Provider effects in antibiotic prescribing: Evidence from physician exits*

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Abstract

In the fight against antibiotic resistance, reducing antibiotic consumption while preserving healthcare quality presents a critical health policy challenge. We investigate the role of practice styles in patients' antibiotic intake using exogenous variation in patient-physician assignment. Practice style heterogeneity explains 49% of the differences in overall antibiotic use and 83% of the differences in second-line antibiotic use between primary care providers. We find no evidence that high prescribing is linked to better treatment quality or fewer adverse health outcomes. Policies improving physician decision-making, particularly among high-prescribers, may be effective in reducing antibiotic consumption while sustaining healthcare quality.

Keywords: antibiotic prescribing, practice styles, primary care providers

JEL codes: I11, J44, I12

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1 Introduction

Yearly, more than 700,000 global deaths are caused by antibiotic and antimicrobial resistant infections. This number surpasses the annual toll of 627,000 total deaths from malaria, 685,000 deaths from breast cancer, or 500,000 deaths attributable to drug use including opioid overdose.[1] The continuing spread of resistant bacteria turns even common infections and standard surgical procedures into high-risk events. Without efforts to contain the rise of antibiotic resistance, current forecasts predict 10 million global deaths will occur per year due to antibiotic resistant infections within the next three decades (World Bank 2017; Laxminarayan 2022).

Human antibiotic prescribing has been identified as the single most important determinant of antibiotic resistance (Adda 2020; Costelloe et al. 2010). Major public health campaigns and policies aim to reduce antibiotic consumption in the patient population by targeting physicians and other health professionals.[2] The effectiveness and efficiency of such policies depend vitally on the extent to which physicians affect their patients' antibiotic intake and health outcomes. Medical providers have been subject to extensive study in terms of their contribution to healthcare costs, the prescription of addictive drugs, and referrals to specialists (Fadlon and Van Parys 2020; Agha et al. 2022; Finkelstein et al. 2021). However, remarkably little is known about the quantitative importance of physicians' practice styles for antibiotic prescribing. Given the urgency and importance of the antibiotic resistance crisis, much more decisive policy action in the health care sector may be warranted if antibiotic overuse is largely associated with idiosyncratic provider practice styles.

[1] See WHO Malaria Fact Sheet (https://www.who.int/news-room/fact-sheets/detail/malaria), WHO Breast Cancer Fact Sheet (https://www.who.int/news-room/fact-sheets/detail/breast-cancer), and WHO Opiod Overdose Fact Sheet (https://www.who.int/news-room/fact-sheets/detail/opioid-overdose) (access: 20 December 2021).

[2] For example, the annual European Antibiotic Awareness Day campaign (https://antibiotic.ecdc.europa.eu/en) provides informational material to health care professionals (access: 13 June 2021) and the World Health Organization offers an online course on antibiotic stewardship for practicing clinicians (https://openwho.org/courses/AMR-competency) (access: 20 December 2021). In the medical literature, a number of randomized control trials evaluate behavioral interventions to affect physician prescribing behavior, e.g. Hallsworth et al. (2016) and Meeker et al. (2016).

In this study, we measure the effect of physician antibiotic prescribing style in primary care on patients' antibiotic consumption. Our empirical strategy exploits exogenous variation in patient-provider assignments due to physician exits. When physicians move or retire from their clinic, or when a clinic closes due to their exit, their patients must switch to a new provider and are thereby exposed to a different practice style. We quantify these practice style differences in antibiotic prescribing using large-scale administrative data from Denmark and examine how practices styles are associated with physician and clinic characteristics. Finally, we link our practice style estimates to measures of prescribing quality and adverse patient health events.

The field of health economics has seen a surge in literature exploring the concept of practice style, which refers to all components of a physician's treatment decision that affect different patients in a consistent manner. Practice styles have been shown to persistently drive within- and between-region variation in health care utilization (e.g. Chandra and Staiger 2007; Epstein and Nicholson 2009; Molitor 2018; Cutler et al. 2019; Simeonova et al. 2022). Antibiotic prescribing provides a critical but overlooked context to study physician practice styles. Antibiotics represent one of the most frequently used medical treatments. In many countries, including Denmark, physicians play a critical role in facilitating access to these vital drugs. Specific to antibiotics, physicians' prescribing decisions involve an important public health trade-off: While the use of any antibiotic imposes an external cost by contributing to antibiotic resistance, lower intensities of antibiotic prescribing may put patient health at risk. The bulk of policy interventions aimed at reducing the emergence of antibiotic resistance target the individual physician but lack an efficiency evaluation and do not consider patient health outcomes. Whether interventions targeting physicians are effective and efficient hinges on whether variation in antibiotic prescribing reflects differences in patient needs or differences in practice styles. Thus, for designing effective public policies, it is crucial to quantify and characterize the role of physicians in antibiotic prescribing.

The institutional environment in Denmark provides a powerful setting to examine the

importance of practice style variation in antibiotic use. Denmark's universal healthcare system ensures equal access and is based on a gatekeeper system, where each citizen enters the healthcare system through an assigned primary care provider. Moreover, standards in medical education are high and consistent.[3] Finally, Denmark is at the forefront of antibiotic stewardship efforts. A physician prescription is required to access antibiotics, while financial incentives to prescribe antibiotics are largely absent. Hence, antibiotic consumption is comparatively low in Denmark (Coenen et al. 2007). We observe that even in such a low-prescribing and homogeneous setting, antibiotic prescribing differs drastically across primary care clinics, with a national mean of 0.72 (0.86) prescriptions per patient per year and a standard deviation of 1.64 (1.72) in 2005 (2012).

Our main analysis reveals that practice styles account for 49 percent of the antibiotic prescribing differences between primary care providers, implying that standardizing practice styles could reduce differences by half. However, the magnitude of the effect differs by antibiotic subcategory and is greatest, 83 percent, when we limit our analysis to the second-line antibiotic classes macrolides, lincosamides and streptogramins, cephalosporins, and quinolones, which carry higher public health and antibiotic resistance costs. Variation in practice styles among physicians may arise due to their individual backgrounds, experiences, preferences, and factors such as diagnostic skills (e.g. Currie et al. 2016). We find that higher prescribing intensities are linked to observable physician and clinic characteristics such as physician age and education background, staff size, and the availability of diagnostic tests.

Our main identifying assumption requires that antibiotic prescribing to patients exposed to a physician exit would have followed the same trends as prescribing to other patients, if not for the physician exit. This parallel trend assumption is backed up by our empirical setting as the timing of physician exits is plausibly exogenous to antibiotic consumption. In addition, we show that pre-trends in antibiotic consumption prior to a physician exit are mostly absent. To ensure unbiased estimates of the role of provider effects in antibiotic

[3]For example, physicians begin their careers as interns distributed across the country based on a lottery system. Fadlon et al. (2020) find strong persistence in physician location based on a physician's initial draw.

consumption differences, we further require that patients are not systematically assigned to post-exit physicians with regard to their antibiotic prescribing. We can largely rule out selective assignment in our setting, as patients' choice of primary care physician is highly restrictive in Denmark so that most patients are as good as randomly re-assigned after a physician exit. Furthermore, we provide supporting evidence against selective assignment based on observable characteristics.

Finally, we do not find evidence indicating that practice styles with higher intensity antibiotic prescribing reflect more efficient prescribing benefiting patients at the margin. Instead, we can link higher intensity antibiotic prescribing styles to higher numbers of avoidable treatment failures and more prescriptions without diagnostic tests, compared to the mean. Additionally, we do not find that being assigned to a higher-prescribing physician reduces patients' risk for adverse health events, as measured by hospitalization rates for infection-related ambulatory care sensitive conditions. In fact, our results suggest that exposure to a higher intensity prescribing style for penicillins, the most commonly prescribed class of antibiotics with high levels of antibiotic resistance, increases a patient's rate of avoidable infection-related hospitalizations.

Our findings have important implications for the design of health care policy measures to combat the rise of antibiotic resistance. While a correlation between antibiotic prescribing and resistance is well established, it is difficult to infer whether physicians who prescribe with higher intensity exacerbate the issue of antibiotic resistance *unnecessarily*. Medical practitioners may believe that their prescribed treatments are optimal based on individual patient circumstances. Antibiotics can provide sizeable benefits when bacterial infections are difficult to treat or when patients' health needs necessitate a higher intake. In such cases, policies aimed at reducing antibiotic prescribing could have adverse effects on high-risk patients who require a more intensive treatment approach. However, our study suggests that incentivizing high-prescribing physicians to emulate low prescribers' practice styles could be an effective strategy without negatively impacting patient health in primary care. In countries

where provider influence is amplified by weaker antibiotic stewardship efforts, complex financial incentives, and a more heterogeneous healthcare workforce, policy interventions targeting physicians or further standardization of care may promise even greater benefits.

Several studies emphasize the importance of identifying causes of variation in health care provision to effectively target policies. Finkelstein et al. (2016) separate local place effects from patient-specific drivers of geographic variation in health care utilization in the United States using patient mobility. Migration of patients has also been exploited to study geographic variation in prescription opioid abuse in the United States (Finkelstein et al. 2021), regional health care and mortality in Norway (Godøy and Huitfeldt 2020), or ambulatory care utilization in Germany (Salm and Wübker 2020). Fadlon and Van Parys (2020) highlight the role of physicians by using physician exits from Medicare to investigate the impact of switching to a primary care provider with a high-utilization practice style on patients' health care utilization in the United States. In our study, we also employ a physician migration framework but we isolate physician agency from patient factors in a setting where variation in the institutional setup and financial incentives across regions are minimal. To inspect the potential (in)efficiency of practice style variation, we characterize prescribing practice styles by physician characteristics and quality of care. The physician migration framework allows us to hold patients' social, economic, and health environment fixed, which is particularly useful in the context of antibiotic prescriptions, where community-acquired infections can vary in their transmissibility and treatment difficulty.

Our research adds to an extensive economic literature on antibiotic prescribing that has shaped public health policies (e.g. Laxminarayan et al. 2001; Currie et al. 2014; Bennett et al. 2015; Ellegård et al. 2018). However, while this literature has focused on institutional factors and payment schemes, there remains a limited understanding of the quantitative importance of physicians' prescribing practices and their efficiency implications. Ribers and Ullrich (2022) focus on antibiotic prescribing for urinary tract infections to study heterogeneity in physicians' preferences and their abilities to diagnose bacterial infections. Our study contributes to this

body of evidence by identifying physicians and their practice styles as the relevant targets for effective policies that aim to curb the growth of antibiotic resistance.

The remainder of the paper is organized as follows. Section 2 presents a model of prescribing to conceptualize the causal mechanism we aim to measure. Section 3 describes the institutional background and data. Section 4 provides descriptive evidence motivating the main analysis. Section 5 describes identification and estimation and Section 6 presents results. Section 7 provides further results on provider practice styles in antibiotic prescribing, including correlates with observable characteristics and quality of care. Section 8 concludes.

2 Measuring practice style differences

Our primary objective is to quantify the importance of provider practice styles in determining patients' consumption of prescribed antibiotics. Practice styles encompass a range of factors such as diagnostic skill and technologies used, medical treatment philosophies and preferences, or the quality of management and logistics in a clinic (Currie et al. 2016). We formalize practice styles as provider-specific, time-invariant determinants in a model of prescription decisions. Based on this framework, we measure the impact of provider effects on patients' antibiotic consumption by the share of the differences in antibiotic prescribing attributable to differences in practice styles.

In some cases, clinics are run by multiple physicians. For these, we measure provider effects over time-varying sets of physicians.[4] Our definition of practice styles hence encompasses differences in prescribing between clinics and within clinics, where the identity or number of physicians changes over time. We refer to a set of primary care providers in the same clinic as *physicians* where unambiguous.

We adopt a stylized linear and additively separable model in which antibiotic prescribing

[4]We apply a standardization procedure to ensure that our estimated practice styles are comparable across single- and multi-physician clinics. Furthermore, our main analysis yields qualitatively similar results when restricted to single-physician clinics.

by physicians j to their assigned patient i in year t is expressed as:

$$y_{ijt} = \alpha_i + \delta_{j(i,t)} + x_{it}\beta + \epsilon_{it}, \tag{1}$$

where y_{ijt} is a measure of antibiotic prescribing, α_i denotes all time-invariant individual factors affecting patient i's antibiotic prescriptions, $\delta_{j(i,t)}$ denotes the antibiotic prescribing practice style of physicians j assigned to patient i in year t, and x_{it} is a vector of time-varying patient and clinic characteristics, with β denoting the corresponding vector of coefficients.[5] Finally, ϵ_{it} denotes an idiosyncratic error term.

In order to identify the impact of practice styles on antibiotic prescribing, variation in physician assignments is necessary. When patients i and k are assigned to different providers j and j', they may receive different levels of antibiotic treatment, denoted as y_{ijt} and $y_{kj't}$, respectively. Such differences in antibiotic prescribing could be due to patient factor differences, characterized by $\alpha_i \neq \alpha_k$, or exposure to distinct practice styles, characterized by $\delta_j \neq \delta_{j'}$, ignoring time-varying characteristics x_{it} for now. To separate the extent to which differences in practice styles contribute to differences in prescribing to patients, we require a source of exogenous variation in patient-provider assignments.

Our source of quasi-experimental variation is based on physician exits from clinics. When a physician leaves a clinic, patient i is either assigned to a new clinic altogether or she stays at the clinic but is no longer treated by the exiting physician. A physician exit hence moves patient i's assigned provider from j to j'. This change in physicians results in a shift in the practice style to which patient i is exposed from δ_j to $\delta_{j'}$.

Hence, patients exposed to a change in their assigned set of physicians due to physician exits provide information about the difference in practice styles, $\delta_{j'} - \delta_j$. To operationalize this approach, we define a treatment indicator D_{it} that is equal to one after patient i experiences a physician exit event, and zero otherwise, $D_{it} = 1\{j(i,s) = j, j(i,t) = j', s < t\}$. We can

[5]Equation (1) follows from an expected utility maximization problem, where physicians make antibiotic prescribing decisions on behalf of their patients. We discuss the underlying utility model in Appendix A.

rewrite Equation (1) as:

$$y_{ijt} = \alpha_i + \delta_j + (\delta_{j'} - \delta_j) \times D_{it} + x_{it}\beta + \epsilon_{it}.$$

With exogenous treatment D_{it}, the difference in practice styles $\delta_{j'} - \delta_j$ is identified. However, because identification is in differences rather than levels, practice styles are not directly comparable across treated patients.[6] For example, the average difference in practice styles $\delta_{j'} - \delta_j$ across all sets of physicians $\{j, j^{'}\}$ could be zero if treated patients are equally likely to switch to physicians with higher or lower prescribing practice styles than their pre-exit physician.

To obtain a generalizable measure, we adjust the difference in practice styles between two sets of physicians assigned to a treated patient by the difference in their patients' mean consumption of antibiotic treatments. This scaling procedure also ensures that provider effects are comparable between clinics where all patients are assigned to new physicians and multi-physician clinics where some patients retain their original physician. When a physician leaves a clinic, prescribing behavior towards patients whose physicians stay at the clinic is likely to change only slightly, which can dampen the difference in practice styles $\delta_{j'} - \delta_j$. However, in such cases, the *scaled* provider effects can still be large if the difference in mean antibiotic consumption under two sets of physicians j' and j is small due to partial patient reassignment after a physician exit.

The scaled difference in practice styles represents the proportion of provider effects that contribute to differences in mean antibiotic prescribing between physicians. We denote the scaled difference in practice styles between two sets of physicians j and j' as $\frac{\delta_{j'} - \delta_j}{y_{j'} - y_j}$. Here, $y_j = \mathbb{E}[y_{ijt}|D_{it} = 0]$ represents mean prescribing of physicians j to patients never or not yet exposed to treatment.

[6] Identification is restricted to differences due to the one-directional nature of reassignments, from a treated set of physicians to an untreated set of physicians. If we could observe chains of reassignments between patients and providers, identification of practice styles in levels up to a provider average would be possible (see Hull 2018).

For a patient i exposed to a physician exit, we define the difference in mean prescribing between their pre-exit physician, j, and the physician they are reassigned to, j', as $\Delta_{i(j',j)} = y_{j'} - y_j \equiv \Delta_i$. This measure Δ_i captures all differences in antibiotic prescribing between physicians j and j', which can result from differences in patient pools, provider effects, or time-varying control variables. We can then rewrite Equation (1) as:

$$y_{ijt} = \alpha_i + \delta_j + \theta \times D_{it} \times \Delta_i + x_{it}\beta + \epsilon_{it}, \tag{2}$$

where the parameter θ measures the extent to which differences in prescribing patterns can be attributed to differences in provider practice styles. The parameter θ is zero if variation in antibiotic prescribing is solely determined by differences in physicians' patient pools.[7]

In summary, the parameter θ provides an answer to the question: By what proportion could we reduce the difference in antibiotic consumption between patients assigned to exiting physicians j and patients assigned to destination physicians j' by standardizing prescribing practice styles? Thus, this measure quantifies the extent of variation in antibiotic consumption induced by heterogeneity in physicians' prescribing styles.

3 Institutional background and data

The Danish healthcare system provides major advantages to quantify the role of practice style variation in antibiotic consumption, which is often confounded by institutional factors. As the system is characterized by notable homogeneity in access to health care, medical education, and centrally designed public health efforts, we would expect few factors driving variation in health care provision. Our analysis is based on linked administrative data on the population level, which includes information on health care claims, individual-level socio-economic characteristics, and all purchases of antibiotics through primary care across

[7]For never-treated patients, Δ_i is set to zero and θ is undefined.

Denmark.[8] Information can be linked using patients' personal identification numbers and their primary care provider's license numbers.

3.1 Institutional setting

We study antibiotic prescribing practice styles using administrative data that covers Denmark's entire population. Denmark has a tax-funded public health insurance system that fully covers all primary care. Primary care providers act as gatekeepers; visits to most specialists and scheduled hospital procedures require a referral from primary care to be covered by insurance. Primary care providers are self-employed and work under nationally regulated contracts. Every clinic must acquire a unique license number (ydernummer) to file reimbursement claims. During our observation period of 2005-2012, approximately 3,280 primary care clinics file claims.

Patients are assigned to a fixed primary care provider through a list system. Switching to another provider requires a small fee of 150 DKK (about 20 USD), and the new provider must be located within 15 km (5 km in metropolitan areas) of patients' residence (see Simonsen et al. 2021). When switching, the only public information patients can obtain about clinics in their choice set are the names and ages of physicians as well as the addresses of clinics. Physicians cannot turn away patients selectively but clinics may close for new patients after reaching 1,600 listed patients per physician. Patients rarely switch away from their default primary care provider except when moving, also because capacity constrained clinics limit the actual choice set of primary care providers. Kristiansen and Sheng (2022) document that in the majority of municipalities in 2010, primary care physicians were close to or larger than the capacity limit of 1,600 patients per physician.

In Denmark, patients require a physician prescription to obtain antibiotic drugs, which are purchased at pharmacies with a small copayment. About 90% of human antibiotics are purchased in outpatient care, out of which about 75% are prescribed through primary care

[8] See Statistics Denmark (2012b,d,e,a,f,g,c,h,i) and The Danish Health Data Authority (2012a,b).

physicians.[9] Danish physicians are not remunerated for prescriptions, therefore the type and number of antibiotics dispensed is not driven by physicians' financial incentives. In general, Denmark has low rates of antibiotic consumption and conservative prescribing practices (Coenen et al. 2007).

When a physician leaves a clinic due to retirement or relocation, her patients are as good as randomly reassigned to new physicians conditional on location. After a physician exit, a clinic either closes or, for clinics with multiple physicians, the remaining physicians can continue operating. If the clinic closes, the local government reassigns all patients based on their residential address, either to existing providers or to new physicians who have acquired the entire patient list (and often the physical practice).[10] If a clinic with multiple physicians continues operating after a physician leaves, the clinic may reduce the number of patients and reassign some patients to nearby clinics. In this case, all patients are first off-listed and, subsequently, required to re-apply for the clinic on a first-come, first-serve basis. The local government is responsible to ensure that all patients have access to at least two nearby clinics in their region.

