Do New Patients Displace Existing Patients' Treatment?*

Jori Barash†

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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. In turn, patients experience 0.001 additional avoidable hospitalizations, consistent with crowd-out exacerbating 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, which would imply 1534 fewer avoidable hospitalizations.

JEL classification: I11, I14, J45, H51, I18
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†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 might 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).[2]

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. Although this crowd-out effect is economically small, enrollment also increases avoidable hospitalizations. 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 impact of a counterfactual registration policy 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

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

[2] Likewise, primary care physicians per capita is a strong correlate of the causal effects of commuting zone on life expectancy, estimated using patient moves (Finkelstein, Gentzkow and Williams, 2021).

increase in enrollment from auto-reassigned patients is compounded by the fact that other patients in 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 needs 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. Crowd-out is consistently small among particular types of treatment, alternative specifications, and subsets of patients and physicians. 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. Finally, I find a precise increase in incumbents' avoidable hospitalizations, suggesting that crowd-out in treatment intensity harms 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 financial 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.[3]

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 of 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 only harms incumbent patients if the resulting treatment intensity is inefficiently low. In absence of data on the efficient level of treatment, I show that crowd-out increases avoidable hospitalizations. I also 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 (both 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.

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

To the best of my knowledge, this paper is the first to estimate the effect of the number of registered 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 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; Chan, 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. Unlike emergency physicians, primary care physicians do not choose their patients, they are responsible for those patients for long periods of time, they do choose their working hours, and they have financial incentives on the intensive margin of care.

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, Jena and Barnett, 2021; Sabety, 2023).[4] Physician workload shocks disrupt care differently than when patients switch physicians. For example, 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, hetero-

[4]This paper extends a related analysis in Sabety (2023), Table A19. First, I decompose imprecise clinic-level effects to show crowd-out among incumbents. Second, I use quasi-random variation in enrollment. After physician exit, patients' health and remaining physicians' quality might influence the choice to switch establishments.

geneity 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.

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.[5]

The identifying variation in this paper comes from administrative assignment, which occurs when an 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.[6] 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.5 shows the distribution of auto-reassignments and Appendix Figure A.4 illustrates the corresponding distribution of enrollment increases. 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.[7] These records contain nearly all of Norway's residents and primary care physicians from 2008 to 2017.

The estimation sample excludes potentially confounding variation. Appendix Table A.1 shows the number of physicians impacted by each sample condition. Each physician must practice in a single location during the entire period and each patient must be registered for the entire period. I also

[5] On average, 80 percent of primary care visits are with the registered physician.

[6] Typically, 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. Table 4 evaluates robustness by excluding these cases.

[7] See Appendix A.1 for additional details.

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 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.

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	372.52	210.86	51.00	368.00	641.00
Spending	10,858.77	27,152.92	1,056.89	10,111.69	17,832.71
Hours	103.25	42.84	45.29	107.94	153.00
Avoidable Hosp.	3.61	2.93	1.00	3.00	7.00
Follow-up Visits	110.29	74.31	21.00	102.00	204.00
Bill Lines	1,140.84	700.84	322.00	1,060.00	2,019.00
Diagnostics	72.59	88.72	8.00	51.50	150.00
Procedures	274.05	215.50	57.00	228.00	541.00
per Visit	2.94	0.84	2.04	2.81	4.01
Physician-Spells	2,722				

Notes: Summary statistics reflect monthly totals six months prior to auto-reassignment, across physicians. 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. I classify Avoidable Hospitalizations based on diagnosis codes (see Appendix A.1.

The final analysis sample reflects 2,065 unique physicians and 2,335,982 patients, representing nearly one-third of the population. 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. 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 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.[8]

Consider a physician who simultaneously chooses treatment intensity m_i for each registered patient $i \in 1, ..., N$, 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,

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

[8]Perhaps 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.

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

$$u'\left(\sum_i \pi(m_i)\right)\pi'(m_{i'}) - l'\left(\sum_i m_i\right) + \alpha h_m(m_{i'}, \lambda) + \mu = 0$$

$$\sum_i m_i \leq \bar{M}\,,$$

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

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 ($\frac{d^2h}{dmd\lambda} > 0$), 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:

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

$$\text{Enroll}_{jt} = \gamma_1 \text{Auto}_{jt} + \gamma_j + \gamma_t + \varepsilon_{jt} \,. \tag{2}$$

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 ($t = -6, -5, -4, ..., 4, 5, 6$). 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.[9] 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 physicians.[10] 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.

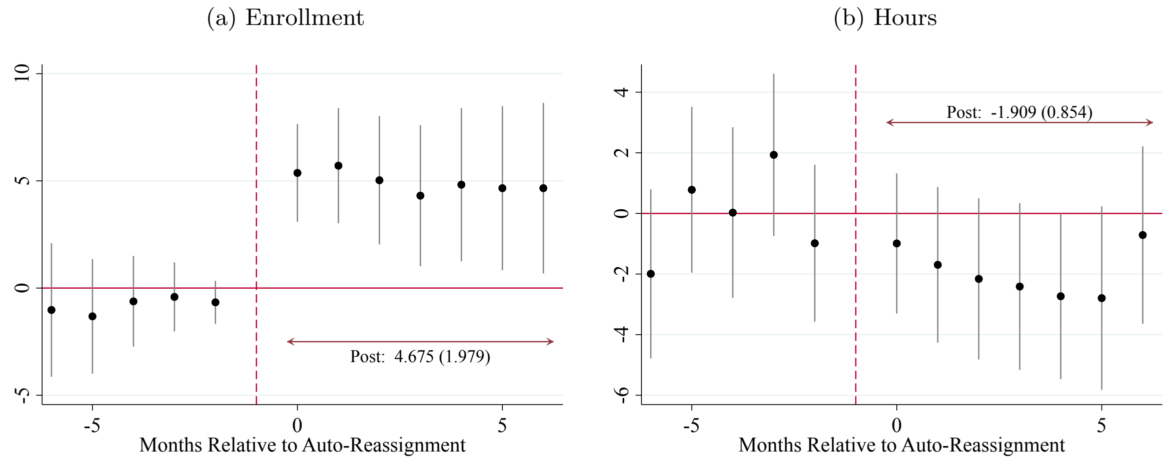
Ginja et al. (2022) report that administrative assignment is conditionally random and show that assigned physicians' value-added does not correlate with reassigned patient characteristics. In particular, auto-reassignment should be random conditional on availability and municipality, which are absorbed 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

