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Abstract

While it is widely recognized that treatment choices of health care providers vary across similar patients, reasons for this remain poorly understood. This paper estimates the spatial evolution of opioid prescription leniency of health care providers. Using exits and entries of primary care providers into local markets I document spillover effect in opioid prescription leniency across primary care practices. My results imply that an increase in opioid leniency of 1 standard deviation of a random provider peer increases the leniency of the focal provider by 7% of a standard deviation. Finally, I apply the network model to estimate how increased opioid use is harmful to patients' labor market outcomes.

JEL Classification: I11 ; I12

Keywords: Labor market; Opioids; Provider Practice Style

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1 Introduction

This paper documents spillovers in general practitioners' tendency to prescribe opioids. According to the OECD, the availability of prescription opioids has been increasing in western countries in recent decades (OECD (2019)). The United States has been particularly affected by the consequences of opioid use. From 1999 to 2019, it is estimated that the opioid crisis has claimed over 500,000 lives in the United States alone, and recent estimates suggest that 136 lives are lost each day due to the pandemic(CDC (2021)). The increase in overdose deaths affects individuals of all genders, ages, and racial groups, leading medical professionals, public figures, and politicians to declare the opioid epidemic a national public health emergency (Dart *et al.* (2015)). This paper explores an important mechanism behind these trends.

There is broad consensus that healthcare providers are important in the dissemination of opioids. Overall, the existing literature distinguishes between three waves of opioid related deaths in the US. While the latter two waves were driven by illicit drugs, the first which began in the late 90's were primarily driven by legally prescribed analgesics (Maclean *et al.* (2022),CDC (2021)). The progression from prescription to illicit opioids is also reflected in the extensive literature documenting how legally obtained opioids often precipitate the transition into illegal substance use (Jones *et al.* (2013), Muhuri *et al.* (2013)). Despite the severe risks associated with opioid use, extensive work documents large and persistent differences in opioid prescription leniency - the propensity to prescribe opioids for a fixed set of patient characteristics - across providers and geographical regions(Finkelstein *et al.* (2018)). Hence; despite potentially adverse consequences of opioid use, whether you get prescribed opioids is to some extent the result of where you live, and which health care provider you see. The high levels of prescribed opioids have been met by initiatives targeting the providers with the aim to reduce the amount of opioids prescribed. These efforts include but are not limited to state-wide regulations such as prescription drugs monitoring programs, pain management practice laws and doctor shopping laws. (Maclean *et al.* (2022), Kilby (2016), Weiner *et al.* (2017)).

To estimate the spatial spill-over effect I define a non-overlapping complete network of the universe of practices in Denmark over the year 2004-2010. I leverage within practice variation in peer sets to estimate a spatial spill-over effect by using excluded peers - peers of peers that are not directly related to the focal practice - to identify the spill-over effects(e.g. Bramoullé *et al.* (2009),De Giorgi *et al.* (2020)). Using detailed Danish data on all prescription drug claims and within-practice variation in the composition of neighboring practices, the study demonstrates that primary care practices located near high leniency practices are more lenient themselves. I find that providers are more influenced by similar, spatially close peers. The finding that providers with lenient peers are more likely to be lenient themselves has important policy implications for understanding how to reduce the overall number of opioids prescribed. On the one hand, the presence of inter-practice spill-overs exacerbates the negative impact of lenient prescribers, as high leniency of a provider not only affects the patients of that particular provider, but also indirectly influences patients affiliated with other providers. On the other hand, the existence of spill-overs also provides an opportunity for interventions to address the opioid epidemic, as targeting and removing a high leniency prescriber can have a wider impact beyond the patients of that particular provider.

Previous work has identified several potential channels for spatial spill-over effects. First, as is shown in a recent literature on competition and provider behavior, inter-practice competition is a potential driver of spill-overs. Markussen and Røed (2017) estimate the impact of competition for patients on the provider leniency in approving sickness absence. They find that providers on a fixed-fee contract to a lesser extent respond to the intensity of local competition than providers reimbursed on a per capita basis. Their findings are supported by Brekke *et al.* (2017) who show that exposing the same physician to environments with less competition decreases the amount of approved sickness absence. Gravelle *et al.* (2018) further supports spatial competition as a source of spill-over, as they find that increased competition among English primary care physician practices improves quality. In line with these previous findings, one could expect that providers respond to high opioid prescription leniency of peers, by increasing their own leniency to retain or attract patients.

Alignment of leniency could also be the result of knowledge spill-overs. Yang *et al.* (2014) provide evidence of this, as they estimate positive peer-effects within-hospital physicians. Closest to my setting, Nosal (2016) identifies peer effects in adoptation of new technology as primary care physicians work in several practices. Hence, inter-collegial discussions of opioid treatment behavior is another plausible gateway to leniency spill-overs. [1]

Finally, Burke *et al.* (2010) propose that regional treatment alignment is a result of spill-overs in patient preferences as e.g. neighbours or coworkers would discuss the treatment style of their primary care providers and providers would receive signals of this, through the patient demands.

Estimating the effect of opioid use on socioeconomic outcomes has proven challenging. This is despite researchers having allocated substantial efforts towards studying the relationship between opioid use and health, health care utilization, crime, and labor market outcomes (Eichmeyer and Zhang (2021) Maclean *et al.* (2022). While there is ample evidence - mostly from evaluating prescription monitoring programs - that increased use of opioids negatively impacts both health and health care outcomes; the evidence on labor market outcomes are more scarce. In this paper I deepen the understanding of this relationship. After establishing that spillovers constitutes an important component in determining opioid prescription leniency, I rely the estimated spill-over model to shed light on the twoway deadlock between employment and opioid use. The estimated time-varying opioid leniency attributable to whether providers in the immediate vicinity practice relatively more or less aggressively offers an instrumental variables candidate. For fixed patient and provider characteristics - including a provider fixed effect - I estimate the impact of opioid treatment intensity on a series of labor market outcomes. Conceptually, this simulated IV strategy resembles that of Currie and Gruber (1996) who simulate changes in state level mean eligibility to health insurance from a Medicaid Expansion, and Chetty *et al.* (2014), who estimate the impact of tax credits on labor supply by modelling local knowledge of the earned income taxed credit.

Overall, this paper contributes to two branches of literature. First, I add to a large literature on provider practice style. It is well documented that providers vary in their treatment choices (e.g. Epstein and

[1] In a Danish setting with primary care physicians operating as self-employed entities, Jansbøl *et al.* (2012) interview primary care physicians and find that experience sharing across practices is a primary source of updating knowledge regarding new treatments and clinical guidelines. That Danish primary care physicians rely on physicians outside the practice is further supported by Kjellberg (2012), who concludes that in 11% of the consultations physicians interact with clinicians outside the practice.

Nicholson (2009),Grytten and Sorensen (2003), Cutler *et al.* (2015), Koulayev *et al.* (2017), Fadlon and Van Parys (2020), Simeonova *et al.* (2020), Simonsen *et al.* (2019)). Recent work suggests that the provider style is not static but is dynamic in nature(e.g.Molitor (2018), Silver (2019)). I add to this literature by demonstrating how the provider practice style evolves spatially and temporal across primary care provider practices. My estimates imply that if a random peer of the focal provider increases prescription leniency by 1 standard deviation, the focal provider increases her prescription leniency by app. 7% of a standard deviation. This effect is robust to various weightings of peers and specifications of network relations.

Secondly, I add to a literature on consequences of provider heterogeneity. Several papers have linked provider heterogeneity to patient outcomes (e.g., Currie *et al.* (2016), Currie and MacLeod (2020) Simeonova *et al.* (2020)) and the link between health and labor market performance is well established (e.g., Grossman (1972), Case and Deaton (2005) and Smith (2005). Specifically recognizing that treatment of pain is important for labor market outcome is evident from empirical work by Garthwaite (2012) and Butikofer and Skira (2015). Both papers present evidence that reducing pharmaceutical pain coverage is associated with lower employment rates and higher exit rates to disability. There is a new and rapidly growing literature shedding light on the relationship between opioid use and labor market outcomes. In addition to observational studies, the overarching identification strategies span either state-level reforms to access of opioids (Kilby (2016)) or regional variation in opioid prescription level (Harris *et al.* (2017), Laird and Nielsen (2016) and Currie *et al.* (2018)). In this paper I rely on the network model to estimate a simulated provider opioid leniency, which I - in the sprit of e.g. Currie and Gruber (1996) and Chetty *et al.* (2014) - utilize in a simulated instrumental variable setup to estimate the impact of opioid treatment intensity on labor market outcomes for patients. I find that an increase of 1 standard deviation in simulated provider leniency increases opioid treatment intensity by app. 1.5%, and find that on the mean an increase of 10% in opioid treatment intensity significantly reduces labor market income ranking by 0.43 percentage points and decreases the probability of employment by approximately 1 percentage points.

The rest of the paper is organized as follows: Section 2 reviews the existing literature and presents the institutional setting. Section 3 presents the data and descriptive statistics and section 4 lays out the empirical strategy. Section 5 presents the results . Finally section 7 concludes.

2 Institutional setting and Provider spill-overs

2.1 Primary Care in Denmark

All Danish citizens are covered by the Danish universal health care system. This tax-funded system is organized in five regions that supply the health care. They run and manage all in-patient care and hospital based out-patient care. Primary Care Providers operate as private entities and act as gatekeepers to the rest of the public health care system. Physicians are organized in practices, which can house several practising physicians.[2] All primary care providers, practicing specialists, physiotherapists etc. are licensed by the

[2] The level of provider observation in this paper is the practice. This is because while the data contains information on which physicians work in each practice, I only know which practice the patient is linked to. A practice can have several physicians,

state, and are reimbursed based on terms established in collective bargaining agreements between a national board for wages and tariffs and the organizations representing the respective profession. Central to this paper, the geographical dispersion and location of practices are controlled by the regions through the allocation of practising licenses [3]. Furthermore, once the practicing license is obtained by a provider, the regions cannot decide to revoke a license e.g. to reallocate practices to areas with low provider to population ratios.

To be able to obtain care under the publicly funded health insurance, patients must consult a physician at the practice with which they are enlisted. The set of practices that individuals can choose to enlist with, is restricted by whether the practice is closed for take-up of new patients and geographical distances from their residence. Practices are capped by 2,500 patients per physician, and after reaching 1,600 per physician the practice can state that it is closed for take-up. The scope for supply side selection is very limited: A practice cannot legally prohibit patients from choosing it as primary care provider as long as the practice is not closed for patient entry. Furthermore, a patient can only in very extremes cases e.g. violence or threats be excluded from practice.[4] Patients in rural areas must choose a practice within 15 km of their residential address. Patients in more densely populated areas must choose within 5 km. Once individuals have chosen their primary care provider, there is a token fee of 150 DKK - equivalent of app. 25 dollars - associated with shifting to another one. This geographical anchoring of practices effectively defines a *catchment area* for each practice. All individuals living within 15km (5km in cities) constitute the set of *potential patients*. These are the individuals who can choose this particular practice if it is open for take-up. The set of potential patients is obtained for each practice from a unique dataset measuring distances from all primary care practices in Denmark to the residential address of each individual in Denmark[5]. This allows me to relate all primary care practices in Denmark to each other spatially.

After obtaining a prescription from your primary care provider, prescription drugs are administered through licensed pharmacies. These are private entities, but prices faced by patients are identical across the country as they are determined nationally in a bi-weekly auction between pharmaceutical companies. The co-insurance ranges from 0% to 85% - with a 100% coinsurance for catastrophic coverage or chronics - in discrete steps that depends on the total expenditure in the current re-imbursement year. For details on the scheme see e.g. Simonsen *et al.* (2016).

2.2 Sickness Pay, Disability and Pensions

From 2004-2010 the Danish Labor market had several income insurance schemes depending on your age, health, and job status. Individuals older than 65 are eligible for the public state pension, which is a fixed transfers irrespectively of your assets or the working status of your spouse. From the age of 60, individuals can choose to go on early retirement, which is a lower transfer than the public state pension.

Individuals that fall ill are eligible for short term disability regardless of their employment status. The

but physicians cannot work at several practices. In interviews with Danish primary care providers Jansbøl *et al.* (2012) find that within practice treatment variation is seen as an undesirable feature, which physicians are actively trying to minimize.

[3]However, once obtained, the licence to practice medicine in a certain area can be resold to other physicians

[4]This happens very rarely. A total of 458 patients experienced this in 2017.

[5]I am grateful to Sergei Koylayev, Emilia Simeonova, and Niels Skipper for letting me use this dataset

employer is liable for the short term disability for the initial weeks, before the state takes over the liability. In case the individual is deemed not able to return to an employable state, one can apply for Disability Insurance. Disability insurance provides a constant stream of income every year until individuals choose to go on early retirement, or are eligible for the public state pension.

3 Practice networks & Data

3.1 Opioid-users & Primary Care Practice - Patient link

I use Danish administrative data covering the years 2004 - 2010 to construct a dataset contains yearly information on individuals with at least one opioid claim, the total number of defined daily doses each year, and a link between individuals and their prescribing practice. I obtain this information from the Danish Pharmaceutical Database, which contains information on the entire universe of prescription drug purchases in Denmark. I limit the sample to individuals who only obtain opioids from a single provider in a given year.[6] I further augment the prescription data with information on demographics(age, income, education, gender) and comorbidities calculated as an aggregated Charlson-Comorbidity Index as of December 31st each year (Quan *et al.* (2005)) [7]. As measure of labor market income I calculate the labor market income rank, constructed as an average on cohort, gender and time level - similar to that implemented in Chetty *et al.* (2014).