3.2 Sample construction

To construct a sample of patients matched to clinics between 2005 and 2012, we follow a two-step process. Firstly, we identify primary care clinics in which physician exits occurred. Secondly, we match patients to their main primary care clinic in order to create a yearly panel of patient-level observations.

In the first step, we consider all primary care physicians in the Danish registry of clinics. This registry links physicians' personal identification number to their clinics' license number and registers the in- and outflow of physicians to clinics. For reasons of data minimization the

[9] See information from the Ministry of Health (https://sum.dk/nyheder/2017/juli/ny-dansk-handlingsplan-skal-bremse-antibiotikaresistens-) (access: 03 April 2023).

[10] Patients have the right to change from the default reassigned provider free of charge but have to actively choose an alternative clinic that is open for intake. See information from the City of Copenhagen (https://international.kk.dk/live/healthcare/going-to-a-doctor/changing-your-doctor) (access: 16 September 2022), and regulations LBK no. 903 of 26/08/2019 and BEK nr 1056 af 31/05/2021).

Danish Health Data Protection Authority provided only a non-selective portion of the license number registry. Out of all 3,280 clinics that file claims, we consider the 1,605 clinics for which registry records are complete. We supplement information on the outflow of physicians by adding data from the national death registry, the employment registries, and the health claims registries. We assume a physician exit for deaths or retirements, or when a physician joins a new clinic. We identify clinic closures by the last year a clinic files claims.

We impose two sample restrictions to construct pre- and post-exit periods of a clinic exposed to a physician exit. First, we keep only the 1,197 clinics during our sample period, 2005 to 2012, without a physician exit in 2005. Second, we only consider long-term exits and exclude clinics with multiple physician exits in different years or clinics with physician entries that do not coincide with an exit. The resulting sample contains 980 clinics. We refer to clinics exposed to physician exits in exactly one year as *treated clinic* and the physician exit as *treatment*. We refer to clinics never exposed to treatment as *never-treated clinics*. We refer to never-treated clinics with patient intakes from treated clinics as *destination clinics*.

In the second step, we match patients to their primary care clinic. We use weekly claims data and find the modal clinic for each patient in every year.[11] We consider all patient-year observations assigned to any of the 980 primary care clinics in our analysis, keeping 25.33% of the overall patient-year claim observations. We include patients only when they switch their modal clinic at most once, dropping 15.68% of the remaining patient-years. We refer to patients who are ever exposed to a physician exit as *treated patients* and the complementary set of patients as *never-treated patients*. We refer to treated patients prior to exposure to the physician exit as *not-yet-treated* patients.

For patients who switch exactly once, we ensure that patients are exposed to at most two practice styles, defined by the pre- and the post-exit period. We exclude all patients

[11]We consider unique claim weeks by aggregating all claims filed during the same week by the same clinic. Among patients with multiple modes, we assign a patient to the modal clinic that files the most antibiotic prescriptions or, in case of a tie, the earliest claim in a given year for this patient. Some patients switch back and forth in their modal clinic over the years. In these cases, we impute their matched clinic to be the same as the one they switched back and forth. These cases account for 1.25% of all patient-years.

assigned to more than one treated clinic, dropping 0.32% of observations. For never-treated patients who switch their primary care clinic, we keep observations from their modal clinic and exclude patients for whom the mode cannot be recovered, dropping 1.64%. For patients switching from a treated clinic, we keep all observations if the switch coincides with the exit; otherwise, we keep only observations associated with the treated clinic, dropping 0.05%. For patients switching to a treated clinic, we keep only observations at the treated clinic, dropping 0.05%. We exclude a patient's first observed year at a treated clinic if that year is the treatment year, dropping 0.15%.

The final sample contains 7,789,908 patient-year observations matching 1,371,604 patients to 805 primary care clinics.[12] Of these, 1,526,215 patient-year observations matched to 242 clinics are exposed to physician exits. In 211 out of the 242 treated clinics, the physician exit leads to clinic closure. After a physician exit, patients are reassigned to an average of 23.0 destination clinics, and an average of 11.27 patients are reassigned to each destination clinic.

3.3 Variable definitions

In the final sample, we construct outcome variables measuring antibiotic prescriptions, along with the main explanatory variable, the difference in mean antibiotic prescribing following a physician exit.

Outcomes. Our main outcome variable is the number of primary care antibiotic prescriptions dispensed to each patient per year at Danish pharmacies. We define an antibiotic prescription as all packages of drugs belonging to the same level 3 Anatomic Therapeutic Chemical (ATC) class in the therapeutic subgroup of antibacterials for systemic use J01, dispensed on the same day to a given patient.

We analyze both the total number of all systemic antibiotic prescriptions (All J01) and three separate subcategories at the level of therapeutic-pharmacological classes (ATC 3):

[12]We exclude 10 clinics with insufficient prescribing to never-treated or not yet treated patients (fewer than 100 patient-year observations), and 0.26% of the sample that are singleton observations. Appendix B provides an overview over the number of clinics, patients, and patient-year observations in our sample by treatment status.

penicillins, second-line antibiotics, and other classes. Penicillins (J01 C) are the most-prescribed antibiotic class in primary care. They are important from the perspective of individual patients as they typically reflect an initial antibiotic treatment. Second-line antibiotics encompass all macrolides, lincosamides, and streptogramins (J01 F), cephalosporins (J01 D), and quinolones (J01 M). The European Surveillance of Antimicrobial Consumption (ESAC) considers the consumption of these antibiotics as a quality indicator since their use may suggest 'poor practice' when combined with other evidence Coenen et al. (2007). The majority of second-line drugs are broad-spectrum antibiotics, which are more likely to promote the development of antibiotic resistance and impose higher public health costs than other antibiotics.[13] We group the remaining antibiotic classes into a single subcategory.

Table 1 shows descriptive statistics by antibiotic class. Penicillins (J01 C) represent the largest share of prescribed antibiotics, followed by macrolides, lincosamides and streptogramins (J01 F), and sulfonamides and trimethoprim (J01 E).

Table 1: Descriptive statistics for antibiotic prescribing

Panel A	**Number of antibiotic prescriptions by ATC 3 class**		
ATC 3[a]	Pharmacological subgroup	Share of total prescriptions	Share of non-zero observations
J01 C	Beta-lactam antibacterials, penicillins	60.12%	19.22%
J01 F	Macrolides, lincosamides, streptogramins	18.25%	6.52%
J01 E	Sulfonamides and trimethoprim	9.97%	2.92%
J01 A	Tetracyclines	4.33%	1.03%
J01 M	Quinolone antibacterials	3.87%	1.41%
J01 X	Other antibacterials	3.40%	0.87%
J01 D	Other beta-lactam antibacterials	0.07%	0.02%
All J01	Antibacterials for systemic use	100%	26.52%

Panel B	**Number of antibiotic prescriptions by subcategory**		
		Mean	SD
All antibiotics	All J01	0.56	(1.277)
Penicillins	J01 C	0.35	(0.817)
Second-line	J01 F, D, M	0.12	(0.46)
Other	J01 excluding J01 C, F, D, M	0.09	(0.673)

[a] Prescriptions of J01 G (Aminoglycoside antibacterials) are omitted due to their low frequency in order to ensure anonymity.

[13] The distinction between first- and second-line antibiotic treatments depends on the disease indication. We refer to macrolides, lincosamides, streptogramins (J01 F), cephalosporins (J01 D), and quinolones (J01 M) collectively as second-line drugs as they are labeled as such in the ESAC framework. Macrolides, cephalosporins, and quinolones are also often characterized as broad-spectrum antibiotic drugs, with the exception of erythromycin (J01 FA01) (ECDC, EFSA Panel on Biological Hazards (BIOHAZ) and EMA Committee for Medicinal Products for Veterinary Use (CVMP) 2017). Broad-spectrum antibiotic drugs are active against a broad range of bacterial groups and, hence, more likely to cause multi-drug resistances.

Physician exits. We define a physician's exit from a clinic as the treatment and the period following the exiting event as post-treatment period. An exit may or may not lead to the clinic's closure. In any case, a physician exit impacts the patient-physician relationship by altering the pool of available physicians who can provide treatment. In cases where the clinic remains open after a physician exit, we account for two distinct practice styles by treating the pre- and post-treatment clinics as separate sets of physicians.

Measuring differences in mean prescribing. To construct the measure of differences in mean prescribing Δ_i defined in equation (2), we estimate mean prescribing y_j by the average number of antibiotic prescriptions by physicians j to patients who have never received treatment or have not yet been treated. That is, we ensure that the patient pools of pre- and post-treatment physicians are kept separate by excluding post-exit observations.[14] We estimate mean prescribing separately for each patient based on a leave-one-out average, excluding a patient's own prescriptions. The difference in mean prescribing is nonzero for all patients for whom we observe a change in the set of physicians that they are assigned to.

The scaling factor Δ_i differs from that of Fadlon and Van Parys (2020) who use the difference in mean prescribing unconditional on treatment status. However, scaling by the difference in unconditional mean prescribing can lead to varying shares of provider effects depending on the relative proportions of treated and never-treated patients.[15] Scaling in the differences in mean prescribing *conditional on being not-yet-treated or never-treated* ($D_{it} = 0$) offers the advantage that there are no overlaps in patient pools assigned to physicians j and j'. Consequently, θ does not depend on the proportions of patients exposed to treatment.

[14]For instance, in case of a clinic exposed to a physician exit, we estimate pre-exit mean prescribing based on observations from not-yet-treated patients until the exit event, and post-exit mean prescribing based only on observations from never-treated patients. To reduce noise in the average prescribing estimates, physicians with fewer than 100 observations from never- or not yet treated patients as well as physicians with zero average prescribing are dropped from the final analysis sample. To account for estimation error in average prescribing, we employ a bootstrap procedure in the main analysis.

[15]Intuitively, as the share of treated patients increases, the patient pool of treated and destination physicians overlap, and patient factors differ less. As the scaling factor becomes smaller, the relative importance of provider effects becomes larger. We provide a formal explanation of this relationship in Appendix C.

4 Descriptive evidence

We first present descriptive evidence of considerable variation in antibiotic prescribing across primary care clinics in Denmark. To demonstrate that our sample is not systematically selected in terms of treatment, we show that there are no observable differences in summary statistics of patients assigned to primary care clinics with a physician exit and patients not assigned to such clinics. We also discuss some observable differences between treated and never-treated clinics. Lastly, we provide descriptive evidence that patient-physician reassignments due to physician exits coincide with a visible shift in treated patients' antibiotic prescriptions, which motivates our subsequent causal analysis.

4.1 Variation in antibiotic prescribing

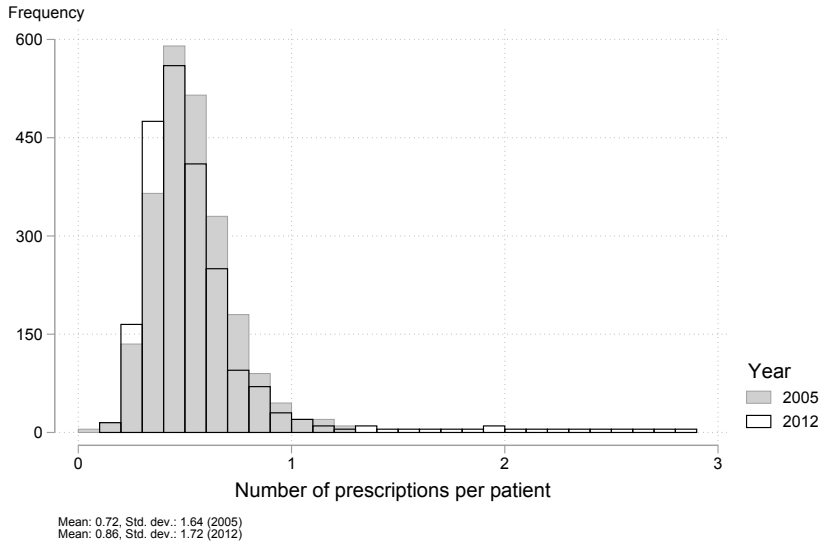
We document persistent variation in antibiotic prescribing across primary care clinics in Denmark in Figure 1, which shows the distribution of the clinic-level average number of antibiotic prescriptions per patient and year in 2005 and 2012. The means of these two distributions are 0.72 and 0.86 with standard deviations of 1.64 and 1.72, indicating considerable variation in antibiotic prescribing even for a low-prescribing country such as Denmark.[16]

4.2 Sample summary statistics

Table 2 presents descriptive statistics for treated and never-treated patients. Panel A shows means and standard deviations for antibiotic prescribing, the main outcome variable. Panel B displays descriptive statistics for basic demographics and health characteristics, including the rate of hospitalizations for infections, and Panel C shows information on family and education characteristics. On average, treated patients are older than those never exposed to physician exit. However, most other characteristics are similar between the two groups.

[16]Figure 7 in Appendix D.1 shows the distribution of antibiotic prescribing over clinics for all sample years.

Figure 1: Distribution of antibiotic prescribing over primary care clinics



Notes: Average number of antibiotic prescriptions per patient at the clinic-level in 2005 and 2012. Bunched in groups of five clinics to ensure the required data anonymization. The upper five percentiles are omitted.

Table 3 presents descriptive statistics for clinics with and without physician exits. Panel A shows average levels of antibiotic prescribing per patient. Panel B shows averages for observable physician characteristics, and Panel C shows averages for clinic-level characteristics. Clinics with a physician exit are older on average, have a smaller share of female physicians, and a smaller average household size than never-treated clinics.[17] While treated clinics have fewer patients per physician, staff sizes are similar as in never-treated clinics, with 1.49 physicians on average compared to 1.42. The majority of clinics in both groups are operated by a single physician, 72.7% of clinic-years without and 69.9% with a physician exit. In our overall sample, 89.6% of clinics are operated by no more than 2 physicians and 99.0% by no more than 4 physicians. Notably, there are only small differences in antibiotic prescribing between treated and untreated clinics and patients.[18]

[17]A potential concern is that our variation in prescribing mechanically reflects patient reassignments from older to younger physicians. However, Figure 8 in Appendix D.2 indicates considerable variability in the age differences between the origin and destination clinics, despite an average shift toward younger physicians. We also do not assert that antibiotic prescribing practice styles are independent of physician age or other physician characteristics. Instead, we consider practice styles as encompassing age effects and investigate the role of physician age among other physician characteristics in our analysis of practice style correlates.

[18]In Appendix D.3 we show summary statistics for excluded clinics. Given that we exclude clinics with

Table 2: Descriptive statistics for treatment and comparison group patients

	Never-exposed to physician exit		**Exposed to physician exit**	
	Mean	SD	Mean	SD
A: Antibiotic prescriptions				
All antibiotics (All J01)	0.56	(1.28)	0.55	(1.26)
Penicillins (J01 C)	0.35	(0.82)	0.34	(0.81)
Second-line (J01 F, D, or M)	0.12	(0.46)	0.11	(0.45)
Other (J01 excluding J01 C, F, D, M)	0.09	(0.68)	0.09	(0.66)
B: Basic demographics and health				
Age	42.89	(24.02)	44.42	(23.33)
Female	0.54	(0.50)	0.55	(0.50)
Pregnant	0.02	(0.14)	0.02	(0.14)
Household size	2.61	(1.37)	2.58	(1.36)
Any visit to an emergency department	0.15	(0.36)	0.15	(0.35)
Any call to an emergency doctor	0.19	(0.39)	0.17	(0.38)
Any hospitalization for infection (ACSC)[a]	0.005	(0.07)	0.005	(0.07)
C: Family background and education				
Married couple	0.55	(0.50)	0.56	(0.50)
Cohabiting couple with children	0.07	(0.25)	0.06	(0.24)
Cohabiting couple without children	0.06	(0.25)	0.06	(0.24)
Single	0.32	(0.47)	0.31	(0.46)
First generation migrant (nordic)	0.01	(0.09)	0.01	(0.08)
First generation migrant (other country)	0.07	(0.25)	0.07	(0.25)
Second generation migrant	0.03	(0.17)	0.03	(0.17)
Missing education	0.21	(0.41)	0.18	(0.39)
School grade 7 to 10	0.26	(0.44)	0.28	(0.45)
High school or vocational training	0.32	(0.47)	0.33	(0.47)
Short higher education	0.03	(0.17)	0.03	(0.17)
Medium higher education	0.12	(0.33)	0.12	(0.32)
Long higher education	0.05	(0.22)	0.05	(0.22)
Phd education	0.003	(0.057)	0.003	(0.052)
No education	0.001	(0.030)	0.001	(0.030)
Total observations (patient-years)	6,224,823		1,565,085	

[a] Hospitalizations for acute ambulatory care-sensitive conditions (ACSC) commonly caused by bacterial and non-bacterial infections (see Appendix I.1 for a complete list of ICD-10 codes).

multiple long-term staff changes, out-of-sample clinics have more physicians and interns than in-sample clinics. However, antibiotic prescribing differs little between out-of-sample and in-sample clinics. In Appendix D.4, we also show summary statistics for patient-year observations which we drop during our sampling process, observing only small differences in antibiotic prescribing compared to in-sample observations.

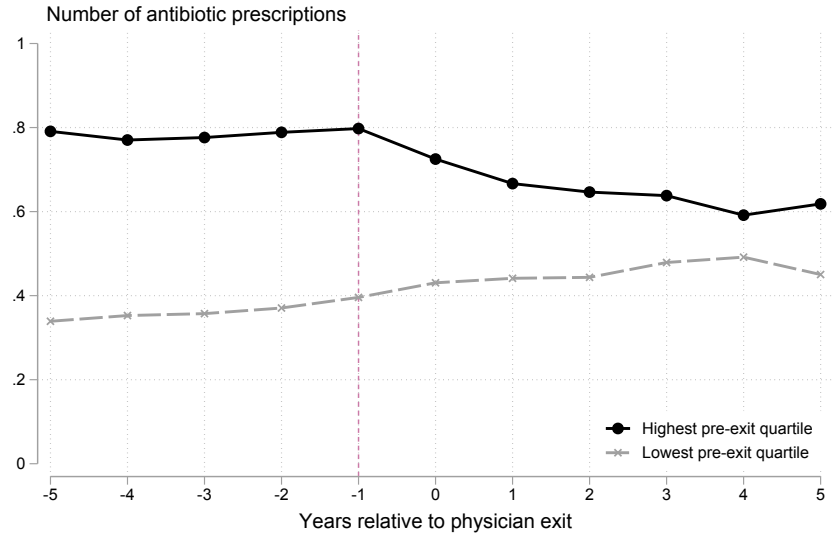
Table 3: Descriptive statistics for treatment and comparison group clinics

	Without physician exit		**With physician exit**	
	Mean	SD	Mean	SD
A: Average antibiotic prescriptions per patient				
All antibiotics (All J01)	0.55	(0.16)	0.54	(0.20)
Penicillins (J01 C)	0.34	(0.10)	0.34	(0.12)
Second-line (J01 F, D, or M)	0.12	(0.06)	0.11	(0.08)
Other (J01 excluding J01 C, F, D, M)	0.09	(0.04)	0.09	(0.06)
B: Average physician characteristics				
Age	55.41	(6.45)	59.28	(6.15)
Female	0.35	(0.43)	0.28	(0.38)
First generation migrant (nordic)	0.004	(0.050)	0.006	(0.070)
First generation migrant (other country)	0.03	(0.16)	0.03	(0.15)
Second generation migrant	0.01	(0.08)	0.01	(0.08)
Phd education	0.01	(0.07)	0.01	(0.07)
C: Size and equipment				
Number of physicians	1.42	(0.82)	1.49	(0.89)
Number of interns	0.22	(0.50)	0.15	(0.45)
Number of patients per physician	1987.81	(739.80)	1748.62	(852.01)
Diagnostic culture available	0.98	(0.15)	0.94	(0.24)
Diagnostic microscopy available	0.75	(0.43)	0.79	(0.41)
Telephone consultation available	1.000	(0.022)	0.999	(0.028)
Total observations (clinic-years)	4,023		1,254	

4.3 Shifts in prescribing

Figure 2 depicts average per-patient antibiotic prescribing to treated patients over years relative to the treatment, which is a physician exit. The figure illustrates that treated patients who were initially assigned to clinics with lower quartile average prescribing tend to consume more antibiotics after the physician exit. In contrast, treated patients assigned to clinics with upper quartile average prescribing tend to consume fewer antibiotics post-treatment. The figure demonstrates a reversal to the mean in antibiotic prescriptions, providing descriptive evidence that patients' antibiotic consumption is influenced by practice styles.

