HHS Public Access

Author manuscript

*Addiction*. Author manuscript; available in PMC 2023 February 14.

# Effects of a prior authorization policy for extended-release/long-acting opioids on utilization and outcomes in a state Medicaid program

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## Abstract

**Background and aims—**In response to the opioid overdose epidemic, US state Medicaid programs have adopted restrictive policies for opioid analgesics, yet effects on prescribing patterns and health outcomes are uncertain. This study aimed to examine effects of a prior authorization policy for extended-release/long-acting (ER/LA) opioids on opioid use in the Oklahoma, USA state Medicaid program.

**Design—**Retrospective difference-in-differences design study comparing changes in opioid use in Oklahoma Medicaid to control (Oregon Medicaid).

**Setting—**Oklahoma and Oregon, USA.

**Participants—**Medicaid beneficiaries in the Oklahoma and Oregon fee-for-service Medicaid programs between July 2007 and June 2009 (33724 in Oklahoma and 13520 in Oregon)

**Measurements—**The primary outcome was incident opioid-naive ER/LA opioid use. Secondary outcomes included other opioid and non-opioid pain medication use. We also examined indicators of high-risk prescribing (e.g. high-dosage opioid use) and opioid-related hospitalizations or emergency department (ED) visits.

**Findings—**The prior authorization policy was associated with a 0.7 percentage point reduction in the likelihood of incident opioid-naive ER/LA opioid use [95% confidence interval (CI) = −1.16

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Declaration of interests
None.

Supporting Information
Additional supporting information may be found online in the Supporting Information section at the end of the article.

to −0.33 percentage points; 70% pre-policy mean reduction, a 1.4 percentage point decrease in likelihood of any new ER/LA opioid prescriptions (95% CI = −2.1 to −0.7 percentage points; 33% pre-policy mean reduction) and a decline of 0.16 in total ER/LA opioid prescriptions per enrollee (PPE) (95% CI = −0.29 to −0.04 PPE)]. There was a significant increase in the number of short-acting opioids filled after the policy (0.36; 95% CI = 0.22–0.50 PPE), increases in likelihood of having overlapping opioids and benzodiazepines, but significant reductions in likelihood of having overlapping opioids. No significant changes in opioid-related hospitalizations or ED visits were observed.

**Conclusions—**In Oklahoma, USA's July 2008 prior authorization policy for extended-release/long-acting opioids appears to have reduced the number of opioid-naive patients initiating extended-release/long-acting opioid use by more than half, but may also have increased short-acting opioid prescriptions by 7%.

### Keywords

Analgesics; benzodiazepines; Medicaid; opioid; opioid-related disorders; pain; retrospective studies

## INTRODUCTION

The opioid overdose epidemic is a major public health emergency in the United States. The epidemic began in the 1990s, driven mainly by a dramatic increase in the prescribing of opioid pain relievers [1], and considerable effort has been directed at reducing high-risk opioid prescribing [2]. To assist clinicians and policy makers, federal, state and local authorities have developed and disseminated evidence-based opioid prescribing guidelines [3–9]. In addition, health-care systems and payers have re-aligned policy and payment mechanisms to encourage safer opioid prescribing [2,10–13].

In the United States, each state manages an entitlement program (called Medicaid) for low-income citizens. Because the burden of the opioid overdose epidemic is high in state Medicaid programs [14], states have advanced policies to curb the overuse of prescription opioids [11]. A recent survey of state Medicaid programs reports that 21 states have adopted the Centers for Disease Control and Prevention (CDC) *Guideline for Prescribing Opioid for Chronic Pain* published in 2016 [15,16]. State Medicaid programs employ a wide variety of tools to manage pharmacy utilization, such as cost-sharing, preferred drug lists, step therapy requirements, quantity limits and prior-authorizations [17–19]. Pharmacy benefit managers can employ these tools in ways ranging from eliminating coverage for higher risk products (e.g. methadone, fentanyl) to requiring prior approval for high-risk prescriptions such as those with a high daily dosage, concurrent with a benzodiazepine or a long-acting formulation [12]. Nearly every state Medicaid program uses some type of utilization management policy aimed at improving prescription opioid use. Data collected from 2015 show that 46 states restrict opioid quantities, 45 states use opioid prior authorization policies and 32 states employ opioid step-therapy policies [16].

