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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 percent of the differences in overall antibiotic use and 83 percent 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.

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I. 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 ten 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; Finkelstein et al. 2025; Agha et al. 2022). 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 healthcare sector may be warranted if antibiotic overuse is largely associated with idiosyncratic provider practice styles.

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. We leverage that the timing of such an exit is random with respect to patients' individual health and antibiotic consumption needs. Thus, when physicians move or retire from their clinic, or when a clinic closes due to their exit, their patients must exogenously 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 practice 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

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) (accessed January 22, 2026).

2. For example, the annual European Antibiotic Awareness Day campaign (https://antibiotic.ecdc.europa.eu/en) provides informational material to healthcare professionals (accessed January 22, 2026), and the World Health Organization offers an online course on antibiotic stewardship for practicing clinicians (https://whoacademy.org/coursewares/course-v1:WHOAcademy-Hosted+H0098EN+H0098EN_Q4_2024?source=edX, accessed January 22, 2026). In the medical literature, a number of randomized control trials evaluate behavioral interventions to affect physician prescribing behavior, for example, Hallsworth et al. (2016) and Meeker et al. (2016).

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that affect different patients in a consistent manner. Practice styles have been shown to persistently drive within- and between-region variation in healthcare utilization (for example, Chandra and Staiger 2007; Epstein and Nicholson 2009; Molitor 2018; Cutler et al. 2019; Simeonova, Skipper, and Thingholm 2024). 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 central 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 (for example, Currie, MacLeod, and Van Parys 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

3. For example, physicians begin their careers as interns distributed across the country based on a lottery system. Fadlon, Lyngse, and Nielsen (2024) find strong persistence in physician location based on a physician's initial draw.

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 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 reassigned 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 healthcare 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 encouraging high-prescribing physicians to emulate low-prescribers' practice styles could be an effective strategy without negatively impacting patient health in primary care. Policies to achieve stronger standardization of care may include the adjustment and increased dissemination of guidelines, emphasizing antibiotic stewardship in medical education, and promoting the use of high-quality diagnostics. We find that variation in antibiotic prescribing can partly be attributed to differential practice styles associated with physician age differences, where age is positively correlated with prescribing intensity. This result suggests a particularly important role for continued education as a policy lever to promote antibiotic stewardship. 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 may promise even greater benefits.

Several studies emphasize the importance of identifying causes of variation in healthcare provision to effectively target policies. Finkelstein, Gentzkow, and Williams (2016) separate local place effects from patient-specific drivers of geographic variation in healthcare utilization in the United States using patient mobility. Migration of patients

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has also been exploited to study geographic variation in prescription opioid abuse in the US (Finkelstein et al. 2025), regional healthcare and mortality in Norway (Godøy and Huitfeldt 2020), and 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' healthcare utilization in the US. 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 (for example, Laxminarayan and Brown 2001; Currie, Lin, and Meng 2014; Bennett, Hung, and Lauderdale 2015; Ellegård, Dietrichson, and Anell 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 (2023) 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.

Section II presents a model of prescribing to conceptualize the causal mechanism we aim to measure. Section III describes the institutional background and data. Section IV provides descriptive evidence motivating the main analysis. Section V describes identification and estimation, and Section VI presents results. Section VII provides further results on provider practice styles in antibiotic prescribing, including correlates with observable characteristics and quality of care. Section VIII discusses the implications of our findings. Section IX concludes.

II. 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, MacLeod, and Van Parys 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, and the physician exit does not lead to a clinic closure. For these clinics, we measure provider effects

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

$$(1) \quad y_{ijt} = \alpha_i + \delta_{j(i,t)} + \mathbf{x}_{it}\beta + \epsilon_{it},$$

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 $\mathbf{x}_{it}$ 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 $\mathbf{x}_{it}$ 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 they stay at the clinic but are 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 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

4. We apply a standardization procedure to ensure that our estimated practice styles are comparable across different types of physician exits, such as between clinic closures and nonclosures or between single- and multi-physician clinics. Our main analysis yields qualitatively similar results when restricted to single-physician clinics.

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 Online Appendix A.

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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, $y_j = E[y_{ijt}|D_{it} = 0]$ represents mean prescribing of physicians j to patients never or not yet exposed to treatment.

For a patient i exposed to a physician exit, we define the difference in mean prescribing between their pre-exit physicians, j, and the physicians 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:

$$(2) \quad y_{ijt} = \alpha_i + \delta_j + \theta \times D_{it} \times \Delta_i + \mathbf{x}_{it}\beta + \epsilon_{it},$$

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.

III. Primary Care Provision in Denmark

We study antibiotic prescribing practice styles using administrative data that cover Denmark's entire population. Denmark has a tax-funded public health

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).

