The Effects of Intervening the Supply Side of Fraudulent Sick Leaves Market in Chile

PABLO A. CELHAY[1], NICOLÁS FIGUEROA[1], CARLA GUADALUPI[2], and BENJAMÍN OTEÍZA[1]

[1]PUC-Chile

[2]Universidad Andrés Bello

July 14, 2025

ABSTRACT. This paper examines the effects of a crackdown on fraudulent sick leave in Chile, where 176 doctors were sanctioned for issuing excessive certificates. Physician decisions are influenced by patient needs and incentives, often resulting in suboptimal care. We analyze whether audits and sanctions change doctor behavior, whether non-sanctioned doctors are affected, and how patient behavior responds. Using data on around 22 million sick leaves from January 2018 to October 2022, we apply difference-in-differences (DiD) and regression discontinuity in time (RDiT) methods. Results show a 40.49% reduction in sick leave issuance among sanctioned doctors (DiD) and decreases between 34.46% and 50.12% (RDiT). We also find spillover effects: non-sanctioned doctors reduced issuing sick leaves by 9.33% to 14.19% after the intervention. On the demand side, patients treated by sanctioned doctors experienced an 18.94% decline in sick leave usage, saving approximately $12.6 million for the public insurer. Overall, sanctions on doctors effectively reduced sick leave issuance, partly due to patients switching providers, highlighting how supply-side interventions can influence healthcare practices and patient behavior

CONTACT. Celhay: pacelhay@uc.cl. The data for this project was made available by FONASA a formal agreement between with Pontificia Universidad Católica de Chile in year 2019. The authors would like to thank María Isabel Romero and Francisco León for their valuable comments and participants at the BFI/UNAB workshop 2024 and TOI/ISCI Workshop 2024. The opinions expressed in this paper are those of the authors.

1. Introduction

Physicians play a pivotal role in health insurance schemes as their decisions affect patient behavior, healthcare consumption, and so affect the costs of the system and its efficiency (Arrow, 1965). Doctors' decisions and behaviors are influenced not only by patient needs but also by their own incentives and preferences (McGuire, 2000). As such, physicians may prioritize factors such as financial incentives, professional norms, or personal beliefs when making clinical decisions (Celhay et al., 2019; Clemens and Gottlieb, 2014; Cutler et al., 2019; Currie and MacLeod, 2020; Alexander and Schnell, 2024; Gaynor et al., 2004; Gertler and Kwan, 2024; Kolstad, 2013). While there is a substantial body of literature on factors influencing prescription behavior, a further understanding of physician agency in different settings is necessary to explore mechanisms and regulatory alternatives that can help reduce waste in medical care.

This paper investigates over prescription in the market of sick leaves. In particular we analyze the effects of intervening in the supply side of the sick leave market in Chile, where the authorities sanctioned 176 physicians for their excessive issuance of sick leave certificates, partially suspending their capabilities to issue them. While these physicians account for only 0,5% of total doctors they were responsible for more than 8% of total expenditures associated to medical leaves. This intervention occurs in the context of relatively high expenditure on sick leaves by the public insurer, an increasing trend of issuance over time, and a heavy concentration of issuing patterns by a few high-issuing physicians. Unexpected rising costs in sick leave have also become a concern in other OECD countries.[1]

In particular, we exploit the exogenous timing of the intervention to explore three main research questions: (1) Do audits and sanctions affect physician behavior? (2) Are there any spillover effects on non-sanctioned doctors? (3) How do these changes translate to alterations in patient behavior?

The data encompasses all sick leaves issued in Chile between January 2018 and October 2022, resulting in approximately 22 million observations. We focus on authorized, non-COVID, curative sick leaves issued after the beginning of the pandemic. Additionally, we use data on patient characteristics, covering the entire population in 2019 and 2020. Finally, we have partial information about doctor characteristics, including their specialty status and geographical distribution.

[1] See the following news articles Bloomberg, Fortune, and The Economist.

Our empirical strategy relies on a difference-in-differences (DiD) empirical model alongside a regression discontinuity in time (RDiT) approach to analyze the impacts on both sanctioned and non-sanctioned doctors. The DiD model is accompanied by a matching strategy to find a valid control for the sanctioned doctors. We match these physicians using patient, sick leave, and doctor characteristics, finding one nearest neighbor. As there may be spillovers, and the non-sanctioned matched doctors could be indirectly treated (for instance, receiving information about the sanctions), we rely on the RDiT strategy for each group of physicians, exploiting the exogenous timing of the intervention.

For the demand side, we focus on high-receiving patients, considering their previous exposure to sanctioned doctors, and estimate a DiD model, taking into account the non-random exposure to them, applying the methodology discussed in Borusyak and Hull (2023). For this, we exploit the fact that the sanctioned doctors distribute among the top issuers, but not all the top issuers were sanctioned. This allows us to simulate interventions based on the empirical distribution of sanctioned physicians among the issuance of sick leaves.

Results reveal a significant decrease in sick leave issuance among sanctioned doctors. In the DiD specification, we find a decrease of 40.49% in the weekly sick leaves post-intervention compared to the matched doctors. In the RDiT specification, we observe decreases ranging from 50.12% to 34.46%. Additionally, there is some evidence for spillovers. For the matched doctors of the sanctioned group using the RDiT model, we find decreases from 14.19% to 9.33% in the number of sick leaves after the intervention. There is no consistent evidence for the rest of the supply side, as we find small increases or small decreases.

On the demand side, when we compare fully exposed-to- sanctioned individuals to non-exposed individuals, we find a reduction of 18.94% in their consumption of sick leaves. For the average patient, the intervention reduces the number of sick leaves received by 1.89%. With a back-of-the-envelope calculation, this intervention is estimated to result in savings of approximately $12.6 million USD in one year for this group. Although the intervention impacted the volume of sick leaves issued, quantifiable savings for the public insurer suggest that high-receiving patients are capable of imperfectly substituting the issuance of sick leaves, indicating an unintended effect of the intervention.

This study adds to the existing research on what influences physician decision-making and how it affects healthcare costs and quality (McGuire, 2000; Celhay et al., 2019; Gaynor et al., 2004; Gertler and Kwan, 2024). We provide evidence that sanctions against doctors for over-prescription can lead to long-term reductions in sick leave issuance. Using the timing of the sanctions as an exogenous shock, we find significant decreases in physicians' issuing behavior. These results support the idea that audits and penalties are effective tools to change physician behavior and reduce waste in healthcare (Currie and MacLeod, 2020; Schnell, 2024).

On the demand side, we find that patients whose doctors were sanctioned used 18.94% fewer sick leaves, which results in lower costs for the public insurer (Pichler et al., 2021). Patients can switch doctors easily in systems without gatekeeping, which can reduce the long-term impact of supply-side policies (Hesselius et al., 2009; Godøy and Dale-Olsen, 2018). This suggests that policies combining regulation with measures to influence patient choices might be more effective at decreasing unnecessary sick leave use. Our findings show that physician sanctions can work, but their success depends on how easily patients can change doctors (Wagner et al., 2024; Daniels, 2020).

2. Medical sick leave system in Chile and stylized facts

In this section, we briefly describe the institutions related to sick leaves in Chile, altogether with the intervention in the issuance market that motivates this investigation.

2.1. Sick leaves in Chile and intervention

In Chile, the issuance of sick leaves is administered by two different institutions. On one hand, for private health system affiliates (ISAPRE), the same institution is in charge to overseeing the sick leave issuance. For public health system affiliates (FONASA), the *Comisión de Medicina Preventiva e Invalidez* (COMPIN) is the one responsible for administration and authorization (Decreto Supremo 3, 1984).

The implication of issuing a sick leave is that the worker is allowed to be absent from her job following the term set by the doctor. For these workers there is a work incapacity benefit (SIL).[2] This subsidy, conditional on meeting certain requirements of affiliation and contribution in the health system, supposes a payment during the

[2]SIL stands for *Subsidio por Incapacidad Laboral*, which translates to temporarily-disability-at-work benefit.

absence caused by the sickness. This income is paid in function of the sick leave length: if the sick leave length is less than 11 days, the first 3 days are not paid, but it is fully paid if the length is 11 days or more, considering the worker's net salary (Decreto con Fuerza de Ley 44, 1978).

