

Do New Patients Displace Existing Patients' Treatment?*

Jori Barash[†]

April 2024

Abstract

This paper estimates the effect of a physician's number of registered patients ("enrollment") on short-run treatment intensity in the context of Norwegian primary care. I instrument for enrollment with quasi-random administrative patient assignments. The estimated effect of enrollment is negative but small for several measures of treatment intensity. For example, with one new patient registration, the average physician spends 3 fewer minutes per month across incumbent patients. Descriptive evidence suggests that crowd-out exacerbates undertreatment. Crowd-out is larger among physicians who reach their stated capacity or initially work part-time. To explain crowd-out, I draw on a model of physician decision-making and heterogeneity analysis. I find more evidence for capacity constraints than income effects, which suggests that increasing the number of physicians may more effectively reduce crowd-out than incentives for greater treatment per physician. Fixing physician supply, an alternative patient assignment rule could reduce crowd-out from administrative assignment by 86 percent.

JEL classification: I11, I14, J45, H51, I18

Keywords: Health care supply; Health care treatment intensity; Automatic enrollment; Physician retirement

*This work was supported by the Research Council of Norway [grant number 256678]. Data made available by Statistics Norway and the Norwegian Patient Registry have been essential for this project. I am grateful to my advisor Victoria Marone for guidance and to Yumin Hong, Bokyung Kim, Jinyeong Son, Marika Cabral, Hayden Parsley, and Margarita Petrushevich for helpful suggestions. Comments from participants at UT Austin seminars are gratefully acknowledged. Thank you to Laura Yon for excellent research assistance.

[†]Department of Economics, University of Texas at Austin. Email: joribarash@utexas.edu.

1 Introduction

Healthcare policy interventions often target the supply of physicians, underpinned by the assumption that physicians who take on too many patients may deliver an inadequate amount (“intensity”) of treatment. For example, regulatory bodies impose patient limits on physicians, and governments incentivize entry into underserved and less profitable areas.¹ The assumption aligns with economic theory, which predicts that an increase in a physician’s workload could lead to a decrease in patients’ treatment intensity, due to time constraints or stronger incentives for leisure. Suggestive evidence reinforces the theoretical prediction: health outcomes tend to be worse in areas with more patients per physician (Macinko, Starfield and Shi, 2007; Chang, O’Malley and Goodman, 2016; Falcettoni, 2018; Vallejo-Torres and Morris, 2018; Basu et al., 2019).

Causal evidence is limited on whether increasing the number of patients per physician impacts treatment or damages health. Related correlations may be biased because unobserved factors likely influence the equilibrium number of patients per physician. For example, patients with worse health may live in areas that are less attractive to physicians. Within an area, patients may choose to receive treatment in greater numbers from relatively high-quality physicians.

In this paper, I estimate the effect of a primary care physician’s number of registered patients (“enrollment”) on short-run treatment intensity. I find that enrollment lowers several measures of treatment intensity, but this crowd-out effect is economically small. I present a model of physician decision-making to illustrate competing explanations for crowd-out: capacity constraints and income effects. Building on the model, heterogeneity across physicians is most consistent with capacity constraints. Consequently, increasing the number of physicians may reduce crowd-out more effectively than incentives for existing physicians to increase treatment. I contextualize findings by bounding the effects of counterfactual registration policies on treatment intensity and introducing suggestive evidence that crowd-out exacerbates undertreatment.

Norway’s institutional features are ideal for isolating exogenous variation in enrollment from potential confounding factors. First, Norwegian municipalities often automatically reassign the patients of physicians exiting the market. Auto-reassignment to nearby physicians is quasi-random, so the resulting changes to enrollment are plausibly independent of patient health and physician quality. The mechanical increase in enrollment from auto-reassigned patients is compounded by the fact that other patients in

¹Limits on the number of patients exist in at least Norway, Denmark, and China.

the same region suddenly must choose among fewer nearby physicians. Both mechanisms result in a persistent increase in nearby physicians' enrollment. Second, Norway's registration system encourages long-term patient-physician relationships, which allows me to create a large balanced panel of patients. With a balanced panel, I can control for persistent heterogeneity and make relatively weak assumptions about patient composition and unobserved determinants of health. With an unbalanced panel, average treatment intensity may vary due to the idiosyncratic health of others joining or leaving the physician, leading to biased estimates. Third, national agreements dictate an identical reimbursement schedule for physicians, ruling out confounding changes to treatment intensity from financial incentives. In other contexts, physicians with greater enrollment may be able to negotiate higher reimbursement rates, which in turn incentivize greater treatment intensity and attenuate estimates. Fourth, virtually all of Norway's primary care physicians and residents are part of the country's universal public healthcare system, so the design of the registration system can have far-reaching consequences for health and public expenditure. Moreover, the scale of administrative data provides the statistical power to estimate even small effects, particularly for subsamples to test model predictions, e.g., part-time physicians or chronically ill patients.

I estimate the effect of enrollment on short-run treatment intensity using two-stage least squares, instrumenting for enrollment with the number of auto-reassigned patients. Each observation is a physician's aggregate monthly treatment intensity among incumbent patients. I find suggestive evidence supporting the exclusion assumption that auto-reassignments' timing and size are independent of factors determining changes to existing patients' health that are specific to a physician. First, the composition of nearby physicians' patients does not predict the number of auto-reassigned patients, conditional on availability, municipality, and at least one auto-reassigned patient. Second, prior to auto-reassignment, trends in incumbents' treatment intensity are uncorrelated with the number of auto-reassigned patients. I show this using a complementary event study design, comparing physicians with large and small auto-reassignments. Third, consistent with monotonicity and relevance assumptions, auto-reassignment shifts the entire distribution of enrollment increases relative to other months. To minimize threats to identification, I include fixed effects for each physician and event-month.

I find that an increase in enrollment results in no more than a small decrease in several measures of primary care treatment intensity among incumbent patients in the short run. For example, one new patient registration results in 3 fewer minutes of encounters among all incumbent patients, equivalently 0.05 percent of the mean. I do not find evidence of large decreases among alternative specifications or subsets of patients and physicians. I also do not find evidence of decreases in revenue, suggesting that

physicians replace high-value services like diagnostics and procedures with high-reimbursement services. When including both incumbents and new patients, I do not find evidence of a net increase in physicians' aggregate treatment intensity, consistent with capacity constraints. Physicians meet the needs of new patients with a commensurate decrease in treatment intensity among incumbent patients.

Heterogeneity in the effect of enrollment on physician hours helps distinguish between the mechanisms of income effects and capacity constraints. First, crowd-out effects appear concentrated among physicians who reach their stated capacity. Second, I do not find strong evidence of heterogeneity between physicians with different marginal incentives. Third, crowd-out effects appear concentrated among part-time physicians. Given other findings, this heterogeneity suggests that capacity constraints are idiosyncratic and physicians with lower capacity select into part-time work. Descriptive evidence is consistent with idiosyncratic constraints among a fraction of physicians: weekly hours bunch near a physician-specific maximum for a small number of physicians over ten years.²

Capacity constraints imply that increasing the supply of physicians would most reduce crowd-out. However, with large fixed costs of physician entry and small crowd-out effects, such a policy may not be cost-effective. Also, Norway's current policy limiting maximum enrollment may not have meaningful impacts on treatment intensity, because crowd-out effects are largest among low-workload physicians. Using a fixed supply of physicians, I explore the implications of a counterfactual registration policy. I simulate a new auto-reassignment rule for patients of departing physicians designed to minimize crowd-out. This exercise incorporates estimated heterogeneity in predicted crowd for combinations of patient and physician characteristics. Relative to status quo random assignment, targeted assignment can eliminate 86 percent of the crowd-out from status-quo auto-reassignment. Crowd-out may be inefficient if baseline treatment intensity is too low.

I contextualize findings with three stylized facts. First, incumbent patients in the analysis sample have lower initial treatment intensity and higher avoidable hospitalizations than observably similar patients in the full population (5 percent of the mean). Crowd-out effects increase that gap. Second, prior survey evidence suggests that both patients and physicians consider treatment intensity to be too low. Third, national statistics show that treatment intensity is low in Norway relative to most OECD countries.

To the best of my knowledge, this paper is the first to estimate the effect of the number of patients per physician on treatment intensity. As a design parameter within public healthcare systems, physician enrollment is understudied relative to patient cost-sharing (see Kiil and Houlberg, 2013, for a review) and

²By contrast, the distribution of physician labor supply does not bunch near a shared limit, e.g., 60 hours per week.

physician payments (Clemens and Gottlieb, 2014; Brekke et al., 2017; Einav, Finkelstein and Mahoney, 2018; Eliason et al., 2018; Cabral, Carey and Miller, 2021). This paper also complements studies of other healthcare disruption effects. First, I extend well-documented disruption effects in emergency care (Jena et al., 2017; Gruber, Hoe and Stoye, 2018; Hsia and Shen, 2019; Hoe, 2022) to the less-studied context of primary care. Representing a third of physicians in Norway, primary care physicians deliver the bulk of diagnoses, basic procedures, and chronic disease management, with large impacts on patient health, specialist referrals, and total healthcare spending. Second, I estimate the effect of a persistent shock to workload, rather than a temporary disruption (Shurtz et al., 2018; Harris, Liu and McCarthy, 2020; Freedman et al., 2021; Kovacs and Lagarde, 2022). Variation from persistent shocks may have stronger external validity for answering the central policy question of whether health systems have a sufficient supply of physicians. Other persistent shocks are relatively difficult to study due to simultaneous changes to incentives, e.g., insurance expansions (Garthwaite, 2012; Carey, Miller and Wherry, 2020). Third, I measure spillover effects of physician exit: I study the patients of nearby physicians while prior work focuses on the patients of departing physicians (Kwok, 2018; Fadlon and Van Parys, 2020; Bischof and Kaiser, 2021; Simonsen et al., 2021; Zhang, 2022; Sabety, 2023). Switching physicians disrupts care differently than workload shocks, e.g., new physicians may lack soft information from long-term relationships. I also show suggestive evidence of a limitation of this literature: patients’ choice of a new physician may be endogenous even if the timing of physician exit is exogenous. The few patients who make active choices experience a different trajectory of treatment intensity than those who are quasi-randomly reassigned.

The remainder of the paper proceeds as follows. Section 2 describes the empirical setting including identifying variation and data. Section 3 presents a model of physician-making and uses its comparative statics to guide the empirical strategy. Section 4 describes the baseline estimates of crowd-out, heterogeneity for subsamples of physicians and patients, and robustness. Section 5 discusses policy implications with suggestive evidence of status quo undertreatment and a counterfactual assignment rule. Section 6 concludes.

2 Background

2.1 Institutional Setting

Norway’s public healthcare system guarantees all residents access to a primary care physician. Norwegians frequently visit their physician – on average 3 times per year – for chronic disease management, consultations about symptoms, diagnostic tests, and general medical procedures, but also sick leave certification and referrals to specialists and non-urgent hospital care (Norway, 2023). Patients must be referred by physicians to specialists and non-emergency hospital visits. Norwegians may elect to purchase private health insurance to lower wait times for private specialists, but primary care nearly exclusively occurs within the public system.