Despite their widespread adoption, the impact of these policies is poorly understood. Morden *et al.* examined state Medicaid prior authorization policies for controlled release

oxycodone (OxyContin™) and found substantial variation in effects throughout states on oxycodone utilization [20]. The Massachusetts Medicaid program implemented prior authorization and enforced high dosage limits for several opioids, and observed significant reductions in total opioid utilization but mixed effects with respect to dosage [10]. Neither of these studies examined the effect of these policies on high-risk opioid use or opioid-related harms. A study of Medicaid beneficiaries in Pennsylvania found that beneficiaries enrolled in Medicaid plans which had a larger number of opioid prior authorizations tended to have lower rates of opioid abuse diagnoses than plans with no prior authorizations [21]. Despite these interventions, Tehrani *et al.* found that the average days' supply of selected opioid medications in the overall Medicaid population increased from 2005 to 2014 [22].

The state of Oklahoma, which often ranks poorly for substance abuse issues [23,24], has developed several programs to decrease opioid misuse and abuse in their Medicaid programs [11]. In July 2008, the Oklahoma Health Care Authority implemented a prior authorization policy requiring a trial of a short-acting opioid prior to initiating extended-release/long-acting (ER/LA) opioid therapy, which is consistent with CDC's guideline for prescribing opioids for chronic non-cancer pain [15,25]. Initiation of opioid therapy with ER/LA opioids is associated with subsequent long-term opioid use [26,27] and ER/LA opioids are associated with increased risk of overdose in several populations [28–30].

The prior authorization policy implemented in Oklahoma Medicaid consisted of a single form, which was completed by the dispensing pharmacist and/or prescribing physician on behalf of the patient. Prior authorizations were reviewed by clinical pharmacists and approved or denied within 24 hours of receipt. Patients who had previously been on ER/LA opioids continuously in the months leading up to implementation were eligible for continuation at initiation of the policy.

While national efforts have been made to address the opioid crisis, it is important to understand which practice or policy had meaningful effects on curbing the rise in inappropriate opioid use. Policies with meaningful reductions may then be implemented in similar settings. The objective of this study therefore was to examine the effects of this prior authorization policy on prescription drug utilization, opioid-related emergency department (ED) encounters and hospitalizations.

## METHODS

Using administrative Medicaid claims data, we conducted a retrospective quasi-experimental study designed to examine the effects of Oklahoma's ER/LA opioid prior authorization on high-risk opioid use and opioid-related harms. To detect changes in Oklahoma, we compared it to a control state's fee-for-service Medicaid program (Oregon) using the difference-in-differences approach. This study was part of a larger multi-state CDC-funded research project involving Medicaid data from the states of Oklahoma and Oregon (1U011CE002500). Because Oregon's managed care population may be systematically different from fee-for-service enrollees in Oregon and Oklahoma, we restricted our analysis to fee-for-service enrollees in both states. The difference-in-differences design is used commonly to examine the effects of a health-care policy in which changes occurring after

an intervention are compared to changes occurring in a comparison group that has not experienced the same or similar intervention during the study period [31]. The effect of the intervention is estimated by the difference across time in the group exposed to the intervention versus the difference across time in the group that is not. For our study, each individual had two sequential observations before and after the policy implementation. During the review period, no policy changes were made to the Oregon Medicaid ER/LA opioid policies which would limit its use as a historical control. Similarly, no other policies related to opioid prescribing were implemented in Oklahoma Medicaid.