Figure 2: Antibiotic prescribing by clinics in high and low quartile of pre-exit prescribing



Notes: This figure shows the average number of antibiotic prescriptions per patient per year, relative to a physician exit, for two groups of clinics based on their pre-exit antibiotic prescribing: those in the upper quartile with a comparably high number of prescriptions, and those in the lower quartile with a comparably low number of prescriptions. Relative year -1 is the last pre-exit period, relative year 0 is a transitional period, and relative year 1 is the first post-exit period.

5 Empirical Strategy

To measure the causal treatment effect defined in Section 2, we first detail the assumptions required for identification and then outline how we estimate the parameters of interest.

5.1 Identification

We discuss the identification of provider effects in the model in Section 2 using the potential outcomes framework. We define $y_{it}(1) \equiv y_{ij't}$ as the potential antibiotic prescribing to patient i in year t when an exiting event has occurred, and $y_{it}(0) \equiv y_{ijt}$ as the potential prescribing when an exiting event has not occurred. These potential outcomes can be written as:

$$y_{it}(1) = \alpha_i + \delta_{j'} + x_{it}\beta + \epsilon_{it}$$

$$y_{it}(0) = \alpha_i + \delta_j + x_{it}\beta + \epsilon_{it}.$$

Thus, the difference in practice styles, $\delta_{j'} - \delta_j$, is equal to the difference $y_{it}(1) - y_{it}(0)$. To obtain a standardized measure of the importance of provider effects, denoted as θ, we scale $\delta_{j'} - \delta_j$ by the difference in physicians' observed mean prescribing, $\Delta_i = y_{j'} - y_j$. Hence, θ defines an average scaled treatment effect on the treated, where the treatment D_{it} corresponds to exposure to a physician exit:

$$\theta = \mathbb{E}\left[\frac{y_{it}(1) - y_{it}(0)}{\Delta_i} \mid D_{it} = 1\right]. \tag{3}$$

Identification of θ is based on a staggered difference-in-differences design, as described in Sun and Abraham (2021), because the timing of treatment onset - the year of a physician exit - can vary among treated patients. We define all patients with treatment onset in the same calendar year as a cohort, and denote by E_i the cohort that patient i belongs to, with realizations $e \in \{2006, ..., 2012, \infty\}$. The never-treated group of patients forms its own cohort, denoted by $e = \infty$.

We require the standard assumptions in difference-in-differences designs, namely parallel trends, no anticipation, and no attrition.[19] Additionally, we need to impose two further assumptions in our design. *Assumption 1* ensures that we can attribute a causal interpretation to θ as the measure of the role of provider effects on patient antibiotic consumption, while *Assumption 2* is required to identify θ as the correctly weighted average treatment effect on the treated in a difference-in-differences design with staggered treatment onset.

Assumption 1 Patients do not sort selectively to physicians based on antibiotic prescribing, $\mathbb{E}\left[\frac{y_{it}(1)-y_{it}(0)}{\Delta_i} \mid D_{it} = 1, \Delta_i\right] = \mathbb{E}\left[\frac{y_{it}(1)-y_{it}(0)}{\Delta_i} \mid D_{it} = 1\right] = \theta$. If for example patients with relatively high antibiotic consumption at low-prescribing pre-exit physicians systematically sort into high-prescribing post-exit physicians, we would underestimate the share of provider effects θ. We formally test for selective sorting based on observable predictors of patients' antibiotic consumption using a two-step estimation approach.

[19] We describe these assumptions for our setting in Appendix E.

Assumption 2 The average treatment effect on the treated over cohorts is homogeneous for all time periods and cohorts, $\mathbb{E}\left[\frac{y_{it}(1)-y_{it}(0)}{\Delta_i} \mid E_i = e, \Delta_i\right] = \mathbb{E}\left[\frac{y_{it}(1)-y_{it}(0)}{\Delta_i} \mid D_{it} = 1, \Delta_i\right] \forall\ t, e$. We impose this assumption in the main analysis but relax it along both dimensions to explore potential violations in robustness checks. First, in an event study specification, we allow treatment effects to differ by time relative to the physician exit. Second, we estimate cohort-specific treatment effects to account for heterogeneity in treatment effects across cohorts. This approach allows for treatment effects to differ between early-treated patients and later-treated patients. To obtain average treatment effects from the cohort-specific specification, we aggregate the cohort-specific treatment effects with weights depending on cohort size as proposed by Sun and Abraham (2021).

5.2 Estimation

Based on our identification strategy, we estimate the causal treatment effect using two-way patient and year fixed effects in a static and in an event study setup.

We first estimate a static specification:

$$y_{it} = \tilde{\alpha}_i + \theta \times D_{it} \times \hat{\Delta}_i + x_{it}\beta + \epsilon_{it}, \tag{4}$$

where we measure antibiotic prescribing y_{it} by the number of prescriptions. The patient fixed effects $\tilde{\alpha}_i = \alpha_i + \delta_j$ subsume the initial physicians' fixed effect δ_j because our approach identifies the difference in physician fixed effects $\delta_{j'} - \delta_j$. Hence, patient fixed effects α_i and the initial physician fixed effect δ_j are not separately identified. Our main coefficient of interest is θ, associated with the interaction between D_{it}, the indicator for the post-exit period, and $\hat{\Delta}_i$, the empirical estimate of the difference in mean prescribing between j and j'. In the baseline model, x_{it} includes calendar-time fixed effects x_t, the indicator D_{it}, and an indicator for the transitional year of the exit. Given that exogeneity holds, $\mathbb{E}[\epsilon_{it}|\tilde{\alpha}_i, D_{it} \times \hat{\Delta}_i, x_{it}] = 0$, based on the identifying assumptions in Subsection 5.1, the estimate of θ represents the

causal effect of provider effects on differences in antibiotic prescribing.

Second, we estimate an event study specification:

$$y_{it} = \tilde{\alpha}_i + \sum_{\substack{r=-5,\\ r\neq -1}}^{r=5} \theta_r \times I_r \times \hat{\Delta}_i + x_{it}\beta + \varepsilon_{it}, \tag{5}$$

where $r(i,t)$ defines the year relative to the exiting event, and $I_r = 1\{r(i,t) = r\}$ is an indicator that is one during relative year r. The omitted category is $r = -1$, the year before the exiting event. In the baseline specification, the vector of control variables x_{it} includes calendar year fixed effects x_t, the indicator D_{it} for the post-exit period, and relative year interactions outside of our effect window $I_r \times \hat{\Delta}_i$ with $r < -5, r > 5$. Under the identifying assumptions, the event study specification allows us to test for differential trends between the pre-exit antibiotic consumption of treated patients and the antibiotic consumption of never-treated patients, and it enables us to detect dynamic treatment effects.

For statistical inference, we bootstrap Equations (4) and (5) with 50 repetitions drawn at the patient level. Within each bootstrap repetition, we compute the leave-one-out estimator of mean prescribing, y_j, for each patient and construct the corresponding difference in mean prescribing for patients exposed to treatment, Δ_i. The bootstrapped standard errors account for estimation error in mean prescribing y_j and Δ_i.

6 Results

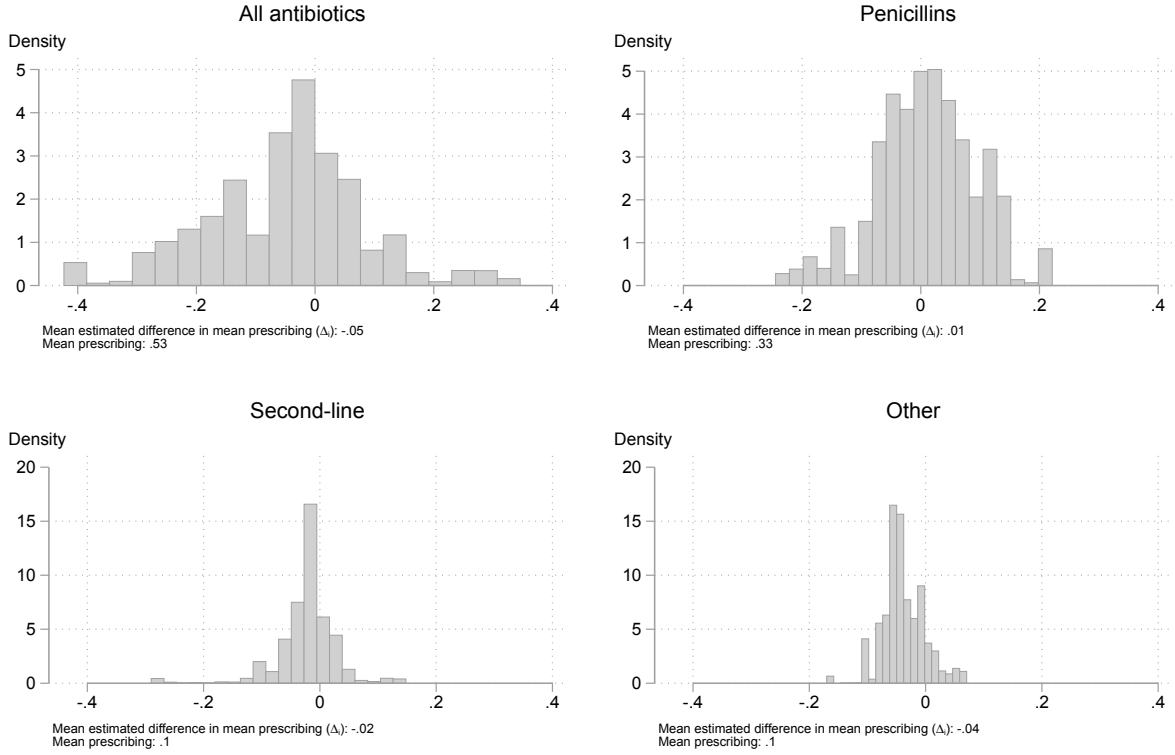
6.1 Provider effects in antibiotic prescribing

We estimate the role of provider effects in antibiotic consumption measured by the number of antibiotic prescriptions purchased by a patient in a given year. Our main parameter of interest is θ, the share of provider practice style differences that determines antibiotic consumption differences between primary care providers, associated with $\hat{\Delta}_i \times D_{it}$.

Figure 3 displays a histogram of estimates for Δ_i, the difference in the average number of

antibiotic prescriptions between providers assigned to treated patients. This figure highlights the striking variation in antibiotic prescribing among providers. For instance, a notable proportion of treated patients are reassigned to providers who prescribe, on average, more than 0.2 yearly antibiotic treatments above or below their original providers. Considering that patients receive an average of 0.53 antibiotic prescriptions per year, these observed prescribing differences among providers are economically meaningful.

Figure 3: Distribution of the difference in average antibiotic prescribing between treated patients' pre- and post-exit physicians ($\hat{\Delta}_i$)



Notes: To ensure the required data anonymization, the top and bottom 0.5% of values are winsorized, and values are bunched for groups of five patients with similar estimated mean difference in average prescribing.

Table 4 shows estimation results for the static baseline specification in Equation (4) for all antibiotics and by subcategories. Provider practice styles determine 49.4 percent of the differences in the number of all antibiotic prescriptions between providers.[20] Provider shares

[20]In Appendix F.1, we observe that our results are consistent when measuring antibiotic prescribing by the

in antibiotic prescribing differences are at around 43.5 percent when only penicillins are considered. In the case of second-line antibiotics, provider shares are highest at approximately 82.8 percent.[21] These results show provider effects play a substantial role in explaining variation in antibiotic consumption in primary care.

Table 4: Estimation results for the share of provider effects in antibiotic prescribing

	Number of prescriptions Two-way fixed effects estimation[a]			
	All antibiotics	Penicillins	Second-line	Other
$\hat{\Delta}_i \times D_{it}$	0.494***	0.435***	0.828***	0.485***
	(0.033)	(0.034)	(0.036)	(0.072)
Event dummies[b]	yes	yes	yes	yes
Observations	7,789,908	7,789,908	7,789,908	7,789,908
Groups (patients)	1,371,604	1,371,604	1,371,604	1,371,604

This table reports the average share of antibiotic prescribing differences between clinics that is attributable to provider practice style differences, the coefficient of $\Delta_i \times D_{it}$. The term Δ_i denotes the difference in mean antibiotic prescribing between patient i's assigned sets of physicians, and is estimated by $\hat{\Delta}_i$, the leave-one-out estimator of antibiotic prescribing based on untreated patients. The term D_{it} represents the post-treatment indicator. Standard errors are calculated using a bootstrap with 50 repetitions at the patient level. *** $p < 0.01$.

[a] Two-way fixed effects estimation with calendar year fixed effects and patient fixed effects.

[b] Event dummies include an indicator for treatment onset (relative year 0) and post-treatment.

Daily Defined Dose prescribed, except in the case of other antibiotics. The difference from our main results for this subcategory might be due to large variation in average Daily Defined Dose per prescription between antibiotic classes. Our main conclusions remain unchanged when we limit our analysis to single-physician clinics, as presented in Appendix F.2.

[21] In Appendix F.3, we estimate provider shares separately for macrolides, lincosamides, and streptogramins (J01 F), cephalosporins (J01 D), and quinolones (J01 M). They are largest for macrolides, lincosamides, and streptogramins, and quinolones. We also estimate provider shares for the group of second-line antibiotics excluding erythromycin (J01 FA01) and find similar effect sizes.

Next, we estimate provider shares in antibiotic prescribing for each year prior to or after the physician exit using the event study specification of Equation (5). This allows the share of provider effects to vary flexibly over time relative to treatment. Figure 4 presents the results, which demonstrate an absence of systematic pre-trends with high precision. Where statistically significant, pre-trends are economically negligible compared to the treatment effect estimates. The limited presence of pre-trends supports our identifying assumption that there is no anticipatory behavior prior to treatment onset. The event study figures also indicate that changes in antibiotic prescribing resulting from physician exits do not diminish in the years following the changes. Instead, effect sizes are highly persistent and suggest the main estimates are unlikely driven by systematic changes in patient health over time.

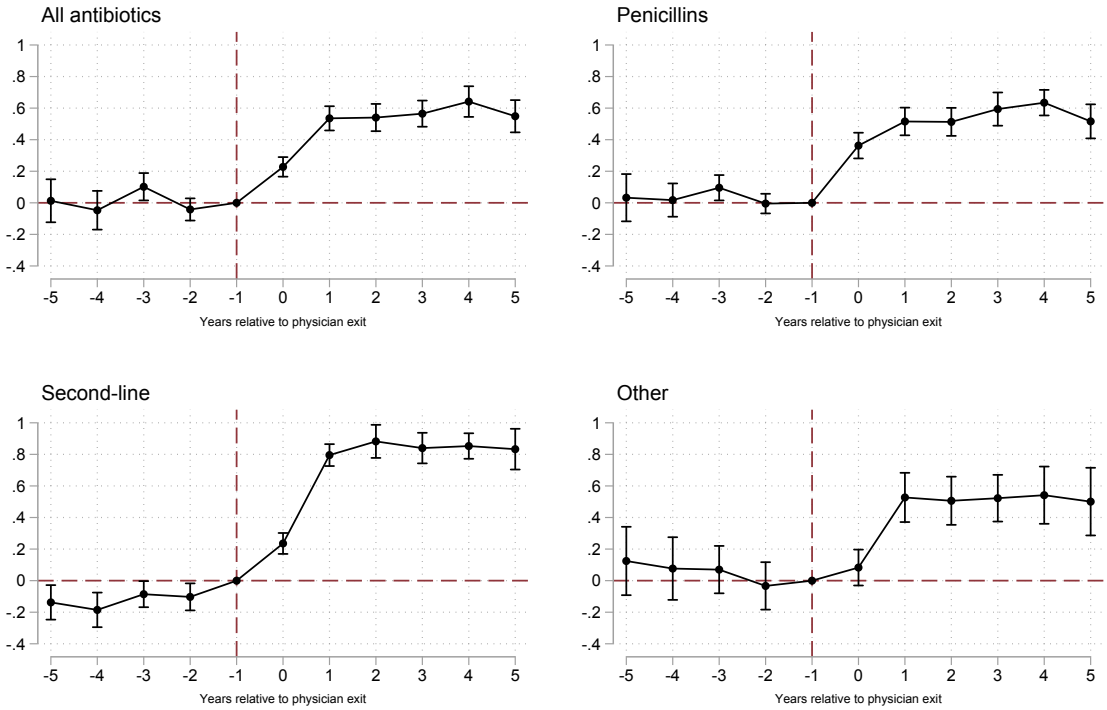
In summary, our findings reveal that provider effects account for roughly half of the differences in antibiotic prescribing between providers, and these differences reflect considerable spread in antibiotic consumption. However, the extent of provider effects varies across antibiotic subcategories. Specifically, our results indicate that the greatest leverage physicians have on antibiotic prescribing is for second-line drugs, even if they still exert substantial influence on penicillin prescriptions. This suggests that practice styles matter in particular for the composition of antibiotics prescribed. This finding is particularly relevant as the effects of antibiotic use on resistance vary by drug class, with broad-spectrum antibiotics having the most pronounced impact on resistance.

6.2 Sensitivity analyses

We investigate the robustness of our results by inspecting a number of alternative specifications.

First, we relax the parallel trends assumption to hold conditional on patient observable characteristics. We re-estimate Equations (4) and (5) to include time-varying observable patient characteristics, which account for changes in a patient's underlying health conditions that may impact the amount of antibiotics they require. However, we avoid including time-varying patient characteristics that are likely affected by a physician exit, as this could

Figure 4: Event study estimates of the share of provider effects



Notes: The figures display event study estimates for the share of antibiotic prescribing differences between clinics that is attributable to provider practice style differences. Estimations include patient fixed effects, calendar year fixed effects, and indicators for treatment onset and post-exit. Relative year -1 is the last pre-exit period, relative year 0 is a transitional period, and relative year 1 is the first post-exit period. Lines represent the 95% confidence intervals, with standard errors calculated using a bootstrap with 50 repetitions at the patient level.

interfere with our ability to identify the treatment effect. For example, a patient's diagnosed medical conditions may affect their antibiotic consumption but also depend directly on the physician's practice style. Such mediator variables would jeopardize identification of our treatment effect. To control for changes in health status, we include a quadratic function of age, pregnancy status, and emergency service utilization measured by any visit to the emergency department of a hospital and any claim at an on-call doctor.Additionally, we include the number of interns present for less than a year at a clinic as control variable in order to capture short-term differences in antibiotic prescribing behavior. The results are consistent and presented in Appendix G.[22]

[22]Table 16 in Appendix G displays the estimates for the static specification, and Figure 12 shows the results

Second, we allow for treatment heterogeneity by year of the physician exit. Equations (4) and (5) provide unbiased estimates of θ if the average share of provider effects θ is consistent across cohorts defined by year of treatment onset. However, if this assumption does not hold, the two-way fixed effects estimator for θ is a weighted average of relative time-specific provider effects, with negative weights possible (Goodman-Bacon 2021). We relax the treatment homogeneity assumption by estimating a cohort-saturated two-way fixed effects specification that accounts for cohort-relative time specific treatment effects. To estimate the average treatment effect on the treated, we aggregate cohort-relative time-specific treatment effects as suggested by Sun and Abraham (2021).[23] The interaction-weighted treatment effect estimates are similar to our main results and shown in in Appendix G.[24] The only exception is antibiotic prescribing for non-penicillin, non-second line antibiotics (Other), for which the estimated share of provider effects is lower when allowing for cohort heterogeneity.

