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EFFECTS OF OPIOID-RELATED POLICIES ON OPIOID UTILIZATION, NATURE OF MEDICAL CARE, AND DURATION OF DISABILITY

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ABSTRACT

We examine the effects of must-access prescription drug monitoring programs (PDMPs) and recent regulations limiting the duration of initial opioid prescriptions on care received by patients with work-related injuries, focusing on opioid utilization and medical care related to pain management. We find that must-access PDMPs contributed to declines in opioid utilization, while regulations limiting duration of initial opioid prescriptions had little effect on whether workers receive opioids, but reduced opioid use among those with prescriptions. We find limited evidence that must-access PDMPs affected utilization of other medical care related to pain management, and that must-access PDMPs and limits on initial prescriptions had little impact on the duration of temporary disability benefits captured at 12 months of maturity.

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Introduction

The last decade saw extensive policy focus on opioid abuse and prescribing. Policies adopted included implementation of electronic prescription drug monitoring programs (PDMPs), which first encouraged and later mandated their use before a prescription could be issued, and, more recently, limits on durations of initial opioid prescriptions. We examine the effects of these state-level policies on utilization of opioids, the nature of care, and the duration of temporary disability benefits among workers recovering from work-related injuries, exploring their effects in the workers' compensation system.

To the best of our knowledge, we are the first to provide comprehensive evidence on the effects of these policies, across states, in the workers' compensation setting.[1] There is one study of policies limiting initial opioid prescriptions in a group health setting (Sacks et al., 2019), and there is research on PDMPs and other earlier opioid-related policies in the group health and Medicare/Medicaid settings (see reviews by Maclean et al., 2020, and Ben-Shalom et al., 2020).

The adoption of these policies coincided with a decline in opioid utilization in the workers' compensation system, although opioid use remains common among workers injured at work (Thumula et al., 2019; Hayes and Swedlow, 2019; Texas Department of Insurance, 2019). Figure 1 shows that opioid use has declined substantially since 2010. The top panel shows a decline in the morphine milligram equivalent amount (MME) of opioids prescribed per claim, and the bottom panel shows a decline in the percentage of claims with opioids. Both graphs show the data for all claims and for the subset of claims with more than 7 days of lost work time (which entail more serious injuries and hence higher values for both measures). Between 2010 and 2019, the average MME amount of opioids prescribed per claim at 12 months of maturity decreased 74 percent. This reflects decreases in the percentage of workers with injuries who were prescribed opioids as well as the amount of opioids prescribed per claim with opioids. As the bottom graph shows, 10 years ago nearly half of claims with more than 7 days of lost work time had an opioid prescription, as did nearly 20 percent of all claims. These percentages dropped to about 30

[1] Thumula (2017) examined the impact of Kentucky's House Bill 1 that introduced regulations for pain clinics and established standards for dispensing and prescribing of opioids, including mandating use of the state PDMP.

percent and 10 percent, respectively, by 2018. Nevertheless, many workers continue to receive opioids on a prolonged basis. Across 22 states, in 2016, among workers who received opioid prescriptions, the percentage that received at least 60 days of opioids supply over any 90-day period ranged from 6 percent to 33 percent (Thumula et al., 2019).

These policy developments raised several important concerns among workers' compensation stakeholders. Of course the most direct question is how opioid-related policies contributed to changes in opioid utilization. We examine this question by looking at a number of measures of opioid prescribing.[2] These measures include whether workers had any opioid prescriptions; the number of prescriptions; the MME amount of opioids prescribed; and whether workers experienced potentially problematic prescriptions (i.e., longer-term prescribing of opioids within 12 months of injury,[3] more than 90 days of opioids prescribed, or more than 120 mg morphine equivalent daily dose [MED] of opioids prescribed).[4]

The potential for reductions in opioid prescribing raises the question of how workers with injuries managed their pain. Opioid prescribing and pain treatment guidelines recommend adoption of alternatives to opioids when treating acute and chronic pain (Centers for Disease Control and Prevention [CDC], 2016; Hegmann et al., 2014; American College of Occupational and Environmental Medicine, 2017). These alternatives include non-opioid pain medications (e.g., nonsteroidal anti-inflammatory drugs [NSAIDs] and non-opioid analgesics), as well as non-pharmacologic treatment (e.g., active and passive physical medicine, chiropractic care, acupuncture, and cognitive behavioral services). However, the adoption of these alternatives in response to policies intended to reduce opioid prescribing has not been examined.

