NBER WORKING PAPER SERIES

HOW WELL DO DOCTORS KNOW THEIR PATIENTS? EVIDENCE FROM A MANDATORY ACCESS PRESCRIPTION DRUG MONITORING PROGRAM

Thomas C. Buchmueller
Colleen M. Carey
Giacomo Meille

Working Paper 26159
http://www.nber.org/papers/w26159

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2019

The authors are grateful to David Hopkins and Adam Berrones from KASPER and Amanda Garrett and Kara Slusser from INSPECT for assistance in accessing the data and understanding the details of the Prescription Drug Monitoring Programs in Kentucky and Indiana. We thank Mike Mei for sharing ARCOS data. Jamie Fogel, Rebecca Haffajee, Bernardo Modenesi, Anita Mukherjee, Edward Norton, Adam Sacarny, and seminar participants at University of Michigan provided helpful comments. The authors have no conflicts of interest to disclose. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

How Well Do Doctors Know Their Patients? Evidence from a Mandatory Access Prescription Drug Monitoring Program

Thomas C. Buchmueller, Colleen M. Carey, and Giacomo Meille

NBER Working Paper No. 26159
August 2019
JEL No. H75,I12,I18

ABSTRACT

Many opioid control policies target the prescribing behavior of health care providers. In this paper, we study the first comprehensive state-level policy requiring providers to access patients' opioid history before making prescribing decisions. We compare prescribers in Kentucky, which implemented this policy in 2012, to those in a control state, Indiana. Our main difference-in-differences analysis uses the universe of prescriptions filled in the two states to assess how the information provided affected prescribing behavior. As many as forty percent of low-volume opioid prescribers stopped prescribing opioids altogether after the policy was implemented. Among other providers, the major margin of response was to prescribe opioids to approximately sixteen percent fewer patients. While providers disproportionately discontinued treating patients whose opioid histories showed the use of multiple providers, there were also economically-meaningful reductions for patients without multiple providers and single-use acute patients.

Thomas C. Buchmueller
Stephen M. Ross School of Business
University of Michigan
701 Tappan
Ann Arbor, MI 48109
and NBER
tbuch@umich.edu

Giacomo Meille
University of Michigan
gmeille@umich.edu

Colleen M. Carey
Department of Policy Analysis and Management
Cornell University
MVR Hall, Room 298
Ithaca, NY 14853
and NBER
colleenmariecarey@gmail.com

I. Introduction

The number of prescription opioids filled in the U.S. increased by roughly 300% in the first decade of the twenty-first century (Kunins, Farley and Dowell, 2013), contributing to a similarly dramatic increase in overdose deaths (Chen, Hedegaard and Warner, 2014; Dart et al., 2015; Rudd et al., 2016). Although the total volume of prescriptions has declined since 2010, nearly 2 million Americans had an opioid addiction in 2015 (Han et al., 2017). While illicitly imported or manufactured narcotics are now a significant contributor, the epidemic has its roots in the misuse of prescriptions legally obtained from medical professionals. Thus, a number of policy responses to the opioid epidemic have targeted prescribing behavior.

Among the most significant policy responses to this public health crisis are state-level Prescription Drug Monitoring Programs (PDMPs). These systems track all purchases of DEA-scheduled drugs in the state and generate patient histories. The data are often used to flag inappropriate or suspicious utilization. Current programs are designed to influence the behavior of healthcare practitioners by providing comprehensive and timely information about patients' prescription histories. Forty-nine states have established a PDMP and many have strengthened their programs over time.

Historically, most PDMPs relied on providers to take the initiative to access patient prescription histories. Evidence from several states suggests that when PDMPs are voluntary, provider engagement is low (Haffajee, Jena and Weiner, 2015). This may explain results from several studies, which find no effect of PDMPs on a variety of opioid-related outcomes (Paulozzi, Kilbourne and Desai, 2011; Jena et al., 2014; Brady et al., 2014; Li et al., 2014). Since 2007, 17 states have increased provider engagement via "mandatory access" laws. These policies require prescribers to consult the PDMP in certain circumstances before prescribing opioids and other DEA-scheduled drugs. Recent studies suggest that such mandates reduce the volume of opioids prescribed and indicators of misuse (Wen et al., 2017; Buchmueller and Carey, 2018; Meinhofer, 2018; Haffajee et al., 2018) and fatal opioid overdoses (Dowell et al., 2016).

The first comprehensive "mandatory access" policy was enacted and implemented in Kentucky in 2012. It required all providers in the state, with limited exceptions, to check the PDMP before prescribing opioids to new patients and at intervals for continuing patients. In contrast, previous mandates in other states applied only to certain types of providers or circumstances. Subsequently, several other states (including New Mexico, New York, Tennessee and West Virginia) enacted similar laws. Thus, Kentucky represents an excellent case study for investigating the impact of comprehensive "mandatory access" legislation on opioid prescribing.

We examine how this policy altered the prescribing behavior of Kentucky providers compared to providers in a neighboring control state, Indiana. Indiana represents a good counterfactual for Kentucky for several reasons. Both states were among the top ten in opioid prescriptions per capita in 2012 (Paulozzi, Mack and Hockenberry, 2014). The two states are also very similar in terms of demographics, economic conditions, health systems, and health insurance coverage during the period of our analysis. Furthermore, until Kentucky's 2012 reform, the two states' PDMPs and other opioid policies were quite similar, as detailed in Section II.

We estimate the policy's effect on the total morphine-equivalent dosage (MED) prescribed by each provider in each quarter. We find that after the policy went into effect, Kentucky providers significantly decreased MED prescribed relative to Indiana providers. To shed light on the changes providers made, we estimate the effect of the policy on four distinct margins: (1) whether the provider writes any opioid prescriptions; (2) the number of patients to whom they prescribe opioids; (3) the number of days supplied per patient; and (4) the average MED per day.

Our results suggest that providers primarily responded along the first two margins. After the policy went into effect, there was a 3.8 point decline in the percentage of providers writing opioid prescriptions in Kentucky (relative to the change in Indiana). Among providers that wrote any opioid prescriptions in a quarter, there was a roughly 16% decline in the number of patients. Decreases in the days per prescription and MED per day are smaller in magnitude and sensitive to specification.

To test for heterogeneous policy effects, we sort providers into quartiles based on their prescribing in the six months prior to the mandatory access policy. Like previous research, we observe substantial variation in opioid prescribing across providers. In our data, prior to the policy change, providers in the top quartile account for 97% of total MED supplied, while, conditional on prescribing, the modal provider in the lowest quartile had only one patient with an opioid prescription. The heterogeneity analysis reveals that the decrease in the percentage of providers writing opioid prescriptions was largely limited to low-volume providers. Among high-volume providers, the main response to the policy was to prescribe opioids to fewer patients.

Ideally, increasing provider PDMP engagement will not simply reduce opioid prescribing, but will result in more appropriate prescribing. We thus investigate whether providers targeted their reductions on patients with histories suggestive of high-risk use. We characterize patients using three mutually-exclusive categories. The first consists of "single use" patients who fill a single prescription in a quarter and none in the following quarter. This utilization pattern suggests post-surgical acute care and is generally considered low risk.

We divide patients who fill multiple prescriptions into two groups, depending on whether or not they exhibit behaviors consistent with "shopping," which we define as obtaining opioids from three or more prescribers or pharmacies in a quarter.

The results from this part of the analysis suggest that providers target their reductions on those who meet the "shopping" criteria. The average provider reduced the total number of patients with opioid prescriptions by 19% and the number of "shoppers" by one-third. Providers reduce opioid supply to other patient types by a smaller, but statistically significant amount. Prescriptions to single-use patients fell by 12%. Thus, while prescriptions to "shoppers" were most affected, the mandatory access policy may have induced a broader "chilling" effect on opioid prescribing.

II. PDMP and Other Opioid Policies in Kentucky and Indiana

By 2012 both Kentucky and Indiana had well-established PDMPs. The Kentucky All Schedule Prescription Electronic Reporting (KASPER) system first became operational, with its data available to providers and dispensers, in July 1999. Indiana's PDMP, known as the INSPECT system, was established one year earlier, but access was initially limited to state regulators; providers and dispensers gained access in March, 2009. Law enforcement agencies in both states are allowed to access the data in connection with ongoing investigations. Both systems capture data on prescriptions for DEA Schedule II - V drugs. During our entire sample period, physicians in both Indiana and Kentucky could delegate access to the PDMP to a nurse or other employee.