Table I presents descriptive statistics of opioid-users in Denmark by year. The gender and age distribution of opioid users are stable over time, which also is the case for the distribution of educations. Individuals with no education exceeding primary school and individuals with vocational educations constitute the majority of users. The number of opioid users living with 1 or more comorbidity has increased. The average opioid user have remained under the median in the income distribution across my observation period. The mean defined daily doses claimed by the opioid users over the entire period is 80.24 (std dev of 189).

Table II contains descriptive information on the practices included in the sample. The first panel describes the evolution of the organizational characteristics of practices. From 2004 to 2010 fewer practice are solo practices, more often the practice has a female physician working there, and the average number of patients associated with practices have increased. The (practice specific) physician density, which is calculated as the number of practices the average potential patient of each practice can choose, remains stable. Panel B of the table includes data on the composition of potential patients. These are the characteristics I use to control for general trends in the area, which might be driving co-variation in prescription behaviour. The number of focal practices, identical to the number of distinct yearly networks, fluctuates some but remains above 2000 distinct practices each year.

[6] In this step I to drop 4.8% of the individuals with an opioid prescription.

[7] I implement a dynamic version, where hospital admissions are carried over each year, to capture a development in mortality risk as well as a level. I lag the index one year relative to the observation period to avoid simultaneity in the determination of health stock and opioid consumption.

Table I. Descriptive statistics for opioid users by year

Year		2004	2005	2006	2007	2008	2009	2010
Age (0/1)	-30	0.08	0.08	0.07	0.07	0.07	0.07	0.07
	30-40	0.16	0.16	0.15	0.15	0.14	0.14	0.14
	40-50	0.22	0.23	0.23	0.23	0.23	0.23	0.23
	50-60	0.29	0.28	0.28	0.27	0.27	0.27	0.26
	60-70	0.25	0.26	0.27	0.28	0.29	0.29	0.29
Education (0/1)	Pri School	0.42	0.41	0.40	0.39	0.39	0.38	0.37
	Sec. School	0.03	0.03	0.03	0.03	0.03	0.03	0.03
	Vocational	0.34	0.34	0.34	0.35	0.35	0.35	0.36
	Short Ter.	0.17	0.17	0.18	0.18	0.18	0.19	0.19
	Long Ter.	0.05	0.05	0.05	0.05	0.05	0.05	0.05
CCI (0/1)	0	0.77	0.75	0.74	0.72	0.71	0.70	0.69
	1	0.13	0.14	0.15	0.15	0.16	0.16	0.16
	2	0.06	0.06	0.07	0.07	0.07	0.08	0.08
	3	0.02	0.02	0.02	0.03	0.03	0.03	0.03
	4	0.01	0.01	0.01	0.01	0.01	0.01	0.01
	5+	0.01	0.01	0.01	0.02	0.02	0.02	0.02
Male (0/1)		0.43	0.43	0.43	0.43	0.43	0.43	0.43
Defined Daily Doses		77.2	76.9	78.7	80.0	82.4	83.0	82.5
		(185.78)	*(185.78)*	*(185.78)*	*(185.78)*	*(185.78)*	*(185.78)*	*(185.78)*
Income Percentile Rank		0.383	0.385	0.386	0.389	0.386	0.386	0.387
		(0.255)	*(0.256)*	*(0.256)*	*(0.258)*	*(0.256)*	*(0.255)*	*(0.255)*
Any Sickness Absence		0.16	0.17	0.18	0.17	0.17	0.17	0.16
Short term disability		114.8	120.5	133.8	136.6	136.7	136.7	132.4
(Conditional on any STD)		*(115.9)*	*(118.9)*	*(129.7)*	*(129.1)*	*(131.2)*	*(126.2)*	*(123.5)*
Observations		153,679	164,206	171,614	176,956	180,597	184,591	194,699

Notes: Descriptive statistics for opioid users by year. Age is a dummy for age in a specific interval. Education is a dummy for each category of education. CCI is a dummy for the dynamic Charlson Comorbidity Index taking a specific value. For the continous variables I also report the standard deviation. Defined Daily Dosis is measured in the contemporary period. Income percentile rank and sickness absence is measured at period t+1.

3.2 Practice Networks

In this section I describe how I use data on distance from practice to individual addresses to construct a complete, non-perfectly overlapping network, where I for each practice can construct a distinct sub-network of diameter three. Defining practice networks in Denmark is a non-trivial task. Previous work on peer effects has relied on affiliation with predefined meaningful entities such as classrooms, workplaces or municipalities (Sacerdote (2014)). As no such natural entity exists for primary care practices in Denmark, I define peers of primary care practices in terms of spatial vicinity. The logic is in line with previous work documenting a geographic dimension in provider spill-overs (e.g. Markussen and Røed (2017),Gravelle *et al.* (2018)). Given the geographical restrictions in provider choice of Danish patients, each practice has a fixed set of *potential patients* - patients able to choose a specific provider. This set of potential patients defines the *catchment area* of a practice. Subsequently, I define peers as practices that have overlapping catchment areas[8] To operationalize this concept, I define as a *catchment area* for each

[8]This definition creates a tangible *closeness*/ *proximity* ranking of peers, as measured by the extent of overlap of catchment areas. This ranking will be a part of the extensive sensitivity checks I expose my results to.

Table II. Practice level descriptive statistics

Panel A: Practice Characteristics							
Year	2004	2005	2006	2007	2008	2009	2010
Single Prac.(0/1)	0.66	0.65	0.64	0.62	0.59	0.55	0.58
Any Female (0/1)	0.43	0.45	0.47	0.49	0.51	0.53	0.53
Prac . Dens.	38.8	39.0	39.0	40.0	39.1	39.3	39.1
	(13.1)	*(13.1)*	*(13.1)*	*(12.7)*	*(13.0)*	*(13.1)*	*(13.2)*
Patients	2,431	2,429	2,444	2,461	2,495	2,545	2,604
	(1,565)	*(1,571)*	*(1,602)*	*(1,621)*	*(1,654)*	*(1,677)*	*(1,698)*
Peers	152	153	153	157	154	154	153
	(60)	*(60)*	*(60)*	*(58)*	*(59)*	*(59)*	*(60)*
Clinics	2,170	2,128	2,118	2,110	2,072	2,046	2,023
Panel B: Patient Characteristics							
Year	2004	2005	2006	2007	2008	2009	2010
Male (0/1)	0.48	0.47	0.47	0.47	0.47	0.47	0.47
Immigrant (0/1)	0.08	0.08	0.09	0.09	0.09	0.10	0.10
Income	287,482	290,634	295,446	301,864	302,584	294180	297,353
	(33,433)	*(35,073)*	*(34,235)*	*(35,051)*	*(35,442)*	*(31,957)*	*(36,953)*
PCP Expenditures	53.1	53.12	54.9	56.3	57.8	60.2	60.9
	(3.51)	*(3.52)*	*(3.60)*	*(3.88)*	*(3.96)*	*(3.46)*	*(3.35)*
Age	48.36	48.55	48.71	48.87	48.88	48.92	48.98
	(2.81)	*(2.88)*	*(2.91)*	*(2.92)*	*(3.06)*	*(3.16)*	*(3.31)*
CCI	0.01	0.01	0.01	0.01	0.02	0.04	0.06
	(0.003)	*(0.005)*	*(0.007)*	*(0.01)*	*(0.01)*	*(0.01)*	*(0.01)*
Potential Patients	89,458	90,377	91,432	90,250	93,414	95,017	96,362
Common Patients	7,916	8,104	8,297	8,051	8,596	8,771	9,058
Overlap	0.089	0.090	0.091	0.089	0.092	0.092	0.094

Notes: Practice level descriptive statistics. Panel A contains information on organizational level characteristics. Panel B contains information on average patient characteristics of all patients who live in the treatment area of the practice .

provider, the 15km catchment radius surrounding the practice where residing individuals potentially can choose that particular practice as the site of their primary care[9]. First-level peers are practices who have overlapping catchment areas in a given year -that is they have at least one patient who can choose either practice. Having identified first-level peers for each practice, second level peers are identified for each

[9]5km for non-rural practices

focal provider, by identifying first level peers of first level peers that are are not first level peer to the focal practice. Similarly third level peers are obtained by identifying first level peers of second level peers that are not first level peer to the focal practice nor first level peer to the first level peers of the focal practice.

4 Empirical Strategy

In this section I present my empirical strategy. This consists of two parts. First I lay out how I separate practice prescription leniency from patient demand. Second, I describe how I construct practice networks and use temporal variation in excluded provider peers to estimate how opioid prescription leniency interact over time across connected provider practices.

4.1 Prescription Leniency

Prescription leniency is the parameter that determines the degree to which similar patients obtain different opioid treatment intensity, solely due to being affiliated with different practices. To get a measure that controls for individual specific components as well as allow for correlation between these components and the prescription leniency, I follow previous literature and model the intensity of opioid treatment for an individual in a given year, O_{it}, as a linear function of an individual specific component α_i, time-varying individual covariates X_{it}, the opioid leniency of ones primary care practice γ_{jt} and with idiosyncratic shocks ω_{it} capturing temporal individual shocks not related to the practice. The prescription leniency is allowed to vary across time as I wish to estimate a dynamic treatment style:

$$O_{it} = \alpha_i + \gamma_{jt} + \mathbf{X}_{it}\beta + \omega_{it}, \tag{1}$$

This linear model with two-sided heterogeneity has been frequently implemented in the labor literature (see e.g. Abowd *et al.* (1999),Bagger *et al.* (2013), or Card *et al.* (2015)). In the field of health economics, Markussen and Røed (2017), Finkelstein *et al.* (2016) and Finkelstein *et al.* (2018) are recent examples of the utilization of the two way fixed effects model. [10]

The model exploits that over time individuals are observed at different practices across time to simultaneously estimate distributions of individual and practice contributions to opioid consumption(See Abowd *et al.* (1999) or Lachowska *et al.* (2020) for details on the identification of the model)[11].

[10] Markussen and Røed (2017) estimate the model to investigate the impact of competition on practice leniency in approving sickness benefits for Norwegian workers, and Finkelstein *et al.* (2018) uses the model as a basis for decomposing the share of person- and place-specific factors in opioid abuses.

[11] Unbiasedness of α_i and γ_{jt} hinges on the absence of particular mobility patterns. In particular, I assume that match-specific components are not present. This implies that while α_i and γ_{jt} are allowed to correlate, individual patients are assumed not to select practices based on gains for that specific individual others would not have achieved. Limited mobility bias - a lack of identifying shifters - is another problem that might lead Abowd *et al.* (1999) show that γ_{jt} is only identified relative to an arbitrarily set reference practice such that prescription leniency of practices is measured relative to each other within a given year.

4.2 The Network of Practices and Identification of Peer Effects

Any researcher attempting to estimate peer effects is faced with two inherently intertwined problems: Properly defining peers and identifying exogenous variation within the defined set of peers (Angrist (2014),Sacerdote (2014)). As the catchment areas are not perfectly overlapping, any focal practice will have peer practices that have peers not in the peer-set of the focal practice. This type of peers are referred to as *second level peers* or *excluded peers* (Bramoullé *et al.* (2009)). As this concept easily generalizes to 3rd level peers (and even further if need be), I can construct a complete non-overlapping network to separate the endogenous peer effects from the contextual peer effects following a recent literature on networks (e.g. Bramoullé *et al.* (2009),Boucher and Fortin (2015), Paula (2020)). Conditional on the existence of excluded peers Bramoullé *et al.* (2009) show how contextual characteristics of the excluded peers can be used as instruments of the endogenous peer effect for the focal individual. This strategy has been applied to estimate peer effects in e.g. labor market participation (Nicoletti *et al.* (2016)), Consumption (De Giorgi *et al.* (2020)) and academic achievement (De Giorgi *et al.* (2010)).

While the application of the network-structure allows me to estimate the *leniency multiplier*, variation in peers that is plausible orthogonal to the behavioural leniency decision of the focal practice is required to identify a causal leniency multiplier(Angrist (2014)). To achieve this I restrict my analysis to within network variation in practice peers. This variation originates from entries and exits of practice in local markets [12].

Figure A.1 illustrates the structure of the practice networks and how exits (or alternatively entries) of practices can lead to variation in the third level peers used for identification. Panel A depicts a sample network for focal practice A. The blue dotted circles indicate practice specific catchment areas[13]. Solid lines indicate first level connections, dashed lines indicate second level connections, and dotted lines indicate third level connections. In period t, practice A has three first level peers (B,C, and D), three second level peers (F,H, and G) and two third level peers(E and I). In period t+1, practice C decides to close. This reduces the number of third level peers in practice A's network to one (I). Ultimately, the exit of practice C leads to temporal variation within-practice A in the characteristics of third level peers, which is used to identify the endogenous peer effect.