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

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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 filed claims.

In Denmark, patients require a physician prescription to obtain antibiotic drugs, which are purchased at pharmacies with a small copayment. About 90 percent of human antibiotics are purchased in outpatient care, out of which about 75 percent are prescribed through primary care physicians.[8] Danish physicians are not remunerated for prescriptions, so 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).

A. Patient Assignment and Physician Exits

Patients are assigned to a fixed primary care provider through a list system.[9] Switching to another provider requires a small fee of 150 DKK (about 23 USD in 2026), and the new provider must be located within 15 km (5 km in metropolitan areas) of patients' residence. 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 (2025) document that in the majority of municipalities in 2010, primary care clinics were close to or above the capacity limit of 1,600 patients per physician.

Hence, when a physician leaves a clinic, their patients are as good as randomly reassigned to new physicians conditional on location. Reassignment of patients following a physician exit, most often due to a physician's retirement but also in cases of relocation, forms the basis of our identification strategy, as this implies that the timing of the reassignment between patients and clinics is exogenous to patient health. In particular, patients do not switch for reasons related to the healthcare quality of their original clinic.[10]

8. See information from the Danish Ministry of Health (https://www.ssi.dk/aktuelt/nyheder/2017/2017-ny-dansk-handlingsplan-skal-bremse-antibiotikaresistens, accessed January 22, 2026).

9. We closely follow Simonsen et al. (2021) in describing the details on patient assignment to primary care clinics in Denmark.

10. Consistent with this notion, it is extremely rare that a physician exit occurs because of disciplinary actions related to clinic quality in the Danish healthcare system. For example, in 2014, the Danish Disciplinary Board reported only two malpractice litigation cases across all clinical specialties, see Statistical information about patient complaints 2014 (https://stpk.dk/media/pxqdfylg/statistiske-oplysninger-om-patientklagerfor-2014.pdf, accessed January 22, 2026). Financial reasons for leaving or closing a clinic are also highly unlikely, as fees are set through national agreements, and most clinics operate at or near their patient capacity (Kristiansen and Sheng 2025).

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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).[11] 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 reapply 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.

B. 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 numbers to their clinics' license numbers and registers the in- and outflow of physicians to clinics. For reasons of data minimization, the Danish Health Data Protection Authority provided only a nonselective 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 service 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, 2005–2012, 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 clinics* 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.[12] We consider

11. 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, accessed January 22, 2026), and regulations LBK no. 903 of 26/08/2019 and BEK nr 1056 af 31/05/2021).

12. 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 percent of all patient–years.

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all patient–year observations assigned to any of the 980 primary care clinics in our analysis, keeping 25.33 percent of the overall patient–year claim observations. We include patients only when they switch their modal clinic at most once, dropping 15.68 percent 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 assigned to more than one treated clinic, dropping 0.32 percent 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 percent. 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 percent. For patients switching to a treated clinic, we keep only observations at the treated clinic, dropping 0.05 percent. We exclude a patient's first observed year at a treated clinic if that year is the treatment year, dropping 0.15 percent.

The final sample contains 7,789,908 patient–year observations matching 1,371,604 patients to 805 primary care clinics.[13] 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.

C. 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.

1. 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): penicillins, second-line antibiotics, and other classes. Penicillins (J01 C) are

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

Table 1
Descriptive Statistics for Antibiotic Prescribing in Primary Care

ATC 3[a]	Pharmacological Subgroup	Share of Total Prescriptions	Any Prescription
Panel A: Number of Antibiotic Prescriptions by ATC 3 Class			
J01 C	Beta-lactam antibacterials, penicillins	62.49%	22.96%
J01 F	Macrolides, lincosamides and streptogramins	17.18%	7.33%
J01 E	Sulfonamides and trimethoprim	9.84%	3.49%
J01 A	Tetracyclines	3.83%	1.11%
J01 M	Quinolone antibacterials	3.57%	1.57%
J01 X	Other antibacterials	3.03%	0.93%
J01 D	Other beta-lactam antibacterials	0.06%	0.02%
All J01	Antibacterials for systemic use	100.00%	30.55%
Panel B: Number of Antibiotic Prescriptions by Subcategory			
		Mean	SD
All antibiotics	All J01	0.56	(1.28)
Penicillins	J01 C	0.35	(0.82)
Second-line	J01 FDM	0.12	(0.46)
Other	J01 excluding J01 C,F,D,M	0.09	(0.67)

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

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.[14] We group the remaining antibiotic classes into a single subcategory.

Table 1 shows descriptive statistics. On average, 0.56 antibiotic prescriptions are filled in primary care per patient–year, and we observe the purchase of any antibiotic

14. 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 because 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); see 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.