[9] 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.

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

physicians.

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 correspond 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: $Y_{jt} = \beta_{0t} + \beta_1\text{AutoHigh}_j \times 1[t \geq 0] + \beta_j + \epsilon_{jt}$. See Appendix Figures A.8 and A.9 show event-study estimates for other outcomes.

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. Section 4.3 discusses the second-stage effect of enrollment on hours implied by event-study estimates. Appendix Figures A.8 and A.9 show event-study estimates for other outcomes and Appendix Figure A.2 shows the trends in mean outcomes.

Relevance and Monotonicity. Auto-reassignments must strictly increase enrollment. 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, in Column (1) of Table 2 shows that the first stage coefficient is precise and indistinguishable from 1. The coefficient could have been larger than 1 because auto-reassignment coincides with physician exit. As shown in Appendix Figure A.3, for remaining nearby physicians with availability, enrollment increases due to both auto-reassigned patients and voluntarily switching patients. Voluntarily switching patients have to choose among fewer physicians than before the physician exit.

Appendix Figure A.6 shows that the change in enrollment is approximately monotonic in the number of auto-reassigned patients. Likewise, 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 for physicians with large auto-reassignments relative to physicians with a single 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. The average incumbent loses just 0.14 seconds of treatment per month.[11] 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. Without a proportional decrease in spending, enrollment affects the qualitative composition of treatment, e.g., replacing relatively low-reimbursement treatment with high-reimbursement alternatives.[12] Columns (6)-(9) show that enrollment decreases intensity for several subsets of treatment and these effects are of similar size relative to the mean: physicians perform fewer services per visit, and crowd-out is similar among high-value services like procedures and diagnostics.

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.[13] 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. Although enrollment does not precisely affect physicians' total labor supply, point estimates are generally positive. Likewise, the effects on incumbents' treatment intensity in Table 2 represent 20-38

[11] I divide the effect size by the average of 1262 incumbent patients per physician.

[12] To see this, suppose there are two services with fixed reimbursement rates: m_L reimbursed at p_L and m_H reimbursed at $p_H > p_L$. If Spending $p_L m_L + p_H m_H$ is unchanged while Bill Lines $m_1 + m_2$ decreases, then either m_H or m_L (but not both) must increase. The physician has a greater incentive for m_H to increase.

[13] The sample includes incumbent patients, auto-reassigned patients, and other currently registered patients who do not have identification numbers or switch physicians during the 13-month spell.

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	−0.345	2.230	0.004
		(0.026)	(0.055)	(4.597)	(0.152)
		[0.070]	[<0.001]	[0.628]	[0.980]
Dep. Var. Mean	1274.917	101.349	1126.464	10299.286	370.160
New Pat. Mean		0.124	1.313	131.205	0.400
F-Statistic	117.129	20.101	30.679	9.458	28.624
Observations	35,386	35,386	35,386	35,386	35,386
	Lines Per Visit (6)	Procedures (7)	Diagnostics (8)	Follow-ups (9)	Avoidable Hosp. (10)
Enrollment	−0.002	−0.038	−0.058	−0.023	0.001
	(0.000)	(0.019)	(0.015)	(0.021)	(0.000)
	[<0.001]	[0.045]	[<0.001]	[0.268]	[0.024]
Dep. Var. Mean	2.940	70.908	270.428	135.084	3.619
New Pat. Mean	3.244	0.096	0.293	0.165	0.004
F-Statistic	36.423	46.320	43.653	26.550	6.869
Observations	34,578	35,386	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. New Pat. Mean is the average of the dependent variable among auto-reassigned patients, averaged across physicians in the six months after auto-reassignment. Regressions also include fixed effects for spell and event-month.

percent of average treatment intensity among auto-reassigned patients.[14]

Consistent with crowd-out in utilization being harmful to patients, Column (10) of Table 2 shows that enrollment increases incumbents' avoidable hospitalizations. The estimate is precise but small: enrollment would need to increase by 82 patients for any one patient to experience an additional avoidable hospitalization in the following year.[15] New patients have relatively high avoidable hospitalizations, and accordingly, Appendix Table A.3 shows a larger point estimate from a regression using all registered patients. Prior work (e.g., Sabety, 2023) suggests that new patients may be harmed by switching physicians.

[14] This range excludes imprecise point estimates and Lines Per Visit.

[15] The calculation is $\frac{1}{1.017 \times 10^{-3}} \times \frac{1}{12} = 82$.

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. Differences in point estimates across subsamples are at best suggestive because the lack of precision.