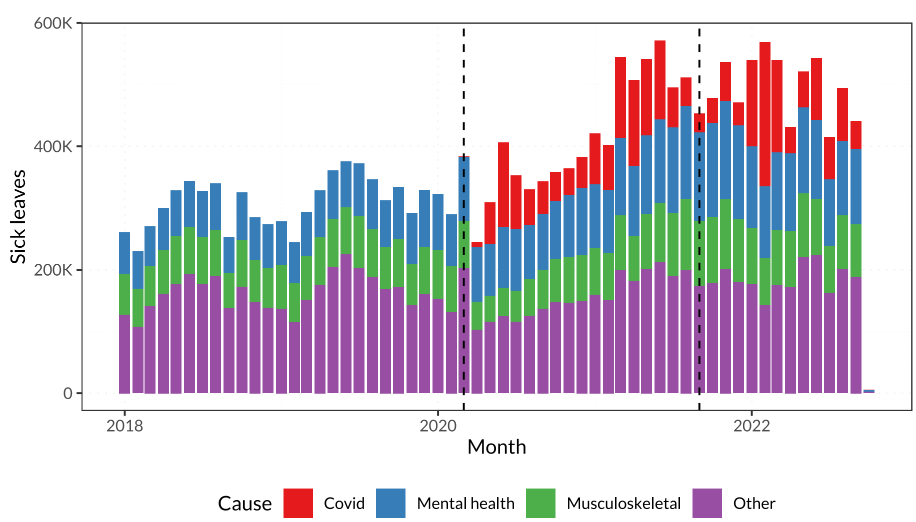
For the public health system, COMPIN can audit the issuance process. Specifically, they can ask the doctors the background information related to their issuance of sick leaves. If the doctor does not give an answer to COMPIN, they can, through a resolution, establish a monetary sanction implying potentially a suspension of issuance for 15 days (Ley 20.585, 2012). Furthermore, if they determine that the sick leaves do not have any justification, an investigation can be started. Because of this process, there can be more sanctions, that in the extreme case can mean an issuance suspension for one year, in addition to a financial penalty (Ley 20.585, 2012).

In this context, in September 2021, the COMPIN audited the sick leaves from high-emission doctors. If they were not able to justify their sick leaves, the COMPIN sanctioned them, suspending the electronic issuance system. In total, 188 were sanctioned, of which 176 can be observed in the database during the studied period.

2.2. Stylized facts

The issuance of sick leaves in Chile has not been constant. In figure 1 we show the monthly evolution of the sick leaves issuance from 2018 to October 2022.

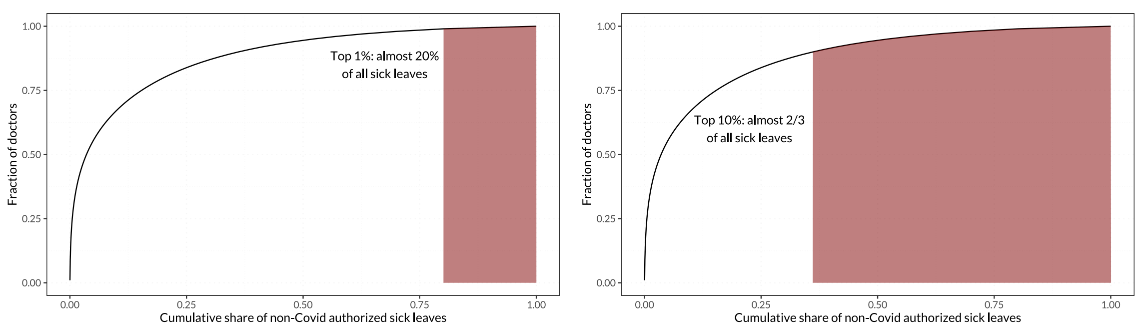
Figure 1: Issuance of authorized sick leaves



In the Covid-19 pandemic months, there is an increase in the number of sick leaves, especially at the beginning of 2021. Although at the beginning of the pandemic the growth in the issuance series is explained mainly by the Covid-19 sick leaves, when one excludes those sick leaves, the increase is still present with the pandemic advance.

Other relevant element of the sick leave market in Chile is its high concentration in a few doctors. In figure 2 we show the cumulative share of non-Covid sick leaves issued by each fraction of physicians. This data considers the period after the beginning of the pandemic and the month before to the intervention. We highlight the concentration of the top-1 and top-10% higher issuers in the market. As noted, the top issuers in this market concentrate a large proportion of the sick leaves issued. The top-1% issued almost 20% of all sick leaves, and the top-10% represent almost two thirds of the market.

Figure 2: Sick leave market concentration

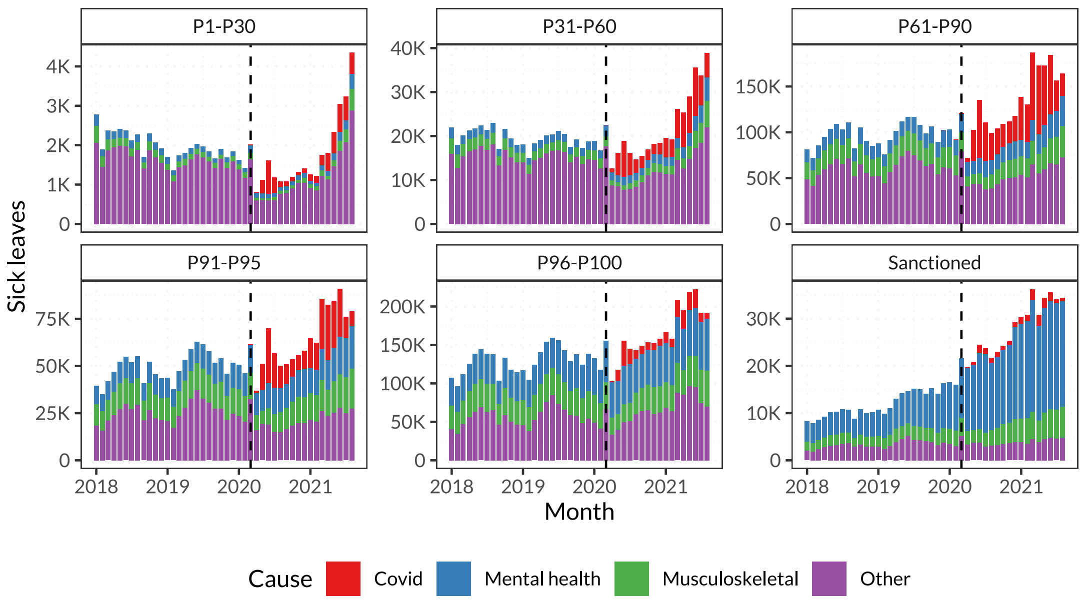


(a) Top-1% **(b)** Top-10%

Additionally, the doctors issue different kinds of sick leaves, depending how much they issue. In figure 3 we present the causes of the sick leaves issued by different groups of physicians. We separate these doctors in 5 percentile groups (p1 to p30, p31 to p60, p61 to p90, p91 to p95 and p96 to p100), and in a separate group the sanctioned doctors. We show four causes of sick leaves: (1) Covid-19, (2) mental health related, (3) musculoskeletal related and (4) the rest of causes, grouped as "other". Note that the mental health and musculoskeletal related are the top-2 causes for sick leaves issuance in the country.

One key aspect that is visible is that when one looks at high issuing doctors, they issue less Covid-19 sick leaves, and they issue more mental health related leaves. This

Figure 3: Causes of sick leaves by doctor issuance percentiles



pattern is more notable in the sanctioned physicians, where they almost have no issuance of Covid-19 leaves, and the proportion of mental health related ones is high. The latter is not true for doctors around the median, where the issuance of mental health related is much lower and the presence of Covid-19 sick leaves is present in a higher proportion.

