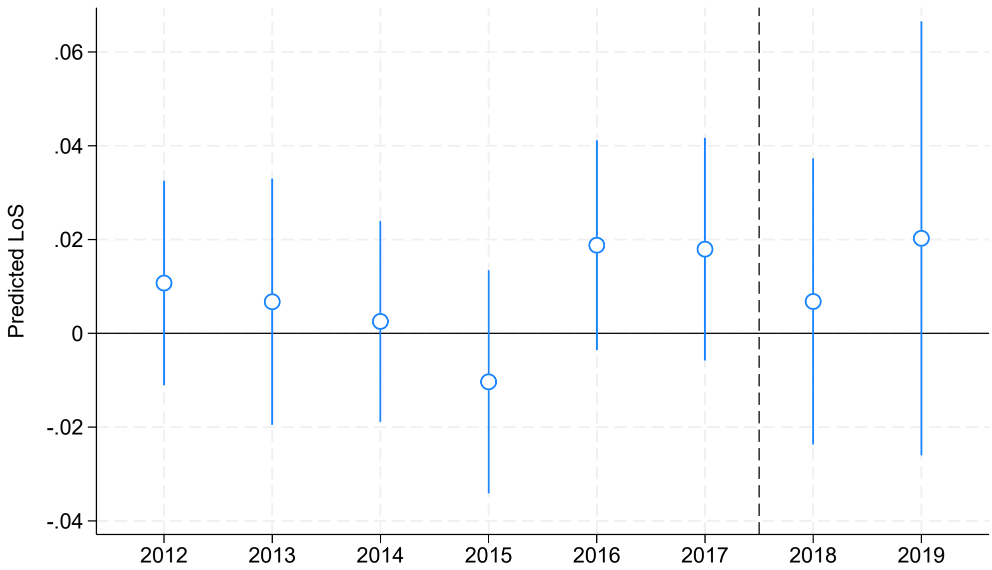
Figure A16. Geographic distribution of hospitals in Spain

Notes: This figure plots the geographic distribution of hospitals in Spain. Each dot represents a municipality with at least one active hospital in 2024.

Figure A17. Dynamic effects of the political polarization shock on predicted health outcomes



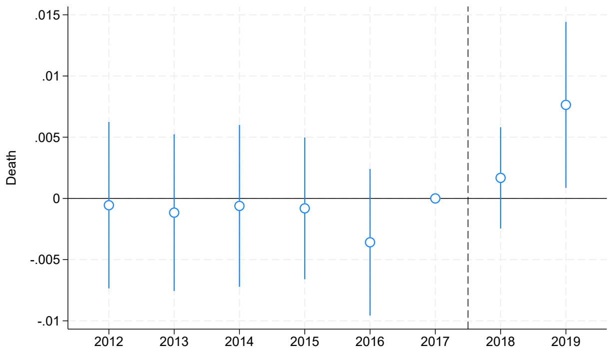
(a) Predicted Death



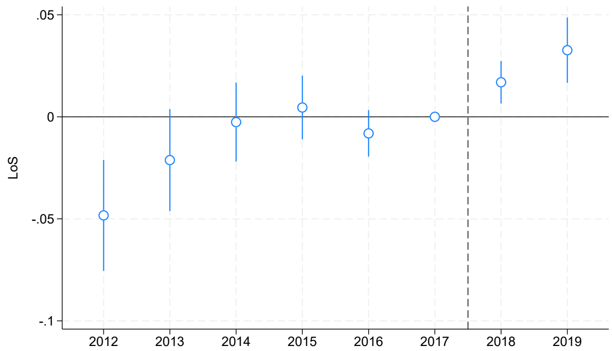
(b) Predicted LoS

Notes: This figure presents event study evidence of the late 2017 events effects on predicted health outcomes, following Equation 5. In both panels, regressions are conducted at the province level. Panel A reports results for the predicted death rate, while Panel B focuses on the predicted length of stay. These variables are constructed by first estimating patient-level models with *Death* and *LoS* as outcomes, controlling for patients' demographics and diagnoses. We then predict these outcomes for each patient and aggregate them to the province level. Each dot corresponds to an estimated coefficient, and vertical lines indicate corresponding 95% confidence intervals. Standard errors are clustered at region level.

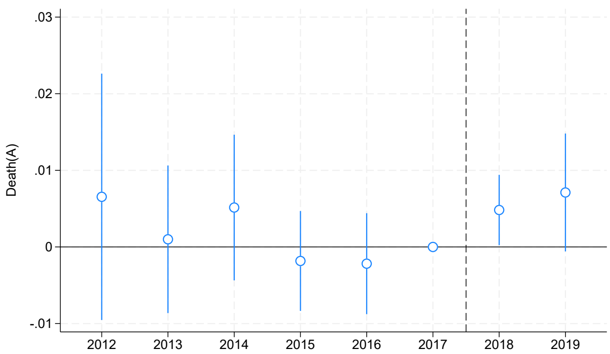
Figure A18. Dynamic effects of the political polarization shock on health outcomes