6.3 Selective sorting into patient-physician reassignments

Selective sorting of patients to physicians after a physician exit could threaten our identification strategy. To investigate the presence of such sorting based on observable characteristics, we follow the two-step procedure outlined in Fadlon and Van Parys (2020).

In the first step, we estimate a prediction function of the number of antibiotic prescriptions using basic demographics and health variables (Panel B of Table 2) as well as family background and education (Panel C of Table 2). This step includes all observations from

for the dynamic specification.

[23] Specifically, we estimate the following specification:

$$y_{it} = \tilde{\alpha}_i + x_t + \sum_e \sum_r \theta_{e,r} \times I_e \times I_r \times \hat{\Delta}_i + \epsilon_{it},$$

where x_t represent the time fixed effects, cohort $e \in \{2006, ..., 2012, \infty\}$ defines the year in which a patient is exposed to physician exit, and the remaining notation follows prior discussion. The category $e = \infty$ characterizes the never-treated group, which is the omitted category. $\theta_{e,r}$ denotes cohort e-relative time r specific treatment effects. We derive relative time specific treatment effects $\hat{\theta}_r$ by aggregating cohort-relative time specific treatment effect estimates weighed by relative cohort size.

[24] Table 16 in Appendix G shows the average interaction-weighted treatment effect estimates, and Figure 12 displays relative time specific treatment effect estimates.

never-treated patients as well as treated patients before they are exposed to a physician exit. In the second step, we predict post-treatment prescriptions for treated patients after they are exposed to treatment. We regress predicted prescribing on the difference in average prescribing between the post- and pre-exit physicians that treated patients are assigned to. These second step regressions include fixed effects for the pre-exit set of physicians of treated patients and calendar years. We use the same bootstrap procedure as for the main analysis to compute standard errors. If patients sort to post-exit physicians based on observables and this sorting is systematically related to antibiotic prescribing, we would expect predicted prescribing based on observables to be correlated with differences in average prescribing between those physicians treated patients are assigned to.

Table 5 shows that the estimated relationship between predicted prescribing and the difference in average prescribing is small in magnitude or not statistically significant. For example, among patients with the same pre-exit physicians, if the post-exit number of antibiotic prescriptions is predicted to be higher by one unit based on observable patient characteristics, this is associated with the post-exit physicians' average number of prescriptions being systematically higher by only 0.0016. Although we cannot rule out the possibility of selective sorting on unobservable characteristics such as preferences, we believe that it is unlikely for patients to choose their primary care provider primarily based on antibiotic prescribing styles. This presumption is supported by the Danish institutional setting, where patients' information and available physician choices are limited. These circumstances make 'shopping' for high prescribers of antibiotics, for example, very difficult.

Table 5: Selective re-assignment of patients to physicians based on observable characteristics

	Difference in average antibiotic prescribing $(\hat{\Delta}_i)^a$			
	All antibiotics	Penicillins	Second-line	Other
	(1)	(2)	(3)	(4)
Predicted prescribing	0.0016**	0.0020***	0.0011	0.0005
	(0.0005)	(0.0004)	(0.001)	(0.0004)
Observations	413,663	413,663	413,663	413,663

This table reports the estimated relationship between antibiotic prescribing as predicted by patient observable characteristics, and the difference in average antibiotic prescribing between post- and pre-exit physicians for treated patients. A strong relationship would suggest that patients choose post-exit physicians based on observable characteristics that are predictive of antibiotic prescribing. We proceed in two steps. First, we predict post-treatment antibiotic prescribing to treated patients based on basic demographics, health, family background, education (as in Table 2), and including age squared. We use a linear prediction model trained on data from never-treated patients and treated patients prior to the physician exit. Second, we regress predicted prescribing on the difference in average prescribing between post- and pre-exit physicians assigned to treated patients. The second-step regressions include calendar year fixed effects and fixed effects for the pre-exit physicians, with observations on the patient-year level. Standard errors are calculated using a bootstrap with 50 repetitions at the patient level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[a] Δ_i denotes the difference in mean prescribing between patient i's assigned sets of physicians, and is estimated by $\hat{\Delta}_i$, the average prescribing to untreated patients.

7 Characterizing practice style heterogeneity

7.1 Correlates of provider practice styles

To investigate patterns in practice style heterogeneity, we first report the correlations between antibiotic prescribing styles and observable physician and clinic characteristics. Physician characteristics include age, post-graduate training, gender, and migration background.[25] Clinic characteristics include the availability of point-of-care diagnostics and practice size.[26]

We compute correlations between prescribing styles and physician and clinic characteristics using a two-step procedure. First, we estimate the difference in prescribing styles between providers to which treated patients are assigned *for each pair of physicians*. We thus estimate provider effects separately for each pair of origin-destination physicians, instead of scaling and aggregating the effects. Second, we regress our estimated provider effects

[25] For clinics with multiple physicians, we compute averages over physicians.

[26] Appendix H.1 describes the variables in detail.

onto differences in standardized observable characteristics between pairs of physicians.[27] We explore the relationships between provider practice styles and physician observables using two estimation approaches: bivariate ordinary least squares (OLS), which regresses prescribing style differences on single physician observables, and multivariate post-LASSO OLS, which additionally accounts for correlation between observed characteristics (Belloni et al. 2013).[28]

Figure 5 shows correlates of antibiotic prescribing practice style differences, where each row represents the association between a one standard deviation increase in the observed characteristic and the corresponding change in prescribing styles. The left columns display coefficients from bivariate OLS regressions, while the right columns show coefficients from multivariate OLS regressions using variables selected by a first-stage LASSO regression.

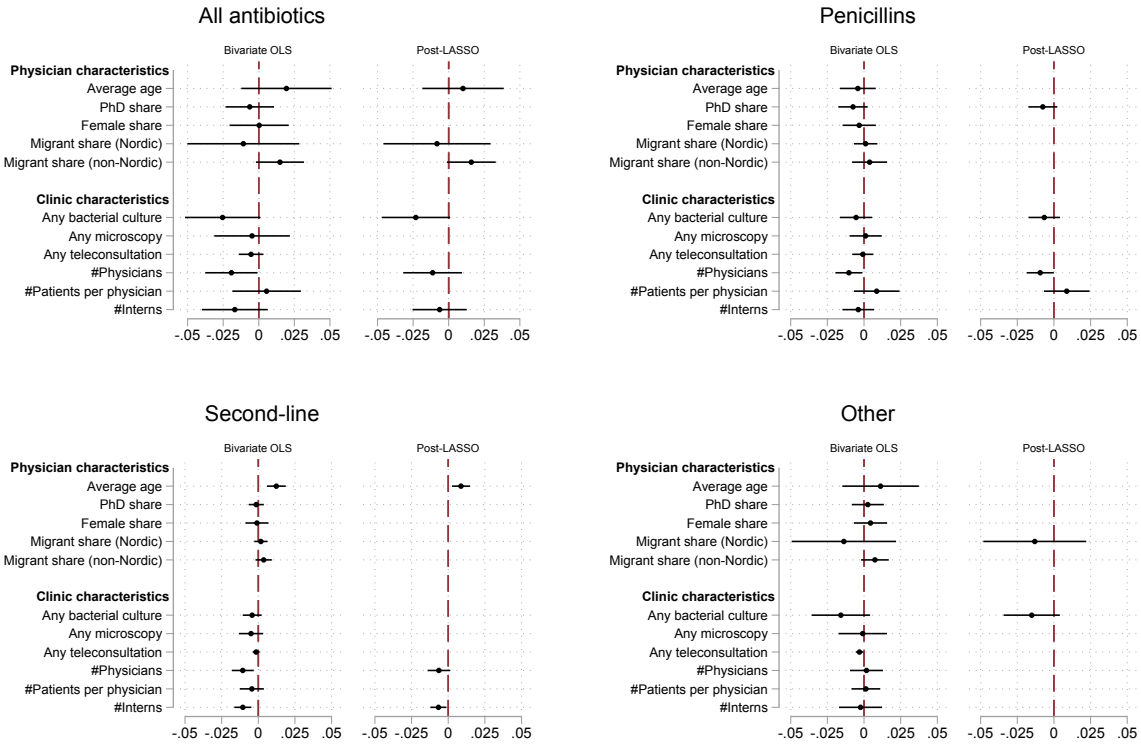
On the physician-level, higher intensity prescribing styles are positively correlated with age and non-Nordic migration background and, to a lesser degree, negatively correlated with PhD training. These correlations indicate that generational differences, a medical education in Nordic countries, and a PhD education may contribute to low-prescribing styles among physicians.

On the clinic level, higher intensity prescribing styles are negatively correlated with staff size, which could be explained by differences in weighing the private benefit versus the social cost of antibiotic prescribing. While an antibiotic prescription may provide a private benefit of increasing the chances of recovery for a patient, it also comes with the externality of contributing to antibiotic resistance in the community. Larger clinics with less pronounced patient-physician relationships may place lower weight on the private benefit of antibiotics compared to smaller clinics. Larger clinics may also benefit from information sharing and collaborative efforts to maintain high efficiency of care. Finally, clinics with a wider range of diagnostic tools may be better equipped to target antibiotic prescriptions more effectively, which is also reflected in the negative correlation between prescribing intensity and the availability of bacterial culture diagnostics.

[27] More details on our estimation procedure can be found in Appendix H.2.

[28] We find similar results using a linear fixed effects regression approach as shown in Appendix H.3.

Figure 5: Correlates of provider practice style differences in antibiotic prescribing



Notes: The figure presents estimated changes in antibiotic prescribing styles associated with a one standard deviation increase in physician or clinic characteristics using bivariate OLS (left) and post-LASSO OLS (right). We obtain these estimates by regressing the estimated difference in antibiotic prescribing practice style on differences in observed characteristics between pairs of physicians that treated patients are assigned to. For the post-LASSO estimates, we first run a LASSO regression on the full set of characteristics, with the penalty level selected via 10-fold cross validation to minimize mean squared error, and then perform OLS regression using only the set of variables selected by the LASSO regression. Missing coefficients indicate that a variable has not been selected in the LASSO regression. Lines represent the the 95% confidence intervals, with standard errors calculated using a parametric bootstrap with 50 repetitions at the patient level to draw differences in prescribing practice styles. Physician and clinic characteristics are standardized to have mean 0 and standard deviation 1 prior to differencing.

7.2 Quality of care

So far, we have focused on identifying heterogeneity in provider practice styles. Heterogeneity in antibiotic prescribing may be desirable if it generates the best health outcomes. However, if it reflects antibiotic use that does not prevent adverse patient health outcomes, then it would be prudent to reduce practice style variation by lowering antibiotic prescribing intensity. To inspect quality differences in practice styles, we use a difference-in-differences approach

measuring the link between prescribing intensity and quality of care. The explanatory variable of interest is the interaction between exposure to a physician exit and the estimated difference in physicians' prescribing styles, that is, origin-destination physician-specific provider effects.[29] This analysis answers the following question: When patients are exogenously reassigned to physicians with more intense antibiotic prescribing styles, do the additional prescriptions they receive correspond to higher standards of care?

We measure quality of care in several ways. First, we consider the number of follow-up prescriptions after initial antibiotic treatments. A follow-up prescription that deviates from the initial antibiotic used indicates a low-quality initial treatment decision as antibiotic treatment may have failed, for example due to antibiotic resistance, a mis-match with the bacterial cause of infection, or side effects that may have occurred. We define a follow-up prescription as an antibiotic from a different ATC 4 subgroup prescribed within seven days of an initial prescription.[30] Second, prescriptions without diagnostic testing indicate a lack of reliance on available diagnostic tools such as bacterial culture, microscopic examination, or rapid tests. Such diagnostics can allow for better targeting and efficient use of antibiotics. To construct this measure, the number of all antibiotic prescriptions without use of a diagnostic, we link each clinic's weekly claims for diagnostic tools to prescriptions. Finally, we investigate whether higher prescribing physicians manage patient health better as measured by ambulatory care sensitive conditions (ACSC). ACSCs are indications that are potentially preventable under sufficient primary care and are commonly used to measure quality of care (e.g. World Health Organization Regional Office for Europe 2016). We consider acute ACSC that can be caused by bacterial infections: Skin and soft tissue infections, ear, nose and throat infections, perforated or bleeding ulcer, urinary tract infections, and pneumonia.[31]

Figure 6 shows regression coefficients, representing the estimated changes in the outcome associated with an increase in antibiotic prescribing style by one prescription per patient per

[29]We discuss our estimation procedure in detail in Appendix I.2.

[30]The ATC 4 level segments pharmaceuticals into chemical-therapeutic-pharmacological subgroups.

[31]We provide details on all outcome variables in Appendix I.1, including the claim and ICD-10 codes used to construct these variables as well as basic summary statistics.

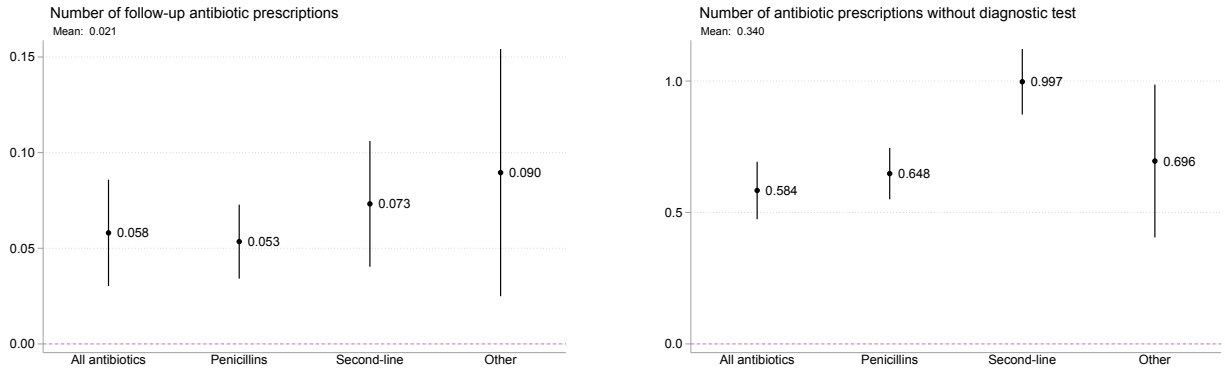
year, for all antibiotics as well as broken down by antibiotic subcategories.[32] Given an average share of 0.021 out of all prescriptions, the coefficient estimates between 0.053 and 0.09 in Figure 6(a) show that follow-up prescriptions are strongly positively associated with higher antibiotic prescribing intensity. For all antibiotics, exposure to a one-prescription more intense prescribing style corresponds to an almost threefold increase of follow-up prescriptions, which could be avoided by higher-quality initial treatment. In a similar vein, patients receive 0.584 more antibiotic prescriptions without any diagnostic test, nearly two times the mean value of 0.34, when exposed to a prescribing style of one more antibiotic prescription per patient and year. This increase in prescriptions without diagnostic tests is particularly prominent among high prescribers of second-line antibiotics, with a coefficient of 0.997. Overall, our findings do not support the hypothesis that excess antibiotic use by patients assigned to high-intensity prescribing physicians is the result of judicious decision-making.

Turning to health outcomes, the results in Figure 6b show no significant negative association between antibiotic prescribing intensity and hospitalizations. On the contrary, more adverse health events occur when patients are exposed to more intense penicillin prescribing styles. The point estimate of 0.0039 indicates this positive association is sizable given an average infection-related ACSC hospitalization rate of 0.0052. One potential explanation for this finding is that viral infections, particularly upper respiratory tract infections, are common cause for ineffective antibiotic treatment (Fleming-Dutra et al. 2016). As penicillins make up the majority of antibiotic prescriptions for these infections, patients assigned to physicians who prescribe them more frequently may be more likely to be hospitalized if their physicians' antibiotic prescribing crowds out alternative, effective treatments. In Appendix I.4 we show that the positive association between hospitalization rates and penicillin prescribing is indeed driven by upper respiratory infections in the ear, nose, and throat.[33]

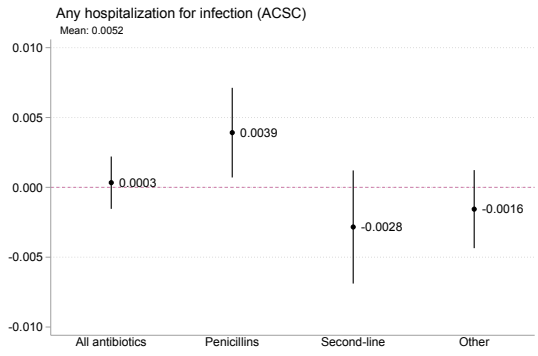
[32]We present sensitivity results for an extended set of control variables in Appendix I.3. The estimates are largely unchanged compared to our main results.

[33]Separate estimates for each ACSC in Appendix I.4 show that the results for penicillin prescribing are driven by hospitalizations for ear, nose, and throat infections. We observe a weak negative link between second-line antibiotic prescribing style and hospitalizations for ear, nose, and throat infections, but these associations do not translate into fewer overall hospitalizations.

Figure 6: Quality of care and antibiotic prescribing intensity



(a) Low quality prescribing



(b) Patient health

Notes: The figure shows the estimated changes in quality of care associated with a practice style of one additional antibiotic prescription, based on patient-year level regressions that include patient fixed effects, calendar year fixed effects, and indicators for treatment onset and post-exit. We consider an increase by one overall antibiotic prescription, as well as separately one more penicillin, second-line, or other antibiotic prescription. Figure 6a shows the relation between higher antibiotic prescribing intensity and low quality prescribing, measured by follow-up antibiotic prescriptions within seven days after an initial prescription of a different ATC 4 class (left), and prescriptions without any claim for diagnostic tests (right). Figure 6b shows the change in patient health associated with higher antibiotic prescribing intensity, measured by the propensity for any hospitalization for an infection-related ambulatory care sensitive condition (ACSC). We estimate changes in antibiotic prescribing styles as separate provider effects for each pair of physicians among treated patients, controlling for patient fixed effects, calendar year fixed effects, and indicators for treatment onset and post-exit. Lines represent the 95% confidence intervals, with standard errors based on a parametric bootstrap with 50 repetitions at the patient level to draw differences in prescribing practice styles.

8 Conclusion

Identifying who drives antibiotic use, and to what extent, is fundamental for the development of policies aimed at curbing inefficient antibiotic consumption. We fill this gap by measuring the share of differences in antibiotic use that can be attributed to physicians in primary care. To separate provider effects from patient-specific factors, we leverage exogenous reassignments of patients to physicians due to physician exits.

Provider effects determine antibiotic use to a substantial degree. In our setting, harmonizing provider practice styles would reduce differences in antibiotic prescriptions in primary care by one half. Notably, we find potential reductions in differences across clinics as large as 82.8% for macrolides, lincosamides and streptogramins, cephalosporins, and quinolones. These antibiotics are considered second-line treatments as they are more likely to cause antibiotic resistance and the evidence on clinical benefits is limited.

Our findings suggest that prescribing intensity is positively correlated with physician age and migration background, and negatively correlated with diagnostic availability and staff size, indicating a role for developments in medical education over time, the use of diagnostic tools, and benefits of team-work and scale. Finally, our results do not support the notion that high prescribers ensure higher quality of care and better patient health outcomes. High-prescribing physicians appear to require more follow-up treatments, prescribe frequently without using diagnostic tools, and do not see lower avoidable infection-related hospitalization rates.

Our study reveals that reducing overall and, importantly, second-line antibiotic consumption may be achieved by targeting individual physicians without impairing patient health. Further research is urgently needed to investigate the specific nature of practice styles associated with varying intensities of antibiotic prescribing, including interactions with heterogeneous patient needs, for the design of effective policies. Measuring the implications of practice styles for health outcomes is key as to ensure that policies lead to efficiency gains and avoid unintended adverse effects.