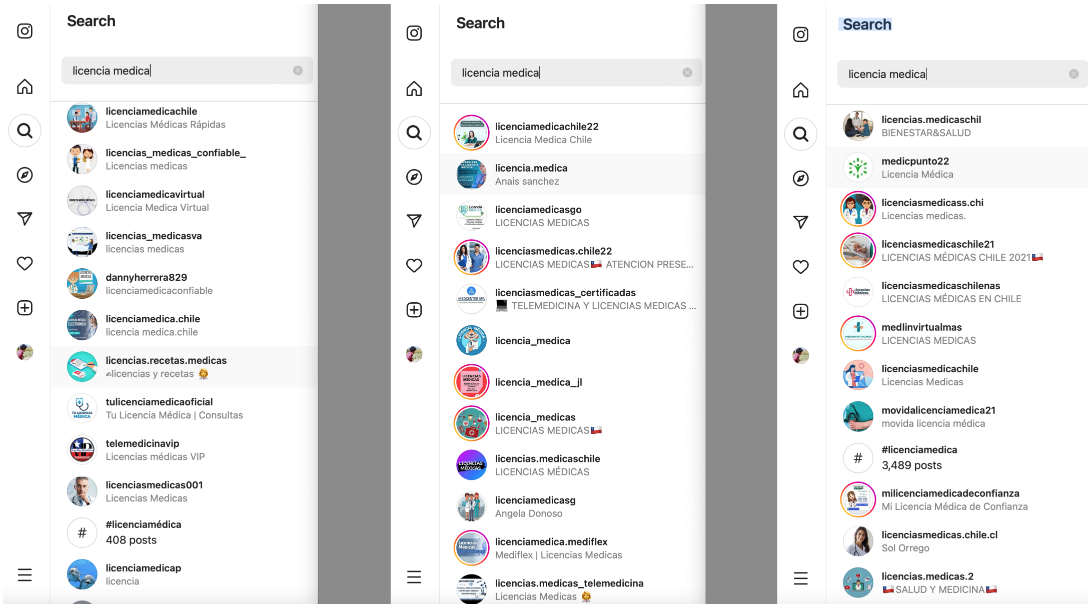
Finally, anecdotical evidence shows that medical sick leaves are being sold in a informal market with prices varying according to days of absence prescribed. In Figure 4 we show several profiles on Instagram that are dedicated to selling medical sick leave. In addition, several reports on the news shows that this is the case with sick leaves being sold for $30 thousand CLP for 11 days, $ 40 thousand for 15 days, $56 thousand for 21 days and $70 thousand for 70 days.[3]

3. Data

For the analysis of the intervention, we use data for all sick leaves issued between January 2018 and October 2022, provided by the public insurer, Fonasa, accounting for almost 22 million observations. This data considers the exact date of issuance,

[3]See this link.

Figure 4: Example of Instagram profiles that sell medical sick leaves



doctor-patient match, patient working place, diagnosis associated with the sick leaves, status (approved, rejected, or pending), type of sick leaves, issuance method, and days granted.

We additionally have information about the whole population in the country that is part of any health insurance system, for the years 2019 and 2020 (representing almost 22 million observations). Here we observe the gender, age, nationality, health insurer, titularity in the health insurance scheme, region and indicators for a set of pre-existing health conditions.

We focus on an specific subset of sick leaves. For our analysis, we consider the authorized, non-Covid-19 and type 1 sick leaves, issued since the beginning of the Covid-19 pandemic (March 2020). We exclude Covid-19 sick leaves because, in order to issue one, a doctor must have a certified test that proves the existence of Covid-19. This leaves much less room to fraudulent behavior. In addition, we consider type 1 sick leaves, that in Chile consider common diseases, and exclude the pregnancy related leaves. Finally, we consider the leaves since the beginning of the pandemic, in order to analyze an homogeneous period, considering the outbreak of Covid-19. In table 1 we present descriptive statistics at the doctor level before the intervention, considering if they were sanctioned or not. We include average patient characteristics and average characteristics of the sick leaves issued. As noted, the sanctioned doctors receive much more patients, and issue more leaves. Additionally, they issue relatively more sick leaves related to mental health, as, in average, they issue a proportion almost 4 times larger than the non-sanctioned ones. Finally, their patients are more concentrated in the Metropolitan macrozone (the largest in terms of population in the country).

Table 1: Descriptive statistics of physicians

	Non-Sanctioned		Sanctioned		
	Mean	SD	Mean	SD	p-value
Average patient characteristics					
Age	43.23	7.61	40.16	3.46	< 0.01
Woman (%)	0.550	0.266	0.584	0.094	< 0.01
Foreigner (%)	0.061	0.126	0.083	0.082	< 0.01
HI: FONASA A (%)	0.007	0.035	0.003	0.003	< 0.01
HI: FONASA B (%)	0.387	0.238	0.392	0.101	0.5
HI: FONASA C (%)	0.203	0.180	0.198	0.044	0.16
HI: FONASA D (%)	0.388	0.236	0.396	0.069	0.18
Non-titular	0.005	0.036	0.003	0.003	< 0.01
Macrozone: North (%)	0.071	0.232	0.037	0.157	< 0.01
Macrozone: Central (%)	0.136	0.308	0.079	0.230	< 0.01
Macrozone: Metropolitan (%)	0.423	0.446	0.625	0.420	< 0.01
Macrozone: Southern Central (%)	0.212	0.374	0.168	0.324	0.07
Macrozone: South (%)	0.126	0.307	0.069	0.224	< 0.01
Macrozone: Austral (%)	0.018	0.120	0.011	0.093	0.31
Macrozone: Unknown (%)	0.013	0.061	0.010	0.008	< 0.01
Mean pre-existing health conditions	0.479	0.412	0.355	0.093	< 0.01
Average sick leaves characteristics					
Sick leaves	116.4	278.6	2719.4	1351.2	< 0.01
Patients	72.5	159.9	1149.7	633.1	< 0.01
Sick leaves per patient	1.39	0.64	2.65	1.17	< 0.01
Mean days given	15.15	9.34	18.76	7.23	< 0.01
Disease: Mental health (%)	0.161	0.272	0.639	0.350	< 0.01
Disease: Musculoskeletal (%)	0.150	0.210	0.173	0.205	0.13
Disease: Respiratory (%)	0.084	0.167	0.047	0.105	< 0.01
Disease: Injury/Poisoning (%)	0.072	0.139	0.036	0.085	< 0.01
Disease: Digestive (%)	0.135	0.284	0.013	0.024	< 0.01
Disease: Nervous system (%)	0.031	0.089	0.029	0.060	0.59
Disease: Genitourinary (%)	0.055	0.154	0.008	0.023	< 0.01
Disease: Circulatory (%)	0.057	0.158	0.007	0.014	< 0.01

Notes: This table shows the average values of the patients and the sick leaves issued by each physician, considering if they were sanctioned or not. The last column is the p-value of a two-sided t-test of the mean values for sanctioned and non-sanctioned physicians.

4. Empirical Strategy

In this section, we describe the empirical strategy that we follow in order to estimate the causal effect of the intervention. We separate the analysis by the supply and demand side of the market, as we follow different strategies. The supply side section presents the econometric framework to estimate effects for sanctioned and non-sanctioned doctors, relying on difference-in-differences and regression discontinuity in time. By the demand side, we introduce the strategy to analyze the effects of the intervention on the high receivers, accounting for their nonrandom exposure.

4.1. Supply side

We start by the fact that sanctioned doctors are different from the rest of the supply in this market. Specifically, these physicians are characterized by a high issuance pattern. Our first approach to estimate the causal effect of the intervention is to use matching in order to find a proper control group.

We start using matching with one nearest neighbor, following Ho et al. (2007).[4] For the matching, we consider doctor sick leaves information and average patient characteristics to compute a propensity score, obtained the period before to the intervention. For the former, we consider the monthly issuance of sick leaves,[5] the total number of patients, the mean days given for leave, and the proportion of sick leaves given by each of the main group of diseases in Chile. For the latter, we include mean age of their patients, the proportion of women and foreigners, the proportion of each of the health insurers, the mean number of the main pre-existing health conditions, and the proportion of patients in each macrozone.[6]

In addition, we impose exact matching on two variables. We consider the medical specialty (binary) and the geographical region, to control for local market similarity. These variables comes from our incomplete database of medical professionals, so if we do not observe doctors, in the case of the specialty we assign a 0, and in the case of the region we assign them to an "unknown" category.[7]

We include as potential matched physicians all the doctors that are issued at least the same number of sick leaves than the minimum percentile of the sanctioned doctors (percentile 57). This makes that our pool of potential matched doctors are similar to the sanctioned ones in terms of total issuance of leaves.

The results of the matching procedure are displayed in tables A.3, A.4 and A.5 in

[4] In order to find the control group, we follow Ho et al. (2011).

[5] This allows us to observe a common trend of issuance for the control group.

[6] This allows us to account for spatial distribution of their market.

[7] Note that, as we include in the propensity score calculation the proportion of patients in each macrozone, this procedure operates as a robust way to account for market similarity, so the incompleteness of the data is not problematic for this purpose.

the online appendix. We observe that the matched doctors are almost statistically indistinguishable to the sanctioned ones. The only variables where we find statistical and economically relevant differences are in the number of patients and the number of sick leaves they issue before the intervention. This reinforce the fact that the sanctioned doctors are high issuers. In any case, when we observe the number of sick leaves per patient, we achieve balance between these two groups.

More prominently, we observe that sanctioned and clone (matched) doctors are systematically different from the rest of the market. Among almost every variable that we include in the matching, we do not observe balance, and we have economically significant differences. This corroborates the assumption about a structural behavioral gap between sanctioned doctors and the rest of the supply side.

In our empirical strategy, we split our analysis in two. We begin using the matched sample as a counterfactual group to estimate the impact of the sanction on the directly affected. Then, we study the response of each group independently, in order to account for potential spillovers of the intervention.