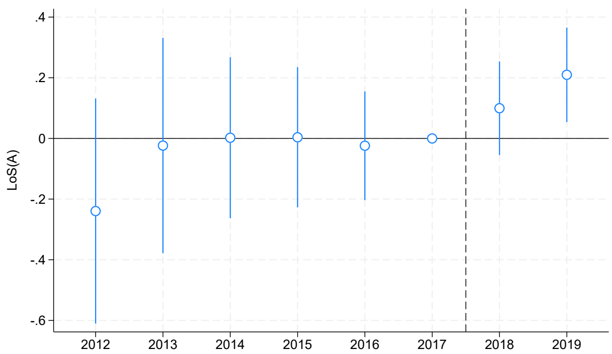
(a) Death



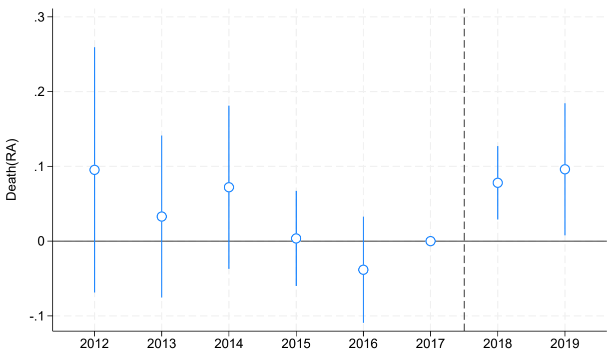
(b) LoS



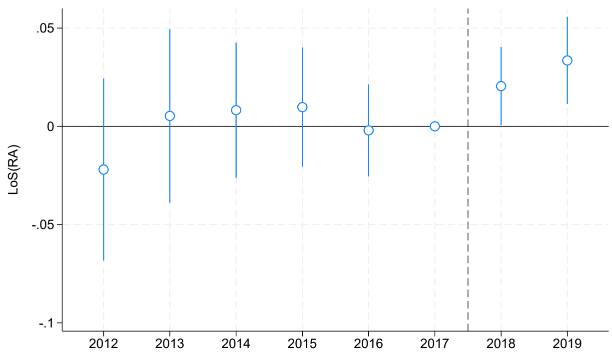
(c) Death(A)



(d) LoS(A)



(e) Death(RA)



(f) LoS(RA)

Notes: This figure presents event study evidence of the late 2017 events effects on health outcomes, following Equation 5. In panels A and B the regression is at the individual-level and the outcome variables are *Death* and *LoS*, respectively. In panels C and D the regression is at the province-level and the outcome variables are *Death(A)* and *LoS(A)*, respectively. In panels E and F the regression is at the province-level and the outcome variables are *Death(RA)* and *LoS(RA)*, respectively. For a detailed description of each of the variables, see Table A1. Each dot corresponds to an estimated coefficient, and vertical lines indicate corresponding 95% confidence intervals. Standard errors are clustered at region level.

Table A1. Variable Definitions

Variables	Definition
MIR variables	
Academic record	Academic record. Standardized by year.
Exam score	Score obtained in the MIR exam. Standardized by year.
Score	Total score obtained in the MIR call (weighted average of *Academic record* and *Exam Score*). Standardized by year.
Score(A)	Median *Score* at the hospital-specialty level.
International	An indicator variable equal to one if the university of graduation is located in a different country, and zero otherwise.
Local	An indicator variable equal to one if the university of graduation is located in the same region as the hospital, and zero otherwise.
Other Region	An indicator variable equal to one if the university of graduation is located in a different region from the hospital, and zero otherwise.
Female	An indicator variable equal to one if the individual is a woman, and zero otherwise.
Foreign	An indicator variable equal to one if the individual is not naturalized in Spain, and zero otherwise.
Positions by province	The natural logarithm of the number of positions offered by year, province and specialty.
Positions by region	The natural logarithm of the number of positions offered by year, region and specialty.
Health variables	
Death	An indicator variable equal to one if the patient dies, and zero otherwise.
LoS	Number of days hospitalized.
Death(A)	Death rate at the province level
LoS(A)	Average number of days hospitalized at the province level.
Death(RA)	Risk-adjusted death rate at the province level.
LoS(RA)	Risk-adjusted average number of days hospitalized at the province level.

Variables	Definition
Hospital variables	
Admissions	The natural logarithm of the annual number of hospital admissions.
Trauma	The natural logarithm of the percentage of Traumatology admissions plus one.
Surgery	The natural logarithm of the percentage of Surgery admissions plus one.
Internal	The natural logarithm of the percentage of Internal medicine admissions plus one.
Palliative	The natural logarithm of the percentage of Palliative admissions plus one.
Intensive	The natural logarithm of the percentage of Intensive care admissions plus one.
Doctors	The natural logarithm of the number of physicians plus one.
Nurses	The natural logarithm of the number of nurses plus one.
Technicians (I)	The natural logarithm of the number of technicians with intermediate degree plus one.
Technicians (II)	The natural logarithm of the number of technicians with higher degree plus one.
Investment	The natural logarithm of total investments.
Purchases	The natural logarithm of purchases of pharmaceutical material.
Beds	The natural logarithm of the annual number of available hospital beds.
Other variables	
CAT	An indicator variable equal to one if the unit of observation is in Catalonia, and zero otherwise.
Post	An indicator variable equal to one if the year of observation is 2018 onwards, and zero otherwise.
Votes	Measured as the percentage of votes cast in favor of pro-independence parties at the municipal level in the Catalan regional elections.