To facilitate access, Norwegian primary care is carefully regulated. First, the number of registered patients per physician must generally remain between 500 and 2500 (Lovdata, 2017). Second, national guidance states that patients should not wait more than five days for a consultation in most circumstances. Within these limits, physicians should prioritize the treatment of patients with the greatest health need. Third, Norway regulates the supply of primary care physicians through a fixed number of contracts in each municipality. Physicians may move across municipalities or retire, but the total number of contracts and the locations of practices are slow to change. Fourth, patients register with physicians through a centralized system. Patients can request a new physician twice each year, but switches are rare (Iversen and Lurås, 2011). Physicians must accept all new patients until reaching a maximum number of patients. Physicians and municipalities must agree on the upper bound and weekly hours of operation to provide sufficient capacity for all nearby residents. As a result, these contract details rarely change. Fifth, prices for healthcare services are generally fixed across physicians. Some prices change once per year when renegotiated between the regulator and the physicians’ union (Legeforening, 2022). Physicians also receive a fixed fee for each registered patient, representing approximately 30 percent of revenue. In heterogeneity analyses, I exploit one exception to fixed prices: physicians with additional training receive supplementary reimbursement per patient visit. Sixth, when registered physicians are unavailable, patients can receive treatment at primary care emergency centers, which are comparable to stand-alone urgent care centers in the United States. Patients can also seek second opinions from other primary care physicians.³

The identifying variation in this paper comes from administrative assignment, which occurs when an

³On average, 80 percent of primary care visits are with the registered physician.

exiting physician ends his contract and no new physician accepts ownership of the patient list. Contracts typically end because the physician is retiring or moving to a different municipality. Exiting physicians provide the municipality with six months to find a replacement. In 30 percent of exits, no new physician accepts ownership of the list and the municipality notifies patients on the list that they might be reassigned. Approximately 8 percent of patients actively choose a new physician and the rest are administratively assigned. Patient reassignment is random among nearby physicians with enrollment lower than their upper bounds.⁴ Municipalities do not have access to patient characteristics when determining reassignment. Appendix Table A.2 verifies that patient composition is conditionally uncorrelated with auto-reassignments. Appendix Figure A.3 illustrates the distribution of enrollment increases induced by auto-reassignments. After being auto-reassigned, 83 percent of patients choose to stay at their assigned physician for at least six months. If auto-reassigned patients do not request a preferred physician, they may be auto-reassigned again to expand the patient list of entering physicians.

2.2 Data

The estimation sample is a balanced panel of physician-months in the six months before and after auto-reassignment. Outcomes reflect aggregated treatment intensity among incumbent patients who are registered to physicians both before and after auto-reassignment. The panel structure helps attribute short-term changes in treatment intensity to sudden changes in enrollment rather than the composition of patients' underlying treatment need. I construct the sample using restricted administrative records on registration, individual demographics, and healthcare reimbursement, which are maintained by the Norwegian Directorate of Health and Statistics Norway.⁵ These records contain nearly all of Norway's residents and primary care physicians from 2008 to 2017.

The estimation sample excludes potentially confounding variation. Each physician must practice in a single location during the entire period and each patient must be registered for the entire period. I also exclude physicians with fewer than 500 patients or with overlapping changes in reimbursement rates due to additional training. Both the physician and patient must have identification numbers to attribute treatment intensity to a particular physician of interest, which excludes recent migrants. I also exclude temporary replacement physicians and shared patient lists.

To assess pre-trends in treatment intensity before auto-reassignment, physicians must not receive

⁴Typically, only a few new patients are auto-reassigned to each nearby physician. Sometimes, administrative assignments coincide with an increase in physicians' upper bound on enrollment. This increase may reflect renegotiation so that a municipality can satisfy requirements for capacity.

⁵See Appendix A.1 for additional details.

Table 1: Summary Statistics: Treatment Intensity per Physician

Variable	Mean	Std. Dev.	10th Percentile	Median	90th Percentile
Enrollment	1,262.00	354.23	811.00	1,237.50	1,710.00
Visits	372.61	211.04	51.00	370.00	642.00
Spending	10,837.85	26,126.46	1,035.83	10,146.45	17,828.48
Hours	103.25	42.86	45.34	108.16	152.69
Unusual	23.18	21.94	3.00	18.00	48.00
Follow-up Visits	110.30	74.44	22.00	102.00	204.00
Bill Lines	1,140.72	701.29	321.00	1,064.50	2,011.00
Diagnostics	72.52	88.66	9.00	52.00	152.00
Procedures	273.99	215.61	57.00	228.00	540.00
per Visit	2.94	0.84	2.04	2.81	4.00
Physician-Spells	2,722				

Notes: Summary statistics reflect monthly totals six months prior to auto-reassignment. The unit of observation is an incumbent physician. A physician may appear more than once if they experience multiple auto-reassignment spells. All treatment intensity measures are restricted to primary care at the assigned physician. Spending (reimbursement) is measured in USD.

auto-reassignments from exiting physicians in the six months before a focal auto-reassignment. Physicians may receive subsequent auto-reassignments during the following six months. Moreover, I restrict auto-reassignments to those coming from exiting physicians with a dissolved patient list of at least 20 auto-reassigned patients. I exclude physicians who immediately lower their enrollment upper bound. In these cases, incumbent patients might be auto-reassigned. This restriction reinforces that physicians in the sample experience a persistent increase in enrollment from auto-reassignment. Appendix Table A.1 shows the number of physicians impacted by each sample condition.

The final analysis sample reflects nearly one-third of patients. Appendix Figure A.4 shows that sample restrictions result in only negligible changes to the distribution of enrollment changes. The vast majority of incumbents experience a small number of auto-reassigned patients joining their physician. In 64 percent of physician-spells, only 1 patient is initially auto-reassigned. The final sample reflects 2,335,982 patients at 2,065 unique physicians. Table 1 shows the distribution of aggregate treatment intensity six months before new patients are auto-reassigned to the physician. Treatment intensity varies widely across physicians, and the most physicians spend less than 40 hours per week with patients.

3 Research Design

3.1 Conceptual Framework

To guide intuition about the treatment intensity effects of increased enrollment, I extend the model of physician decision making in Ellis and McGuire (1986). Distinguishing between mechanisms is important because disruption in treatment intensity due to income effects can be mitigated through reimbursement incentives with a fixed supply of physicians, while avoiding disruption due to capacity constraints may require increasing the supply of physicians.⁶

Consider a physician who simultaneously chooses treatment intensity m_i for each registered patient $i \in 1, \dots, N$, given that patient's initial illness severity. The physician has additive preferences over net income, leisure, and health production. Each patient draws a random severity from a distribution $\lambda_i \sim F$. Importantly, F is fixed for the incumbent patients of a physician when enrollment N changes. Health production $h(m_i, \lambda)$ is a function that measures the value of health given each patient's initial severity and treatment intensity. Net income $\sum_i \pi(m_i)$ and workload $\sum_i m_i$ aggregate across patients, so the choice of one patient's m_i depends on other patients' severity $\lambda_{i'}$. The physician's relative weight on health production is altruism α . Functions u, π, h are strictly increasing and weakly concave, while distaste for workload l is strictly increasing and strictly convex. Finally, the physician has a capacity constraint on workload, $\sum_{i'} m_{i'} \leq \bar{M}$, with shadow cost μ . The problem takes the form,

$$\begin{aligned} \max_{m_i \geq 0, \forall i \in 1, \dots, N} & u \left(\sum_i \pi(m_i) \right) - l \left(\sum_i m_i \right) + \alpha \sum_i h(m_i, \lambda) \\ \text{s.t.} & \sum_i m_i \leq \bar{M} \quad [\mu] \end{aligned}$$

with the following optimality conditions $\forall i' \in 1, \dots, N$,

$$\begin{aligned} u' \left(\sum_i \pi(m_i) \right) \pi'(m_{i'}) - l' \left(\sum_i m_i \right) + \alpha h_{m'}(m_{i'}, \lambda) + \mu &= 0 \\ \sum_i m_i &\leq \bar{M}, \end{aligned}$$

resulting in the optimal choice of treatment intensity $m_i^*(\lambda_1, \dots, \lambda_N)$.

⁶Perhaps a regulator can incentivize investments in increased capacity, but healthcare is labor-intensive with respect to high-skilled labor, technology investments are unlikely to change the fundamental tradeoffs, especially in the short and medium run.

If enrollment N exogenously increases and capacity is slack ($\mu = 0$), then expected treatment intensity decreases through income effects. In expectation, new patients require some treatment, resulting in higher aggregate treatment intensity, which lowers leisure, weakly increasing the marginal utility of leisure and decreasing the marginal utility of net income. The physician responds by setting the loss in marginal health production equal to the loss in marginal utilities of net income and leisure. For each realization of severity λ , these changes unambiguously lower optimal treatment intensity for an incumbent patient. Enrollment only lowers treatment intensity in expectation because realized treatment intensity may increase due to idiosyncratic draws of λ .

Curvature of preferences results in larger decreases in treatment intensity, e.g., if the physician's initial workload is large. A physician with a relatively large initial workload has a high marginal distaste for incremental treatment intensity relative to leisure. When enrollment increases, any new treatment intensity from new patients has to be compensated by relatively large decreases in treatment intensity for incumbent patients. On the other hand, there is an ambiguous relationship between crowd-out and observed correlates of severity λ . If sicker patients benefit more from marginal treatment ($\frac{d^2 h}{dm d\lambda} > 0$), then their crowd-out effects should be smaller.

The capacity constraint also drives incumbents' treatment intensity to decrease in enrollment. When the constraint binds, the total workload stays the same, so incremental treatment for new patients must correspond to lower treatment for incumbents. The distaste for the total workload is unaffected. A binding capacity constraint implies that no policy can fully counteract the short-run crowd-out effect of increased enrollment. By contrast, with slack capacity, income effects imply that well-targeted reimbursement incentives ($\frac{d\pi}{dm}$) can mitigate crowd-out effects.

The intuition behind these comparative statics also holds with several extensions to the model. First, F can vary arbitrarily across patients and physicians. Second, u, π, l, α, h , and \bar{M} can vary arbitrarily across physicians as long as they are unaffected when N exogenously changes. Third, physicians may choose treatment intensity sequentially in expectation over other patients' severity.

3.2 Empirical Strategy

To estimate the effect of enrollment on treatment intensity, I use two-stage least squares:

$$Y_{jt} = \beta_1 \widehat{\text{Enroll}}_{jt} + \beta_j + \beta_t + \epsilon_{jt} \quad (1)$$

$$\text{Enroll}_{jt} = \gamma_1 \text{Auto}_{jt} + \gamma_j + \gamma_t + \varepsilon_{jt}. \quad (2)$$

Here, Y_{jt} is the outcome of interest, e.g., physician hours with incumbent patients, where subscript j indexes physicians and t indexes months relative to auto-reassignment ($t = -6, -5, -4, \dots, 4, 5, 6$). Enroll_{jt} is physician j 's total enrollment in month t , including incumbents and newly joined patients. Auto_{jt} reflects the cumulative number of patients auto-reassigned.⁷ β_1 is the coefficient of interest, representing the causal effect of increasing enrollment by one patient. γ_1 represents the effect of one recently auto-reassigned patient on enrollment. β_j is a fixed effect for physician-spell j , controlling for unobserved heterogeneity in average treatment intensity across patient.⁸ β_t residualizes trends in treatment intensity relative to auto-reassignment, absorbing general disruption effects of auto-reassignment. ϵ_{jt} and ε_{jt} are random idiosyncratic errors. All regressions cluster standard errors by physician. The empirical strategy requires three identifying assumptions.