4.1.1. Difference-in-differences

Considering the sanctioned as the treated group and the matched doctors as the control, we estimate a standard difference-in-differences regression:

$$y_{dt} = \beta \left(\text{Post}_t \times S_d\right) + \phi_d + \phi_t + \varepsilon_{dt},$$

where y_{dt} is the number of sick leaves issued by a doctor d in the week t, Post_t an indicator equal to 1 the weeks before the intervention, S_d an indicator equal to 1 for the sanctioned doctors, and ϕ_d and ϕ_t are doctor and week fixed effects, respectively. We cluster the standard errors at the doctor level.

We also estimate an event study version of the latter equation, at a weekly level:

$$y_{dt} = \sum_{k=-79}^{60} \beta_k \left(S_d \times \mathbb{1}[k=t]\right) + \phi_d + \phi_t + \varepsilon_{dt},$$

where we have an estimator of β_k to each week before and after the intervention. We normalize β_{-1} to zero, so all comparisons are relative to the week before to the sanction.

The key identification assumption here is that, without the intervention, the issuance trends of the sanctioned and their matched clones would have evolved in a parallel way. Although this assumption is not testable, we could address the parallel trends between these two groups before the intervention. Specifically, we expect that the estimated β_k for $k < 0$ are indistinguishable to zero.

We estimate these two equations with OLS. As a robustness check, as the dependent variable follows a count model distribution, we estimate the regressions using a Poisson model.

4.1.2. Regression discontinuity in time

As the intervention occurs in a market context where information could flow between doctors, patients, media or health facilities, the possibility of externalities is present. In high issuers, the intervention could have had positive effects if there is substitution on the demand side, or a negative effect if they perceive a high cost of issuing a sick leave. This would lead the strategy presented in the latter section to be downward biased.

To abstract from this problem, we rely on the exogenous timing of the intervention. Exploiting the timing, we compare the doctors' trend and level of issuance before and after the intervention. By this, we estimate a regression discontinuity in time for each group (sanctioned, clones, and the rest of the market).

The main equation to estimate here is given by:

$$y_{dt} = \beta \text{Post}_t + \ell(t) + r(t) + \varepsilon_{dt},$$

where $\ell(t)$ and $r(t)$ are flexible polynomials of the running variable (time). At our main specifications, we consider polynomials of order 1 and 2, and a bandwidth of 60 weeks. To account for serial correlation at the doctor level, we cluster standard errors at that unit.

The key identification assumption for this strategy is that the intervention is exogenous, and doctors could not anticipate it. In a common regression discontinuity design setting, we could check for bunching, but as our running variable is time, we are not able to do so. Despite this, the intervention was effectively deployed at a non-anticipable time, so concerns about a behavioral response just before the intervention are unlikely to happen.

We implement different robustness checks, as suggested in Hausman and Rapson

(2018) for regression discontinuity in time settings. In particular, we estimate a donut regression discontinuity to account for additional concerns about anticipation. We estimate our same regression, excluding τ weeks before and after the intervention. We consider $\tau = \{2, 4, 6, 8\}$ weeks.

Additionally, we test for autoregression, considering our dependent variable. First, we test the existence of an AR(1). Then, we explicitly include the lagged dependent variable $y_{d,t-1}$ as a control in our main specification.

4.2. Demand side

As the number of individuals that demand medical sick leaves is considerable larger, and their presence is less frequent than the doctors, we focus on high receivers. We consider them as all patients that received 8 or more sick leaves the period before to the intervention. This number corresponds to the 90th percentile in the received sick leaves distribution in a time span of 18 months (on average, a sick leave each 2.25 months).

Patients have different degrees of exposure to the intervention. For instance, if a patient, whose doctor was sanctioned, would be more affected if she was the only doctor that she knew. On the contrary, if a patient had a more diversified portfolio of physicians, the potential effect (if any) of the intervention might be lower.

To account for this potential heterogeneity, we construct a measure of exposure e_i at the patient i level, defined as the proportion of sick leaves received from a sanctioned doctor during the pre intervention period.

With this measure of exposure, we would estimate a difference-in-differences model as it follows:

$$y_{it} = \beta \left(\text{Post}_t \times e_i\right) + \phi_i + \phi_t + \nu_{it},$$

where y_{it} is the number of sick leaves or the number of days received by the patient i in the period t, Post_t an indicator equal to 1 the period after the intervention, and ϕ_i and ϕ_t fixed effects at the patient and period level. The treatment here is continuous, defined as the exposure e_i. In estimating this equation we cluster the standard errors at the patient level.

The problem of the latter approach, is that, although the shock is exogenous, the previous exposure to sanctioned doctors is not. For example, patients that are always treated by a sanctioned doctor, chooses to be systematically treated by them.

Problems like this are discussed in Borusyak and Hull (2023). The main identification problem that they discuss is given by trying to estimate a relationship:

$$y_i = \beta x_i + \varepsilon_i,$$

where x_i is a treatment given a non-random exposure measure. Note, as in our context, there might be a good-as-random shock due to the timing of the sanction, but the exposure to them is not. Borusyak and Hull (2023) propose to exploit the randomness of the shock in order to compute an expected exposure μ_i to a shock in order to identify the causal effect of the shock. With this expected exposure, they propose to instrument the actual exposure with a residualized variable given by $\tilde{z}_i = x_i - \mu_i$. Alternatively, they suggest that controlling for the expected exposure μ_i would be a solution for the non-random exposure problem.

In our context, we exploit the randomness of the intervention. Note that, although the intervention is focused on high issuers, not all of them were sanctioned. For instance, from the top-10 issuers, 4 where sanctioned in a not clear order (for more details, see table A.6 in the online appendix).

In figure 5 we present the empirical probability of being sanctioned. As shown in the figure, the intervened doctors are mostly in the top percentile. Although, within the 100th percentile, one third of the doctors are sanctioned. By this, we argue that the intervention is as-good-as-random conditional on being a high issuer.

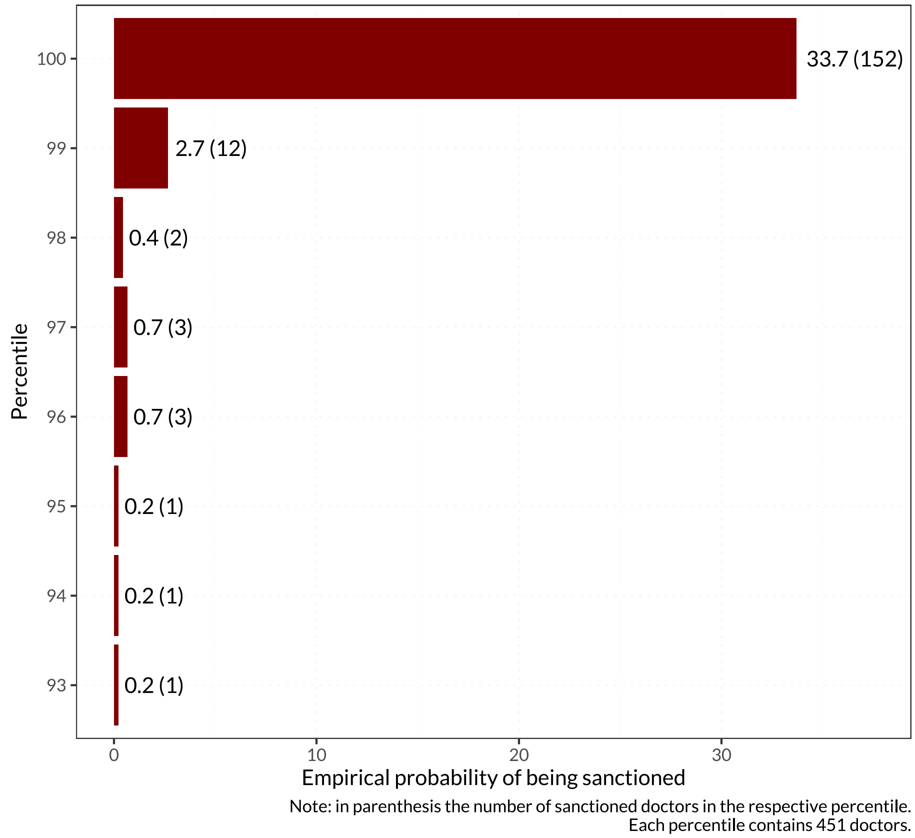
In order to compute the expected exposure to each patient, we simulate 1000 sanctions considering the empirical probabilities within each percentile. We use the following procedure: (1) for each percentile of issuance before the intervention, we select the number of actual sanctioned doctors without replacement s^p in each percentile p,[8] (2) then, we would obtain the exposure for each patient e_i^f (where f index each simulation), (3) iterate 1000 times, and (4) obtain μ_i as:

$$\mu_i = \frac{\sum_{f=1}^{1000} e_i^f}{1000}.$$

By this procedure, for each patient, we have an actual exposure to the intervention, and an expected exposure to an intervention of this nature. We present both

[8]Note that, in each percentile, we could select actually sanctioned doctors.