Variables	Definition
Wage	Natural logarithm of the annual wage, including estimated compensation for on-call shifts.
Rental price index	Natural logarithm of the rental housing price index (base year = 2015).

Table A2. Correlation matrix: MIR score components

	Score	Academic record	MIR score
Score	1		
Academic record	0.603***	1	
MIR score	0.997***	0.545***	1

Notes: This table reports the correlation matrix between *Score*, *Academic record* and *MIR score*. The underlying data are at the individual level and cover the period 2012-2024.

Table A3. Summary Statistics: MIR exam

	Catalonia		Rest of Spain	
	Pre'18	Post'18	Pre'18	Post'18
Academic record	0.096	0.061	-0.018	-0.011
	(1.035)	(1.061)	(0.992)	(0.988)
MIR score	0.073	-0.066	-0.013	0.012
	(1.084)	(1.057)	(0.983)	(0.988)
Score	0.078	-0.058	-0.014	0.011
	(1.083)	(1.057)	(0.983)	(0.989)
Score (by specialty)	0.091	-0.100	-0.017	0.019
	(1.094)	(1.049)	(0.976)	(0.986)
Percentile	53.416	49.267	49.954	50.722
	(30.213)	(29.783)	(28.579)	(28.686)
International (%)	0.237	0.245	0.149	0.128
	(0.425)	(0.430)	(0.356)	(0.334)
Local (%)	0.566	0.525	0.511	0.473
	(0.496)	(0.499)	(0.500)	(0.499)
Other Region (%)	0.198	0.230	0.340	0.399
	(0.398)	(0.421)	(0.474)	(0.490)
Foreign (%)	0.231	0.226	0.140	0.109
	(0.421)	(0.418)	(0.347)	(0.312)
Female (%)	0.655	0.650	0.665	0.659
	(0.475)	(0.477)	(0.472)	(0.474)
Positions(#)	35.929	43.439	43.684	47.512
	(70.229)	(83.187)	(91.389)	(91.676)
N	5,605	8,514	30,404	45,374

Notes: This table provides summary statistics by treatment status, before and after the shock. For each variable, the table displays the mean and standard deviation (in parentheses). *Academic record* is the average academic record in the medical degree program, standardized by year. *MIR score* is the score obtained in the MIR exam, standardized by year. *Score* is the total score, standardized by year. *Score (by specialty)* is the total score, standardized by year and specialty. *Percentile* denotes the percentile rank. *International*, *Local*, and *Other Region* represent the percentage of residents who graduated from universities abroad, local universities, or universities in other Spanish regions different from the hospital of residence, respectively. *Foreign* and *Female* are the percentage of foreign and female MIR residents. *Positions* is the number of positions offered yearly by town. The number of observations for each subsample is reported at the bottom. The underlying data cover the period 2012-2024.

Table A4. Summary Statistics: Health outcomes

	Catalonia		Rest of Spain	
	Pre'18	Post'18	Pre'18	Post'18
A: Death rate				
AMI (%)	0.056	0.051	0.065	0.056
	(0.004)	(0.000)	(0.004)	(0.002)
Heart Failure (%)	0.088	0.102	0.100	0.103
	(0.004)	(0.004)	(0.002)	(0.002)
Pneumonia (%)	0.068	0.066	0.086	0.078
	(0.003)	(0.002)	(0.005)	(0.003)
B: Length of stay				
AMI (%)	6.481	6.355	6.678	6.281
	(0.100)	(0.034)	(0.264)	(0.030)
Heart Failure (%)	7.244	7.547	7.919	7.806
	(0.130)	(0.030)	(0.084)	(0.072)
Pneumonia (%)	6.857	6.646	7.722	7.322
	(0.080)	(0.010)	(0.224)	(0.078)
C: Patient characteristics				
Female (%)	0.418	0.424	0.415	0.419
	(0.109)	(0.114)	(0.100)	(0.106)
Age	71.779	72.025	71.623	71.695
	(6.627)	(7.236)	(5.964)	(6.379)
N	52,954	45,237	228,242	198,332

Notes: This table presents summary statistics by treatment status, before and after the shock. For each variable, it reports the mean and standard deviation (in parentheses). Panel A shows the yearly death rates for *AMI*, *Heart Failure*, and *Pneumonia*, respectively. Panel B presents the yearly length of stay (in days) for the same diagnostic conditions. Panel C reports the yearly percentage of female patients (*Female*) and the yearly average age of patients (*Age*). The number of observations for each subsample is provided at the bottom. The data span the period 2012–2019.