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in 30.55 percent of all patient–years in our sample. Penicillins (J01 C) represent the largest share of prescribed antibiotics, followed by macrolides, lincosamides, and streptogramins (J01 F), and sulfonamides and trimethoprim (J01 E).

2. 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.

3. 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.[15] 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.[16] 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.

IV. 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

15. For instance, in the 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 and 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.

16. 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 Online Appendix C.

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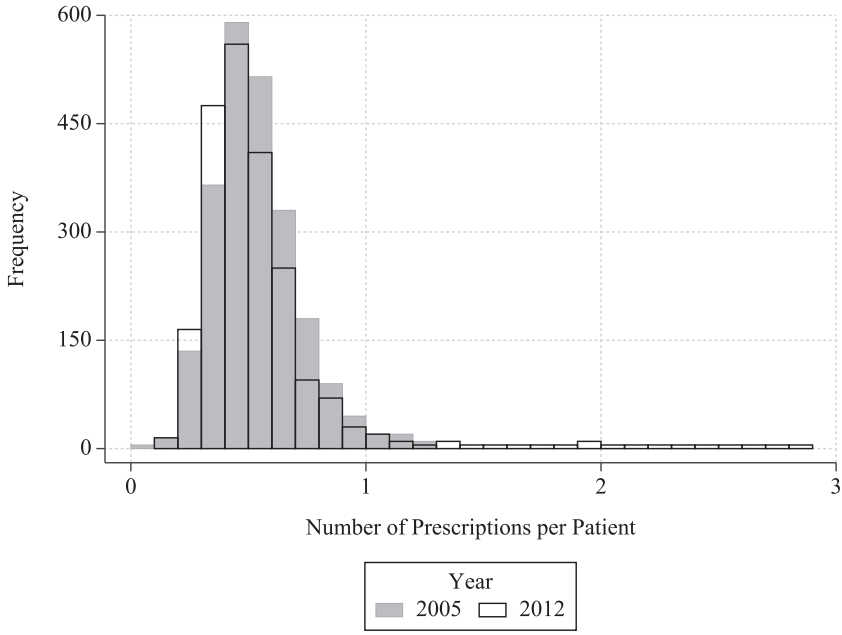

Figure 1

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.

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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.

A. 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.[17]

17. Online Appendix D.1 Figure A.1 shows the distribution of antibiotic prescribing over clinics for all sample years.

Table 2
Descriptive Statistics for Treatment and Comparison Group Patient

	Never Exposed to Physician Exit		Exposed to Physician Exit	
	Mean	SD	Mean	SD
Panel 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)
Panel 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 visits 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)
Panel 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)	0.031	(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 7–10	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	(0.06)	0.003	(0.05)
No education	0.001	(0.03)	0.001	(0.03)
Total observations (patient–years)	6,224,823		1,565,085	

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

B. 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.

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Table 3
Descriptive Statistics for Treatment and Comparison Group Clinics

	Without Physician Exit		With Physician Exit	
	Mean	SD	Mean	SD
Panel 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)
Panel 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	(0.05)	0.006	(0.07)
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)
Panel 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	1.00	(0.02)	0.999	(0.03)
Total observations (clinic–years)	4,023		1,254	

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, a lower number of interns, and they see fewer patients per physician than never-treated clinics. These differences can be largely explained by the fact that most physician exits are due to retirement.[18] While treated clinics have fewer patients per physician, staff sizes are similar as in never-treated clinics, with 1.49 physicians on average

18. The large majority of never-treated clinics absorb patients after a physician exit. Online Appendix D.2 Table A.2 shows descriptive statistics for these destination clinics, as well as for treated clinics prior to the physician exit. We observe that treated clinics have notably older physicians than never-treated destination clinics. One potential concern could be that our variation in prescribing mechanically represents shifts in provider age. However, Online Appendix D.2 Figure A.2 indicates that there is still considerable variability in the age differences between origin and destination clinics. We 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 and discuss the policy implications of observing generational shifts.