In our analyses, we study whether workers received other care that may be a substitute for opioid

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[2] We examine opioid prescriptions filled by workers with injuries that were paid for by workers' compensation payors. We do not observe whether workers actually take opioids, and we do not observe prescriptions filled outside the workers' compensation system.
[3] We measure longer-term prescribing as opioid prescriptions within the first three months after an injury and three or more filled opioid prescriptions between the 7th and 12th months after an injury.
[4] These measures were used in Thumula et al. (2019) and Sacks et al. (2019). Data limitations prevent us from examining other measures that may indicate an increased risk of abuse, such as the number of prescribers who prescribed opioids or the number of pharmacies that dispensed prescriptions.

therapies. Following recommendations in treatment guidelines, we examine whether workers shift to non-opioid pain medications, to interventional pain management treatment, or to use of active physical medicine services.[5] We study substitution across types of care for a work-related injury or illness, and we focus on care for broadly-defined groups of injuries (e.g., low-back cases, fractures, lacerations and contusions, or inflammations).[6] The focus on these groups of injuries is beneficial because opioid prescribing rates differ widely across injury groups, with the highest opioid utilization among neurologic spine pain cases. Furthermore, the responsiveness to policies also likely differs by injury group, since treatment guideline recommendations regarding prescribing of opioids, and the appropriateness of alternative treatments, differ depending on the type of injury. For example, evidence-based treatment guidelines recommend against long-term use of opioids for low-back cases (e.g., Bigos et al., 1994).

Finally, we examine whether opioid-related policies had an effect on the duration of temporary disability benefits. Workers receive temporary disability benefits when they cannot work while recovering from injuries, and hence the duration of temporary disability benefits is strongly correlated with the length of time until return to work (although not identical). An increase in the duration of temporary disability as a result of opioid-related policies may indicate that limits on access to opioid therapies may have delayed workers' recovery after an injury, or vice versa.

Opioid-Related Policies and Prior Evidence

This section provides more details about the opioid prescribing policies examined in this study and reviews relevant prior literature. More attention to opioid prescribing by policymakers, payors, and providers in response to the opioid crisis was likely an important contributor to the declines in opioid prescribing documented in Figure 1. A flurry of state-level policies and regulations—some part of the workers' compensation system, and some more general—were implemented to help prevent opioid abuse.

[5] Interventional pain management treatment includes discography and disc decompression, electrical stimulation implants, epidural procedures, pump implants, trigger point injections, vertebra augmentation procedures, other injections, and nerve blocks. These are typically done in doctors' offices and are more invasive than prescriptions. Active physical medicine services are defined as strength training and/or conditioning exercises performed by patients under the direction of a physical therapist.

[6] In workers' compensation, substitution among services is not expected to be influenced by prices paid by consumers (workers), as there are no co-pays or deductibles. However, the costs of the services and reimbursements for those costs can affect provider behavior (Savych and Fomenko, 2019; Yee et al., 2015).

The policies that targeted various dimensions of opioid prescribing include (but are not limited to) must-access PDMPs; limits on initial opioid prescriptions; general as well as pain treatment guidelines; mandatory prescriber education and physician licensing; pain clinic regulations; and state workers' compensation regulations about dispensing and utilization of opioids (i.e., drug formularies). Our analysis focuses on two policies: must-access PDMPs, which were shown to be effective in reducing opioid use in the general health setting (although there are no studies of the effects of these policies in workers' compensation); and limits on initial opioid prescriptions, which have not been examined much in the prior literature. There were also concurrent federal policy interventions, including CDC guidelines (CDC, 2016), changes in the Controlled Substance Act (CSA), and Risk Evaluation and Mitigation Strategies (REMS) programs. In this study, we focus on the state policies, in part because it is more difficult to identify the effects of federal policy changes since they were enacted everywhere and hence can be confounded with other sources of changes in opioid prescribing. Our analysis estimates the effects of these state policies above and beyond any effects of the federal policies.

Prescription Drug Monitoring Programs

PDMPs may be used by states to identify excessive prescribing of opioids and potential opioid abuse. These programs create a centralized listing of prescriptions that gives health care providers information on patients' prescription histories. PDMPs are intended to limit access to prescription drugs when individuals are likely to misuse them—as evidenced by multiple prescriptions from multiple prescribers at the same time—leading to a reduction in prescription opioids among those with higher use. At the same time, PDMPs should not limit access to opioid prescriptions when the medications are used appropriately.[7] PDMP programs differ across states in a number of dimensions, including what information is collected in the database; who should report prescriptions and how often; whether providers are required to check the PDMP; whether the PDMP is implemented electronically; and who can and should access the PDMP and for what purpose (Thumula et al., 2019).