Table 3: Heterogeneity in the Effect of Enrollment on Hours

	Capacity		Fee Level		Schedule	
	Slack (1)	Binds (2)	Low (3)	High (4)	Part-Time (5)	Full-Time (6)
Enrollment	−0.025	−0.051	−0.045	−0.039	−0.083	−0.005
	(0.023)	(0.027)	(0.027)	(0.030)	(0.004)	(0.010)
	[0.281]	[0.057]	[0.095]	[0.197]	[<0.001]	[0.609]
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		Diagnoses		Gender	
	Under 65 (7)	Over 65 (8)	Healthy (9)	Chronic (10)	Male (11)	Female (12)
Enrollment	−0.042	−0.007	−0.037	−0.012	−0.013	−0.036
	(0.020)	(0.006)	(0.020)	(0.006)	(0.007)	(0.019)
	[0.035]	[0.248]	[0.061]	[0.045]	[0.068]	[0.053]
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.

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.

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 capacity constraints dominate. All else equal, with income effects, part-time physicians should have a lower marginal taste for 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.7 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 column, 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 relatively large among patients who are young, not chronically ill, and female. To the extent that such characteristics proxy for low severity, this heterogeneity is consistent with marginal crowd-out being less harmful for 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 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)	[0.109]	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	61.752	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)	Constant Ceiling	−0.077	(0.057)	[0.178]	102.791	72.963	29,328
(8)	Avoidable Hosp.	−0.081	(0.005)	[<0.001]	103.043	88.793	32,097
(9)	Alt. 1st Stage	−0.050	(0.026)	[0.053]	101.349	3509.565	35,386
(10)	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 during which the contracted upper bound on enrollments is non-increasing. (8) subsets to spells that begin with at least one avoidable hospitalization. (9) 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. (10) 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. For example, the potential salience of large auto-reassignments does not appear to produce a nonlinear relationship between auto-reassignments and treatment intensity. 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 exclude physicians who increase their contracted upper bound on enrollment during the spell. One might be concerned that auto-reassignment is not quasi-random among these physicians. Eighth, I subset to physicians that have an avoidable hospitalization six months before auto-reassignment. Intuitively, incumbent crowd-out should be limited for patients with a salient health shock. Instead, the point estimate is larger but still small. Ninth, I use an alternative specification for the first stage which interacts total auto-reassignments with the number of months relative to auto-reassignment. Tenth, I weight observations by the number of patients because underlying treatment decisions are at the patient level and effects might correlate with baseline enrollment.

The event study design illustrated in Figures 1, A.8, and A.9 is also consistent with the main results: an increase in enrollment leads to a small decrease in treatment intensity. For example, dividing point estimates, 1 additional patient leads to $\frac{1.909}{4.675} = 0.408$ (standard error = 0.239) fewer hours spent with incumbent patients. This point estimate is equivalent to less than 2 seconds per month per patient. The 0.408 estimate is larger than, but not statistically distinguishable from, the baseline estimate of -0.048 (0.026) in Table 2.

This paper focuses on the baseline instrumental variable design rather than the event study because the former equally weights all auto-reassignment events to produce a policy-relevant treatment effect. The event study design limits identifying variation to an arbitrary margin like 1 versus 2+ auto-reassignments. A related caveat of the event study is that the comparison group is still treated. Physicians with a single auto-reassigned patient experience a relatively small shock, but enrollment still grows on average. Alternative comparison groups also have issues. Physicians without availability are not eligible to receive auto-reassignments, and full patient lists might suggest that these physicians differ systematically. Other physicians might be affected even without auto-reassignments because all patients have fewer choices once a physician exits.

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 need less 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). 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 targeted assignment.[16] I show that targeted assignment can lower crowd-out by 86 percent.

First, I estimate crowd-out effects for four subsamples including combinations of 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.[17] 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.[18] With targeting,

[16]Changes 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.

[17]I 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.

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

predicted crowd-out over ten years drops from 83,877 to 11,483 hours. This corresponds to 1534 fewer avoidable hospitalizations if I apply the point estimates from Table 2.[19]

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 base payments may be justified.[20] To minimize crowd-out, policymakers might consider requiring subsidy recipients to schedule full-time 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 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.

[19] The calculation is $(83,877 - 11,483) \div 0.048 \times 0.001017$.

[20] Norway has an equalization grant for physicians in municipalities with fewer than 5000 residents.

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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). Base 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.

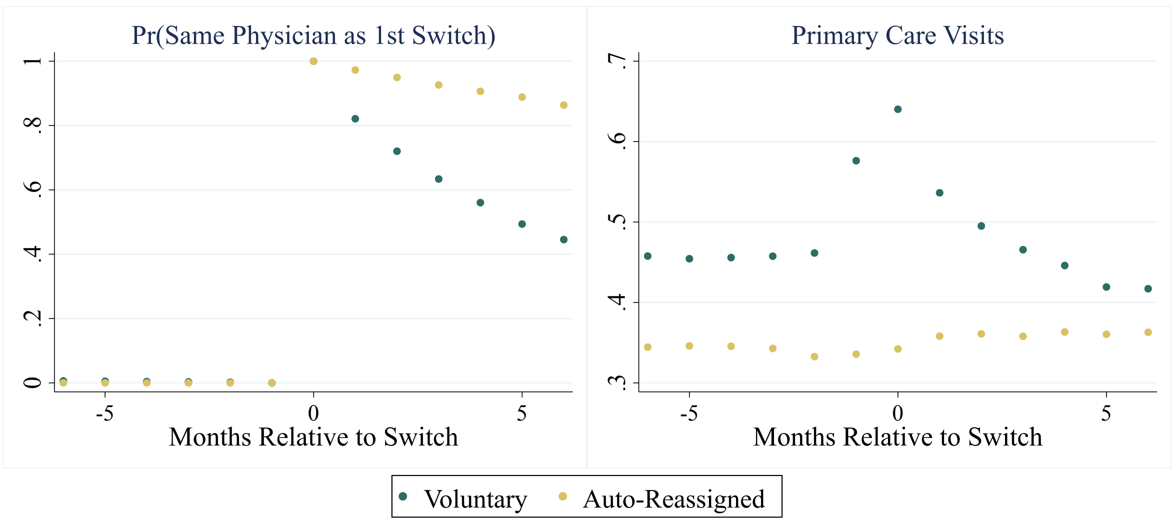
A.2 Additional Figures and Tables

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.[21] 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