Our study sample consisted of Medicaid beneficiaries in the Oklahoma and Oregon fee-for-service Medicaid programs between July 2007 and June 2009 (12 months pre-intervention and 12 months post-intervention) who had at least one prescription opioid fill. Patients were included if they were aged 18–64 years, had no dual Medicare eligibility throughout the study and were enrolled for a minimum of 75% of the study period. We performed a sensitivity analysis to compare differences between including only patients with continuous eligibility (100%) versus 75%.

## Outcomes

For each patient we characterized opioid-related pharmacy and medical service utilization 1 year before (pre) and after (post) 1 July 2008. Using pharmacy claims data, we estimated several measures of opioid utilization and potential indicators of high-risk opioid use, or opioid use that is suggestive of misuse, abuse or diversion [32]. Because the policy was aimed at reducing opioid-naive patients' initiation of an ER/LA opioid, our primary effect measure was defined as a new ER/LA opioid fill (no ER/LA opioid in past 180 days) among opioid-naive patients. We defined opioid-naive as the absence of any opioid fills projected to end in the prior 30 days, i.e. no opioid supply during the prior 30 days [33]. We estimated opioid end dates by adding day supply to the prescription dispensing date. We also conducted sensitivity analyses to examine a 60- and 90-day window to define opioid-naive. Secondary outcomes included all new ER/LA opioid fills and any ER/LA opioid fill. In order to evaluate the policy effects on use of potentially substitutable medications, we examined changes in the total number of prescriptions for short-acting opioids, ER/LA opioids, any opioids and non-opioid pain relievers. Non-opioid pain relievers included drugs for neuropathic pain (e.g. anti-seizure medications, tricyclic antidepressants), arthritis (e.g. non-steroidal anti-inflammatory drugs, etanercept) and headache (e.g. triptans). See the Supporting information for a comprehensive list.

We analyzed changes in several indicators of high-risk opioid use. In particular, because initiation of ER/LA opioid use has been associated with development of long-term use, we evaluated changes in long-term opioid use [26]. We defined long-term opioid use as having at least three opioid prescriptions dispensed within any 90-day interval [34]. We also examined changes in high-dosage opioid use [> 100 morphine equivalents (ME) per day], overlapping opioids, concurrent use of benzodiazepines and indicators of multiple provider use (pharmacies and prescribers) [35]. Concurrent use was defined as having two or more prescriptions with at least 7 days of overlap [36]. We defined multiple provider use by having opioids through four or more prescribers or pharmacies during the entire period [37].

Finally, we measured changes in the frequency of hospitalizations or emergency department encounters from opioid-related poisoning. The Supporting information summarizes drug classifications, indicator and diagnosis code definitions.

### Statistical analysis

We used the following regression model to estimate the difference-in-differences for each outcome: $Y_{it} = f(\beta_0 + \beta_1 STATE_i + \beta_2 POST_t + \beta_3 STATE_i \times POST_t + X_i B + \varepsilon_{it})$. We used propensity scores to weight the Oregon population so that they were similar in observed covariates to the Oklahoma population [38]. Covariates for the propensity score model included age, gender, race and component conditions in the Gagne comorbidity score and several behavioral health diagnosis codes identified in the pre-period [39]. Diagnostic codes used to determine covariates for propensity score modeling are described in the Supporting information.

The variable STATE was set to 1 if the individual was from Oklahoma and 0 if the individual was from Oregon. POST was set to 1 if the observation occurred during the post period (July 2008–June 2009) and 0 if in the pre-period (July 2007–June 2008). The coefficient $\beta_3$ estimates the interaction between STATE and POST (difference-in-differences estimator), and is a measure of the change in Oklahoma following the prior authorization policy relative to the difference in Oregon at the same time. We also controlled for baseline patient characteristics $X_i$ included in the propensity score weighting model. We used a logit link function for dichotomous outcomes and Poisson function for count variables, and calculated marginal effects [40]. We clustered standard errors on each Medicaid beneficiary. Analyses were performed using Stata version 14 (StataCorp Inc., College Station, TX, USA).