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A Model of prescribing and practice style differences – Details

We characterize antibiotic prescribing in a stylized model that follows Finkelstein et al. (2016). We assume that patient i's utility from consuming an amount of antibiotics y at point t is determined as follows:

$$u(y|\alpha_i, h_{it}) = \alpha_i y - \frac{1}{2}(y - h_{it})^2,$$

where α_i denotes individual time-invariant factors and h_{it} denotes patient health, and patient utility is additively separable in patient-specific factors and time-varying health. The optimal level of care patients would choose under full information is given by $y^*_{ijt} = \text{argmax}_y \, \tilde{u}_j(y|\alpha_i, h_{it}) = \alpha_i + h_{it}$. The individual time-invariant factors α_i absorb patient-level drivers of antibiotic consumption that remain fixed over time including, for example, preferences for higher antibiotic consumption or location-specific effects. A higher demand for antibiotics due to time-invariant patient-level drivers is represented by a higher value of α_i. A higher demand for antibiotics due to time-varying patient health is represented by a higher value of h_{it}, which implies worse health.

Each patient i in each year t is matched to a provider, which we denote by a set of physicians j. Physicians j make antibiotic prescribing decisions on account of their patients such that the utility from treating patients is maximized, but also taking into account physicians' personal preferences and costs. We operationalize the physicians' utility as follows:

$$\tilde{u}_j(y|\alpha_i, h_{it}) = u(y|\alpha_i, h_{it}) + (\delta_j - c_{jt})y,$$

where δ_j denotes j's prescribing practice style and c_{jt} denotes time-varying clinic characteristics. Physicians' utility $\tilde{u}_j(y|\alpha_i, h_{it})$ thus captures differences between prescribing decisions that

arise due to heterogeneity in the time-invariant provider practice styles (differences in δ_j) and time-varying clinic characteristics that affect the cost of antibiotic prescribing (differences in c_{jt}). We assume that time-varying and time-invariant factors are additively separable.

Finally, Equation 1 is obtained by maximizing physicians' utility:

$$\tilde{u}_j(y|\alpha_i, h_{it}) = \alpha_i y - \frac{1}{2}(y - h_{it})^2 + (\delta_j - c_{jt})y.$$

In denoting Equation 1, we subsume time-varying patient health h_{it} and time-varying clinic characteristics c_{jt} into a vector of time-varying characteristics x_{it}.

B Number of observations

Table 6 provides an overview over the number of clinics, patients, and observations on the patient-year level in our sample by treatment status.

Our sample includes patients at 556 never-treated clinics, as well as patients at 242 clinics that are exposed to any type of physician exit within the period of 2005 to 2012. The majority of physician exits leads to the closure of a clinic.

Note that never-treated patients can be assigned to a treated clinic, for example if they are assigned to that clinic strictly after the physician exit has already occurred.

Table 6: Number of observations

	Clinics	**Patients assigned to clinics**	**Observations**
Never-treated	563	1,103,672	6,263,693
Treated	242	329,414	1,526,215
Clinic closure	211	221,045	882,718
No clinic closure	31	108,369	643,497
Total	805	1,371,604[a]	7,789,908

[a] The deviation in the total number of patients is due to patients being observed at two clinics when they are exposed to physician exit.

C Definition of scaling factor

Our scaling factor is based on the difference in mean prescribing to untreated patients rather than the difference in unconditional mean prescribing as used for example by Fadlon and Van Parys (2020). We thus prevent that our estimates of the share of provider effects in antibiotic prescribing differences become affected by the share of treated patients who cause an overlap in patient pools.

To see the effect of the share of treated patients in a simplified setting, let all treated patients change from physicians j to j' and ignore time-varying characteristics x_{it}. Denote average patient effects in the pool of treated patients assigned to physician j by $\alpha^j = \mathbb{E}[\alpha_i | j(i) = j, D_{it} = 0]$ and note that these patients are not-yet-treated. Average patient effects in the pool of never-treated patients assigned to physician j' are $\alpha^{j'} = \mathbb{E}[\alpha_i | j(i) = j', D_{it} = 0]$. Patient pools treated by j and j' prior to the physician exit differ arbitrarily, $\alpha^j \neq \alpha^{j'}$. Mean prescribing is determined by $\mathbb{E}[y_{ijt}] = \alpha^j + \delta_j$.

Unconditional mean prescribing can be written as a weighted sum of mean prescribing to never-treated or not-yet-treated patients ($D_{it} = 0$) and mean prescribing to patients exposed to physician exit ($D_{it} = 1$). By construction, all patients assigned to j are not yet treated, but patients assigned to j' are either never-treated or exposed to physician exit. The difference in unconditional mean prescribing is $\tilde{\Delta}_i = \{w_D \mathbb{E}[y_{ij't} | D_{it} = 1] + (1 - w_D)\mathbb{E}[y_{ij't} | D_{it} = 0]\} - \mathbb{E}[y_{ijt} | D_{it} = 0]$, where $w_D \in [0, 1]$ denotes the proportion of patients of j' who changed from j to j'.

Because patient effects are fixed over time, only provider effects change once patients are exposed to physician exit. Mean prescribing to the pool of treated patients pre-exit is $\mathbb{E}[y_{ijt} | D_{it} = 0] = \alpha^j + \delta_j$, and it is $\mathbb{E}[y_{ij't} | D_{it} = 1] = \alpha^j + \delta_{j'}$ post-exit. Mean prescribing to the never-treated patient pool is always $\mathbb{E}[y_{ij't} | D_{it} = 0] = \alpha^j + \delta_{j'}$. The difference in unconditional mean prescribing can now be written as follows: $\tilde{\Delta}_i = (\delta_{j'} - \delta_j) + (1 - w_D)(\alpha^{j'} - \alpha^j)$. Scaling provider effects $\delta_{j'} - \delta_j$ by $\tilde{\Delta}_i$ implies that provider effects are weighted more the larger the proportion of treated patients w_D for a given difference in patient pools $\alpha^{j'} - \alpha^j$.

D Further descriptives

D.1 Distribution of antibiotic prescribing over clinics

Figure 7 shows the distribution of average antibiotic prescribing per patient over general practice clinics in Denmark for each year of our sample period from 2005 to 2012. While average prescribing in Denmark is low, there is substantial and persistent heterogeneity between clinics.

D.2 Physician age

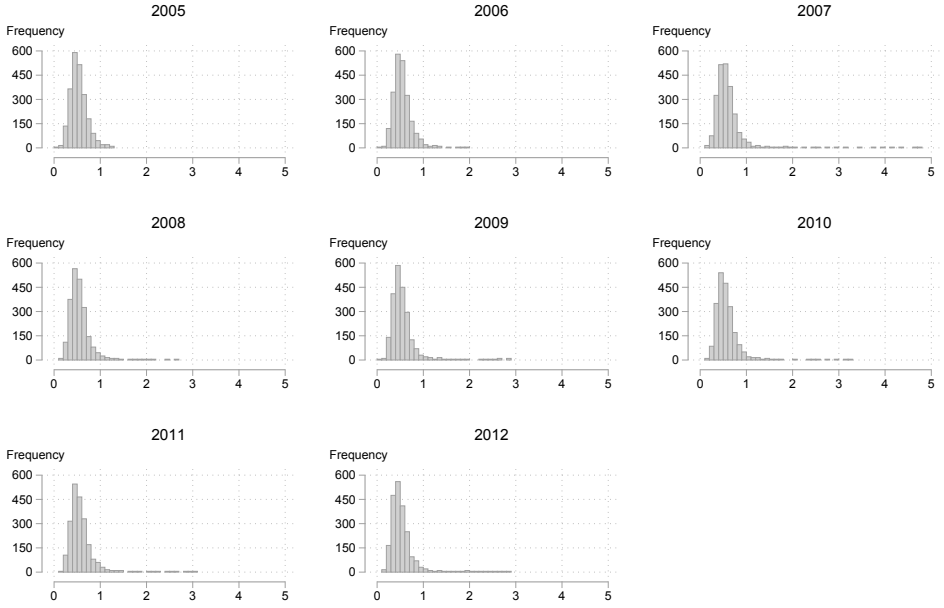
Figure 8 shows that, while the majority of reassignments for treated patients is from older to younger physicians, we also observe reassignments from younger to older physicians. Moreover, on average the age difference between physicians is not substantial.

D.3 Summary statistics for out-of-sample clinics and patients

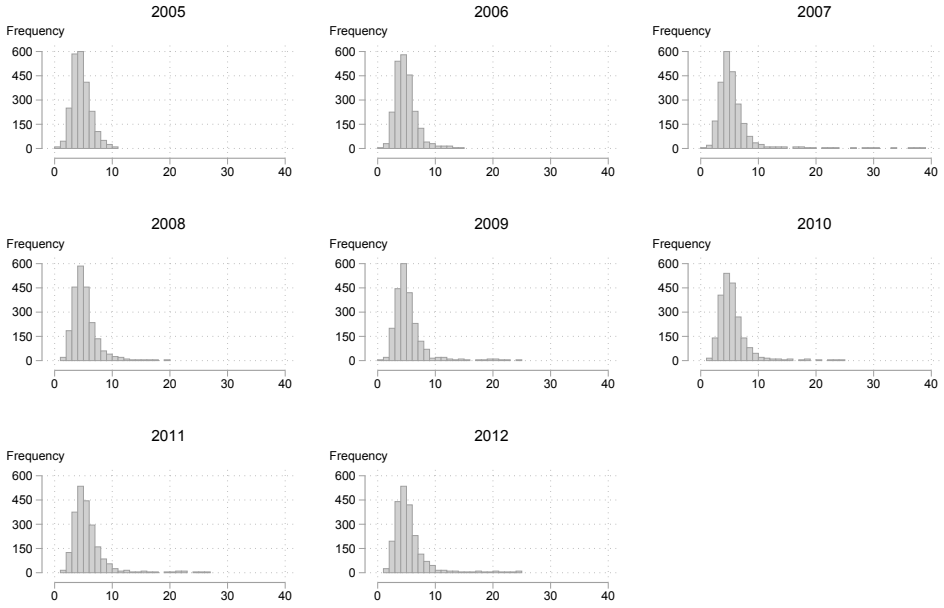
We compute summary statistics for characteristics in out-of-sample clinics which are listed in the Danish registry of clinics but which we have excluded from our analyses, as well as for patients assigned to those clinics. In particular, these are general practice clinics that underwent physician entries or multiple long-term staff changes over our period of observation from 2005 to 2012.

Table 7 shows summary statistics for patient-year observations and Table 8 shows average clinic-level characteristics. Most noticeably, staff sizes for out-of-sample clinics are larger than for in-sample clinics. This is not surprising, as larger clinics may have more fluctuation in staff. Moreover, patients assigned to out-of-sample clinics tend to be younger. However, antibiotic prescribing does not differ substantially from our main sample.

Figure 7: Distribution of average antibiotic prescribing per patient over general practice clinics



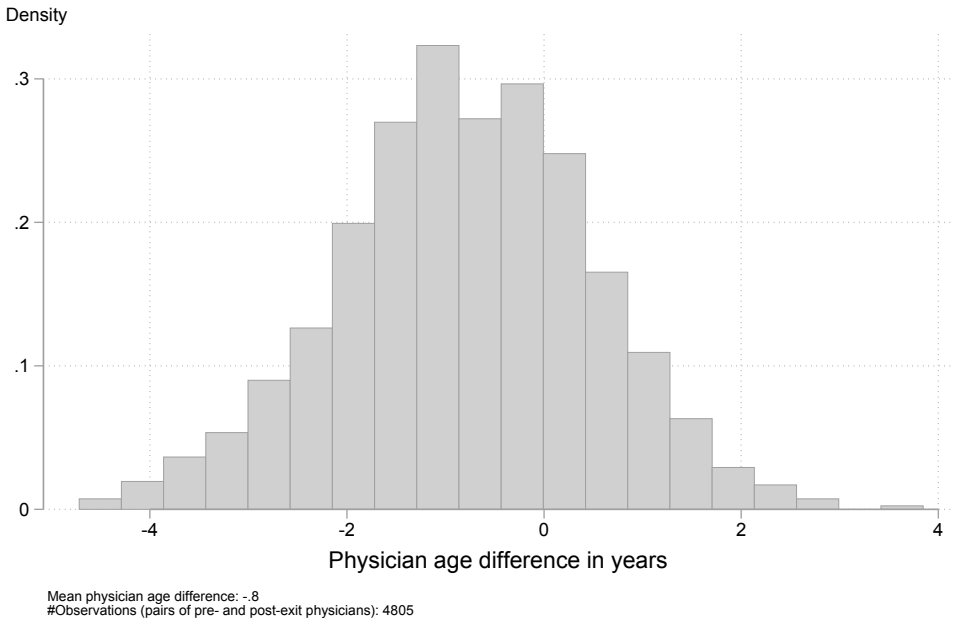
(a) Number of antibiotic prescriptions (levels)



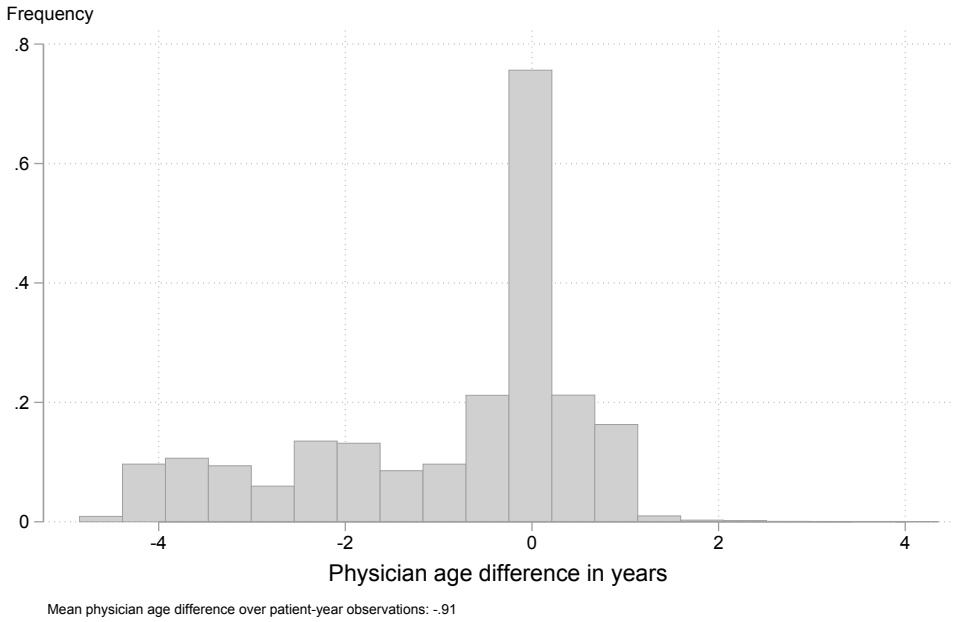
(b) Daily Defined Dose (levels)

Notes: Average antibiotic prescriptions dispensed per patient and year at the clinic-level. Bunched in groups of five clinics to ensure the required data anonymization. The upper five percentiles are omitted.

Figure 8: Difference in average age between post- and pre-exit physicians



(a) Distribution of pairs of pre- and post-exit physicians



(b) Distribution of treated patients weighted by years observed

Notes: In Figure 8a, values are bunched for groups of five physicians with similar age difference due to data anonymization. In Figure 8b, values are bunched for groups of five patient-year observations.

D.4 Summary statistics for excluded patient-year observations

We further compute summary statistics for observations of patients in our sample but which we have excluded from the final panel of patient-years. We exclude patient-year observations in order to ensure that any switch in a patient's general practice clinic is associated with the treatment, a physician exit, and we exclude observations when treated patients are assigned to out-of-sample clinics. For a subset of never-treated and treated patients in our sample, we thus drop observations and end up with an unbalanced panel.

Table 9 shows averages in observations which we drop for in-sample patients. In the case of never-treated patients, most noticeably the average age is lower than in our main sample. We might observe a lower average age because these observations are dropped when never-treated patients switch clinics for reasons unrelated to a physician exit as we then only keep observations at the modal clinic. Presumably, switching clinics is more common among younger patients, who could for example be more likely to move geographically or be more selective about choosing their physicians. In the case of treated patients, average characteristics are similar to the main sample. In both cases, antibiotic prescribing in the excluded observations does not differ substantially from our main sample, alleviating concerns about selective attrition based on antibiotic prescribing.

Table 7: Summary statistics for out-of-sample observations

	Out of sample	
	Mean	SD
A: Number of antibiotic prescriptions		
All antibiotics (All J01)	0.54	(1.24)
Penicillins (J01 C)	0.34	(0.80)
Second-line (J01 F, D, or M)	0.11	(0.44)
Other (J01 excluding J01 C, F, D, M)	0.09	(0.64)
B: Basic demographics and health		
Age	41.21	(23.53)
Female	0.54	(0.50)
Pregnant	0.02	(0.15)
Household size	2.61	(1.37)
Any visit to an emergency department	0.15	(0.36)
Any call to an emergency doctor	0.18	(0.39)
Any hospitalization for infection (ACSC)[a]	0.005	(0.069)
C: Family background and education		
Married couple	0.54	(0.50)
Cohabiting couple with children	0.07	(0.25)
Cohabiting couple without children	0.07	(0.26)
Single	0.32	(0.47)
First generation migrant (nordic)	0.01	(0.08)
First generation migrant (other country)	0.06	(0.24)
Second generation migrant	0.03	(0.16)
Missing education	0.21	(0.40)
School grade 7 to 10	0.27	(0.47)
High school or vocational training	0.32	(0.47)
Short higher education	0.03	(0.17)
Medium higher education	0.12	(0.32)
Long higher education	0.05	(0.21)
Phd education	0.003	(0.052)
No education	0.001	(0.029)
Total observations (patient-years)	15,691,250	

[a] Hospitalizations for acute ambulatory care-sensitive conditions (ACSC) commonly caused by bacterial and non-bacterial infections considered (see Appendix I for a complete list of ICD-10 codes). Referrals from general practitioners and delayed internal hospital referrals are excluded.

Table 8: Summary statistics for out-of-sample clinics

	Out of sample	
	Mean	SD
A: Average number of antibiotic prescriptions per patient		
All antibiotics (All J01)	0.54	(0.16)
Penicillins (J01 C)	0.33	(0.10)
Second-line (J01 F, D, or M)	0.11	(0.06)
Other (J01 excluding J01 C, F, D, M)	0.09	(0.04)
B: Average physician characteristics		
Age	55.63	(6.31)
Female	0.36	(0.39)
First generation migrant (nordic)	0.01	(0.06)
First generation migrant (other country)	0.03	(0.15)
Second generation migrant	0.01	(0.07)
Phd education	0.01	(0.08)
C: Size and equipment		
Number of physicians	1.88	(1.36)
Number of interns	0.25	(0.53)
Number of patients per physician	1878.69	(781.37)
Diagnostic culture available	0.98	(0.16)
Diagnostic microscopy available	0.80	(0.40)
Telephone consultation available	0.999	(0.027)
Total observations (clinic-years)	6,853	

Table 9: Summary statistics for excluded observations of in-sample patients

	Never-exposed to physician exit		Exposed to physician exit	
	Mean	SD	Mean	SD
A: Number of antibiotic prescriptions				
All antibiotics (All J01)	0.53	(1.21)	0.52	(1.31)
Penicillins (J01 C)	0.34	(0.79)	0.33	(0.80)
Second-line (J01 F, D, or M)	0.10	(0.43)	0.09	(0.40)
Other (J01 excluding J01 C, F, D, M)	0.09	(0.61)	0.10	(0.80)
B: Basic demographics and health				
Age	36.42	(21.67)	43.30	(23.22)
Female	0.57	(0.49)	0.58	(0.49)
Pregnant	0.04	(0.20)	0.03	(0.17)
Household size	2.56	(1.37)	2.56	(1.36)
Any visit to an emergency department	0.16	(0.37)	0.15	(0.36)
Any call to an emergency doctor	0.22	(0.41)	0.22	(0.41)
Any hospitalization for infection (ACSC)[a]	0.005	(0.073)	0.005	(0.073)
C: Family background and education				
Married couple	0.45	(0.50)	0.52	(0.50)
Cohabiting couple with children	0.08	(0.26)	0.06	(0.24)
Cohabiting couple without children	0.11	(0.31)	0.08	(0.27)
Single	0.36	(0.48)	0.34	(0.47)
First generation migrant (nordic)	0.01	(0.09)	0.01	(0.08)
First generation migrant (other country)	0.07	(0.26)	0.07	(0.25)
Second generation migrant	0.03	(0.17)	0.03	(0.17)
Missing education	0.20	(0.40)	0.17	(0.38)
School grade 7 to 10	0.25	(0.43)	0.26	(0.44)
High school or vocational training	0.32	(0.47)	0.33	(0.47)
Short higher education	0.03	(0.17)	0.03	(0.18)
Medium higher education	0.13	(0.33)	0.13	(0.33)
Long higher education	0.06	(0.24)	0.06	(0.24)
Phd education	0.003	(0.054)	0.003	(0.059)
No education	0.001	(0.030)	0.001	(0.040)
Total observations (patient-years)	245,030		68,039	

[a] Hospitalizations for acute ambulatory care-sensitive conditions (ACSC) commonly caused by bacterial and non-bacterial infections (see Appendix I for a complete list of ICD-10 codes). Referrals from general practitioners and delayed internal hospital referrals are excluded.