Table A5. Impact of political polarization on human capital, with *Percentile* as outcome variable

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CAT x Post	-4.917***	-4.918***	-3.906***	-4.288***	-4.137***	-4.345***	-3.881***	-3.981***	-3.845***
	[0.459]	[0.454]	[0.571]	[0.430]	[0.461]	[0.410]	[0.411]	[0.385]	[0.343]
CAT	3.462	3.463	2.301						
	[2.530]	[2.545]	[2.265]						
Post	0.768								
	[0.459]								
Adjusted R^2	0.001	0.001	0.678	0.710	0.732	0.768	0.825	0.835	0.842
Observations	89,897	89,897	89,897	89,897	89,897	89,897	89,771	89,771	89,750
Year FE	✗	✓	✓	✓	✓	✓	✓	✓	✓
Specialty FE	✗	✗	✓	✓	✓	✓	✓	✓	✓
Region FE	✗	✗	✗	✓	✓	✓	✓	✓	✓
Province FE	✗	✗	✗	✗	✓	✓	✓	✓	✓
Town FE	✗	✗	✗	✗	✗	✓	✓	✓	✓
Hospital x Specialty FE	✗	✗	✗	✗	✗	✗	✓	✓	✓
Year x Specialty FE	✗	✗	✗	✗	✗	✗	✗	✓	✓
University FE	✗	✗	✗	✗	✗	✗	✗	✗	✓

Notes: This table presents the impact of late 2017 events on human capital. In all columns, the outcome variable, *Percentile*, measures the total score expressed in percentile rank, calculated separately for each exam year. Estimates are from the difference-in-differences specification in Equation 1. The underlying data are at the individual level and cover the period 2012-2024. Fixed effects are denoted at the bottom of the table. Standard errors are displayed in brackets and are clustered at region level. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table A6. Impact of political polarization on human capital by intensity of the treatment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
CAT x Post x Votes	-0.306***	-0.306***	-0.316***	-0.317***	-0.293***	-0.180***	-0.149***	-0.146***	-0.104***
	[0.000]	[0.001]	[0.004]	[0.004]	[0.004]	[0.003]	[0.001]	[0.004]	[0.014]
CAT x Post	-0.022	-0.022	0.018	0.005	-0.002	-0.060***	-0.054***	-0.059***	-0.071***
	[0.015]	[0.015]	[0.014]	[0.011]	[0.011]	[0.011]	[0.010]	[0.011]	[0.009]
CAT x Votes	0.000***	0.000	0.178***	0.175***	0.507***				
	[0.000]	[0.000]	[0.011]	[0.011]	[0.011]				
CAT	0.093	0.093	-0.031						
	[0.085]	[0.086]	[0.072]						
Post	0.025								
	[0.015]								
Adjusted R^2	0.001	0.001	0.651	0.684	0.707	0.747	0.804	0.815	0.824
Observations	89,897	89,897	89,897	89,897	89,897	89,897	89,771	89,771	89,750
Year FE	✗	✓	✓	✓	✓	✓	✓	✓	✓
Specialty FE	✗	✗	✓	✓	✓	✓	✓	✓	✓
Region FE	✗	✗	✗	✓	✓	✓	✓	✓	✓
Province FE	✗	✗	✗	✗	✓	✓	✓	✓	✓
Town FE	✗	✗	✗	✗	✗	✓	✓	✓	✓
Hospital x Specialty FE	✗	✗	✗	✗	✗	✗	✓	✓	✓
Year x Specialty FE	✗	✗	✗	✗	✗	✗	✗	✓	✓
University FE	✗	✗	✗	✗	✗	✗	✗	✗	✓

Notes: This table presents the heterogeneous impact of late 2017 events on human capital by intensity of the treatment. In all columns, the outcome variable, *Score*, measures the total score, standardized by year. *Votes* corresponds to the percentage of votes at the town level for secessionist parties in the September 2015 regional elections. Estimates are from the difference-in-differences specification in Equation 6. The underlying data are at the individual level and cover the period 2012-2024. Fixed effects are denoted at the bottom of the table. Standard errors are displayed in brackets and are clustered at region level. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table A7. Impact of political polarization on students' performance

	MIR score		Score	
	(1)	(2)	(3)	(4)
Cat_Uni x Post	-0.024	-0.025	-0.009	-0.011
	[0.034]	[0.033]	[0.033]	[0.032]
Adjusted R^2	0.110	0.121	0.104	0.115
Observations	89,876	89,876	89,876	89,876
Controls	×	✓	×	✓
Year FE	✓	✓	✓	✓
University FE	✓	✓	✓	✓

Notes: This table presents the impact of late 2017 events on the academic performance of students enrolled in universities located in Catalonia. In columns (1) and (2), the outcome variable, *MIR score*, measures the exam score, standardized by year. In columns (3) and (4), the outcome variable, *Score*, measures the total score, standardized by year. *Cat_Uni* denotes a dummy variable that takes the value of one for candidates from universities in Catalonia. The underlying data are at the individual level and cover the period 2012-2024. Fixed effects are denoted at the bottom of the table. Standard errors are displayed in brackets and are clustered at region level. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table A8. Impact of political polarization of human capital: Regional spillovers

	(1)	(2)	(3)	(4)	(5)
Panel A: Excluding regions with the highest inflows					
CAT x Post	-0.113***	-0.115***	-0.112***	-0.106***	-0.116***
	(0.009)	(0.009)	(0.011)	(0.013)	(0.010)
Adjusted R^2	0.821	0.822	0.808	0.802	0.806
Observations	81,185	79,503	62,442	48,036	43,764
Panel B: Excluding regions with the highest outflows					
CAT x Post	-0.113***	-0.109***	-0.104***	-0.103***	-0.105***
	(0.009)	(0.010)	(0.012)	(0.014)	(0.015)
Adjusted R^2	0.821	0.819	0.802	0.811	0.812
Observations	81,185	66,778	49,719	45,208	42,416