The Oregon Health and Science University Institutional Review Board (OHSU IRB00011118) approved this study and data use agreements executed with Oklahoma Health Care Authority and the Oregon Health Authority.

## RESULTS

The demographics and diagnostic characteristics of the 47224 Medicaid beneficiaries (33724 individuals in Oklahoma and 13520 individuals in Oregon) who met inclusion criteria are summarized in Table 1. The mean age of the sample was approximately 38 years, 75% were female and 33% were of non-white race. Among these opioid users, more than half had a diagnosis for a musculoskeletal pain-related condition; more than a third with a spine-related pain condition; and approximately one in five with diagnosis of headache. Between 1 and 5% of the study sample had a diagnosis of an opioid use disorder. Standardized differences between states larger than 0.1 prior to propensity weighting were present for several diagnostic categories. The largest differences between Oklahoma and Oregon were observed for opioid use disorder (1.2 versus 5.4%), alcohol use disorder (4.4 versus 9%), other drug use disorders (6.9 versus 11.8%), adjustment disorder (1.2 versus 4.2%) and hypertension (33.4 versus 25%). After applying the propensity score weighting to the Oregon population, the standardized differences between the two populations were all within ± 0.05.

Table 2 summarizes the difference-in-differences estimates for pharmacy-related utilization. Although incident opioid-naive ER/LA use was uncommon in both states (~1–2% of individuals), the policy was associated with a significant reduction of 0.7 percentage points [95% confidence interval (CI) = −1.16 to −0.33 percentage points], a 70% reduction from the pre-policy mean in Oklahoma. Expanding the opioid-naive lookback window to 60 or 90 days did not change this estimate meaningfully (see Supporting information). The policy was also associated with a 1.4 percentage point reduction in new ER/LA opioid prescription claims, regardless of past opioid use (95% CI = −2.1 to −0.7 percentage points), a 33% decline from the pre-policy mean. There was no significant change in the probability of any ER/LA opioid prescription among total opioid prescriptions. With respect to prescribing intensity, we observed a 0.16 percentage point reduction in the number of ER/LA opioid prescriptions filled per enrollee (95% CI = −0.29 to −0.041; 17.6% relative decline) after the policy change. There were also small but statistically significant changes in short-acting opioid fills (0.36 fill increase; 95% CI = 0.22–0.50), total opioid fills (0.31 prescription increase; 95% CI = 0.14–0.48) and prescriptions for non-opioid pain medications (0.37 prescription decrease; 95% CI= −0.49 to 0.25). The results of the sensitivity analysis based on 100% enrollment versus 75% enrollment were generally consistent with the primary analysis (Supporting information).

Table 3 summarizes changes in high-risk opioid use and opioid-related ED visits or hospitalizations. Paradoxically, the probability of long-term opioid use increased significantly by 3.3 percentage points (95% CI = 2.2–4.4 percentage points; 7.7% relative increase). There were modest reductions in several indicators of high-risk opioid use, such as overlapping opioids (−3.1 percentage points; 95% CI = −4.0 to −2.2 percentage points), multiple prescriber use (−7.0 percentage points; 95% CI = −7.8 to −6.3 percentage points) and multiple pharmacy use (−0.5 percentage points; 95% CI = −1.0 to −0.04 percentage points). We also observed a small increase in opioid/benzodiazepine co-prescribing (1.1 percentage points; 95% CI = 0.4–1.8). ED visits or hospitalizations were rare, and there were no significant changes during the year after the policy was introduced.

## DISCUSSION

As payers and health-care systems develop strategies to confront the opioid overdose epidemic, it is important to ensure that policies which aim at safer use of opioids for pain by restricting access are having their desired effect [41]. This can be especially important in populations which include enrollees in Medicaid programs, as they often include patients with limited support or abilities to navigate the health-care system [17]. Additionally, policies found to be ineffective at curbing prescription opioid misuse should be re-evaluated. In this study, we employed a quasi-experimental design to examine the effects of a prior authorization policy for LA/ER opioids in Oklahoma’s Medicaid program.