[21] Voluntary 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.

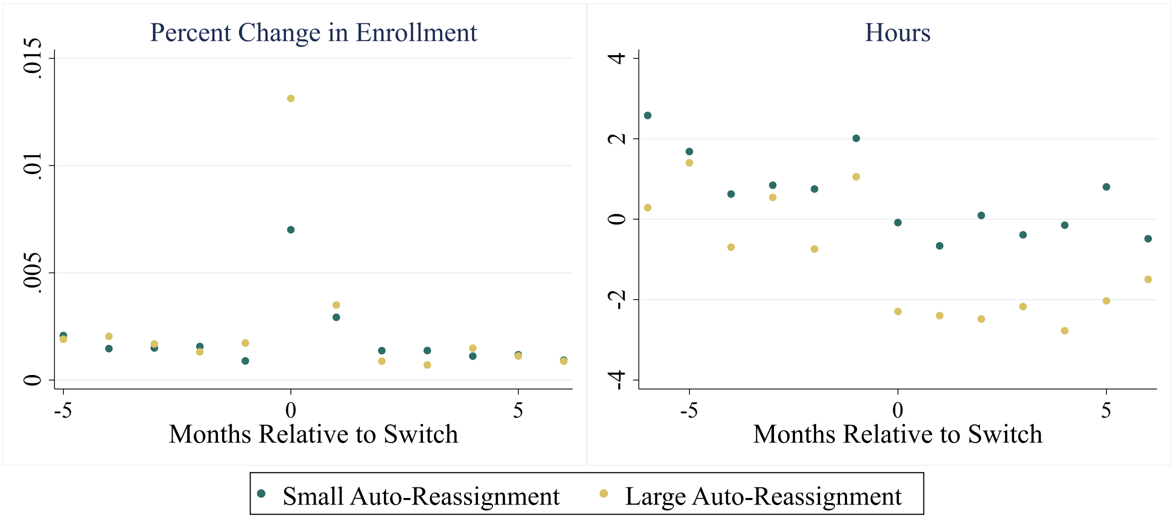
will remain with for longer and visit more often for at least six months. In the United States, patients of exiting physicians must all choose a new physician and the choice of physician likely incorporates unobserved determinants of treatment intensity like match quality even if the timing is exogenous. 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



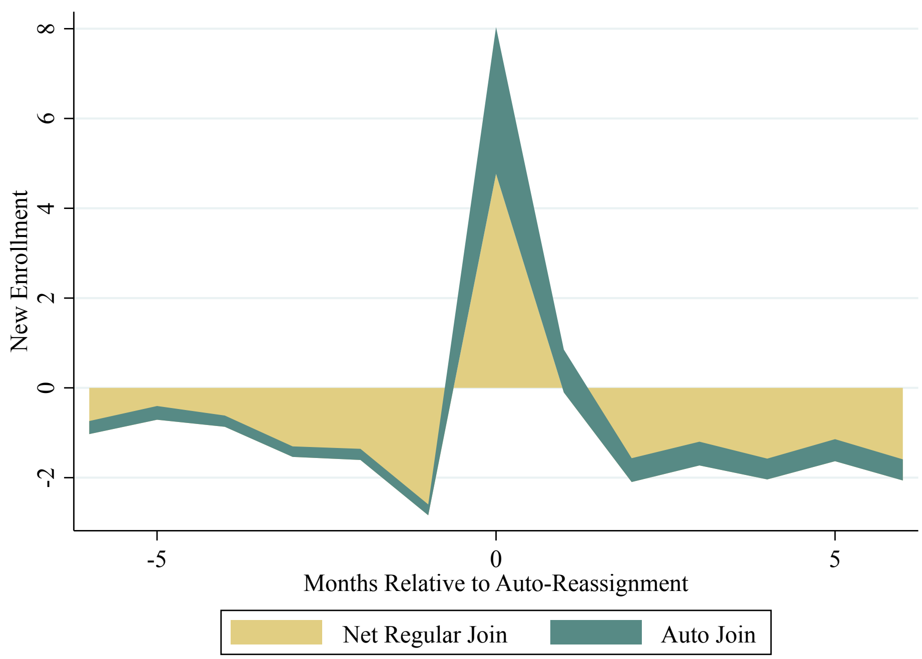
Notes: Both panels reflect raw means among patient-months within 13-month spells, based on switching patients of exiting physicians whose spells are not necessarily balanced. For auto-reassigned patients, Month 0 is the first month after they are administratively reassigned to an incumbent's physician. 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.