E Staggered difference-in-differences assumptions

Assumptions 3-5 are standard in a staggered difference-in-differences design and sufficient to identify $\delta_{j'} - \delta_j$.

Assumption 3 The potential outcome under no exposure to exit follows parallel trends, $\mathbb{E}[y_{it'}(0) - y_{it}(0) \mid E_i = e] = \mathbb{E}[y_{it'}(0) - y_{it}(0)] \; \forall \; t, t'$. This assumption requires that, were it not for the physician exit, antibiotic prescribing to treated patients would have followed the same trend as prescribing to untreated patients. The parallel trends assumption implies that any change in prescribing to a treated patient i after treatment onset can be attributed to the physician exit, rather than to underlying differences in trends between cohorts, including the never-treated group. As the timing of physician exits is arguably exogenous to underlying patient trends in antibiotic prescribing, we believe this assumption to be plausible. In a sensitivity analysis, we relax the parallel trends assumption to hold conditional on time-varying patient characteristics.

Assumption 4 Patients do not change their antibiotic consumption in anticipation of a physician exit, $\mathbb{E}[y_{it}(1) - y_{it}(0) \mid E_i = e] = 0 \; \forall \; t < e$. This assumption requires that treated patients do not engage in anticipatory behavior regarding their antibiotic consumption prior to being exposed to the physician exit. If this assumption holds for all pre-treatment periods, treated patients do not exhibit pre-trends in antibiotic consumption. To test this assumption, in an event study specification we allow for pre-trends that differ between treated and never-treated patients.

Assumption 5 Attrition of patients from our panel of patient-calendar year observations is independent of potential outcomes. Our panel is unbalanced as some patients are unobserved in the beginning or the end of the sample period, their assigned general practice clinic is not matched to our sample of clinics, or they change their clinic without being exposed to physician exit. Absence of selective attrition requires that patients do not leave our panel

systematically with regard to their potential antibiotic prescribing outcomes.[34]

[34]In Table 9 of Appendix D.4 we show that there are no substantial differences in average antibiotic prescribing between our main sample and excluded observations from a subset of patients with incomplete spells. We show summary statistics for excluded observations of patients for whom a clinic change does not correspond to a physician exit or the assignment pre- or post-treatment is to an out-of-sample clinic.

F Further results

F.1 Analysis based on Daily Defined Dose

We also measure antibiotic prescribing using Daily Defined Dose (DDD) as an alternative approach. DDD is a commonly used, technical unit of measurement defined by the World Health Organization that expresses the average dose per day for a drug used in adults under the drug's main indication.

Table 10 shows summary statistics based on DDD. The average total DDD per prescription varies strongly between antibiotic classes. Notably, the share of total prescriptions measured by DDD differs considerably from the share of the total number of prescriptions for some antibiotic classes, particularly for classes other than penicillins and second-line antibiotics (Other antibiotics), such as Tetracyclines (J01 A) or Other antibacterials (J01 X). We also observe a high average DDD for these antibiotic classes, compared to other antibiotics. Due to this variation in DDD, we focus our main analysis on antibiotic prescribing measured by the number of prescriptions.

Table 11, Figure 9, and Figure 10 present our main analyses. The findings are generally consistent with our primary results, except in the case of non-penicillin, non-second line antibiotics (other antibiotics). The difference for other antibiotics compared to our main results might be due to variation in average Daily Defined Dose per prescription in this subcategory.

Table 10: Descriptive statistics for antibiotic prescribing

Panel A	**Antibiotic prescribing by ATC 3 class**			
ATC 3[a]	Pharmacological subgroup	Share of total prescriptions (number)	Share of total prescriptions (DDD)	Average DDD
J01 C	Beta-lactam antibacterials, penicillins	60.12%	56.09%	9.01
J01 F	Macrolides, lincosamides, streptogramins	18.25%	15.65%	8.28
J01 E	Sulfonamides and trimethoprim	9.97%	8.09%	7.83
J01 A	Tetracyclines	4.33%	7.47%	21.24
J01 M	Quinolone antibacterials	3.87%	2.90%	7.22
J01 X	Other antibacterials	3.40%	9.72%	21.66
J01 D	Other beta-lactam antibacterials	0.07%	0.09%	12.77
All J01	Antibacterials for systemic use	100%	100%	9.66

Panel B	**Daily Defined Dose of antibiotic prescriptions by subcategory**		
		Mean	SD
All antibiotics	All J01	5.24	(20.619)
Penicillins	J01 C	3.08	(8.571)
Second-line	J01 F, D, M	0.94	(4.906)
Other	J01 excluding J01 C, F, D, M	1.22	(17.004)

[a] Prescriptions of J01 G (Aminoglycoside antibacterials) are omitted due to their low frequency in order to ensure anonymity.

Table 11: Estimation results for the share of provider effects in antibiotic prescribing

Panel A	**Daily Defined Dose** Two-way fixed effects estimation[a]			
	All antibiotics	Penicillins	Second-line	Other
$\hat{\Delta}_i \times D_{it}$	0.36***	0.458***	0.702***	0.073
	(0.041)	(0.033)	(0.041)	(0.061)
Event dummies[b]	yes	yes	yes	yes
Time-varying controls[c]	no	no	no	no
Observations	7,789,908	7,789,908	7,789,908	7,789,908
Groups (patients)	1,371,604	1,371,604	1,371,604	1,371,604
Panel B	**Daily Defined Dose** Two-way fixed effects estimation[a]			
	All antibiotics	Penicillins	Second-line	Other
$\hat{\Delta}_i \times D_{it}$	0.387***	0.479***	0.711***	0.073
	(0.042)	(0.037)	(0.042)	(0.059)
Event dummies[b]	yes	yes	yes	yes
Time-varying controls[c]	yes	yes	yes	yes
Observations	7,647,003	7,647,003	7,647,003	7,647,003
Groups (patients)	1,344,910	1,344,910	1,344,910	1,344,910

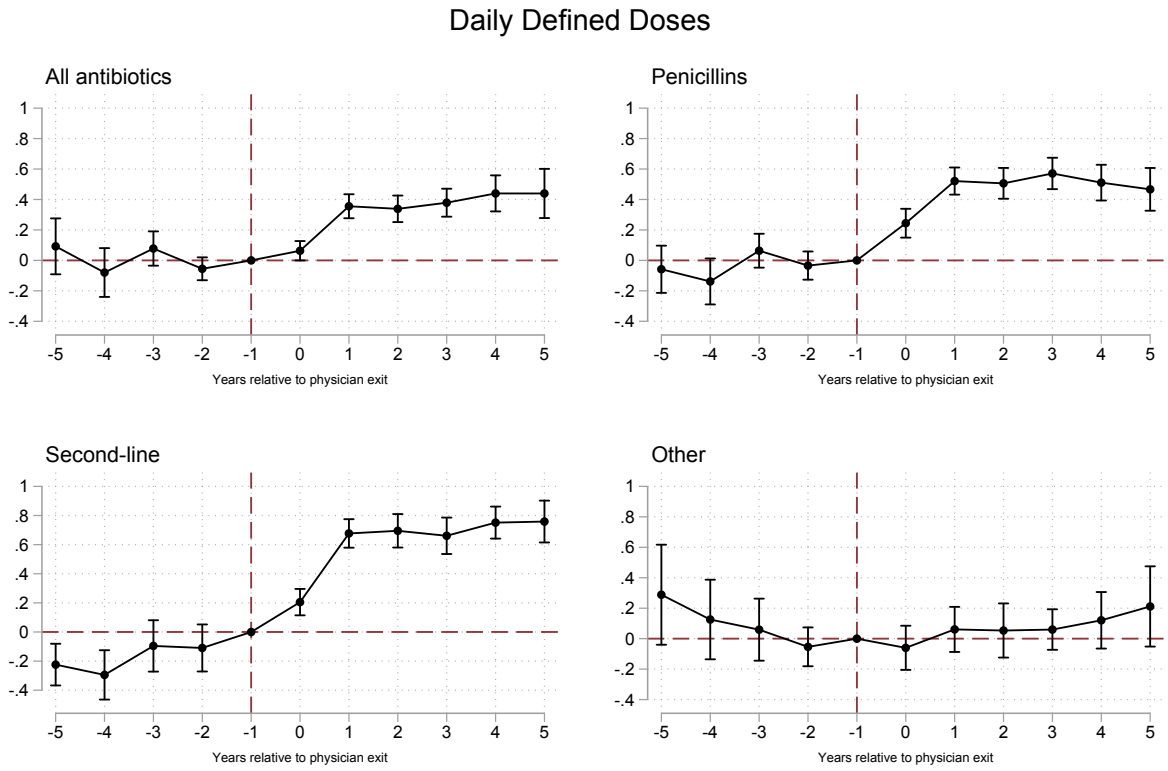
This table reports the average share of antibiotic prescribing differences between clinics that is attributable to provider practice style differences, the coefficient of $\Delta_i \times D_{it}$. Δ_i denotes the difference in mean prescribing between patient i's assigned sets of physicians and is estimated by $\hat{\Delta}_i$, the average prescribing to untreated patients. D_{it} denotes a post-treatment indicator. Standard errors are calculated using a bootstrap with 50 repetitions at the patient level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[a] Two-way fixed effects estimation with calendar year fixed effects and patient fixed effects.

[b] Event dummies include an indicator for treatment onset (relative year 0) and post-treatment.

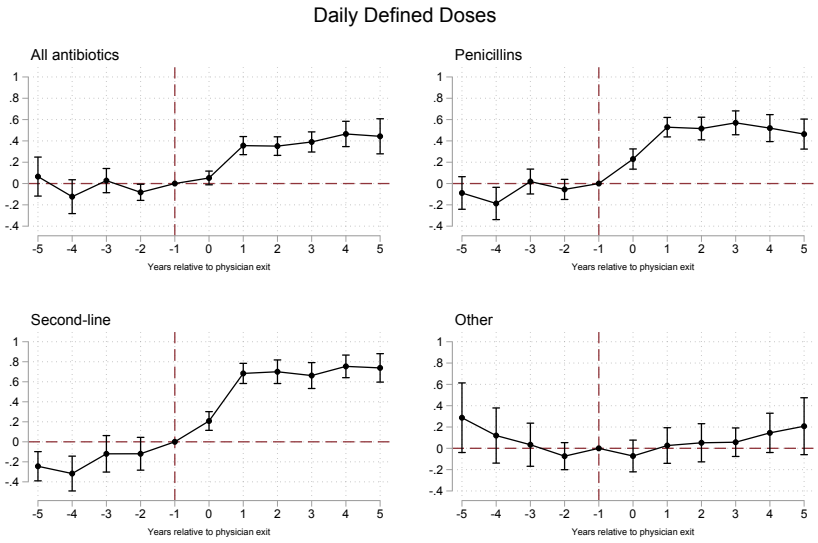
[c] Control variables include Number of interns at the clinic, Age squared, Household size, Pregnant, Any visit to emergency department, and Any call to an emergency doctor.

Figure 9: Event study estimates of the share of provider effects (DDD)

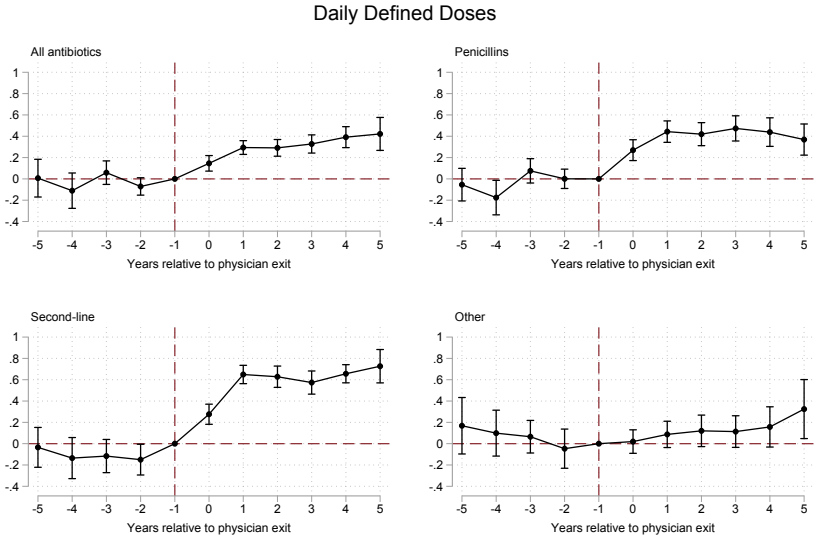


Notes: The figures display event study estimates for the share of antibiotic prescribing differences between clinics that is attributable to provider practice style differences. Estimations include patient fixed effects, calendar year fixed effects, and as time-varying characteristics indicators for the year of treatment onset and the post-exit period. Relative year -1 is the last pre-exit period, relative year 0 is a transitional period, and relative year 1 is the first post-exit period. Lines represent the 95% confidence intervals, with standard errors calculated using a bootstrap with 50 repetitions at the patient level.

Figure 10: Event study estimates of the share of provider effects (DDD), alternative specifications



(a) Time-varying controls



(b) Sun-Abraham interaction-weighted estimation

Notes: The figures display event study estimates for the share of antibiotic prescribing differences between clinics that is attributable to provider practice style differences. Lines represent the 95% confidence intervals, with standard errors calculated using a bootstrap with 50 repetitions at the patient level. Figure 10a displays event study estimates from estimations that include patient fixed effects, calendar year fixed effects, and indicators for treatment onset, post-exit, pregnancy, any visit to an emergency department, any call to an emergency department, any call to an emergency doctor, and as continuous variables age squared and household size. Figure 10b displays Sun-Abraham style interaction weighted estimates from fully saturated fixed effects specifications that include patient and calendar year fixed effects as well as interactions between relative period indicators and cohort indicators, where cohorts are defined by the calendar year of treatment onset. In a first step, cohort-relative year specific treatment effects are estimated. In the second step, relative year specific treatment effects are calculated as relative cohort size weighted averages by relative year.

F.2 Analyses excluding clinics with multiple physicians

We confirm that our main results are not substantially affected by restricting our sample to single-physician clinics.

In our main analysis we consider physician exits from both clinics with only one practicing physician and clinics with multiple physicians. A physician exit from a single-physician clinic implies that all former patients are assigned to a new physician and exposed to a new practice style. In contrast, when a physician leaves from a clinic with multiple physicians, the physician-patient assignment might not change for some patients if they are treated by one of those physicians who stay at the clinic.

Our measure, the share of provider effects in antibiotic prescribing differences, is consistent across both types of treatment events, regardless of whether all or only some physicians at a treated clinic exit. Our empirical strategy relies on scaling the changes in antibiotic prescribing to a treated patient by the difference in average antibiotic consumption at the two sets of physicians that may prescribe to the treated patient. As long as there is a change in the set of physicians that a treated patient can be assigned to, patients are *on average* exposed to a different post-exit practice style compared to pre-exit, and the scaling factor adjusts accordingly.

Table 12 describes the sample once we exclude clinics with multiple physicians. That is, we only consider treated patients who are exposed to a physician exit from a single-physician clinic as well as never-treated patients at single-physician clinics. The number of treated clinics exposed to a physician exit drops from 242 clinics in the full sample to 169 clinics.[35] The number of destination clinics, to which patients switch after an exit, drops from 556 clinics in our full sample to 390 clinics.

Table 13 shows estimates for the share of provider effects in antibiotic prescribing in our

[35]By construction, a physician exit from a single-physician clinic leads to clinic closure. For one treated clinic within the full sample, the physician exit (in December 2008) and the official reported clinic closure date (in January 2009) do not coincide and we do not code the exit event as clinic closure. This does not affect our results.

sample of single-physician clinics. The estimated provider effect shares in prescribing for antibiotics overall, penicillins, and second-line antibiotics are somewhat higher than in our main analysis. Figure 11 reveals that, once we allow for effects to differ by relative years, almost all 95% confidence intervals overlap with the event study estimates on the full sample. Qualitatively, the results based on the restricted sample of single-physician clinics are similar to the results based on the full sample.

Table 12: Numbers of observations

	Clinics	**Patients assigned to clinics**	**Observations**
Never-treated	414	601,189	3,379,289
Treated	172	152,284	641,064
Total	586	725,946	4,020,353

[a] The sum of never-treated and treated patient observations does not equal the total number of patients because patients are observed at two clinics if exposed to a physician exit.