Results indicate that Oklahoma experienced a reduction in ER/LA opioid initiations by opioid-naive patients, all ER/LA opioid initiations and total ER/LA opioid prescriptions when compared to Oregon. These findings align with the policy goal of reduction in ER/LA opioid use, particularly if the patient was naive to opioids. There was an increase in short-acting opioids and total opioid prescriptions [22]. Similar findings have been noted in other

policy reviews and seem to indicate that when a policy is put into place in one area of opioid use, there are corresponding changes in utilization of other types of drugs [42]. This increase might also explain our finding of no significant change in high-dosage opioid use, as it could offset any effect on dosage from reduction in ER/LA opioid use. This net effect might attenuate the desired impact of the policy on reducing overall risk for overdose.

Opioid–opioid overlap and multiple provider use (both multiple prescriber and multiple pharmacy) also declined in Oklahoma, which suggests that the policy may also have had effects on other types of high-risk prescribing. We did not observe a decline in opioid-related ED encounters or hospitalizations, but these events were relatively uncommon in our study period. The findings of long-term opioid use, opioid–benzodiazepine overlap and non-opioid pain medication use were unexpected. In cases of long-term opioid use and opioid–benzodiazepine overlap, small increases in Oklahoma coupled with small declines in the control state resulted in net statistically significant changes. One possible explanation for the increase in long-term opioid use is that prior authorization-related administrative hurdles may have induced individuals to remain on therapy. The reason for the increase in opioid–benzodiazepine overlap may be due to the increase in the number of short-acting opioid prescriptions. Regardless, both changes were relatively small and might be spurious findings. Non-opioid pain medications increased in both states during the study period. However, the increase was significantly less in Oklahoma relative to the increase in Oregon, producing a net reduction in the difference-in-differences estimate. The reasons for the significant increase in non-opioid pain medication use in Oregon are unclear, and this finding needs to be interpreted with caution.

This research has several limitations. First, as with any research using secondary data sources such as paid claims for medical services or prescriptions, the data were collected for payment purposes and not for research. Thus, there can be coding errors showing services or prescriptions that were not actually received by the patient. However, these errors are assumed to be distributed equally throughout the data and do not represent systematic bias. Secondly, although there are federal rules governing Medicaid programs, states have considerable flexibility to develop programs which meet the unique needs of their citizens. In addition, the Oregon population at the time was covered largely by commercial managed care, while Oklahoma was not. This may increase the differences between the two populations. To minimize these differences, underlying population characteristics were controlled for using a propensity score weighting. After applying propensity score weighting to the Oregon population, differences in patient characteristics were generally similar; however, unmeasured differences might still exist or have been exacerbated. Thirdly, we utilized a two-point difference-in-difference analysis. Therefore, we could not test a parallel pre-trend assumption. Fourthly, analysis of paid prescription claims as an indicator of actual utilization of any medications, including opioids, is only a representation of the prescriptions which the plan (in this case Medicaid) actually paid for members as part of their benefit. Prescriptions which were filled outside of the payer system or paid for in cash were not captured and, thus, were not included in this study. While this research noted decreases in opioid utilization, these decreases may reflect only those prescriptions that were submitted to Medicaid for payment and may not reflect final receipt of prescriptions to the patients themselves. Recent studies suggest that cash payment for opioids is not

uncommon [27,43]. Finally, buprenorphine prescriptions in our analysis could potentially be used for pain, medication-assisted treatment (MAT) or both. Because diagnoses are not included on pharmacy claims, it is challenging to assign an indication for specific prescriptions. However, buprenorphine comprised less than 0.5% of all opioid prescriptions in our analysis, and therefore any use for MAT is probably inconsequential.