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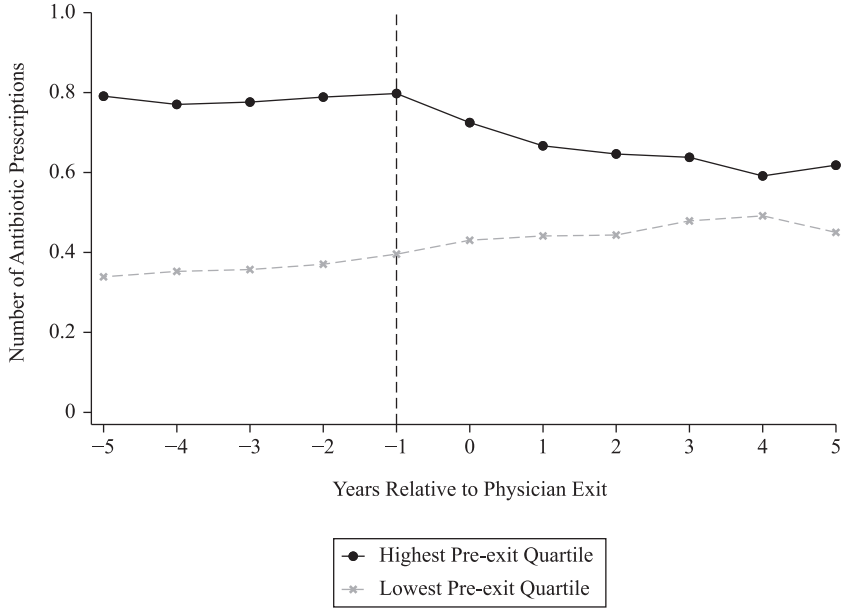

Figure 2

Antibiotic Prescribing by Clinics in High and Low Quartiles 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.

compared to 1.42. The majority of clinics in both groups are operated by a single physician, 72.7 percent of clinic–years without and 69.9 percent with a physician exit. In our overall sample, 89.6 percent of clinics are operated by no more than two physicians and 99.0 percent by no more than four physicians. Notably, there are only small differences in antibiotic prescribing between treated and untreated clinics and patients.[19]

C. 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

19. In Online Appendix D.3, we show summary statistics for excluded clinics. Given that we exclude clinics with 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 Online Appendix D.4, we also show summary statistics for patient–year observations that we drop during our sampling process, observing only small differences in antibiotic prescribing compared to in-sample observations.

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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.

V. Empirical Strategy

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

A. Identification

We discuss the identification of provider effects in the model in Section II 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:

$$y_{it}(1) = \alpha_i + \delta_{j'} + \mathbf{x}_{it}\beta + \epsilon_{it},$$
$$y_{it}(0) = \alpha_i + \delta_j + \mathbf{x}_{it}\beta + \epsilon_{it}.$$

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:

$$\theta = E\left[\frac{y_{it}(1) - y_{it}(0)}{\Delta_i} \,\middle|\, D_{it} = 1\right] \tag{3}$$

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 $e \in \{2006, \ldots, 2012, \infty\}$. 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.[20] 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.

20. We describe these assumptions for our setting in Online Appendix E.

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Assumption 1. Patients do not sort selectively to physicians based on antibiotic prescribing, $E\left[\frac{y_{it}(1)-y_{it}(0)}{\Delta_i} \;\middle|\; D_{it}=1,\Delta_i\right] = E\left[\frac{y_{it}(1)-y_{it}(0)}{\Delta_i} \;\middle|\; D_{it}=1\right] = \theta$. 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.

Assumption 2. The average treatment effect on the treated over cohorts is homogeneous for all time periods and cohorts, $E\left[\frac{y_{it}(1)-y_{it}(0)}{\Delta_i} \;\middle|\; E_i=e,\Delta_i\right] = E\left[\frac{y_{it}(1)-y_{it}(0)}{\Delta_i} \;\middle|\; D_{it}=1,\Delta_i\right] \forall t,e$. 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).

B. 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:

$$(4)\quad y_{it} = \widetilde{\alpha}_i + \theta \times D_{it} \times \hat{\Delta}_i + \mathbf{x}_{it}\beta + \varepsilon_{it},$$

where we measure antibiotic prescribing y_{it} by the number of prescriptions. The patient fixed effects $\widetilde{\alpha}_i = \alpha_i + \delta_j$ 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, $\mathbf{x}_{it}$ 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, $E[\epsilon_{it}|\widetilde{\alpha}_i, D_{it} \times \hat{\Delta}_i, \mathbf{x}_{it}] = 0$, based on the identifying assumptions in Subsection V.A, the estimate of θ represents the causal effect of provider effects on differences in antibiotic prescribing.

Second, we estimate an event study specification:

$$(5)\quad y_{it} = \widetilde{\alpha}_i + \sum_{r=-5,\, r\neq -1}^{r=5} \theta_r \times I_r \times \hat{\Delta}_i + \mathbf{x}_{it}\beta + \varepsilon_{it},$$

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 $\mathbf{x}_{it}$ 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.

VI. Results

A. 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.

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.[21] Provider shares 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.[22] These results

21. In Online Appendix F.1, we show comparable estimates when we measure antibiotic consumption by indicators for any antibiotic prescribing. In Online Appendix F.2, we observe that our results are also consistent when using the Daily Defined Dose prescribed, except in the case of other antibiotics, which might be due to large variation in average Daily Defined Dose per prescription between antibiotic classes of this subcategory. Furthermore, our main conclusions remain unchanged when we limit our analysis to single-physician clinics, as presented in Online Appendix F.3.