Table 13: Estimation results for the share of provider effects in antibiotic prescribing, excluding clinics with multiple physicians

Panel A	**Number of prescriptions** Two-way fixed effects estimation[a]			
	All antibiotics	Penicillins	Second-line	Other
$\hat{\Delta}_i \times D_{it}$	0.607***	0.588***	0.918***	0.572***
	(0.042)	(0.046)	(0.049)	(0.172)
Event dummies[b]	yes	yes	yes	yes
Time-varying controls[c]	no	no	no	no
Observations	4,018,137	4,018,137	4,018,137	4,018,137
Groups (patients)	723,730	723,730	723,730	723,730
Panel B	**Number of prescriptions** Two-way fixed effects estimation[a]			
	All antibiotics	Penicillins	Second-line	Other
$\hat{\Delta}_i \times D_{it}$	0.641***	0.595***	0.931***	0.609***
	(0.041)	(0.046)	(0.050)	(0.172)
Event dummies[b]	yes	yes	yes	yes
Time-varying controls[c]	yes	yes	yes	yes
Observations	3,944,700	3,944,700	3,944,700	3,944,700
Groups (patients)	709,217	709,217	709,217	709,217

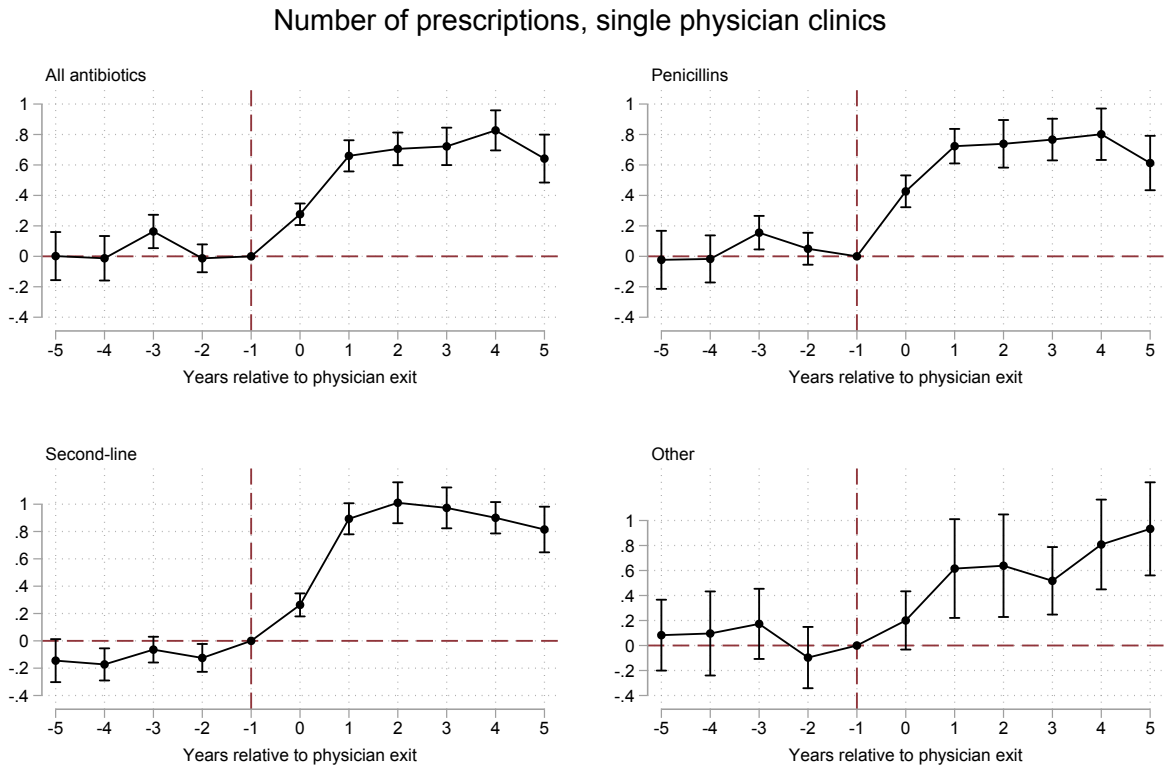
This table reports the average share of antibiotic prescribing differences between clinics that is attributable to provider practice style differences, the coefficient of $\Delta_i \times D_{it}$. Δ_i denotes the difference in mean prescribing between patient i's assigned sets of physicians and is estimated by $\hat{\Delta}_i$, the average prescribing to untreated patients. D_{it} denotes a post-treatment indicator. Standard errors are calculated using a bootstrap with 50 repetitions at the patient level. *** $p < 0.01$.

[a] Two-way fixed effects estimation with calendar year fixed effects and patient fixed effects.

[b] Event dummies include an indicator for treatment onset (relative year 0) and post-treatment.

[c] Control variables include Number of interns at the clinic, Age squared, Household size, Pregnant, Any visit to emergency department, and Any call to an emergency doctor.

Figure 11: Event study estimates of the share of provider effects, excluding clinics with multiple physicians



Notes: The figures display event study estimates for the share of antibiotic prescribing differences between clinics that is attributable to provider practice style differences. Estimations include patient fixed effects, calendar year fixed effects, and indicators for treatment onset and post-exit. Relative year -1 is the last pre-exit period, relative year 0 is a transitional period, and relative year 1 is the first post-exit period. Lines represent the 95% confidence intervals, with standard errors calculated using a bootstrap with 50 repetitions at the patient level.

F.3 Analyses for second-line antibiotic drugs

Table 14 shows estimation results for the share of provider effects in second-line antibiotic drugs separately for each ATC level 3 drug class: macrolides, lincosamides, and streptogramins (J01 F), cephalosporins (J01 D), and quinolones (J01 M). Columns (1) to (3) of Table 14 show baseline estimation results using as outcomes the number of antibiotic prescriptions, Columns (4) to (6) show estimation results when we allow for time-varying control variables, and Column (7) to (9) show estimation results when we measure prescribing by Daily Defined Dose. The results indicate that the substantial share of provider effects from analyzing these classes collectively are driven by the group of macrolides, lincosamides, and streptogramins (J01 F), and the group of quinolones (J01 M). In contrast, provider effects are much smaller in the group of cephalosporins (J01 D).

Table 15 shows estimation results for the share of provider effects in broad-spectrum antibiotic drugs. Specifically, this analysis includes all macrolides, lincosamides, and streptogramins (J01 F), cephalosporins (J01 D), and quinolones (J01 M), but excludes erythromycin. Broad-spectrum antibiotic drugs are active against a large range of bacterial groups. However, their excessive consumption can disrupt the native bacterial flora and enable multidrug resistances. Physicians are therefore in general advised to avoid prescribing broad-spectrum antibiotics .[36] The results show that the share of provider effects remain large in broad-spectrum antibiotic prescriptions.

The categorization into broad- and narrow-spectrum classes is not fixed.[37] For example, in the ESAC framework macrolides, lincosamides, and streptogramins, excluding erythromycin (J01 F, D, M, excluding J01 FA01), are considered broad-spectrum antibiotics,[38] whereas

[36]See Levy, Stuart B. 1998. "The Challenge of Antibiotic Resistance." Scientific American, 278(3): 46–53.

[37]See Acar, Jacques. 1997. "Broad- and Narrow-Spectrum Antibiotics: An Unhelpful Categorization." Clinical Microbiology and Infection, 3(4): 395–396.

[38]See ECDC (European Centre for Disease Prevention and Control), EFSA BIOHAZ Panel (European Food Safety Authority Panel on Biological Hazards) and CVMP (EMA Committee for Medicinal Products for Veterinary Use), 2017. "ECDC, EFSA and EMA Joint Scientific Opinion on a List of Outcome Indicators as Regards Surveillance of Antimicrobial Resistance and Antimicrobial Consumption in Humans and Food-Producing Animals." EFSA Journal 2017, 15(10):5017, 70 pp.

macrolides, lincosamides, streptogramins (J01 F) are not considered broad-spectrum antibiotics by the Danish Health Data Authority (https://medstat.dk/en).

Table 14: Estimation results for the share of provider effects in antibiotic prescribing

Panel A	**Number of prescriptions** Two-way fixed effects estimation[a]		
	J01 F	J01 D	J01 M
$\hat{\Delta}_i \times D_{it}$	0.870***	0.046	0.559***
	(0.038)	(0.090)	(0.058)
Event dummies[b]	yes	yes	yes
Time-varying controls[c]	no	no	no
Observations	7,789,908	7,789,908	7,789,908
Groups (patients)	1,371,604	1,371,604	1,371,604
Panel B	**Number of prescriptions** Two-way fixed effects estimation[a]		
	J01 F	J01 D	J01 M
$\hat{\Delta}_i \times D_{it}$	0.877***	0.051	0.586***
	(0.039)	(0.090)	(0.058)
Event dummies[b]	yes	yes	yes
Time-varying controls[c]	yes	yes	yes
Observations	7,647,003	7,647,003	7,647,003
Groups (patients)	1,344,910	1,344,910	1,344,910

This table reports the average share of antibiotic prescribing differences between clinics that is attributable to provider practice style differences, the coefficient of $\Delta_i \times D_{it}$. Δ_i denotes the difference in mean prescribing between patient i's assigned sets of physicians and is estimated by $\hat{\Delta}_i$, the average prescribing to untreated patients. D_{it} denotes a post-treatment indicator. Standard errors are calculated using a bootstrap with 50 repetitions at the patient level. *** $p < 0.01$.

[a] Two-way fixed effects estimation with calendar year fixed effects and patient fixed effects.

[b] Event dummies include an indicator for treatment onset (relative year 0) and post-treatment.

[c] Control variables include Number of interns at the clinic, Age squared, Household size, Pregnant, Any visit to emergency department, and Any call to an emergency doctor.

Table 15: Estimation results for the share of provider effects in antibiotic prescribing

	Number of prescriptions	
	Two-way fixed effects estimation[a]	
	J01 F, D, M, excl.	J01 FA01
	(1)	(2)
$\hat{\Delta}_i \times D_{it}$	0.849***	0.868***
	(0.040)	(0.041)
Event dummies[b]	yes	yes
Time-varying controls[c]	no	yes
Observations	7,789,908	7,647,003
Groups (patients)	1,371,604	1,344,910

This table reports the average share of antibiotic prescribing differences between clinics that is attributable to provider practice style differences, the coefficient of $\Delta_i \times D_{it}$. Δ_i denotes the difference in mean prescribing between patient i's assigned sets of physicians and is estimated by $\hat{\Delta}_i$, the average prescribing to untreated patients. D_{it} denotes a post-treatment indicator. Standard errors are calculated using a bootstrap with 50 repetitions at the patient level. *** $p < 0.01$.

[a] Two-way fixed effects estimation with calendar year fixed effects and patient fixed effects.

[b] Event dummies include an indicator for treatment onset (relative year 0) and post-treatment.

[c] Control variables include Number of interns at the clinic, Age squared, Household size, Pregnant, Any visit to emergency department, and Any call to an emergency doctor.

G Alternative econometric specifications

We relax a number of our identifying assumptions and present results for these sensitivity analyses.

Table 16 shows results for our static sensitivity specifications. In Panel A of Table 16, we estimate the share of provider effects in antibiotic prescribing accounting for time-varying observable patient characteristics. In Panel B, we estimate Sun-Abraham style interaction-weighted specifications, which account of treatment heterogeneity by the year of treatment onset.

We present the corresponding dynamic effect estimates in Figure 12. Figure 12a shows estimates of provider effects over relative years when we control for time-varying characteristics. Figure 12b shows estimate provider effects over relative years based on Sun-Abraham interaction-weighted specifications.

Overall, the results are similar to our main estimates. In the Sun-Abraham interaction-weighted estimations, we obtain smaller estimates of provider effects in second-line antibiotic prescribing and non-penicillin, non-second line prescribing. However, our conclusions remain the unchanged.

Table 16: Estimation results for the share of provider effects in antibiotic prescribing

Panel A	**Number of prescriptions** Two-way fixed effects estimation[a]			
	All antibiotics	Penicillins	Second-line	Other
$\hat{\Delta}_i \times D_{it}$	0.53***	0.456***	0.841***	0.509***
	(0.035)	(0.039)	(0.037)	(0.071)
Event dummies[b]	yes	yes	yes	yes
Time-varying controls[c]	yes	yes	yes	yes
Observations	7,647,003	7,647,003	7,647,003	7,647,003
Groups (patients)	1,344,910	1,344,910	1,344,910	1,344,910
Panel B	**Number of prescriptions** Sun-Abraham interaction-weighted estimation[d]			
	All antibiotics	Penicillins	Second-line	Other
$\hat{\Delta}_i \times D_{it}$	0.486***	0.515***	0.743***	0.228***
	(0.036)	(0.053)	(0.041)	(0.059)
Event dummies[c]	yes	yes	yes	yes
Time-varying controls[d]	no	no	no	no
Observations	7789908	7789908	7789908	7789908
Groups (patients)	1,371,604	1,371,604	1,371,604	1,371,604

This table reports the average share of antibiotic prescribing differences between clinics that is attributable to provider practice style differences, the coefficient of $\Delta_i \times D_{it}$. Δ_i denotes the difference in mean prescribing between patient i's assigned sets of physicians and is estimated by $\hat{\Delta}_i$, the average prescribing to untreated patients. D_{it} denotes a post-treatment indicator. Standard errors are calculated using a bootstrap with 50 repetitions at the patient level. *** $p < 0.01$.

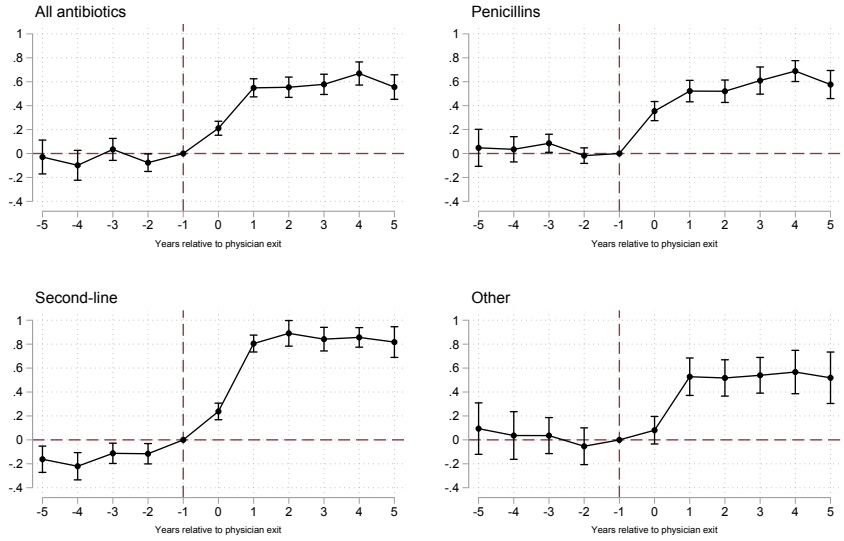
[a] Two-way fixed effects estimation with calendar year fixed effects and patient fixed effects.

[b] Event dummies include an indicator for treatment onset (relative year 0) and post-treatment.

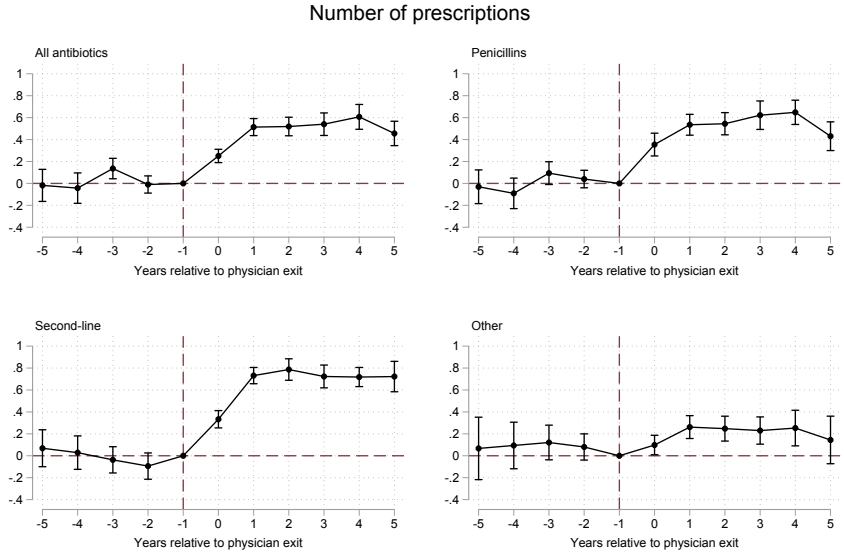
[c] Control variables include Number of interns at the clinic, Age squared, Household size, Pregnant, Any visit to emergency department, and Any call to an emergency doctor.

[d] Sun-Abraham style interaction weighted estimation based on fully saturated fixed effects specifications including patient and calendar year fixed effects as well as interactions between relative period indicators and cohort indicators, where cohorts are defined by the calendar year of treatment onset. Reported coefficients correspond to estimated treatment effects aggregated over cohorts and relative year, weighted by cohort size.

Figure 12: Event study estimates of the share of provider effects, alternative specifications



(a) Time-varying controls



(b) Sun-Abraham interaction-weighted estimation

Notes: Lines represent the 95% confidence intervals. Standard errors are calculated using a parametric bootstrap to draw mean prescribing on the level of physician sets, with 50 repetitions at the patient level. Figure 12a displays event study estimates from estimations that include patient fixed effects, calendar year fixed effects, and indicators for treatment onset, post-exit, pregnancy, any visit to an emergency department, any call to an emergency department, any call to an emergency doctor, and as continuous variables age squared and household size. Figure 12b displays Sun-Abraham style interaction weighted estimates from fully saturated fixed effects specifications that include patient and calendar year fixed effects as well as interactions between relative period indicators and cohort indicators, where cohorts are defined by the calendar year of treatment onset. In a first step, cohort-relative year specific treatment effects are estimated. In the second step, relative year specific treatment effects are calculated as relative cohort size weighted averages by relative year.

H Estimation of practice style correlates

H.1 Observable physician and clinic characteristics

Below we describe how the variables in our practices style correlates analysis are defined.

Physician characteristics. We construct physician individual-level characteristics and aggregate them over all physicians in a given set of physicians. We have to aggregate the individual-level characteristics as we can only observe the identity of the clinic in a given year for each prescription, but not the identity of the prescribing physician.

As personal characteristics we consider the average age, the share of physicians with a PhD degree, the share of female physicians, and the share of physicians with migration backgrounds. We separate migration backgrounds by Nordic origin country (Finland, Iceland, Norway and Sweden), and non-Nordic origin country.

Clinic-level characteristics. We further include a set of variables to describe diagnostic availability and staff size at a general practice clinic. From claims data, we construct dummy variables that indicate whether microscopy, bacterial culture, and teleconsultations were available. We assume that either diagnostic method was available in a given year if any of the corresponding claim code were used at least once in a given year.[39] We also impute diagnostic methods as available if both in the previous and the following year any of the corresponding claim codes have been used. To describe staff size, we include the maximum number of general practitioners, the number of unique patients per general practitioner, and the maximum number of short-term medical staff working at the same time in a clinic in a year. We construct the number of unique patients at a clinic as the total number of unique social security numbers in a clinic's claims records. The number of short-term medical staff covers all recorded stays of up to a year. We refer to those short-term medical staff as interns.

[39]We use 6-digit SPECIALE claim codes to identify relevant procedures. For microscopic examinations, we consider the codes 807102 – 807104, 807122 – 807124. For diagnostics based on bacterial cultures, we consider the codes 807105 – 807107. For teleconsulting, we consider the codes 800200–800203, 800500–800501, 803200–803201, 808294.

H.2 Estimation details

We estimate the association between prescribing practice styles and observable physician characteristics in two steps. In the first step, we obtain the pair-specific differences in practice styles by estimating the following equation:

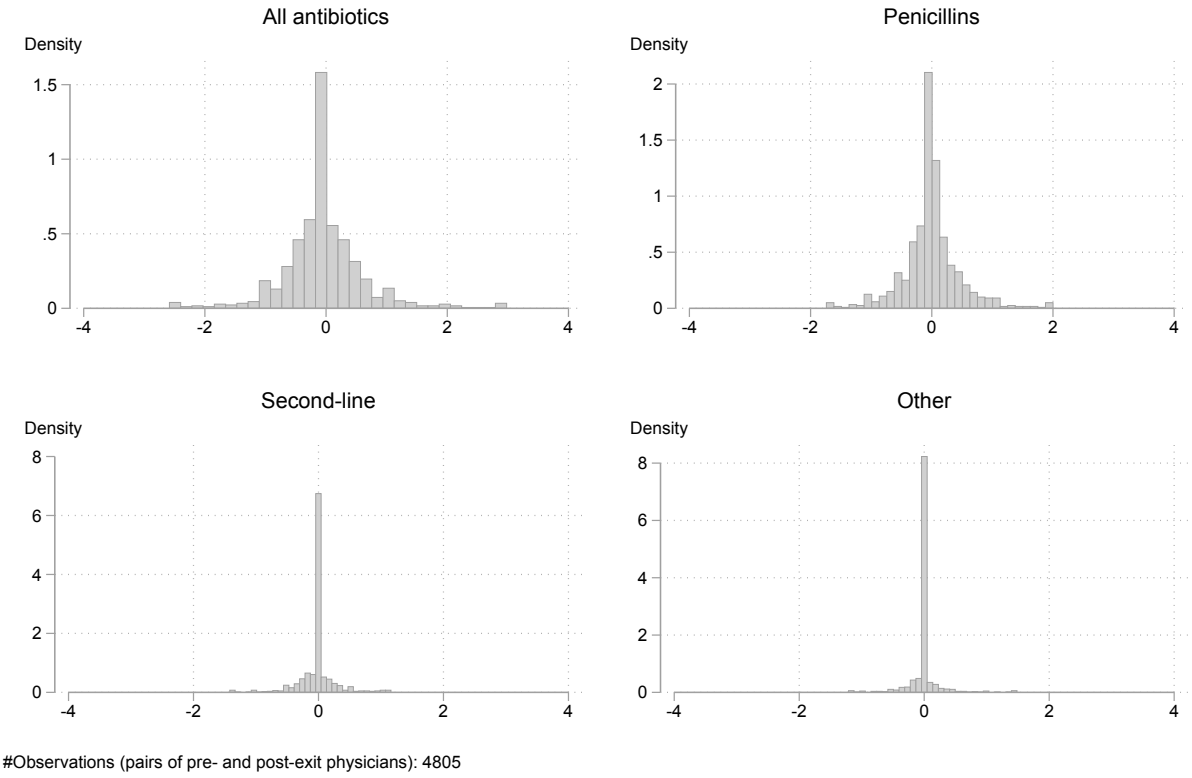
$$y_{it} = \tilde{\alpha}_i + (\delta_{j'} - \delta_j) \times D_{it} \times I_{j(i,t<r_0(i))=j,j(i,t>r_0(i))=j'} + x_{it}\beta + \nu_{it}, \tag{6}$$

where $\tilde{\alpha}_i, \beta$ are parameters and D_{it}, x_{it} are variables as defined above. ν_{it} is the error term. $r_0(i)$ denotes the calendar year in which patient i is exposed to a physician exit. $I_{j(i,t<r_0(i))=j,j(i,t>r_0(i))=j'}$ is an indicator which is one if a treated patient i is assigned to the set of physicians j before the physician exit occurs in $t = r_0(i)$ and to a different set of physicians j' after the physician exit has occurred. $\delta_{j'} - \delta_j$ denotes the difference in prescribing practice styles between the origin and destination physicians j' and j. Estimating Equation (6) fully specified in all possible pairs of sets of physicians $\{j, j'\}$ thus yields estimates for the pair-specific differences in practice styles. Figure 13 shows histograms of all estimated pair-specific differences in practice styles.

