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PHYSICIAN LABOR SUPPLY, FINANCIAL INCENTIVES, AND ACCESS TO HEALTHCARE

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Physician labor supply, financial incentives, and access to healthcare

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Abstract

To empirically assess how physicians respond to financial incentives, we leverage a quasi-natural experiment in France where most GPs' fees are regulated. In 2017, a wide-scale regulatory change caused the price of a visit to increase from €23 to €25. Relying on granular claims data covering the universe of patients, doctors, and visits, we show that physician activity grew by nearly 9% after the price increase, yielding a unitary price elasticity of healthcare provision. The number of distinct patients examined increased substantially, while the provision of medical services per patient hardly changed, resulting in a slight increase in physicians' number of days worked. Drug prescription per patient is also shown to decrease, suggesting that the policy was cost-effective and enhanced access to healthcare, with limited adverse effects. Early-career physicians responded strongly to these financial incentives, while later-career physicians hardly changed their labor supply behavior.

Keywords: Physician labor supply; Financial incentives; Claims data; Access to healthcare; Medical spending.

JEL Classification: I11, I18, J44.

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1 Introduction

How do physicians react to financial incentives? The existing estimates, across countries and specialties, are (to say the least) dispersed. A precise answer goes beyond labor supply considerations since it impacts access to healthcare and individuals' health. All the more so that access to physicians is increasingly difficult in most OECD countries *not only* because of population aging, an increase of obesity, cancers, diabetes, as well as an expansion of other chronic diseases, including a deterioration of individuals' mental health, *but also* because healthcare availability tends to vary across space.

In the U.S. for instance, the government has designated primary care Health Professional Shortage Areas (HPSAs) to address such unequal access to healthcare by encouraging physicians to locate in deprived areas using various financial incentives. In France, a similar policy was introduced with limited success according to (Cour des Comptes, 2024).[1] Providing incentives on the extensive margin (namely, to create new practices in medically deprived areas, sometimes referred to as "medical deserts" especially when they are subject to pervasive primary care provider shortages) is likely to be more difficult than providing incentives on the intensive margin (providing more medical services, for those already practicing in these deprived areas).[2] To foster supply, doctors' unions and the public health insurance administration agreed on an increase in physicians fees, which went from €23 in 2016 to €25 in 2017, up to €30 in December 2024.

In this article, we exploit the increase in payments received after an office visit by a very large fraction (92%) of French general practitioner (GP)'s. Such payments moved from €23 to €25 after May 1st, 2017, a +8.7% price increase which we view as a quasi-natural experiment, and which allows us to assess how GPS react to financial incentives. This change was large-scale since each and every *secteur 1* (sector 1, hereafter) general practitioner (GP) working in France was affected by the reform. According to sector 1 regulations, GPs cannot charge any other price than the regulated one. By contrast, *Secteur 2* (sector 2 hereafter) GPs may charge higher prices, but do not benefit from any

[1] Pilvar (2024) studies a similar program in the UK.

[2] It is indeed difficult to induce current doctors to postpone retirement, and it takes time to train more graduates in medical schools. In France, for instance, the *numerus clausus* determined the maximal number of medical students who made it to their second year. This quota was set each year by government decree, this practice prevailing from 1971 to 2020. Attracting foreign doctors was seen as another option, even though the share of doctors with a foreign MD never exceeded 10%.

rebate on their payroll taxes. Hence, self-employed sector 2 GPs, as well as direct access specialists, and salaried doctors were not affected by this reform. Our empirical analysis will compare sector 1 GPs, the treated group, with various untreated comparison groups. As mentioned above, direct access specialists were not affected by the reform. We will also compare sector 1 GPs with those in sector 2. We will also exploit within-year time variations of sector 1 GPs' activity, considering 2016 as a baseline year and 2017 as a treated year.[3] We further check that our empirical strategy passes falsification tests, and we implement a matching approach before our DiD to assess its robustness with respect to the comparability of treatment and control groups.

Our analysis is based on granular claims data from the *Système National des Données de Santé* (SNDS), which records all doctor visits for all patients in France between 2016 and 2018. We further avail of exhaustive information on each and any healthcare provider in this country over that period. Exploiting such unique databases allows us to cover the universe of both patients and healthcare providers in the French system, and thus to have a complete picture of this market. To the best of our knowledge, this contrasts with numerous studies based on a subsample of patients or physicians. We observe sociodemographics of both patients and physicians, and we have information on doctors' legal status (self-employed, salaried, or mixed), specialty, not to omit practices (including their location at the municipality level). Claims data also contains information at the visit level on actual fees charged, as well as on the exhaustive list of drug prescriptions. Importantly, our data permits to obtain precise estimations within a causal inference framework like the one based on the exogenous shock mentioned above.

Equipped with that research design and such data, our identification strategies all conclude to an estimated price elasticity of healthcare provision of about 1, which is lower than the estimate reported for U.S. physicians by Clemens and Gottlieb (2014) based on the population of Medicare enrollees. By contrast to their patient- and procedure-based analysis, our empirical analysis is conducted from practice-level information at the exclusion of hospitals or healthcare centers. Availing of that information on physicians further allows us to investigate the heterogeneity of treatment effects with respect to specialty. For instance, we document that GPs, cardiologists, and psychiatrists notably differ in their

[3] The latter identification strategy is used by Brandily et al. (2021) to evaluate the impact of COVID-19 pandemics on various outcomes (mortality, consumption, savings, etc.).

response to stronger financial incentives: GPs increase their activity while psychiatrists decrease theirs and cardiologists hardly respond. Our findings also contrast with long-lasting empirical evidence of null, small, or even negative elasticities obtained when income effects are large. As a result, we estimate that physician income rose by nearly 18% following the regulatory change, and thus that medical visit spending increased by roughly the same amount since the GPs considered here are not allowed to charge excess fees (on top of the regulated rate that is reimbursed by the public health insurance). Interestingly, we are able to unravel the mechanisms by which sector 1 GPs have accordingly modified their behavior. In particular, we investigate whether they have adjusted their workload (measured in working days) or the intensity of care (proxied by the number of medical services provided to each patient). We also wonder whether access to healthcare (namely, the number of patients seen) has changed, as well as drug prescription and retirement behaviors (the extensive margin response). Our estimates indicate that the intensity of care hardly varied, but that the reform mostly enhanced access to healthcare in the sense that GPs have more patients after the reform. More precisely, they see more patients each day (presumably thanks to longer business hours or/and shorter visit duration), and their workload, measured in terms of working days, slightly increased. Also, GPs have more patients after the reform, whom they see as their regular GPs (*médecin traitant* in French and hereafter), consistent with these doctors admitting new patients, and becoming their *médecin traitant*. Overall, we find that travel times from patients to doctors did not substantially vary, even though new patients may be located slightly closer from physicians. Drug prescription per patient also diminished, especially as antibiotics are concerned, which is consistent with a positive correlation between the length of a visit and drug prescription. The reform has also induced concerned GPs to postpone their retirement. As regards the heterogeneity of treatment effects, two dimensions drive much of physicians' response to financial incentives: age and exposure to competition. Early-career GPs between 30 and 39 are twice as more price-sensitive as the average, while old-age GPs are almost inelastic. This marked heterogeneity along the life-cycle seems quite naturally explained by stronger income effects prevailing at older ages. In areas with a high medical density, where competition is fierce, GPs are also found to react more to the provision of stronger financial incentives. Overall, our results suggest that increasing doctors' fees per visit, which are low by OECD standards, could be a cost-effective public policy in order to enhance the access to healthcare. It should for instance be noted that the direct costs of higher healthcare provision are partially offset by a thriftier drug prescription. A more complete cost-benefit analysis is nonetheless beyond the scope of this paper since it would require monetizing patients' well-being related to the mere fact of going to the doctor, and the improvement (hopefully) of their health conditions as a consequence.

Literature At the source of the literature devoted to physician labor supply, Feldstein (1970), Brown and Lapan (1972), Fuchs and Kramer (1972) generally found small negative price elasticities of labor supply, i.e. backward-bending supply curves that are consistent with relatively strong income effects that dominate substitution effects. Yet those studies were based on aggregate time series data: they typically relied on small sample sizes, which raises concerns as regards the precision of the measure. Sloan (1975) used U.S. census data from 1960 and 1970, and concluded to small positive wage elasticities, on average, combined with a backward-bending labor supply curve for a minority of doctors at the top of the income distribution. The first causal evidence based on micro data dates back to Rizzo and Blumenthal (1994) who instrument for physicians' wages by experience, and focus on young physicians aged less than 40 in order to abstract from retirement considerations. Based on a measurement of labor supply in working hours, they find a short-run wage elasticity of 0.27, and a larger one for female doctors. According to their estimates, the income elasticity of hours amounts to –0.17, but is positive for female physicians, suggesting that leisure is an inferior good for them. Showalter and Thurston (1997) rely on another instrument for wages, namely exploiting geographic variation in the maximum marginal tax rate. They find that self-employed physicians have a 0.33 short-run wage elasticity, whereas employed physicians are inelastic. In Norway, Baltagi et al. (2005) leverage a natural experiment in which some male hospital doctors received a 15% wage increase while others did not. Their results point out to an elasticity ranging from 0.3 to 0.34. Another natural experiment in Scotland led Ikenwilo and Scott (2007) to estimate a 0.12 elasticity in the case of NHS hospital consultants. Other studies including Sæther (2005) and Andreassen et al. (2013) adopt structural approaches to estimate physicians' labor supply elasticity, and tend to find even smaller estimates. By contrast, Clemens and Gottlieb (2014) exploit a 1997 Medicare geographic boundary re-adjustment, which yielded area-specific price shocks in physician payments. They strikingly obtain a larger value for that elasticity of labor supply since a +2% increase in reimbursement rates is associated with a +3% increase in healthcare provision to the population of patients aged more than 65. In a later work, Gottlieb et al.

(2023) estimate a smaller value comprised between 0.2 and 0.4, though. In Canada where the typical fee per visit is €40, Fortin et al. (2021) find that an increase in the price of all clinical services caused physicians to supply less labor, consistent with both leisure being a normal good and income effects being large there. In France, where doctors' visits tend to be more affordable than in comparable OECD countries, the most recent evidence we are aware of is provided by Coudin et al. (2015) who also find a negative price elasticity of doctors' activity. However, their results are basically obtained from cross-sectional evidence, and their identification is local. This is in contrast with our longitudinal analysis that exploits a wide-scale natural experiment, which should confer a higher external validity to our findings.

The rest of the article is organized as follows. Section 2 presents the institutional background. Section 3 describes the data. Our empirical analysis is exposed in section 4. Section 5 contains our main results, an analysis of the heterogeneity of treatment effects, and various robustness checks including falsification tests. Section 6 discusses plausible mechanisms explaining our empirical findings as regards both physician and patient behavior in the healthcare market, and section 7 concludes.

2 Institutional setting

In France, one half of doctors are self-employed, including 60% of the 100,000 GPs, according to Table 7 in Appendix 2 of Cour des Comptes (2024), and 45% of the 125,000 specialists, according to Drees. Almost all of them, roughly 115,000, are fee-regulated by the *Caisse nationale de l'assurance maladie* (Cnam), the French public health insurance that provides both mandatory and universal healthcare. Each and any physician must opt for a payment system that is either sector 1, which corresponds to a fixed reimbursement rate set by the regulator, or sector 2, a more flexible regime whereby prices for medical services may exceed that rate. According to the *Conseil National de l'Ordre des Médecins*, the corporation of physicians, these unregulated fees should yet be chosen with *tact et mesure*, i.e., within 'ethical' limits. For instance, charging extra fees to low-income patients is prohibited. As a counterpart for regulated fees, sector 1 physicians benefit from reduced social contributions.[4] The access to sector 2 has been tightened since a 1990 reform studied in more details by Coudin et al. (2015): eligibility conditions require qualifying university teaching for at least 2 years, and some hospital practice. Besides, it is forbidden to switch from sector 1, now an absorbing state, to sector 2 (the reverse being possible, though). In practice, few GPs opted for the sector 2 before that reform, and even less afterwards. 85,000 physicians, namely more than 90% of GPs and a small majority of specialists, choose the sector 1 option while 30,000 opt for the sector 2.

The regulation of the French healthcare market ensures that patients' out-of-pocket (OOP) expenditures are negligible when they visit their regular general practitioner. As regards sector 1 physicians, the rate of an office visit is set by Cnam after a negotiation process with doctors' unions. Patients usually pay for medical services at the end of the visit, and are then reimbursed by Cnam. Exceptions include patients who benefit from *tiers payant*: these patients are exempted from upfront payment, depending on their socio-economic status and the coverage provided by their supplementary health insurance. In 2017, 4% of the French population was deprived of private health insurance.

To mitigate moral hazard concerns and to temper the increasing trend in health expenditures, a €1 deductible had been implemented by the public health insurance since 2005. By construction, this deductible implies that OOP expenditures are non zero. That deductible has doubled since March 31st, 2024.[5]

In 2016, a nationwide bargaining session, the so-called *Convention nationale 2016-2021*, took place between the regulator, i.e. Cnam, and doctors' unions. At the end of these negotiations, a statement was publicly released on August 25, 2016. It was then announced that a supplementary fee of €2 per visit would prevail from May 1, 2017 onward. Before that date, the reimbursement rate of an office visit amounted to €23. The reform evaluated in the present article thus increased that rate to €25 (Figure 1). We view this 8.7% increase in fees for medical services as an exogenous shock on doctors' financial incentives, namely as a quasi-natural experiment. The motivation for such an increase in

[4] Namely those that relate to family and health public insurance. Family payroll taxes may be discounted by 60, 75, or even 100%, depending on annual income. When the latter is below 1.4×€47,100, sector 1 physicians benefit from full exemption, and they enjoy a reduced 40% rate for the part of their income above 2.5×€47,100. (N.B. €47,100 is the reference threshold for social contributions and entitlements to social benefits.) Similarly, the health-specific payroll tax rate is 6.5%, but it may be reduced to 0.1%, still depending on income.

[5] Young individuals and foreigners, among others, are exempted from that deductible.

the reimbursed price of a visit is related to the fact that France lies at the lower bound of the range of comparable rates within the OECD. Belgium aside, where a similar €25 then prevailed, the average price of a medical visit was about €46. In Germany, Italy, or the Netherlands, the typical fee charged by a doctor was roghly €75. In the UK, medical consultations are complimentary in the public sector, but are more expensive in the private sector (between €95 and €315). The same prevails in Spain, but with a much lower rate in the private sector (about €30-€40, slightly below the corresponding rates in Portugal). As a counterpart for that increase of their reimbursed fees, doctors commit to prescribe less, even though this commitment may seem hard to enforce in practice.

Though the agreement concerned sector 1 GPs only, some sector 1 specialists were affected by that regulatory change, too. In France, healthcare pathways are such that reimbursement of specialists' fees is conditional on having a referral from a GP. This rule does not concern direct access specialists (pediatricians, ophthalmologists, gynecologists, and stomatologists), though, since patients may visit them and be fully reimbursed without any referral. Sector 1 specialists at the exclusion of direct access specialists are allowed to charge specific extra fees that increased by €2 from July 1, 2017 onward. Also, pediatricians have benefited from an increase in the reimbursed fee of their office visits, depending on (essentially decreasing with) the age of the child. Mandatory procedures specific to infants were raised by €8, as opposed to other procedures that experienced a €1 increase only, combined with no price change for pupils aged 6 to 16 (when not referred to by their regular GP). Other changes occurred for cardiologists and psychiatrists, but with a different timing than the one that prevailed for office visits to sector 1 GPs. Their fees were also increased by €2, but two months later on July 1, 2017 (Figure B9). To sum up, direct access specialists that are not pediatricians, namely ophthalmologists, gynecologists, and stomatologists, were unaffected by previous regulatory change.

Importantly as regards our identification strategy exposed below, the overall volume for healthcare has remained stable over the period, i.e. slightly above than 250 million visits per year (see Figure 2 in Cour des Comptes (2024)).