To formally measure the impact of provider networks I estimate the following model: Let F_{jt}, S_{jt} and T_{jt} denote the set of first-level, second-level, and third-level peers respectively. Let $\hat{\gamma}_{jt}$ be the estimates obtained from equation (1). The traditional linear-in-means model (Manski (2000)) relates the outcome of the focal practice to behavior of peers.

$$\hat{\gamma}_{jt} = \beta_1 \overline{\gamma}_{F_{jt}} + \overline{\mathbf{X}}_{F_{jt}} \beta_2 + \mathbf{X}_{jt} \beta_3 + \alpha_j + \overline{\mathbf{X}}_{P_{jt}} \beta_4 + \delta_t + \varepsilon_{jt}. \tag{2}$$

That is the leniency of the focal practices , $\hat{\gamma}_{jt}$, is modelled as a function of average peer prescription leniency $\overline{\gamma}_{F_{jt}} = \frac{1}{\#F_{jt}} \sum_{h \in F_{jt}} \hat{\gamma}_{ht}$, average first level peer contextuals, $\overline{\mathbf{X}}_{F_{jt}}$, own contextuals, $\mathbf{X}_{jt}$, and time

[12] Entries are highly limited by the government issued licenses, but the exits are due to a wide range of reasons - the most prevalent being provider retirement. As part of the extensive robustness analysis I do, I investigate the sensitivity to the type of exits, and find qualitatively identical results.

[13] For simplicity they are only presented for a selected set of practices

dummies, δ_t. The contextuals are practice characteristics that should predict leniency. These include i) whether the practice is a single physician practice, ii) whether there are any females providers operating in the practice iii) the number of patients affiliated with the clinic, iv) and the practice density measured as the mean number of practices the potential patients can choose among[14]. To restrict the analysis to within network variation in practice peers, my preferred specification includes a practice fixed effect, α_j.

A naive OLS estimate of β_1 in equation (2) would suffer from the reflection problem in opioid leniency among peers. To circumvent this simultaneity, I instrument $\overline{\gamma}_{F_{jt}}$ by $\overline{\mathbf{X}}_{kt}$ where$k \in T_{jt}$. Bramoullé *et al.* (2009) show that instrumenting the endogenous peer effect $\overline{\gamma}_{F_{jt}}$ with exogenous contextual characteristics of third level peers $\overline{\mathbf{X}}_{T_{jt}}$ solves the reflection problem: consider the evolution in third level peers for practice A in figure A.1. β_1 in equation (2) is identified off the change in third level peer characteristics. In period 1 this is practices I and E. In period t+1 this is only practice I.

Even though I estimate the peer effects based on plausible exogenous changes in peer compositions, one might be concerned about local trends in omitted variables. Previous literature have attempted to include advanced local time-trends or observables that reflect the suspected omitted variables to alleviate this concern (e.g. Black *et al.* (2013). [15]. In my case, I could mistake e.g. an increasingly ageing or sicker (potential) patient population with a greater need for opioid treatment for increased leniency. For this reason I include $\overline{\mathbf{X}}_{P_{jt}} = \{\overline{\mathbf{X}}_{p_{jt}}, \overline{\mathbf{X}}_{a_{jt}}\}$ where $\mathbf{X}_{p_{jt}}$ is mean characteristics of the potential patients and $\mathbf{X}_{a_{jt}}$ is mean characteristics of *all* patients at the clinic. The included characteristics are age, highest completed education, charlson comorbidities and sex. These should be viewed as flexible, very local time varying regional controls of the composition of patients.

Finally, while I control for the time-varying correlated effects through the composition of potential patients, non-random provider selection into networks based on the characteristics of the practice environment orthogonal to $\{\overline{\mathbf{X}}_{F_{jt}}, \mathbf{X}_{jt}, \alpha_j, \overline{\mathbf{X}}_{P_{jt}}, \delta_t\}$ could hypothetically still be present. This would be the case if providers perfectly choose their networks, based on the evolution of opioid leniency, when opening a practice. Ultimately, the absence of such selection will be a matter of assumption in this paper. I do consider the assumption plausible justified for two primary reasons: First, the freedom to choose one's peers perfectly is highly impeded - if not impossible, as a license to operate is needed to open a practice. The practice specific networks means that each practice is geographically anchored, and the scope for assortative selection not controlled for by the practice fixed effects and patient composition is highly limited. As far as exits concern, I show in appendix C how low prescribing providers are not more likely to exit if located in a highly lenient market, and vice versa. I take this to indicate that neither high or low leniency practices are "driven" to retirement by local practice style.

[14]The variation in the practice contextuals is almost exclusively driven by between practice variation. The between practice variance constitutes 87%-93%. Further details on the exogenous characteristics are in the next section

[15]I have estimated models included a municipality by year fixed effects. The results do not qualitatively changes, and are available upon request.

5 Results

In this section I present my results. Section 5.1 results from the two-way fixed effect model, documenting meaningful variation in opioid leniency, and then proceeds to show that prescription leniency of a practice is affected by prescription leniency of neighbouring practices.

5.1 Evidence of network effects in prescription leniency

5.1.1 Estimating opioid leniency

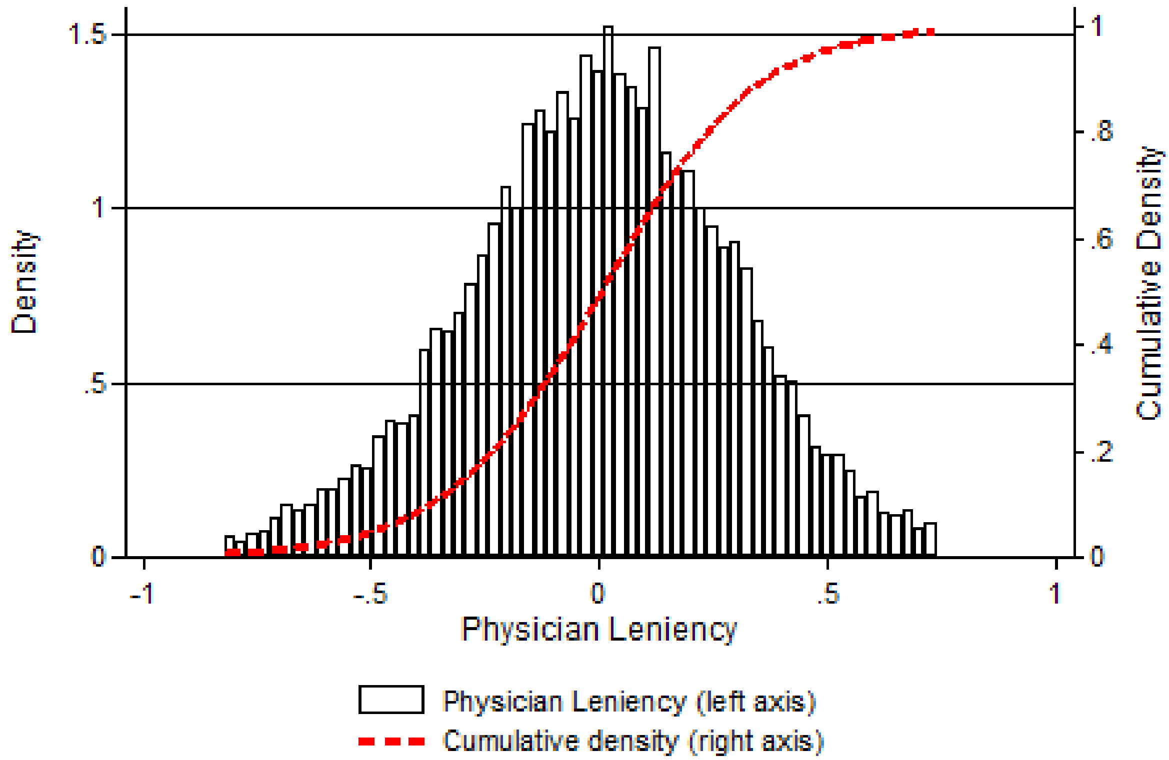
To obtain a measure of opioid prescription leniency, I estimate a two way fixed effect model of log of yearly defined daily doses of opioid[16]. The two-way fixed effects model allows me to simultaneously estimate a yearly practice fixed effect and a time constant individual patient fixed effect, while allowing these to be correlated. From the sample of opioid-users the model is estimated on the largest possible connected set of individuals and practice, which constitutes app. 99% of the entire sample.

The distribution of yearly prescription leniency, γ_{jt} is estimated by equation (1). That the practice matters for opioid treatment intensity is readily available from figure I. This figure depicts the distribution of estimated opioid prescription leniency(left axis) and the cummulative distribution(right axis). The depicted distribution is centered around the mean and I cap them at the 1^{st} and 99^{th} percentiles. The standard deviation is 0.32. The variation in leniency of going from one practice to another can be substantial substantial. As the outcome is measured in logs, moving one standard deviation in the leniency distribution results in an increase of opioids treatment intensity of $(e^{0.32} - 1) = 37.7\%$ in DDDs. [17] This would imply, that if the mean patient (80.24 DDDs) moves to a practice with a 1 standard deviation higher leniency it would increases his treatment intensity by 30.3 DDDs.

[16] I use the log of DDDs as the distribution of DDDs is left-skewed. The mean in 80.24, the median is 17

[17] To grasp the magnitude of the variation in prescription style, a move from the 1^{st} percentile(least lenient) to the 99^{th} percentile(most lenient) of prescription leniency amount to a difference of $(e^{1.52} - 1) = 357.2\%$

Figure I. Distribution and cumulative distributions of estimated opioid leniency.

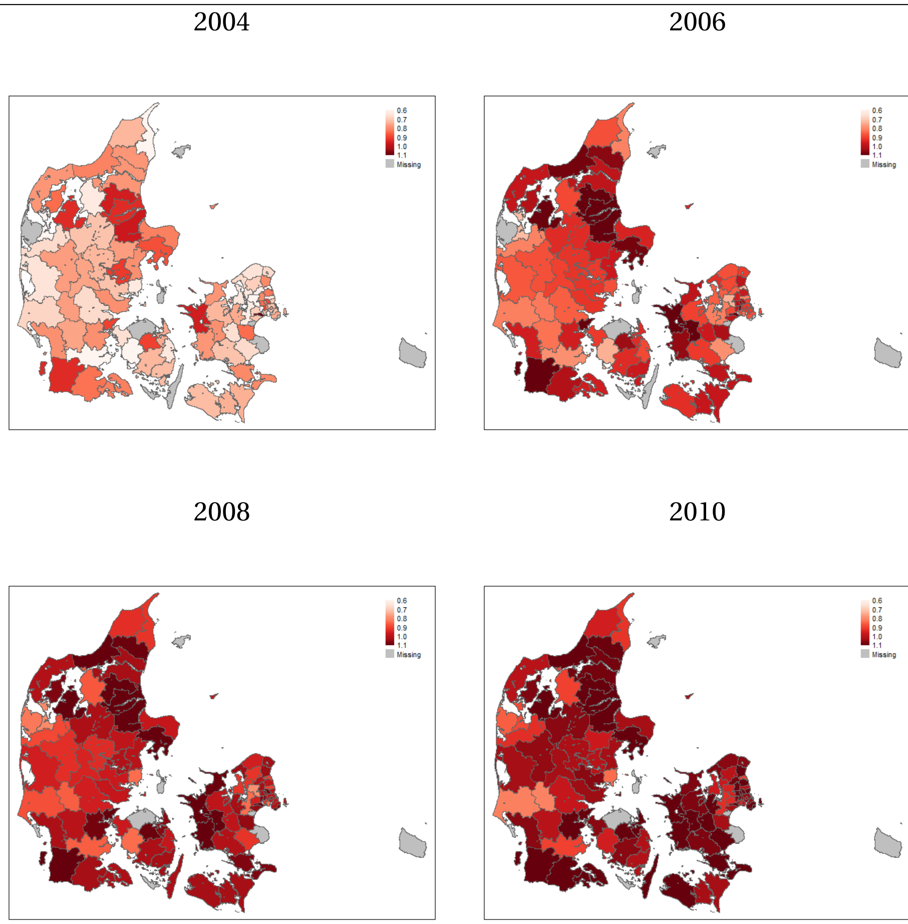


Notes: Distribution and cumulative distributions of estimated opioid leniency. The opioid prescription leniency is estimated from (1), where patient observables and unobservables are controlled for. The leniency is only identified up to a normalization, hence I normalize the mean to zero, such that the distribution is expressed in deviations from the mean. The distribution is capped at the 1^{st} and 99^{th} percentile

Figure II illustrates the geographical dispersion and the spatial spill-overs in prescription leniency. Here, I present a map of the 98 municipalities in Denmark. The map depicts the mean estimated prescription leniencies in the municipalities across time . The intensity of red indicates the intensity of prescription leniency.[18] From this figure, it is evident that in 2004 the high intensity areas are locally centered around the South-Western and North-Eastern parts of the peninsula of Jutland, and the western parts of Zealand. It is also clear that while the estimated leniency increases across the entire country over the observed time period, the leniency intensity seems to diffuse from these high leniency areas. This indicate that there is a spatial component to the evolution of prescription leniency, and thus brings credence to the concept of practice spill-overs among spatially close practices.