Note that our empirical strategy only allows identification of practice style differences. We therefore also construct pair-wise differences in physician observables when estimating the correlates of practice style differences. For each unique set of physicians, we construct the average over years for each observable characteristic. We then standardize each variable to have mean 0 and standard deviation 1. To obtain the covariates for our second-step regressions, we take the pair-wise difference in the standardized and average observable characteristics for each pair of sets of physicians.

In the second step, we perform either bivariate OLS regressions or multivariate post-LASSO OLS regressions. For the bivariate specifications, we regress the difference in practice styles on the differences in standardized physician characteristics. For the post-LASSO specifications, we regress the difference in practice styles on the differences in all standardized

Figure 13: Histograms of differences in antibiotic prescribing practice styles between physician pairs



Notes: The figures show pairwise differences in antibiotic prescribing practice styles between pre- and post-exit physicians. Pairwise differences in practice styles correspond to treatment effects from our main analysis, estimated separately for all pairs of pre- and post-physicians treated patients are assigned to. Values are bunched for groups of five patients with similar estimated mean difference in average prescribing due to data anonymization. The top and bottom 0.5 percentiles are winsorized.

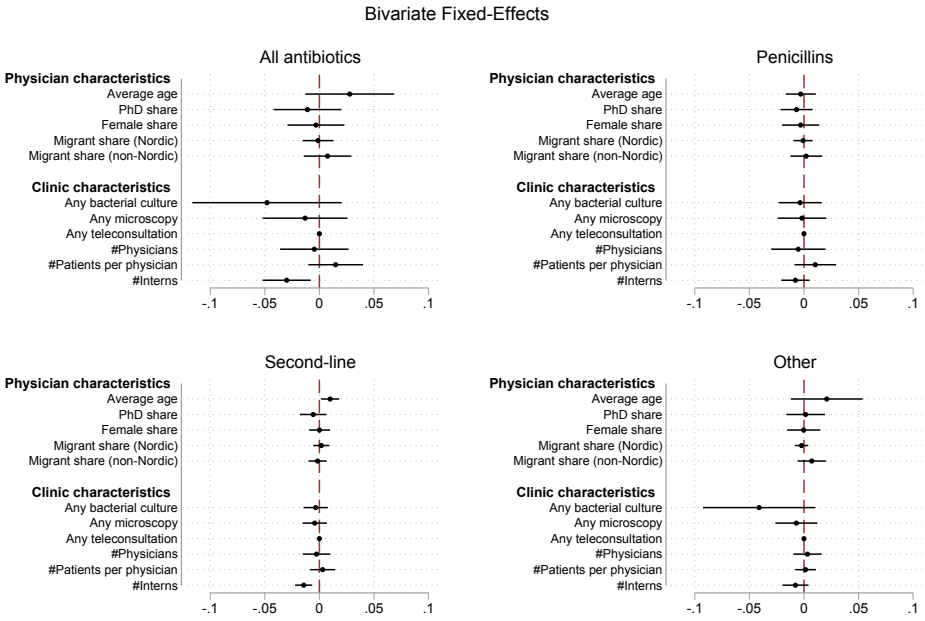
physician characteristics that have been selected by a first-step LASSO regression. The unit of observation is a pair of sets of physicians to which at least one treated patient is assigned.

To obtain standard errors we perform a parametric bootstrap with 50 repetitions. In each repetition, we draw the difference in practice styles for each pair of sets of physicians $\{j, j'\}$ from a normal distribution with mean $\widehat{\delta_{j'} - \delta_j}$ and standard deviation $se(\widehat{\delta_{j'} - \delta_j})$ estimated from our first-step estimation of practice style differences, where *se* denotes the standard error. The bootstrapped standard errors account for estimation error from our first step estimation.

H.3 Correlates with practice style differences estimated by fixed effects

Figure 14 shows coefficients estimates from bivariate fixed effects regressions, with fixed effects for the initial pre-exit set of physicians that treated patients are assigned to. The fixed effects regressions rely on variation in the difference in physician characteristics that result from patients being assigned to different destination set of physicians after being exposed to an exit at the same initial set of physicians. The results are similar to our main results.

Figure 14: Correlates of practice style differences



Notes: The figure presents estimated changes in prescribing styles associated with an one standard deviation increase in a physician or clinic characteristics using bivariate fixed effects regression. We obtain these estimates by regressing the difference in antibiotic prescribing practice style on differences in observed characteristics between sets of pre- and post-exit physicians that treated patients are assigned to, with fixed effects for the pre-exit physicians. Standard errors are calculated using a parametric bootstrap with 50 repetitions at the patient level to draw differences in prescribing practice styles. Physician and clinic characteristics are standardized to have mean 0 and standard deviation 1 prior to differencing.

I Practice style differences and quality of care

I.1 Measures of quality of care

We measure the quality of care based on physicians' prescription quality and patients' health outcomes.

To obtain the number of prescriptions without diagnostic test in a given year, we identify diagnostic use based on claims submitted by primary care physicians. However, we only observe the week during which a physician submits claims with the public insurer. In contrast, we know the exact date on which patients purchase the antibiotic prescription. Because claims can be submitted with some delay, but are generally submitted at least once per month in order for physicians to get compensated for services performed, we allow the submission week of a diagnostic test to be up to four weeks from the date of the antibiotic prescription. We use 6-digit SPECIALE claim codes to identify diagnostic tests. As rapid tests (stix tests, strep test), we consider the claim codes 807101, 807109. As microscopic examinations, we consider the codes 807102 – 807104, 807122 – 807124. As bacterial cultures, we consider the codes 807105 – 807107. We consider a prescription as without diagnostic test only if it cannot be linked to any test claim from the same week up to four weeks in the future. To construct our outcome variable, we lastly take the sum over all prescriptions without a diagnostic test in a given year.

We identify ambulatory care sensitive conditions as based on diagnostic codes used in health services research.[40] We restrict the analysis to acute conditions that are both frequently caused by infectious agents including bacteria, and commonly encountered in general practice: cellulitis, ear, nose and throat infect ions, perforated or bleeding ulcer, urinary tract infection, and pneumonia. To measure hospitalizations for infection, we exclude referrals from primary care as well as internal hospital referrals unless the diagnosis was

[40]See Bardsley, Martin, Ian Blunt, Sian Davies, and Jennifer Dixon. 2013. "Is Secondary Preventive Care Improving? Observational Study of 10-Year Trends in Emergency Admissions for Conditions Amenable to Ambulatory Care." BMJ Open, 3(1).

made at the first day of a patient's hospitalization spell. Table 17 shows means and standard deviations for hospitalizations due to infection-related ambulatory care sensitive conditions. Table 18 lists the conditions including sub-categories and their corresponding ICD-10 codes.

Table 17: Summary statistics of prescription quality and hospitalizations due to ambulatory care sensitive conditions

	Mean	SD
Any hospitalization		
All infections	0.005	(0.072)
Cellulitis	0.001	(0.037)
Ear, nose, and throat infections	0.001	(0.033)
Perforated or bleeding ulcer	0.001	(0.022)
Urinary tract infection or pyelonephritis	0.001	(0.036)
Pneumonia	0.001	(0.031)
Number of prescriptions		
Follow-up prescriptions	0.021	(0.195)
Prescriptions without diagnostic tests	0.340	(0.940)
Observations (patient-years)	7,796,767	

Table 18: List of ICD-10 codes for infection-related ambulatory care sensitive conditions

ICD-10 code	Category
Cellulitis	
L03	Cellulitis
L04	Acute lymphadenitis
L08	Other local infections of skin and subcutaneous tissue
L88	Pyoderma grangrenosum
L980	Pyogenic granuloma
L983	Eosinophilic cellulitis
Ear, nose and throat infections	
H66	Otitis media, unspecified
H67	Otitis media in diseases classified elsewhere
J02	Acute pharyngitis
J03	Acute tonsillitis
J06	Acute upper respiratory infections of multiple and unspecified sites
J312	Chronic pharyngitis
Perforated/bleeding ulcer	
K250-K252	Gastric ulcer
K254-K256	
K260-K262	Duodenal ulcer
K264-K266	
K270-K272	Peptic ulcer, site unspecified
K274-K276	
K280-K282	Gastrojejunal ulcer
K284-K286	
Urinary tract infection/Pyelonephritis	
N10	Acute tubulo-interstitial nephritis
N11	Chronic tubulo-interstitial nephritis
N12	Tubulo-interstitial nephritis, not specified as acute or chronic
N136	Pyonephrosis
N390	Urinary tract infection, site not specified
Pneumonia	
J13	Pneumonia due to Streptococcus pneumoniae
J14	Pneumonia due to Haemophilus influenzae
J153	Pneumonia due to streptococcus, group B
J154	Pneumonia due to other streptococci
J157	Pneumonia due to Mycoplasma pneumoniae
J159	Bacterial pneumonia, unspecified
J168	Pneumonia due to other specified infectious organisms
J181	Lobar pneumonia, unspecified
J188	Other pneumonia, organism unspecified

I.2 Estimation details

Our analysis of correlations between prescribing styles and observable physician and clinic characteristics are based on estimates for differences in prescribing styles between each separate pair of origin- and destination physicians, as described in Section H.

To estimate the change in prescription quality associated with a more intense prescribing style, we use the following baseline specification:

$$h_{it} = \tilde{\alpha}_i + \eta \times D_{it} \times \widehat{(\delta_{j'} - \delta_j)} + x_{it}\beta + \omega_{it}, \tag{7}$$

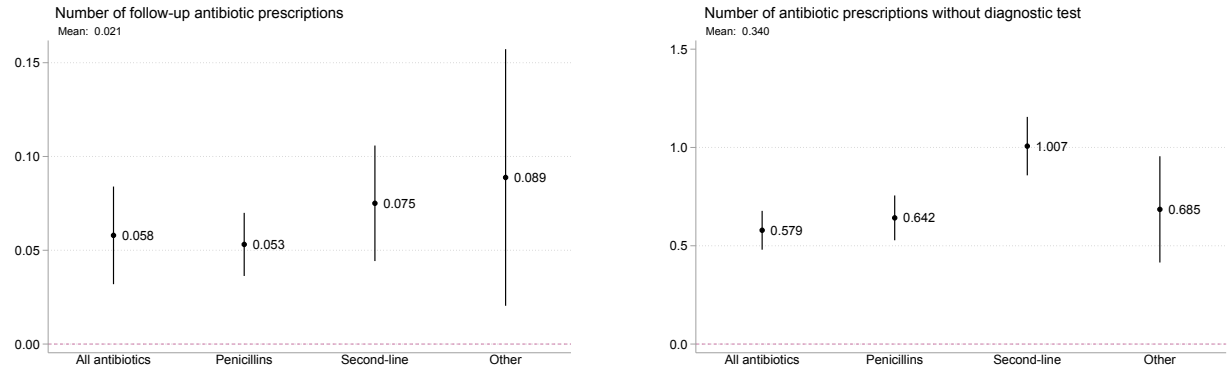
where h_{it} is a measure of prescription quality or health outcome for patient i in year t, α_i denotes patient-fixed effects, x_{it} includes calendar-year fixed effects, the post-exit indicator D_{it}, and an indicator for the year of the exit, and ω_{it} is an error term. Our coefficient of interest is η associated with the interaction between the post-exit indicator D_{it} and the estimated difference in prescribing styles $\widehat{(\delta_{j'} - \delta_j)}$.

We acknowledge that our estimated effects are not necessarily causal. For example, while differences in practice style can affect hospitalization rates, they may not be entirely driven by differences in antibiotic prescribing intensities. However, to investigate a mostly immediate link between antibiotic prescribing and prescription quality, we limit our analysis to direct measures of low-quality prescribing and infection-related hospitalizations, for which antibiotic treatment decisions are integral.

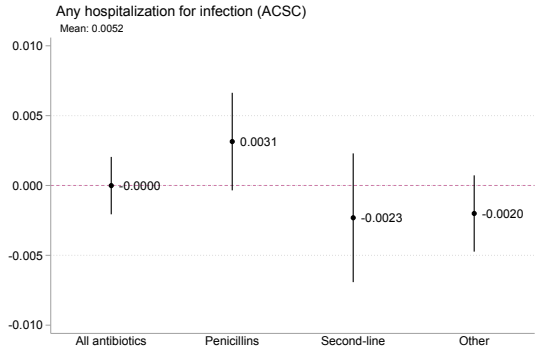
I.3 Sensitivity analysis results

Figure 15 presents results from alternative specifications, where we control for Number of interns at the clinic, Age squared, Household size, Pregnant, Any visit to emergency department, and Any call to an emergency doctor, in addition to the basic time control variables. The coefficient estimates are weaker but otherwise similar to our main results.

Figure 15: Quality of care and antibiotic prescribing intensity, time-varying control variables



(a) Low quality prescribing



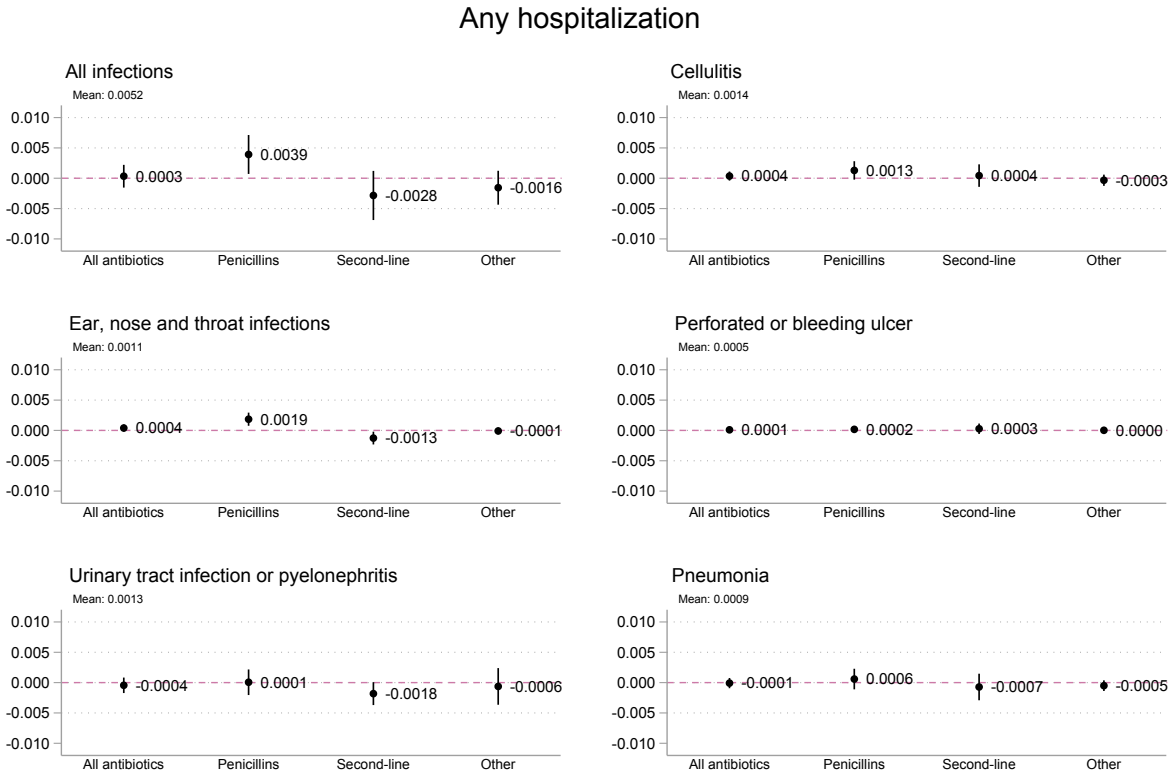
(b) Patient health

Notes: The figure shows the estimated changes in quality of care associated with a practice style of one additional antibiotic prescription, based on patient-year level regressions that include patient fixed effects, calendar year fixed effects, and indicators for treatment onset, post-exit, pregnancy, any visit to an emergency department, any call to an emergency department, any call to an emergency doctor, as well as the continuous variables age squared and household size. We consider an increase by one overall antibiotic prescription, as well as separately one more penicillin, second-line, or other antibiotic prescription. Figure 15a shows the relation between higher antibiotic prescribing intensity and low quality prescribing, measured by follow-up antibiotic prescriptions within seven days after an initial prescription of a different ATC 4 class (left), and prescriptions without any claim for diagnostic tests (right). Figure 15b shows the change in patient health associated with higher antibiotic prescribing intensity, measured by the propensity for any hospitalization for an infection-related ambulatory care sensitive condition (ACSC). We estimate changes in antibiotic prescribing styles as separate provider effects for each pair of physicians among treated patients, controlling for patient fixed effects, calendar year fixed effects, and indicators for treatment onset and post-exit. Lines represent the 95% confidence intervals, with standard errors based on a parametric bootstrap with 50 repetitions at the patient level to draw differences in prescribing practice styles.

I.4 Condition-specific hospitalization rates

Figure 16 shows results for condition-specific estimations for the relation between antibiotic prescribing styles and hospitalization rates. The figure shows that the positive link between hospitalization rates for infection and penicillin prescribing prescribing is driven by hospitalizations for ear, nose, and throat infections (statistically significant on the 1% level). We also observe a weaker negative association between increases in second-line antibiotic prescribing and hospitalizations for ear, nose, and throat infections (statistically significant on the 10% level). However, on the margin, we observe no link between second-line antibiotic prescribing and lower hospitalization rates.

Figure 16: Hospitalization rates for ambulatory care sensitive conditions and antibiotic prescribing intensity, by condition



Notes: The figure shows the estimated changes in hospitalization rates for infection-related ambulatory care sensitive condition associated with a practice style of one additional antibiotic prescription, in the aggregate and by condition, based on patient-year level regressions that include patient fixed effects, calendar year fixed effects, and indicators for treatment onset and post-exit. We consider an increase by one overall antibiotic prescription, as well as separately one more penicillin, second-line, or other antibiotic prescription. We estimate changes in antibiotic prescribing styles as separate provider effects for each pair of physicians among treated patients, controlling for patient fixed effects, calendar year fixed effects, and indicators for treatment onset and post-exit. Lines represent the 95% confidence intervals, with standard errors based on a parametric bootstrap with 50 repetitions at the patient level to draw differences in prescribing practice styles.