3 Data

Our empirical analysis is based on the *Système National des Données de Santé* (SNDS), a French comprehensive database, and the administrative source that records each and any medical service for every patient who ever utilizes healthcare. Indeed, these claims data cover the universe of both patients and healthcare providers in the French system. It contains detailed information on office visits, patients' home visits, or video visits, but also on prescription behavior, hospitalization, death, etc. Our observation period runs from 2016 to 2018, but we choose to exclude November and December 2018 from our sample because of right-censoring. Indeed, the processing time for claim files may last up to 40 days, and the date of record may thus well fall after the end of 2018. We mostly aggregate our data at the doctor-month level, though we also perform doctor-day analyses and also exploit information collected at the patient level. In January 2016, our working sample contains slightly more than 55,000 self-employed GPs, either fee-regulated (sector 1) or not (sector 2), defined as such based on their specialty identifier,[6] with a positive activity along the 34 months when they may be observed (see Appendix Figure A1). Activity is measured here by the number of medical services (office visits, patients' home visits, telemedicine[7]) provided by doctors. For identification reasons exposed below, our sample also includes more than 6,000 self-employed direct access specialists at the exclusion of pediatricians. We restrict our attention to doctors aged 30 to 79 practicing in mainland France, namely metropolitan France (i.e. without overseas) at the exclusion of Corsica. For technical reasons, we also impose that their practice be located in a municipality for which information on population and density is non-missing in databases provided by Insee, the French institute in charge of statistics. Similarly, for each of these municipalities, one should have a non-missing continuous index called APL (for *Accessibilité Potentielle Localisée*, that is, a location-based index for potential access to healthcare) that helps Drees, the statistical institute of the Department of Health define medical deserts. Our working sample contains about 1.7 million observations at the physician-month level. Depending on our identification strategy, self-employed direct access specialists (gynecologists, ophthalmologists, and stomatologists) who fulfill the above inclusion criteria may also be included in our sample.

[6]General medicine corresponds to codes 1, 22, and 23 in the corresponding classification.

[7]Remote visits were indeed marginal at the time, and remained stable over our observation period. The development of telemedicine was spectacular during the COVID-19 pandemics.

There is a pervasive trend to rarefaction of supply since the number of GPs keeps on decreasing over the period until about 54,000 in October 2018, and this diagnosis is in line with Cour des Comptes (2024). About 20% of these GPs are not active every month, as shown by Appendix Figure A2. An age-sex pyramid is provided by Figure A3, which indicates that female doctors are much younger than their male colleagues. It hence suggests that in the near future that profession will become more feminine, and this is confirmed by panel a) of Figure A4: the share of women increased from 35% in January 2016 to 39% in October 2018, i.e. an increase of that share by more than 11% (+4pp) within less than three years. Panel b) of the same Figure shows that a vast majority (about 92%) of GPs are sector 1 doctors.[8] We learn from panel c) that nearly 40% of GPs' practices are located in dense areas, and that about 1/3 of practices can be found in intermediate areas, while slightly less than 30% are in weakly dense areas. Practices in non dense areas are extremely scarce: they account for only 0.2% of the total.

The distribution of medical services provided per doctor is given by the histogram of Figure 2. A typical GP makes about 350 visits per month, yet there is some heterogeneity in this regard. The median number of visits per month is higher for men (Figure 3, panel a). It exhibits an inverted U-shape with respect to age (Figure 3, panel b), which is almost quadratic with an acme of nearly 400 monthly visits reached at 54 (Figure A10). Denser areas are seemingly associated with lesser visits (Figure 3, panel c), remembering that it is not statistically meaningful to put much weight on what happens in non dense areas.[9] A peak of doctors' activity[10] occurs every Mondays, and activity keeps on decreasing along the rest of the week with a rebound just before the week-end. From the viewpoint of care provision, Saturdays correspond to 1/3 of an ordinary weekly working day, and Sundays

[8]This share exhibits an upward trend, which is partly mechanical and due to the fact that women enroll more often in sector 1 than men.

[9]Activity tends to be higher in medical deserts (see Figure A11). Medical deserts are defined according to the classification into ZIP or ZAC areas, based on the APL. Almost 3/4 of the French population resides either in ZIP or in ZAC.

[10]See Figure 3, panel d) that does not separate GPs from specialists.

are rather anecdotal.[11]

As regards the evolution of GPs' activity over time, an imperfect measure for the aggregate demand for healthcare in the presence of capacity constraints, Figure 4 indicates that it remained rather stable, and, if anything, slightly decreased at a quiet pace from January 2016 to October 2018. This is again in line with Figure 2 in Cour des Comptes (2024).

A proxy for the intensity of care, the median number of medical services per patient (defined as the ratio of the number of medical services over the number of patients seen within a month), tends to be quite homogeneous around slightly less than 1.2 per month, regardless of the location of doctors' practices (Figure A15, panel c), but it monotonically increases with age (Figure A15, panel b) and it is higher for men (Figure A15, panel a).

4 Empirical strategy

4.1 Identification

Our research design relies on a policy-induced shock on doctors' financial incentives, the 2017 increase in the regulated price charged by sector 1 general practitioners for an office visit, which we view as a quasi-natural experiment. We adopt a differences-in-differences (DiD) approach, whereby we consider fee-regulated GPs as a treatment group. Our comparison group is composed of direct access specialists at the exclusion of pediatricians, i.e. gynecologists, ophthalmologists, and stomatologists, who were not concerned by this change in financial incentives. These specialists charge fees that are equally reimbursed to the patient whether or not she is referred to by her regular GP.

Table 1 compares the treatment with the comparison group in terms of observed physician characteristics like gender, age, location, and composition of patients. Direct access

[11]Figure A5 further confirms that 1/3 of doctors -again, that Figure relates to both GPs and specialists- work 6 days a week (hence including Saturdays, most of the time), 1/4 of doctors work 5 days a week, while 1/4 have a part-time activity in the sense that they work 4 days or less. On average, GPs work 22 days each month but, again, heterogeneity prevails in this regard. As before, being a male aged 50 to 59 and having a practice located in weakly dense areas is associated with a higher number of working days, both per month (Figure A7) and per year (Figure A8). It is also related to working more often on Saturdays (Figure A9). The number of distinct patients seen per month covaries similarly along those dimensions (Figures A13 and A14).

specialists considered here are more often women, they tend to be slightly older than sector 1 GPs, but most strikingly their practice is located in dense, urban areas, including Paris.

Our identification strategy rests on two assumptions: the absence of spillovers, namely the Stable Unit Treatment Value Assumption (SUTVA), an hypothesis stating that the comparison group was not affected by the regulatory change, and the common trend assumption (CTA), according to which all the outcomes we consider would have evolved similarly in both groups in the absence of that policy. These direct access specialists were not affected by the reform, regardless of their affiliation status with the public health insurance, i.e. of whether they belong to sector 1 or sector 2.[12] Since GPs and the above specialists do not compete in the same market because they provide medical services that are horizontally differentiated, we believe that violations of SUTVA are unlikely in the current setting. Figure 5 further confirms that the average visit fee charged by these direct access specialists has remained unchanged after May 1, 2017, which suggests that the latter have been unaffected by the regulatory change at stake.

Based on pre-event data, Figure 6 suggests that this assumption cannot be dismissed as regards our two main outcomes of interest, medical activity and health insurance spending since no differential pre-trend can be observed, an empirical assessment that indeed comforts the plausibility of the CTA in our setting.

As a robustness check and to alleviate concerns about any threat to identification, we also consider an alternate identification strategy related to other comparison groups: a first one composed of sector 2 GPs, and a second one made up of sector 1 GPs in 2016, i.e. one year before 2017 which now plays the role of the treatment. We further rely on these two comparison groups when we pay attention to specific outcomes for which direct access specialists would not be proper control groups. When looking at drug prescription behavior, it makes more sense to compare sector 1 with sector 2 GPs since ophthalmologists do not prescribe antidepressants, for instance; so does it when considering the intensity of care, proxied by the number of visits per patient, or travel times from patients to physicians. The same logic applies to retirement behavior, the share of patients seen as a *médecin traitant*,

[12]Salaried physicians might also be considered as a comparison group, since they were not concerned either by the regulatory change at stake. However, we found that the CTA was unlikely to hold in that case. Another identification strategy could have consisted in allowing for the whole universe of self-employed specialists to be a comparison group, but again the CTA seemed less plausible to us.

or the share of home visits, for which it is even more appropriate to perform comparisons among sector 1 GPs between 2016 and 2017.

Columns 1 and 2 of Table 1 compare sector 1 with sector 2 GPs in terms of observed characteristics. The latter are more often males, much older than sector 1 GPs (in fact, they are close to retirement), and their practice is very often located in dense, urban areas, especially in Paris.

An objection to the latter identification strategy could lie in a possible violation of SUTVA due to sector 2 GPs being indirectly affected by the reform, for instance because they compete in the same market as sector 1 GPs. Figure 7 shows that the average visit fee charged by sector 2 GPs has slightly increased by about €1.4 (about €38.7 before May 1, 2017 and €40.1 after that date) after May 1, 2017, which suggests that the latter have been somehow affected by the regulatory change at stake. Yet previous objection is debatable because the population of patients tends to be rather segmented in this respect, wealthier patients primarily going to sector 2 GPs. Our comprehensive data enable us to compute the number of patients going to a sector 2 GP among the pool of patients who visit at least a sector 1 GP. In 2016, that share does not exceed 0.8%. Second, to empirically assess the plausibility of SUTVA in our context, we wonder in section 5.3.1 whether sector 2 GPs have been affected by this regulatory change in terms of activity, relying on direct access specialists as a comparison group, and considering these sector 2 GPs as a 'treated' group. This falsification test enables us to verify that they did not seem to react to the reform, hereby comforting the empirical plausibility of SUTVA.

Based on pre-event data, Figure 8 suggests that the CTA cannot be ruled out as regards both medical activity and health insurance spending.

When we restrict our attention to the sole sector 1 GPs, we view the year 2017 as a 'treated' year and the year 2016 as a 'baseline' year', so that the comparison between both years permits to account for seasonality. Such a methodology has been recently used by, e.g., Brandily et al. (2021), Andersen et al. (2022), and Bonnet et al. (2022) to evaluate the impact of the COVID-19 pandemics on various outcomes (mortality, earnings, consumption, and savings, among others). It rests on the assumption that sector 1 GPs face identical monthly seasonality in 2016 and 2017, since SUTVA mechanically follows from differences in the timing. The empirical plausibility of that hypothesis can be assessed from pre-event data, as is the case with the CTA. Figure 9 suggests that this assumption is not verified when January is included, but that it is likely based on the sole February and March months. This Figure makes clear that sector 1 GPs have increased their activity in response to the regulatory change: the level of medical services provided the months after that change is identical in 2016 and 2017, while it is lower on February and March 2016 by comparison with February and March 2017. The same diagnosis prevails for health insurance spending. A short-term anticipation effect can also be observed in April 2017 where we see a drop in the activity of sector 1 GPs, followed by some spike in May 2017, which suggests that physicians postponed some visits from April to May in order to take advantage from higher regulated fees. This adjustment behavior is all the more likely for elective medical services since doctors were clearly incentivized to postpone non-emergency encounters so that they could benefit from the extra €2 per visit. In all subsequent estimations, our preferred specification is obtained when we exclude this 2-month window from our sample in order to get rid of that intertemporal substitution phenomenon, even though including it or not does not dramatically alter our results. In a similar vein, we check that our estimates are not driven by the sole extensive margin, i.e. by the mere fact that some physicians might have been discouraged to exit, or incentivized to enter the healthcare market following the reform. To that aim, in each estimation we impose, or not, that physicians be present in both January 2016 and October 2018.[13]

Last, when we evaluate the causal impact of the regulatory change for sector 1 cardiologists and psychiatrists, we rely on their sector 2 counterparts (see Figure B10 for an eyeball assessment of the CTA).

4.2 Econometric specification

Denoting various outcomes (e.g., physician activity, health insurance spending, drug prescription, etc.) by Y, and indexing doctors by j and time available at a monthly frequency by t, we consider the following linear model:

$$Y_{jt} = \beta \; T_j \times \mathrm{Post}_t + \alpha_j + \gamma_t + \varepsilon_{jt}, \tag{1}$$

[13] Being more conservative and imposing that doctors be active each of the 34 months observed, hence considering a balanced panel of doctors along the period, would only lead to a substantial drop in the size of our estimation sample (doctors may take holidays that exceed one month in the Summer), without changing our results either: see below for the corresponding robustness check.

where T_j is a binary indicator for a doctor j belonging to the treatment group, Post_t is a dummy equal to 1 after May 2017, α_j is a physician fixed effect, γ_t are month-year fixed effects, and ε_{jt} is an idiosyncratic shock. Standard errors are clustered at the physician level. The exogeneity of T_j results from the change in the fee per visit being decided by the policy-maker. In this context, our parameter of interest is the coefficient β. Since most of our dependent variables are expressed in natural logarithm, the parameter obtained corresponds to a semi-elasticity: it measures the response of the outcome considered to the treatment, in log points. To the extent that β is small, it approximately accounts for that relative response, in percentage points. When we investigate for the heterogeneity of treatment effects, we specify $\beta \equiv \beta(X_j)$ where X_j designate doctor j's covariates (gender, age, location, 2016 income, etc.).

We also perform an event study analysis in which we allow for this coefficient to further vary over time, namely to be month-specific over the whole period:

$$Y_{jt} = \beta_t \times T_j + \alpha_j + \gamma_t + \varepsilon_{jt}, \tag{2}$$

where t_0 designates the time of the treatment, May 2017, viewed as the event, and where we normalize $\beta_{t_0-1} = 0$ if. Testing the null hypothesis $H_0 : \beta_h = 0,\ \forall h < t_0 - 1$ provides a statistical test for the absence of any differential pre-trend across treatment and comparison groups. The post-reform coefficients $\beta_h,\ \forall h \geq t_0$ provide us with dynamic (monthly) treatment effects. However, as mentioned by Coudin et al. (2015), a possibly group-specific seasonality suggests to interpret any potential month-by-month difference with caution. Note also that it helps explain why previous statistical test may be rejected in the data while not jeopardizing the core of our identification strategy, though. For this reason, we rather consider the homogeneous β as the relevant policy parameter.

Last, in the case when the treatment group corresponds to the year 2017 and our control group to the year 2016, our estimating equation writes as:

$$Y_{jmy} = \beta T_y \times \text{Post}_m + \nu_{jy} + \mu_m + \eta_{jmy}, \tag{3}$$

where $m = 1, \ldots, 12$ accounts for a given month of a year, Post_m is a dummy equal to 1 when $m \geq 5$, and y is the year considered, either 2016 or 2017, so that $T_y = \mathbb{1}\{y = 2017\}$. Depending on whether one wants to retrieve static or dynamic treatment effects, one shall allow the parameter β to be month-specific or not. In a different vein, it is possible to specify either doctor ν_j or doctor-year ν_{jy} fixed effects.

5 Results

We now provide an evaluation of the previous regulatory change on various outcomes: activity (measured in medical services), its margins (activity per patient, and number of patients), health insurance spending,[14] drug prescription and health spending, workload (measured in working days), and the probability of retirement (somehow the extensive margin). For each outcome considered, the empirical plausibility of the CTA can be assessed by considering the respective evolutions in both treatment and comparison groups, which we plot in Appendix Figures B1, B2, and B3. We emphasize that, within the causal inference framework exposed previously, our granular and comprehensive data permit to obtain precise estimations of the treatment effects considered. We first present our main results before investigating the heterogeneity of treatment effects.

5.1 Main results

5.1.1 GPs

Table 2 displays the results issued from our main identification strategy based on the comparison between sector 1 GPs and direct access specialists. The estimated coefficient, which can approximately be interpreted as the price semi-elasticity of physician activity, is comprised between 8.8 and 9.7, our favorite estimate from Column 2, depending on whether the 'anticipation window' of April and May 2017 is excluded, and whether our attention is restricted or not to the sole physicians present in both January 2016 and October 2018. Following the 8.7% increase in fees per visit, sector 1 GPs have increased their activity, measured by the number of medical services provided, by 10.1% in our preferred specification (column 2). This empirical finding points out to a price elasticity of activity that is slightly above 1.1. As a consequence, health insurance spending rose by almost 19% (Table 3). In other words, when the public health insurance considers increasing GPs' regulated fees by 1%, it should almost forecast a 2% rise in visits' reimbursement rates.

[14]Sector 1 GPs' income roughly coincides with health insurance spending since their fees are regulated and fully covered, a €1 deductible per visit aside.

This result is certainly important from a policy viewpoint. In particular, it contrasts with the one previously obtained by Coudin et al. (2015) in France, which pointed out to a negative elasticity, even lower than -1,[15]. It is yet closer to Clemens and Gottlieb (2014) who found a medium-run price elasticity of 1.5 for US doctors in charge of Medicare patients. It should perhaps not be surprising that more attention is paid to older patients, and that the reaction is even stronger in that case, but we insist on the facts that our finding holds for the whole universe of both doctors and patients, and that our identification has a more substantial external validity than, e.g., in Coudin et al. (2015) who relied on a specific sample of compliers.