[18]To allow for cross year comparisions, this figure is constructed by calculating quintiles of estimated prescription leniency for the year 2004, and then applying these as cutoffs in the legend for the years 2006, 2008 and 2010. Furthermore, due to data confidentiality, at this stage I am only able to present evidence based on municipality level regressions. The municipalities with missing values are omitted due to data-confidentiality

Figure II. Evolution in estimated prescription leniency over time

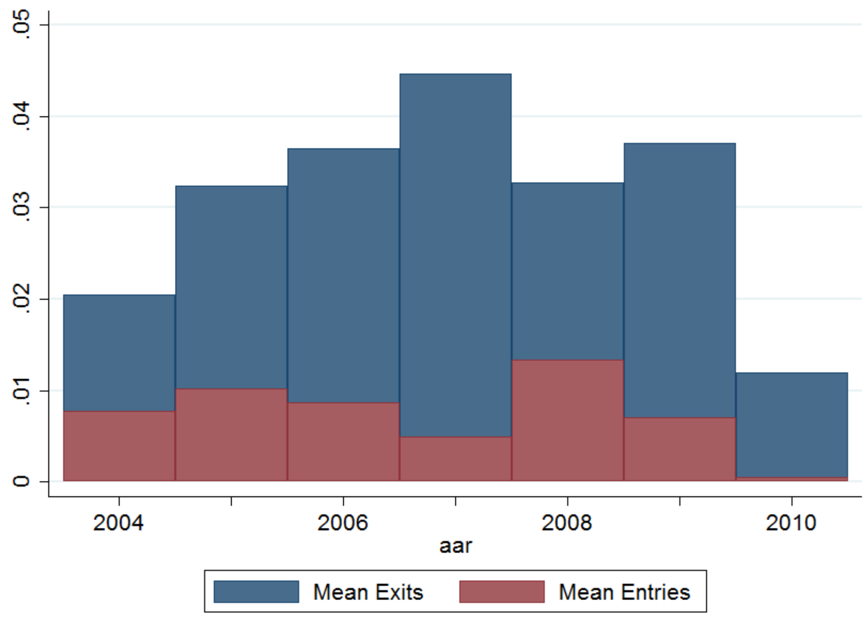


Notes: Evolution in estimated prescription leniency over time. Higher intensity of red indicate higher intensity of presciption leniency. Due to data confidentiality, the effects in the map are estimated on municipality level.

5.1.2 Estimating the impact of practice environment

From 2004-2010 I construct a network of 2,381 unique practices over an average of app. 6 years for a total of 14,254 observations. The identification of the peer effects hinges on within-practice variation over time, and while table II shows a rather stable average number of practices in the network, figure III highlights that the compositions of practices changes over time. The figure depicts the fraction of first-time observed practices (entries; in red) and the fraction of practices that are inactive the following year(exits; in blue) [19]. Evidently there are systematically more exits than entries, reflecting the declining number of practices in table II.

Figure III. Exits and entries of practices by year.



Notes: Exits and entries of practices by year. The figure shows the fraction of practices who exit (are not included next period) and enters (are observed for the first time) in the sample by year.

In panel A of table III I present the coefficients from an OLS regression of first level peer opioid leniency on mean third level peer characteristics. The third level peer characteristics (contextuals) included are whether the practice is a single provider practice, the number of patients at the practice, the number of alternative practices the average potential patient can choose from and whether there is any female physician in the practice. E.g. the coefficient in column 1 implies that if the fraction of third level peers who were single physician practices increased by 10 percentage points, opioid leniency of the first level peer would decrease by 0.77 - or approx. 2.5% of a standard deviation. In Panel B I show how the between practice variance dominates the within practice variation in all the characteristics. Hence using these *contextuals* ensures that the identifying variation is in fact primarily driven by practice entries and exits.

[19] While I do not have data on the reason for either entering or exiting, I will impute the decision to retire from the distribution of provider age at the time of closure. In appendix B, I do separate analysis on this sample, where these practice exits are the only variation in provider network over time. Results are qualitatively the same.

Table III. Impact Peer exogenous characteristics on first level peer opioid leniency

Panel A				
	Single Phys. Practice	Number of Patients	Mean number of competitors	Any Female
Effect on (std) opioid leniency	-0.077***	-0.021***	-0.001***	-0.072***
	(0.010)	(0.003)	(0.0001)	(0.010)
Observations	1,688,880	1,688,880	1,688,880	1,688,880
Panel B: Fraction of variance				
	Single Phys. Practice	Number of Patients	Mean number of competitors	Any Female
Between	0.87	0.93	0.99	0.93
Within	0.13	0.07	0.01	0.07

Notes: Panel A: Impact of third level Peer exogenous characteristics on first level peer opioid leniency. The joint F-test is 38.4. Panel B: The fraction of variation in the particular exogenous third level characteristic attributable to between and within variation. Standard errors in panel A are clustered at the practice level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

The peer effect estimate I present is an *Average Peer Effect* De Giorgi *et al.* (2020). Measured in standard deviations, this is the effect of a random peer increasing their prescription leniency by 1 standard deviation. Or in other words, how many standard deviations of prescription leniency does the focal practice increase her leniency in response to a uniformly random peer increasing her prescription leniency by 1 standard deviation. In addition to its straightforward interpretability, the average peer effect carries the benefit, that it allows for comparisons across settings where the size of the peer set changes (De Giorgi *et al.* (2020))

Table IV. Regressions of prescription leniency on practice environment

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Leniency	Leniency	Leniency	Leniency	Leniency	Leniency
Avg. Peer Effect	0.004**	0.003**	0.003**	0.073***	0.067***	0.069***
	(0.002)	(0.001)	(0.001)	(0.022)	(0.023)	(0.026)
Observations	14,254	14,254	14,254	14,254	14,254	14,254
R-Squared	0.761	0.762	0.762	0.761	0.762	0.762
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Practice FE	Yes	Yes	Yes	Yes	Yes	Yes
Peer Contextual	No	Yes	Yes	No	Yes	Yes
Own Contextual	No	No	Yes	No	No	Yes
Patient Comp.	Yes	Yes	Yes	Yes	Yes	Yes
IV	No	No	No	Third Level	Third Level	Third Level

Notes: The outcome is leniency of focal practice. The outcome is standardized by year. The first row presents the estimated peer effects, where columns (1)-(4) show the results based on OLS gradually controlling for Peer contextuals, own contextuals and the composition of potential patients living in the treatment area of the focal practice. All models control for time and practice fixed effects. Columns (5)-(8) estimates 2SLS models using contextuals of third level peers as instruments of per leniency. Standard errors are clustered at the practice level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table IV presents the results. In the first three columns, I run the naive OLS estimates, where the simulteneity issue is not adressed. All models include a practice fixed effect, time fixed effects, and the composition of potential and actual patients $\overline{\mathbf{X}}_{P_{jt}}$. In models (1)-(3) I gradually control for peer contextuals (2) and practice covariates (3), In neither case, the coefficient changes much, and my estimates imply that an increase of one standard deviation in prescription leniency by a random peer increases the prescription leniency of the focal practice by 0.4% of a standard deviation.

Models (4) through (6) present 2SLS estimates using variation in third level peers contextual to instrument for leniency of first level peers in order to separate the endogenous and exogenous peer effect. The first stage using third level peer characteristics produces a F-statistic of 38.4. The robust IV estimates are systematically higher than the OLS estimates regardless of specification. My estimates imply that an increase of a random peer by 1 standard deviation increases the leniency by the focal practice by app. 7% of a standard deviation[20].

In appendix C I conduct a battery of additional analyses to strengthen the case that the estimated network effects are not reflecting spurious correlations driven by endogenous network formation. First, I dwelve into *homophily* and the importance of peers. Evidence that practices react stronger to spatially close peer-practices and/or more observationally similar peer-practices would strengthen the belief that I estimate essential and meaningful network effects. I show how restricting first-level peer practices to increasingly spatially closer practices leads to increased network effects - suggesting that practices operating in the immediate vicinity of the focal are more likely to influence the focal practice. I also verify this index by estimating network effects where I weight according to the degree of overlap of potential patient. I find that increasing the weight put on providers spatially close to the focal provider increases my estimated network effect. Finally I show the presence of homophily by weighting peer-practices by a similarity index similar to the one implemented in De Giorgi *et al.* (2020). This similarity index puts larger weight on observational similar peer-practices in terms of mean age and income of physicians in the practice, as well as whether a female physician is working in the practice. I estimate larger average peer effects when weigthing according to similarity.

Secondly, I do analysis to assess whether endogenous network formation is a threat to the validity of my estimates. If providers select non-randomly into the networks, my estimated network effects would be severely bias. I do several checks to asseess this threat to my analysis. To check for network selection on observables, I conduct a balance test where I regress the proximity ranking on the components of the similarity index. Here I find no effect of selection. Testing for selection on unobservables is fundamentally impossible, and I have to rely on institutional features for obtaining a license to argue against selection *into* network. Selection *out* of the network can to some extent be tested. In appendix C I show that regardless of the leniency level of the focal provider, local leniency level does not correlate with the probably that the focal exits the market.

[20] 7% of the increase in DDDs from 1 standard deviation in leniency (30.3 DDDs) is 2.1 DDDs for the mean patient

6 Effects of Opioid Consumption on Patient Labor market outcomes

So far this paper has been focused on the provider-spillovers. In this section I use the spill-overs to show how opioid treatment intensity has adverse effects on labor market performance. In the previous section I confirmed findings from previous literature (e.g. Sabatino *et al.* (2018), Smulowitz *et al.* (2016) and Upadhye *et al.* (2018)) that providers vary in terms of opioid leniency. This fact creates an immediate and intuitive appeal to use provider leniency as an instrument of opioid use, in the mold of the extensive case-worker literature (e.g.Kling (2006) , Doyle (2007)). However, as primary care providers and patients (contrary to e.g. judges and suspect) rarely are assigned randomly, applying this strategy in the PCP-setting comes with it's own set of obvious caveats. Particularly, high demand patients, with poor expected labor market performance, might select more lenient providers. Such sorting would drive a negative relationship between opioids and labor market performance. Additionally, if a provider who is very lenient with opioids e.g. also has a higher tendency to overlook high cholesterol levels or prescribe anti-depressants extensively, the impact of opioid prescription leniency does not exclusively measure the impact of increased opioids. Ultimately, patient selection and the potential direct effects of the provider could drive a negative relationship between opioid use and labor market outcomes that invalidate the leniency measure as an instrumental variable candidate.

Instead of utilizing prescription leniency, this paper circumvent the selection issues by leveraging the extent to which temporal changes in treatment behavior of other nearby providers, drive changes in focal provider opioid leniency. As it was established in the previous section that a provider's network is known to influence her treatment decision, time-varying opioid leniency attributable to whether providers in the immediate vicinity practice relatively more or less aggressively offers an instrumental variables candidate. Conceptually, the simulated IV strategy resembles that of Currie and Gruber (1996) who simulate changes in state level mean eligibility to health insurance from a Medicaid Expansion, and Chetty *et al.* (2014), who estimate the impact of tax credits on labor supply by modelling local knowledge of the earned income taxed credit. A more recent example is Silver (2019), who uses the induced behavior of peers to estimate the effect on patient health of cutting down time spent with patients in an ER setting.

Stage 0: Let λ^*_{jt} denote the predicted leniency from 2. Let λ_{jt} denote the simulated leniency of practice j in period t. The latter is the variation in λ^*_{jt} that is orthogonal to the practice fixed effects, α_j, and characteristics of potential and actual patients ($\overline{\mathbf{X}}_{P_{jt}}$). Controlling for potential and actual patients ensures that I do not confuse changes in patient composition at the specific clinic with leniency. In practice the simulated leniency is obtained as the residuals from a linear regression of

$$\lambda^*_{jt} = \alpha_j + \overline{\mathbf{X}}_{P_{jt}} \nu_1 + \delta_t + \lambda_{jt}$$

Stage 1: I predict opioid treatment intensity in terms of Defined Daily Doses, O_{it}, from the first stage on simulated leniency:

$$O_{it} = \theta_1 \lambda_{jt} + \mathbf{Z}_{it} \theta_2 + \alpha_j + \delta_t + \varepsilon_{it} \tag{3}$$

where $\mathbf{Z}_{it}$ contains information on individual characteristics including education, gender, age and health status. α_j is a practice fixed effect.

Stage 2: Finally, I estimate the impact of opioid consumption on labor market outcomes, Y_{it} of individuals:

$$Y_{it} = \mu_1 \widehat{O}_{it} + \mathbf{Z}_{it}\mu_2 + \alpha_j + \delta_t + \epsilon_{it} \tag{4}$$

The estimated prescription leniency is a valid instrument for opioid utilization if λ_{jt} is relevant in (3) and λ_{jt} is uncorrelated with ϵ_{it} in (4). There are several potential threats to the validity. The treats will be explored in appendix D.

Figure A.2 depict λ^*_{jt} (in grey) and λ_{jt} (in black). The standard deviation of λ_{jt} is 0.02, and is the variation that identifies the changes in opioid treatment intensity. such that on the mean, an increase in simulated leniency of 1 standard deviation amounts to app. 14 defined daily doses for the patient[21],or 16% of the mean. As simulated leniency is a generated variable, I will bootstrap all standard errors in the following regressions, and cluster on municipality level[22].

Under the assumption of no selection into simulated leniency conditional on practice fixed effects and patient covariates[23], I can estimate the impact of opioid treatment intensity on labor market outcomes using simulated leniency as an instrument of treatment intensity. The labor market outcomes of primary interest are labor market income percentile ranking and whether the individuals take up any short term disability. I measure outcomes one year ahead.

Table V show the results of the first stage, equation (3). In columns (1) and (2) I run the first stage without the practice fixed effects, which I include in columns (3) and (4). I also report the F-test of the simulated leniency. Neither controlling for individual characteristics or practice fixed effects changes the first stage, which implies that an increase in simulated leniency of one standard deviation increases opioid treatment intensity by 1.4%.