This research aimed to examine the effect that a policy for ER/LA opioids had on high-risk opioid use and outcomes. In this study, it appears that the prior authorization policy decreased the initiation of ER/LA opioid prescriptions, opioid–opioid overlap and multiple prescriber use, while having no effect on opioid-related emergency department visits or hospitalizations. Some changes to other high-risk use patterns may also be related to the policy, such as increased short-acting opioid prescriptions, opioid–benzodiazepine overlap and persistent opioid use. Based on the findings of this study, the policy implementation appears to have reduced ER/LA opioid use; however, the small increase in opioid persistence merits further study in larger samples. Given the inherent risks associated with ER/LA opioid formulations, a small increase in short-acting opioid use may be preferable.

## Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

## Acknowledgements

This work was supported through a cooperative agreement with the National Center for Injury Prevention and Control, Centers for Disease Control and Prevention in Atlanta, GA (1U011CE002500). S.L.K. received funding through a research fellowship grant from Purdue Pharma after the completion of this research for unrelated fellowship oversight. The findings and conclusions in this report are those of the authors and do not necessarily represent the official position of the Centers for Disease Control and Prevention.

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*Addiction*. Author manuscript; available in PMC 2023 February 14.

**Table 1**

Summary of demographic and comorbidity characteristics in the full and propensity-adjusted population.

| | Oklahoma (%) n = 33 724 | Oregon (%) n= 13 520 | Unadjusted standardized difference | Weighted Oregon % | Propensity-weighted standardized difference |
|---|---|---|---|---|---|
| Mean age (SD) | 37.6 (13.2) | 38.2 (12.8) | −0.04 | 38.2 | −0.05 |
| Sex (female) | 25 596 (75.9) | 10 037 (74.2) | 0.04 | 74.7 | 0.03 |
| Race (non-white) | 10 631 (31.5) | 5140 (38) | 0.14 | 70.0 | −0.03 |
| Gagne comorbidity conditions | | | | | |
| Congestive heart failure | 2501 (7.4) | 552 (4.1) | 0.14 | 8.0 | −0.02 |
| Dementia | 176 (0.5) | 32 (0.2) | 0.05 | 0.5 | 0.00 |
| Renal failure | 732 (2.2) | 260 (1.9) | 0.02 | 2.4 | −0.02 |
| Weight loss | 565 (1.7) | 126 (0.9) | 0.07 | 1.7 | 0.00 |
| Hemiplegia | 659 (2) | 278 (2.1) | −0.01 | 2.2 | −0.02 |
| Cardiac arrhythmias | 1800 (5.3) | 650 (4.8) | 0.02 | 5.7 | −0.01 |
| Pulmonary disease | 8179 (24.3) | 3173 (23.5) | 0.02 | 24.4 | 0.00 |
| Coagulopathy | 535 (1.6) | 247 (1.8) | −0.02 | 1.7 | −0.01 |
| Complicated diabetes | 1661 (4.9) | 604 (4.5) | 0.02 | 5.6 | −0.03 |
| Anemia | 3001 (8.9) | 1062 (7.9) | 0.04 | 9.3 | −0.01 |
| Fluid and electrolyte disorders | 3850 (11.4) | 1330 (9.8) | 0.05 | 12.1 | −0.02 |
| Liver disease | 1451 (4.3) | 905 (6.7) | −0.11 | 4.3 | 0.00 |
| Peripheral vascular disease | 1439 (4.3) | 290 (2.1) | 0.12 | 4.8 | −0.02 |
| Pulmonary circulation disorders | 300 (0.9) | 85 (0.6) | 0.03 | 0.9 | −0.01 |
| HIV/AIDS | 179 (0.5) | 92 (0.7) | −0.02 | 0.6 | −0.01 |
| Hypertension | 11 261 (33.4) | 3385 (25) | 0.18 | 35.1 | −0.04 |
| Pain conditions | | | | | |
| Spinal disorders | 12 538 (37.2) | 5953 (44) | −0.14 | 37.4 | 0.00 |
| Other musculoskeletal disorders | 17 820 (52.8) | 8085 (59.8) | −0.14 | 53.7 | −0.02 |
| Headache | 6208 (18.4) | 3067 (22.7) | −0.11 | 18.3 | 0.00 |
| Cancer | 1405 (4.2) | 656 (4.9) | −0.03 | 4.2 | 0.00 |
| Neuropathic pain | 2634 (7.8) | 1343 (9.9) | −0.07 | 7.9 | 0.00 |
| Other behavioral conditions | | | | | |