Interestingly, the decomposition of previous effect into two margins, the access to healthcare (proxied by the number of patients) and the intensity of care (proxied by the number of services per patient) in Table 3 reveals that concerned doctors have mostly increased the number of patients seen each month (the magnitude of the effect, 10.5 log-points, being almost the same as the effect on activity), but that the number of medical services provided per patient hardly varied (the effect is positive and statistically significant at usual levels, but small from an economic viewpoint: +0.2 log-point).[16] The latter finding suggests that supply-induced demand mechanisms have been limited, and hence that the quality of care was not substantially altered by the regulatory change at stake.

To further dig into the mechanisms that led them to admit more patients each month, sector 1 GPs see more patients each day (that number increasing by nearly 5.2% after May 1, 2017), while their workload increased by 4.3% (the number of monthly working days increasing accordingly). These empirical findings suggest that these doctors were responsive to stronger financial incentives, adjusted their labor supply at the intensive margin, and might have shortened the duration of a visit or/and worked longer hours each day.

We further investigate whether this regulatory change induced doctors to postpone

[15] In this article, the authors found that activity rose by 41% following a 54% price decrease.

[16] For this outcome, we rely on sector 2 GPs as a comparison group since the plausibility of the CTA is higher than with respect to direct access specialists: see panels f of Appendix Figures B1 and B2.

their retirement.[17] Based on a linear probability model and on sector 1 GPs in 2016 as the sole relevant comparison group for this outcome (see panel h of Appendix Figure B3), we find that the corresponding coefficient is significant at 5%. The magnitude of the effect is not large, about 0.6 pp, but when compared with the baseline probability of 4.6% for doctors aged 60 or more, it seems rather substantial (a relative decrease of nearly 13%).

We then check whether the composition of patients going to a sector 1 GP was substantially altered by the reform. In particular, we observe that the number of patients that she sees as a *médecin traitant*[18] has increased after the regulatory change at stake, by 4%, when compared with 2016.[19] This result suggests that not only do sector 1 GPs see more patients, but also that they admit new patients as *médecins traitants*. In that sense, it reinforces previous finding as regards an improved access to healthcare.

It is thus natural to determine who are these 'new patients'. Overall, sector 1 GPs see slightly less women and patients residing in another municipality, but especially less patients aged 65 or more. Though the corresponding magnitudes are small in the gender and space dimension (namely, -0.19pp as regards the share of women, to be compared with a baseline of 57%, on average; and -0.17pp as regards the share of patients in the same city, to be compared with a baseline of 50%, on average), it is more substantial as regards the latter: the share of 'old' patients has causally diminished by 1.4pp (the baseline being 30% on average), which suggests that 'new patients' are younger. De facto, when restricting our attention to patients never seen from January 1, 2016, to April 30, 2017, and now admitted into sector 1 GPs' practices, Table 4 confirms that they are younger: the median 'new patient' is 34 on average, as opposed to 47 for the other patients. They seem to travel from further away since only 39% of 'new patients' live in the same city as their sector 1 GP, against 54% of the other patients. On the whole, this descriptive evidence suggests that these 'new patients' are further from the healthcare system, and have hence benefited

[17]We may define a 'possible retirement' as the fact of providing no medical service from a given month onward during our observation period, and when aged 60 or more. By definition, the probability of a possible retirement increases over time. To measure retirement more precisely, we also resort to direct information providing the reason why a doctor's practice has been closed. Both measures of retirement lead to similar results, and corresponding estimates are available upon request.

[18]In the French healthcare system, visiting a specialist without being referred from one's regular GP to that specialist is (more) costly, for instance. By definition, patients visiting direct access specialists without a referral are not subject to such overcharges.

[19]For this outcome, a comparison with direct access specialists would not make sense, and it would also be less relevant with sector 2 GPs.

from the policy. For instance, a higher fraction of them did not visit any sector 1 GP in 2016 (9.8%, against 3.4%). Besides, 'new patients' tend to be in better health in the sense that they tend to suffer less from chronic diseases (12.5% against 23.7% for the other patients). 5.1% of them did not have a *médecin traitant* before their visit, as opposed to 0.6% only for the other patients, hence an increase in the access to healthcare from this viewpoint.

On average, travel times between patients and doctors are found to decrease slightly (Table 5), associated with a lower dispersion. The overall distribution of travel times computed at the GP-month level did not substantially vary after the public policy change at stake, but the fact that these travel times have diminished does not suggest that this reform did not cause travel costs to raise; on the contrary, from this viewpoint, the access to healthcare was made easier in the sense of a shorter distance between (new) patients and physicians.

We next wonder whether the reform altered prescription behavior. The average amount of prescribed drugs per patient has decreased by about 4.3%. Given previous hypothesis as regards shortened length of visits, this result is consistent with a positive correlation between visit duration and drug prescription. The reduction in drug prescription was particularly marked for antibiotics (-8.8%), but less for antidepressants (-3.1%), while for opioids the coefficient is not significant at usual levels. This empirical finding is consistent with the informal commitment made by physicians during the *Convention médicale* bargaining session with the public health insurance, namely a less intensive drug prescription behavior. Importantly, the regulation authority in charge of recommendations as regards drug prescriptions, the *Haute Autorité de Santé* (HAS) provided doctors with new guidelines in 2017 against antibiotics. While such advices are not strictly enforced, it may be the case that sector 2 GPs, the relevant comparison group for this outcome, might have been less responsive to these recommendations. In that case, this contemporaneous regulatory change could act as a confounder and partly explain the result found on antibiotics. Another interpretation of that result might follow from patients being in a better health, and thus with less needs in this respect, consistent with previous empirical finding. While reimbursements by the public health insurance have increased by 6% following the 8.7% rise in sector 1 GPs' fees, corresponding here to the sum of medical visits' reimbursed fees and of drugs' reimbursed amounts, they have been reduced by 1.5% at the patient level: from that viewpoint, this regulatory change was cost effective in that it *already saved money in the short-run*, keeping the market size constant.

Last, we assess whether sector 1 GPs changed their behavior as regards home visits, which became relatively less attractive than office visits in pecuniary terms after May 1, 2017. We indeed find a significant and negative impact on their share of home visits of -0.44pp, when compared with that of 2016. Although the baseline share amounts to about 9% in both groups, the impact of the policy change at stake on this outcome does not appear as substantial from an economic viewpoint.

5.1.2 Specialists

We here present similar results for sector 1 cardiologists and specialists (Table 8). Interestingly, the income effect seems to dominate for psychiatrists, contrary to what prevails for GPs: the former have indeed diminished their activity following that regulatory change. As regards cardiologists, the income effect and the substitution compensate since they do not seem to adjust their activity.

5.2 Heterogeneity

We here investigate how the estimated price elasticity of physician activity varies depending on observed characteristics like gender, age, location, or income, among others. The results are displayed by Figure D1.

Female GPs seem to adjust more strongly their activity to a change in financial incentives (panel a), by +12.2%, as opposed to +8.9% for male GPs, which corresponds to elasticities of 1.33 and 0.98, respectively. Yet the most striking dimension of heterogeneity is age: we find markedly dispersed behavioral responses in this regard (panel b), and the effect is monotone.

While doctors aged 30 to 39 increase their activity by up to 20% following that reform (panel b), the oldest GPs, still active beyond 70, hardly change their activity at all, doctors who are 60 to 69 moderately increase theirs by about 5%, and other intermediate responses are estimated for doctors aged 50 to 59 (+10%) or 40 to 49 (+13%). Interestingly, once age has been controlled for, women and men exhibit similar responses (panel c): it should thus be concluded that the apparently gender-specific reaction was mostly driven by a composition effect, whereby female doctors are simply younger than their male colleagues.

Moreover, it is likely that age acts as a proxy for wealth, or permanent income. In that sense, income effects may be small at younger ages, and dominated by substitution effects, while becoming more important at older ages due to life-cycle accumulation of wealth and (almost) equalizing substitution effects in absolute. This likely explanation helps rationalize our findings and those of the literature devoted to physician labor supply, including reconciling cases when there is an apparent discrepancy. *De facto*, panel d shows that GPs tend to respond less to financial incentives when they have a higher 2016 income, even though the magnitude of the differential is neither statistically nor substantially significant. We here make an exception for the bottom 10%, which mostly corresponds to a few temporary inactive doctors due to, e.g., retirement, maternity leave, career interruption, exit from self-employment, etc.; these doctors turn out to respond substantially more, but the estimation is imprecise and the documented increase, more than 20% of their activity, is partly mechanical in that it results from a low starting point. Note also that the above gradient with respect to age is only attenuated once the time elapsed since the opening of a practice has been controlled for.[20] While this remark is certainly relevant from the viewpoint of causal inference, it is not sure that it alters much policy implications. In particular, according to previous estimates, targeting young doctors would make sense in order to stimulate aggregate physician labor supply, and previous caveat would not fundamentally alter this recommendation.

As regards location, we find no significant heterogeneity between dense and weakly dense areas (panel e),[21] or between medical deserts and other areas (panel f), though the response tends to increase as one moves away from a medical desert.

Another noteworthy finding relates to the effect of the exposure to competition. A clear-cut determinant of the intensity of the reaction to financial incentives turns out to be the medical density (Table 6), computed as the ratio of the number of GPs over the population of the *département*. Doctors located in more dense, and thus more competitive areas react more to the revaluation of their regulated fees. In that vein, we may also measure the exposure to competition at a more granular level based on the share of 'shoppers', in IO

[20]Since fixed costs of a practice are high, typically €100,000, young doctors that recently graduated from medical schools are for instance more likely to make an effort so as to reimburse the corresponding loan.

[21]Remember that doctors practicing in non dense areas are not numerous enough to produce precise estimations.

terms, i.e. of patients who visit another GP at least once in the year.[22] Panel g indicates that the degree of exposure to competition has an inverted U-shaped impact on GPs' response to financial incentives. When the degree of competition is reasonably small, i.e. below the 9th decile of the distribution of that share, fiercer competition with surrounding doctors tends to exacerbate the response to financial incentives. But one cannot reject that the top 10% of doctors with the highest shares of shoppers react similarly as the median doctor, and even as the bottom 20% of doctors in that dimension. By comparison with their colleagues facing less competition, namely those between P80 and P90 of that distribution, these doctors most exposed to competition (e.g., in Paris) may face specific (capacity) constraints that prevent them from adjusting further their labor supply.

Table 6 confirms that, *ceteris paribus*, the primary drivers of the reaction to financial incentives are: age, income, and competition (measured by either the medical density or the presence of shoppers among patients). In particular, early-career physicians are much more likely than old-age doctors to adjust their provision of care, conditional on income, location, and competition, while later-career physicians remain largely unaffected. GPs in rural ares tend to be slightly more responsive, but not by much. There is evidence of income effects in the expected direction, but they are rather at play at the bottom (low-income GPs increase more their labor supply in response to higher financial incentives) than at the top of the distribution (high-income GPs do not adjust their labor supply differently from middle-income GPs). The inverted U-shape effect of competition documented above remains conditional on previous observed characteristics.

Last, the effect of the reform has been gradual over time, as made clear by Figure C1, which performs the monthly event study analysis. However, as mentioned above and in Coudin et al. (2015), the monthly activity displays a high degree of seasonality, and not too much attention should be paid on fluctuations obtained at this granular level.

5.3 Robustness checks

In this subsection, we perform several robustness checks. We first verify that our results remain under various identifying assumptions, namely CTAs, based on alternate compari-

[22] For the median GP in this dimension, the 2016 share of 'shoppers' amounts to 0.11, meaning that 89% of her patients are 'loyal'. As the intuition suggests, the share of such captive patients is higher in medical deserts.

son groups.[23] This for instance includes assessing the empirical plausibility of SUTVA as regards the identification strategy that leverages sector 2 GPs as the comparison group, by documenting whether their activity has been affected by the regulatory change at stake. We also check the robustness of our DiD identification strategy with respect to prior matching of treated with control units based on observable characteristics. We further simulate a fake reform as a falsification test in order to gain confidence over causality. We last proceed to various sensitivity analyses as regards sample definition, econometric specification, etc.

5.3.1 Alternate comparison groups

We here implement two other identification strategies to assess the robustness of previous empirical findings. Reassuringly, the results obtained under alternate corresponding assumptions concur with previous evidence: Table 7 shows that when comparing sector 1 GPs with sector 2 GPs (resp. sector 1 GPs the year before), we still obtain that these GPs have increased their activity by 9.2% (resp. 7.1%), which, given the +8.7% price increase, points out to an almost unitary price elasticity. Note that taking the possibility of spillover on sector 2 GPs into account would only yield to interpret the former estimate as a lower bound of the effect, comforting thus our 1.1 price elasticity of healthcare provision as our main estimate.

To nevertheless address the latter issue seriously, we assess the plausibility of SUTVA in this context by documenting limited response of sector 2 GPs to the policy change at stake. To do so, we rely on direct access specialists as a comparison group for which the SUTVA is more likely, and we consider sector 2 GPs as a possibly treatment group. Reassuringly, sector 2 GPs have hardly adjusted their activity at all, the corresponding point estimate being not significant at the 5% level. This empirical evidence hence concurs to rationalize the approach that relies on sector 2 GPs as a valid comparison group, and may help explain why the 'lower bound' of the reaction of sector 1 GPs, 9.2%, is actually close to the one found when relying on direct access specialists as a comparison group, 10.1%.[24]

[23] Another robustness check would consist in isolating a single specialty, namely gynecology, ophthalmology, and stomatology, as a comparison group. Though these approaches result in lower statistical power, each of these comparison groups may be viewed as more homogeneous than the whole set of direct access specialists. Corresponding estimates remain fairly unchanged, which indicates that previous findings are not driven by a single group of specialists. They are available upon request.

[24] To complement this analysis, we have performed that same exercise on another outcome, the number of patients per month. We here obtain that this number has slightly increased, by 2%, in a statistically significant fashion.

5.3.2 Prior matching

The absence of concern about both the exogeneity of the policy-induced shock on financial incentives and the likelihood of the CTA in our setting renders prior matching, i.e. before we perform our DiD empirical analysis, less necessary. However, to insure a higher comparability between our treatment and comparison groups, we here present the results obtained with optimal full matching () based on observed physician characteristics in 2016 (gender, age, type of area: dense, intermediate, or less than weakly dense, and type of medical desert: ZIP, ZAC, or not a medical desert). We choose optimal full matching due to different sample sizes between our treatment and comparison groups. By definition, a full matching procedure assigns one treated unit to one or more control units, or one control unit to one or more treated units (Hansen, 2004). Balancing checks reported in Table 1 confirm that this procedure improves comparability, including in terms of patient composition. Figure B4 shows that the plausibility of the CTA in the matched sample has been neither depreciated nor substantially improved by pre-matching. Applying then our DiD to the matched sample by appropriate reweighting of both treated and control GPs leads to reinforce previous similar diagnosis: if anything, the effect would be even higher after matching, namely about +12.7%, pointing out to a 1.4 elasticity.

5.3.3 Placebo experiments

We next perform falsification tests in which we simulate a fake reform occurring in May 2016, i.e. one year before the actual regulatory change. To avoid any confusion with the actual reform that took place one year later, we select the period following May 1, 2017, out of our sample for the sake of this exercise. For the sake of that simulation, we first proceed to prior matching since we do not rely here on any actual quasi-experimental variation like the one we exploit with our DiD approach. The corresponding results shown in Table 7 point out to no significant effect, regardless of the comparison group chosen (direct access specialists or sector 2 GPs).

5.3.4 Other sensitivity analyses

We last proceed to various sensitivity analyses mostly related to statistical issues (bottom panel of Table 7), including (i) balancing of our panel, (ii) trimming of outliers, (iii) econometric specification, and (iv) prior matching. Previous results remain mostly unchanged (or even slightly amplified) when (i) we restrict our attention to doctors with a positive activity each and any month between January and October 2018, (ii) we eliminate outliers with more than 1,000 office visits per month, and (iii) we allow for Paris region to have a different seasonality than the rest of mainland France by interacting calendar month fixed effects with a regional dummy for *Île-de-France*, composed of 8 French *départements* including Paris and its surroundings.

6 Discussion

Previous empirical findings suggest that the intensity of care has remained quite unaltered by the reform, but that the access to healthcare has improved following stronger financial incentives provided by the public health insurance when rising sector 1 GPs' fees by 8.7%. That a higher number of patients visit a GP after the regulatory change has been made possible thanks to two channels: first, doctors have worked 4% more; second, they have seen 5% more patients each day on average, possibly by shortening visit duration or by working longer hours each day. Moreover, sector 1 GPs have prescribed less drugs to each patient, which is consistent with shorter visits if the correlation between drug prescription and visit duration is positive. Another tentative explanation is that doctors behaved according to their informal commitment during the bargaining session with the public health insurance. Elbaum (2014) explains p.128 that in a 2005 bargaining session of the same kind, doctors were already asking for a revaluation of their fees, and therefore committed to prescribe less drugs (especially antibiotics and antidepressants) as a counterpart. Last, previous findings suggest that sector 1 GPs have admitted new patients, essentially younger ones, sometimes becoming their regular GP (*médecin traitant*).