[7] Some studies do, however, suggest that PDMPs may also contribute to reduction in opioid use due to the hassle costs they pose (Alpert et al., 2020; Sacks et al., 2019).

Evidence from the general health setting indicates that PDMPs are effective in reducing opioid prescribing when states require that medical providers access PDMPs before issuing a new prescription, while voluntary use PDMPs did not affect opioid use.[8] This difference likely reflects increased utilization of PDMPs when states mandate their use (Pew Charitable Trusts, 2016), whereas voluntary PDMPs did not create strong incentives for providers to change their prescribing behavior.[9] We thus focus our estimates on adoption of must-access PDMPs as one of the policies of interest in our analysis.

Table 1 provides information about timing of the policies we examine. The first column shows when states implemented must-access PDMPs. We use the dates of policy implementation drawn primarily from Davis et al. (2019) and Thumula et al. (2019), supplementing with our own investigation when the timing of policies was inconsistent with other sources of information on effective dates of policies. Prior studies show that choices about how to date the timing of policies contributed to differences in the estimated effects of the policies (Horwitz et al., 2018), such as dates of adoption versus implementation of access to PDMPs. (We study the effects of must-access PDMPs, but from here we often refer to these as simply "PDMPs.") We use the dates the PDMPs became effective; there is less uncertainty about interpreting these dates than the dates policies were enacted.

Limits on Initial Opioid Prescriptions

Policies introducing limits on initial opioid prescriptions respond to a concern that extensive new prescriptions to treat acute pain may contribute to longer-term use of opioids (Shah et al., 2017). These policies limit the initial opioid prescriptions—most commonly to 7 days of supply, although some states use limits of 3, 5, or 14 days of supply. A few states also limit the morphine equivalent daily dose that can be prescribed per day (North Carolina, Nevada, Tennessee), or specify that prescriptions are limited to the lowest effective dose (Maryland). As Table 1 shows, Massachusetts was the first state to limit initial opioid prescriptions, in 2016. By end of March 2018, 14 of our analysis states (out of 33 total) introduced

[8] See, e.g., Bao et al. (2018), Buchmueller and Carey (2018), Grecu et al. (2019), Kaestner and Ziedan (2019), Mallatt (2018), Meinhofer (2018), Wen et al. (2019), and Ziedan and Kaestner (2020).
[9] The effects of must-access PDMPs may instead reflect the implementation of electronic access to the program and a wider sharing of electronic records in the medical field (Kaestner and Ziedan, 2019; Wang, 2020).

such limits, and a few more states introduced them subsequently. Thus, for most states we only capture the early experience after limits were implemented. Furthermore, in many states these policies were implemented after the CDC published voluntary guidelines for primary care practitioners that recommend the "lowest effective dose" when deciding on prescriptions for opioids. The CDC guidelines indicate that 3 days or less of opioids will often be sufficient for treating acute pain, and more than 7 days will rarely be needed (CDC, 2016). While these guidelines are voluntary, they may have an effect on prescribing behavior across all states, thus limiting the effectiveness of state mandates after the CDC guidelines were issued.

Policies that limit prescribing vary in other dimensions, including the types of prescriptions affected (whether the limits apply to all prescriptions, acute pain prescriptions, only initial prescriptions, or only a subset of opioids). The policies also vary with respect to exceptions granted—including exceptions for chronic pain, cancer, substance use disorder treatment, or surgical pain, and exceptions based on a clinician's professional judgment (Thumula et al., 2019). We do not examine the differential effects of these variations in prescribing limits.

There is limited evidence about the effects of prescribing limits in the general health setting. Studies indicate a decrease in the percentage of opioid prescriptions above the days of supply limit after states implemented prescribing limits (Agarwal et al., 2019; Hincapie-Castillo et al., 2020). Sacks et al. (2019) compared trends in prescribing among states with and without the policies and found that limits on initial prescriptions appear to have reduced the length of initial opioid prescriptions for a population of patients with group health insurance, and increased the likelihood of short prescriptions. However, they also found an increase in the likelihood of opioid prescriptions dispensed to new users.