| | Oklahoma (%) n = 33 724 | Oregon (%) n= 13 520 | Unadjusted standardized difference | Weighted Oregon % | Propensity-weighted standardized difference |
|---|---|---|---|---|---|
| Opioid use/abuse disorder | 404 (1.2) | 733 (5.4) | −0.24 | 1.2 | 0.00 |
| Non-opioid drug use/abuse disorder | 2309 (6.9) | 1588 (11.8) | −0.17 | 7.0 | −0.01 |
| Alcohol use/abuse disorder | 1494 (4.4) | 1215 (9) | −0.18 | 4.5 | 0.00 |
| Schizophrenia | 2265 (6.7) | 455 (3.4) | 0.15 | 7.4 | −0.03 |
| Depression | 9261 (27.5) | 4243 (31.4) | −0.09 | 27.5 | 0.00 |
| Bipolar disorder | 2635 (7.8) | 894 (6.6) | 0.05 | 8.0 | −0.01 |
| Anxiety disorder | 7245 (21.5) | 2987 (22.1) | −0.01 | 21.7 | −0.01 |
| Adjustment disorder | 395 (1.2) | 564 (4.2) | −0.19 | 1.2 | 0.00 |
| Attention deficit hyperactivity disorder | 449 (1.3) | 297 (2.2) | −0.07 | 1.4 | −0.01 |

SD = standard deviation.

**Table 2**

Prescription drug utilization per enrollee.

| State | Pre (SE) | Post (SE) | Difference (SE) | Difference-in-differences (95% Cl) | Relative change[a] |
|---|---|---|---|---|---|
| New ER/LA opioid in opioid-naive patients: proportion of total opioid prescriptions | | | | | |
| Oklahoma | 0.0107*** (0.0006) | 0.0033*** (0.0003) | −0.0074*** (0.0006) | −.0074537*** (−.01158 to −0.003329) | −69.7% |
| Oregon | 0.0227*** (0.0014) | 0.0227*** (0.0015) | 0.0000 (0.0020) | | |
| New ER/LA opioid: proportion of total opioid prescriptions | | | | | |
| Oklahoma | 0.0426*** (0.0011) | 0.0230*** (0.0008) | −0.0196*** (0.0013) | −0.0140*** (−0.0207 to −0.0072) | −32.8% |
| Oregon | 0.0560*** (0.0023) | 0.0503*** (0.0022) | −0.0057* (0.0032) | | |
| ER/LA opioid: proportion of total opioid prescriptions | | | | | |
| Oklahoma | 0.0868*** (0.0014) | 0 0796*** (0.0014) | −0.0072*** (0.0011) | −0.0027 (−0.0090 to 0.0036) | −3.1% |
| Oregon | 0.1246*** (0.0032) | 0.1201*** (0.0031) | −0.0045 (0.0030) | | |
| ER/LA opioid: count of prescriptions per enrollee | | | | | |
| Oklahoma | 0.9273*** (0.0472) | 0.8495*** (0.0429) | −0.0778*** (0.0208) | −0.1630*** (−0.2848 to −0.04115) | −17.6% |
| Oregon | 1.4156*** (0.0803) | 1.5008*** (0.0791) | 0.0852 (0.0597) | | |
| Short-acting opioid; count of prescriptions per enrollee | | | | | |
| Oklahoma | 5.1388*** (0.0397) | 5.6315*** (0.0439) | 0.4928*** (0.0332) | 0.3633*** (0.2228 to 0.5039) | 7.1% |
| Oregon | 4.1569*** (0.0651) | 4.2863*** (0.0747) | 0.1294** (0.0637) | | |
| Total opioid: count of prescriptions per enrollee | | | | | |
| Oklahoma | 5.9608*** (0.0498) | 6.4701*** (0.0540) | 0 5093*** (0.0379) | 0.3088*** (0.1421 to 0.4756) | 5.1% |
| Oregon | 5.2105*** (0.0844) | 5.4109*** (0.0932) | 0.2004*** (0.0760) | | |
| Non-opioid pain medications: count of prescriptions per enrollee | | | | | |
| Oklahoma | 3.7429*** (0.0447) | 3.9643*** (0.0469) | 0.2213*** (0.02 53) | −0.3675*** (−0.4869 to −0.2482) | −9.8% |
| Oregon | 4.3153*** (0.0819) | 4.9041*** (0.0891) | 0.5888*** (0.0557) | | |