Kentucky's mandatory access requirement was established by House Bill 1 (HB 1), which was passed in April 2012 and went into effect that July. The law requires all providers who are licensed to prescribe DEA-scheduled drugs to register with the PDMP and refers non-compliers to the Kentucky Board of Medical Licensure. With limited exceptions, providers are also required to query the PDMP the first time they order any Schedule II prescription (and Schedule III containing hydrocodone) for a patient and every three months thereafter. During the period we analyze, providers in Indiana faced no such requirements.

It is important to note that the Kentucky mandatory access requirement did *not* change any aspect of the reporting of controlled substance prescriptions to the Kentucky PDMP. Over the entire sample period, pharmacies reported all fills of DEA-scheduled prescriptions to KASPER using established procedures, which are unrelated to provider registration or querying behavior.

Indiana makes a useful comparator for Kentucky due to its similarity on numerous dimensions, including other policies that might affect the demand for and supply of pre-

scription opioids, such as demographics, income, and physicians per capita.[1] Neither state allowed the medical or recreational use of marijuana. During the period of our analysis, Indiana did not have a law allowing access to naloxone. In Kentucky, such a law went into effect in June 2013. One difference between the two states is that HB 1 also included a provision regulating pain clinics. According to KASPER, between mid-2012, when the policy went into effect, and mid-2015, 24 pain clinics closed. While we cannot definitively disentangle the effect of the mandatory access policy from these pain clinic provisions, we provide evidence suggesting that our results are driven by the mandatory access policy.

Figure 1 uses raw administrative data on the monthly number of requests for KASPER records between 2010 and 2016. The gray bar is at 2012q3, when the mandatory access policy was implemented. The figure shows a five-fold increase in KASPER requests coincident with the policy implementation. Similarly, the number of providers registered with KASPER rose from 37% of DEA registrants in June 2012 to 97% a year later (Freeman et al., 2015). Available evidence suggests that during the period of our analysis PDMP queries in Indiana were comparable to pre-period usage in Kentucky (Allain, 2012).

III. Data and Descriptive Analysis

A. Aggregate State-Level Data

Data from Indiana's PDMP begins in the first quarter of 2012, only two quarters before mandatory access went into effect in Kentucky. Thus, it is not possible with our main data sources to test for parallel pre-trends in the two states. We address this issue by considering other data sources. Figure 2 presents annual data from the Centers for Disease Control (CDC) on opioid prescriptions per capita and quarterly data from the DEA's ARCOS database on MED[2] per capita for the years 2006 to 2016.

Both sources indicate that opioid prescriptions in Kentucky and Indiana were trending in a roughly parallel fashion from 2006 through 2011. The ARCOS series, but not the CDC series, shows a level shift up in MED beginning with 2010q2. This level shift is due entirely to oxycodone, which jumps up in Kentucky and continues a near-linear trend in Indiana. The timing of the shift in oxycodone shipments to Kentucky coincides with two important events related to this commonly abused drug.

One is the reformulation of the extended release version of the drug, Oxycontin. Al-

[1] A disadvantage of using a neighboring state is the potential for cross-border contamination. In the Appendix, we show our results are virtually unchanged when we exclude the regions near the border where cross-border contamination is expected to be the largest.

[2] We converted ARCOS and PDMP opioid prescriptions to their morphine equivalents using conversion factors from the following three sources: Palliative.org (2016); CMS (2015); Ohio Bureau of Workers' Compensation (2016).

though this change had a large effect on the demand for oxycodone products and their substitutes, there is little reason to expect a large positive effect on shipments to Kentucky relative to Indiana. Alpert, Powell and Pacula (2016) show that Kentucky and Indiana had the same rates of Oxycontin misuse before the reformulation and thus were similarly exposed to it.

The other change occurring around this time is a major crackdown on pill mills in Florida (Kennedy-Hendricks et al., 2016). These clinics were widely reported to be a source of drugs sold in other states. Indeed, Interstate 75, which runs through both Florida and Kentucky, was dubbed the "Oxy Express." Evans, Lieber and Power (2019) develop a measure for cross-state comparisons that suggests Kentucky was obtaining considerably more opioids from Florida than Indiana was prior to the crackdown. Thus, it is very plausible that as the supply from Florida was reduced, the demand for oxycodone from in-state providers increased more in Kentucky than Indiana.

Table 1 examines trends in aggregate quarterly per capita MED consumption in Kentucky and Indiana using ARCOS data from 2006q1 to 2012q2. The models in columns 1 and 2 include just an indicator for Kentucky, a linear trend and the interaction of the two. In columns 3 and 4, we include a second Kentucky intercept to capture the level shift in oxycodone shipments after the Florida pill mill crackdown.

Using the logged outcome (column 1), the results indicate that in both states the volume of opioids grew by roughly 8% per quarter over the seven year period. When the dependent variable is specified in levels, the simpler specification suggests stronger growth in Kentucky in the pre-period. However, when we include a second intercept for Kentucky from 2010q2 onwards, we no longer find a difference in the time trends between Kentucky and Indiana. This suggests that indeed there was simply a level shift in Kentucky rather than a divergence in the trend.

Table 2 reports difference-in-differences estimates using quarterly MED per capita for 2006q1 to 2013q4, before and after Kentucky's implementation of the mandatory access policy (but prior to the Affordable Care Act expansions). Because the implementation quarter (2012q3) is partially treated we allow it its own dummy. In these results we again consider the impact of allowing Kentucky a second intercept for the period beginning in 2010q2. Including this additional variable does not alter the results in a qualitative sense. The first column suggests that the PDMP mandate reduced the volume of opioids in Kentucky by 11%. Allowing Kentucky to have a second intercept (similar to beginning the analysis in 2010q2) increases our estimate of the policy impact to 14%. Similarly, when the dependent variable is specified in levels both specifications indicate that the effect of Kentucky's PDMP mandate was statistically and economically significant.

B. Prescription Records

Via data use agreements with KASPER and INSPECT, we obtained the states' complete PDMP records. Each record contains the following fields: encrypted identifiers for patients, providers and pharmacies, National Drug Code (from which we derive ingredient, strength, and route of administration), number of units, days supply, patient zip code, and provider location.

Our analysis period begins in 2012q1. Though PDMP data for both states are available through 2016, we end our sample period in 2013q4 to avoid a possible confounding effect of the Affordable Care Act. Kentucky implemented the ACA Medicaid expansion in January 2014 and also established its own marketplace. Indiana did not expand Medicaid until 2015 and participated in the Federal Healthcare.gov marketplace. Whereas in 2013 a similar percentage of each state's population was uninsured (14.3% in Kentucky, 14.0% in Indiana), between 2013 and 2014, the percent uninsured declined by 5.8 percentage points in Kentucky compared to only 2 points in Indiana (Smith and Medalia, 2015).

A key advantage of PDMP administrative data over other opioid utilization data is that we observe all or substantially all of an in-state provider's outpatient opioid prescribing,[3] which allows us to conduct a provider-level analysis. By comparison, analyses of aggregate opioid supply (e.g., ARCOS data) do not report data at the provider level, and claims from a subsample of patients (e.g., Medicare data) do not fully capture a provider's prescribing behavior. In particular, our PDMP data include all cash purchases, which is predictive of other suspicious behaviors (Cepeda et al., 2013).[4] Therefore, analyses based on PDMP data yield the maximal insight on how providers respond to mandatory access policies.

We limit our sample to providers who practiced in Kentucky or Indiana. Prescriptions filled in Kentucky and Indiana but written by out-state providers are disregarded because those providers are subject to other states' opioid regulations. Our main analyses are done on a balanced panel consisting of quarterly observations of providers who wrote at least one opioid prescription in any quarter between 2012q1 and 2013q4.

[3]KASPER and INSPECT capture about 95% of the total MED shipped to a state (as reported by ARCOS) or about 95% of all prescriptions filled in the state (as reported by the CDC). We expect the PDMP to capture less than 100% of the ARCOS volume, which includes opioids administered to hospital inpatients (not reported to PDMPs). The CDC data is based on a sample of retail pharmacies; the CDC does not give detailed information about its methods.

[4]While our PDMP datasets always report prescriptions purchased with cash, information on the source of payment is not available in Kentucky until 2015. In 2015, after both states had expanded their Medicaid programs, 8% of prescriptions in KASPER and 10% of INSPECT prescriptions were purchased with cash. Prior to Medicaid expansion, 14% of Indiana's prescriptions were purchased with cash.