Other Opioid-Related Policies

Other state policies related to opioid treatment also have been implemented. State and national treatment guidelines highlight preferred approaches to prescribing practices.[10] Treatment guideline

[10] The national guidelines include the CDC guidelines for prescribing opioids for chronic pain, the general treatment guidelines of the American Pain Society and the American Academy of Pain Management, the occupational medical

approaches vary widely. Some state workers' compensation systems adopted general treatment guidelines that outline evidence about appropriate care, including use of opioids. Other states have adopted specific guidelines related to chronic pain and pain management. An important dimension of this policy is whether treatment guidelines are required to be followed and whether the state has mandatory utilization review to confirm that the care follows the guidelines. In general, guidelines discourage the use of opioids early after an injury, except for traumatic injuries or for patients with severe pain. When opioids are prescribed, the guidelines may recommend limits on the duration of prescriptions,[11] limits on maximum daily dose,[12] and checking PDMPs when prescribing opioids (Wang, 2017, Appendix B). Overall, there is limited evidence about the effects of adoption of medical treatment guidelines (Haegerich et al., 2014).

Some states have implemented drug formularies to directly target prescribing behavior in the workers' compensation system. These regulations often require additional approval steps for a subset of drugs (often including opioids along with many other medications). Evidence indicates that fewer non-recommended drugs (including opioids) were prescribed in Texas after the state implemented a drug formulary (TDI, Texas Workers' Compensation Research and Evaluation Group, 2013). Evidence from the 2018 formulary in California indicates a decrease in the share of prescriptions for drugs that were not exempt from prospective utilization review in the formulary, with opioid prescriptions contributing more than half of the decrease (Hayes and Swedlow, 2019).

States have also implemented a number of other policy interventions aimed at limiting excessive prescribing and opioid abuse. These include regulating pain clinics and limiting the dispensing of controlled substances by physicians. Prior studies suggest that opioid prescriptions dispensed to injured workers decreased in Florida after 2011 legislation regulating pain clinics and banning physician dispensing of Schedule II and III opioids, and in Kentucky after 2012 rule changes limiting dispensing of

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treatment guidelines of the American College of Occupational and Environmental Medicine (ACOEM), and the Official Disability Guidelines (ODG) by MCG Health.

[11] For example, ACOEM guidelines recommend limiting opioid prescriptions to two weeks. When treating acute pain, CDC guidelines suggest that physicians should prescribe the lowest effective dose, that 3 days of opioid prescriptions may often be enough, and that physicians should rarely need to prescribe more than 7 days of opioids (CDC, 2016). The guidelines are not mandatory and leave decisions to the discretion of the provider.

[12] For example, the 2014 update of the ACOEM guidelines sets the MME dose at 50 milligrams.

Schedule II and III opioids (Thumula, 2014, 2017). Since the primary goal of these policies was to limit physician dispensing of opioids, they are outside the scope of this analysis.

We provide several tests that demonstrate that these additional policies are unlikely to substantially affect our estimated effects of PDMPs and initial prescribing limits. First, we examined changes in outcomes prior to the policy changes we study to ensure that states that did and did not introduce the policies that we examine had similar evolution of outcomes prior to the policy interventions. Second, we show (in the online appendix) that some additional policies by and large did not influence the outcomes we study, and that including them in our analysis does not alter the estimated effects of PDMPs and initial prescribing limits.

The policies outlined above do not cover all of the initiatives designed to curb opioid overuse and abuse. Some states implemented mandatory provider education for safe opioid prescribing, conducted educational campaigns for patients about the dangers of opioid prescribing, and required written pain management plans for patients who receive opioids. States may also require continuing medical education on opioid prescribing and chronic pain management as a requirement for license renewal. Evidence about the effects of these interventions is quite limited, with earlier studies showing that most states did not mandate provider education (Davis and Carr, 2016). A review of the evidence in Ben-Shalom et al. (2020) found only a few studies on the effects of provider education with strong research designs; randomized control trials showed little effect of provider education on opioid prescribing (Pasquale et al., 2017). Likewise, Ben-Shalom et al. (2020) found only weak evidence of effects of patient education on opioid use. The changing prescribing practices may also be driven by multifaceted interventions combining multiple practices for safe prescribing introduced by health systems as well as some states. These interventions often include implementation of guidelines, provider training, patient outreach, and improved monitoring and communications between providers and patients (for a review, see Ben-Shalom et al., 2020). In our analysis, we examine the sensitivity of our estimates to adding controls for various treatment guidelines, but we did not collect information on provider or patient education requirements. At the federal level, changes in the CSA or REMS programs may have influenced opioid prescribing across all states. We address concerns about these federal regulations when we discuss our empirical approach.[13]