EXCLUSION. The exclusion assumption is that the number of auto-reassigned patients only affects incumbent patients' treatment intensity through the change in enrollment. In the econometric model, physician-spell fixed effects absorb time-invariant characteristics, and event-month fixed effects absorb physician-invariant trends from receiving auto-reassigned patients, so the timing and size of auto-reassignment must be independent of factors determining *changes* to incumbent illness severity that are specific to a physician, e.g., missed preventative care or a local viral outbreak.

Ginja et al. (2022) report that administrative assignment is conditionally random and show that assigned physicians' value-added does not correlate with reassigned patient characteristics. In particular, auto-reassignment should be random conditional on availability and municipality, which are absorbed by physician-spell fixed effects. Appendix Table A.2 verifies that the number of auto-reassigned patients does not correlate with destination physician characteristics conditional on availability, a shared municipality, and the exiting physician. Fixed effects absorb any level differences in treatment intensity across physicians.

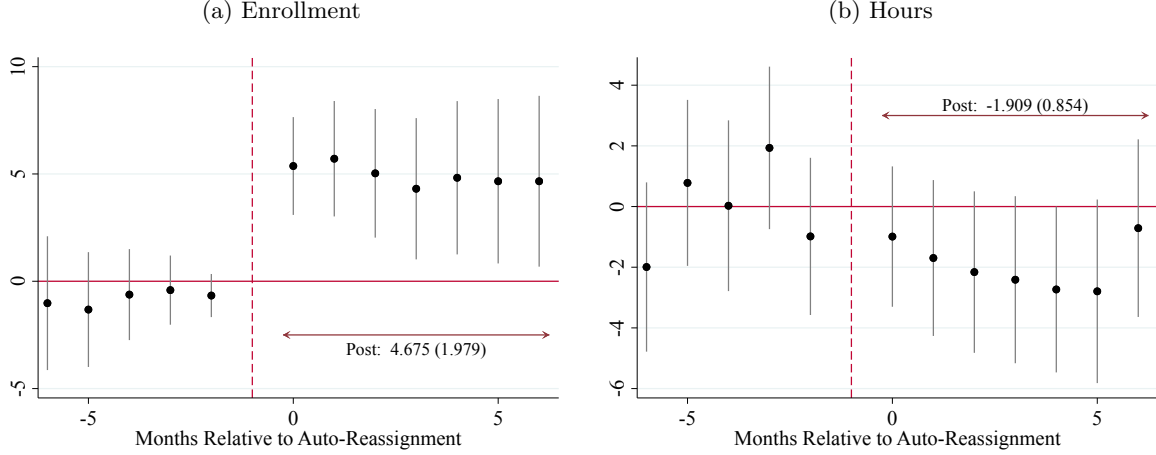
Likewise, physicians with relatively large or small auto-reassignments have similar trends in outcomes prior to auto-reassignment. Figure 1 uses an alternative event-study design to show that differences in the trend before auto-reassignment are not statistically distinguishable from zero for enrollment and hours. Appendix Figure A.2 shows the trends in mean outcomes for both groups of physicians and Appendix Figures A.6 and A.7 show event-study estimates for other outcomes.

RELEVANCE AND MONOTONICITY. Auto-reassignments must strictly increase enrollment. Identifying

⁷ Auto_{jt} is similar to the interaction of post-treatment and treatment status in a difference-in-differences design, where $\text{Auto}_{jt} = 0 \forall t < 0$. Most auto-reassigned patients register in event-month 0. Auto_{jt} is cumulative to reflect that additional patients are sometimes auto-reassigned after event-month 0.

⁸ A physician-spell includes the six months before and after auto-reassignment for physician receiving auto-registrations.

Figure 1: Trends Among Incumbent Patients: Large versus Small Auto-Reassignments



Notes: This figure shows estimates and 95% confidence intervals of the difference between physicians with large and small auto-reassignments in each month relative to auto-reassignment. The omitted month is -1. I define large auto-reassignments as those with two or more reassigned patients (969 of 2,722 physician-spells). Each panel represents a separate regression with the dependent variable as indicated. The physician-month sample reflects aggregate treatment intensity among a fixed set of incumbent patients. Standard errors are clustered at the physician level. Estimates refer to β_{1t} in the following regression: $Y_{jt} = \beta_{0t} + \beta_{1t}\text{AutoHigh}_j + \beta_j + \epsilon_{jt}$. “Post” indicates the average difference in outcomes after auto-reassignment between physicians with large and small auto-reassignments. Standard errors are in parentheses, and the coefficient is β_1 in the following regression: $Y_{jt} = \beta_{0t} + \beta_1\text{AutoHigh}_j \times 1[t \geq 0] + \beta_j + \epsilon_{jt}$.

variation for enrollment comes from the fact that auto-reassigned patients are joining rather than exactly replacing incumbent patients. The relevance assumption is directly testable through the first-stage F-statistic ($F > 117$ in the baseline specification). Consistent with a mechanical increase in enrollment, the first stage coefficient is precise and indistinguishable from 1. Appendix Figure A.4 shows that auto-reassignments shift the full distribution of enrollment changes rightward. In months with auto-reassignments, large enrollment increases become more likely and small increases (or decreases) become less likely. Figure 1 shows that physicians with large auto-reassignments experience a sudden precise increase in enrollment that is persistent. The point estimate implies that enrollment increases by five more patients between physicians with large auto-reassignments, i.e., more than one auto-reassigned patient.

4 Enrollment and Treatment Intensity

4.1 Baseline Results

Table 2 shows that enrollment lowers physician treatment intensity, but the effects are small. One additional patient lowers hours spent in incumbent patient encounters by 0.048, equal to 0.05 percent of the

mean. Importantly, this crowd-out effect is shared among all incumbents. With average initial enrollment of 1262, each incumbent loses just 0.14 seconds of treatment per month. Bill lines, which reflect the count of distinct services, decrease by 0.342. Estimates for total reimbursement and patient visits are imprecise, but the confidence intervals rule out large decreases. Counterintuitively, the point estimate on spending is positive. Taken at face value, estimates suggest that enrollment affects the qualitative composition of treatment, replacing relatively low-reimbursement treatment with high-reimbursement alternatives. Columns (6)-(10) consider effects on subsets of treatment. Physicians perform fewer services per visit. Moreover, relative crowd-out is similar among high-value services like procedures and diagnostics. Hours outside of conventional shifts and services per visit also decline with enrollment. Finally, Column (1) shows that cumulative auto-reassigned patients is a relevant instrument for enrollment. The point estimate implies that one additional auto-reassigned patient increases enrollment by approximately 1.

By contrast, Appendix Table A.3 fails to find evidence that enrollment increases physician’s total labor supply. In this table, outcomes reflect the treatment intensity among all registered patients rather than only incumbent patients. Point estimates are generally small and imprecise, with confidence intervals ruling out large changes. No effect on total treatment intensity would be consistent with the theoretical framework: physicians meet the needs of new patients by reducing the treatment intensity of existing patients.

4.2 Heterogeneity and Mechanisms

Drawing on the comparative statics of the conceptual framework, physician heterogeneity helps distinguish income effects and capacity constraints as explanations for crowd-out. In this section, I focus on heterogeneity in the effect of enrollment on physicians’ total hours spent treating incumbent patients. Appendix Tables A.5, A.6, A.7, and A.8 show heterogeneity for the first-stage and other measures of treatment intensity.

First, columns (1) and (2) suggest that crowd-out is concentrated among physicians who have plausibly binding capacity. To proxy capacity, I divide the sample based on whether physicians reach 99 percent of their contracted upper bound on enrollment. For physicians near capacity, crowd-out is precise and similar in magnitude to the full sample. For physicians with slack capacity, the point estimate is imprecise. The subsample with binding capacity has a greater average workload, so income effects could also explain the heterogeneity.

Table 2: Effect of Enrollment Among Incumbent Patients

	Enrollment (1)	Hours (2)	Bill Lines (3)	Spending (4)	Visits (5)
Cuml. Auto-Joins	1.055 (0.055) [<0.001]				
Enrollment		−0.048 (0.026) [0.070]	−0.345 (0.055) [<0.001]	2.230 (4.597) [0.628]	0.004 (0.152) [0.980]
Dep. Var. Mean	1274.917	101.349	1126.464	10299.286	370.160
F-Statistic	117.129	11.473	12.550	8.827	8.950
Observations	35,386	35,386	35,386	35,386	35,386
	Unusual Hours (6)	Lines Per Visit (7)	Procedures (8)	Diagnostics (9)	Follow-ups (10)
Enrollment	−0.014 (0.007) [0.032]	−0.002 (0.000) [<0.001]	−0.038 (0.019) [0.045]	−0.058 (0.015) [<0.001]	−0.023 (0.021) [0.268]
Dep. Var. Mean	22.940	2.939	70.908	270.428	135.084
F-Statistic	3.161	6.102	3.750	9.120	88.335
Observations	33,403	34,576	35,386	35,386	35,386

Notes: This table displays estimates of coefficients from regressions of Equations 1 and 2. Each column represents a separate regression with the dependent variable as indicated in the table. The physician-month sample reflects aggregate treatment intensity among a fixed set of incumbent patients. Standard errors clustered at the physician level are reported in parentheses, and p-values are reported in brackets. "Cuml. Auto-Joins" indicates the number of patients auto-reassigned to a physician since the start of the spell. Regressions also include fixed effects for spell and event-month.

Second, columns (3) and (4) show similar noisy estimates of crowd-out for low- and high-fee physicians. Over half of primary care physicians are certified as general practitioners, and certification entitles these physicians to supplementary reimbursement for each patient visit. All else equal, with slack capacity constraints and strong income effects, low-fee physicians would have more crowd-out. By contrast, if capacity constraints bind for both sets of physicians, then the difference in incentive would have no impact. Without a large difference in estimates across subsamples, additional counterfactual reimbursement may be unlikely to eliminate crowd-out.