Figure 5: Empirical probability of being sanctioned



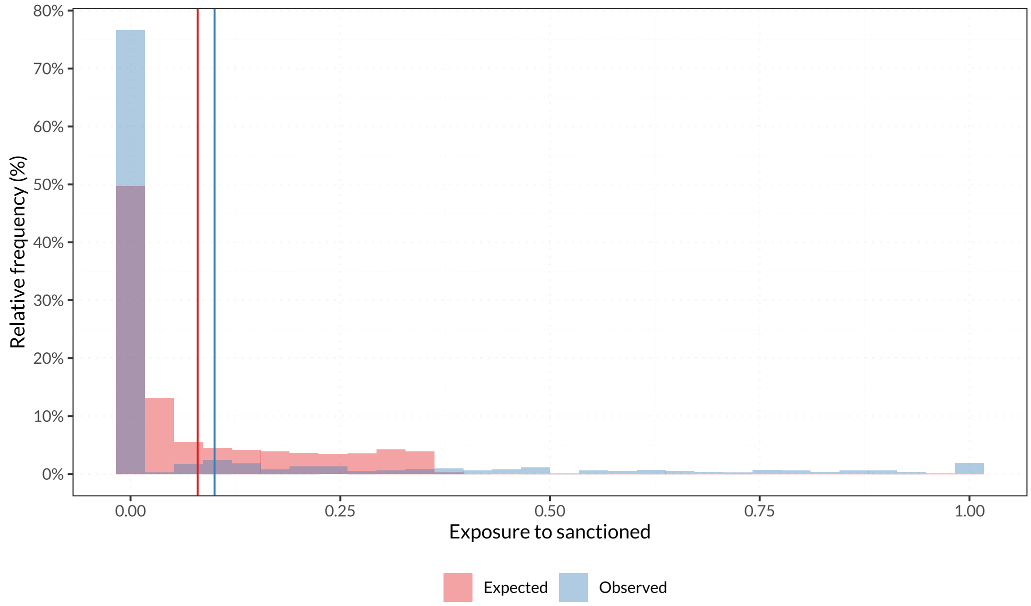
distributions in figure 6. In a more intuitive way, we interpret the expected exposure to the intervention as the degree of relationship of patients with high issuing doctors in a general sense, as the pool of doctors where the sanction occurred is given by those physicians. We observe that this expected exposure is, on average, lower than the actual exposure, and distributionally is less concentrated in the tails of the exposure distribution.

Considering this expected exposure μ_i, we take the two approaches suggested in Borusyak and Hull (2023). We start by estimating the effect of the intervention in high receivers by instrumental variables, considering as first stage:

$$\text{Post}_t \times e_i = \gamma \left[\text{Post}_t \times (e_i - \mu_i)\right] + \phi_i + \phi_t + u_{it},$$

where $e_i - \mu_i$ is the residualized instrument for the actual exposure e_i. Then, the

Figure 6: Observed and expected exposure to the intervention



Note: vertical lines represent the mean observed exposure (0.1) and expected exposure (0.08).

second stage would be:

$$y_{it} = \beta \left(\text{Post}_t \times \widehat{e_i} \right) + \phi_i + \phi_t + \nu_{it},$$

where $\widehat{e_i}$ is the predicted value of the exposure given the first stage. As an alternative, we estimate the naive approach presented at the beginning of this subsection, but controlling explicitly for the expected exposure μ_i interacted with the indicator Post_t, leading to the following equation:

$$y_{it} = \beta \left(\text{Post}_t \times e_i \right) + \gamma \left(\text{Post}_t \times \mu_i \right) + \phi_i + \phi_t + \nu_{it}.$$

Here, β is the causal effect of the intervention in the number of sick leaves/days in the high receivers. The main identification assumption comes from the exogeneity of the intervention. This leads to the instrument $e_i - \mu_i$ satisfying theoretically the exclusion assumption of instrumental variables.

5. Results

Following our different empirical strategies, in this section we describe the results of the intervention among the different analyzed groups. We start with the difference-in-differences specification, following with regression discontinuity in time for all doctors groups. Then we describe the results for patients (high receivers), following the strategy accounting for nonrandom exposure to the shock. In both empirical strategies on the supply side, we find large effects for the sanctioned doctors, and some evidence of impacts in their nearest neighbors, suggesting the existence of spillovers. We also find effects on patients that were more exposed to sanctioned doctors, but not as large in magnitude as the estimated by the supply side, implying that there is substitution between physicians on the demand side.

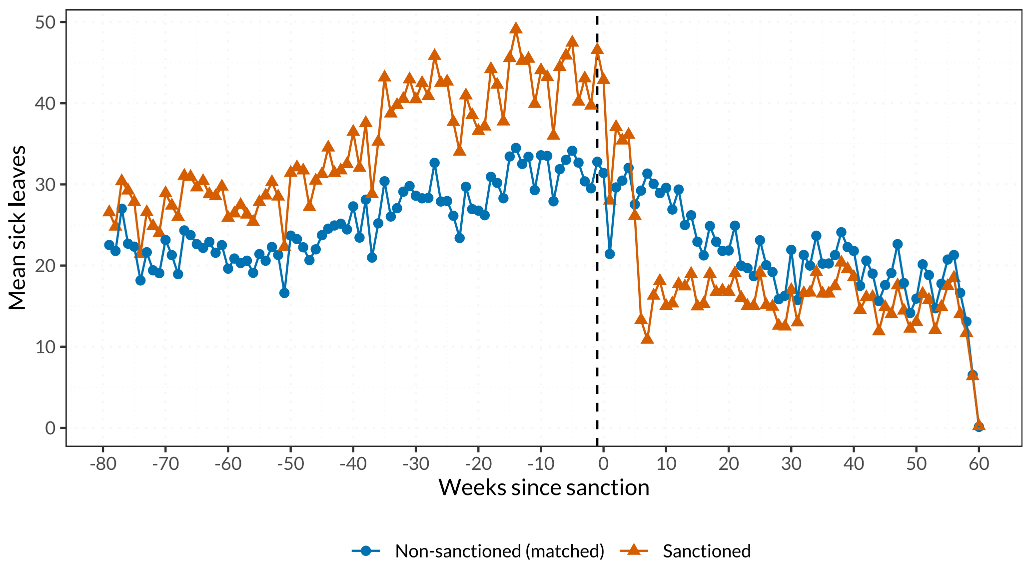
5.1. Supply side effects

We start by showing the weekly raw trends of issuance for the sanctioned doctors and their clones, presented in figure 7. Note that before the intervention, although sanctioned doctors issue on average more sick leaves, their mean trend is almost identical to the control group matched. After the sanction, we observe large immediate reductions for the directly affected doctors, not observable for the non-sanctioned clones. Around two months after this shock, the trend for the sanctioned doctors stabilizes, while the other doctors show a slightly declining tendency. By the beginning of 2022 (4 months after the intervention) both groups converged to an almost equal level.

In table 2 we show the results with difference-in-differences for the sanctioned doctors using the matched doctors as a counterfactual. Column 1 reports the effect estimating using OLS. We find consistent evidence with the graphical raw trends presented before. After the intervention, the sanctioned doctors reduced their weekly issuance by 13.2 sick leaves, which is statistically significant at conventional levels. This difference is almost equivalent to the gap between these two groups before September 2021, so, as described before, both groups converge to a similar level.

Column 2 presents the difference-in-differences estimation using Poisson. The results are qualitatively similar. We find a reduction of 40.49% on the number of sick

Figure 7: Trends of issuance for sanctioned and clone doctors



leaves for the sanctioned doctors after the intervention relative to their clones.[9] Again, this result is large and statistically different from zero.

Figure 8 shows the event study plots for the weekly specification. Panel (a) shows the OLS specification and panel (b) using a Poisson specification for count data. One relevant aspect that we observe in this figure plot is the absence of pre-trends before the intervention. Although this is not conclusive evidence about the parallel trends assumption, it is consistent with the assumption that we would expect a similar behavior in the absence of the intervention as they were behaving in a similar way before the intervention.