Data and Methods

Our analysis sample was derived from payment information on workers' compensation claims—the Workers Compensation Research Institute (WCRI) Detailed Benchmark/Evaluation (DBE) database. The DBE covers claims from national and regional insurers (including residual market carriers), state funds, and self-insured employers (from their third-party administrators). Since our main objective is to examine utilization of care and duration of disability, we extracted data on filled prescriptions, medical services that were provided, and temporary disability benefits made within 12 months after an injury.[14] The DBE includes detailed prescription transaction data and medical billing data that were collected from workers' compensation payors and their bill review and pharmacy benefit vendors. The data available for each prescription identify the specific medication prescribed based on National Drug Code (NDC), the date on which the prescription was filled, amounts charged and paid, the number of pills (for orally-administered opioids), and the strength of the medication in milligrams.[15] The data on other medical services include information on procedure type and date. The analysis includes information for workers injured between October 1, 2009, and March 31, 2018, in the 33 states covered in the DBE database.[16] These states represent 85 percent of benefits paid in 2017 (Weiss et al., 2019).

We focus our analysis on all claims and on the subset of claims with opioids (when studying characteristics of the opioid prescriptions). Many studies of workers' compensation outcomes focus on

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[13] States also adopted policies to try to reduce opioid overdoses and deaths. These include Good Samaritan laws that provide immunity from criminal prosecution for individuals helping themselves or others experiencing an overdose, laws providing support for naloxone access, and syringe exchange programs. Since we do not examine potential overdoses and death, these policies are also outside the scope of our analysis.
[14] Our results are robust to using data capturing care provided within 6 months after an injury (see the online appendix). The different observation windows after an injury reflect different "maturities" of the data that may be available. We cannot use longer maturities because a good deal of the policy variation we study is recent.
[15] We linked the prescription transactions to the Medi-Span® data by NDCs to identify the type of medication (e.g., drug name, therapeutic class, formulation, and strength). We used the classification scheme provided by Medi-Span®'s Generic Product Identifier (GPI) to assign drugs into different therapeutic drug groups.
[16] The states are Alabama, Arkansas, Arizona, California, Connecticut, Delaware, Florida, Georgia, Hawaii, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maryland, Massachusetts, Michigan, Minnesota, Mississippi, Missouri, Nevada, New Jersey, New Mexico, New York, North Carolina, Oklahoma, Pennsylvania, South Carolina, Tennessee, Texas, Virginia, and Wisconsin.

claims with more than 7 days of lost work time, as it is these claims, in general, that entail indemnity benefits and receive most of the medical care.[17] However, we do not want to condition on the number of days of lost work time because it may be influenced by opioid prescribing behavior, leading to biased estimates of the policy effect.[18]

We estimate the effects of state policies—must-access PDMPs and initial prescribing limits—by estimating regression models that compare how outcomes changed in states that adopted the policies ("treated states") relative to states that did not ("control states"), accounting for various other factors that could have affected these outcomes. While the federal efforts to combat opioid overuse likely contributed to the decline in opioid utilization across all states, our estimates reflect effects of state policies above and beyond the effects of the federal policy changes that should have affected every state.[19]

For our results to be valid, we have to believe that there were no other factors that led to divergent paths of the outcomes in the treated and control states. We ensure this, in part, by including in our statistical models control variables for other determinants of differential outcomes by state. These include industry composition,[20] average county-level unemployment rates,[21] average county-level median household income, average county-level percentage disabled, and average county-level percentage without health insurance.[22] The county-level information was added to the individual claim level data and then aggregated to the state level.[23] These controls address concerns about a changing mix of industries in treatment versus control states, changes in the access to employer-provided medical insurance over time

[17] Among claims that did not have more than 7 days of lost time, 9.1 percent had prescriptions for opioids.
[18] For example, suppose a particular policy both reduced opioid prescribing and decreased the days of lost work time. Then, if we restricted the analysis to claims with more than 7 days of lost work time, we might fail to detect the decline in opioid prescribing.
[19] Therefore, our estimates likely understate the combined effects of federal and state policy changes.
[20] We control for the shares in high-risk services, low-risk services, clerical/professional occupations (regardless of industry), manufacturing, construction, trade, and other industries, in the sample of injured workers that we use.
[21] We use the U.S. Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS); see https://www.bls.gov/lau/.
[22] These measures were derived from the U.S. Health Resources and Services Administration's Area Health Resources Files (AHRF); see https://data.hrsa.gov/data/download/.
[23] We match county-level information to our data using workers' county of residence (derived from workers' residence zip code information). When the residence location information is not available, we use location of the most frequent visit for evaluation and management services, location of the accident, or location of the employer.