To dig further into this issue, we conduct a patient-level analysis. We define the dummy Visit_{imy} equal to 1 when patient i visits any sector 1 GP on month m of year y, and 0 otherwise (when she does not consult any doctor, or when she visits any other doctor including specialists or sector 2 GPs).

First, we do not rely on any comparison group and estimate either a linear model:

$$\text{Visit}_{imy} = \beta_i \text{Post}_{my} + \alpha_i + \delta_m + \gamma_y + \varepsilon_{imy}, \tag{4}$$

or a Logit model if we want to allow for a nonlinear effect of the reform β_i on the individual probability of visiting a GP:

$$\text{Visit}_{imy} = \mathbb{1}\{\beta_i \text{Post}_{my} + \alpha_i + \delta_m + \gamma_y + \varepsilon_{imy} \geq 0\}, \tag{5}$$

where ε_{imy} follows a logistic distribution. In that approach, the identification of the reform's impact on that probability results from a simple comparison between patient behavior before and after May 1, 2017, controlling for seasonality and individual FE. Since that change in patient behavior may vary along observed characteristics, we may depart from the homogeneous treatment effects case and instead specify $\beta_i = \beta(X_i)$ where covariates X_i include gender, age, a dummy for some chronic disease, and indicators for living in deprived areas (ZIP or medical deserts, but also ZAC) from the viewpoint of healthcare accessibility.

Consistent with previous empirical evidence documented at the doctor level is the (dual) fact that the share of patients visiting a sector 1 GP at least once a month has increased by 0.6pp following the reform (Table 9) on average,[25] a 2.5% increase of the baseline value before the reform (24.2%, see also Figure A16). According to a Logit model with patient FE, the individual probability of such visits would have been pushed up by 4.1%.

Still consistent with previous findings at the doctor level, but now relaxing the assumption of homogeneous treatment effects, this approach confirms that patients who consult more frequently a GP after May 1, 2017 are younger: the corresponding age distribution of these patients is monotonously shifted to the left (Table 10, Column 1). For instance, children less than 9 have a differential probability to consult of +2.2pp with respect to the 40-49, compared with a -0.6pp reduction for the 70-79. Women visit a GP more frequently after the reform (+0.2pp with respect to men). Patients suffering from a chronic disease also tend to consult more often (+0.1pp than those who do not suffer from any chronic disease), but the difference is not significant at 5%.

Second, we use a DinD approach whereby we define a treatment dummy $T_{c(i)}$ which equals 1 when patient i resides in a city c where there is at least one sector 1 GP. It can be thought of a proxy for availability of physicians at the extensive margin.[26] Put differently, we here rely on a comparison group composed of patients living in municipalities that do not have any sector 1 GP, hence that are a priori less affected by the reform at stake.

[25] Here considering the homogeneous case where $\beta_i = \beta$.

[26] As a caveat, this method requires to observe that city at all dates, which is only the case when a patient actually visits some healthcare provider. To overcome that technical difficulty, we ignore patients' residential mobility for now.

Figure B11 in Appendix suggests that the corresponding CTA cannot be discarded on the basis of pre-event data, which comforts the empirical strategy adopted here. We then consider the following DinD equation, denoting the calendar month by t as in section 4 above:

$$\text{Visit}_{it} = \beta_i \text{Post}_t \times T_{c(i)} + \alpha_i + \gamma_t + \varepsilon_{it}. \tag{6}$$

Under the assumption of homogeneous treatment effects, we estimate a +0.25pp ATE (Table 9, Column 3). A Logit model yields an estimated increase of +1.8% (Table 9, Column 6). When we relax that assumption (Table 10, Column 2), we find that the impact of the reform has been higher for children aged less than 9 (+2.4pp with respect to patients aged 40 to 49) and for women (+0.2pp with respect to men), which confirms the results obtained above in the absence of any comparison group. We find no differentiated impact for patients suffering from chronic disease, but a weaker effect for patients living in more deprived areas (-0.1pp for ZAC and -0.2pp for ZIP, namely medical deserts). These results suggest that while the *overall* access to healthcare has improved (namely, the total number of patients visiting a sector 1 GP) following May 1, 2017, this access may have also been even *more spatially unequal*, hereby magnifying the preexisting gap between more and less deprived areas. Reassuringly, we cannot reject that a placebo experiment in which we simulate a fake reform on May 1, 2016 would have zero impact on patients' probability to visit a sector 1 GP (Table 11).[27]

To confirm previous finding, we third consider two continuous treatments $\text{Access}_{c(i)}$ that measure the access to primary care at the intensive margin: (i) the number of sector 1 GPs' practices located in the same city $c(i)$ as patient i, and (ii) the APL of that very same city $\text{APL}_{c(i)}$. Note that the former does not vary over time (as a consequence of our assumption as regards patients' residential mobility), while the latter does (from year to year since the APL is computed at an annual frequency). Estimating finally:

$$\text{Visit}_{it} = \beta \text{Post}_t \times \text{Access}_{c(i)} + \theta \text{Access}_{c(i)} + \alpha_i + \gamma_t + \varepsilon_{it}, \tag{7}$$

[27] Note that we may also consider the dependent $\text{Visit Regular}_{it}$, which equals one when patient i visits her regular sector 1 GP at that time. Results remain unchanged, from a qualitative viewpoint. By contrast, no significant effect is found when the dependent variable is a dummy related to an admission in the emergency department Emergency_{it}.

we are particularly interested in learning whether one can reject $H_0 : \beta > 0$ or not. Failing to reject that hypothesis indicates that, when compared to patients living in more deprived areas, patients living in less deprived areas benefited the most from the reform and its consequences, namely a higher availability of consultation slots as documented by our physician-level analysis. Empirically, H_0 cannot be rejected at 5% in both cases (Table 12). These findings point out to an unintended effect of the reform, which has seemingly reinforced the unequal access to healthcare across the French territory. In that sense, the reform might have in fact exacerbated that problem, far from solving it, by inducing some Mathew effect.

7 Conclusion

This article has empirically assessed the effects of higher financial incentives provided to GPs on various outcomes related to the access to healthcare, including the provision, the intensity of care, and doctors' workload, at the intensive margin, but also on retirement behavior, at the extensive margin, as well as on health insurance spending and drug prescription, among others. Based on a quasi-natural experiment and comprehensive data on the universe of both patients and doctors in France, we find a substantial response to these incentives, especially for early-career physicians, which takes the form of higher GPs' activity. Doctors see more patients, and this adjustment seems to operate through both a higher workload, measured in days, and a higher number of patients seen each day. Besides, the share of their patients that they see as a *médecin traitant* increases, suggesting that concerned doctors admit new patients and become their *médecin traitant*, hence strengthening the result on a better access to healthcare. It seems however that 'new patients', mostly young individuals further away from the healthcare system are ready to incur higher travel costs to visit their doctor. Overall, providing stronger financial incentives sounds like a cost-effective policy to enhance the access to healthcare, with limited adverse effects on the intensity of care since we do not detect any change in the number of visits per patient, for instance.

These empirical results are derived from a clear-cut research design due to both the reform being wide-scale and the exhaustive nature of our data, and we insist on their high external validity. We hence believe that they should be taken into account by the policy maker when designing policies targeted at deprived medical areas. In that sense, they indicate that newly graduates from medical schools should be prioritized.

A natural extension of this research would consist in determining whether any change in the quality of care has been observed following that reform, which requires the availability of relevant indicators of quality (mortality, readmission, hospitalizations, be they avoidable or not, etc.). To further complement the current analysis, it would also be interesting to document any displacement effects from emergency rooms. Last, empirically assessing the response to similar shocks that intervened at the end of 2023 (resp. 2024), when the fee per visit was raised, again, from €25 to €26.5 (resp. €30), is another promising area of further research.

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8 Figures

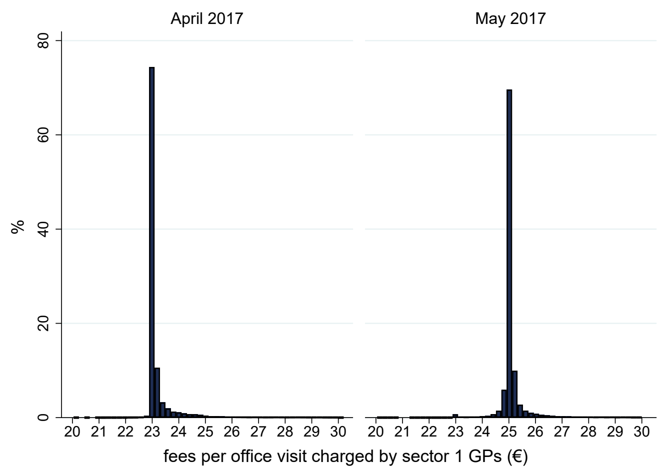


Figure 1: Actual fees charged by sector 1 GPs

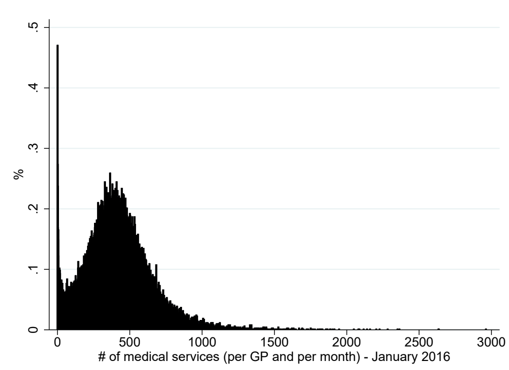
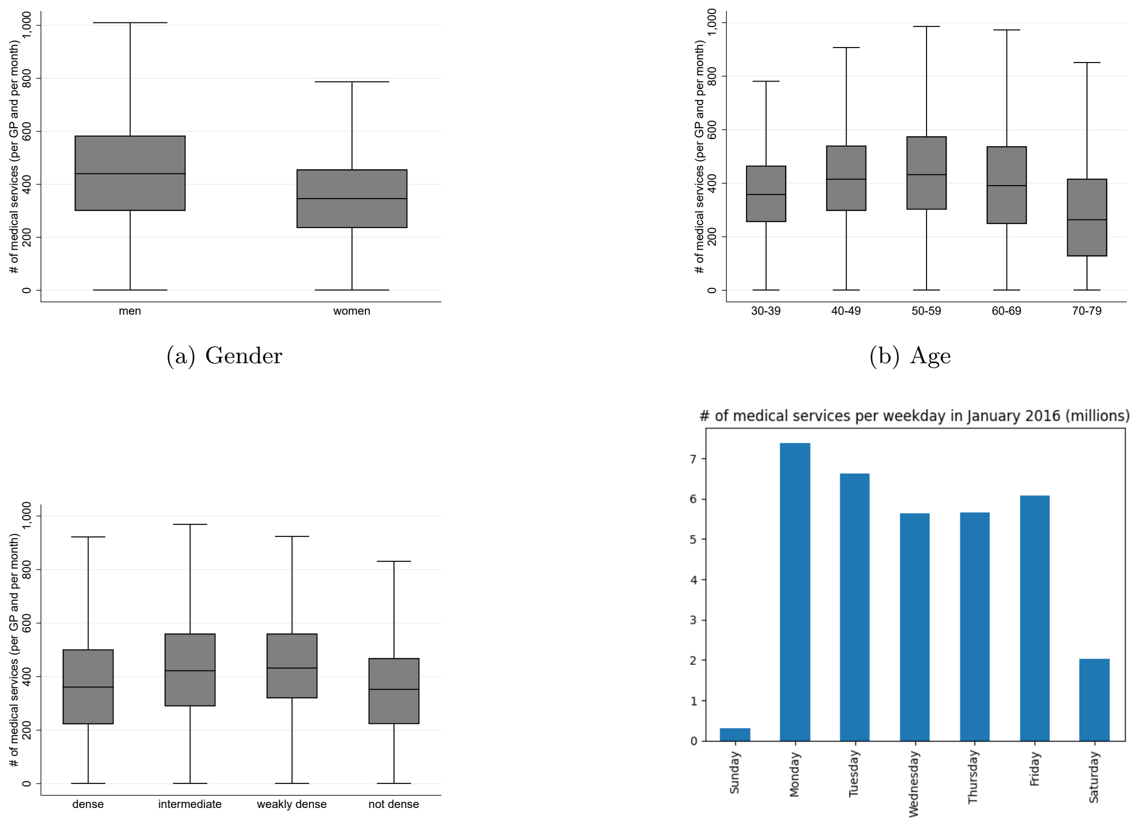


Figure 2: Physician activity



(a) Gender

(b) Age

(c) Location

(d) Weekday

Figure 3: Doctors' activity, depending on various dimensions

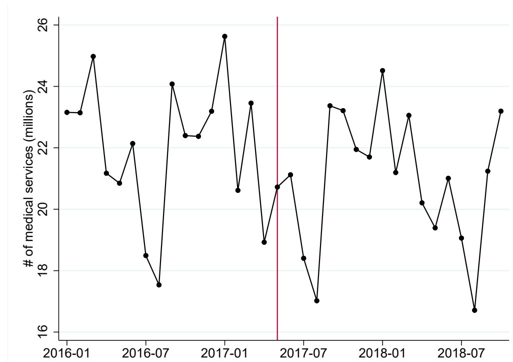


Figure 4: Evolution of GPs' activity

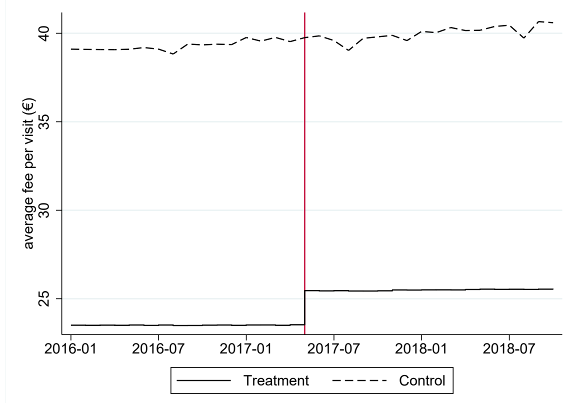
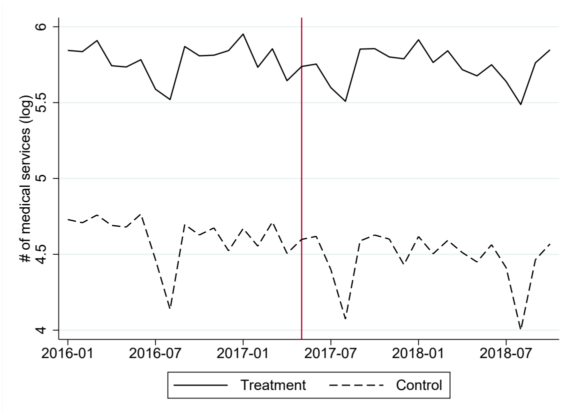
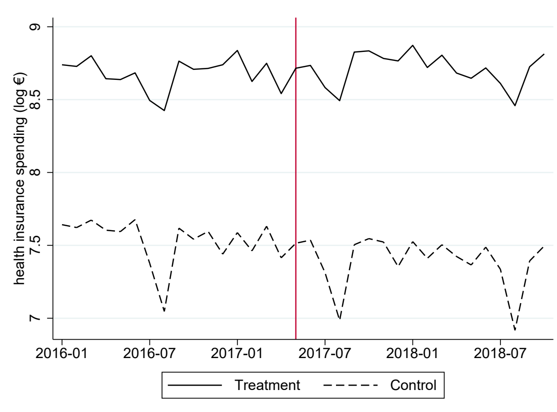


Figure 5: Change in doctors' fees (T: Sector 1 GPs, C: Direct access specialists)



(a) Activity (log)



(b) Health insurance spending (log €)

Figure 6: Evolution of primary outcomes (T: Sector 1 GPs, C: Direct access specialists)