22. In Online Appendix F.4, 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.

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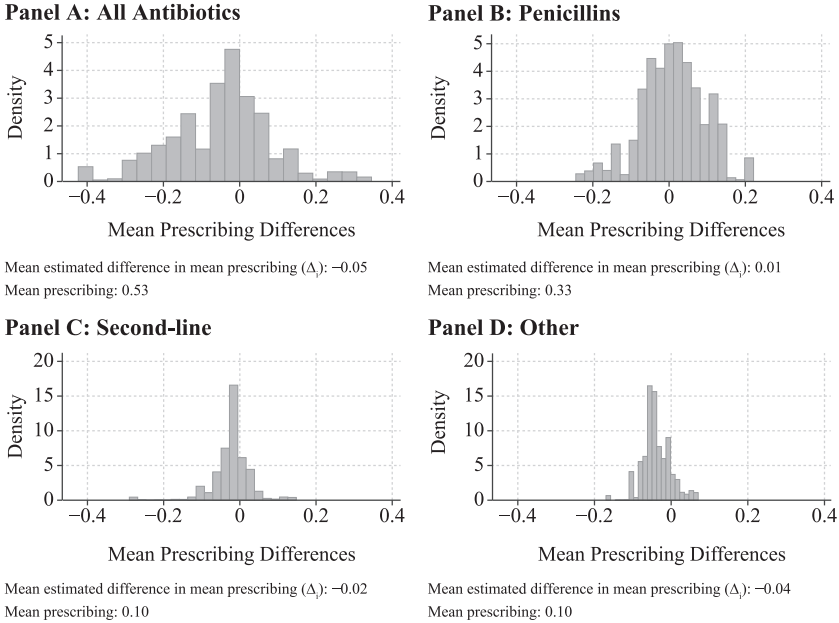

Figure 3

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 percent of values are winsorized, and values are bunched for groups of five patients with similar estimated mean difference in average prescribing.

show provider effects play a substantial role in explaining variation in antibiotic consumption in primary care.

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.

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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

Notes: This table reports the average share of antibiotic prescribing differences between clinics that is attributable to provider practice style differences, the coefficient of $\hat{\Delta}_i \times D_{it}$. 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.

This suggests that practice styles matter in particular for the composition of antibiotics prescribed. This finding is especially relevant, as the effects of antibiotic use on resistance vary by drug class, with broad-spectrum antibiotics having the most pronounced impact on resistance.

B. 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 reestimate 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 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 a control variable to capture short-term differences in antibiotic prescribing behavior. The results are consistent (Online Appendix G).[23]

23. Panel A of Online Appendix G Table A.14 displays the estimates for the static specification, and Online Appendix G Figure A.6(a) shows the results for the dynamic specification.

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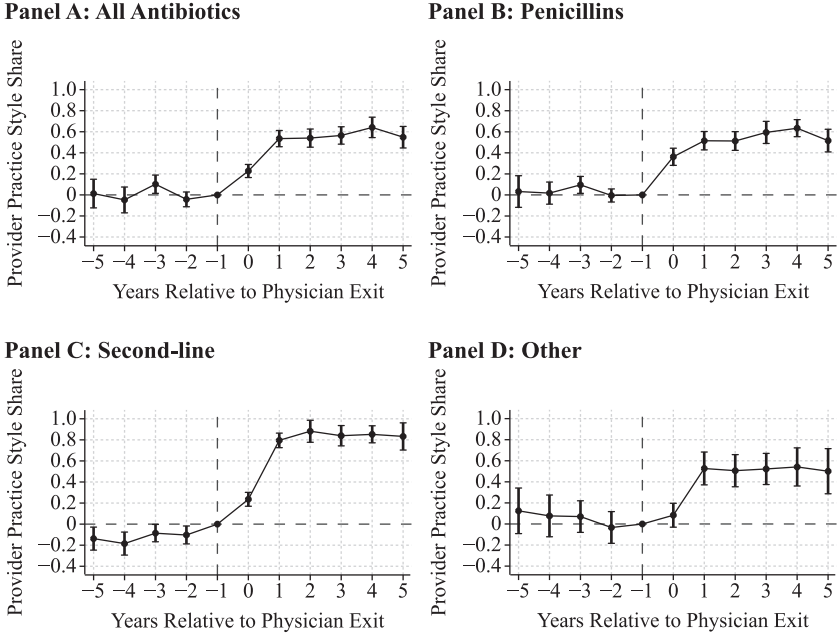

Figure 4

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 percent confidence intervals, with standard errors calculated using a bootstrap with 50 repetitions at the patient level.