in different states, etc. We also control generally for other state-specific factors that do not change over time and time effects that are common across states, by including fixed state effects and fixed quarter effects (for each unique quarter in the data).[24]

We also pay attention to the possibility that there are unmeasured factors that drive variation in outcomes across states. Since they are unmeasured, we cannot directly incorporate them into our models. However, we check whether, prior to the policy change, trends in outcomes in the treated and control states, which may be attributable to unmeasured influences on the outcomes, are the same or not (i.e., do the "parallel trends" hold?). If there are parallel trends, we are more confident that the control states provide valid predictions for what would have happened in the treated states had the policy changes not occurred; if parallel trends are violated, we have less confidence in this approach and have to consider modifications of our statistical model that control for these trend differences to provide more reliable evidence, if possible. In fact, this part of our analysis turns out to be quite important for estimating the effects of initial prescribing limits.

Above, we explained that we focus on two types of state policy changes intended to restrict opioid prescribing: must-access PDMPs, and limits on the initial supply of opioids. We focus on these two policies mainly because there are too few instances of the other types of opioid-related state policy changes to be able to reliably estimate their effects. However, we also show (in the online appendix) that there is no clear evidence that these other policies mattered, and that excluding them from our analysis does not materially impact our estimated effects of PDMPs and initial prescribing limits.

We aggregate data to cells defined by state and calendar quarter of injury, and weight by the number of observations within each cell. These cells provide average outcomes of those who were injured in a quarter and reflect experience up to 12 months postinjury. Our analysis also accounts for partial exposure to policies during the 12 months, which can occur when a policy was implemented during the 12-month period after an injury date, affecting only a subset of the services provided. For example, if the

[24] The former account for differences across states in time-invariant workers' compensation policies, and other measures that are constant over time. The latter reflect factors that are changing across all states at the same time, such as federal rules and regulations.

policy was implemented 3 months after an injury, then the medical services for the injury provided after month 3 would be affected by the policy, while services provided in the first 3 months would not be subject to the policy. That is, the effects up to any point in the 12 months since the injury may reflect not simply the policy in effect at that point but the cumulative effects of the policy up to that point—which can depend on how long the policy has been in effect. Since we estimate regressions with single variables capturing each of the two policy changes—PDMPs and prescribing limits—we capture the partial exposure of those injured prior to policy changes in a richer manner than simply using a dummy variable for whether a policy was in effect. In particular, we define the policy variable as 1 if it was in effect for all 12 months of postinjury exposure (like a standard dummy variable), 0.75 if it was in effect for 3 of the 4 quarters of postinjury exposure, 0.5 if it was in effect for 2 of the 4 quarters of postinjury exposure, and 0.25 if it was in effect for only the last quarter of the 4 quarters of postinjury exposure.[25]

Results for Opioid Prescribing

We begin with "event-study" estimates of the effects of opioid prescription policies. We plot results from regressions that estimate the differential evolution of the outcomes we study in states that did and did not adopt these policies, by quarter. (For the states that did not adopt these policies, the regression model allows for different outcomes by quarter, via the inclusion of quarter indicator variables.) These estimates address a few key questions. First, the estimates prior to policy adoption tell us whether there were different prior trends in the treated and control states. If so, these would contradict the parallel trends assumption that underlies the identification strategy, implying that the post-treatment estimates should not be strongly interpreted as causal, at least without correcting for these prior trends (if possible).[26] Second, the estimates after policy adoption are informative about the evolution of the policy effect. One thing we expect is that the effects of the policies should emerge over the first year of adoption, depending on how

[25] We also show in the online appendix that the results are not sensitive to using nonlinear partial exposure weights that reflect the cumulative number of opioid prescriptions in different parts of the 12 months postinjury experience, rather than partial exposure weights based simply on the proportion of time "exposed" to the policy. This addresses concerns that medical services provided in months 4 through 12 may differ from medical services for injuries where the policy was in place for the full 12 months, and that the cumulative effects of the policies are not necessarily proportional to the number of quarters exposed to the policy change.