Notes: This table presents the impact of late 2017 events on human capital. In all columns, the outcome variable, *Score*, measures the total score, standardized by year. In Panel A, columns (1)-(5) exclude cumulatively those regions receiving the largest inflows of residents from Catalonia (in order, Valencia, Balearic islands, Madrid, Andalucia and the Basque Country). In Panel B, columns (1)-(5) exclude cumulatively those regions sending the largest outflows of residents to Catalonia (in order, Valencia, Andalucia, Madrid, Galicia, and Aragon). Estimates are from the difference-in-differences specification in Equation 1. The underlying data are at the individual level and cover the period 2012-2024. Fixed effects are denoted at the bottom of the table. Standard errors are displayed in brackets and are clustered at region level. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table A9. Impact of political polarization on human capital: Including control variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CAT x Post	-0.116***	-0.106***	-0.103***	-0.104***	-0.104***	-0.103***	-0.127***
	[0.026]	[0.026]	[0.026]	[0.026]	[0.025]	[0.026]	[0.031]
Positions by province		-0.152***	-0.108**	-0.106**	-0.106**	-0.106**	-0.109**
		[0.031]	[0.039]	[0.040]	[0.040]	[0.040]	[0.039]
Positions by region			-0.086**	-0.089**	-0.089**	-0.088**	-0.092**
			[0.035]	[0.035]	[0.035]	[0.033]	[0.033]
Female				-0.032***	-0.032***	-0.032***	-0.032***
				[0.004]	[0.004]	[0.004]	[0.004]
Foreign					0.007	0.007	0.006
					[0.010]	[0.010]	[0.010]
Wage						0.049	0.165
						[0.455]	[0.430]
Rental price index							1.126*
							[0.542]
Adjusted R^2	0.830	0.831	0.831	0.831	0.831	0.831	0.831
Observations	33,284	33,284	33,284	33,284	33,284	33,284	33,284
Year FE	✓	✓	✓	✓	✓	✓	✓
Specialty FE	✓	✓	✓	✓	✓	✓	✓
Region FE	✓	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓
Town FE	✓	✓	✓	✓	✓	✓	✓
Hospital x Specialty FE	✓	✓	✓	✓	✓	✓	✓
Year x Specialty FE	✓	✓	✓	✓	✓	✓	✓
University FE	✓	✓	✓	✓	✓	✓	✓

Notes: This table presents the impact of late 2017 secessionist events on human capital. In all columns, the outcome variable, *Score*, measures the position in the ranking, standardized by year. Estimates are from the difference-in-differences specification in Equation 1. To ensure consistency in the number of observations across all regressions in the table, the analysis uses individual-level data spanning the years 2012 and 2018–2022, years for which both wage and rent price data are available. Additionally, Navarre and the Basque Country are excluded due to the lack of data on rental prices in these regions. Control variables include the logarithm of the number of positions offered by specialty, region and year, and the logarithm of the number of positions offered by specialty, province and year. Fixed effects are denoted at the bottom of the table. Standard errors are displayed in brackets and are clustered at town level. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table A10. Alternative clustering methods

	(1)	(2)	(3)
CAT x Post	-0.118***	-0.118***	-0.118***
	[0.009]	[0.020]	[0.027]
Wild Bootstrap p-value			0.025
Adjusted R^2	0.824	0.824	0.824
Observations	89,750	89,750	89,750
Year FE	✓	✓	✓
Specialty FE	✓	✓	✓
Region FE	✓	✓	✓
Province FE	✓	✓	✓
Town FE	✓	✓	✓
Hospital x Specialty FE	✓	✓	✓
Year x Specialty FE	✓	✓	✓
University FE	✓	✓	✓
Cluster level	Region	Province	Town
Clusters (#)	19	52	174

Notes: This table explores whether alternative methods of clustering standard errors affect our baseline results. In all columns, the outcome variable, *Score*, measures the position in the ranking, standardized by year. Estimates are from the difference-in-differences specification in Equation 1. Column (1) display the baseline specification with the standard errors clustered at the region level, as in column (9) from Table 1. Columns (2) and (3) cluster the standard errors at the province and town levels, respectively. Since there are few regions, we also report a wild bootstrap p-value in column (3). *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table A11. Impact of political polarization on human capital: Aggregate evidence

	(1)	(2)
CAT x Post	-0.119***	-0.142**
	[0.026]	[0.053]
Observations	676	676
Year FE	✓	✓
Province FE	✓	✓
Estimation method	TWFE	SDID

Notes: This table presents the impact of late 2017 secessionist events on human capital at the province level, using alternative estimation methods. In all columns, the outcome variable, *Score*, measures the average total score, standardized by year and aggregated at province level. Estimates are from the difference-in-differences specification in Equation 1. The underlying data are at the province level and cover the period 2012-2024. Fixed effects are denoted at the bottom of the table. Standard errors are displayed in brackets. In column (1), standard errors are clustered at the region level, while in column (2), they are obtained using bootstrapping procedures. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table A12. Summary Statistics: Hospitals