Figure A.2: Trends in Outcomes among Incumbent Patients



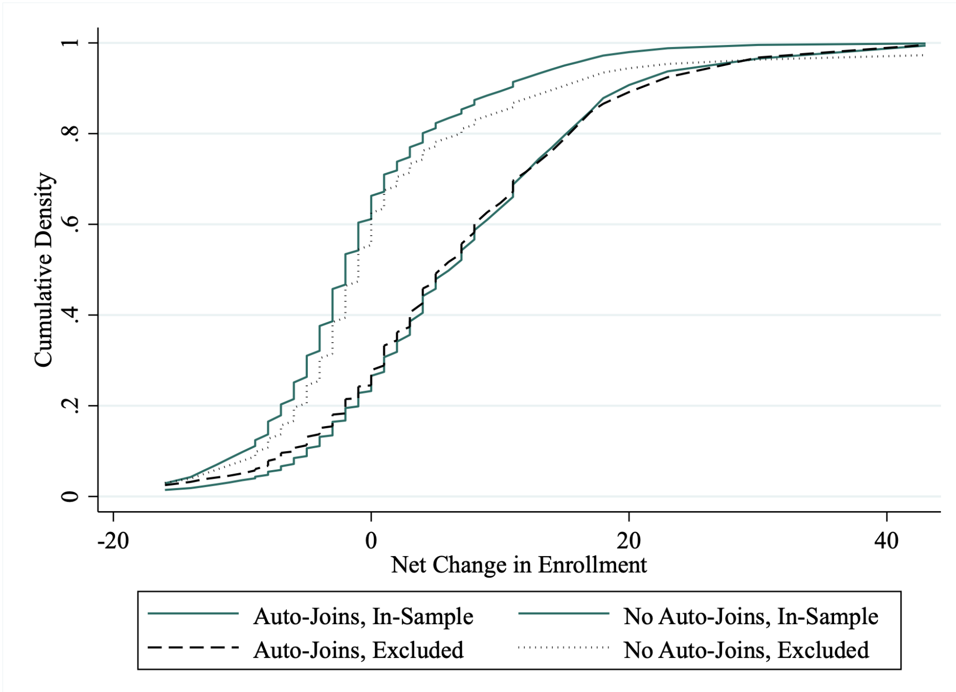
Notes: Both panels reflect means among physician-months within 13-month spells. Month 0 is the first month with auto-reassigned patients. Panel (a) shows the average percent change in the total number of registered patients. Panel (b) shows seasonally adjusted physician hours among incumbent patients, 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



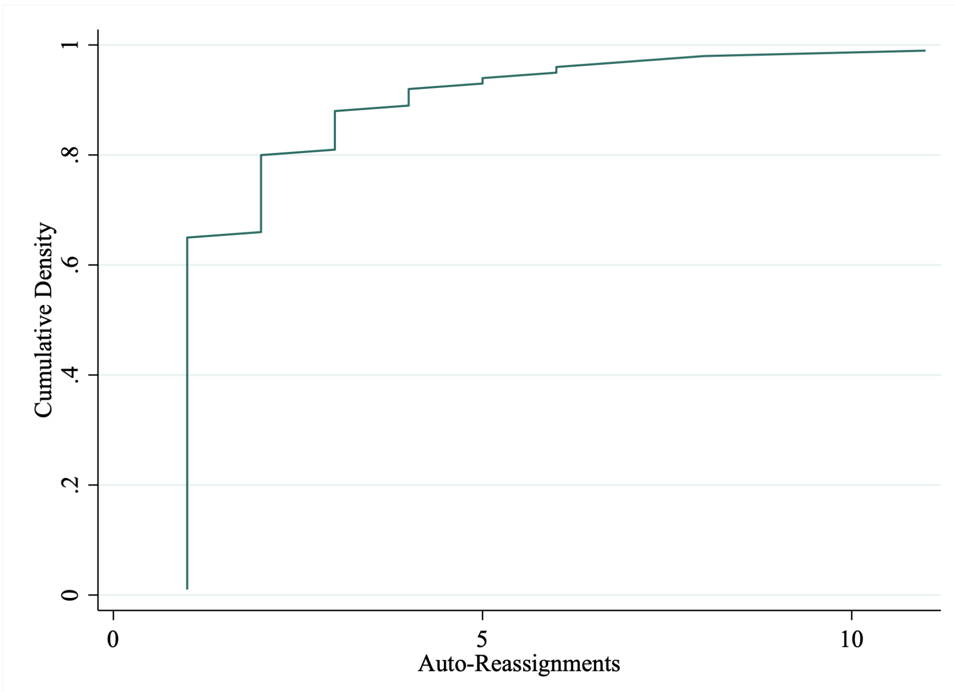
Notes: This figure decomposes the average one-month net change in enrollment between net regular joins and auto-joins. "Net Regular Join" is the average of the difference between patients that voluntarily leave and patients that voluntarily join. "Auto Join" is the average new registrations of patients 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.