[26] See, e.g., Callaway and Sant'Anna (forthcoming) for how these concerns can affect estimators like those we use.

many months of the 12-month maturity period the policy is in effect. Third, this evidence tells us about the dynamics of the effects of the policy changes—for example, whether the effects of the policy fade sometime after adoption. However, we do not have very much post-adoption evidence given how late the policies changed in some states, and hence our estimates become noisier (from a statistical perspective) the further we look post-policy adoption.[27]

The estimates (and 95 percent confidence intervals) are displayed in Figure 2. The height of the plotted points is the estimated difference between treated and control states, at the leads and lags (by quarter) indicated on the horizontal axis. The vertical lines through the plotted points are 95 percent confidence intervals.

The estimates for PDMPs are on the left-hand side. Looking first at the pre-trends (estimates before quarter 0), there is very little evidence of violations of the parallel trends assumption. There is one instance (for MME for all claims, in the top row) where there is some hint of a downward trend prior to the policy change, but the trend is not present in the few quarters before the change.

Looking at the treatment effect for PDMPs (beginning with quarter zero), for all outcomes we see evidence of declines in the opioid prescribing measure post-treatment. As we would expect, in the quarters of partial exposure there is less evidence of an effect. And for four of the five outcomes (MME for all claims, number of prescriptions for all claims, MME for claims with opioids, and number of prescriptions for claims with opioids), there is evidence of a persistent and perhaps even continuing decline after full exposure to the policy change (indicated by the second vertical line).

The estimates for prescribing limits are on the right-hand side of Figure 2. Looking first at the pre-trends, two of the three outcomes for all claims—any opioids, and the number of opioid prescriptions—appear problematic, with both trending up before the treatment. Moreover, the trend largely continues, in linear fashion, after the treatment. This implies that estimated regression models that do not take account of these trends are likely to generate spurious evidence of a positive effect of prescribing limits on whether opioids are prescribed, whereas models that do account for these trends

[27] These longer-term estimates also become less representative of all treated states.

appear unlikely to generate evidence of an effect. For the claims with opioids, there are no apparent pre-trends. Moreover, it appears that there may be a negative effect on MME (for claims with opioids) from initial prescribing limits.[28]

While the estimates in Figure 2 are useful for describing the path of changes in opioid prescribing that are associated with the policy changes we study, they do use the data rather inefficiently because, effectively, separate treatment effects are estimated each quarter. We therefore build on what we have learned so far to construct regression models that capture what our detailed analyses have shown, but in a single parameter that measures the effect of the policy change.

In particular, we estimate regressions with two single variables capturing each of the two policy changes—PDMPs and prescribing limits. However, reflecting the partial exposure to those injured before the policy changes, rather than simply using a dummy variable to capture when a policy "turns on," we create the variable denoting the policy change as 0.25 when we have one postinjury quarter of exposure, 0.50 when we have two postinjury quarters of exposure, 0.75 when we have 3 quarters, and then 1 for four or more postinjury quarters. We refer to these as "timing-adjusted dummy variables." Furthermore, we vary our empirical specifications to address concerns about divergent trends in outcomes even before policies were introduced (as highlighted in some of the panels in Figure 2 for the effects of initial prescribing limits). In particular, we show results that account for different state-specific trends in outcomes, and, given the difficulty of estimating a causal effect of initial prescribing limits on outcomes for the all claims sample, we also highlight results from the specifications that focus only on PDMPs.

Table 2 reports our regression estimates for the five outcomes we use to characterize opioid prescribing. Columns (1)–(3) report estimates (for all claims) for MME, for whether any opioids were prescribed (the proportion of claims with opioids prescribed), and for the number of prescriptions. Columns (4) and (5) report results for MME and the number of prescriptions for the subset of claims with

[28] The online appendix also includes descriptive graphs on the effects of policy changes state by state. These graphs are useful in providing evidence on how heterogeneous effects might be across states, which is relevant to the question of how meaningful an estimate of the "average" effect of an opioid prescription policy across states is. We read the graphs as indicating that there is not too much heterogeneity, although there are some exceptions.

opioid prescriptions. The first rows of each panel in the table report the regression coefficients that measure the impact of the policy changes on the outcome. We also show the sample means of the dependent variables and the percentage change in the outcomes implied by applying the estimated impact to the sample mean.