In table VI, I present results from estimating (4) with labor market income percentile rank and probability of having any short term disability in year t+1 as outcomes. Columns (1)-(2) presents the raw correlations between labor market income percentile ranks and defined daily doses of opioid. Columns (3)-(4) show the reduced form results, and Columns (5)-(6) presents the effect of opioid usage as instrumented by simulated leniency. In columns (2), (4) and (6) I control for individual covariates which include a cubic polynomial of age, gender , educational categories and charlson co-morbidities.

The naive OLS estimates show that increasing opioid intensity decreases the income percentile rank but also decreases the probability of having any short term disability. The estimates implies that a 10 percentage point increase in DDD equates a 0.30 percentage point decrease in labor market income percentile ranking the following year and a decrease of 0.26 percentage points decrease in the probability of getting any short term disability.

[21] The LHS of (**??**) is a standardized distribution, hence a difference between two physicians in simulated leniency of 1 corresponds to a difference in estimated prescription leniency of 1 standard deviation. On the mean, a difference of 0.49 in simulated leniency corresponds to $(e^{(0.49)(0.32)} - 1)80.24 = 13.6$ DDD. This corresponds to the first-stage in table V that allow for between-physician variation

[22] There are 98 municipalities in Denmark. Due to computational feasibility, I do not include the estimation of the simulated leniency in the bootstrap. In online appendix table AI I reproduce my main results, where the estimation of simulated leniency is included for in the bootstrap. As expected it slightly decreases the precision but does not change the conclusions

[23] $E[\lambda_{jt}\varepsilon_{it}|\alpha_j, Z_{it}] = 0$. Section **??** is devoted to checking the validity of this assumption.

Table V. First stage regressions.

	(1)	(2)	(3)	(4)
Outcome	Log(DDD)	Log(DDD)	Log(DDD)	Log(DDD)
Sim. Leniency (std)	0.014***	0.014***	0.014***	0.014***
	(0.003)	(0.003)	(0.003)	(0.003)
Observations	1,227,157	1,227,157	1,227,157	1,227,157
R-squared	0.007	0.086	0.033	0.109
F-test	21.1	23.7	24.0	27.8
Time	Yes	Yes	Yes	Yes
Ind Covariates	No	Yes	No	Yes
Practice FE	No	No	Yes	Yes

Notes: First stage regressions. All columns include Time and regional dummies. Columns (2) and (4) include individual characteristics. Columns (3) and (4) includes practice fixed effects. Standard errors are clustered at the practice level. $^{***}p < 0.01$, ** $p < 0.05$, $^{*}p < 0.1$

My reduced form results estimate the effect of affiliation with a more lenient provider on labor market outcomes. The results in columns (3)-(4) indicate that having a provider who is more lenient in opioid prescription leads to significant reductions in labor market income ranking. However, I find that in contrast to the correlations estimated in the previous columns, a more lenient provider does not reduce the probability of taking up short term disability. The coefficients imply that increasing the simulated leniency by 1 standard deviation decreases income percentile rank with app. .1 percentage points(significant) and *increases* the probability of having any short term disability by 0.05 percentage points. The estimated effects on short term disability are however very imprecisely estimated.

Supposing that opioid usage is the channel, I instrument opioid intensity use with the simulated leniency. This yields estimates that equate a .4 percentage point decrease in labor market income percentile rank resulting from an increase in opioid treatment intensity of 10% on the mean. Additionally, one has to keep the lavishness with which opioids are prescribed in mind. Extrapolating these results, implies that moving up one 1 standard deviation in opioid treatment intensity (189.4 DDDs) leads to a 7.5 percentage points decrease in the labor market percentile rank[24]. This is 19.4% of the mean.

I find that increased opioid use leads to increased take-up of short term disability. The estimates are rather imprecisely estimated, but taking the estimates at their face value, I find that increasing opioid usage by 10% increases the probability of having any short term disability the following year by 0.4 percentage points at the mean. This implies that I, with 95 % certainty cannot rule out an effect of up to .94 percentage points increased short term disability resulting from an increase in opioid usage of 10%. While the instrumented impacts on days on disability are not statistically significant, the sign reversal is rather striking. This suggests that contrary to what the previous literature on pain management and labor supply Garthwaite (2012),Butikofer and Skira (2015)) finds, opioids seemingly do not possess the same labor market enhancing effects as alternative analgesics. In the online appendix tables AII-AIV, I show how

[24]This exercise requires some extrapolation of the identifying variation, as an increase of 1 standard deviation of simulated leniency increases opioid use with 1.4%

Table VI. OLS, Reduced form and 2SLS estimates of labor market

Panel A

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	LMI	LMI	LMI	LMI	LMI	LMI
Log(DDD)	-0.0302***	-0.0288***	-	-	-0.0451***	-0.0428***
	(0.0006)	(0.0005)	-	-	(0.012)	(0.013)
Sim. Len (std)	-	-	-0.001***	-0.001***	-	-
	-	-	(0.0002)	(0.0002)	-	-
Observations	1,199,033	1,199,033	1,199,033	1,199,033	1,199,033	1,199,033
Outcome Mean	0.386	0.386	0.386	0.386	0.386	0.386
R-squared	0.065	0.176	0.030	0.147	0.030	0.147
Time	Yes	Yes	Yes	Yes	Yes	Yes
Ind Covariates	No	Yes	No	Yes	No	Yes
IV	No	No	No	No	Yes	Yes
Prac. FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Disability	Disability	Disability	Disability	Disability	Disability
Log(DDD)	-0.026***	-0.014***	-	-	0.035	0.040
	(0.001)	(0.001)	-	-	(0.028)	(0.027)
Sim. Len (std)	-	-	0.0005	0.0005	-	-
	-	-	(0.0004)	(0.0003)	-	-
Observations	1,227,157	1,227,157	1,227,157	1,227,157	1,227,157	1,227,157
Outcome mean	0.17	0.17	0.17	0.17	0.17	0.17
R-squared	0.02	0.08	0.01	0.07	0.01	0.07
Time	Yes	Yes	Yes	Yes	Yes	Yes
Ind Covariates	No	Yes	No	Yes	No	Yes
IV	No	No	No	Yes	Yes	Yes
Prac. FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: OLS, Reduced form and 2SLS estimates of labor market income percentile ranking (Panel A) and probability of any short term disability on opioid usage (Panel B). Outcomes are measured one period ahead. Columns (1) - (2) present results from OLS, columns (3) - (4) present the Reduced form estimates and columns (5)-(6) present the 2SLS estimates of using simulated leniency as instrument of opioid usage. All columns include a practice fixed effect. Individual covariates are gradually included. Standard Errors are clustered on the practice level. ***$p < 0.01$, ** $p < 0.05$, *$p < 0.1$

the reduction in labor market income in particular is due to exits from employment into non-employment including permanent disability.

The results in this section hinges on several assumptions. In appendix D I run a series of robustness checks to validate my findings. I start off by showing how individual covariates balance across different values of simulated leniency. This indicates that individuals do not select into the simulated leniency. I then proceed to show that the effects are not driven by other treatment styles. I show how controlling for other time-varying treatment style indicators (test for strep-A before prescribing anti-biotics and monitoring cardio-vascular disease) does not change my results. Furthermore, I correlate the estimated leniency with estimates of provider fixed effects from 9 major practices styles obtained from Simonsen *et al.* (2019). Opioid leniency does not correlate with provider level measures of initiation with chronic medications, referral to secondary care or health care utilization. My estimate of opioid leniency only correlates with the provider propensity to administer prescription drugs. Finally I find identical effects when I remove colleagues (current and previous) from the sample to shut down a potential patient-to-patient channel. All in all, I find no evidence that invalidates the simulated leniency as an instrumental variables candidate for opioid use.

7 Conclusion

In this paper I establish the existence of a spatial spill-over in Opioid prescription leniency of general practioners. Creating a complete non-overlapping network based on the geolocation of practices I use variation in the composition of excluded peers (e.g. Bramoullé *et al.* (2009)) to identify spill-over effects. I find that if a random peer of the focal provider increases prescription leniency by 1 standard deviation, the focal provider increases her prescription leniency by app. 7 % of a standard deviation. This effect is robust to various weightings of peers and specifications of network relations.

I then use the extent to which temporal changes in treatment behavior of other nearby providers, drive changes in focal provider opioid leniency to estimate the impact of opioid treatment intensity on labor market income. I find that an increase of 1 standard deviation in simulated provider leniency increases opioid treatment intensity by app. 1.5%, and find that on the mean an increase of 10% in opioid treatment intensity significantly reduces labor market income ranking by 0.43 percentage points and decreases the probability of employment by approximately 1 percentage points.

This paper does not address the decision to initiate opioid on the extensive margin. While this margin is interesting, it is also fundamentally different from the intensive margin - the focus of this paper. First off, due to the large variation in opioid use conditional on having a claim - mean of 80 defined daily doses and std. dev. of 190 defined daily doses - a binary indicator of opioid use would mask a large degree of heterogeneity. Second, investigating the extensive margin requires one to compare individuals treated with opioid analgesics to individuals that potentially was treated, but for some reason was not. This difference in "point of initiation" - or correct specification of an initial "at-risk" group is something that also should be accounted for, when such analysis is conducted. This is left for future research. Finally, one might ask whether opioid abuse would be the correct margin to investigate. Ex ante, I do find it highly unlikely that opioid abuse, unlike increased treatment intensity on the mean, could have any positive bearing on your labor market outcomes. Hence, in this paper, I choose to evaluate opioid treatment intensity.

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A Appendix A

Table A.I. OLS, Reduced form and 2SLS estimates of labor market

Panel A

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	LMI	LMI	LMI	LMI	LMI	LMI
Log(DDD)	-0.0302***	-0.0288***	-	-	-0.0451***	-0.0428***
	(0.0006)	(0.0005)	-	-	(0.017)	(0.017)
Sim. Len (std)	-	-	-0.001***	-0.001***	-	-
	-	-	(0.0003)	(0.0003)	-	-
Observations	1,199,033	1,199,033	1,199,033	1,199,033	1,199,033	1,199,033
Outcome Mean	0.386	0.386	0.386	0.386	0.386	0.386
R-squared	0.065	0.176	0.030	0.147	0.030	0.147
Time	Yes	Yes	Yes	Yes	Yes	Yes
Ind Covariates	No	Yes	No	Yes	No	Yes
IV	No	No	No	No	Yes	Yes
Prac. FE	Yes	Yes	Yes	Yes	Yes	Yes

Panel B

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Disability	Disability	Disability	Disability	Disability	Disability
Log(DDD)	-0.026***	-0.014***	-	-	0.035	0.040
	(0.001)	(0.001)	-	-	(0.031)	(0.029)
Sim. Len (std)	-	-	0.0005	0.0005	-	-
	-	-	(0.0004)	(0.0004)	-	-
Observations	1,227,157	1,227,157	1,227,157	1,227,157	1,227,157	1,227,157
Outcome mean	0.17	0.17	0.17	0.17	0.17	0.17
R-squared	0.02	0.08	0.01	0.07	0.01	0.07
Time	Yes	Yes	Yes	Yes	Yes	Yes
Ind Covariates	No	Yes	No	Yes	No	Yes
IV	No	No	No	Yes	Yes	Yes
Prac. FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: OLS, Reduced form and 2SLS estimates of labor market income percentile ranking (Panel A) and probability of any short term disability on opioid usage (Panel B). Outcomes are measured one period ahead. Columns (1) - (2) present results from OLS, columns (3) - (4) present the Reduced form estimates and columns (5)-(6) present the 2SLS estimates of using simulated leniency as instrument of opioid usage. All columns include a practice fixed effect. Individual covariates are gradually included. Standard errors are bootstrapped (including the estimation of the simulated leniency) and clustered on the provider level. The standard errors are based on 400 replications. ***$p < 0.01$, ** $p < 0.05$, *$p < 0.1$

Table A.II. Effects of opioids on Employment

Outcome	Employment	Employment	Employment	Employment	Employment	Employment
Log(DDD)	-0.082***	-0.055***	-	-	-0.103**	-0.092*
	(0.001)	(0.001)	-	-	(0.054)	(0.049)
Sim. Len	-	-	-0.002**	-0.001*	-	-
	-	-	(0.001)	(0.001)	-	-
Observations	1,227,157	1,227,157	1,227,157	1,227,157	1,227,157	1,227,157
Outcome Mean	0.507	0.507	0.507	0.507	0.507	0.507
R-squared	0.09	0.27	0.02	0.25	0.02	0.25
Time	Yes	Yes	Yes	Yes	Yes	Yes
Ind Covariates	No	Yes	No	Yes	No	Yes
IV	No	No	No	No	Yes	Yes
Practice FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: OLS, Reduced form and 2SLS estimates of employment status on Opioid usage with a practice fixed effect. Outcomes are measured in t+1. Columns (1) - (2) present results from OLS, columns (3) - (4) present the Reduced form estimates and columns (5)-(6) present the 2SLS estimates of using simulated leniency as instrument of opioid usage. All columns include a practice fixed effect and time-varying treatment evolution. Individual covariates are gradually included. Standard errors are clustered on the practice level. ***$p < 0.01$, ** $p < 0.05$, *$p < 0.1$.