Figure A.4: Cumulative Distribution of Enrollment Changes



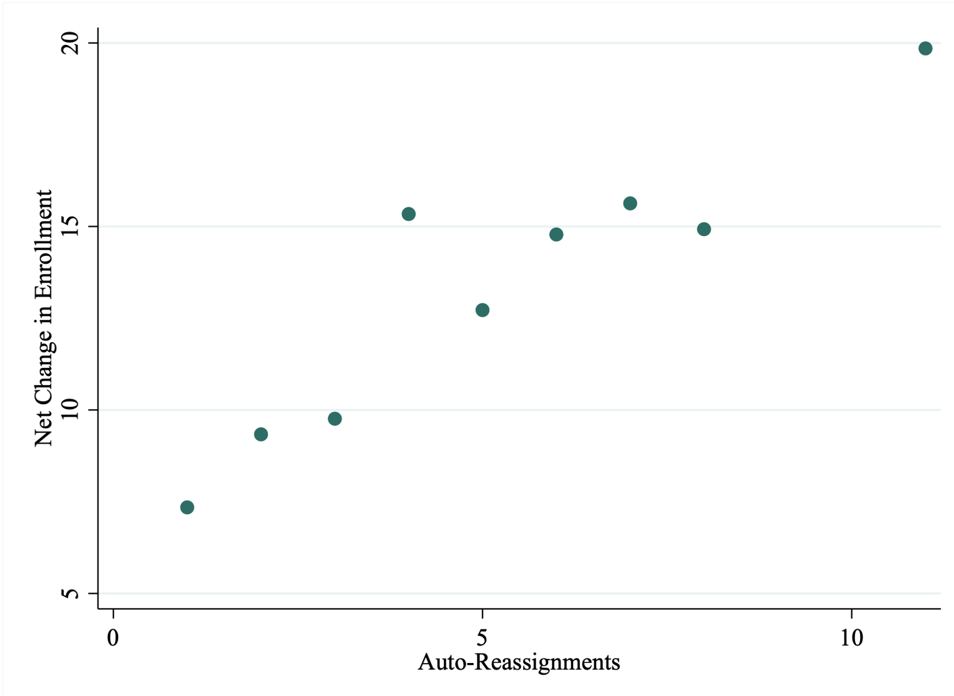
Notes: This figure shows the cumulative distribution of one-month changes in the number of patients for four subsamples of physicians. "Auto-Joins, In-Sample" includes months with any auto-reassignment to a physician in the analysis sample. "No Auto-Joins, In Sample" reflects other months in the analysis 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. Each curve is truncated at the 1st and 99th percentiles.

Figure A.5: Cumulative Distribution of Auto-Reassignments



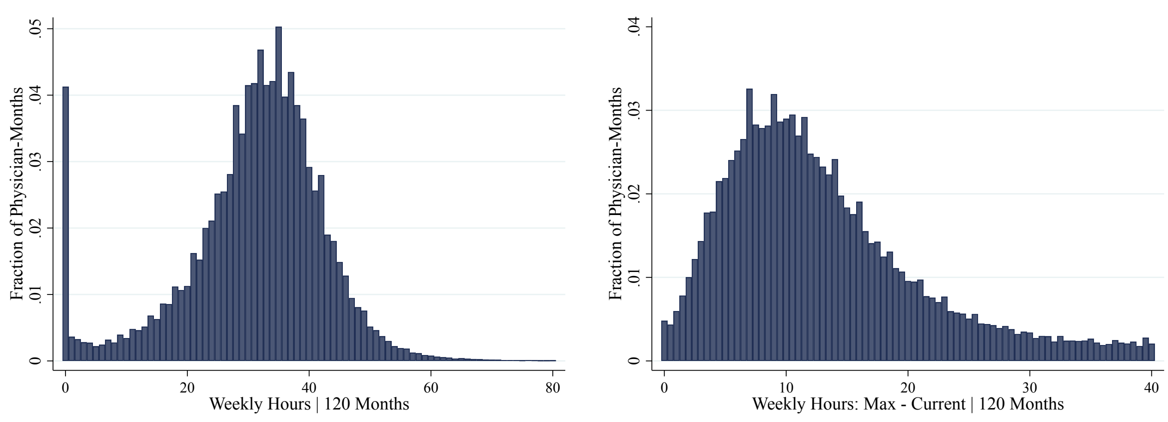
Notes: This figure shows the cumulative distribution of the number of patients auto-reassigned among physicians in the analysis sample in the first month of auto-reassignment (event month 0). The curve is truncated at the 99th percentile.