C. *Hypotheses and Outcome Measures*

There are several possible provider responses to a PDMP use mandate. The requirement that providers have an active account and check the database before prescribing opioids introduces fixed compliance costs. Some providers may cease prescribing opioids altogether rather than bear the cost associated with learning to navigate the system. Therefore, we test whether the policy induced a change on the extensive margin of writing any opioid prescriptions in a quarter.

Fundamentally, PDMPs are designed to alert providers of possible doctor shopping and other suspicious patterns of patient behavior. Previous research based on Medicare claims data finds that mandatory access policies significantly reduce the number of patients receiving prescriptions from multiple providers and the number of "new patient" visits (Buchmueller and Carey, 2018). Thus, we hypothesize that among providers who continue to prescribe opioids, the strongest effect of a mandatory access policy will be on the number of patients to whom they prescribe.

It is possible that a provider encountering a PDMP report that suggests a patient is overusing opioids may not refuse to prescribe to that patient, but rather will write a weaker prescription in hopes of weaning the patient off high-dosage or chronic opioid use. Additionally, the mandate may indirectly affect prescribing intensity. In a survey of Kentucky prescribers, roughly three-quarters of respondents said they believed they were being more closely monitored after the policy went into effect (Freeman et al., 2015). This perception may have led some to prescribe more conservatively. And checking the PDMP more often may affect prescribing intensity by raising the salience of safe prescribing practices. We analyze two measures of prescribing intensity: days supplied per patient and the average MED per day.

Table 3 presents summary statistics for these outcomes aggregated to the provider×quarter level. In the pre-period (2012q1 and 2012q2) the percentage of providers prescribing any opioids was identical in Indiana and Kentucky (74%). Conditional on prescribing opioids, the average Kentucky provider prescribed to more patients (60.2 vs. 54.5). MED per provider are higher in Kentucky because of the difference in the number of patients; the baseline means for days/patient and MED/day were essentially identical in the two states. Figure 3, which presents the provider-level distribution of log MED prescribed for the pre-period, also indicates that prescribing patterns were quite similar in the two states before Kentucky's policy change.

After the policy change, the number of Indiana providers writing any opioid prescriptions increased by 2 percentage points, while the percentage in Kentucky fell by 2 points. The number of patients per provider fell in both states, but more so in Kentucky (-6.1 vs.

-2.4). There was essentially no change in either intensity measure in either state. Overall, the mean MED per provider fell by 9.4% in Kentucky and by 0.4% in Indiana.

IV. Econometric Analysis

A. Overall Impact of Kentucky's Mandatory Access Provision

The total (MED) quantity of opioids a provider i prescribes in quarter t, Y_{it}^{tot}, can be expressed as the product of the other four measures presented in Table 3:

$$Y_{it}^{tot} = \underbrace{Y_{it}^{1}}_{\text{any prescriptions}} * \underbrace{Y_{it}^{2}}_{\text{\# of patients}} * \underbrace{Y_{it}^{3}}_{\text{days per patient}} * \underbrace{Y_{it}^{4}}_{\text{MED per day}}$$

We estimate separate difference-in-differences regressions for each outcome. The econometric specification is:

$$Y_{it}^{j} = \alpha^{j} KYpost_{it} + \beta^{j} KY \times 2012q3_{it} + \delta_{t}^{j} + \delta_{i}^{j} + \varepsilon_{it}^{j}, j = 1, 2, 3, 4 \quad (1)$$

where j indexes the four separate margins that providers may adjust in response to the policy. The policy variable, $KYpost$, equals 1 for Kentucky providers beginning in 2012q4 and 0 elsewhere; Kentucky's partially-treated implementation quarter, 2012q3, is accounted for with its own dummy variable. Because the data from both states include encrypted provider identifiers, we are able to condition on provider fixed effects (δ_i). Our models also include year by quarter fixed effects (δ_t). To account for within-provider serial correlation, we cluster ε_{it} at the provider level. With only two states, asymptotics for consistency will not apply if standard errors are clustered at the state level, but we explore inference under two alternative models in the Appendix.

Y^1 is an indicator variable that equals one if a provider wrote at least one prescription in the quarter and zero otherwise. Because of the fixed effects we specify this equation as a linear probability model. Our preferred specification for the continuous outcomes, Y^2 through Y^4, is a log-linear model, which implies that the policy had the same percent effect on all providers. And as shown in Figure 3, prior to the policy change the distribution of MED prescribed by providers appears to be approximately lognormal. Additionally, tests for model specification recommended by Deb, Norton and Manning (2017) suggest that the log model fits our data better. For robustness, we also report models where the dependent variable is measured in levels.

We also estimate an event study version of the model in which an indicator variable for Kentucky is interacted with each time dummy. Since we have a very short pre-period, we

rely on the previous analysis of ARCOS and CDC data to provide evidence on pre-period trends. We primarily use this specification to confirm that the estimated treatment effect coincides with the quarter when HB 1 was implemented and to examine the dynamics of the treatment effect in the post-period.

The event study results are presented graphically in Figure 4. Each of the four variables exhibits a sharp decline in 2012q3 relative to the previous quarter, with the full impact realized by 2012q4. The fact that the movement in the variables is so tightly linked to the policy timing is reassuring. The pattern in these event studies – a sharp change in prescribing behavior followed by parallel trends in the post-period – is well-captured by a difference-in-differences framework.

Table 4 reports difference-in-differences regressions for each of our four outcomes. The first column indicates that mandatory access reduced the probability of any opioid prescribing by nearly 4 percentage points. This significant effect on the extensive margin is consistent with the hypothesis that fixed compliance costs may have led some providers to stop prescribing scheduled drugs altogether. We provide further support for the fixed-cost hypothesis in the next section.

We hypothesize that requiring providers to check a PDMP before prescribing opioids will have the strongest effect on the number of patients receiving prescriptions. The regression results indicate a large provider response along this margin. The log specification implies that among providers writing any opioid prescriptions in a quarter, Kentucky's mandatory access policy reduced the number of patients by 16% (exp(-.177) – 1 = -.162). Specifying the model as linear in levels also yields a significant policy effect, though, relative to the sample mean, the percent effects are slightly smaller (-11%).

Estimated effects for the average days per patient and MED per day are smaller and more sensitive to specification. As noted, any effects on these margins are likely to be indirect. The changes observed for these outcomes may also reflect a change in the composition of patients receiving opioids after the policy change. As we show below, the effect of the policy on the patient margin was strongest for patients who filled multiple prescriptions. Reducing the number of high-use patients will have the effect of also reducing measures of prescribing intensity. Because of this and the sensitivity of the estimates to specification, we are reluctant to conclude that providers responded to the policy by reducing the number of days or MED per day.

B. Heterogeneity Across Providers by Pre-Period Prescribing Volume

We hypothesize that the Kentucky law had different impacts for higher and lower volume providers. Low-volume providers may be most reluctant to pay the costs of mandatory

access compliance, since opioid prescribing is not critical to their practice. Additionally, low-volume providers may not be sufficiently familiar with opioid prescribing histories to confidently interpret a PDMP record (Carey, Jena and Barnett, 2018). Thus, we expect that low-volume providers are more likely than high-volume providers to stop prescribing.

It is less obvious which types of providers will reduce the number of patients the most. High-volume providers treat hundreds of patients every quarter and are more likely to be pain specialists. Since chronic pain patients are at high risk for opioid misuse, pain specialists may be most likely to learn of suspicious behavior when they begin using the PDMP. On the other hand, these providers may *already* use the PDMP prior to the mandatory access provision. And of course, some high-volume providers may engage in illicit opioid distribution, and thus may be insensitive to the information contained in the PDMP.

To estimate heterogeneous policy effects by volume, we divide the provider sample into quartiles based on the total MED prescribed in the six months before Kentucky's policy went into effect. Table 5 provides summary statistics on providers in each quartile. All of the outcome variables that we analyze increase monotonically across the quartiles, with the differences being most pronounced for the number of patients treated. The first quartile is made up of infrequent prescribers. Only 12% wrote an opioid prescription in each quarter in the pre-period and the modal provider who did so had only one patient. Conditional on prescribing, mean days per patient and MED per day are low for providers in quartile 1 relative to other providers. This is consistent with lower-volume providers treating opioid-naive patients with short-term pain. Quartiles 2 and 3 differ mainly in terms of the number of patients to whom opioids are prescribed. Quartile 4 appears to include many pain specialists. The average provider in this quartile prescribes to a high number of patients and writes prescriptions with longer durations.