Table A.III. Effects of opioid treatment intensity on Unemployment

Outcome	Unemployment	Unemployment	Unemployment	Unemployment	Unemployment	Unemployment
Log(DDD)	-0.004***	-0.003***	-	-	0.013	0.013
	(0.0003)	(0.0003)	-	-	(0.013)	(0.013)
Sim. Len	-	-	0.0004**	0.0004**	-	-
	-	-	(0.0002)	(0.0002)	-	-
Observations	1,227,157	1,227,157	1,227,157	1,227,157	1,227,157	1,227,157
R-squared	0.01	0.02	0.01	0.02	0.01	0.02
Outcome Mean	0.025	0.025	0.025	0.025	0.025	0.025
Time	Yes	Yes	Yes	Yes	Yes	Yes
Ind Covariates	No	Yes	No	Yes	No	Yes
IV	No	No	No	No	Yes	Yes
Practice FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: OLS, Reduced form and 2SLS estimates of unemployment status on opioid usage with a practice fixed effect. Outcomes are measured in t+1. Columns (1) - (2) present results from OLS, columns (3) - (4) present the Reduced form estimates and columns (5)-(6) present the 2SLS estimates of using simulated leniency as instrument of opioid usage. All columns include a practice fixed effect and time-varying treatment evolution. Individual covariates are gradually included. Standard errors are clustered on the practice level. $^{***}p < 0.01$, $^{**}\ p < 0.05$, $^{*}p < 0.1$

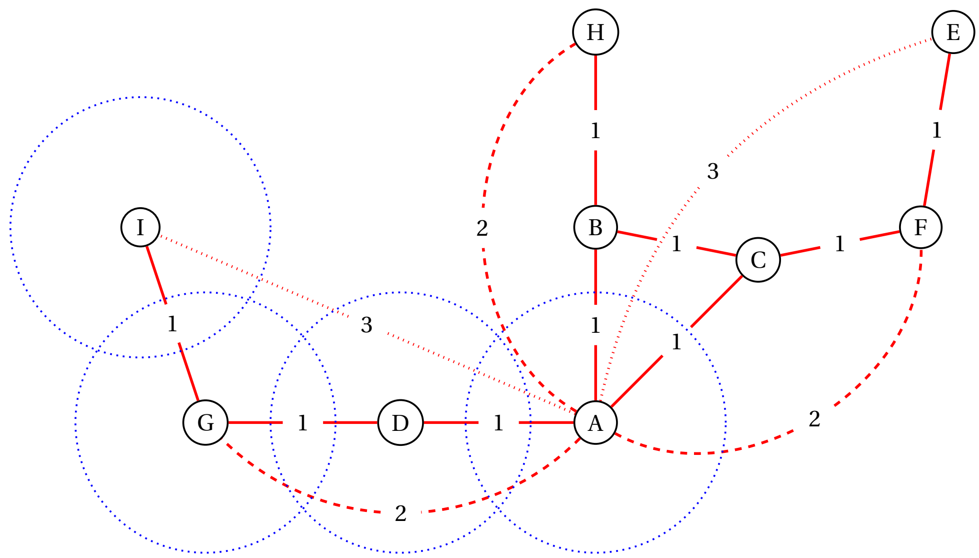
Table A.IV. Effect of opioid treatment intensity on Nonemployment

Outcome	Nonemployment	Nonemployment	Nonemployment	Nonemployment	Nonemployment	Nonemployment
Log(DDD)	0.089***	0.058***	-	-	0.090**	0.080*
	(0.001)	(0.001)	-	-	(0.044)	(0.039)
Sim. Len	-	-	0.002**	0.001*	-	-
	-	-	(0.0005)	(0.0005)	-	-
Observations	1,227,157	1,227,157	1,227,157	1,227,157	1,227,157	1,227,157
R-squared	0.10	0.31	0.02	0.28	0.02	0.28
Outcome Mean	0.458	0.458	0.458	0.458	0.458	0.458
Time	Yes	Yes	Yes	Yes	Yes	Yes
Time	Yes	Yes	Yes	Yes	Yes	Yes
Ind Covariates	No	Yes	No	Yes	No	Yes
IV	No	No	No	No	Yes	Yes
Practice FE	Yes	Yes	Yes	Yes	Yes	Yes

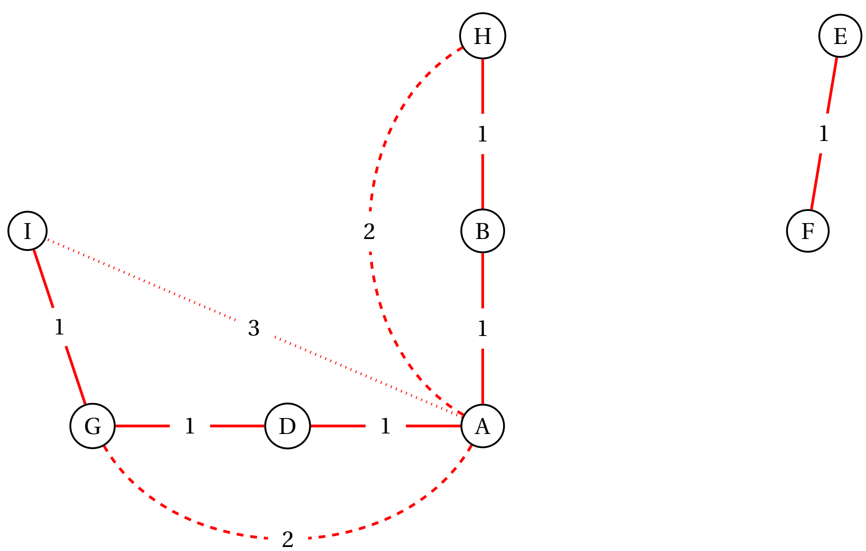
Notes: OLS, Reduced form and 2SLS estimates of unemployment status on opioid usage with a practice fixed effect. Outcomes are measured in t+1. Columns (1) - (2) present results from OLS, columns (3) - (4) present the reduced form estimates and columns (5)-(6) present the 2SLS estimates of using simulated leniency as instrument of opioid usage. All columns include a practice fixed effect and time-varying treatment evolution. Individual covariates are gradually included. Standard Errors are clustered on the practice level. $^{***}p < 0.01$, $^{**}\ p < 0.05$, $^{*}p < 0.1$

Figure A.1. Sample Practice Network

Panel A: Sample network period t before practice C exits

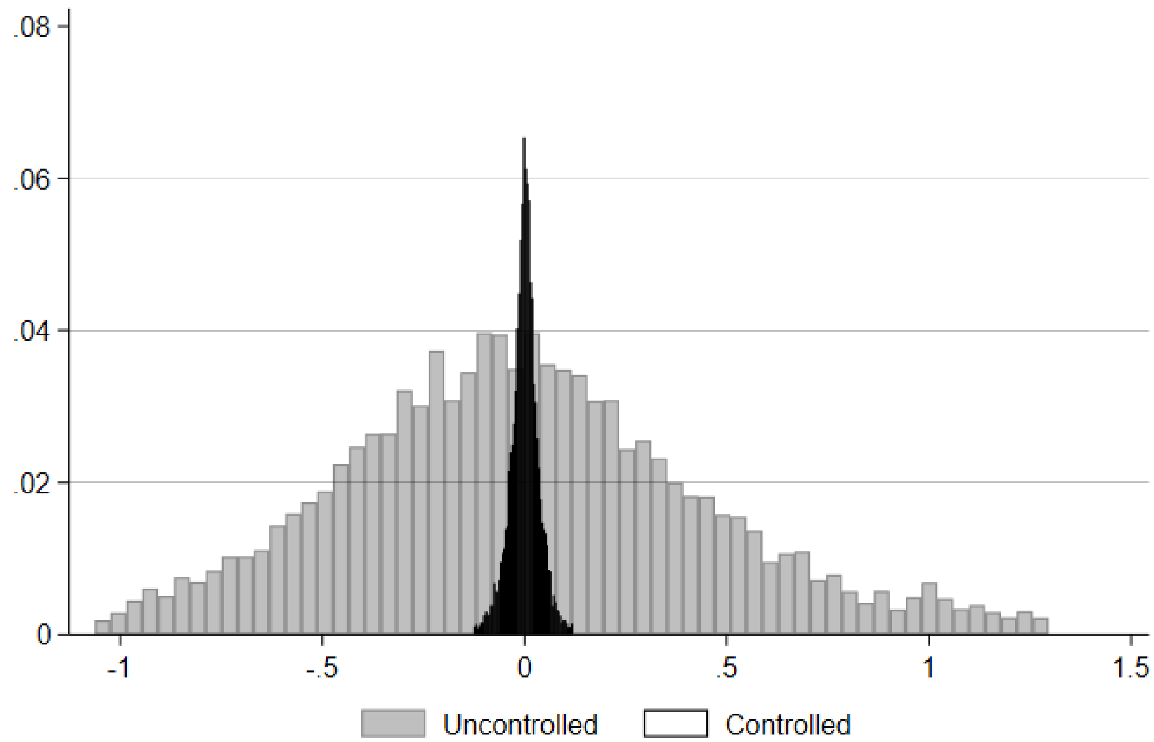


Panel B: Sample network period t+1 after practice C exits



Notes: Simplified illustration of the temporal variation in excluded and non-excluded peers for practice A. The dotted circles indicate catchment areas (only depicted for selected practices for simplicity). The first level connections are marked with solid edges and numbered "1", second level connections are marked with dashed lines and numbered "2", and third level connections are marked with dotted lines and marked "3". In period t provider A has three first level peers (B,C,D), three second level peers (F,G,H), and two third level peers (E,I). In period t+1 practice C closes down and practice A has two first level peers (B,D), two second level peers (H,G), and one third level peer (I).

Figure A.2. Distribution of simulated leniency



Notes: Distributions of "Uncontrolled" and "Controlled" simulated leniency. The distributions are centered around zero and capped at the 1^{st} and 99^{th} percentiles. "Uncontrolled" is λ^*_{jt} (SD=0.17), and the "Controlled" is λ_{jt} (SD=0.02)

B Appendix B

In this appendix I limit the variation in peers over time to practices that close due to physicians retiring. Hence the sample consists of practice that remain in the sample the entire observation period (non-closing and non-entering) and those who close due to retiring. As the reasons for closing is not available to me, the decision to retire must be imputed. Let f_{age} be the empirical distribution of mean ages in practices that close. This is the solid line plotted in figure B.1. It seems likely that the empirical distribution consists of (at least) two underlying components with distinct means and variances. To identify these, and categorize closing practices as retirees and non-retirees, I fit a two-component Gaussian finite mixture model(Jones *et al.* (2013), Friedman *et al.* (2013)) to the distribution of physician ages at closure. Were these to be those who retire and those who close for other reasons, we would expect to detect a distribution with relative higher mean and lower variance than the other. Formally, let these classes be denoted f^R_{age} and f^{NR}_{age}, and let $\pi_R + \pi_{NR} = 1$ be the share of density attributable to each component, then the density can be written as:

$$f_{age} = \pi_R f^R_{age} + \pi_{NR} f^{NR}_{age}.$$

In figure B.1 the posterior distributions are plotted. Furthermore, I also plot the cut-off, which will effectively determine whether I consider an exiting physician a retiree or not. The first distribution have a mean of 54.7 years and a std. dev. of 7.3 years. Meanwhile the second distribution have a larger mean of 64.3 years, and much smaller standard deviation at 3.2 years. While this *soft-bracket* clustering (James *et al.* (2013)) does not give a fixed cut-off, but is probabilistic in its classification, I operationalize the clustering by simulating a cut-off where to classify a practice closure as due to retirement or not[25] The cut-off point, where the two distributions intersect is app. 60 years and I classify all exits, that happens with mean age in practices above 60 as retirees.

Table B.I. Distributions of practice exits classified as retirees and non-retirees

Group	Obs	Mean	Std. Dev.	π
Retirement	222	64.3	3.2	.465
Non Retirement	255	54.7	7.3	.535

Notes: Classification and mean ages of physicians in exiting practices. The classification is simulated from a gaussian two-componentfinite mixture model.