Figure A.6: Auto-Reassignments and Enrollment Changes



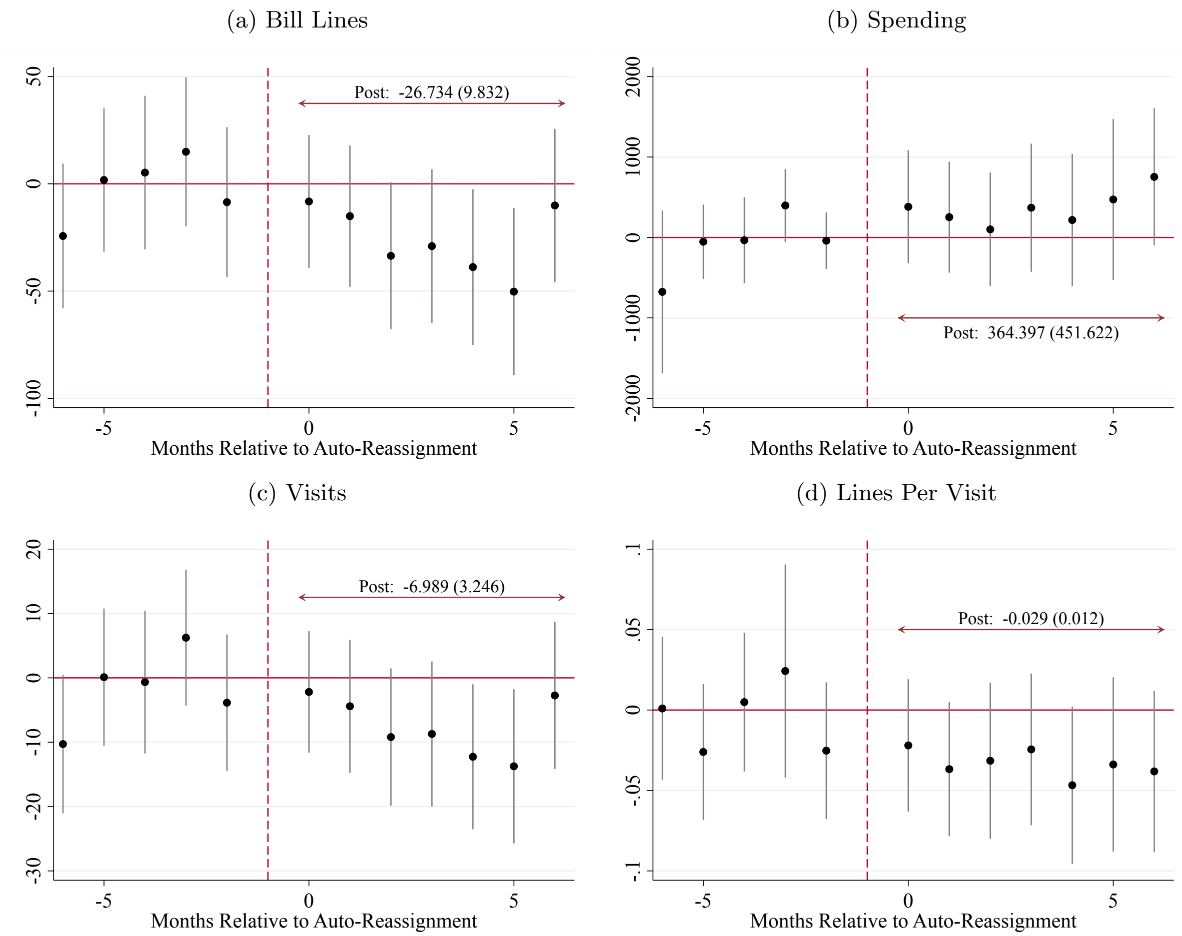
Notes: This figure shows the correlation between the number of patients auto-reassigned and the net change in enrollment. Each point is average among physicians for a percentile of the x-axis. When multiple percentiles have the same value, I use an average of averages. This figures uses the analysis sample in the first month of auto-reassignment (event month 0). The plot is truncated at the 99th percentile.

Figure A.7: 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 ($\tilde{M}_{jt}$). 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.

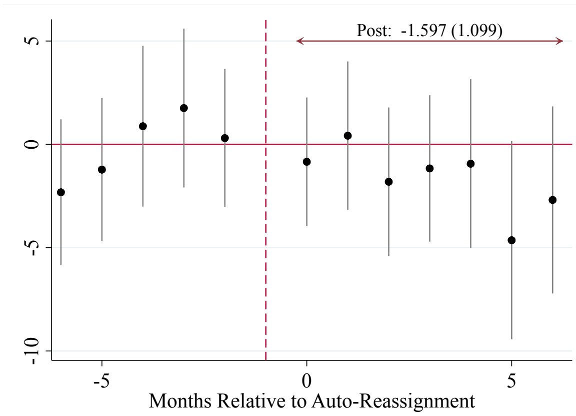
Figure A.8: Treatment Measures Among Incumbent Patients: Large versus Small Auto-Reassignments



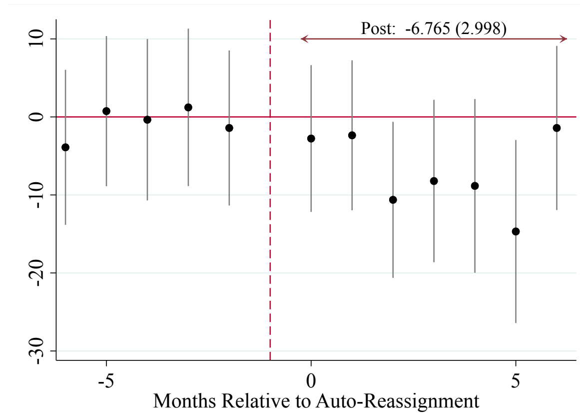
Notes: This figure shows estimates and 95% confidence intervals of the difference between physicians with large and small auto-reassignments in each month relative to auto-reassignment. The omitted month is -1. I define large auto-reassignments as those with two or more reassigned patients (969 of 2,722 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 correspond 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.9: Treatment Types Among Incumbent Patients: Large versus Small Auto-Reassignments

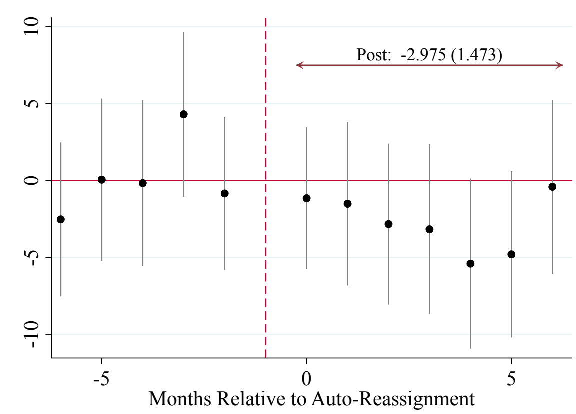
(a) Procedures



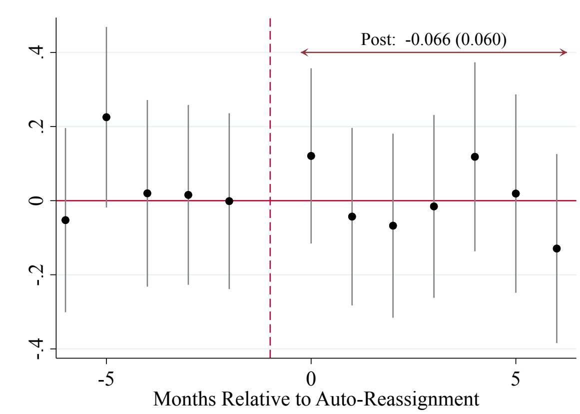
(b) Diagnostics



(c) Follow-ups



(d) Avoidable Hospitalizations



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 correspond 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.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
$\geq$ 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.