*$P < 0.0001$

**$P < 0.001$

***$P < 0.05$.

[a]Relative change calculated by dividing pre-value by difference-in-differences estimate. ER/LA = extended-release/long-acting; SE = standard error; CI = confidence interval.

**Table 3**

High-risk opioid use and opioid-related adverse events per enrollee.

| State | Pre (SE) | Post (SE) | Difference (SE) | Difference-in-differences (95% Cl) | Relative change[a] |
|---|---|---|---|---|---|
| Long-term opioid use | | | | | |
| Oklahoma | 0.4307** (0.0024) | 0.4426** (0.002 5) | 0.0119** (0.002 5) | 0.0333** (0.022 5, 0.0441) | 7.7% |
| Oregon | 0.3408** (0.0045) | 0.3194** (0.0046) | −0.0214** (0.0049) | | |
| High dosage opioid use | | | | | |
| Oklahoma | 0.1870** (0.0021) | 0.1741** (0.0020) | −0.0129** (0.0025) | −0.0070 (−0.0165, 0.0024) | −3.8% |
| Oregon | 0.1364** (0.0035) | 0.1305** (0.0034) | −0.0059 (0.0042) | | |
| Opioid-opioid overlap | | | | | |
| Oklahoma | 0.2069** (0.0020) | 0.1799** (0.0020) | −0.0270** (0.0021) | −0.0305** (−0.0395, −0.0215) | −14.7% |
| Oregon | 0.1668** (0.0036) | 0.1703** (0.0037) | 0.0035 (0.0041) | | |
| Opioid-benzodiazepine overlap | | | | | |
| Oklahoma | 0.1554** (0.0018) | 0.1623** (0.0019) | 0.0069** (0.0018) | 0.0110** (0.0038, 0.0182) | 7.1% |
| Oregon | 0.1098** (0.0030) | 0.1057** (0.0032) | −0.0041 (0.0032) | | |
| Multiple pharmacy use | | | | | |
| Oklahoma | 0.0614** (0.0012) | 0.0626** (0.0013) | 0.0012 (0.0015) | −0.0050* (−0.0097, −0.0004) | −8.2% |
| Oregon | 0.0196** (0.0014) | 0.0258** (0.0016) | 0.0062** (0.0019) | | |
| Multiple prescriber use | | | | | |
| Oklahoma | 0.1899** (0.0020) | 0.1503** (0.0019) | −0.0396** (0.0023) | −0.0704** (−0.0777, −0.0631) | −37.1% |
| Oregon | 0.0348** (0.0018) | 0.0656** (0.0026) | 0.0308** (0.0029) | | |
| Opioid-related ED or hospitalization | | | | | |
| Oklahoma | 0.0031** (0.0003) | 0.0037** (0.0003) | 0.0007 (0.0004) | −0.0014 (−0.0035, .0007) | −44.6% |
| Oregon | 0.0029** (0.0005) | 0.0049** (0.0008) | 0.0020* (0.0010) | | |

*$P < 0.001$

**$P < 0.05$

[a]Relative change calculated by dividing pre-value by difference-in-differences estimate. ED = emergency department; SE = standard error; Cl = confidence interval.