For each quartile, we estimate a separate set of regressions. These results are reported in Table 6, the first column of which repeats the full sample results from Table 4. For brevity, we report only the log models for the continuous outcomes, and show the level models in Appendix Table A3.

The first panel reports the effect of mandatory access on the probability of writing any opioid prescriptions in a quarter. We find that the lowest-volume prescribers are 5 percentage points less likely to write a prescription due to the policy change. Relative to the pre-period mean, this is a 41% decline. The estimated coefficient is slightly larger for quartile 2, though in percentage terms, the effect is smaller. The vast majority of providers in quartile 3 and 4 continue to prescribe after the policy change.

The results by provider quartile support the hypothesis that low-volume providers view

the fixed costs of mandate compliance as excessive relative to the benefits. As a further test of the fixed cost hypothesis we also examine whether the number of providers who never again write an opioid prescription increases. The results, reported in Appendix Table A4, suggest that Kentucky's access mandate did lead low-volume providers to "exit the market". We find that high-volume Kentucky prescribers are *not* more likely to cease prescribing than their Indiana counterparts. This suggests that the closure of pain clinics is not the main driver of our results.[5]

Among providers who continue to prescribe opioids, all quartiles reduce the number of patients they prescribe to. Providers in the first quartile write a prescription to 8% fewer patients; at higher quartiles, this effect size is even larger (between 15% and 17%).

The final two panels show that there are also reductions, albeit smaller, in the intensity of prescribing. Effects on log days per patient are absent for the lowest volume providers, who already write very short duration prescriptions, and small for the highest volume providers. The magnitude of the effect sizes for MED per day are monotonically decreasing. Among providers in the fourth quartile, who account for the vast majority of all opioid prescriptions, we see no significant reduction in MED per day.

C. Prescribing Reductions by Patient Type

The goal of PDMPs is to alert providers to possible drug-seeking and other indicators of high-risk use. We now examine whether providers target reductions on patients with suspicious opioid histories that would be revealed in PDMP records. We are also interested in whether providers reduce prescribing to patients *without* suspicious behaviors. Such a finding would suggest that mandatory access was associated with a general chilling effect, which potentially could have inhibited clinically appropriate prescribing.

To provide insight on how different types of patients were affected by the policy, we define three mutually exclusive patient types. In contrast to our prescriber volume quartiles, which are defined using only pre-period data, we categorize patients contemporaneously because many obtain a prescription in only a single quarter. "Single use" patients fill a single prescription in a quarter and none in the following quarter. As shown in Table 5, in the pre-period, 29% of the average provider's patients (about 12 patients) were single-use patients. Among patients observed filling multiple prescriptions we distinguish between "shoppers" and "non-shoppers." Shoppers are defined as patients who receive prescriptions from three or more providers or fill prescriptions at three or more pharmacies in a

[5]Analyses of prescribing behavior by specialty, such as Levy et al. (2015) suggest that pain management specialists are high-volume prescribers, and Rutkow et al. (2015) show that pain clinic legislation in Florida primarily affected high-volume prescribers.

given quarter. In the pre-period, roughly 14% of each provider's patients met this standard. Individuals exhibiting shopping behavior comprised a similar share of providers' patient set across the volume quartiles. However, since high-volume providers account for the bulk of all prescriptions, most shoppers (more than two-thirds) obtain opioids from these prescribers. The most common consumption pattern was filling multiple prescriptions but not meeting our shopping criteria. Presumably, many of these individuals are chronic pain patients.

We use our categorization to examine whether providers targeted reductions in opioid prescriptions on high risk patients. Results are based on a two-part variant of our previous regressions. For each patient type, we combine the effect on any prescribing (extensive margin) and the effect on the number of patients (intensive margin) into an overall effect using standard methods for two-part models (Deb, Norton and Manning, 2017).

Table 7 reports estimated policy effects by patient type, as well as bootstrapped standard errors (Belotti et al., 2015). Consistent with the goal of the policy, the effect of the policy was largest in percentage terms for shoppers and smallest for single-use patients. The average provider prescribed to 2.6 fewer shoppers, which represents a 34% effect. Reductions in prescribing to shopping patients is exactly what we expect from the provision of PDMP information; without a PDMP it is difficult for a provider to observe prescriptions written by other providers.

However, there are meaningful declines for the other patient types. A 17% decline in non-shopping patients and a 12% decline in single-use patients is consistent with providers imperfectly targeting the reductions in patients. Its possible these patients were adversely affected by a chilling effect. At the same time, the PDMP may include other information suggesting that an opioid prescription would be contraindicated, such as prescriptions for benzodiazepines (Dasgupta et al., 2016). The number of patients with overlapping claims for opioids and benzodiazepines fell by 5% in Kentucky after the mandatory access policy went into effect, while there was no change in Indiana.

V. Conclusion

Prescription drug monitoring programs have the potential to decrease inappropriate prescribing of opioids. But PDMPs will only be effective if healthcare providers access the data. In an effort to increase provider engagement, several states have recently enacted policies requiring providers to query the state's PDMP before prescribing opioids. This paper evaluates the first comprehensive PDMP mandatory access policy, which was enacted by Kentucky in 2012. We find that providers responded to this policy in two main ways. Some, who prescribed low volumes of opioids before the policy went into effect,

stopped prescribing the drugs altogether. This is consistent with the idea that the policy introduced fixed compliance costs that low-volume providers were not willing to bear. Higher volume providers continued to prescribe, but wrote prescriptions to fewer patients.

We also assess what types of patients were affected. Ideally, PDMP data will help providers identify doctor shoppers and other high risk patients. Our results suggest that providers reduced prescriptions to patients whose prescription histories suggest possible doctor or pharmacy shopping. We find large reductions (in percentage terms) in the number of such patients receiving opioid prescriptions. At the same time, we find economically significant reductions in the number of patients without suspicious prescribing histories. These decreases suggest there may also be patients with a clinically-justified need for pain relief who lose access to treatment as a result of the policy.

REFERENCES

Allain, Marty. 2012. "Inspect PDMP 2.0." https://www.deadiversion.usdoj.gov/mtgs/pharm_awareness/conf_2012/december_2012/allain.pdf.

Alpert, Abby, David Powell, and Rosalie Liccardo Pacula. 2016. "Supply-Side Drug Policy in the Presence of Substitutes: Evidence from the Introduction of Abuse-Deterrent Opioids." Available at http://www.rand.org/pubs/working_papers/WR1181.html.

Belotti, Federico, Partha Deb, Willard Manning, and Edward Norton. 2015. "twopm: Two-part Models." *The Stata Journal*, 15(1): 3–20.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-In-Differences Estimates." *The Quarterly Journal of Economics*, 119(1).

Brady, Joanne E., Hannah Wunsch, Charles J. DiMaggio, Barbara H. Lang, James Giglio, and Guohua Li. 2014. "Prescription drug monitoring and dispensing of prescription opioids." *Public Health Reports*, 129.

Buchmueller, Thomas C., and Colleen Carey. 2018. "The Effect of Prescription Drug Monitoring Programs on Opioid Utilization in Medicare." *American Economic Journal: Economic Policy*, 10(1): 77–112.

Carey, Colleen M., Anupam B. Jena, and Michael L. Barnett. 2018. "Patterns of Potential Opioid Misuse and Subsequent Adverse Outcomes in Medicare, 2008 to 2012." *Annals of Internal Medicine*, 168(12): 837–845.

Cepeda, M. Soledad, Daniel Fife, Wing Chow, Gregory Mastrogiovanni, and Scott C. Henderson. 2013. "Opioid Shopping Behavior: How Often, How Soon, Which Drugs, and What Payment Method." *The Journal of Clinical Pharmacology*, 53(1): 112–117.

Chen, Li Hui, Holly Hedegaard, and Margaret Warner. 2014. "Drug-poisoning deaths involving opioid analgesics: United States, 1999-2011." *NCHS data brief*, , (166): 1–8.

CMS. 2015. "Opioid Morphine Equivalent Conversion Factors." https://www.cms.gov/Medicare/Prescription-Drug-Coverage/PrescriptionDrugCovContra/Downloads/Opioid-Morphine-EQ-Conversion-Factors-March-2015.pdf. Accessed: February 2, 2016.