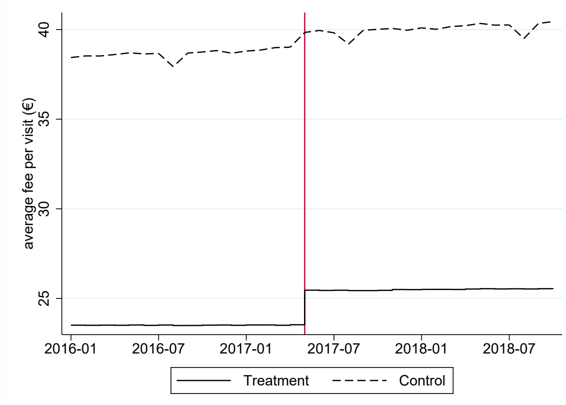
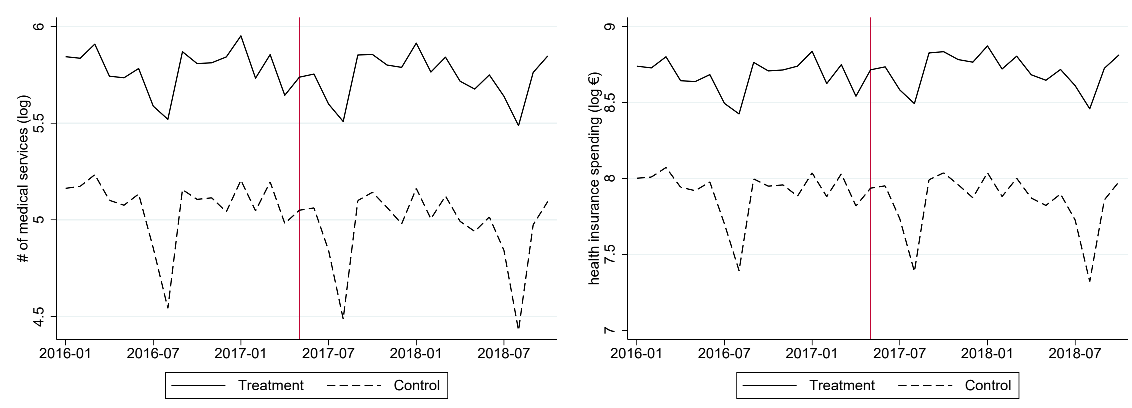


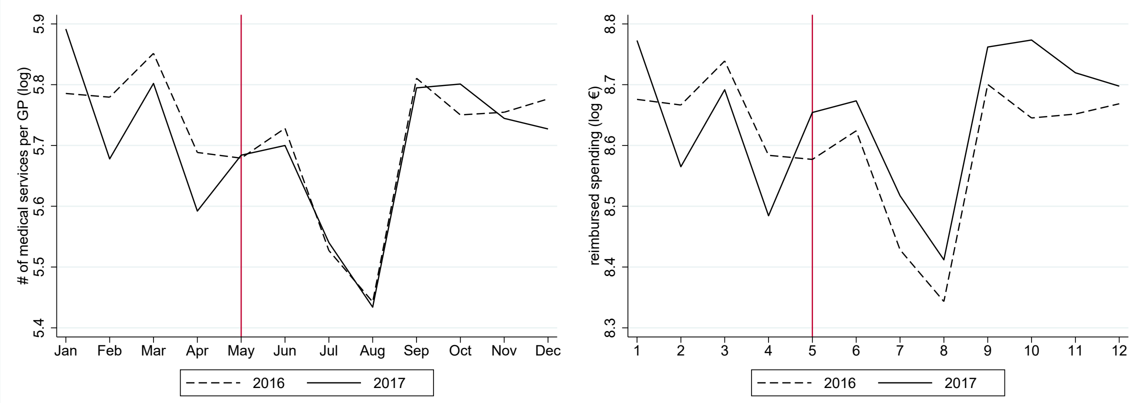
Figure 7: Change in doctors' fees (T: Sector 1 GPs, C: Sector 2 GPs)



(a) Activity (log)

(b) Health insurance spending (log €)

Figure 8: Evolution of primary outcomes (T: Sector 1 GPs, C: Sector 2 GPs)



(a) Activity (log)

(b) Health insurance spending (log €)

Figure 9: Evolution of primary outcomes (T: Sector 1 GPs in 2017, C: Sector 1 GPs in 2016)

9 Tables

Table 1: Summary statistics - Balancing checks

	Sector 1 GPs	Sector 2 GPs	Direct access specialists (Matched)	Direct access specialists (Unmatched)
Women	0.357	0.287	0.315	0.471
Age	53.2	62.2	52.8	57.2
Paris region	0.133	0.406	0.149	0.238
Dense area	0.367	0.658	0.369	0.612
Intermediate density	0.337	0.264	0.387	0.349
Weakly dense	0.290	0.075	0.242	0.039
Not dense	0.006	0.003	0.002	0.001
Medical desert (ZIP)	0.173	0.191	0.165	0.113
ZAC	0.528	0.561	0.543	0.548
Not a medical desert	0.299	0.248	0.291	0.339
Share of female patients	0.580	0.647	0.600	0.655
Share of 65^{+} patients	0.297	0.312	0.324	0.295
Observations	50630	4750	6474	6474

Note. Sample means computed in January 2016.

Table 2: Impact of the reform on activity (C: Direct access specialists)

	(1)	(2)	(3)
Post × Treatment	0.0905***	0.0965***	0.0879***
	(0.00540)	(0.00572)	(0.00583)
Physician FE	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes
Excluding April and May 2017	No	Yes	Yes
Present in January 2016 and October 2018	No	No	Yes
Observations	1920379	1806757	1539472
R^2	0.886	0.884	0.885

Note. Dependent variable: Number of medical services per GP and per month (log).

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Clustered standard errors at the physician level

Table 3: Impact of the reform on various outcomes

Dependent variable	Post × Treatment		Comparison group	Unit FE	Time FE	Observations	R^2
Activity (per month, log)	0.0965***	(0.00572)	DA specialists	Physician	Month-year	1806757	0.884
Health insurance spending (per month, log)	0.172***	(0.00584)	DA specialists	Physician	Month-year	1806529	0.885
# of patients (per month, log)	0.105***	(0.00573)	DA specialists	Physician	Month-year	1806757	0.885
# of services (per patient, log)	0.00239***	(0.000677)	Sector 2 GPs	Physician	Month-year	1742230	0.806
# of patients (per day, log)	0.0506***	(0.00364)	DA specialists	Physician	Day-year	33826817	0.507
# of workdays (per month, log)	0.0418***	(0.00278)	DA specialists	Physician	Month-year	1806757	0.723
Probability of retirement	-0.00593***	(0.00204)	Sector 1 GPs in 2016	Physician-year	Month	344600	0.812
# of patients as *médecin traitant* (per month, log)	0.0398***	(0.00154)	Sector 1 GPs in 2016	Physician-year	Month	922795	0.932
Share of home visits	-0.00443***	(0.000214)	Sector 1 GPs in 2016	Physician-year	Month	1004277	0.943
Share of women among patients	-0.00190***	(0.000244)	Sector 1 GPs in 2016	Physician-year	Month	1004277	0.714
Share of 65+ among patients	-0.0138***	(0.000262)	Sector 1 GPs in 2016	Physician-year	Month	1004277	0.892
Share of patients living in same city	-0.00174***	(0.000282)	Sector 1 GPs in 2016	Physician-year	Month	1004277	0.951
All drugs (per patient, log €)	-0.0442***	(0.00554)	Sector 2 GPs	Physician	Month-year	1742208	0.856
Antibiotics (per patient, log €)	-0.0925***	(0.00611)	Sector 2 GPs	Physician	Month-year	1685289	0.735
Antidepressants (per patient, log €)	-0.0318***	(0.00664)	Sector 2 GPs	Physician	Month-year	1623056	0.818
Opioids (per patient, log €)	-0.0367***	(0.00910)	Sector 2 GPs	Physician	Month-year	1640372	0.771
All drugs + reimbursed fees (per patient, log €)	-0.0154***	(0.00308)	Sector 2 GPs	Physician	Month-year	1742230	0.792
All drugs + reimbursed fees (log €)	0.0701***	(0.00381)	Sector 2 GPs	Physician	Month-year	1742230	0.939

Note. DA: Direct access. April and May 2017 are excluded from estimation samples.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Clustered standard errors at the physician level

Table 4: New patients vs. other patients (2017)

	New patients	Other patients
Dyadic-level analysis		
Number of patient-physician dyads	22,361,517	42,607,554
Number of patients	17,490,773	38,554,013
Same city as GP's practice	0.361	0.479
Patient-level analysis		
Number of patients	8,832,628	38,554,013
Age (median)	30	49
Less than 18	0.278	0.184
Women	0.481	0.556
Chronic disease	0.141	0.332
Chronic disease during visit	0.063	0.209
including cancer	0.240	0.273
including diabetes	0.220	0.317
including mental disorder	0.210	0.143
Never visited any sector 1 GP before May 1, 2017	0.432	0
(Uncensored) Median duration since last visit to some sector 1 GP	255	116
Never visited any sector 1 GP in 2016	0.483	0.034
Never visited any another specialist in 2016	0.766	0.611
Never visited any direct access specialist in 2016	0.873	0.814
Never visited any sector 2 GP in 2016	0.917	0.958
No regular GP	0.034	0.005

Sample. 47,386,641 patients visiting some sector 1 GP from May to December 2017.
All differences across patient groups are statistically significant at 5%.

Table 5: Impact of the reform on travel times between patients and GPs (in minutes)

Dependent variable	mean	sd	p10	p25	p50	p75	p90
Post × Treatment	-0.0652	-0.174**	-0.0599**	-0.0342	-0.00367	-0.0283	-0.106
	(0.0399)	(0.0802)	(0.0298)	(0.0302)	(0.0360)	(0.0551)	(0.105)
Physician FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1101847	1095290	1101847	1101847	1101847	1101847	1101847
R^2	0.726	0.685	0.527	0.644	0.719	0.616	0.575

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$
Clustered standard errors at the physician level

Table 6: Heterogeneity of the response to financial incentives (Activity)

Post × Treatment	-1.277***
	(0.0758)
Medical density × Post × Treatment	1.425***
	(0.0788)
Age (ref: 70-79)	
30-39 × Post × Treatment	0.170***
	(0.00784)
40-49 × Post × Treatment	0.115***
	(0.00724)
50-59 × Post × Treatment	0.0891***
	(0.00692)
60-69 × Post × Treatment	0.0448***
	(0.00682)
Gender (ref: men)	
Women × Post × Treatment	-0.00245
	(0.00257)
Density of area (ref: dense)	
Intermediate × Post × Treatment	0.000583
	(0.00325)
Weakly dense × Post × Treatment	0.00866**
	(0.00357)
Not dense × Post × Treatment	0.0297*
	(0.0177)
Medical desert classification (ref: not a medical desert)	
ZIP × Post × Treatment	0.00155
	(0.00377)
ZAC × Post × Treatment	-0.00122
	(0.00278)
Income (in 2016, ref: Group 2)	
Group 1 × Post × Treatment	0.0660**
	(0.0304)
Group 3 × Post × Treatment	-0.0273
	(0.0187)
Group 4 × Post × Treatment	-0.0372**
	(0.0170)
Group 5 × Post × Treatment	-0.0326**
	(0.0165)
Group 6 × Post × Treatment	-0.0397**
	(0.0164)
Group 7 × Post × Treatment	-0.0513***
	(0.0164)
Group 8 × Post × Treatment	-0.0489***
	(0.0164)
Group 9 × Post × Treatment	-0.0487***
	(0.0164)
Group 10 × Post × Treatment	-0.0423**
	(0.0164)
Share of shoppers (ref: Group 2)	
Group 1 × Post × Treatment	-0.0138
	(0.0143)
Group 3 × Post × Treatment	0.00592*
	(0.00359)
Group 4 × Post × Treatment	0.00960***
	(0.00364)
Group 5 × Post × Treatment	0.00683*
	(0.00375)
Group 6 × Post × Treatment	0.00657*
	(0.00389)
Group 7 × Post × Treatment	0.0114***
	(0.00394)
Group 8 × Post × Treatment	0.0139***
	(0.00435)
Group 9 × Post × Treatment	0.0107**
	(0.00486)
Group 10 × Post × Treatment	-0.0291***
	(0.00703)
Département FE × Post × Treatment	Yes
Constant	5.627***
	(0.00257)
Observations	1717386
R^2	0.879

Dependent variable: Number of medical services per GP and per month (log).
Clustered standard errors at the physician level
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Impact of the reform on activity - Robustness checks

Robustness check	Post × Treatment		Comparison group	Unit FE	Time FE	Observations	R^2
Activity (Sector 1 GPs)	0.0879***	(0.00488)	Sector 2 GPs	Physician	Month-year	1742230	0.874
Activity (Sector 1 GPs)	0.0686***	(0.00165)	Sector 1 GPs in 2016	Physician-year	Month	1004277	0.877
Activity (Sector 2 GPs)	0.00803	(0.00736)	DA specialists	Physician	Month-year	336834	0.873
Pre-matching	0.120***	(0.0257)	DA specialists	Physician	Month-year	1673711	0.876
Placebo reform (May 1, 2016)	0.0317	(0.0196)	DA specialists	Physician	Month-year	823477	0.906
Placebo reform (May 1, 2016)	0.0199	(0.0138)	Sector 2 GPs	Physician	Month-year	798915	0.902
Balanced panel	0.0804***	(0.00523)	DA specialists	Physician	Month-year	1428580	0.871
Trimming	0.0961***	(0.00602)	DA specialists	Physician	Month-year	1779080	0.882
Paris seasonality	0.0941***	(0.00872)	DA specialists	Physician	Month-year	1806757	0.886

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Clustered standard errors at the physician level

Table 8: Impact of the reform on activity for psychiatrists and cardiologists (T: Sector 1, C: Sector 2)

	Cardiologists	Psychiatrists
Post × Treatment	0.0118	-0.0199**
	(0.0132)	(0.00863)
Physician FE	Yes	Yes
Month-year FE	Yes	Yes
Observations	125740	183473
R^2	0.863	0.817

Note. Dependent variable: Number of medical services per physician and per month (log).

April and May 2017 are excluded from estimation samples.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Clustered standard errors at the physician level.

Table 9: Impact of the reform on patients' monthly probability of visiting a sector 1 GP

	Linear probability model			Logit model		
Dependent variable: Visit to any sector 1 GP						
Post	0.00615***	0.00615***		1.0329***	1.0412***	
	(0.000357)	(0.000357)		(0.00203)	(0.00253)	
Post × Treatment			0.00246***			1.0177***
			(0.000292)			(0.00320)
Baseline	[0.2422]	[0.2422]	[0.2422]			
Comparison group	No	No	Yes	No	No	Yes
Patient FE	No	Yes	Yes	No	Yes	Yes
Year FE	Yes	Yes	No	Yes	Yes	No
Month FE	Yes	Yes	No	Yes	Yes	No
Month-Year FE	No	No	Yes	No	No	Yes
Observations	22058826	22058826	22058826	22058826	20942062	20942062

Note. Balanced panel at the monthly level of a random sample of patients at rate 1/100.
Clustered standard errors at the patient level in parentheses (except in last column)
Linear probability model: Baseline corresponds to average value before May 1, 2017
Logit model: Reported coefficients correspond to odds ratios
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Heterogeneity of TE on patients' monthly probability of visiting a sector 1 GP

Post	0.00868***	
	(0.000774)	
Post × Treatment		0.00632***
		(0.000927)
Age (ref: 40-49)		
0-9 × Post (× Treatment)	0.0219***	0.0237***
	(0.000924)	(0.000996)
10-19 × Post (× Treatment)	-0.00522***	-0.00477***
	(0.000718)	(0.000788)
20-29 × Post (× Treatment)	0.000367	0.00142*
	(0.000760)	(0.000827)
30-39 × Post (× Treatment)	-0.00127*	-0.000581
	(0.000720)	(0.000784)
50-59 × Post (× Treatment)	0.000863	0.000715
	(0.000727)	(0.000798)
60-69 × Post (× Treatment)	-0.00386***	-0.00397***
	(0.000763)	(0.000841)
70-79 × Post (× Treatment)	-0.00584***	-0.00561***
	(0.000871)	(0.000957)
80-89 × Post (× Treatment)	-0.0265***	-0.0254***
	(0.00116)	(0.00126)
90-99 × Post (× Treatment)	-0.120***	-0.116***
	(0.00233)	(0.00249)
100-109 × Post (× Treatment)	-0.375***	-0.372***
	(0.00824)	(0.00874)
110+ × Post (× Treatment)	-0.500***	-0.495***
	(0.0449)	(0.0472)
Gender (ref: men)		
Women × Post (× Treatment)	0.00217***	0.00214***
	(0.000424)	(0.000463)
Medical desert classification (ref: not a medical desert)		
ZIP × Post (× Treatment)	-0.00272***	-0.00222***
	(0.000596)	(0.000645)
ZAC × Post (× Treatment)	-0.00149***	-0.00131**
	(0.000549)	(0.000575)
Chronic disease × Post (× Treatment)	0.000965	0.000738
	(0.000651)	(0.000709)
Constant	0.244***	0.245***
	(0.000194)	(0.000259)
Comparison group	No	Yes
Patient FE	Yes	Yes
Year FE	Yes	No
Month FE	Yes	No
Month-Year FE	Yes	Yes
Observations	20926590	20926590
R^2	0.214	0.214

Note. Balanced panel at the monthly level of a random sample of patients at rate 1/100.
Clustered standard errors at the patient level in parentheses (except in last column)
Linear probability model: Baseline corresponds to average value before May 1, 2017
Logit model: Reported coefficients correspond to odds ratios
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Impact on patients' monthly probability of visiting a GP (Falsification test)

Post (May 2016)	-0.00873***	-0.00868***
	(0.000352)	(0.000683)
Post (May 2016) × Placebo Treatment		-0.000065
		(0.000722)
Baseline	[0.2562]	[0.2562]
Comparison group	No	Yes
Patient FE	Yes	Yes
Month FE	Yes	No
Observations	10380624	10380624
R^2	0.258	0.258

Note. Balanced panel at the monthly level of a random sample of patients at rate 1/100.