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).[24] The interaction-

24. Specifically, we estimate the following specification: $y_{it} = \alpha_i + x_t + \sum_e \sum_r \theta_{e,r} \times I_e \times I_r \times \hat{\Delta}_i + \epsilon_{it}$, where x_t represents the time fixed effects, cohort e ∈ {2006, … , 2012, ∞} 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.

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Table 5
Selective Reassignment of Patients to Physicians Based on Observable Characteristics

	Difference in Average Antibiotic Prescribing ($\hat{\Delta}_i$)[a]			
	All Antibiotics (1)	Penicillins (2)	Second-Line (3)	Other (4)
Predicted prescribing	0.0016**	0.0020***	0.0011	0.0005
	(0.0005)	(0.0004)	(0.0010)	(0.0004)
Observations	413,663	413,663	413,663	413,663

Notes: 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.

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weighted treatment effect estimates are similar to our main results and shown in Online Appendix G.[25] 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.

C. 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 never-treated patients and 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-

25. Panel B of Online Appendix G Table A.14 shows the average interaction-weighted treatment effect estimates, and Online Appendix G Figure A.6(b) displays relative time-specific treatment effect estimates.

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 sorted 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.

VII. Characterizing Practice Style Heterogeneity

A. 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.[26] Clinic characteristics include the availability of point-of-care diagnostics and practice size.[27]

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 onto differences in standardized observable characteristics between pairs of physicians.[28] 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 and Chernozhukov 2013).[29]

26. For clinics with multiple physicians, we compute averages over physicians.
27. Online Appendix H.1 describes the variables in detail.
28. More details on our estimation procedure can be found in Online Appendix H.2.
29. We find similar results using a linear fixed effects regression approach as shown in Online Appendix H.3.

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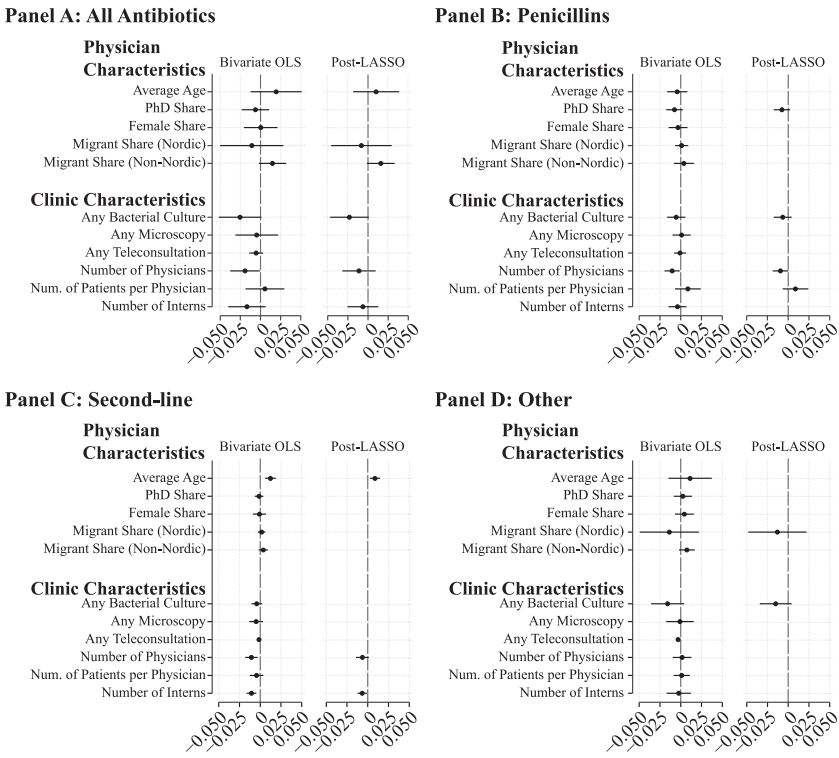

Figure 5

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 tenfold 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 95 percent 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 zero and standard deviation one prior to differencing.

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

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likely 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 more staff and 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.