	Catalonia		Rest of Spain	
	Pre'18	Post'18	Pre'18	Post'18
Admissions	7.58	7.66	8.26	8.26
	(1.79)	(1.72)	(1.67)	(1.67)
Trauma	0.08	0.10	0.12	0.14
	(0.10)	(0.12)	(0.11)	(0.12)
Surgery	0.14	0.16	0.21	0.24
	(0.15)	(0.17)	(0.13)	(0.15)
Internal	0.12	0.08	0.21	0.15
	(0.16)	(0.14)	(0.16)	(0.14)
Palliative	0.03	0.03	0.02	0.02
	(0.08)	(0.07)	(0.07)	(0.08)
Intensive	0.00	0.00	0.01	0.01
	(0.01)	(0.01)	(0.02)	(0.02)
Doctors	3.75	3.84	4.66	4.73
	(1.69)	(1.75)	(1.29)	(1.30)
Nurses	4.03	4.13	4.64	4.73
	(1.41)	(1.40)	(1.44)	(1.44)
Technicians (I)	4.25	4.33	4.62	4.72
	(1.00)	(0.99)	(1.23)	(1.24)
Technicians (II)	1.57	1.66	2.64	2.90
	(1.71)	(1.79)	(1.76)	(1.69)
Investment	12.03	12.59	11.60	12.51
	(3.07)	(2.42)	(4.02)	(3.58)
Purchases	13.12	13.27	14.47	14.63
	(3.07)	(3.25)	(2.46)	(2.54)
Beds	4.80	4.81	4.93	4.94
	(0.82)	(0.83)	(1.03)	(1.00)
N	906	302	2640	880

Notes: This table presents summary statistics by treatment status, before and after the shock. For each variable, it reports the mean and standard deviation (in parentheses). For a detailed description of each of the variables, see Table A1. The underlying data covers the period 2012-2019.

Table A13. Impact of political polarization on healthcare demand

	Catalonia					
	(1) Admissions	(2) Trauma	(3) Surgery	(4) Internal	(5) Palliative	(6) Intensive
CAT x Post	0.024	-0.006**	-0.009	0.011	-0.002	0.001
	[0.017]	[0.003]	[0.006]	[0.007]	[0.003]	[0.001]
Observations	4,728	4,728	4,728	4,728	4,728	4,728
Year FE	✓	✓	✓	✓	✓	✓
Hospital FE	✓	✓	✓	✓	✓	✓
Empirical strategy	SDID	SDID	SDID	SDID	SDID	SDID

Notes: This table presents the impact of late 2017 secessionist events on healthcare demand. *Admissions* is the logarithm of the number of admissions. *Trauma* is the percentage of admissions treated in the traumatology area. *Surgery* is the percentage of admissions treated in the surgical area. *Internal* is the percentage of admissions treated in the internal medicine area. *Palliative* is the percentage of admissions treated in the palliative care area. *Intensive* is the percentage of admissions treated in the intensive care area. Estimates are from the difference-in-differences specification in Equation 8 for the subsample of Catalan hospitals. The underlying data are at the hospital level and cover the period 2012-2019. Standard errors are displayed in brackets and are obtained using bootstrapping procedures. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table A14. Impact of political polarization on the number of doctors by age group working in Catalonia

	(1) <35	(2) 35-44	(3) 45-54	(4) 55-64
Catalonia	0.005	-0.031	0.011	0.026
	(0.027)	(0.039)	(0.028)	(0.040)
Barcelona	-0.026	0.005	0.039	0.002
	(0.061)	(0.099)	(0.037)	(0.061)
Girona	0.151**	-0.133***	0.126**	0.086
	(0.074)	(0.051)	(0.051)	(0.078)
Lleida	-0.015	0.037	0.036	0.019
	(0.074)	(0.076)	(0.055)	(0.081)
Tarragona	-0.092	-0.019	-0.156**	0.013
	(0.067)	(0.083)	(0.064)	(0.079)

Notes: This table presents the impact of late 2017 secessionist events on the number of doctors by age group. Estimates are from the difference-in-differences specification in Equation 9. The underlying data are at the hospital level and cover the period 2012-2019. Estimates shown in column (1) use data for the population of doctors below 35 years old. Estimates shown in column (2) use data for the population of doctors between 35-44 years old. Estimates shown in column (3) use data for the population of doctors between 45-54 years old. Estimates shown in column (4) use data for the population of doctors between 55-64 years old. Each row uses a different treatment group. Standard errors are displayed in brackets and are obtained using bootstrapping procedures. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table A15. Impact of political polarization on the extensive labor margin

	Catalonia			
	(1) Doctors	(2) Nurses	(3) Technicians (I)	(4) Technicians (II)
MIR x Post	0.018	0.011	-0.015	-0.074
	[0.013]	[0.014]	[0.016]	[0.053]
Observations	4,728	4,728	4,728	4,728
Year FE	✓	✓	✓	✓
Hospital FE	✓	✓	✓	✓
Empirical strategy	SDID	SDID	SDID	SDID

Notes: This table presents the impact of late 2017 secessionist events on the extensive labor margin, namely the number of hospital workers. *Doctors* is the logarithm of the number of doctors. *Nurses* is the logarithm of the number of nurses. *Technicians (I)* is the logarithm of the number of technicians with a high-level degree. *Technicians (II)* is the logarithm of the number of technicians with an intermediate-level degree. Estimates are from the difference-in-differences specification in Equation 8. The underlying data are at the hospital level and cover the period 2012-2019. Standard errors are displayed in brackets and are obtained using bootstrapping procedures. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table A16. Impact of political polarization on purchases and investment