Third, columns (5) and (6) show that crowd-out is concentrated among part-time physicians, which is inconsistent with symmetric income effects. I classify physicians as full-time if they treat patients for at least six hours per weekday before auto-reassignment. Part-time physicians have a lower initial workload, so greater crowd-out is surprising regardless of whether income effects or (symmetric) capacity constraints dominate. All else equal, with income effects, part-time physicians should have a lower marginal taste for

Table 3: Heterogeneity in the Effect of Enrollment on Hours

	Reached Capacity		High Fee		Full-Time	
	(1)	(2)	(3)	(4)	(5)	(6)
Enrollment	−0.025 (0.023) [0.281]	−0.051 (0.027) [0.057]	−0.045 (0.027) [0.096]	−0.039 (0.030) [0.197]	−0.083 (0.004) [<0.001]	−0.005 (0.010) [0.609]
Subsample	X		X		X	
Dep. Var. Mean	94.515	107.514	84.576	113.236	78.104	113.770
1 st Stage F-Stat.	33.577	134.240	83.519	46.769	100.088	66.967
Observations	16,783	18,603	14,677	20,709	12,324	23,062
	Age 65+		Chronic Illness		Female	
	(7)	(8)	(9)	(10)	(11)	(12)
Enrollment	−0.042 (0.020) [0.035]	−0.007 (0.006) [0.248]	−0.037 (0.020) [0.061]	−0.012 (0.006) [0.045]	−0.013 (0.007) [0.068]	−0.036 (0.019) [0.053]
Subsample	X		X		X	
Dep. Var. Mean	72.288	29.061	58.032	43.316	42.905	58.444
1 st Stage F-Stat.	112.037	112.037	112.037	112.037	112.037	112.037
Observations	35,386	35,386	35,386	35,386	35,386	35,386

Notes: This table displays estimates of coefficients from regressions of Equation 2. Each column represents a separate regression among a subsample with the dependent variable as indicated in the table. Columns (1-6) reflect aggregate treatment intensity among all incumbent patients for subsets of physicians. Columns (7-12) reflect aggregate treatment intensity among subsets of incumbent patients. For example, Column (1) includes physicians with enrollment consistently less than 99 percent of initial stated capacity, and Column (2) includes all other physicians. Standard errors clustered at the physician level are reported in parentheses, and p-values are reported in brackets. High-fee physicians receive supplementary reimbursement for each visit due to a certificate from additional training. Full-time physicians treat patients for at least six hours per weekday, on average, during the six months prior to auto-reassignment. Regressions also include fixed effects for spell and event-month.

leisure and smaller crowd-out. Likewise, with a symmetric capacity constraint, e.g., 60 hours per week, capacity should be slack for part-time physicians, and crowd-out should be smaller or non-existent. To further distinguish between explanations for crowd-out, Appendix Figure A.5 plots the distribution of weekly hours. Although hours do not bunch near a global maximum (Panel A), hours sometimes bunch near a physician-specific maximum (Panel B). Taken together, this evidence suggests that, at least in the short run, physicians have idiosyncratic capacity constraints. Idiosyncratic capacity constraints also rationalize the precise crowd-out among physicians that reach their stated capacity, which can be as low as 500 patients.

Finally, columns (7)-(12) show estimates of crowd-out among six subsets of patients. In each subset, outcomes reflect sums of treatment intensity among the indicated subset of incumbent patients. The sample still includes all physician-months in the sample. Point estimates are marginally precise and rela-

tively large among young, not chronically ill, and female patients. To the extent that such characteristics proxy for low severity, this heterogeneity is consistent with marginal crowd-out less negatively impacting the health of low-severity patients.

4.3 Robustness

Table 4 shows that the effect of enrollment on hours is small, similar in magnitude, and generally precise across nine alternative specifications and samples.

Table 4: Robustness of the Effect of Enrollment on Hours

	Estimate			Mean	F-Stat.	Obs.
(1) Add Controls	-0.044	(0.030)	[0.149]	101.349	88.295	35,386
(2) Top 5%	-0.045	(0.028)	[0.111]	96.568	77.400	2,158
(3) Drop Event-Month	-0.045	(0.024)	[0.056]	101.349	100.906	35,386
(4) Calendar Month	-0.061	(0.023)	[0.007]	101.349	114.135	35,386
(5) Hours Always 8+	-0.034	(0.020)	[0.095]	110.662	69.265	30,472
(6) Drop Middle Months	-0.056	(0.030)	[0.064]	101.028	61.412	27,220
(7) Avoidable Hosp.	-0.081	(0.005)	[<0.001]	103.043	88.793	32,097
(8) Alt. 1st Stage	-0.050	(0.026)	[0.053]	101.349	3509.565	35,386
(9) Weighted	-0.057	(0.022)	[0.011]	101.349	98.327	35,386

Notes: This table displays estimates of coefficients from regressions that vary the specification in Equation 2. Standard errors clustered at the physician level are reported in parentheses, and p-values are reported in brackets. The other columns are the mean of the dependent variable, the first-stage F-statistic, and the number of observations (physician-months). Variations to the specification are not cumulative. Specification (1) adds controls, including stated capacity, system entry and exit, and average patient characteristics: age 65+, chronic illness, female, income, and disability benefit receipt. (2) subsets to spells with auto-reassignments per incumbent above the 95th percentile. (3) drops fixed effects for months relative to auto-reassignment. (4) includes fixed effects for calendar months, e.g., January. (5) subsets to spells during which physician total hours are always above eight per month. (6) drops observations within 1 month of auto-reassignment. (7) subsets to spells that begin with at least one avoidable hospitalization. (8) Alt. 1st Stage uses an alternative specification: enrollment is a function of event-months interacted with the number of total auto-reassignments per incumbent during the 13-month spell. (9) weights observations by the number of incumbents.

First, I add physician time-varying controls for stated capacity and patient composition, e.g., the share age 65 or older and mean income. The point estimate is stable which further allays concerns about instrument validity. Second, I restrict the sample to physicians who receive the most auto-reassigned patients, producing a nearly identical point estimate. The estimate might change if auto-reassignment directly affected incumbents' treatment intensity, i.e., not through enrollment. Third, I remove fixed effects for the month relative to auto-reassignment. Fourth, I add fixed effects for calendar months to control for seasonality. Fifth, I exclude physicians who ever have less than eight hours of treatment per month to address the possibility of intertemporal substitution. Physicians might respond to a temporary

constraint in one month by increasing treatment intensity in another month. Sixth, for the same reason, I exclude the month of auto-reassignment and surrounding months. Seventh, I subset to physicians that have an avoidable hospitalization six months before auto-reassignment. With the salience of an avoidable hospitalization, incumbent crowd-out might be limited. Instead, the point estimate is larger but still small. Eighth, I use an alternative specification for the first stage which interacts total auto-reassignments with the number of months relative to auto-reassignment. Ninth, I weight observations by the number of patients because underlying treatment decisions are at the patient level and effects might correlate with baseline enrollment.

5 Policy Discussion

The previous sections show that registration of new patients lowers the treatment intensity of existing patients. The effect is small, and idiosyncratic capacity constraints offer the best explanation. Going forward, I consider the policy implications of these findings. Section 5.1 explores whether crowd-out should be avoided. Several stylized facts suggest that baseline treatment intensity is low in Norway, so crowd-out may exacerbate undertreatment. To eliminate crowd-out, capacity constraints imply that incentivizing the entry of physicians would be most effective. However, Norwegian municipalities already struggle to recruit physicians (of Health and Welfare, 2023), so such policies may be prohibitively expensive. Section 5.2 bounds the effects of a relatively costless intervention. Instead of randomly assigning patients of exiting physicians, Norway can target new patient-physician assignments with low expected crowd-out and eliminate 86 percent of the effect.

5.1 Broader Evidence of Undertreatment

Three pieces of evidence suggest that auto-reassigned patients inefficiently lower the treatment intensity of incumbent patients. First, I use data on the universe of Norwegian primary care utilization to benchmark the treatment intensity and health outcomes of incumbent patients in the analysis sample. Appendix Table A.4 shows this comparison, using all Norwegian patient-months in 2015. The coefficient of interest is the mean difference for in-sample patient-months after flexibly controlling for month, age, gender, tenure, and chronic illnesses. Relative to a population mean of 0.122 hours per month, in-sample patients receive 0.006 fewer hours of treatment (p-value < 0.001). Fewer hours do not necessarily imply undertreatment, e.g., if out-of-sample patients receive too much treatment. Comparing health

outcomes within Norway helps alleviate this concern: relative to a mean of 0.0034 avoidable hospitalizations, in-sample patients have 0.0002 more avoidable hospitalizations (p-value ≤ 0.01). Taken together, these associations highlight the potential for policy interventions to mitigate crowd-out of primary care treatment intensity and perhaps improve health outcomes.

Second, as shown in prior surveys, Norwegian residents are concerned about primary care undertreatment. Over 25 percent express dissatisfaction with the duration of consultations (Kjøllesdal et al., 2020). This number is quite large relative to treatment intensity. On average, only 20 percent of residents visit their registered primary care physician each month. Likewise, despite national guidance requiring near-immediate access to physicians, 40 to 50 percent of patients are not satisfied with wait times for appointments (Bjertnæs et al., 2023; Kjøllesdal et al., 2020). Long wait times can lead to adverse health outcomes like avoidable hospitalizations when symptoms are not promptly addressed. With short appointments and long wait times, patients may receive inadequate specialist referrals: 25 percent of patients do not express satisfaction with referrals (Kjøllesdal et al., 2020).

Likewise, Norwegian physicians express dissatisfaction about time spent with patients. Rosta, Aasland and Nylenna (2019) document declining overall job satisfaction, largely driven by increasing dissatisfaction with work hours, amount of responsibility given, and freedom to choose treatment methods. In focus groups, physicians express concern that their workloads are growing and large enough to cause issues for patient safety and physician motivation (Svedahl et al., 2019).

Third, as of 2019, average physician visits are much lower in Norway than OECD countries overall: 4.5 vs. 6.8 (Tikkanen and Abrams, 2020). If on average, patients in OECD countries receive appropriate treatment, then lower visits in Norway would be consistent with undertreatment. Cross-country evidence is more mixed when considering specific primary care services. Relative to population, fewer elderly residents are immunized but more women are screened for breast cancer. Avoidable hospitalizations from diabetes and hypertension are relatively low. Finally, although capacity constraints best explain crowd-out, it may be difficult for Norway to increase capacity by hiring additional physicians. Norway has among the highest physicians per capita: 4.8 per 100,000 vs. an average of 3.5.

5.2 Policy Counterfactual

Since current auto-reassignments crowd out incumbent patients' treatment intensity, and baseline treatment is relatively low, Norwegian policymakers might consider replacing random assignment with tar-

geted assignment.⁹ I show that targeted assignment can lower crowd-out by 86 percent.

First, I estimate crowd-out effects for four subsamples including both physician heterogeneity and patient heterogeneity. High-type physicians either work part-time at baseline or reach 99 percent of stated capacity with reassigned patients. High-type patients are younger than 65. Second, I use point estimates to predict counterfactual crowd-out.¹⁰ For each auto-reassignment event, I only consider assignments to physicians in the same municipality. For each physician, crowd-out is a sum of effects among young and old patients. If physicians in the same municipality do not have enough spots, I assume remaining reassignments to other municipalities have the worst-case crowd-out.

For the counterfactual, I first ration auto-reassigned patients to the physicians with the lowest total crowd-out, until updated enrollment reaches 99 percent of stated capacity. When enrollment approaches stated capacity, crowd-out is higher. I compare aggregate predicted hours of crowd-out under this counterfactual to random assignment across available slots within a municipality.¹¹ With targeting, predicted crowd-out over ten years drops from 83,877 to 11,483 hours.

6 Conclusion

This paper provides causal evidence that new patients minimally crowd out the short-run primary care treatment intensity of existing patients. I leverage administrative assignment of new patients following physician exit for exogenous variation in nearby enrollment. This finding implies that physicians can shift along their labor supply curve without large frictions. In Norway, relaxing the regulated bounds on enrollment might allow highly demanded physicians to serve more patients without excessive declines in existing patients' treatment intensity. For example, Denmark allows physicians to choose patients after enrollment reaches an upper bound (Forde et al., 2016). Likewise, existing subsidies for rural physicians through supplementary capitation may be justified.¹² To minimize crowd-out, policymakers might consider linking subsidies to opening hours. If the finding holds in other institutional settings, policies like insurance expansions that increase the number of covered patients can increase healthcare access without resulting in large negative effects for the already-insured. However, estimates may not be informative in settings where patients' treatment needs already meet or exceed physicians' latent

⁹Changes in treatment intensity may not fully reflect changes in welfare from this counterfactual, e.g., if patients have low taste for physicians with low crowd-out.

¹⁰I estimate imprecise positive point estimates for subsets with low crowd-out physicians. To be conservative, I treat these as zero reduction in hours per auto-reassigned patient.