Dart, Richard C., Hilary L. Surratt, Theodore J. Cicero, Mark W. Parrino, Geoff Severtson, Becki Bucher-Bartelson, and Jody L. Green. 2015. "Trends in Opioid Analgesic Abuse and Mortality in the United States." *New England Journal of Medicine*, 372(3).

Dasgupta, Nabarun, Michele Jonsson Funk, Scott Proescholdbell, Annie Hirsch, Kurt M. Ribisl, and Steve Marshall. 2016. "Cohort Study of the Impact of High-Dose Opioid Analgesics on Overdose Mortality." *Pain Medicine*, 17(1): 85–98.

Deb, Partha, Edward G. Norton, and Willard G. Manning. 2017. *Health Econometrics Using Stata.* Stata Press.

Donald, Stephen G, and Kevin Lang. 2007. "Inference with Difference-in-Differences and Other Panel Data." *The Review of Economics and Statistics*, 89(2): 221–233.

Dowell, Deborah, Kun Zhang, Rita K. Noonan, and Jason M. Hockenberry. 2016. "Mandatory Provider Review And Pain Clinic Laws Reduce The Amounts Of Opioids Prescribed And Overdose Death Rates." *Health Affairs*, 35(10): 1876–1883.

Evans, William N, Ethan Lieber, and Patrick Power. 2019. "How the Reformulation of OxyContin Ignited the Heroin Epidemic." *The Review of Economics and Statistics*, CI(1).

Freeman, Patricia, Amie Goodin, Suzanne Troske, and Jeffrey Talbert. 2015. "Kentucky House Bill 1 Impact Evaluation." University of Kentucky Department of Pharmacy Practice and Science, Lexington, KY.

Haffajee, Rebecca L., Anupam B. Jena, and Scott G. Weiner. 2015. "Mandatory Use of Prescription Drug Monitoring Programs." *JAMA*, 313(9): 891–892.

Haffajee, Rebecca L, Michelle M Mello, Fang Zhang, Alan M Zaslavsky, Marc R Larochelle, and J Frank Wharam. 2018. "Four states with robust prescription drug monitoring programs reduced opioid dosages." *Health Affairs*, 37(6): 964–974.

Han, Beth, Wilson M Compton, Carlos Blanco, Elizabeth Crane, Jinhee Lee, and Christopher M Jones. 2017. "Prescription opioid use, misuse, and use disorders in US adults: 2015 National Survey on Drug Use and Health." *Annals of Internal Medicine*, 167(5): 293–301.

Jena, Anupam B., Dana Goldman, Lesley Weaver, and Pinar Karaca-Mandic. 2014. "Opioid Prescribing By Multiple Providers in Medicare: Retrospective Observational Study of Insurance Claims." *British Medical Journal*, 348.

Kennedy-Hendricks, Alene, Matthew Richey, Emma E McGinty, Elizabeth A Stuart, Colleen L Barry, and Daniel W Webster. 2016. "Opioid overdose deaths and Floridas crackdown on pill mills." *American journal of public health*, 106(2): 291–297.

Kunins, Hillary V., Thomas A. Farley, and Deborah Dowell. 2013. "Guidelines for opioid prescription: why emergency physicians need support." *Annals of Internal Medicine*, 158.

Levy, Benjamin, Leonard Paulozzi, Karin A. Mack, and Christopher M. Jones. 2015. "Trends in Opioid AnalgesicPrescribing Rates by Specialty, U.S., 20072012." *American Journal of Preventive Medicine*, 49(3): 409 – 413.

Li, Guohua, Joanne E. Brady, Barbara H. Lang, James Giglio, Hannah Wunsch, and Charles DiMaggio. 2014. "Prescription drug monitoring and drug overdose mortality." *Injury Epidemiology*, 1(1): 1–8.

Meinhofer, Angélica. 2018. "Prescription drug monitoring programs: The role of asymmetric information on drug availability and abuse." *American Journal of Health Economics*, 4(4): 504–526.

Ohio Bureau of Workers' Compensation. 2016. "MED Table." `https://www.bwc.ohio.gov/downloads/blankpdf/MEDTable.pdf`. Accessed: February 2, 2016.

Palliative.org. 2016. "INSTRUCTIONS FOR MORPHINE EQUIVALENT DAILY DOSE (MEDD)." `http://palliative.org/NewPC/_pdfs/tools/INSTRUCTIONsMEDD.pdf`. Accessed: February 2, 2016.

Paulozzi, Leonard J., Edwin M. Kilbourne, and Hema A. Desai. 2011. "Prescription Drug Monitoring Programs and Death Rates from Drug Overdose." *Pain Medicine*, 12(5): 747–54.

Paulozzi, Leonard J, Karin A Mack, and Jason M Hockenberry. 2014. "Variation among states in prescribing of opioid pain relievers and benzodiazepinesUnited States, 2012." *Journal of safety research*, 51: 125–129.

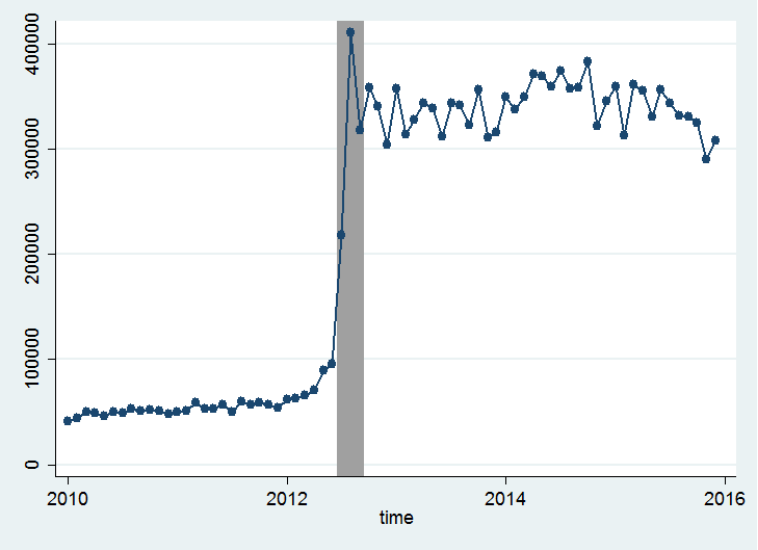
Rudd, Rose A, Noah Aleshire, Jon E Zibbell, and R Matthew Gladden. 2016. "Increases in drug and opioid overdose deathsUnited States, 2000–2014." *American Journal of Transplantation*, 16(4): 1323–1327.

Rutkow, L, Chang H, Daubresse M, Webster DW, Stuart EA, and Alexander G. 2015. "Effect of floridas prescription drug monitoring program and pill mill laws on opioid prescribing and use." *JAMA Internal Medicine*, 175(10): 1642–1649.

Smith, Jessica C, and Carla Medalia. 2015. *Health Insurance Coverage in the United States: 2014*. US Department of Commerce, Economics and Statistics Administration, Bureau of the Census Washington, DC.

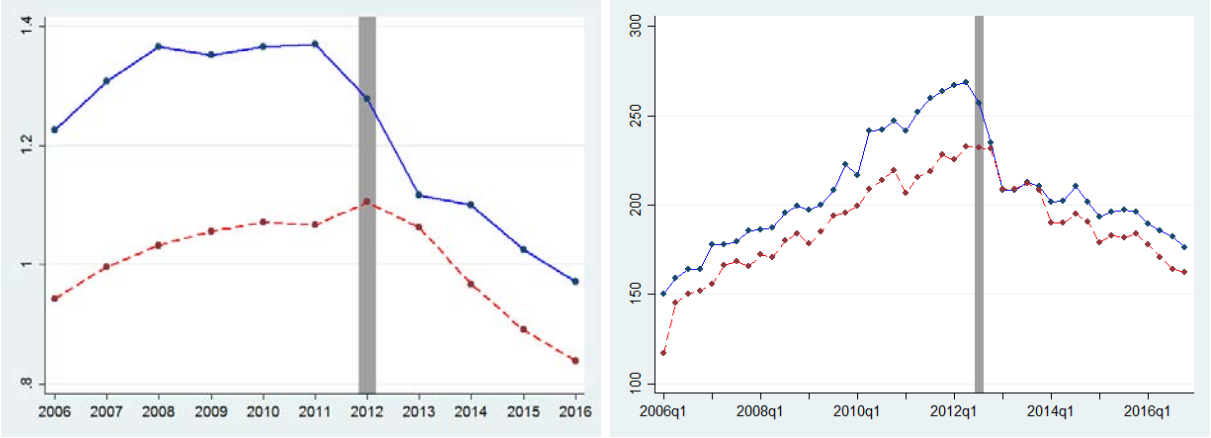
Wen, Hefei, Bruce R. Schackman, Brandon Aden, and Yuhua Bao. 2017. "States With Prescription Drug Monitoring Mandates Saw a Reduction in Opioids Prescribed to Medicaid Enrollees." *Health Affairs*, 36(4): 733–741.