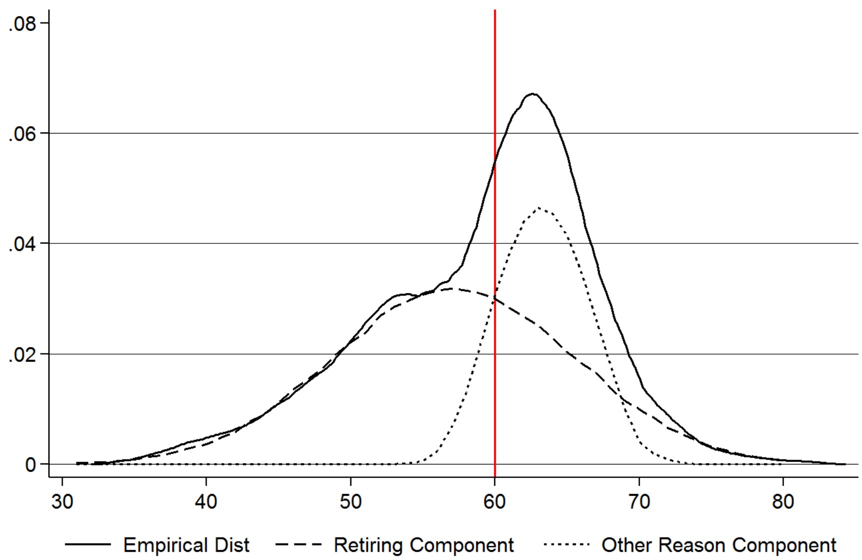
Table B.II. Regressions of prescription leniency on practice environment

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Leniency	Leniency	Leniency	Leniency	Leniency	Leniency
Avg. Peer Effect	0.004**	0.003**	0.003**	0.080***	0.076***	0.073***
	(0.002)	(0.001)	(0.001)	(0.021)	(0.022)	(0.026)
Observations	13,075	13,075	13,075	13,075	13,075	13,075
R-Squared	0.761	0.762	0.762	0.761	0.762	0.762
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Practice FE	Yes	Yes	Yes	Yes	Yes	Yes
Peer Contextual	No	Yes	Yes	No	Yes	Yes
Own Contextual	No	No	Yes	No	No	Yes
Patient Comp.	Yes	Yes	Yes	Yes	Yes	Yes
IV	No	No	No	Third Level	Third Level	Third Level

Notes: The outcome is leniency of focal practice. The sample is conprised of those practices who are present during the entire observation period and those who exit because of retirement. The decision to retire is imputed from a finite mixture model applied to the age of exiting physicians (details in appendix). The first row presents the estimated peer effects, where columns (1)-(4) show the results based on OLS gradually controlling for Peer contextuals, own contextuals and the composition of potential patients living in the treatment area of the focal physician. All models control for time and practice fixed effects. Columns (5)-(8) estimates 2SLS models using contextuals of third level peers as instruments of per leniency. Standard errors are clustered at the practice level.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

[25]The implementing of such a classification will inevitably lead to some misclassification, Alternatively I implement a k-means *hard-bracket* clustering (Friedman *et al.* (2013)). This yields a cut-off that is 57 years, and the results do not change. Furthermore, one could go "fully" bayesian and weight each physician by the probability of being a retiree. This has also been implemented, and the results do not change.

Figure B.1. Age distribution and components



Notes: Empirical distribution of ages at closure, and posterior probabilities for components from a two-component mixture model. The components are gaussian. From the posteriors I impute an age threshold that is used to classify practices as exiters due to either retirement or another reason.

C Appendix C

Homophily and the importance of peers

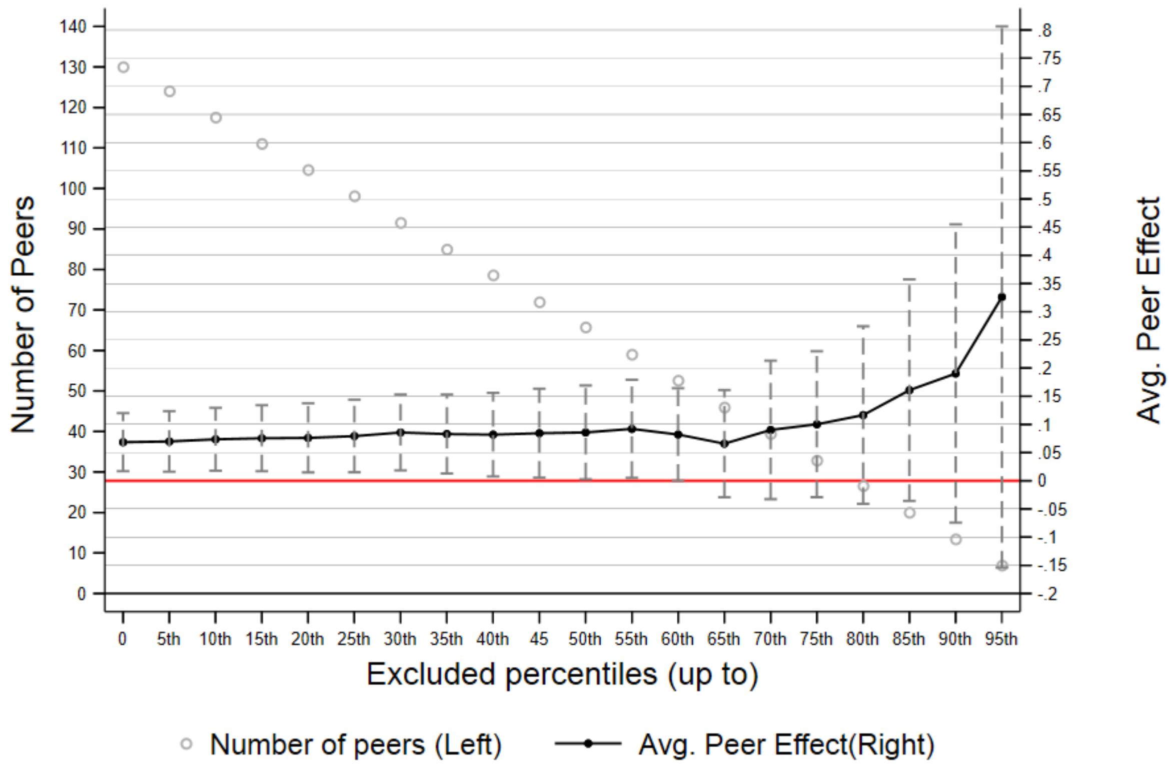
First, to assess the plausibility of the imposed network structure, I show that restricting the provider peers, to peers closer to the focal practice in spatial terms increases the estimated average peer effect. As proximity-measure I use the degree of overlap of *potential* patients for each provider-peer pair and rank each peer by percentile of proximity. In this way the ranking reflects relative proximity irrespectively of the number of peers. If the effects monotonically increases in proximity, it would indicate that the provider-pairs with more overlap of potential patients systematically are more important in determining the leniency of the focal provider. The results are presented in figure C.1. I gradually exclude peers located further away and estimate the model (2) based on the increasingly restricted samples. I report the *average peer effect* (right 2nd axis) and the average number of peers (left 2nd axis). The horizontal axis measures the degree of overlap in percentiles. Starting from the left, the first estimate excludes the lowest 5 percentiles - that is those furthest away from the focal practice. Moving to the right on the 1st axis, peers further away from the focal practice are gradually excluded, and the peer effect is estimated based on peers who on average are closer to the focal practice. Excluding peers in the lowest 25 percentiles of overlap leads to an estimate that implies that a one standard deviation increase in prescription leniency of a uniform randomly chosen peer, leads to an increase in prescription leniency of 7.3% of a standard deviation. The estimates are stable until the peer set is restricted to peers above the 70th percentile of proximity(effect of 9.4%), after which it increases. Estimating off the 5th percentile closest practices (based on an average of 7 peers) yields an average peer effect that is 6 times as large as the baseline results. As the effects only increase as the peer-set is reduced, I feel confident concluding that estimates based on the full set of practices constitute a conservative lower bar. It also suggests that practice operating in the immediate vicinity of the focal, are more likely to influence the focal practice.

The need to include peers that on averages are less influential peers is due to the need for identifying variation. I illustrate this point in figure C.2. Restricting the peer-set to those in the immediate vicinity, leads to a reduction in the share of practices who experience changes in peer composition over time. In the unrestricted set of peers, 56% of practice experience changes in their peer set in a given year. Removing every peer with below median overlap, only reduces this number to 46 %. For the peer-set in the 5^{th} percentile closest vicinity, this number drops to 12%. As the analysis is predicated on within-practice variation in peers across time, the increasingly restrictive definition of peers, leads to decreased variation on which to estimate the peer effects. That is, there exist a trade-off between *economic* significance and *statistical* significance when estimating the impact of neighbouring practices.

A frequently implemented approach to deal with potential varying influence of peers, is to weigh peers according to their expected influenceDe Giorgi *et al.* (2020). There is however no strict guidelines as to how to assess importance to lean on, and I choose to follow De Giorgi *et al.* (2020) and weigh each focal j to peer k increasing by the percentile ranking of proximity denoted p_{jk}.I present the results for a range of weightings in table C.I. As the size of the peer sets changes when weights are applied, I report the average peer effects. In columns (1) I reproduce the result from column (6) in table IV where every peer is weighted equally. In columns (2) through (6) I present results from different specifications of the weighting scheme. Increasing the weight put on providers spatially closer to the focal provider increases the average peer effect, but decreases the precision of the effects.

Providers might react also react stronger to peers that are observationally similar. This is reffered to in the network literature as *Homophily* (Boucher and Fortin (2015)). To check whether this is the case, I follow De Giorgi *et al.* (2020) and construct weights that mitigate the similarity between physician practices by

Figure C.1. Peer effects with varying peer-sets



Notes: Peer effects estimated on varying peer sets - restricting the sample to peers with an increasingly higher level of overlap of catchment areas. Bootstrapped standard errors are clustered at the provider level

constructing a measures of similarity. Let D_{jk} denote the similarity between practice j and k and let A^q_{jt} and I^q_{jt} denote the quartile of mean age and mean income respectively and G^q_{jt} be an indicator for having a female in practice j in year t. The difference is then defined as

$$D_{jkt} = |A^q_{jt} - A^q_{kt}| + |G^q_{jt} - G^q_{kt}| + |I^q_{jt} - I^q_{kt}| \tag{5}$$

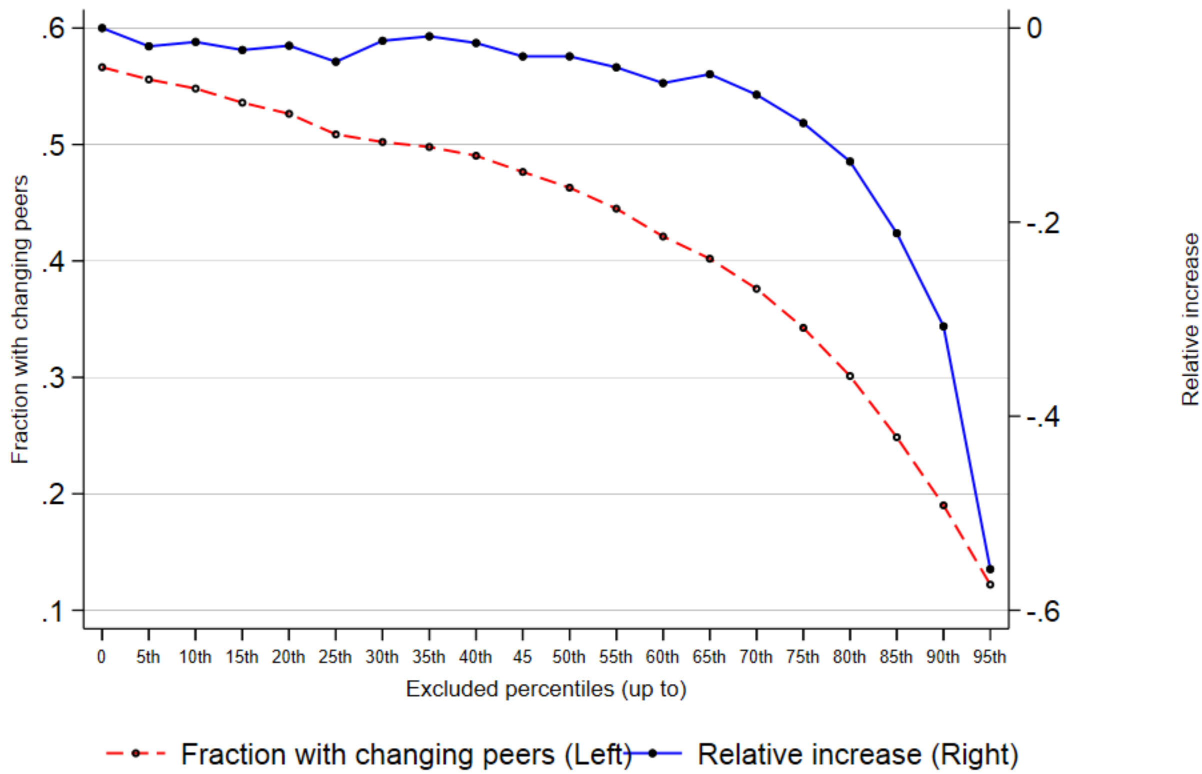
This difference takes the value 0 if the practices are completely alike in terms of which quartiles they belong to, and it takes the value 9 if the practice differ completely in the three dimensions[26] The similarity weighs are calculated as,

$$w_{jk} = (1 + D_{jk})^{-2}.$$

The results from applying the similarity weighs are reported in column (7) of table C.I, and an exponential version is presented in column (8). The estimate of average peer effects weigthed by similarity weight base the estimates off a very small set of peers (12 on average), which leads to larger but less precise average peer effects.

[26] the index also varies over time, but for simplicity the t subscript is omitted

Figure C.2. Fraction of practices who experience changes in the peer set



Notes: Share of practices who experience a change in peer-set in a given year. The graph gradually excludes peer further away, such that a 0.05 on the $1st$ axis, the providers in the 5^{th} percentile of overlap is excluded. The left axis show the fraction with any change, and the right axis plot the change relative to last 5 percentile increment.

Network Formation

In this section, I investigate a remaining threat to interpreting the results as network effects opposed to . This is the possibility that even restricting the analysis to within-practice variation, high leniency providers on average prefer to work in treatment areas with other high leniency providers. If this is the case, the co-varying prescription leniency could reflect physicians with similar prescription behavior working close to each other. Hence the estimated effects would be contaminated by correlated effects ((Angrist, 2014)). First, I test whether practices that are similar in terms of similarity are located closer together. Then I proceed to provide suggestive evidence that unobservables at least does not drive selection out of networks.