Table A.2: Test of Conditionally Random Assignment

	(1)	(2)
Availablity	0.028	0.027
	(0.007)	(0.007)
	[0.000]	[0.000]
Same Municipality	10.542	10.562
	(1.283)	(1.288)
	[0.000]	[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.

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	0.136	11.538	0.391
		(0.052)	(0.285)	(8.667)	(0.331)
		[0.954]	[0.634]	[0.183]	[0.239]
Dep. Var. Mean	1274.917	141.206	1547.545	13941.826	502.655
New Pat. Mean		0.124	1.313	131.205	0.400
F-Statistic	112.037	12.268	24.786	8.944	23.365
Observations	35,386	33,411	33,411	33,411	33,411
	Lines Per Visit (6)	Procedures (7)	Diagnostics (8)	Follow-ups (9)	Avoidable Hosp. (10)
Enrollment	−0.001	−0.086	0.107	0.150	0.007
	(0.001)	(0.050)	(0.094)	(0.080)	(0.001)
	[0.327]	[0.084]	[0.257]	[0.059]	[<0.001]
Dep. Var. Mean	2.922	97.827	367.454	179.930	4.292
New Pat. Mean	3.244	0.096	0.293	0.165	0.004
F-Statistic	84.653	48.531	37.662	23.279	6.405
Observations	33,411	33,411	33,411	33,411	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 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. New Pat. Mean is the average of the dependent variable among auto-reassigned patients, averaged across physicians in the six months after auto-reassignment. 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	0.017
	(0.001)	(0.006)
	[<0.001]	[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 Hospitalization is scaled up by 100.

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

	Capacity		Fee Level		Schedule	
	Slack	Binds	Low	High	Part-Time	Full-Time
	(1)	(2)	(3)	(4)	(5)	(6)
Cuml. Auto-Joins	5.674	1.037	1.041	2.699	1.006	1.206
	(0.789)	(0.039)	(0.048)	(0.498)	(0.036)	(0.142)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
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		Diagnoses		Gender	
	Under 65	Over 65	Healthy	Chronic	Male	Female
	(7)	(8)	(9)	(10)	(11)	(12)
Cuml. Auto-Joins	1.083	1.083	1.083	1.083	1.083	1.083
	(0.069)	(0.069)	(0.069)	(0.069)	(0.069)	(0.069)
	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
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

	Capacity		Fee Level		Schedule	
	Slack (1)	Binds (2)	Low (3)	High (4)	Part-Time (5)	Full-Time (6)
Enrollment	−0.028	−0.376	−0.326	−0.110	−0.445	−0.233
	(0.272)	(0.053)	(0.053)	(0.454)	(0.030)	(0.070)
	[0.919]	[<0.001]	[<0.001]	[0.809]	[<0.001]	[<0.001]
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		Diagnoses		Gender	
	Under 65 (7)	Over 65 (8)	Healthy (9)	Chronic (10)	Male (11)	Female (12)
Enrollment	−0.316	−0.027	−0.265	−0.078	−0.093	−0.251
	(0.031)	(0.037)	(0.042)	(0.020)	(0.017)	(0.042)
	[<0.001]	[0.455]	[<0.001]	[<0.001]	[<0.001]	[<0.001]
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

	Capacity		Fee Level		Schedule	
	Slack	Binds	Low	High	Part-Time	Full-Time
	(1)	(2)	(3)	(4)	(5)	(6)
Enrollment	0.059	−0.003	0.015	0.074	−0.191	0.259
	(0.087)	(0.156)	(0.157)	(0.137)	(0.012)	(0.054)
	[0.496]	[0.983]	[0.925]	[0.591]	[<0.001]	[<0.001]
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		Diagnoses		Gender	
	Under 65	Over 65	Healthy	Chronic	Male	Female
	(7)	(8)	(9)	(10)	(11)	(12)
Enrollment	0.008	−0.003	−0.002	0.007	0.012	−0.007
	(0.127)	(0.022)	(0.108)	(0.040)	(0.048)	(0.100)
	[0.947]	[0.876]	[0.986]	[0.865]	[0.802]	[0.945]
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

	Capacity		Fee Level		Schedule	
	Slack	Binds	Low	High	Part-Time	Full-Time
	(1)	(2)	(3)	(4)	(5)	(6)
Enrollment	−0.839	1.649	0.968	7.135	−2.916	6.976
	(2.419)	(4.526)	(4.257)	(11.029)	(1.978)	(1.852)
	[0.729]	[0.716]	[0.820]	[0.518]	[0.140]	[<0.001]
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		Diagnoses		Gender	
	Under 65	Over 65	Healthy	Chronic	Male	Female
	(7)	(8)	(9)	(10)	(11)	(12)
Enrollment	1.094	0.174	0.617	0.652	0.707	0.561
	(3.590)	(0.640)	(3.026)	(1.141)	(1.339)	(2.829)
	[0.761]	[0.785]	[0.839]	[0.568]	[0.597]	[0.843]
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.