Linear probability model

Clustered standard errors at the patient level

Baseline corresponds to average value before May 1, 2016

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Impact on patients' monthly probability of visiting a GP (Continuous treatment)

Treatment	# of sector 1 GP in same city	APL index
Post × Treatment	0.0001***	0.000915***
	(0.0000007)	(0.000204)
Constant	0.2422***	0.2379***
	(0.000189)	(0.00178)
Patient FE	Yes	Yes
Month-Year FE	Yes	Yes
Observations	22058826	20923564
R^2	0.213	0.213

Note. Balanced panel at the monthly level of a random sample of patients at rate 1/100.

Linear probability model

Clustered standard errors at the patient level

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix

A Working sample: Descriptive statistics

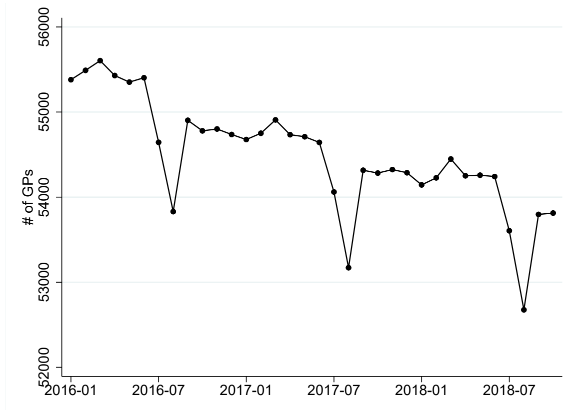


Figure A1: Number of GPs in sample

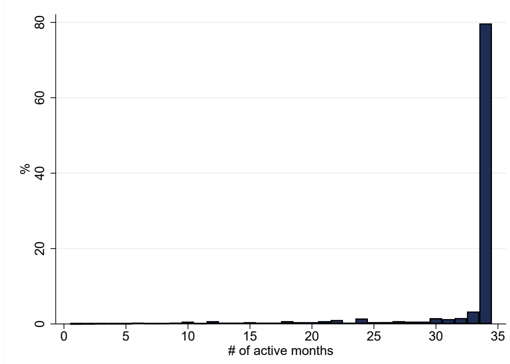


Figure A2: Number of months active in sample

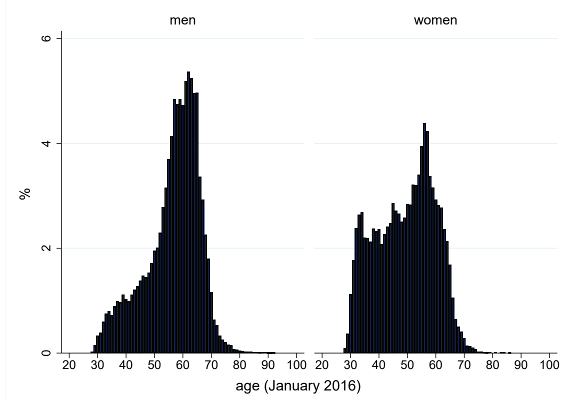
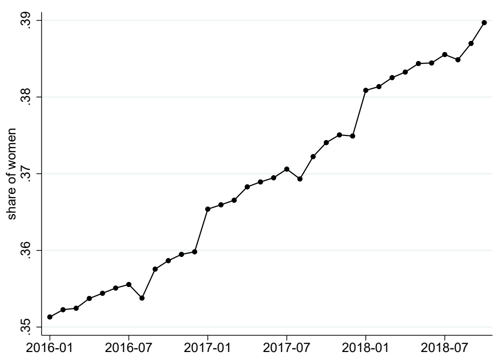
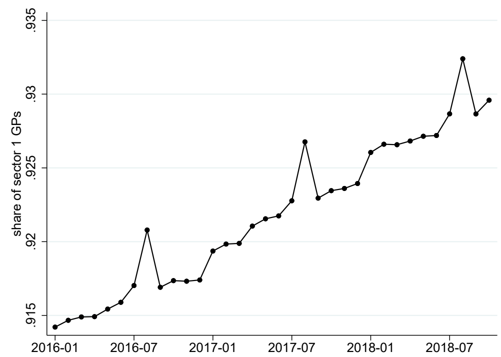


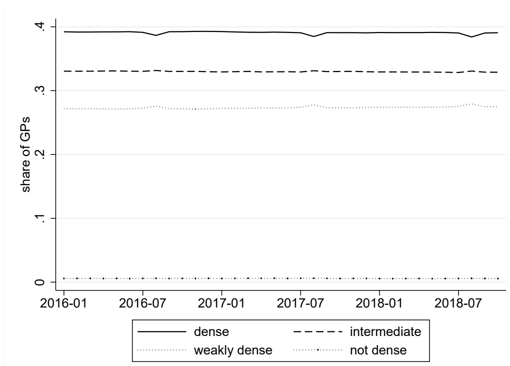
Figure A3: Age of GPs in January 2016 (before trimming)



(a) Gender



(b) Sector 1



(c) Location

Figure A4: GPs (Observed characteristics)

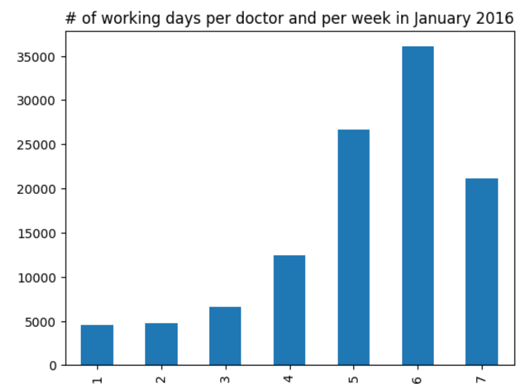


Figure A5: Number of weekly working days

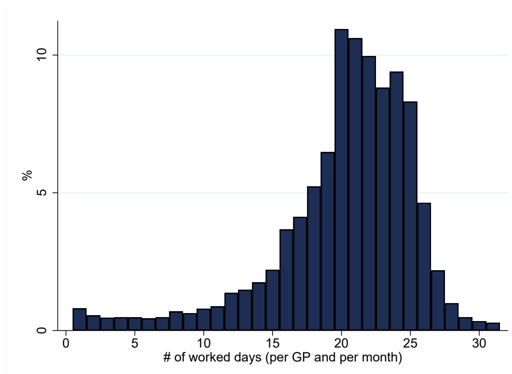
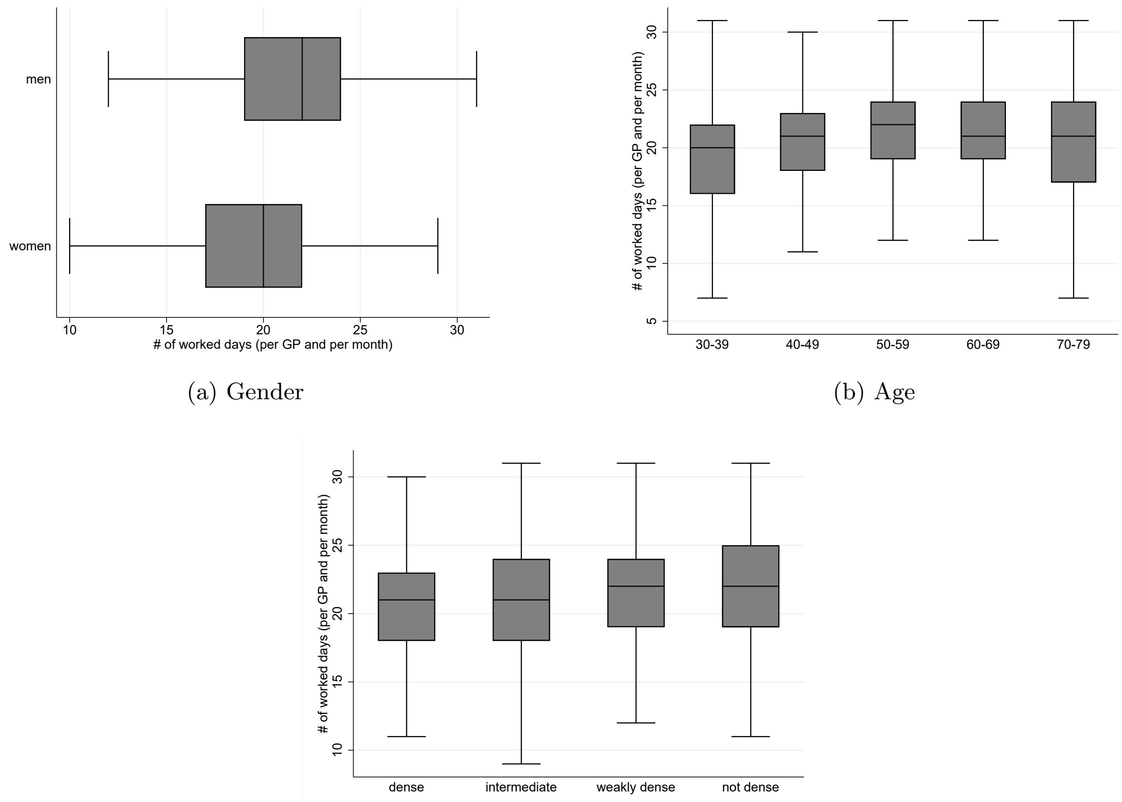


Figure A6: Number of working days (per GP and per month)

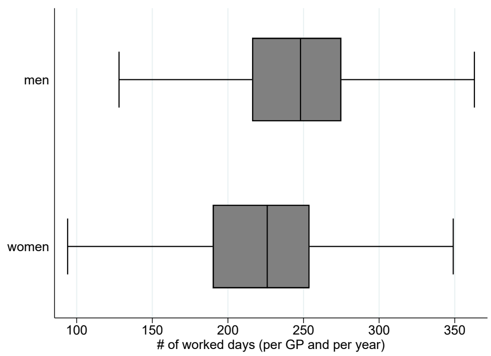


(a) Gender

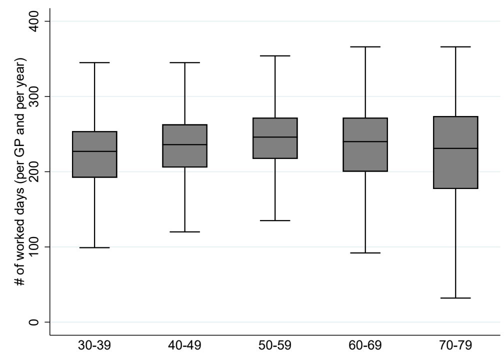
(b) Age

(c) Location

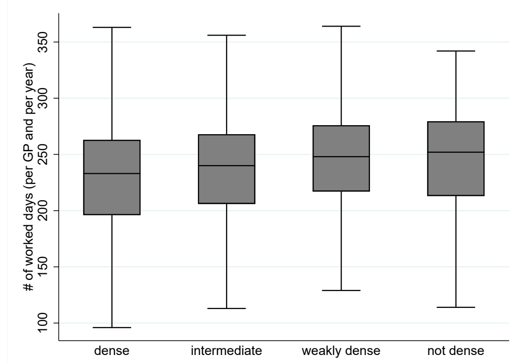
Figure A7: Number of working days (per GP and per month)



(a) Gender

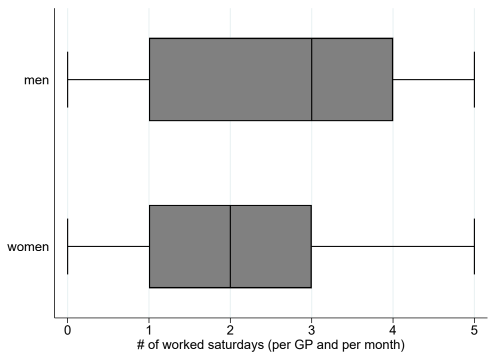


(b) Age

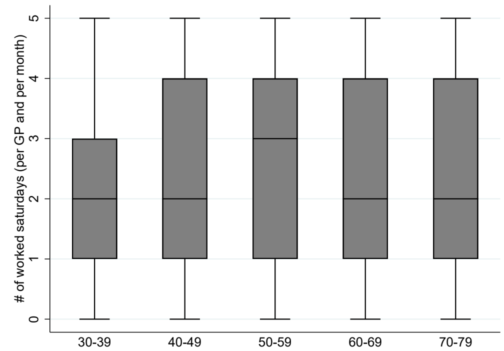


(c) Location

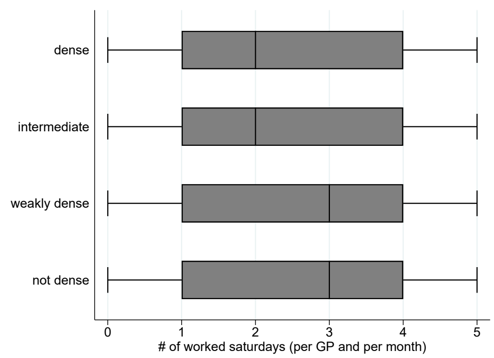
Figure A8: Number of working days (per GP and per year)



(a) Gender



(b) Age



(c) Location

Figure A9: Number of working Saturdays (per GP and per month)

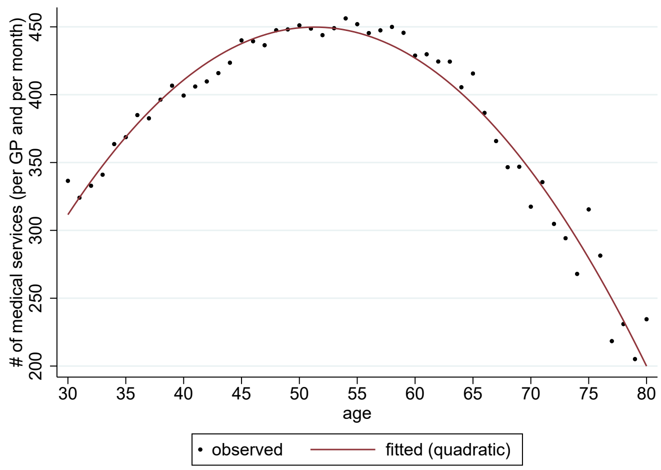


Figure A10: Number of medical services (per GP and per month), depending on age)

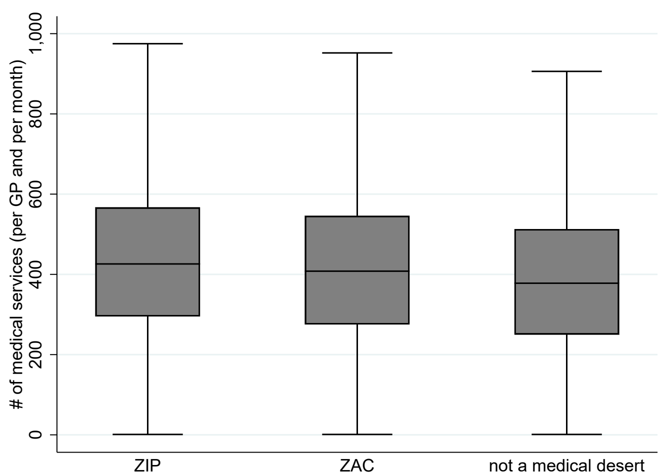


Figure A11: Number of medical services (per GP and per month) in medical deserts

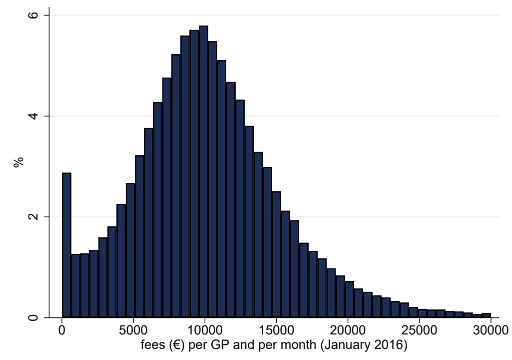
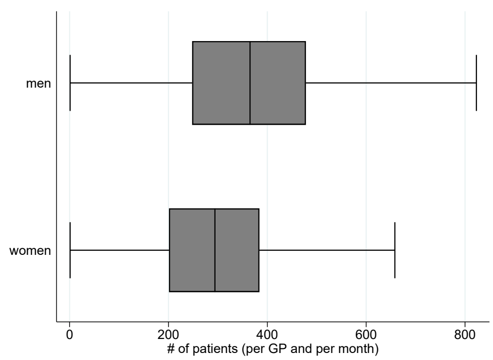
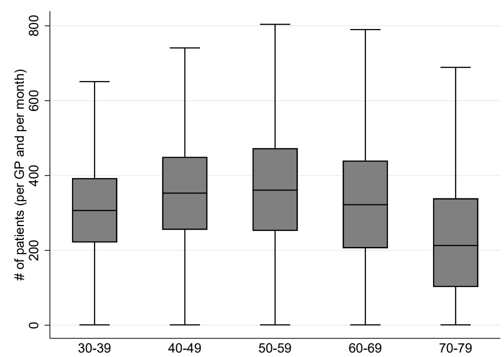