¹¹As before, I assume reassignments to other municipalities have the worst-case crowd-out.

¹²Norway has an equalization grant for physicians in municipalities with fewer than 5000 residents.

capacity.

Guided by a theoretical framework, heterogeneity in treatment effects suggests that idiosyncratic capacity constraints best explain observed crowd-out. To contextualize estimated crowd-out effects, evidence from out-of-sample comparisons, patient and physician surveys, and cross-country comparisons suggest that baseline treatment intensity is low. As a result, policymakers may seek to increase treatment intensity, especially for incumbent patients of physicians with rising enrollment. Since idiosyncratic capacity constraints better explain crowd-out than income effects, expanding capacity by subsidizing physician entry may better reduce crowd-out than incentivizing existing physicians to work more hours. However, both policies may be quite costly relative to the small level of crowd-out. Simulations suggest that Norway can eliminate 86 percent of crowd-out by replacing random auto-reassignment with targeted re-assignment.

References

- Basu, Sanjay, Seth A Berkowitz, Robert L Phillips, Asaf Bitton, Bruce E Landon, and Russell S Phillips.** 2019. “Association of Primary Care Physician Supply With Population Mortality in the United States, 2005-2015.” *JAMA internal medicine*, 179: 506–514.
- Bischof, Tamara, and Boris Kaiser.** 2021. “Who cares when you close down? The effects of primary care practice closures on patients.” *Health Economics*.
- Bjertnæs, Øyvind, Hilde Hestad Iversen, Rebecka Norman, and Jose M. Valderas.** 2023. “Web-Based Public Ratings of General Practitioners in Norway: Validation Study.” *JMIR Formative Research*, 7: e38932.
- Brekke, Kurt R., Tor Helge Holmås, Karin Monstad, and Odd Rune Straume.** 2017. “Do treatment decisions depend on physicians’ financial incentives?” *Journal of Public Economics*, 155: 74–92.
- Cabral, Marika, Colleen Carey, and Sarah Miller.** 2021. “The Impact of Provider Payments on Health Care Utilization: Evidence from Medicare and Medicaid.”
- Carey, Colleen M., Sarah Miller, and Laura R. Wherry.** 2020. “The Impact of Insurance Expansions on the Already Insured: The Affordable Care Act and Medicare.” *American Economic Journal: Applied Economics*, 12: 288–318.
- Chang, Chiang-Hua, A. James O’Malley, and David C. Goodman.** 2016. “Association between Temporal Changes in Primary Care Workforce and Patient Outcomes.” *Health Services Research*, 52: 634–655.
- Clemens, Jeffrey, and Joshua D. Gottlieb.** 2014. “Do Physicians’ Financial Incentives Affect Medical Treatment and Patient Health?” *American Economic Review*, 104: 1320–1349.
- Dahlstrand, Amanda.** 2022. “Defying Distance? The Provision of Services in the Digital Age.”
- Einav, Liran, Amy Finkelstein, and Neale Mahoney.** 2018. “Provider Incentives and Healthcare Costs: Evidence from Long-Term Care Hospitals.”
- Eliason, Paul J., Paul L. E. Grieco, Ryan C. McDevitt, and James W. Roberts.** 2018. “Strategic Patient Discharge: The Case of Long-Term Care Hospitals.” *American Economic Review*, 108: 3232–3265.
- Ellis, Randall P., and Thomas G. McGuire.** 1986. “Provider behavior under prospective reimbursement.” *Journal of Health Economics*, 5: 129–151.

- Fadlon, Itzik, and Jessica Van Parys.** 2020. "Primary care physician practice styles and patient care: Evidence from physician exits in Medicare." *Journal of Health Economics*, 71: 102304.
- Falchetti, Elena.** 2018. "The Determinants of Physicians' Location Choice: Understanding the Rural Shortage." *SSRN Electronic Journal*.
- Forde, Ian, Carol Nader, Karolina Socha-Dietrich, Jillian Oderkirk, and Francesca Colombo.** 2016. "Primary Care Review of Denmark."
- Freedman, Seth, Ezra Golberstein, Tsan-Yao Huang, David J. Satin, and Laura Barrie Smith.** 2021. "Docs with their eyes on the clock? The effect of time pressures on primary care productivity." *Journal of Health Economics*, 77: 102442.
- Garthwaite, Craig L.** 2012. "The Doctor Might See You Now: The Supply Side Effects of Public Health Insurance Expansions." *American Economic Journal: Economic Policy*, 4: 190–215.
- Ginja, Rita, Julie Riise, Barton Willage, and Alexander Willén.** 2022. "Does Your Doctor Matter? Doctor Quality and Patient Outcomes." *SSRN Electronic Journal*.
- Gruber, Jonathan, Thomas P. Hoe, and George Stoye.** 2018. "Saving Lives by Tying Hands: The Unexpected Effects of Constraining Health Care Providers."
- Harris, Matthew C., Yinan Liu, and Ian McCarthy.** 2020. "Capacity constraints and time allocation in public health clinics." *Health Economics*, 29: 324–336.
- Hoe, Thomas P.** 2022. "Does Hospital Crowding Matter? Evidence from Trauma and Orthopedics in England." *American Economic Journal: Economic Policy*, 14: 231–262.
- Hsia, Renee Y., and Yu-Chu Shen.** 2019. "Emergency Department Closures And Openings: Spillover Effects On Patient Outcomes In Bystander Hospitals." *Health Affairs*, 38: 1496–1504.
- Iversen, Tor, and Hilde Lurås.** 2011. "Patient switching in general practice." *Journal of health economics*, 30: 894–903.
- Jena, Anupam B., N. Clay Mann, Leia N. Wedlund, and Andrew Olenski.** 2017. "Delays in Emergency Care and Mortality during Major U.S. Marathons." *New England Journal of Medicine*, 376: 1441–1450.
- Kiil, Astrid, and Kurt Houlberg.** 2013. "How does copayment for health care services affect demand, health and redistribution? A systematic review of the empirical evidence from 1990 to 2011." *The European Journal of Health Economics*, 15: 813–828.
- Kjøllesdal, Marte, Thor Indseth, Hilde Hestad Iversen, and Oyvind Bjertnaes.** 2020. "Patient experiences with general practice in Norway: a comparison of immigrant groups and the majority population following a national survey." *BMC Health Services Research*, 20.
- Kovacs, Roxanne, and Mylene Lagarde.** 2022. "Does high workload reduce the quality of healthcare? Evidence from rural Senegal." *Journal of Health Economics*, 82: 102600.
- Kristiansen, Ida Lykke, and Sophie Yanying Sheng.** 2022. "Doctor Who? The Effect of Physician Patient Match on the SES-Health Gradient." *Working Paper*.
- Kwok, Jennifer.** 2018. "How Do Primary Care Physicians Influence Healthcare? Evidence on Practice Styles and Switching Costs from Medicare."
- Legeforening, Den Norske.** 2022. "Normaltariffen."
- Lovdata.** 2017. "Forskrift om fastlegeordning i kommunene."
- Macinko, James, Barbara Starfield, and Leiyu Shi.** 2007. "Quantifying the health benefits of primary care physician supply in the United States." *International journal of health services : planning, administration, evaluation*, 37: 111–26.
- Norway, Statistics.** 2023. "GPs and emergency primary health care."
- of Health, Norwegian Ministry, and Welfare.** 2023. "Time for action — The personnel in a sustainable health and care service."
- Page, Anthea, Sarah Ambrose, John Glover, and Diana Hetzel.** 2007. "Atlas of avoidable hospitalisations in Australia: ambulatory care-sensitive conditions, Summary."
- Rosta, Judith, Olaf G Aasland, and Magne Nylenna.** 2019. "Changes in job satisfaction among doctors in Norway from 2010 to 2017: a study based on repeated surveys." *BMJ Open*, 9: e027891.
- Sabety, Adrienne.** 2023. "The value of relationships in healthcare." *Journal of Public Economics*, 225: 104927.

- Shurtz, Ity, Alon Eizenberg, Adi Alkalay, and Amnon Lahad.** 2018. “Physician Workload and Treatment Choice: The Case of Primary Care.”
- Simonsen, Marianne, Lars Skipper, Niels Skipper, and Peter Rønø Thingholm.** 2021. “Discontinuity in care: Practice closures among primary care providers and patient health care utilization.” *Journal of Health Economics*, 80: 102551.
- Svedahl, Ellen Rabben, Kristine Pape, Marlen Toch-Marquardt, Lena Janita Skarshaug, Silje-Lill Kaspersen, Johan Håkon Bjørngaard, and Bjarne Austad.** 2019. “Increasing workload in Norwegian general practice – a qualitative study.” *BMC Family Practice*, 20.
- Tikkanen, Roosa, and Melinda Abrams.** 2020. “U.S. Health Care from a Global Perspective, 2019: Higher Spending, Worse Outcomes?”
- Vallejo-Torres, Laura, and Stephen Morris.** 2018. “Primary care supply and quality of care in England.” *The European Journal of Health Economics*, 19: 499–519.
- Zhang, Xuan.** 2022. “The effects of physician retirement on patient outcomes: Anticipation and disruption.” *Journal of Public Economics*, 207: 104603.

A Appendix

A.1 Data Sources

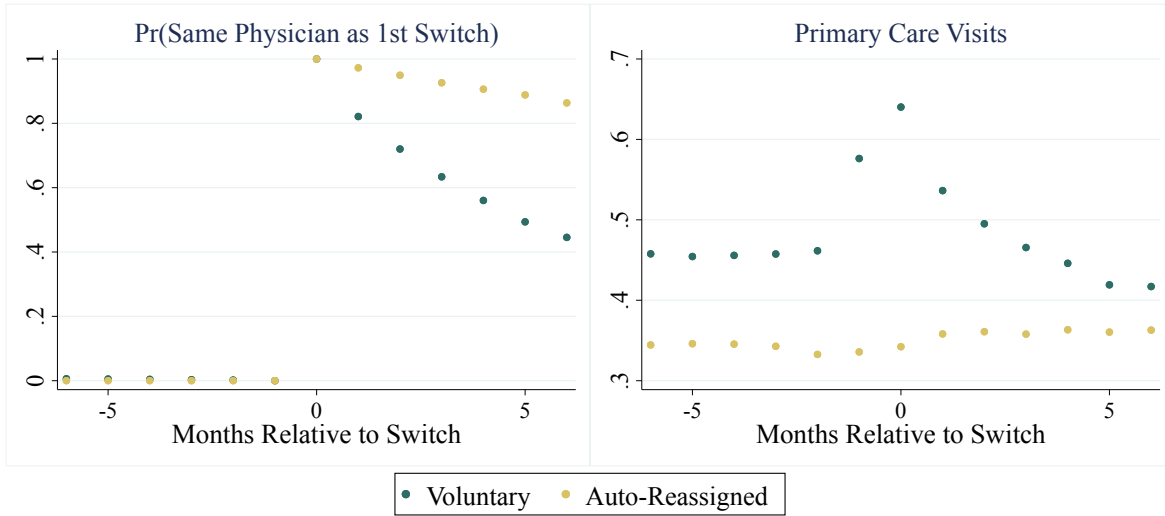
I use several data sources to construct the estimation sample. The Norwegian Control and Payment of Health Reimbursements Database (KUHR) tracks reimbursement for outpatient claims organized at the level of bill line, i.e., reimbursement code, and identifies most patients and physicians. The Norwegian Patient Registry (NPR) is a database of reimbursement for inpatient claims organized at the level of encounter. I use ICD-10 and ICPC-2 codes from both sources to classify chronic illness. I identify avoidable hospitalizations following Table A1 from Page et al. (2007). Capitation payments come from a basic subsidy rate dataset. Various datasets from the Norwegian GP Registry identify spells when patients are registered to patient lists and when physicians are contracted to provide care to those patient lists. The physician-list dataset also identifies contract details: the maximum number of registered patients and indicators for shared lists and fixed-salary reimbursement. I use anonymous identifiers for physicians, lists, and patients to link datasets and convert spells into monthly panels. Patients’ birth date, gender, disability payment receipt, and income come from tax records.