Figure 1. : Requests for KASPER reports, 2010-2015



Note: Monthly administrative count, obtained via personal communication with KASPER staff. Shaded area represents implementation quarter 2012q3.

Figure 2. : Opioid Utilization Per Capita in Indiana and Kentucky, 2006-2016

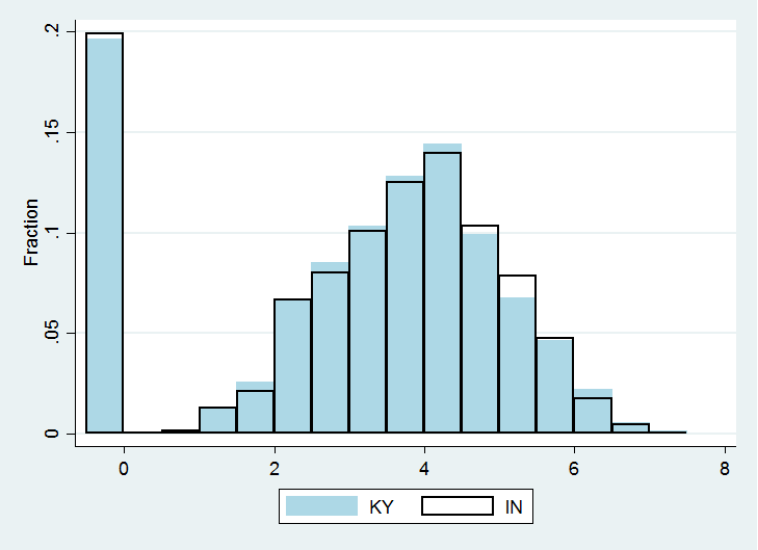


(A) Annual Prescriptions Per Capita
Source: CDC

(B) Quarterly MED Per Capita
Source: ARCOS

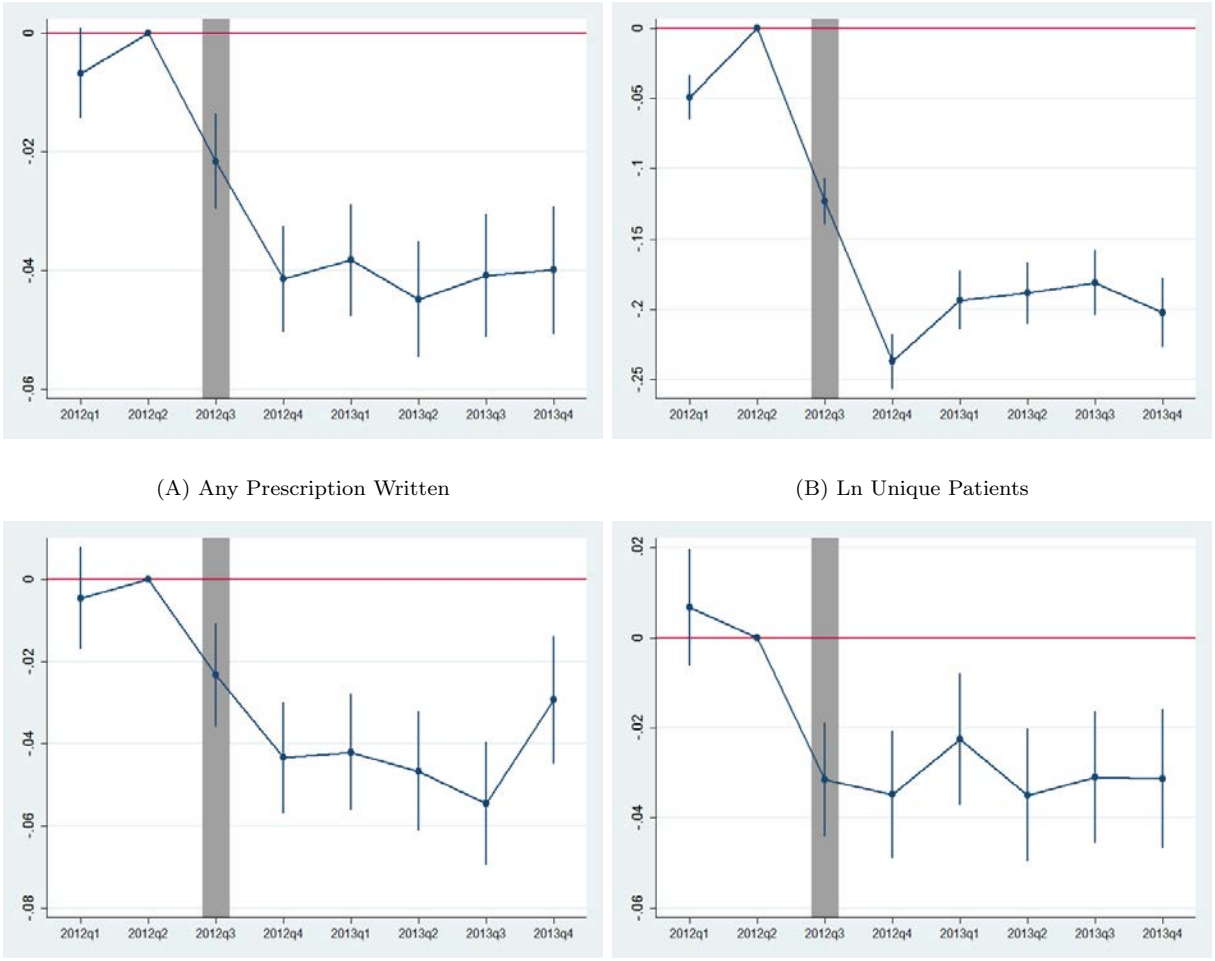
Note: Shaded area represents implementation period (2012 for CDC and 2012q3 for ARCOS.) Blue solid line represents KY and red dashed line represents IN.

Figure 3. : Distribution of Logged (Base 10) Total MED Prescribed by Provider, 2012h1 (0 mapped to -0.5)



Note: Figure depicts distribution of providers in Kentucky (blue) and Indiana (outline) based on the logged (base 10) total MED they prescribe in 2012h1, with those who prescribe zero mapped to -0.5.

Figure 4. : Provider-Level Prescribing Behavior: Event Study, 2012q1-2013q4



(A) Any Prescription Written

(B) Ln Unique Patients

(C) Ln Days Per Patient

(D) Ln MED Per Day

Note: In these event study figures, coefficients represent the deviation from the mean difference between Kentucky and Indiana in each quarter 2012q1 to 2013q4, with 2012q2 normalized to zero. Regressions include provider and quarter fixed effects. Standard errors clustered at provider level. Shaded area represents implementation quarter 2012q3. Bar represents 95 percent confidence interval.