During the first weeks we observe a remarkable decrease in the number of sick leaves issued by the sanctioned doctors, relative to their matched group. This is consistent with the structure of the intervention, as these doctors had their issuing capacities partly suspended. As doctors could start (again) issuing sick leaves, their number is larger relative to the first weeks after the sanction, but lower than the level just before. This reductions are, again, in line with the observed trends shown in the raw evolution of issuance presented in figure 7. Note that, by the panel (b), we observe that the reductions are large as a percentage of the sick leaves issued, with a

[9]As we estimate a Poisson model, the percentage reduction is calculated as $e^{\beta} - 1$.

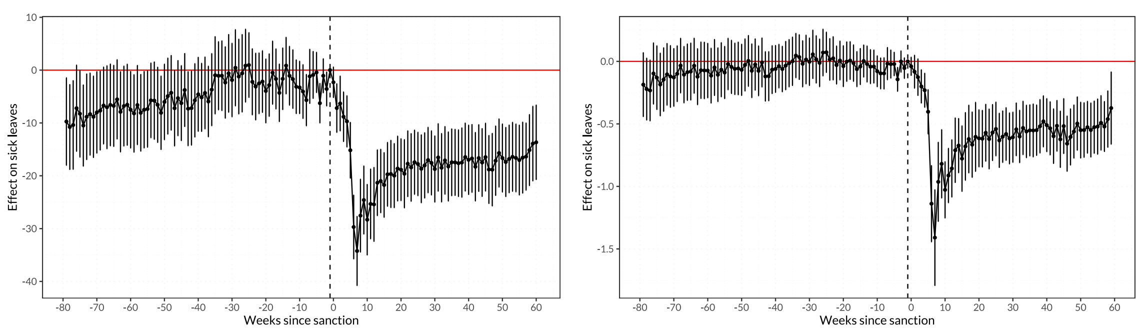
Table 2: Effect on sanctioned doctors (DiD)

	Sick leaves	
	(1)	(2)
	OLS	Poisson
Post × Sanctioned	-13.2***	-0.519***
	(1.58)	(0.075)
Pseudo R^2	0.067	0.412
Observations	49,280	49,280
Doctor fixed effects	✓	✓
Week fixed effects	✓	✓

Notes: clustered standard errors at the doctor level in parentheses. Both estimations include doctor and week fixed effects. Column (1) estimates using OLS, and column (2) with Poisson, to account for the count model distribution of the dependent variable. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

magnitude similar to that found in the difference-in-difference specification ($\approx 40\%$).

Figure 8: Effect on sanctioned doctors (event study)



(a) OLS **(b)** Poisson

Notes: the figure plots the coefficients estimated for each week before and after the intervention, normalizing $\beta_{-1} = 0$. Panel (a) uses OLS, while panel (b) uses Poisson. Both estimations include doctor and week fixed effects.

The difference-in-difference and event study specifications, considering the matched control group for the sanctioned ones show strong evidence in one line: the sanctioned doctors largely reduce their sick leave issuance after the intervention, and that this effect is sustained over time. By looking at the raw trends, we observe that the intervention led to a convergence in the number of sick leaves between these two groups.

As we described in the empirical strategy section, the latter estimations could be biased. The matched group, as they are by construction very similar to the sanctioned ones, could have change they behavior due to the sanction. In particular they could have reduced their issuance if they perceive a higher probability of being audited, or they could increase their issuance if they substitute the sanctioned doctors.

To deal with this, we estimate a regression discontinuity in time exploiting the random timing of the intervention. Table 3 shows the results. Columns 1 and 2 present the effect for the sanctioned doctors, columns 3 and 4 for the clones, and columns 5 and 6 for the rest of the doctors present in the market.

For the sanctioned doctors, the results show large reductions on the number of sick leaves issued after the intervention. In the linear specification, given by column 1, we find a reduction of 50.11% from the pre-sanction level. Column 2, that shows the effect with a quadratic polynomial, displays a decrease of 34.59% from the week before to the intervention. Both results are statistically significant at conventional levels. This result is consistent with the findings of difference-in-differences specifications. Beyond the level drop, we find a trend decline after the sanction, by a 0.55 to 0.59 less sick leaves by each week after September 2021.

The matched group of doctors shows an interesting pattern. After the intervention, column 3 shows a reductions of 14.18% compared to the level before the sanction, and in column 4 the results show a decrease of 9.33%. Both results are significant at the 10% level. We also find reductions of the post trend after the sanction, as in the directly affected doctors. This results confirms the spillover hypothesis: clone doctors that are observationally similar to the sanctioned change their behavior after the intervention. In particular, they decrease their issuance in a less dramatic way in comparison to the sanctioned ones.

Columns 5 and 6 shows evidence for the rest of the market. These doctors are substantially different in comparison to the sanctioned and clones (as showed in tables A.3, A.4 and A.5 in the online appendix). In column 5 the effect is an increase of 6.09% of the pre-sanction level, but in column 6 the effect is a reduction of 2.43%. We interpret this evidence as that there is no clear pattern of increasing or decreasing of the number of sick leaves in the market that should not be affected by the intervention which serves as a placebo empirical analysis. Note that these changes are, in absolute

Table 3: Effect on doctors (RDiT)

	Sick leaves week					
	Sanctioned		Clones		Others	
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-23.3293^{***}	-16.1014^{***}	-4.6480^{***}	-3.0583^{*}	0.1200^{***}	-0.0478^{***}
	(1.8423)	(1.8993)	(1.5036)	(1.6757)	(0.0177)	(0.0167)
Week trend	0.3310^{***}	-0.0082	0.2286^{***}	0.1152	0.0151^{***}	0.0201^{***}
	(0.0474)	(0.1210)	(0.0404)	(0.1089)	(0.0004)	(0.0010)
Post × Week trend	-0.5478^{***}	-0.5870^{***}	-0.4872^{***}	-0.4163^{***}	-0.0238^{***}	-0.0171^{***}
	(0.0744)	(0.1751)	(0.0638)	(0.1496)	(0.0006)	(0.0016)
Week trend^2		-0.0056^{***}		-0.0019		0.0001^{***}
		(0.0019)		(0.0015)		(0.0000)
Post × Week trend^2		0.0119^{***}		0.0026^{*}		-0.0003^{***}
		(0.0022)		(0.0015)		(0.0000)
Observations	21,296	21,296	21,296	21,296	5,422,131	5,422,131
Doctors	176	176	176	176	44,811	44,811
Pre–sanction level	46.55	46.55	32.78	32.78	1.97	1.97
Polynomial	1	2	1	2	1	2
Bandwidth	60.00	60.00	60.00	60.00	60.00	60.00

Notes: clustered standard errors at the doctor level in parentheses. Columns (1) and (2) shows the effect for the sanctioned doctors, with different polynomials. Columns (3) and (4) shows the effect for the clone doctors, and columns (5) and (6) for the rest of doctors in the market. Pre-sanction level is the mean value of sick leaves issued the week before to the intervention. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

value, lower than those found for the sanctioned and clone doctors, reinforcing the interpretation of spillovers for the doctors that are observationally similar to the main affected ones. For these doctors, again, we find decreases in the weekly trend of issuance after the intervention.

In tables A.1 and A.2 we conduct robustness exercises over the RDiT results, where we conduct a donut RDiT exercise and test for AR(1), respectively. The main results remains, where we observe a large decrease for the sanctioned doctors, a smaller decrease for the clone physicians, and relatively small changes for the rest of the market (with a non-consistent pattern).

Considering all these results, we find compelling evidence that the intervention had strong effects on the audited doctors. The immediate decrease is expected, as they could not issue sick leaves for some time. What is not obvious is that this decrease is sustained over time. Another non trivial result is that of the existence of spillovers: we find evidence that, at least for the most similar to the sanctioned doctors, there is a decrease the number of issued sick leaves after the intervention. Although the magnitude is smaller than the present for the sanctioned doctors, the sole decrease is a signal of wider effects than the direct ones found on the intervened.

5.2. Demand side effects

In this section we present the results for a subset of the demand side of this market. As described before, we focus on the top-10% higher receivers of sick leaves before the intervention.

Table 4 presents the effects considering the number of sick leaves received as our the main outcome of interest. Columns 1 and 2 estimate using what we call the naive approach, columns 3 and 4 instrument the exposure to sanctioned doctors with the difference between the actual exposure and the expected one, and columns 5 and 6 control for the expected exposure to sanctioned doctors. The first column within each estimation includes period and patient fixed effects, and the second one controls for patient characteristics interacted with the Post indicator.