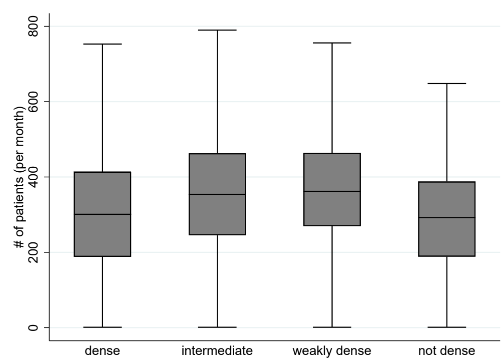
Figure A12: Physician monthly income



(a) Gender

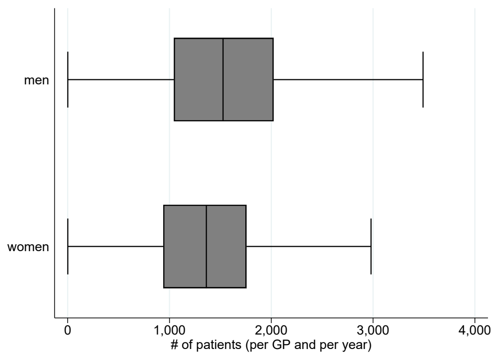


(b) Age

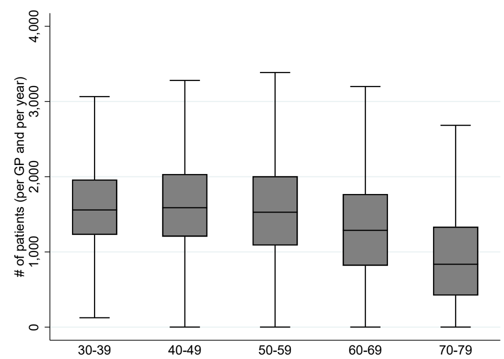


(c) Location

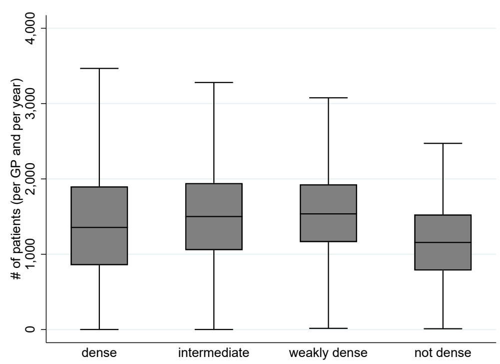
Figure A13: Number of patients (per GP and per month)



(a) Gender

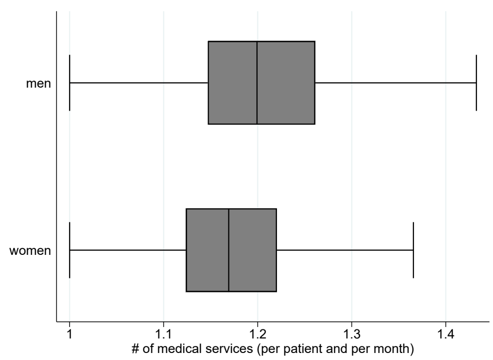


(b) Age

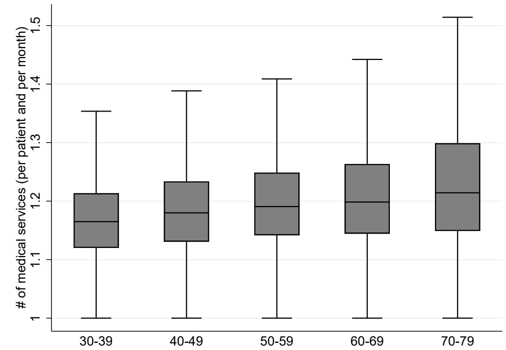


(c) Location

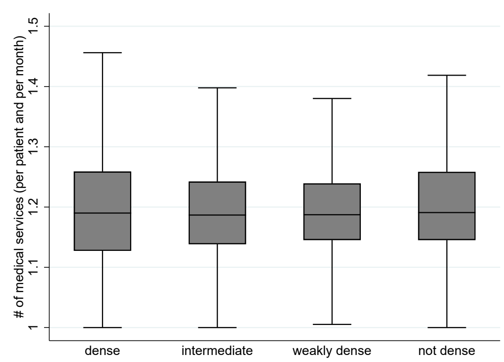
Figure A14: Number of patients (per GP and per year)



(a) Gender



(b) Age



(c) Location

Figure A15: Number of medical services per patient

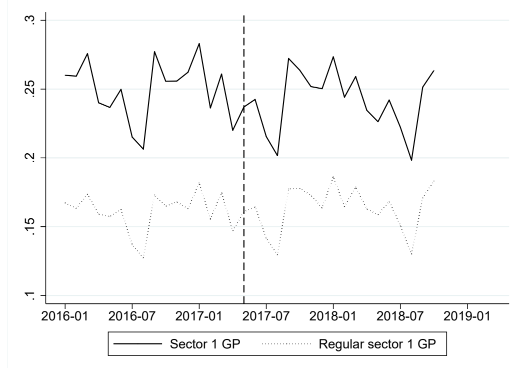
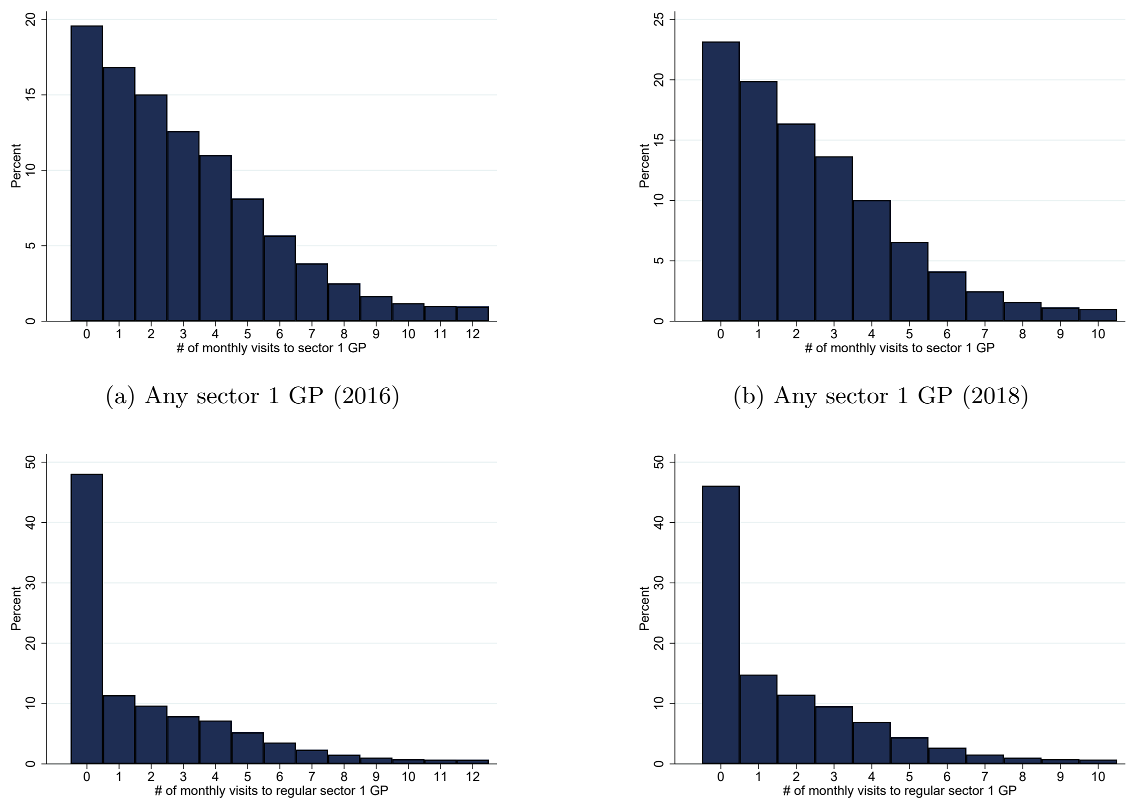


Figure A16: Share of patients visiting GPs at least once in a month



(a) Any sector 1 GP (2016)

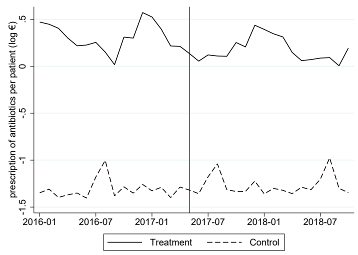
(b) Any sector 1 GP (2018)

(c) Regular sector 1 GP (2016)

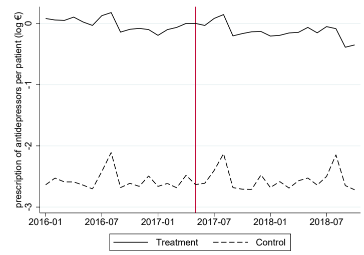
(d) Regular sector 1 GP (2018)

Figure A17: Number of monthly visits to a GP (per patient)

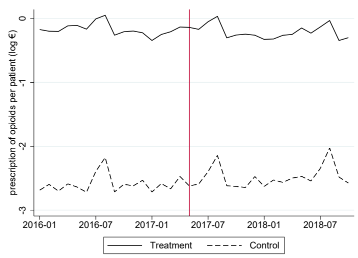
B Plausibility of CTAs



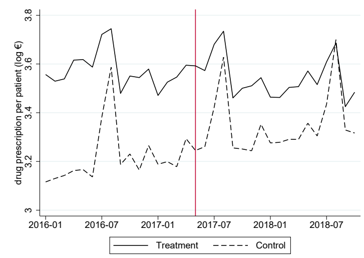
(a) Prescription of antibiotics (per patient, log €)



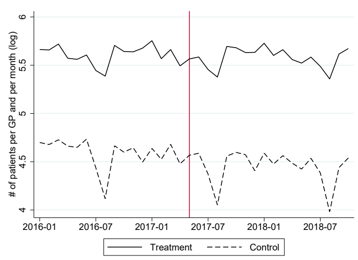
(b) Prescription of antidepressants (per patient, log €)



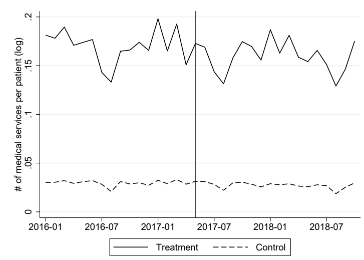
(c) Prescription of opioids (per patient, log €)



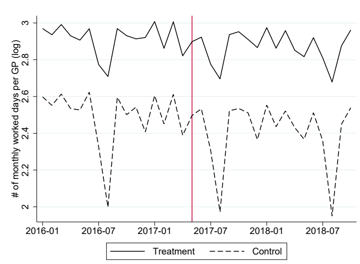
(d) Drug prescription (per patient, log €)



(e) Number of patients per GP and per month

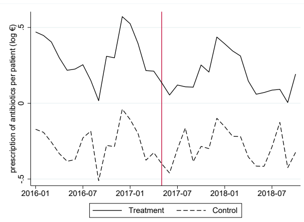


(f) Number of medical services per patient

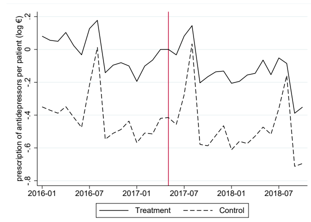


(g) Number of working days per GP and per month

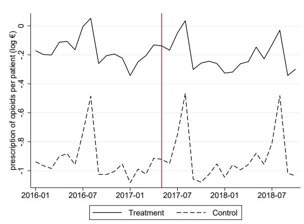
Figure B1: Evolution of outcomes (C: Direct access specialists)



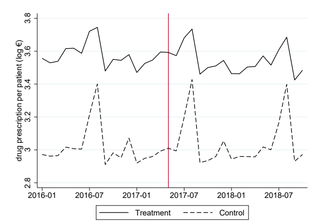
(a) Prescription of antibiotics (per patient, log €)



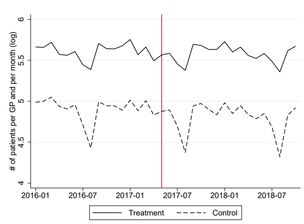
(b) Prescription of antidepressants (per patient, log €)



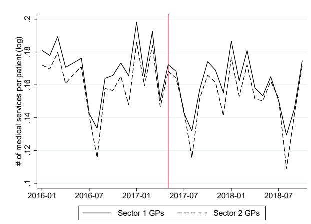
(c) Prescription of opioids (per patient, log €)



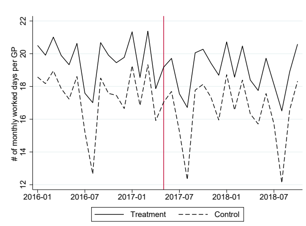
(d) Drug prescription (per patient, log €)



(e) Number of patients per GP and per month

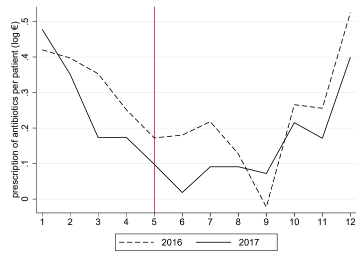


(f) Number of medical services per patient

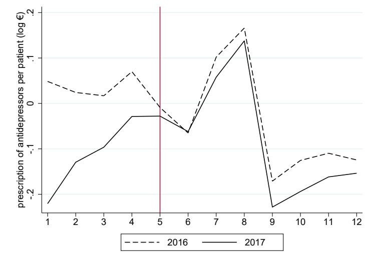


(g) Number of working days per GP and per month

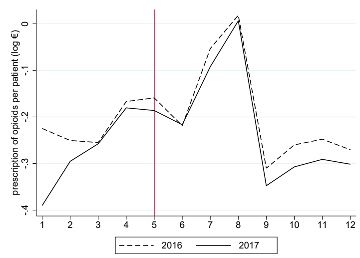
Figure B2: Evolution of outcomes (C: Sector 2 GPs)



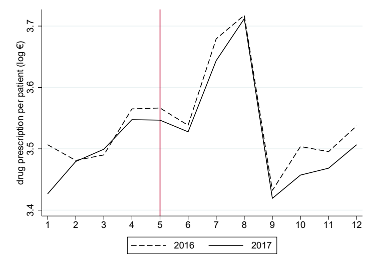
(a) Prescription of antibiotics (per patient, log €)



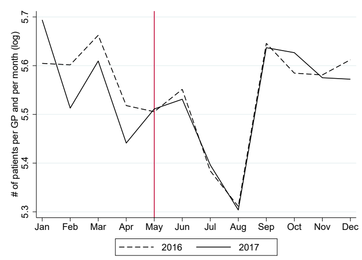
(b) Prescription of antidepressants (per patient, log €)



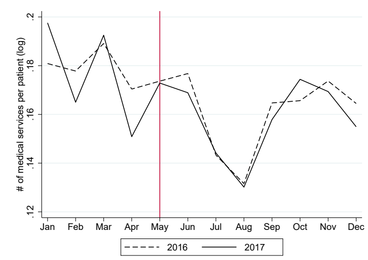
(c) Prescription of opioids (per patient, log €)



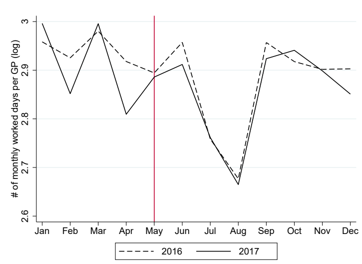
(d) Drug prescription (per patient, log €)