Table 1—: Pre-Period Trends in Aggregate MED Per Capita (ARCOS data), 2006q1-2012q2

	Log	Level	Log	Level
KY	0.143***	37.35***	0.0917***	19.45***
	(0.0232)	(3.466)	(0.0320)	(5.067)
KY#Post2010q2			0.0462**	16.21***
			(0.0185)	(3.366)
Time	0.0840***	15.17***	0.0840***	15.17***
	(0.00740)	(0.899)	(0.00747)	(0.909)
KY#Time	0.00748	3.884***	-0.00219	0.492
	(0.00774)	(0.997)	(0.00908)	(1.220)
2nd intercept for KY	No	No	Yes	Yes
Observations	52	52	52	52

Note: Table reports pre-trends analysis of quarterly aggregates MED per capita for Kentucky and Indiana from ARCOS. Models include a constant (not shown) and end in the quarter prior to Kentucky's implementation of a mandatory access policy. Columns 1 and 3 use logged outcomes. Columns 3 and 4 include a binary variable for Kentucky 2010q2 to 2012q2. Robust Huber-White standard errors reporting throughout. ** p<0.01, * p<0.05, + p<0.1

Table 2—: Difference in Difference Analysis of Aggregate MED Per Capita (ARCOS data), 2006q1-2013q4

	Log	Level	Log	Level
KY	0.116**	23.27**	0.0987**	17.13**
	(0.00871)	(2.027)	(0.0108)	(1.423)
KY#Post2010q2			0.0491**	17.73**
			(0.0123)	(2.120)
KY#2012q3	-0.0141	1.560	-0.0462**	-10.04**
	(0.00871)	(2.027)	(0.00588)	(1.572)
KY#Post2012q3	-0.111**	-22.20**	-0.143**	-33.80**
	(0.00929)	(2.155)	(0.00672)	(1.737)
2nd intercept for KY	No	No	Yes	Yes
Quarterly fixed effects	Yes	Yes	Yes	Yes
Observations	64	64	64	64

Note: Table reports a difference-in-difference analysis comparing aggregate quarterly MED per capita for Kentucky and Indiana from ARCOS before and after Kentucky's implementation of a mandatory access policy in 2012Q3. KY#2012Q3 corresponds to implementation period. Robust Huber-White standard errors. ** p<0.01, * p<0.05, + p<0.1

Table 3—: Sample Means of Quarterly Provider-Level Outcomes

	Indiana			Kentucky		
Outcome	Pre	Post	Change	Pre	Post	Change
MED (1000s)	56.1	55.9	-0.2	62.6	56.7	-5.9
Any Prescription	0.74	0.76	0.02	0.74	0.72	-0.02
Unique Patients | Any	54.5	52.1	-2.4	60.2	54.2	-6.1
Days/Patient | Any	18.5	18.6	0.1	18.9	19.3	0.4
MED/Day | Any	35.6	35.5	-0.1	34.6	34.3	-0.3

Note: This table reports means of quarterly provider-level measures based on PDMP data. The column marked "pre" refers to 2012q1 & 2012q2; the column marked "post" refers to 2012q4 through 2013q4.

Table 4—: Difference-in-Differences Estimates of the Effect of Kentucky's Mandatory Access PDMP Law

	(1)	(2)	(3)	(4)
VARIABLES	Any Prescription	Ln Patients	Ln Days/Patient	Ln MED/Day
	Results with Dependent Variables 2-4 in Logs			
KYPost	-0.0377**	-0.177**	-0.0409**	-0.0343**
	(0.00379)	(0.00913)	(0.00536)	(0.00513)
Observations	290,464	215,409	215,328	215,328
Number of providers	36,308	36,308	36,294	36,294
Mean of LHS in KY				
Pre-Period in Levels	0.739	60.20	18.85	34.61
Treatment Effect in Levels	-0.0377	-10.73	-0.805	-1.211
	Results with Dependent Variables 2-4 in Levels			
KYPost	-0.0377**	-6.466**	-0.0334	-0.418*
	(0.00379)	(0.607)	(0.113)	(0.187)
Observations	290,464	215,409	215,328	215,328
Number of Providers	36,308	36,308	36,294	36,294

Note: Table reports difference-in-difference coefficients from estimation of Equation 1 on a panel of quarterly provider-level measures 2012q1 to 2013q4. Outcomes in second, third, and fourth column are conditional on the provider having any prescribing in the quarter. Standard errors clustered at provider level.
** p<0.01, * p<0.05, + p<0.1

Table 5—: Provider-Level Outcomes by Pre-Period Provider Volume

	All	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Total MED in 2012H1 (000s):					
minimum	0	0	0.136	3.652	31.024
maximum	14,639	0.135	3.652	31.022	14,639
Average Quarterly Outcomes in Pre-Period:					
Any prescriptions in quarter: mean	73.9%	12.1%	85.0%	98.7%	99.8%
Number of patients: mean	41.9	0.17	4.7	35.4	127.4
Percent Single Use	29%	61%	51%	47%	23%
Percent Multiple Use Non-Shoppers	57%	29%	36%	38%	63%
Percent Multiple Use Shoppers	14%	9%	12%	15%	13%
Days per patient	18.6	5.83	9.9	12.3	33.7
MED per day	35.2	14.73	28.9	34.7	43.5

Note: This table reports means of pre-period provider-level measures by provider volume quartile, based on MED prescribed in pre-period (2012h1.) Sample statistics correspond to quarterly averages in in the pre-period.

Table 6—: Difference in Differences Estimates by Provider Quartile

	(1)	(2)	(3)	(4)	(5)
Sample	All	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Total MED 2012 H1	0+	0-135	136-3652	3652.5-31022	31024+
			Any Prescription		
KYPost	-0.0377**	-0.0533**	-0.0644**	-0.0232**	-0.00957**
	(0.00379)	(0.00892)	(0.00729)	(0.00472)	(0.00311)
Observations	290,464	72,696	72,536	72,616	72,616
Number of providers	36,308	9,087	9,067	9,077	9,077
Mean of LHS in KY Pre-Period in Levels	0.739	0.129	0.850	0.989	0.998
			Ln Patients		
KYPost	-0.177**	-0.0824+	-0.180**	-0.190**	-0.167**
	(0.00913)	(0.0470)	(0.0189)	(0.0150)	(0.0148)
Observations	215,409	24,264	51,585	68,426	71,134
Number of providers	36,308	9,087	9,067	9,077	9,077
Mean of LHS in KY Pre-Period in Levels	60.20	1.329	5.251	35.85	142.1
			Ln Days/Patient		
KYPost	-0.0409**	-0.00134	-0.0616**	-0.0505**	-0.0183**
	(0.00536)	(0.0460)	(0.0133)	(0.00844)	(0.00683)
Observations	215,328	24,217	51,559	68,421	71,131
Number of providers	36,294	9,073	9,067	9,077	9,077
Mean of LHS in KY Pre-Period in Levels	18.85	5.659	10.04	12.50	34.98
			Ln MED/Day		
KYPost	-0.0343**	-0.167**	-0.0542**	-0.0340**	-0.00802
	(0.00513)	(0.0580)	(0.0134)	(0.00738)	(0.00553)
Observations	215,328	24,217	51,559	68,421	71,131
Number of providers	36,294	9,073	9,067	9,077	9,077
Mean of LHS in KY Pre-Period in Levels	34.61	16.69	29.95	35.03	40.68

Note: Table reports difference-in-difference coefficients from estimation of Equation 1 on a panel of quarterly provider-level measures 2012q1 to 2013q4 by quartile of provider MED in 2012h1. Outcomes in second, third, and fourth panel are conditional on the provider having any prescribing in the quarter. Standard errors clustered at provider level. ** p<0.01, * p<0.05, + p<0.1.

Table 7—: Change in Number of Patients Seen by a Provider by Patient Type

	(1)	(2)	(3)	(4)
	All	Single-Use	Multiple Use	
			Non-Shoppers	Shoppers
Total Effect in Levels	-9.483**	-1.689**	-5.523**	-2.584**
	(0.510)	(0.127)	(0.334)	(0.0905)
Implied Effect in Percent	-19.4%	-12.2%	-17.1%	-33.9%

Note: Single-use patients defined as patients who receive one prescription in current period and none in next period. Multiple-use shoppers defined as patients who fill a prescription from 3+ providers or at 3+ dispensaries in a quarter. Remaining patients are multiple-use nonshoppers. Estimates based on a two-part model combining a linear probability model of any prescribing to the patient type with an OLS regression of the log number of patients of the given type. For ease of interpretation, the combined effect is reported in both percentage change and levels. All regressions include provider fixed effects. Bootstrapped standard errors based on 1000 replications, which are sampled with replacement at the provider level. ** p<0.01, * p<0.05, + p<0.1

Appendix

A1. Inference Under Alternative Models

Our study uses PDMP data from only two states, and thus we cannot cluster the standard errors at the state level and expect standard arguments for consistency to apply. We instead cluster standard errors in our provider × time regressions at the provider level. In this section, we explore inference under two alternative models.

First, we implement the suggestions of Bertrand, Duflo and Mullainathan (2004) and collapse the data to create a single pre- and post-period observation for each provider, to account for residual serial correlation in the errors. These results as well as our baseline model are in Appendix Table A1. Estimating the model on this data set yields similar point estimates and standard errors that are roughly 30% larger than those obtained using quarterly data. For Y^1 and Y^2, this difference does not qualitatively change our inferences: the estimates remain statistically significant at the 1% level.