	(1) Investment	(2) Purchases
MIR x Post	-0.154	-0.052
	[0.126]	[0.064]
Observations	4,728	4,728
Year FE	✓	✓
Hospital FE	✓	✓

Notes: This table presents the impact of late 2017 secessionist events on purchases and investment. *Investment* denotes the logarithm of total investments. *Purchases* denotes the logarithm of purchases of pharmaceutical material. Estimates are from the difference-in-differences specification in Equation 8. The underlying data are at the hospital level and cover the period 2012-2019. Standard errors are displayed in brackets and are obtained using bootstrapping procedures. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table A17. Impact of political polarization on hospital performance by diagnostic

	Death			LoS		
	(1) AMI	(2) Heart Failure	(3) Pneumonia	(4) AMI	(5) Heart Failure	(6) Pneumonia
CAT x Post	0.003**	0.008***	0.005***	0.047***	0.052***	0.018**
	[0.001]	[0.002]	[0.001]	[0.006]	[0.011]	[0.009]
Observations	426,542	838,703	909,050	426,542	838,703	909,050
Year x Month FE	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓
Age x Gender FE	✓	✓	✓	✓	✓	✓
Mean. Dep. Variable	0.056	0.088	0.068	6.479	7.244	6.853

Notes: This table presents the impact of late 2017 events on hospital performance by diagnostic. In columns (1)-(3), the outcome, *Death*, is a dummy variable that takes the value of one if the individual dies and zero otherwise. In columns (4)-(6), the outcome variable, *LoS*, measures in days the length of stay at the hospital. Columns (1) and (4) restrict the sample to patients diagnosed with AMI, columns (2) and (5) to those with heart failure, and columns (3) and (6) to those with pneumonia. Estimates are from the difference-in-differences specification in Equation 4. The underlying data are at the individual level and cover the period 2012-2019. Fixed effects are denoted at the bottom of the table. Standard errors are displayed in brackets and are clustered at region level. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table A18. Impact of political polarization on hospital performance: patient selection

	Death			LoS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
CAT x Post	0.006***	0.004***	0.005***	0.037***	0.022***	0.035***	0.040***
	[0.001]	[0.001]	[0.001]	[0.006]	[0.005]	[0.006]	[0.006]
Observations	2,174,312	1,977,159	2,070,811	2,174,312	1,977,159	2,070,811	1,992,065
Year x Month FE	✓	✓	✓	✓	✓	✓	✓
Diagnostic x Province FE	✓	✓	✓	✓	✓	✓	✓
Diagnostic x Year FE	✓	✓	✓	✓	✓	✓	✓
Age x Gender FE	✓	✓	✓	✓	✓	✓	✓
Mean. Dep. Variable	0.075	0.065	0.076	6.966	6.662	6.985	6.918

Notes: This table presents the impact of late 2017 events on hospital performance. In columns (1)-(3), the outcome, *Death*, is a dummy variable that takes the value of one if the individual dies and zero otherwise. In columns (4)-(7), the outcome variable, *LoS*, measures in days the length of stay at the hospital. Columns (1) and (4) present the baseline results without sample restrictions. Columns (2) and (5) restrict the sample to emergency admissions, while columns (3) and (6) include only patients residing in the same province as the hospital. Column (7) further restricts the sample to individuals who survive. Estimates are from the difference-in-differences specification in Equation 4. The underlying data are at the individual level and cover the period 2012-2019. Fixed effects are denoted at the bottom of the table. Standard errors are displayed in brackets and are clustered at region level. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table A19. Impact of political polarization on hospital performance: patient selection

	Death		LoS	
	(1)	(2)	(3)	(4)
CAT x Post	0.007***	0.005***	0.042***	0.051***
	[0.001]	[0.001]	[0.007]	[0.006]
Observations	1,644,615	2,048,526	1,644,615	2,048,526
Year x Month FE	✓	✓	✓	✓
Diagnostic x Province FE	✓	✓	✓	✓
Diagnostic x Year FE	✓	✓	✓	✓
Age x Gender FE	✓	✓	✓	✓
Mean. Dep. Variable	0.091	0.073	7.287	7.809

Notes: This table presents the impact of late 2017 events on hospital performance. In columns (1)-(2), the outcome, *Death*, is a dummy variable that takes the value of one if the individual dies and zero otherwise. In columns (3)-(4), the outcome variable, *LoS*, measures in days the length of stay at the hospital. Columns (1) and (3) restrict the sample to individuals aged 65 and older. Columns (2) and (4) restrict the sample to hospital stays of at least two days. Estimates are from the difference-in-differences specification in Equation 4. The underlying data are at the individual level and cover the period 2012-2019. Fixed effects are denoted at the bottom of the table. Standard errors are displayed in brackets and are clustered at region level. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table A20. Impact of political polarization on hospital performance: 2012-2022