B. 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.[30] 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 mismatch 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.[31] 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 for the number of all antibiotic prescriptions without the 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

30. We discuss our estimation procedure in detail in Online Appendix I.2.

31. The ATC 4 level segments pharmaceuticals into chemical-therapeutic-pharmacological subgroups.

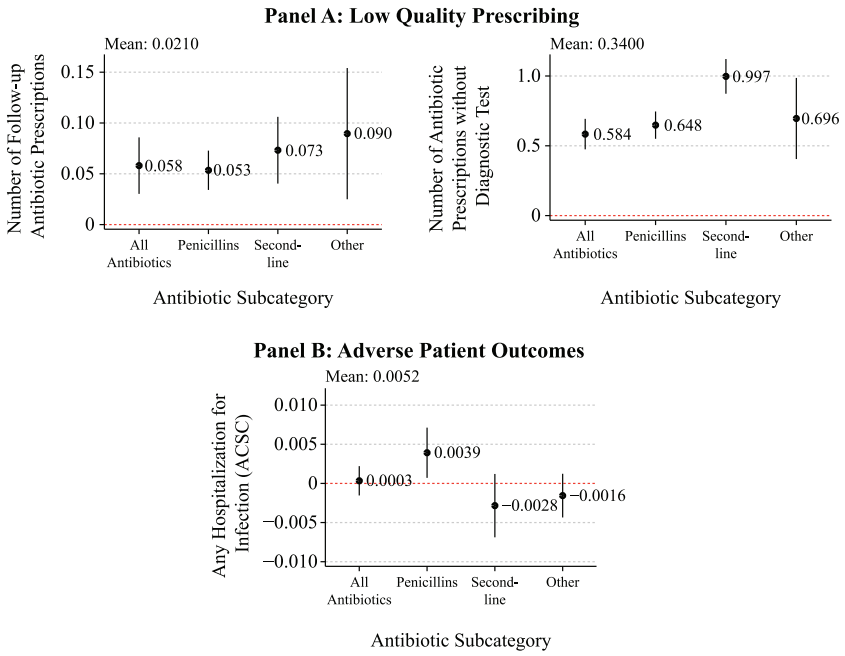

Figure 6

Quality of Care and Antibiotic Prescribing Intensity

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. Panel A 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). Panel B shows the change in adverse patient health outcomes 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 percent confidence intervals, with standard errors based on a parametric bootstrap with 50 repetitions at the patient level to draw differences in prescribing practice styles.

are indications that are potentially preventable under sufficient primary care and are commonly used to measure quality of care (for example, World Health Organization Regional Office for Europe 2016). We consider acute ACSC that can be caused by bacterial infections: skin and soft tissue infections, perforated or bleeding ulcers, urinary tract infections, pneumonia, and ear, nose and throat infections.[32]

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

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

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per patient per year, for all antibiotics as well as broken down by antibiotic subcategories.[33] Figure 6A shows that follow-up prescriptions are strongly positively associated with higher antibiotic prescribing intensity, with coefficient estimates between 0.053 and 0.09 markedly surpassing the average share of 0.021 for follow-up treatments among all prescriptions. 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 subjected to a prescribing style involving one more antibiotic per patient and year receive 0.584 more overall antibiotic prescriptions without any diagnostic test, which is nearly double the mean value of 0.34. The increase in prescriptions without diagnostic tests is even more pronounced, with 0.997 additional prescriptions, among patients exposed to physicians who prescribe one more second-line antibiotic. This suggests that diagnostic information is most lacking among providers with high prescribing styles of second-line antibiotics, which, notably, also represent the more socially critical antibiotic drug classes. 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 a common cause of 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 Online 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.[34]

VIII. Discussion

Our findings show that provider effects determine antibiotic use to a substantial degree. A policy that achieves complete standardization of practice styles would reduce differences in antibiotic prescriptions in primary care by one-half. Such standardization could result in unintended consequences if providers are prompted to under-use antibiotics for patients who require treatment. However, our empirical design alleviates this concern by holding the patient constant. We find no evidence that

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

34. Separate estimates for each ACSC in Online 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.

assigning patients to providers with lower antibiotic prescribing styles results in lower quality of care.[35] Thus, any variation in practice styles in our study may be interpreted as unnecessary from the patient's perspective.

Our definition of practice styles subsumes all factors, for example, variation in physician beliefs or skills, that could lead to heterogeneity in antibiotic prescribing. Thus, standardizing practice style would require high-prescribers to not only prescribe less indiscriminately but to fully emulate the practice styles of low-prescribers. This raises the question of how practice styles can be standardized in practice. Policy efforts to increase standardization may involve, for example, enhancements of guidelines, adjustments in medical education, increased emphasis on continuous training, and promotion of diagnostic technologies.