When it comes to observables I regress the proximity ranking in percentiles on each of the components in the similarity index from (5). If similar physician systematically choose to practice closer to each other the coefficients on the similarity index would come up significant.

$$p_{jtk} = \alpha_j + \beta_1 D^A_{jtk} + \beta_2 D^G_{jtk} + \beta_3 D^I_{jtk} + \varepsilon_{jt} \tag{6}$$

Where $D^A = |A^q_j - A^q_k|$, $D^G = |G^q_j - G^q_k|$, and $D^I = |I^q_j - I^q_k|$. The p-values from the F-tests are 0.32, 0.24, and .59 for the components respectively. This indicates that selection on observables is not a severe

Table C.I. Peer effects with varying weightings.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	Leniency	Leniency	Leniency	Leniency	Leniency	Leniency	Leniency	Leniency
Avg. Peer Effect	0.069***	0.120*	0.166*	0.204	0.236	0.105**	0.272**	0.152***
	(0,025)	(0.055)	(0.093)	(0.135)	(0.18)	(0.041)	(0.105)	(0.064)
Observations	14,252	14,252	14,251	14,253	14,251	14,251	13,394	13,394
R-squared	0.732	0.744	0.762	0.781	0.798	0.734	0.748	0.735
Weighted number of Peers	130	66	44	33	27	83	12	49
Weights	Equal	Linear	Squared	Cubed	Quadrupled	Exponential	Similarity	Sim-Exp

Notes: 2SLS weigthed by peer proximity. In Column 1 all peers are weighted equal as in tab IV. Columns (2) to (5) apply weights with different exponents. Column (6) apply an exponential weigthing. Column (7) presents results from a model where peers are weighted according to their similarity based on 5. Column (8) applies similarity weights that are also exponential. All columns include peer contextuals, own contextuals, local regional controls, time and practice fixed effects. Bootstrapped standard errors are clustered at the practice level. $^{***}p < 0.01$, ** $p < 0.05$, $^{*}p < 0.1$.

problem.

As I do not observe physicians prior to setting up practice, I must rely on the institutional setting to argue against selection *into* networks based on unobservables. Given that peers are practice specific and decision on where practices should open is restricted by the publicly controlled openings, the ability to choose your exact peer set is virtually impossible. Hence, conditioning on practice fixed effects (effectively a very local regional dummy) makes it implausible that providers can select their network perfectly - if not impossible.

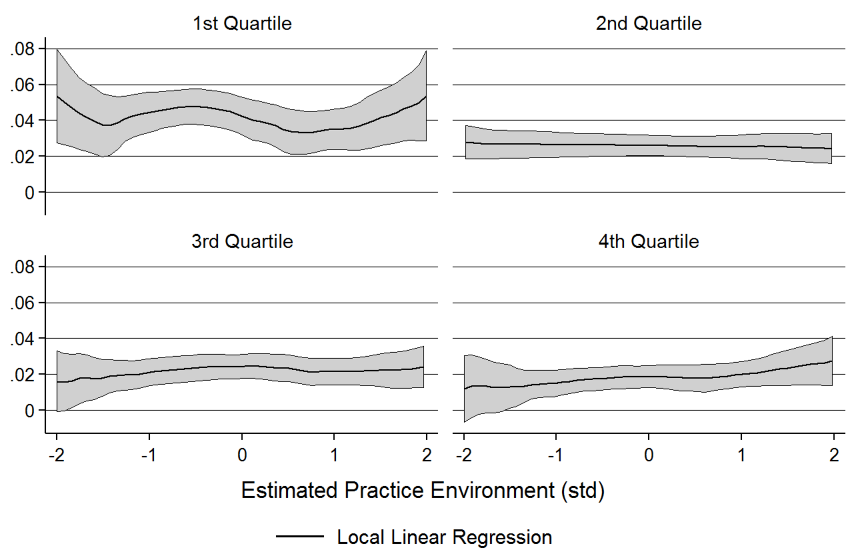
While accounting for selection into networks ultimately is a matter of assumption, selection *out* of networks can be tested. In the following, I provide evidence that the local leniency level does not appear to push out providers deviating from the norm. Figure C.3 contains four panels presenting local linear regressions of the probability to exit from the market next period on simulated leniency. The figure contains 4 panels, one for each quartile of leniency level of the focal practice as estimated from (2). If there was a significant trend in either of these panels, it would indicate the presence of non-random exiting. This would invalidate the peer effects interpretation. Even though the levels differ somewhat, there is no detectable trend for either of the quartiles of leniency. While the argumentation above is not a formal test, it does provide suggestive evidence against non-random selection into specific local treatment practices. Table C.II report equivalent OLS estimates of the impact of mean simulated leniency among peers on the probability of exiting the market.

Table C.II. Impact of practice environment on probabilty of exit following period

Focal Opioid Treatment Leniency :	1^{st} Quartile	2^{nd} Quartile	3^{rd} Quartile	4^{th} Quartile
Practice environment	-0.010	-0.024	-0.015	0.051
(Std.)	(0.058)	(0.042)	(0.056)	(0.042)
Observations	3,096	2,996	3,035	3,275

Notes: Impact of practice environment on probabilty of exit. OLS estimates of the effect of the practice environment on the probability of exiting conditional on quartile of leniency.

Figure C.3. Probability of exiting by leniency level of focal practice



Notes: Epanechnikov Kernel weighted local linear regression of probability of exiting next period on local leniency level by quartiles of leniency level of focal practice

D Appendix D

In this appendix I expose my results to a series of robustness checks, to ensure that my estimates of labor market impacts actually reflect the effect of increased opioid treatment intensity. I provide suggestive evidence that i) simulated leniency is not driven by patient selection on observables ii) that controlling for the trend in other treatment dimensions does not affect the results, iii) that other treatment styles does not correlate with opioid treatment intensity, and finally vi) that patient links does not contaminate the estimates.

To investigate whether simulated leniency suffers from selection on observables, table D.I reports a balance test. This table contains three columns that report the p-values from F-tests of individuals covariates on three different outcomes. All columns contain year dummies and practice fixed effects. The first outcome is the mean defined daily dose of opioids prescribed to other patients at the practice. Age, sex, charlson comorbidity and education levels of opioid users are all highly correlated with average opioid use. The next column report the results from a regression of opioid leniency on individual covariates. Here both age and sex are statistically significantly correlated with prescription leniency, while comorbidities and educational attainment are not. The significant correlations indicates selection into treatment if either leave-one-out mean opioid prescriptions or prescription leniency were used as instrument of opioid use. The last column show how neither of the individual characteristics are associated with the simulated leniency. This provides some evidence that simulated leniency as instrument does not suffer from selection on observables.

Table D.I. Regression of Simulated Leniency on individual covariates

	(1)	(2)	(3)
Outcome	Mean Opioid	Leniency	Simulated Leniency
Age	0	0.01	0.467
Male	0	0.07	0.819
Charlson Comorbidity	0	0.589	0.533
Education	0	0.194	0.259
Observations	1,211,424	1,211,424	1,211,424
R Squared	76%	83%	99%
Time fixed effects	Yes	Yes	Yes
Prac. fixed effects	Yes	Yes	Yes

Notes: The table contains p-values from on F-test of individual characteristics in a regression on (1) mean opioid prescriptions , (2) prescription leniency and (3) simulated leniency. Age and charlson comorbidity are a third order polynomials, male is a dummy, and education is 6 mutually excluding categories. Standard errors are clustered at the practice level

Even though my regressions include a practice fixed effect, and balancing tests indicate that the simulated leniency in fact is a valid instrument, one might still be concerned that it does correlate with other dimensions of provider practice style. That is, whether dynamics of treatment behaviour beyond that of opioid prescription intensity drives the estimated effects. I provide two checks, to alleviate these concerns. To alleviate these concerns I check whether the quality of the practice can explain the effects. To quantify this, I construct time-varying treatment style indicators for each practice and include these as additional controls. The indicators included are,

1. **Q1: Testing for Strep-A before prescribing anti-biotics:** The fraction of all patients at a practice with a pharmacy claim of anti biotics, who had a strep-A test done prior to the pick-up.

Table D.II. Effects of opioid usage on labor market outcomes

Panel A						
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	LMI	LMI	LMI	LMI	LMI	LMI
Log(DDD)	-0.030***	-0.029***	-	-	-0.045***	-0.043***
	(0.001)	(0.001)	-	-	(0.012)	(0.013)
Sim. Len	-	-	-0.001***	-0.001***	-	-
	-	-	(0.0002)	(0.0002)	-	-
Observations	1,199,029	1,199,029	1,199,029	1,199,029	1,199,029	1,199,029
Outcome Mean	0.386	0.386	0.386	0.386	0.386	0.386
R squared	0.07	0.18	0.03	0.15	0.03	0.15
Time	Yes	Yes	Yes	Yes	Yes	Yes
Ind Covariates	No	Yes	No	Yes	No	Yes
IV	No	No	No	No	Yes	Yes
Practice FE	Yes	Yes	Yes	Yes	Yes	Yes
Treatment Evolution	Yes	Yes	Yes	Yes	Yes	Yes

Panel B						
	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	Any SD	Any SD	Any SD	Any SD	Any SD	Any SD
Log(DDD)	-0.026***	-0.014***	-	-	0.034	0.039
	(0.0001)	(0.0001)	-	-	(0.028)	(0.026)
Sim. Len	-	-	0.0005	0.0005	-	-
	-	-	(0.0004)	(0.0004)	-	-
Observations	1,227,153	1,227,153	1,227,153	1,227,153	1,227,153	1,227,153
Outcome mean	.17	.17	.17	.17	.17	.17
R squared	0.02	0.08	0.01	0.07	0.01	0.07
Time	Yes	Yes	Yes	Yes	Yes	Yes
Ind Covariates	No	Yes	No	Yes	No	Yes
IV	No	No	No	No	Yes	Yes
Practice FE	Yes	Yes	Yes	Yes	Yes	Yes
Treatment Evolution	Yes	Yes	Yes	Yes	Yes	Yes

Notes: OLS, Reduced form and 2SLS estimates of Labor market income percentile ranking and days on short term disability on Opioid usage. Columns (1) - (2) present results from OLS, columns (3) - (4) present the Reduced form estimates and columns (5)-(6) present the 2SLS estimates of using simulated leniency as instrument of opioid usage. All columns include a practice fixed effect and time-varying treatment evolution. Individual covariates are gradually included. Standard Errors are clusted on the practice level. ***$p < 0.01$, ** $p < 0.05$, *$p < 0.1$

2. **Q2: Monitoring of cardio-vascular diseases:** the fraction of all patients at a practice with a prescription for statins with a blood test performed to monitor the lipid level

I report the results in appendix table D.II. This table replicates table I adding these time-varying measures of treatment behaviour. The measures are continuous and are based on all patients at the practices. Furthermore, they are test-based and thus reflect in-practice actions completely in control of the provider. My results are qualitatively the same, under this alternative specification.

Finally, while it is highly improbable that patients would directly select a specific practice based on the evolution of exogenous characteristics of third level peers of that practice there is a potential "back-door" that would invalidate the exclusion restriction in (4). When I instrument with 3rd level peer characteristics and show in the first stage that these matter, it might simply be that the *patients* of third-level peers know the *patients* of the focal practice. If this is the case, these patients of the third level peer, who potentially had selected into high prescribing practices, might simply obtain the opioids and give them to the patient at the first-level provider. Of course, I do not have information on social ties in the registers, and this back-door channel ultimately must be assumed away. However, to get an idea of whether connected opioid users drive the results, I can check whether individuals with a history of or currently working in the same firm drives the effect . In appendix table D.III I present results where colleagues are removed from the sample. This is more restrictive than removing only coworkers who are at third level peers of your practice, as these would be a subgroup of the overall set of colleagues. I categorize individuals in three increasingly restrictive groups.

1. **Current Coworkers:** Working together in the same year (removing 293,946 person by year observations from my sample)

2. **Previous Coworkers:** Working in the same firm in same year, or previously having done so (removing 50,651 person by year observations)

3. **Previously or currently a coworker:** Working in the same firm at any point during the observation-period. (removing 344,597 person by year)

Table D.III reproduces the instrumented effects of opioid use on labor market income applying each sample criterion. While the precision is reduced, neither of the results are statistically or economically different from my main specification. I take this as evidence indicating that the patient-to-patient channel does not explain the results.

Table D.III

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	LMI	LMI	LMI	LMI	LMI	LMI
Log(DDD)	-0.043***	-0.044***	-0.044***	-0.043***	-0.043***	-0.046***
	(0.013)	(0.011)	(0.012)	(0.011)	(0.013)	(0.013)
Observations	905,083	905,083	1,148,378	1,148,378	854,428	854,428
R-squared	0.04	0.17	0.03	0.15	0.03	0.16
Time	Yes	Yes	Yes	Yes	Yes	Yes
Ind Covariates	No	Yes	No	Yes	No	Yes
Prac. FE	Yes	Yes	Yes	Yes	Yes	Yes

The table presents regressions of (4) for three increasingly restricted subgroups. In the first subgroup, all individuals who have a current coworker in the sample are omitted. Second, all individuals who in a given year had a coworker in the sample is removed. Third, all who either previously had a coworker ever is working with another opioid user is removed. Outcomes is next years labor market income percentile rank. All regressions include time dummies and practice fixed effects. Clustered standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

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