Donald and Lang (2007) suggest assessing inference when standard errors may be too small by estimating the difference-in-differences coefficient for every consecutive two periods within the sample period. If standard errors are severely underestimated, these placebo tests will return statistically significant results. Appendix Table A2 implements this exercise, reporting the coefficients and t-statistics for all four outcomes across all seven possible two-quarter intervals. Our implementation period is 2012q3, and the two regressions that include that period are bolded for reference. The placebo regressions are generally, though not always null. However, the t-statistics for the implementation period average more than five times the t-statistics for the placebo tests. This suggests that even if the standard errors are somewhat too large, inference is likely to be robust to smaller standard errors.

A2. Robustness of Results to Exclusion of Areas Bordering Kentucky and Indiana

Cross-border migration between the treatment state and its neighboring control has the potential to contaminate our results. A significant population center, Louisville, lies close to the Indiana border, giving rise to a region known as "Kentuckiana." If the PDMP implementation led Kentucky residents to seek more opioids from Indiana providers, then we may observe a reduction in prescribing among Kentucky providers and an increase among Indiana providers. In our difference-in-differences model, this would appear to be a policy-associated reduction in opioid prescribing. Thus, in Appendix Table A5, we repeat our baseline results (top panel) and exclude the three-digit zip codes that lie along the Kentucky-Indiana border (bottom panel). Our results are statistically indistinguishable and nearly the same to the hundredth place. While cross-border contamination need not be limited to the zip codes that lie along the border, we are reassured by the fact that the results are so similar when excluding the individuals for whom border effects are likely to be largest.

Table A1—: Comparison of Baseline (Quarterly) Regressions to 2-period Regressions

VARIABLES	(1) Any Prescription	(2) Ln Patients	(3) Ln Days/Patient	(4) Ln MED/Day
		Baseline (Quarterly) Regressions in Logs		
KYpost		-0.177**	-0.0409**	-0.0343**
		(0.00913)	(0.00536)	(0.00513)
Observations		215,409	215,328	215,328
Number of providers		36,308	36,294	36,294
		2-Period Regressions in Logs		
KYPost		-0.216**	-0.0398**	-0.0398**
		(0.0119)	(0.00651)	(0.00668)
Observations		62,835	62,812	62,812
Number of Providers		35,897	35,882	35,882
		Baseline (Quarterly) Regressions in Levels		
KYPost	-0.0377**	-6.466**	-0.0334	-0.418*
	(0.00379)	(0.607)	(0.113)	(0.187)
Observations	290,464	215,409	215,328	215,328
Number of Providers	36,308	36,308	36,294	36,294
		2-Period Regressions in Levels		
KYPost	-0.0246**	-6.076**	-0.0366	-0.392
	(0.00520)	(0.593)	(0.124)	(0.244)
Observations	72,6165	62,835	62,812	62,812
Number of Providers	36,308	35,897	35,882	35,882

Note: Table reports difference-in-difference coefficients from estimation of Equation 1. Outcomes in second, third, and fourth column are conditional on the provider having any prescribing in the quarter. Baseline regressions are run on a panel of quarterly outcomes. 2-Period regressions are run on a panel of two outcomes (pre and post) for each provider. Standard errors clustered at provider level.
** p<0.01, * p<0.05, + p<0.1

Table A2—: Coefficients from Difference in Differences Regression Including only 2 Time Periods. (Implementation Period Bolded.)

Time Period	Any	Patients	Days/Patient	MEDs/Day
2012Q1-2012Q2	0.00675+	0.0503**	0.00172	-0.00326
	(1.750)	(6.543)	(0.291)	(-0.530)
2012Q2-2012Q3	**-0.0216****	**-0.130****	**-0.0274****	**-0.0336****
	(-5.279)	**(-16.28)**	**(-4.583)**	**(-5.620)**
2012Q3-2012Q4	**-0.0198****	**-0.117****	**-0.0186****	**0.00534**
	(-4.897)	**(-13.92)**	**(-3.078)**	**(0.841)**
2012Q4-2013Q1	0.00323	0.0397**	0.000220	0.0134*
	(0.815)	(5.154)	(0.0381)	(2.070)
2013Q1-2013Q2	-0.00660+	0.00170	-0.00129	-0.0100
	(-1.666)	(0.216)	(-0.221)	(-1.593)
2013Q2-2013Q3	0.00398	0.0115	-0.000757	0.00834
	(0.959)	(1.449)	(-0.133)	(1.369)
2013Q3-2013Q4	0.000927	-0.0167*	0.0236**	0.00181
	(0.230)	(-2.080)	(4.082)	(0.289)

Note: Each coefficient corresponds to one regression. T-stats in parentheses. Standard errors clustered at provider level.
** p<0.01, * p<0.05, + p<0.1

Table A3—: Difference in Differences Regressions by Provider Quartile Using Levels

	(1)	(2)	(3)	(4)	(5)
Sample	All	Quartile 1	Quartile 2	Quartile 3	Quartile 4
Total MEDs 2012 H1	0+	0-135	136-3652	3652.5-31022	31024+
			Patients, Conditional on Any		
KYPost	-6.466**	0.0169	-1.059**	-4.548**	-13.00**
	(0.607)	(0.754)	(0.355)	(0.489)	(1.587)
Observations	215,409	24,264	51,585	68,426	71,134
Number of providers	36,308	9,087	9,067	9,077	9,077
Percent Treatment Effect	-9.7	1.3	16.8	-11.3	-8.4
			Days/Patient		
KYPost	-0.0334	0.0662	-0.280	-0.157	0.274
	(0.113)	(0.613)	(0.224)	(0.180)	(0.196)
Observations	215,328	24,217	51,559	68,421	71,131
Number of providers	36,294	9,073	9,067	9,077	9,077
Percent Treatment Effect	-0.2	1.2	-2.7	-1.2	0.8
			MEDs/Day		
KYPost	-0.418*	-4.416**	-0.742+	-0.338	0.0938
	(0.187)	(1.049)	(0.422)	(0.316)	(0.270)
Observations	215,328	24,217	51,559	68,421	71,131
Number of providers	36,294	9,073	9,067	9,077	9,077
Percent Treatment Effect	-1.2	-20.9	-2.4	-1.0	0.2

Note: Table reports difference-in-difference coefficients from estimation of Equation 1, but with level outcomes instead of logged outcomes. A panel of quarterly provider-level measures 2012q1 to 2013q4 by quartile of provider MED in 2012h1 is used. Outcomes in each panel are conditional on the provider having any prescribing in the quarter.
Standard errors clustered at provider level.
** p<0.01, * p<0.05, + p<0.1

Table A4—: Percent of Providers That Don't Prescribe in 2012q4-2013q4 Among Providers who Wrote a Prescription in 2012h1

	IN Mean	KY Mean	Difference
All	0.067	0.088	0.0213**
Quartile 1	0.296	0.372	0.076**
Quartile 2	0.124	0.157	0.033**
Quartile 3	0.025	0.031	0.006+
Quartile 4	0.007	0.010	0.003

Note: This table reports the share of providers prescribing in 2012h1 who did not not prescribe at all in the post-period (2012q4-2013q4), respectively for Indiana and Kentucky. The third column reports the difference, where asterisks signify the mean differs from zero at ** p<0.01, * p<0.05, + p<0.1.

Table A5—: Comparison of Baseline Results to Sample that Excludes Providers Located on KY-IN border

	(1) Any Prescription	(2) Ln Patients	(3) Ln Days/Patient	(4) Ln MED/Day
All Providers				
KYpost	-0.0377**	-0.177**	-0.0409**	-0.0343**
	(0.00379)	(0.00913)	(0.00536)	(0.00513)
Observations	290,464	215,409	215,328	215,328
Number of providers	36,308	36,308	36,294	36,294
No KY-IN border				
KYpost	-0.0439**	-0.176**	-0.0448**	-0.0327**
	(0.00480)	(0.0117)	(0.00688)	(0.00639)
Observations	207,424	155,157	155,106	155,106
Number of providers	25,932	25,932	25,924	25,924

Note: "No KY-IN border" excludes all providers located in a 3 digit zip code that is on the KY-IN border. Excluded zip codes are 400, 401, 402, 410, 423, 424, 470, 471, 472, 475, 476, and 477. ** p<0.01, * p<0.05, + p<0.1