The naive specification leads to large decreases in the number of sick leaves after the intervention for the more exposed patients, as observed in columns 1 and 2. Comparing a fully exposed with a non exposed, they receive approximately 1.6 less sick leaves after the sanction. In the Poisson estimation we observe a decrease of 39.35% in the number of sick leaves received, when we compare the fully exposed to the non exposed. This reduction is comparable to the reduction in the number of sick leaves issued by the sanctioned doctors. In any case, as presented in figure 6, the average exposure is 10%, so if we compare the average high receiver with a non-exposed, the reduction if about 3.94%. All these results are statistically different from zero and are robust to the inclusion of patient characteristics controls.

When we address the problem of nonrandom exposure to the intervention from the demand side, we find much lower estimates. The IV estimate is 43% lower in the OLS specification, and when we control for the expected exposure is 63% lower. In particular, when we compare a fully exposed to a non-exposed, we find a reduction on 0.91 sick leaves by the instrumental variable estimation, and a reduction of 0.59 sick leaves after the intervention controlling for the expected exposure. The Poisson specification shows very similar results: a reduction of 26.07% in the IV regression, and a reduction of 21.18% of sick leaves after the audit when we compare fully exposed to non-exposed, controlling for the expected exposure.

Table 4: Effect on number of sick leaves in high receivers

	Sick leaves					
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: OLS			
Post × Exposure	-1.527***	-1.598***	-0.919***	-0.912***	-0.635***	-0.591***
	(0.052)	(0.037)	(0.059)	(0.043)	(0.066)	(0.047)
Post × $\mathbb{E}$(Exposure)					-3.184***	-3.640***
					(0.145)	(0.109)
Observations	356,904	356,854	356,904	356,854	356,904	356,854
Patients	178,452	178,427	178,452	178,427	178,452	178,427
			Panel B: Poisson			
Post × Exposure	-0.494***	-0.500***	-0.282***	-0.271***	-0.230***	-0.210***
	(0.013)	(0.013)	(0.013)	(0.013)	(0.016)	(0.016)
Post × $\mathbb{E}$(Exposure)					-0.928***	-1.033***
					(0.033)	(0.034)
Observations	356,904	356,854	356,904	356,854	356,904	356,854
Patients	178,452	178,427	178,452	178,427	178,452	178,427
IV			✓	✓		
Period FE	✓	✓	✓	✓	✓	✓
Patient FE	✓	✓	✓	✓	✓	✓
Patient characteristics		✓		✓		✓

Notes: clustered standard errors at the patient level in parenthesis. IV columns instrument the exposure with the difference between the actual exposure and the expected exposure, computed as we describe in the empirical strategy section. The patient characteristics included in columns 2, 4 and 6 consider the number of sick leaves received before the intervention, age, indicator for main pre-existing health conditions, sex, nationality, an indicator for being registered in RSH, indicators for each health insurance scheme and indicators for each Chilean macrozone. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 5 shows a similar pattern when we look at the number of days granted instead of the number of sick leaves. The difference between different columns using OLS are not particularly large. Using Poisson we observe large reductions when we account for the nonrandom exposure to the intervention. In terms of magnitude, comparing a fully exposed to a non-exposed, we find reductions of approximately 29 days granted of sick leaves.

This result allows us to make a back of the envelope calculation. As reported in Superintendencia de Seguridad Social et al. (2023), a day of subsidy costs to the Chilean state \$22,926 CLP (approximately \$24.28 USD) in 2022. As the mean

Table 5: Effect on days of sick leaves in high receivers

	Authorized days sick leaves					
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel A: OLS			
Post × Exposure	-17.949***	-30.410***	-23.149***	-29.537***	-25.576***	-29.127***
	(1.285)	(1.104)	(1.420)	(1.232)	(1.584)	(1.365)
Post × $\mathbb{E}$(Exposure)					27.206***	-4.640
					(3.565)	(3.134)
Observations	356,904	356,854	356,904	356,854	356,904	356,854
Patients	178,452	178,427	178,452	178,427	178,452	178,427
			Panel B: Poisson			
Post × Exposure	-0.539***	-0.535***	-0.334***	-0.302***	-0.282***	-0.238***
	(0.015)	(0.015)	(0.015)	(0.015)	(0.018)	(0.018)
Post × $\mathbb{E}$(Exposure)					-0.887***	-1.046***
					(0.037)	(0.038)
Observations	356,904	356,854	356,904	356,854	356,904	356,854
Patients	178,452	178,427	178,452	178,427	178,452	178,427
IV			✓	✓		
Period FE	✓	✓	✓	✓	✓	✓
Patient FE	✓	✓	✓	✓	✓	✓
Patient characteristics		✓		✓		✓

Notes: clustered standard errors at the patient level in parenthesis. IV columns instrument the exposure with the difference between the actual exposure and the expected exposure, computed as we describe in the empirical strategy section. The patient characteristics included in columns 2, 4 and 6 consider the number of sick leaves received before the intervention, age, indicator for main pre–existing health conditions, sex, nationality, an indicator for being registered in RSH, indicators for each health insurance scheme and indicators for each Chilean macrozone. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

exposure in this group is 10%, the total savings would be of \$12.6 million USD in one year.

When we compare the results for the sanctioned doctors with what we obtain for the high receivers, there is one relevant finding. The most conservative estimate for the sanctioned doctors is given by a decrease of 34.59% in the number of sick leaves issued. By the demand side, a comparable group to the sanctioned are the fully exposed to the audited physicians. Comparing them to the non-exposed, in the larger estimate (in absolute value) we find a reduction of 26.07% of the number of sick leaves received. This means that the reductions are larger for the supply side than the most exposed part of the demand side. This could be evidence of substitution by the patients, given the shock produced by the intervention.

6. Conclusions

This study investigates physician agency, specifically how sanctions influence doctors' prescribing behaviors in the context of sick leave issuance, through a combination of difference-in-differences and regression discontinuity approaches. Exploiting the exogenous timing of sanctions to identify behavioral responses the results show that sanctions significantly reduce sick leave issuance—by approximately 40-50% among sanctioned doctors—and also lead to a nearly 19% decrease in patient sick leave consumption, translating into substantial cost savings for the public insurer.

Our results show that targeted sanctions and audits can effectively influence physician behavior, leading to a substantial reduction in high-volume sick leave issuance and associated costs. The empirical evidence indicates that doctors respond to regulatory interventions by decreasing their issuance of sick leave certificates, which in turn results in significant savings for public insurers. These findings highlight the importance of enforcement mechanisms, such as sanctions, in curbing excessive prescription practices and promoting more judicious use of healthcare resources.

Furthermore, the analysis reveals that patient behavior also plays a crucial role in mediating the impact of supply-side policies. Patients with access to sanctioned doctors tend to reduce their sick leave consumption, emphasizing the importance of considering patient mobility and choice within healthcare systems. Combining supply-side regulation with measures that influence patient decision-making could potentially enhance policy effectiveness.

References

Alexander, D. and M. Schnell (2024): "The impacts of physician payments on patient access, use, and health," *American Economic Journal: Applied Economics*, 16, 142–177.

Arrow, K. J. (1965): "Uncertainty and the Welfare Economics of Medical Care: Reply (The Implications of Transaction Costs and Adjustment Lags)," *The American Economic Review*, 55, 154–158.

Borusyak, K. and P. Hull (2023): "Nonrandom exposure to exogenous shocks," *Econometrica*, 91, 2155–2185.

Celhay, P. A., P. J. Gertler, P. Giovagnoli, and C. Vermeersch (2019): "Long-run effects of temporary incentives on medical care productivity," *American Economic Journal: Applied Economics*, 11, 92–127.

Clemens, J. and J. D. Gottlieb (2014): "Do physicians' financial incentives affect medical treatment and patient health?" *American Economic Review*, 104, 1320–1349.

Currie, J. M. and W. B. MacLeod (2020): "Understanding doctor decision making: The case of depression treatment," *Econometrica*, 88, 847–878.

Cutler, D., J. S. Skinner, A. D. Stern, and D. Wennberg (2019): "Physician beliefs and patient preferences: a new look at regional variation in health care spending," *American Economic Journal: Economic Policy*, 11, 192–221.

Daniels, B. (2020): "Primary care providers are, fundamentally, risk managers–And this is a challenge for health policy," *The Lancet Regional Health–Western Pacific*, 3.

Decreto con Fuerza de Ley 44 (1978): "Fija normas comunes para los subsidios por incapacidad laboral de los trabajadores dependientes del sector privado," .

Decreto Supremo 3 (1984): "Aprueba reglamento de autorización de licencias médicas por las COMPIN e instituciones de salud previsional," .

Gaynor, M., J. B. Rebitzer, and L. J. Taylor (2004): "Physician incentives in health maintenance organizations," *Journal of Political Economy*, 112, 915–931.

Gertler, P. and A. Kwan (2024): "The Essential Role of Altruism in Medical Decision Making," Tech. rep., National Bureau of Economic Research.