A.2 Additional Tables and Figures

Figure A.1, Panel A shows that in every month after auto-reassignment, some auto-reassigned patients switch again to a third physician, but on average, 83 percent remain for at least six months, so auto-reassignment represents a persistent change in enrollment. For comparison, voluntary switchers are

more likely to remain with their new physician, consistent with stronger preferences.¹³ Panel B shows that auto-reassigned patients' average treatment intensity is higher after switching physicians, consistent with Kwok (2018), and potentially crowding out incumbents. Compared to auto-reassigned switchers, voluntary switchers have persistently higher treatment intensity, including a sudden large increase in the month before switching. Voluntary switchers' treatment intensity declines after switching but remains much higher than before, and the change in treatment intensity is larger than the change among auto-reassigned switchers. The different trajectories of voluntary and auto-reassigned switchers suggest that endogeneity from patient sorting is important in this context. Even if a health shock prompts voluntary switchers to find new physicians rather than wait to be auto-reassigned, they choose physicians that they will remain with for longer and visit more often for at least six months. In the United States, patients of exiting physicians must all choose a new physician and the choice of physician likely incorporates unobserved determinants of treatment intensity like match quality even if the timing is exogenous. As examples of match quality, Kristiansen and Sheng (2022) show that low-SES patients assigned to a physician with a low-SES background have lower mortality, and Dahlstrand (2022) shows that matching high-risk patients to high-quality physicians lowers avoidable hospitalizations.

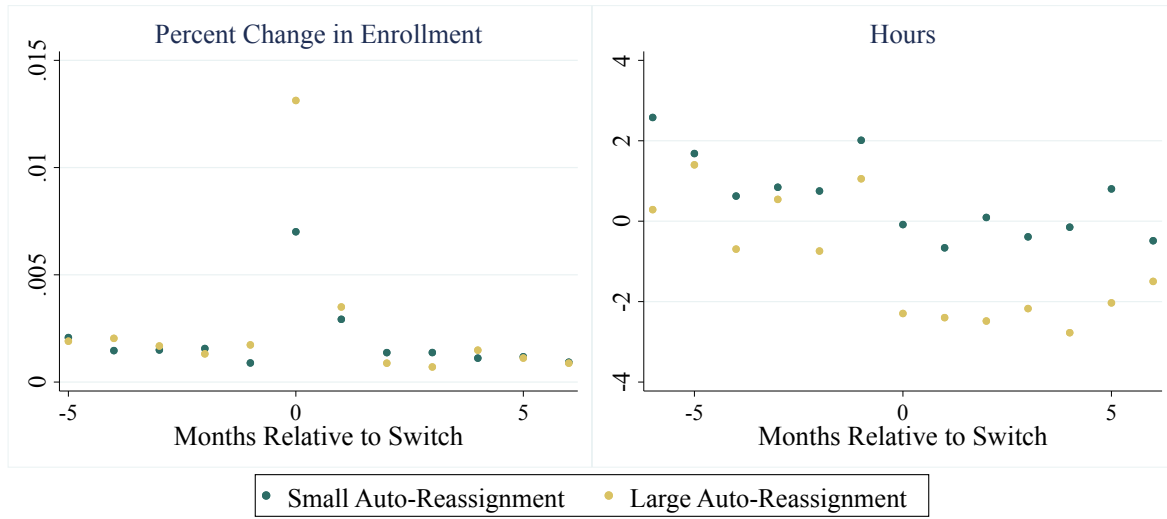
Figure A.1: Patients of Exiting Physicians: Voluntary Switches vs. Auto-Reassigned



Notes: Both panels reflect raw means among patient-months within 13-month spells, based on switching patients of exiting physicians whose spells are not necessarily balanced. For auto-reassigned patients, Month 0 is the first month after they are administratively reassigned to an incumbent's physician. For voluntary switching patients, Month 0 is the first month with a new physician, up to 5 months before their previous physician exits. The left panel shows the share of patients registered to their first physician after the previous one exited. The right panel shows average days per month with a physician encounter.

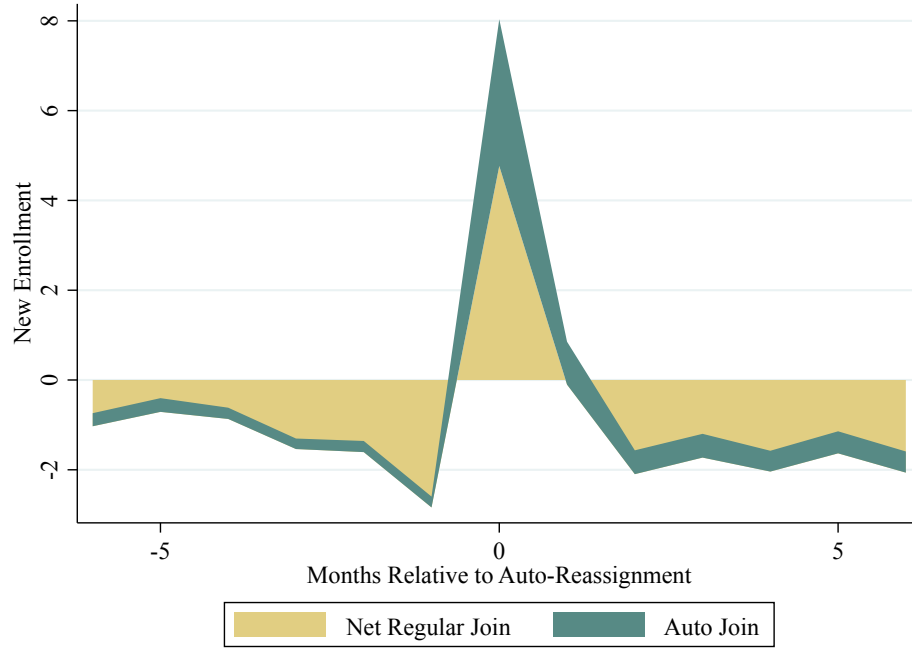
¹³Voluntary switchers do not directly contribute to the identifying variation of this paper. However, they might choose the same physicians to which others are auto-reassigned.

Figure A.2: Trends in Outcomes among Incumbent Patients



Notes: Both panels reflect means among physician-months within 13-month spells. Month 0 is the first month with auto-reassigned patients. Panel (a) shows the average percent change in the total number of registered patients. Panel (b) shows seasonally adjusted physician hours among incumbent patients, the residual from a regression on a time trend and calendar month fixed effects. Large Auto-Reassignment refers to spells with more than one patient auto-reassigned in month 0 (the mean is 5.5).

Figure A.3: Average Net Change to Enrollment



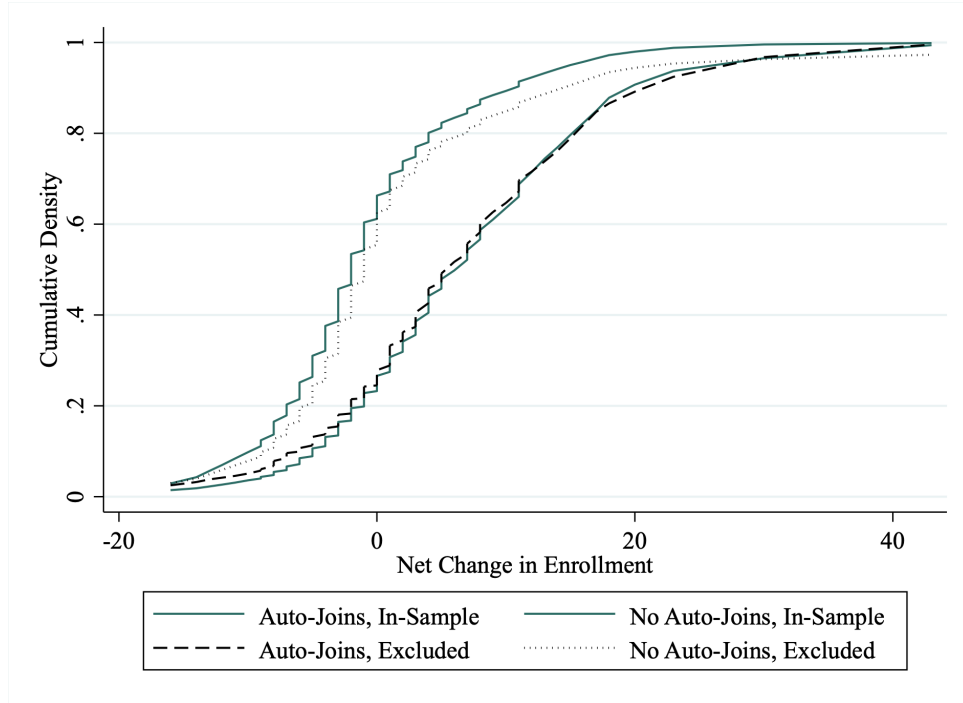
Notes: This figure decomposes the average one-month net change in enrollment between net regular joins and auto-joins. “Net Regular Join” is the average of the difference between patients that voluntarily leave and patients that voluntarily join. “Auto Join” is the average new registrations of patients that are administratively reassigned from other physicians. I average over physicians in the analysis sample in each month relative to the first auto-reassignment. Auto-reassignment in event-months -6 through -1 are from non-exiting physicians.

Table A.1: Sample Selection

Sample Restriction	Physicians	Physician-Months
Full Sample	8,198	538,362
Received auto-reassignments and nearby months	6,190	126,933
Balanced 13-month spells with identifier	3,723	89,732
No change to primary physician	3,693	89,115
≥ 500 incumbent patients	3,452	83,343
No shared list or temporary physician	3,189	77,207
No downsizing	2,362	45,968
No change to reimbursement rate	2,156	40,495
First balanced spell if overlapping	2,065	35,386
Final Sample	2,065	35,386

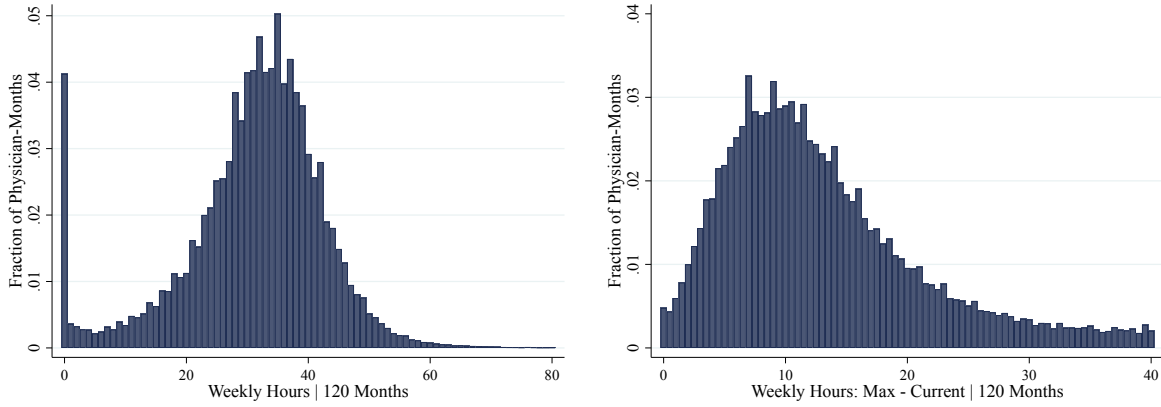
Notes: This table shows how the number of observation decreases with each of the sample conditions described in Section 2.2. Conditions are applied in the order shown.