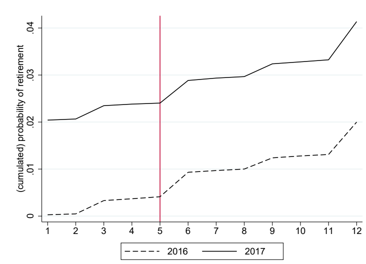
(e) Number of patients per GP and per month



(f) Number of medical services per patient



(g) Number of working days per GP and per month



(h) (Cumulated) Probability of retirement

Figure B3: Evolution of outcomes (C: Sector 1 GPs in 2016)

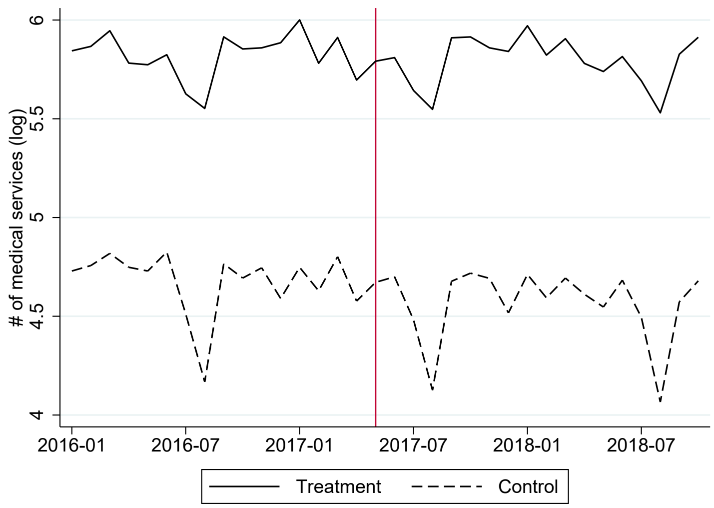


Figure B4: Evolution of activity (C: Direct access specialists, Matched sample)

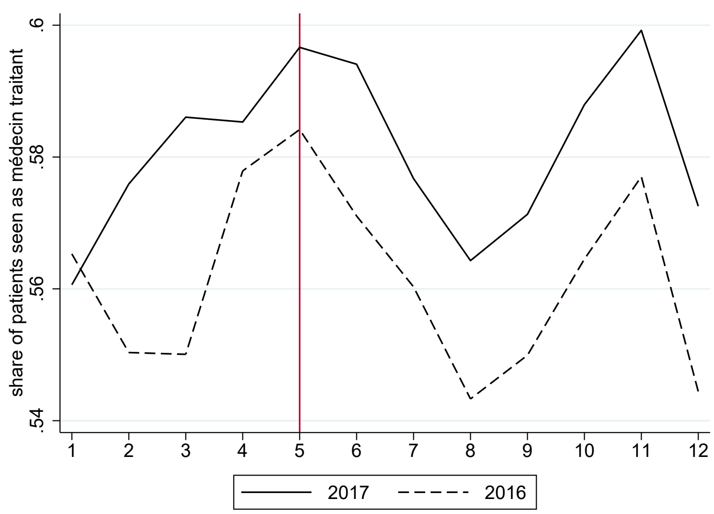


Figure B5: Evolution of the share of patients seen as *médecin traitant* (C: Sector 1 GPs in 2016)

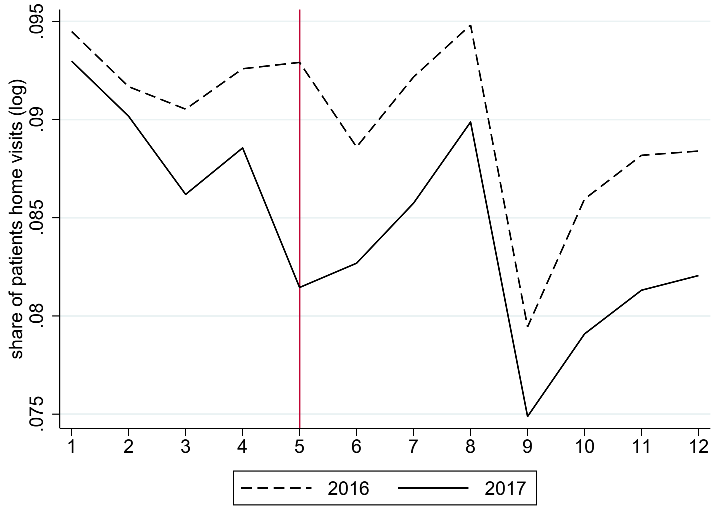
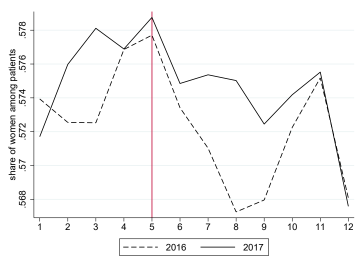
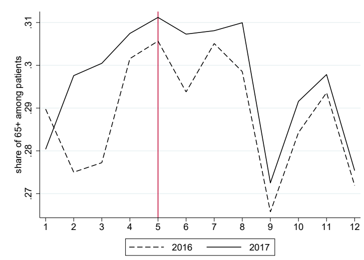


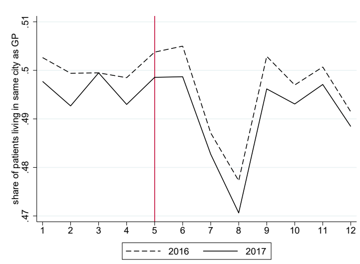
Figure B6: Evolution of the share of home visits (C: Sector 1 GPs in 2016)



(a) Share of women

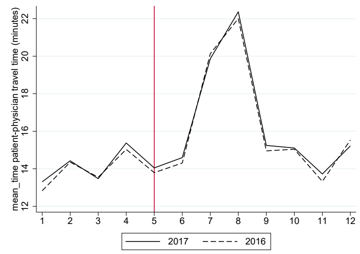


(b) Share of 65+

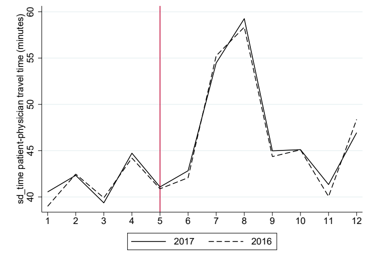


(c) Share of patients in same city

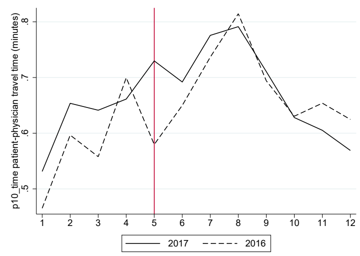
Figure B7: Evolution of the composition of patients (C: Sector 1 GPs in 2016)



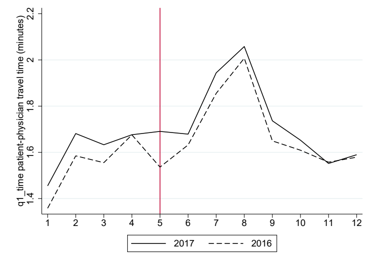
(a) Average travel time



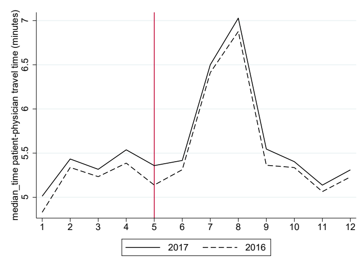
(b) Dispersion of travel times



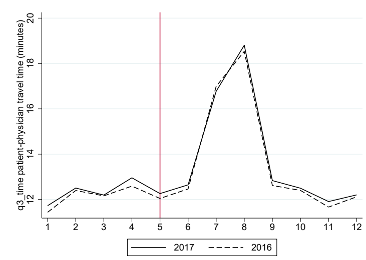
(c) P10 of travel times



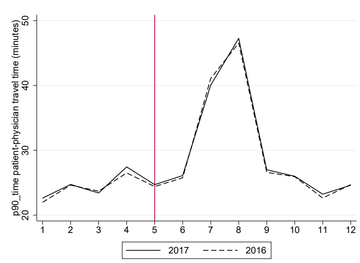
(d) P25 of travel times



(e) P50 of travel times

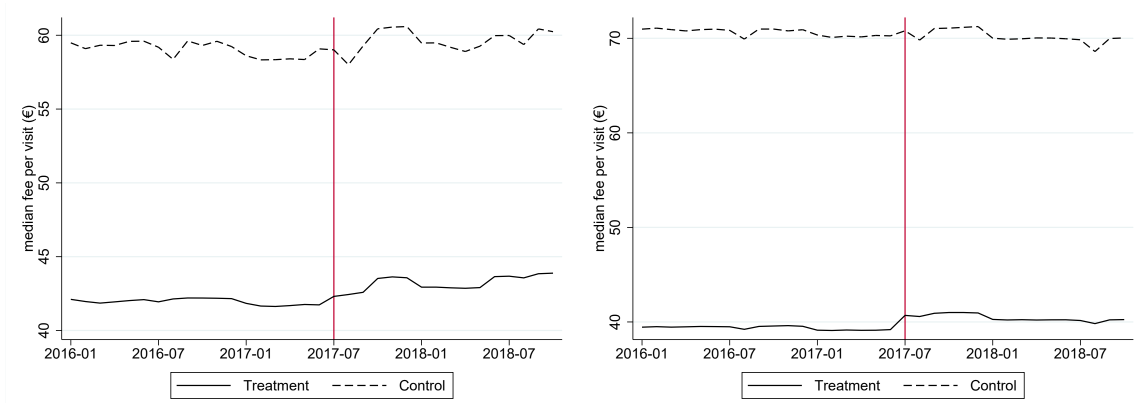


(f) P75 of travel times



(g) P90 of travel times

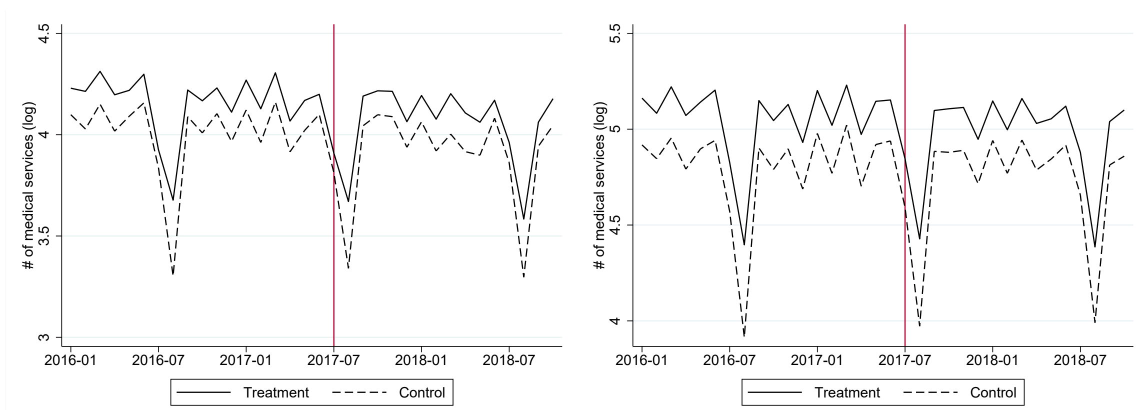
Figure B8: Evolution of the distribution of travel times at the doctor-month level (C: Sector 1 GPs in 2016)



(a) Cardiologists

(b) Psychiatrists

Figure B9: Change in specialists' fees (T: Sector 1, C: Sector 2)



(a) Cardiologists

(b) Psychiatrists

Figure B10: Evolution of specialists' activity (T: Sector 1, C: Sector 2)

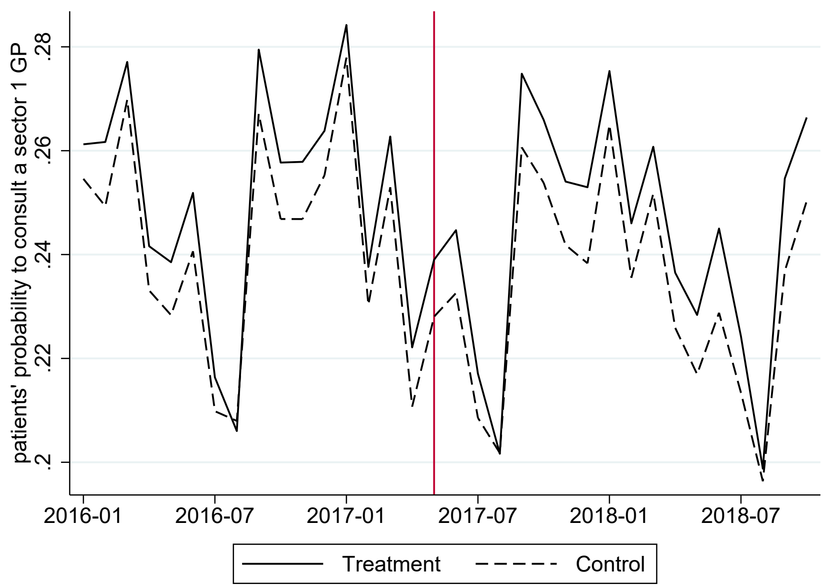


Figure B11: Evolution of patients' probability to visit a sector 1 GP (T: Cities with at least one sector 1 GP, C: Cities with no sector 1 GP)

C Event study analysis

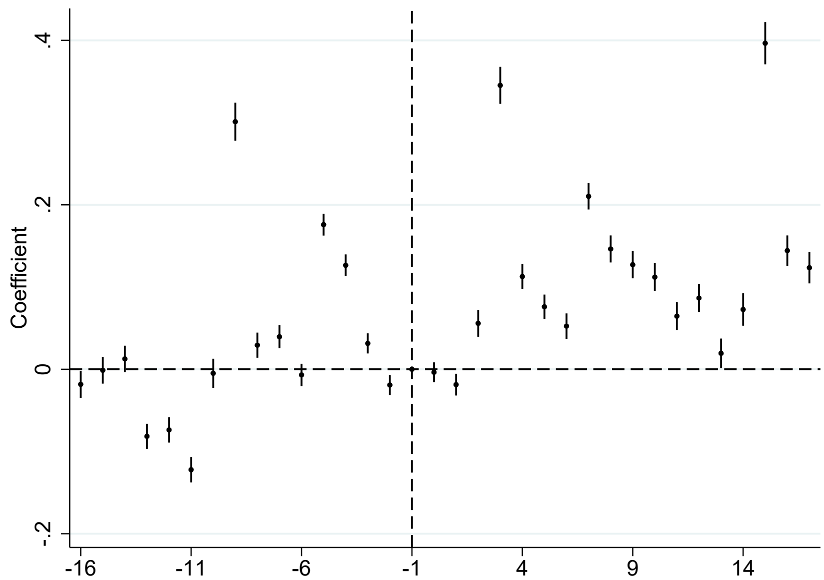
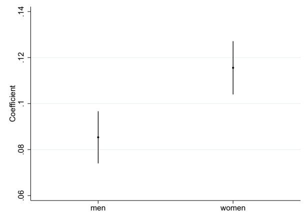
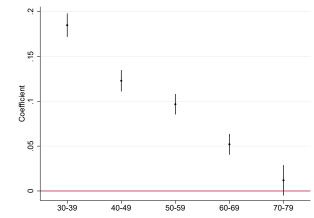


Figure C1: DiD estimates of dynamic TE (C: Direct access specialists)

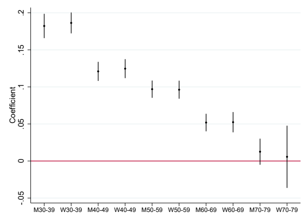
D Heterogeneity of treatment effects



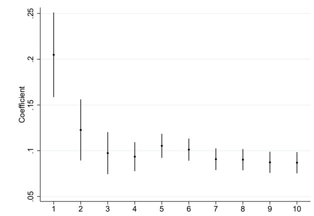
(a) Gender



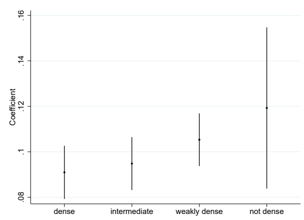
(b) Age



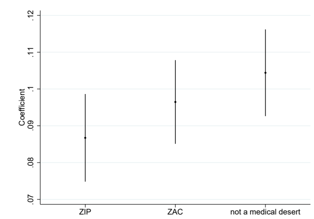
(c) Age × gender



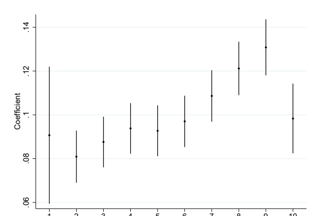
(d) Income (in 2016)



(e) Location



(f) Medical deserts



(g) Competition (Proxy: share of multiple-source patients)

Figure D1: Heterogeneity of treatment effects

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