	(1)	(2)	(3)	(4)	(5)
Panel A: Death					
CAT x Post	0.009**	0.006***	0.007***	0.007***	0.005***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
CAT	-0.012**	-0.012**			
	(0.005)	(0.005)			
Post	0.003				
	(0.003)				
Panel B: LoS					
CAT x Post	0.065***	0.055***	0.060***	0.058***	0.057***
	(0.008)	(0.010)	(0.006)	(0.006)	(0.006)
CAT	-0.088***	-0.087***			
	(0.025)	(0.025)			
Post	-0.044***				
	(0.008)				
Observations	2,813,636	2,813,636	2,813,636	2,813,636	2,813,634
Year x Month FE	×	✓	✓	✓	✓
Diagnostic x Province FE	×	×	✓	✓	✓
Diagnostic x Year FE	×	×	×	✓	✓
Age x Gender FE	×	×	×	×	✓

Notes: This table presents the impact of late 2017 events on hospital performance. In Panel A, the outcome, *Death*, is a dummy variable that takes the value of one if the individual dies and zero otherwise. In Panel B, the outcome variable, *LoS*, measures in days the length of stay at the hospital. Estimates are from the difference-in-differences specification in Equation 4. The underlying data are at the individual level and cover the period 2012-2022. Fixed effects are denoted at the bottom of the table. Standard errors are displayed in brackets and are clustered at region level. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table A21. Impact of political polarization on hospital performance: including stays over 28 days

	(1)	(2)	(3)	(4)	(5)
Panel A: Death					
CAT x Post	0.007***	0.007***	0.008***	0.007***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
CAT	-0.013**	-0.013**			
	(0.005)	(0.005)			
Post	-0.007***				
	(0.001)				
Panel B: LoS					
CAT x Post	0.142***	0.143***	0.153***	0.139***	0.138***
	(0.025)	(0.026)	(0.025)	(0.027)	(0.027)
CAT	-0.020	-0.020			
	(0.030)	(0.030)			
Post	0.003				
	(0.025)				
Observations	2,232,070	2,232,070	2,232,070	2,232,070	2,232,068
Year x Month FE	×	✓	✓	✓	✓
Diagnostic x Province FE	×	×	✓	✓	✓
Diagnostic x Year FE	×	×	×	✓	✓
Age x Gender FE	×	×	×	×	✓

Notes: This table presents the impact of late 2017 events on hospital performance including hospital stays longer than 28 days. In Panel A, the outcome, *Death*, is a dummy variable that takes the value of one if the individual dies and zero otherwise. In Panel B, the outcome variable, *LoS*, measures in days the length of stay at the hospital. Estimates are from the difference-in-differences specification in Equation 4. The underlying data are at the individual level and cover the period 2012-2019. Fixed effects are denoted at the bottom of the table. Standard errors are displayed in brackets and are clustered at region level. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table A22. Alternative clustering methods

	Death		LoS	
	(1)	(2)	(3)	(4)
CAT x Post	0.006***	0.006***	0.037***	0.037***
	[0.001]	[0.002]	[0.006]	[0.011]
Wild Bootstrap p-value	0.100		0.149	
Observations	2,174,312	2,174,312	2,174,312	2,174,312
Year x Month FE	✓	✓	✓	✓
Province FE	✓	✓	✓	✓
Age x Gender FE	✓	✓	✓	✓
Diagnostic x Year FE	✓	✓	✓	✓
Cluster level	Region	Province	Region	Province
Clusters (#)	19	52	19	52

Notes: This table explores whether alternative methods of clustering standard errors affect our baseline results. In columns (1) and (2), the outcome, *Death*, is a dummy variable that takes the value of one if the individual dies and zero otherwise. In columns (3) and (4), the outcome variable, *LoS*, measures in days the length of stay at the hospital. Estimates are from the difference-in-differences specification in Equation 4. Columns (1) and (2) display the baseline specification with the standard errors clustered at the region level. Columns (3) and (4) cluster the standard errors at the province level. Since there are few regions, we also report a wild bootstrap p-value in columns (1) and (3). *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.

Table A23. Impact of political polarization on hospital performance: data aggregated at different time frequencies

	Death(RA)				LoS(RA)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
CAT x Post	0.087***	0.080***	0.070*	0.054	0.022**	0.023**	0.028**	0.027*
	[0.030]	[0.030]	[0.038]	[0.051]	[0.010]	[0.011]	[0.012]	[0.016]
Observations	416	832	1,664	4,992	416	832	1,664	4,992
Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Province FE	✓	✓	✓	✓	✓	✓	✓	✓

Notes: This table presents the impact of late 2017 events on hospital performance at the province level for different time frequencies. In columns (1)-(4) the outcome variable, *Death(RA)*, measures the risk-adjusted death rate. In columns (5) and (8) the outcome variable, *LoS(RA)*, measures the risk-adjusted length of stay. For risk adjustment, we first use pre-treatment patient-level data to estimate a model of the hospital outcome, either *Death* or *LoS*, based on patients' demographics and diagnoses. We then predict the probability of death or the expected length of stay for each patient and aggregate these predictions at the province level. Finally, *Death(RA)* and *LoS(RA)* are defined as the ratio of the observed average death rate and length of stay to their respective predicted values at the province level. In columns (1) and (5), data is yearly aggregated; in (2) and (6), semiannually; in (3) and (7), quarterly; and in (4) and (8), monthly. Estimates are from the difference-in-differences specification in Equation 4. Fixed effects are denoted at the bottom of the table. Standard errors are reported in brackets and obtained using bootstrapping procedures. *, **, *** indicate significance at the 0.1, 0.05 and 0.01 levels, respectively.