Figure A.4: Cumulative Distribution of Enrollment Changes



Notes: This figure shows the cumulative distribution of one-month changes in the number of patients for four subsamples of physicians. “Auto-Joins, In-Sample” includes months with any auto-reassignment to a physician in the analysis sample. “No Auto-Joins, In Sample” reflects other months in the analysis sample. Likewise, out-of-sample physician-months are split between the remaining two categories based on whether an auto-reassigned patient registered with the physician. Out-of-sample indicates with an out-of-sample physician or an out-of-sample month for an in-sample physician.

Figure A.5: Capacity Constraints and Bunching of Workload



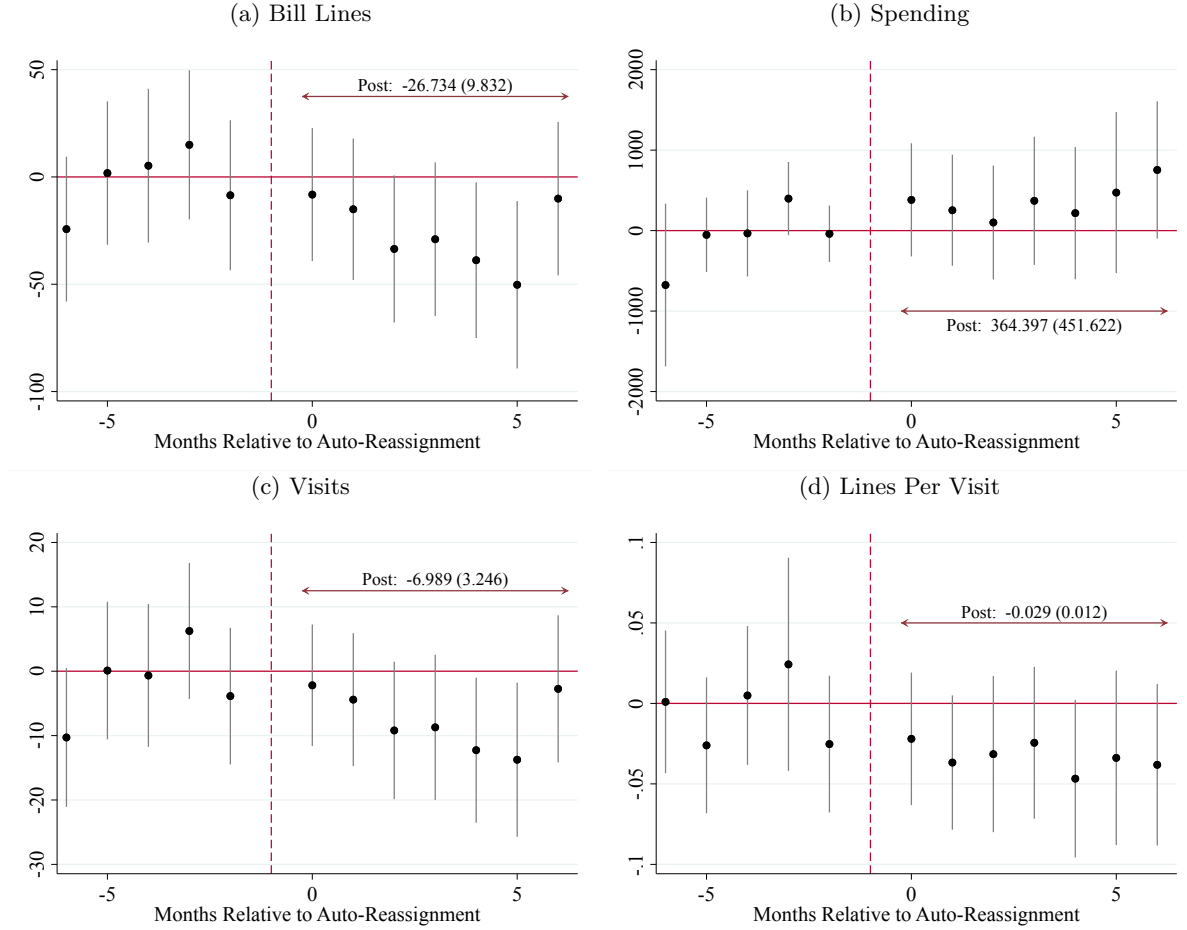
Notes: Both figures reflect weekly hours for physicians observed for the full sample period, 2008–2017. Weekly hours equal hours per month divided by weeks per month. The left figure shows the untransformed distribution of weekly hours, truncated at 80 hours. The right figure shows the distribution of transformed weekly hours (\bar{M}_{jt}). The transformation is $\max_t M_{jt} - M_{jt}$. The x-axis is truncated at 40 and I exclude the first month when a physician works the maximum number of hours.

Table A.2: Test of Conditionally Random Assignment

	(1)	(2)
Availablity	0.028 (0.007) [0.000]	0.027 (0.007) [0.000]
Same Municipality	10.542 (1.283) [0.000]	10.562 (1.288) [0.000]
Share Female		1.387 (1.138) [0.223]
Share Chronic		0.398 (1.097) [0.717]
Share Age 65+		-0.324 (0.761) [0.671]
# Observations	10,777	10,777

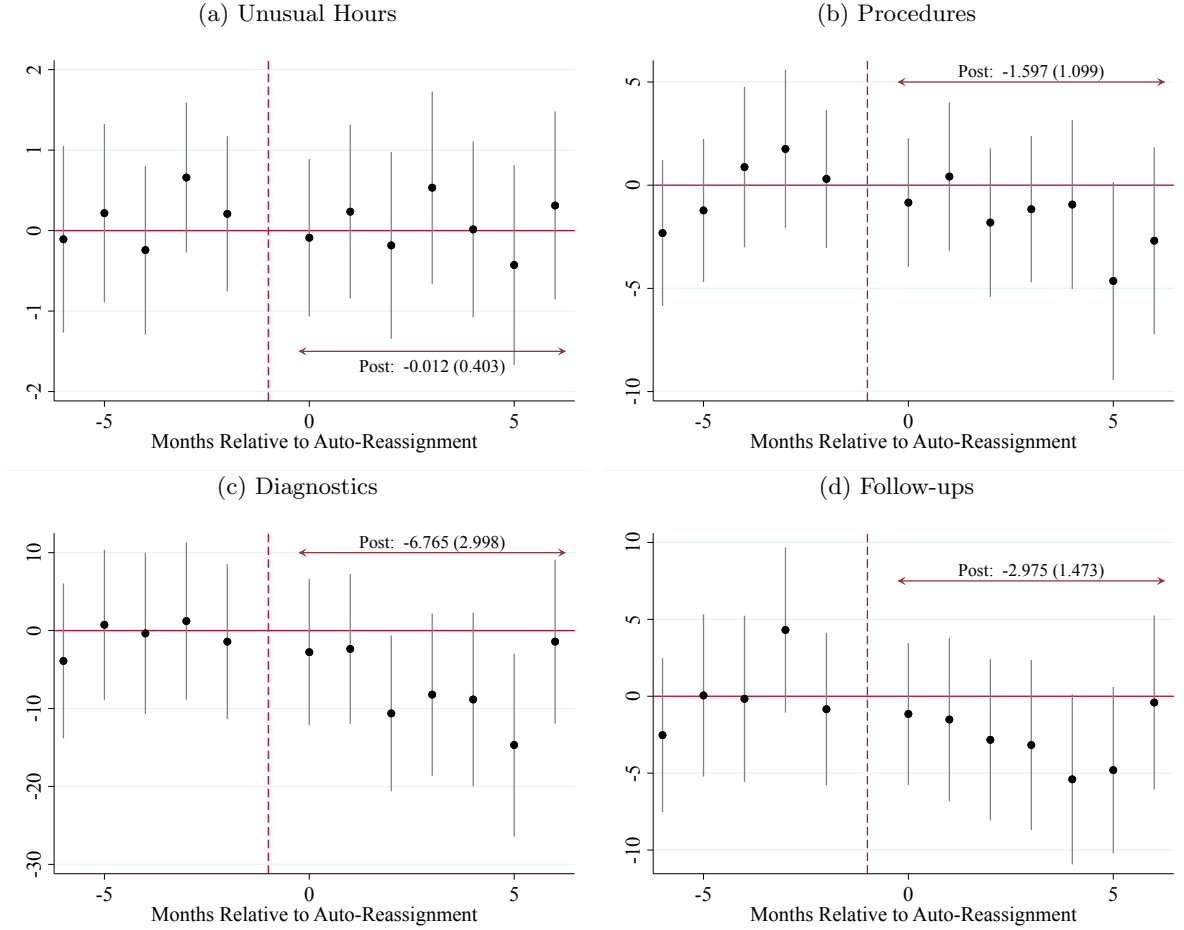
Notes: This table shows regressions of the number of auto-reassignments on the availability (maximum enrollment - lagged enrollment), an indicator for the physician residing in the same municipality as the exiting physician, fixed effects for the combination of the exiting physician and month of exit (not shown), and in column (2), average characteristics of incumbent patients. The unit of observation is a physician-month for physicians receiving auto-reassignments.

Figure A.6: Treatment Measures Among Incumbent Patients: Large versus Small Auto-Reassignments



Notes: This figure shows estimates and 95% confidence intervals of the difference between physicians with large and small auto-reassignments in each month relative to auto-reassignment. The omitted month is -1. I define large auto-reassignments as those with two or more reassigned patients (969 of 2,722 physician-spells). Each panel represents a separate regression with the dependent variable as indicated. The physician-month sample reflects aggregate treatment intensity among a fixed set of incumbent patients. Standard errors are clustered at the physician level. Estimates refer to β_{1t} in the following regression: $Y_{jt} = \beta_{0t} + \beta_{1t} \text{AutoHigh}_j + \beta_j + \epsilon_{jt}$. “Post” indicates the average difference in outcomes after auto-reassignment between physicians with large and small auto-reassignments. Standard errors are in parentheses, and the coefficient is β_1 in the following regression: $Y_{jt} = \beta_{0t} + \beta_1 \text{AutoHigh}_j \times 1[t \geq 0] + \beta_j + \epsilon_{jt}$.

Figure A.7: Treatment Types Among Incumbent Patients: Large versus Small Auto-Reassignments



Notes: This figure shows estimates and 95% confidence intervals of the difference between physicians with large and small auto-reassignments in each month relative to auto-reassignment. The omitted month is -1. I define large auto-reassignments as those with two or more reassigned patients (969 of 2,722 physician-spells). Each panel represents a separate regression with the dependent variable as indicated. The physician-month sample reflects aggregate treatment intensity among a fixed set of incumbent patients. Standard errors are clustered at the physician level. Estimates refer to β_{1t} in the following regression: $Y_{jt} = \beta_{0t} + \beta_{1t}\text{AutoHigh}_j + \beta_j + \epsilon_{jt}$. “Post” indicates the average difference in outcomes after auto-reassignment between physicians with large and small auto-reassignments. Standard errors are in parentheses, and the coefficient is β_1 in the following regression: $Y_{jt} = \beta_{0t} + \beta_1\text{AutoHigh}_j \times 1[t \geq 0] + \beta_j + \epsilon_{jt}$.