Godøy, A. and H. Dale-Olsen (2018): "Spillovers from gatekeeping – Peer effects in absenteeism," *Journal of Public Economics*, 167, 190–204.

Hausman, C. and D. S. Rapson (2018): "Regression discontinuity in time: Considerations for empirical applications," *Annual Review of Resource Economics*, 10, 533–552.

Hesselius, P., P. Johansson, and J. P. Nilsson (2009): "Sick of Your Colleagues' Absence?" *Journal of the European Economic Association*, 7, 583–594.

Ho, D. E., K. Imai, G. King, and E. A. Stuart (2007): "Matching as nonparametric preprocessing for reducing model dependence in parametric causal inference," *Political analysis*, 15, 199–236.

——— (2011): "MatchIt: Nonparametric Preprocessing for Parametric Causal Inference," *Journal of Statistical Software*, 42, 1–28.

Kolstad, J. T. (2013): "Information and quality when motivation is intrinsic: Evidence from surgeon report cards," *American Economic Review*, 103, 2875–2910.

Ley 20.585 (2012): “Sobre otorgamiento y uso de licencias médicas,” .

McGuire, T. G. (2000): “Physician agency,” *Handbook of health economics*, 1, 461–536.

Pichler, S., K. Wen, and N. R. Ziebarth (2021): “Positive Health Externalities of Mandating Paid Sick Leave,” *Journal of Policy Analysis and Management*, 40, 715–743.

Schnell, M. (2024): “Physician behavior in the presence of a secondary market: The case of prescription opioids,” *Working Paper*, 5, 383–410.

Superintendencia de Seguridad Social, FONASA, COMPIN, and Superintendencia de Salud (2023): “Estadísticas de Licencias Médicas y Subsidios por Incapacidad Laboral 2022,” .

Wagner, Z., M. Mohanan, R. Zutshi, A. Mukherji, and N. Sood (2024): “What drives poor quality of care for child diarrhea? Experimental evidence from India,” *Science*, 383, eadj9986.

Online Appendix

The Effects of Intervening the Supply Side of Fraudulent Sick Leaves Market in Chile

A. Additional Figures and Tables

Table A.1: Effect on doctors (Donut RDiT)

	Sick leaves week					
	Sanctioned		Clones		Others	
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Donut RD (2 weeks excluded)					
Post	-26.71^{***}	-21.85^{***}	-4.67^{***}	-2.48	0.18^{***}	0.02
	(2.14)	(2.71)	(1.69)	(2.18)	(0.02)	(0.02)
Observations	20,416	20,416	20,416	20,416	5,198,076	5,198,076
	Panel B: Donut RD (4 weeks excluded)					
Post	-30.02^{***}	-29.61^{***}	-5.41^{***}	-3.85	0.18^{***}	-0.01
	(2.35)	(3.36)	(1.79)	(2.55)	(0.02)	(0.03)
Observations	19,712	19,712	19,712	19,712	5,018,832	5,018,832
	Panel C: Donut RD (6 weeks excluded)					
Post	-30.31^{***}	-30.45^{***}	-5.46^{***}	-3.47	0.21^{***}	0.02
	(2.51)	(3.98)	(1.79)	(2.66)	(0.02)	(0.03)
Observations	19,008	19,008	19,008	19,008	4,839,588	4,839,588
	Panel D: Donut RD (8 weeks excluded)					
Post	-30.59^{***}	-31.31^{***}	-6.71^{***}	-6.96^{**}	0.18^{***}	-0.14^{***}
	(2.56)	(4.28)	(1.79)	(2.81)	(0.03)	(0.04)
Observations	18,304	18,304	18,304	18,304	4,660,344	4,660,344
Doctors	176	176	176	176	44,811	44,811
Pre–sanction level	46.55	46.55	32.78	32.78	1.97	1.97
Polynomial	1	2	1	2	1	1
Bandwidth	60.00	60.00	60.00	60.00	60.00	60.00

Notes: clustered standard errors at the doctor level in parentheses. Columns (1) and (2) shows the effect for the sanctioned doctors, with different polynomials. Columns (3) and (4) shows the effect for the clone doctors, and columns (5) and (6) for the rest of doctors in the market. Pre-sanction level is the mean value of sick leaves issued the week before to the intervention. Each panel excludes the indicated number of weeks before and after the intervention. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.2: Effect on doctors (RDiT); testing for AR(1)

	Sick leaves week					
	Sanctioned		Clones		Others	
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-23.13^{***}	-15.93^{***}	-4.60^{***}	-3.20^{*}	0.12^{***}	0.12^{***}
	(1.85)	(1.87)	(1.44)	(1.69)	(0.02)	(0.02)
Observations	21,147	21,147	21,134	21,134	5,383,388	5,383,388
Doctors	176	176	176	176	44,811	44,811
Pre-sanction level	46.55	46.55	32.78	32.78	1.97	1.97
Polynomial	1	2	1	2	1	2
Bandwidth	60.00	60.00	60.00	60.00	60.00	60.00
AR(1) coef.	0.32	0.32	0.43	0.43	0.52	0.52

Notes: clustered standard errors at the doctor level in parentheses. Columns (1) and (2) shows the effect for the sanctioned doctors, with different polynomials. Columns (3) and (4) shows the effect for the clone doctors, and columns (5) and (6) for the rest of doctors in the market. Pre-sanction level is the mean value of sick leaves issued the week before to the intervention. The AR(1) coefficient indicates the estimation of an AR(1) model for each group of physicians. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.3: Difference in characteristics of doctors' patients

	Mean sanctioned	Clones	Rest
Mean age	40.162	0.055	3.353***
		(0.367)	(0.262)
Women (%)	0.584	-0.005	-0.058***
		(0.01)	(0.007)
Foreigners (%)	0.083	-0.001	-0.027***
		(0.009)	(0.006)
HI: Fonasa C (%)	0.198	0.001	0.012***
		(0.005)	(0.003)
HI: Fonasa D (%)	0.396	-0.003	-0.026***
		(0.007)	(0.005)
Mean prexisting health conditions	0.355	-0.001	0.136***
		(0.01)	(0.007)
Observations	176	176	16,541

Notes: robust standard errors in parenthesis. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.4: Difference in geographical characteristics of doctors' patients

	Mean sanctioned	Clones	Rest
Macrozone: North (%)	0.037	-0.01	0.037***
		(0.015)	(0.012)
Macrozone: Central (%)	0.079	0.003	0.064***
		(0.024)	(0.017)
Macrozone: Metropolitan (%)	0.625	0.004	-0.219***
		(0.044)	(0.032)
Macrozone: Southern Central (%)	0.168	0.006	0.064***
		(0.035)	(0.025)
Macrozone: South (%)	0.069	-0.009	0.048***
		(0.022)	(0.017)
Macrozone: Austral (%)	0.011	0.004	0.007
		(0.011)	(0.007)
Macrozone: Unknown (%)	0.010	0.002	-0.001
		(0.001)	(0.001)
Observations	176	176	16,541

Notes: robust standard errors in parenthesis. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.5: Difference in sick leaves characteristics of doctors

	Mean sanctioned	Clones	Rest
Patients	1149.744	-290.835***	-983.081***
		(68.975)	(47.617)
Sick leaves	2719.381	-709.54***	-2450.875***
		(137.654)	(101.606)
Sick leaver per patient	2.654	-0.062	-1.033***
		(0.118)	(0.088)
Mean days given	18.763	-0.462	-2.003***
		(0.728)	(0.548)
Disease: Mental health	0.639	0.011	-0.408***
		(0.038)	(0.026)
Disease: Musculoskeletal	0.173	-0.009	0.048***
		(0.022)	(0.015)
Disease: Respiratory	0.047	0.001	0.03***
		(0.011)	(0.008)
Disease: Injury/Poisoning	0.036	-0.002	0.056***
		(0.008)	(0.006)
Disease: Digestive	0.013	0.005	0.043***
		(0.006)	(0.002)
Disease: Nervous system	0.029	-0.002	0.008*
		(0.007)	(0.005)
Disease: Genitourinary	0.008	0.001	0.027***
		(0.002)	(0.002)
Disease: Circulatory	0.007	0	0.032***
		(0.001)	(0.001)
Observations	176	176	16,541

Notes: robust standard errors in parenthesis. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table A.6: Top-10 issuers

ID doctor	SL before sanction	Percentile	Sanctioned
1	10,886	100	0
2	8,975	100	1
3	8,303	100	1
4	7,563	100	0
5	7,464	100	0
6	6,821	100	0
7	6,742	100	0
8	6,612	100	1
9	6,188	100	1
10	6,133	100	0