The dissemination of guidelines and their adherence have been demonstrated to prevent adverse health outcomes (Currie and MacLeod 2020; Abaluck et al. 2022; Cuddy and Currie 2026). Still, if physicians hold diagnostic information beyond the scope covered by guidelines, standardizing treatments may be difficult (Manski 2018). We find that standardization would most effectively reduce differences across clinics for second-line antibiotics, which target a broad spectrum of bacteria and are often used when providers fail to identify the pathogen causing an infection. Our results on provider characteristics and the quality of care are consistent with the importance of systematic differences in diagnostic tool use in explaining the larger role of provider effects in second-line antibiotic prescribing. In general, large variation in second-line antibiotic prescribing is also consistent with large heterogeneity in physicians' beliefs and social preferences regarding the increased social cost of second-line antibiotics in terms of antibiotic resistance. Thus, guidelines may be designed in particular to foster practice styles with more targeted antibiotic prescribing.[36]

Medical education can be another policy lever to promote desirable clinical practices. Doyle, Ewer, and Wagner (2010) observe that cost-effective practice styles of physicians are positively associated with medical school rank. Schnell and Currie (2018) show a negative relationship between school rank and opioid prescribing, which flattens for physicians who are educated about opioid treatments. Chan (2021) finds that substantial learning occurs in physicians' early careers, while Molitor (2018) shows this is even the case for mid-career cardiologists. These findings imply that (continued) training may help to standardize practice styles.

We cannot identify the role of medical education for practice styles throughout the career but we observe that generational differences between providers may be important. Table 3 and Online Appendix D.2 Figure A.2 show that patients often switch from older to younger physicians. To investigate this, we replicate our main analysis using a subsample that includes only patients switching to physicians who are less

35. Our results even suggest that patients assigned to low-prescribing providers receive treatments that reflect better clinical decision-making and lower rates of avoidable hospitalizations.

36. During our study period, no national guidelines on antibiotic prescribing existed in Denmark. In November 2012, national antibiotic prescribing guidelines were defined for the first time, VEJ no. 10126 of 15/11/2012. A "national action plan" was further published in 2017 (https://sum.dk/publikationer-sundhed/2017/juli/national-handlingsplan-for-antibiotika-til-mennesker, accessed January 22, 2026). These initiatives aim for standardization in prescribing styles by emphasizing penicillins as first-choice treatments in most conditions and advocating for more clinical testing.

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than one-half standard deviation younger or older than the exiting physicians. Online Appendix J Table A.18 shows that excluding substantial shifts in assigned physician age leads to effect sizes that are approximately one-third smaller than for the full sample, with provider shares dropping from 49.4 to 35.9 percent. While large practice style effects remain even after excluding such age differences, this finding suggests public policy may affect antibiotic prescribing through the medical education system (Pulcini et al. 2015), both at the beginning of and throughout physicians' careers.[37]

Finally, accelerating the diffusion of (new) diagnostic technologies has the potential to reduce variation in prescribing practice styles by compressing the diagnostic skill distribution (Chan, Gentzkow, and Yu 2022). Improved, standardized use of diagnostics has been shown to enhance and harmonize patient care (Abaluck et al. 2016; Agha, Skinner, and Chan 2022; Ribers and Ullrich 2024). Much hope is placed on technological advancements in artificial intelligence, which has been documented to produce large productivity gains in particular at the lower end of the skill distribution (Gruber et al. 2020; Agarwal et al. 2023). Stimulating the use of new technologies may thus be a further promising avenue to reduce inefficient variation in antibiotic prescribing.

IX. Conclusion

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

We quantify the extent to which provider practice styles, as opposed to differential patient demands, determine variation in antibiotic consumption in primary care. In addition, we characterize potential drivers of antibiotic prescribing styles and whether practice style heterogeneity contributes to improved healthcare quality. Our study reveals that reducing overall and, importantly, second-line antibiotic consumption may be achieved by targeting individual physicians without compromising patient health.

To design effective policies in primary care, further research is urgently needed to investigate the specific nature of practice styles linked to varying levels of antibiotic prescribing intensity. Pressing avenues for further research, extending beyond the scope of our framework, involve examining the interactions between prescribing

37. One such example in Denmark was the establishment of the Danish Integrated Antimicrobial Resistance Monitoring and Research Program (DANMAP) in 1995, which not only introduced surveillance of antibiotic resistance within and outside of primary care (Hammerum et al. 2007), but in addition, offers training courses on antibiotic stewardship (https://www.ecdc.europa.eu/en/all-topics-z/antimicrobial-resistance/directoryguidance-prevention-and-control/training-courses, accessed January 22, 2026). Furthermore, the national guidelines on antibiotic prescribing introduced in November 2012 directly address the institutions that train physicians: "The Authority requests that relevant clinical societies, universities, pharmaceutical committees, administrators and others incorporate the new rules into the applicable guidelines, textbooks, local instructions, etc." (VEJ no. 10126 of 15/11/2012).

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styles and heterogeneous patient needs, as well as understanding how physicians learn and adopt recommended practices and technologies. Measuring the implications of practice styles for health outcomes is key to ensuring that policies enhance efficiency and avoid unintended adverse effects.

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