Table A.3: Effect of Enrollment Among All Registered Patients

	Enrollment (1)	Hours (2)	Bill Lines (3)	Spending (4)	Visits (5)
Cuml. Auto-Joins	1.083 (0.069) [<0.001]				
Enrollment		0.003 (0.052) [0.954]	0.136 (0.285) [0.634]	11.538 (8.667) [0.183]	0.391 (0.332) [0.239]
Dep. Var. Mean	1274.917	141.221	1547.787	13944.176	502.732
F-Statistic	112.037	9.348	7.759	7.073	8.724
Observations	35,386	33,403	33,403	33,403	33,403
	Unusual Hours (6)	Lines Per Visit (7)	Procedures (8)	Diagnostics (9)	Follow-ups (10)
Enrollment	-0.014 (0.006) [0.028]	-0.001 (0.001) [0.327]	-0.086 (0.050) [0.084]	0.107 (0.094) [0.257]	0.150 (0.080) [0.059]
Dep. Var. Mean	22.940	2.922	97.844	367.507	179.960
F-Statistic	3.245	2.924	3.341	5.559	40.478
Observations	33,403	33,403	33,403	33,403	33,403

Notes: This table displays estimates of coefficients from regressions of Equations 1 and 2. Each column represents a separate regression with the dependent variable as indicated in the table. The physician-month sample reflects aggregate treatment intensity among all currently registered patients. Standard errors clustered at the physician level are reported in parentheses, and p-values are reported in brackets. "Cuml. Auto-Joins" indicates the number of patients auto-reassigned to a physician since the start of the spell. Regressions also include fixed effects for spell and event-month.

Table A.4: Patients in Sample Experience Gap in Treatment Intensity

	Hours (1)	Avoidable Hosp. (2)
In-Sample	-0.006 (0.001) [<0.001]	0.017 (0.006) [0.002]
Dep. Var. Mean	0.122	0.345
F-Statistic	22.280	9.157
Observations	57,760,517	62,013,800

Notes: This table displays estimates of coefficients from regressions of the indicated dependent variable on an indicator for observations in-sample and fixed effects for each combination of month, 5-year age bin, gender, an indicator for recent registration, and indicators for primary and secondary chronic illnesses. The sample includes all patients in Norway for each month in 2015 and outcomes are patient-specific. Standard errors clustered at the physician level are reported in parentheses, and p-values are reported in brackets. Patient Hours are approximated by multiplying physician-level hours by the share of reimbursement. Avoidable Hospitalizations is scaled up by 100.

Table A.5: Heterogeneity in the Effect of Auto-Reassignment on Enrollment

	Reached Capacity		High Fee		Full-Time	
	(1)	(2)	(3)	(4)	(5)	(6)
Cumulative Auto-Joins	5.674 (0.789) [<0.001]	1.037 (0.039) [<0.001]	1.041 (0.048) [<0.001]	2.699 (0.498) [<0.001]	1.006 (0.036) [<0.001]	1.206 (0.142) [<0.001]
Subsample	X		X		X	
Dep. Var. Mean	1184.388	1356.589	1178.162	1343.490	1150.030	1341.655
F-Statistic	33.577	134.240	83.519	46.769	100.088	66.967
Observations	16,783	18,603	14,677	20,709	12,324	23,062
	Age 65+		Chronic Illness		Female	
	(7)	(8)	(9)	(10)	(11)	(12)
Cumulative Auto-Joins	1.083 (0.069) [<0.001]	1.083 (0.069) [<0.001]	1.083 (0.069) [<0.001]	1.083 (0.069) [<0.001]	1.083 (0.069) [<0.001]	1.083 (0.069) [<0.001]
Subsample	X		X		X	
Dep. Var. Mean	1274.917	1274.917	1274.917	1274.917	1274.917	1274.917
F-Statistic	112.037	112.037	112.037	112.037	112.037	112.037
Observations	35,386	35,386	35,386	35,386	35,386	35,386

Notes: This table displays estimates of coefficients from regressions of Equation 1. Each column represents a separate regression among a subsample with the dependent variable as indicated in the table. Columns (1-6) reflect aggregate treatment intensity among all incumbent patients for subsets of physicians. Columns (7-12) reflect aggregate treatment intensity among subsets of incumbent patients. For example, Column (1) includes physicians with enrollment consistently less than 99 percent of initial stated capacity, and Column (2) includes all other physicians. Standard errors clustered at the physician level are reported in parentheses, and p-values are reported in brackets. High-fee physicians receive supplementary reimbursement for each visit due to a certificate from additional training. Full-time physicians treat patients for at least six hours per weekday, on average, during the six months prior to auto-reassignment. Regressions also include fixed effects for spell and event-month.

Table A.6: Heterogeneity in the Effect of Enrollment on Bill Lines

	Reached Capacity		High Fee		Full-Time	
	(1)	(2)	(3)	(4)	(5)	(6)
Enrollment	−0.028 (0.272) [0.919]	−0.376 (0.053) [<0.001]	−0.326 (0.053) [<0.001]	−0.110 (0.454) [0.809]	−0.445 (0.030) [<0.001]	−0.233 (0.070) [<0.001]
Subsample	X		X		X	
Dep. Var. Mean	1032.164	1211.539	808.396	1351.888	792.144	1305.120
1 st Stage F-Stat.	33.577	134.240	83.519	46.769	100.088	66.967
Observations	16,783	18,603	14,677	20,709	12,324	23,062
	Age 65+		Chronic Illness		Female	
	(7)	(8)	(9)	(10)	(11)	(12)
Enrollment	−0.316 (0.031) [<0.001]	−0.027 (0.037) [0.455]	−0.265 (0.042) [<0.001]	−0.078 (0.020) [<0.001]	−0.093 (0.017) [<0.001]	−0.251 (0.042) [<0.001]
Subsample	X		X		X	
Dep. Var. Mean	783.404	343.060	604.972	521.492	481.626	644.839
1 st Stage F-Stat.	112.037	112.037	112.037	112.037	112.037	112.037
Observations	35,386	35,386	35,386	35,386	35,386	35,386

Notes: This table displays estimates of coefficients from regressions of Equation 2. Each column represents a separate regression among a subsample with the dependent variable as indicated in the table. Columns (1-6) reflect aggregate treatment intensity among all incumbent patients for subsets of physicians. Columns (7-12) reflect aggregate treatment intensity among subsets of incumbent patients. For example, Column (1) includes physicians with enrollment consistently less than 99 percent of initial stated capacity, and Column (2) includes all other physicians. Standard errors clustered at the physician level are reported in parentheses, and p-values are reported in brackets. High-fee physicians receive supplementary reimbursement for each visit due to a certificate from additional training. Full-time physicians treat patients for at least six hours per weekday, on average, during the six months prior to auto-reassignment. Regressions also include fixed effects for spell and event-month.

Table A.7: Heterogeneity in the Effect of Enrollment on Visits

	Reached Capacity		High Fee		Full-Time	
	(1)	(2)	(3)	(4)	(5)	(6)
Enrollment	0.059 (0.087) [0.496]	−0.003 (0.156) [0.983]	0.015 (0.157) [0.925]	0.074 (0.137) [0.591]	−0.191 (0.012) [<0.001]	0.259 (0.054) [<0.001]
Subsample	X		X		X	
Dep. Var. Mean	333.016	403.671	296.589	422.302	253.387	432.563
1 st Stage F-Stat.	33.577	134.240	83.519	46.769	100.088	66.967
Observations	16,783	18,603	14,677	20,709	12,324	23,062
	Age 65+		Chronic Illness		Female	
	(7)	(8)	(9)	(10)	(11)	(12)
Enrollment	0.008 (0.127) [0.947]	−0.003 (0.022) [0.876]	−0.002 (0.108) [0.986]	0.007 (0.040) [0.865]	0.012 (0.048) [0.802]	−0.007 (0.100) [0.945]
Subsample	X		X		X	
Dep. Var. Mean	261.099	109.062	212.772	157.388	155.915	214.246
1 st Stage F-Stat.	112.037	112.037	112.037	112.037	112.037	112.037
Observations	35,386	35,386	35,386	35,386	35,386	35,386

Notes: This table displays estimates of coefficients from regressions of Equation 2. Each column represents a separate regression among a subsample with the dependent variable as indicated in the table. Columns (1-6) reflect aggregate treatment intensity among all incumbent patients for subsets of physicians. Columns (7-12) reflect aggregate treatment intensity among subsets of incumbent patients. For example, Column (1) includes physicians with enrollment consistently less than 99 percent of initial stated capacity, and Column (2) includes all other physicians. Standard errors clustered at the physician level are reported in parentheses, and p-values are reported in brackets. High-fee physicians receive supplementary reimbursement for each visit due to a certificate from additional training. Full-time physicians treat patients for at least six hours per weekday, on average, during the six months prior to auto-reassignment. Regressions also include fixed effects for spell and event-month.

Table A.8: Heterogeneity in the Effect of Enrollment on Reimbursement

	Reached Capacity		High Fee		Full-Time	
	(1)	(2)	(3)	(4)	(5)	(6)
Enrollment	-0.839 (2.419) [0.729]	1.649 (4.526) [0.716]	0.968 (4.257) [0.820]	7.135 (11.029) [0.518]	-2.916 (1.978) [0.141]	6.976 (1.852) [<0.001]
Subsample	X		X		X	
Dep. Var. Mean	9242.519	11252.667	7232.838	12472.557	7517.379	11785.897
1 st Stage F-Stat.	33.577	134.240	83.519	46.769	100.088	66.967
Observations	16,783	18,603	14,677	20,709	12,324	23,062
	Age 65+		Chronic Illness		Female	
	(7)	(8)	(9)	(10)	(11)	(12)
Enrollment	1.094 (3.590) [0.761]	0.174 (0.640) [0.785]	0.617 (3.026) [0.839]	0.652 (1.141) [0.568]	0.707 (1.339) [0.597]	0.561 (2.829) [0.843]
Subsample	X		X		X	
Dep. Var. Mean	7349.652	2949.635	5782.335	4516.951	4466.973	5832.313
1 st Stage F-Stat.	112.037	112.037	112.037	112.037	112.037	112.037
Observations	35,386	35,386	35,386	35,386	35,386	35,386

Notes: This table displays estimates of coefficients from regressions of Equation 2. Each column represents a separate regression among a subsample with the dependent variable as indicated in the table. Columns (1-6) reflect aggregate treatment intensity among all incumbent patients for subsets of physicians. Columns (7-12) reflect aggregate treatment intensity among subsets of incumbent patients. For example, Column (1) includes physicians with enrollment consistently less than 99 percent of initial stated capacity, and Column (2) includes all other physicians. Standard errors clustered at the physician level are reported in parentheses, and p-values are reported in brackets. High-fee physicians receive supplementary reimbursement for each visit due to a certificate from additional training. Full-time physicians treat patients for at least six hours per weekday, on average, during the six months prior to auto-reassignment. Regressions also include fixed effects for spell and event-month.