We begin, in Panel A, with estimates that simply include the timing-adjusted dummy variables to estimate the effects of the introduction of PDMPs and initial prescribing limits, without accounting for the pre-trends we saw in Figure 2 in relation to initial prescribing limits. Looking first at PDMPs, we find that the policy reduced MME for all claims by 20.1, or 12 percent (column (1)), and MME for claims with opioid prescriptions by 131.1—a similar amount in percentage terms (12 percent) because MME conditional on prescriptions excludes many zeros (column (4)); these estimates are significant at the 5 or 1 percent level. We do not find an effect of PDMPs on whether opioids were prescribed or, for all claims, on the number of prescriptions (columns (2) and (3)); in both cases, the estimates are not significantly different from zero, and the estimated effects are small in percentage terms. Among claims with opioid prescriptions, however, we find that PDMPs reduced the number of prescriptions by 0.15, implying a 5 percent reduction, and the estimated effect is strongly statistically significant (at the 1 percent level). These effects of PDMPs are consistent with what we would expect given the goal of the policy. We should expect PDMPs to reduce the number of prescriptions, and we would anticipate this effect to be concentrated among those with prescriptions.

Next, consider the evidence on limits on initial opioid prescriptions. The evidence for all claims points to positive effects on whether opioids were prescribed and on the number of opioid prescriptions—effects that are large (15 percent and 18 percent, respectively) and statistically significant. However, these are the two outcomes for which the event-study graphs in Figure 2 suggested we would find a spurious positive effect if we did not account for the trends in the opioid measures that prevailed even before the policy changes. For that reason, these positive estimates are unreliable.

The evidence on claims with opioids is similar to that for PDMPs. Initial prescription limits appear to reduce MME (a 19 percent reduction), although the effect on the number of prescriptions is small. Figure 2 did not indicate problems with interpreting the estimates for the claims with opioids as problematic based on pre-trends, so we view this evidence as more plausibly causal. Presumably, this evidence reflects initial prescribing limits reducing opioid amounts among those who are prescribed opioids, consistent with how the policies are structured.

Panel B of Table 2 reports estimates of regression models that add state-specific trends. The evidence from Figure 2 suggests these are important for the evidence on the effects of initial prescribing limits on any opioids and on the number of prescriptions for all claims. Indeed, as Panel B shows, the estimated effects of initial prescribing limits on these outcomes now become small and statistically insignificant (columns (2) and (3)), consistent with what Figure 2 suggested. The estimated effects of PDMPs are qualitatively similar but smaller.

The estimates for the claims with opioids, in columns (4) and (5), are qualitatively similar with the state-specific trends included. We still find negative and significant effects of PDMPs on MME and on the number of opioid prescriptions, although the estimates are a little smaller. The estimated negative effect of initial prescribing limits on MME for claims with opioids also remains negative and statistically significant, although it is about one-third smaller (an 11 percent reduction, versus 19 percent in Panel A).

Finally, while the state-specific trends are needed to obtain plausibly causal estimates of the effects of initial prescribing limits on the outcomes for all claims, models with these included do not necessarily recover the most reliable estimates of the effects of PDMPs, since there are no prior trends in the outcomes in relation to the adoption of PDMPs. When state-specific trends are included but not needed, they can mask or distort the actual treatment effect, especially when the treatment effect grows over time (Meer and West, 2016)—exactly what Figure 2's event-study graphs suggest for the effects of PDMPs. Thus, in Panel C we report estimates of the models without the trends, but including only the PDMP policy variable. These estimates are quite similar to those in Panel A, indicating that PDMPs reduce MME for all claims, and reduce MME and the number of prescriptions for claims with opioids.

We next turn to the analysis of outcomes for specific injury groups, and (in the following subsections) to estimated effects on other outcomes, including alternative treatments. Given what we have found and reported regarding the association between policy adoption, outcomes, and pre-trends, going forward when we estimate the effects on outcomes for all claims, we simply omit initial prescribing limits and focus only on the effects of PDMPs (like in Panel C of Table 2). We do so because our preceding analysis suggests there is no causal effect of initial prescribing limits, but if we include the initial prescribing limits variable we have to include state-specific trends, which can create bias toward zero in the estimated effects of PDMPs. On the other hand, we have shown that omitting the initial prescribing limits does not alter the estimated effects of PDMPs. When we estimate effects on outcomes for claims with opioids, however, we do not need the state-specific trends; hence, we use the models in Panel A that include both policy variables—for PDMPs and initial prescribing limits.

With this explanation of the specifications in mind, we next look at evidence for different injury groups. These groups reflect common conditions for workers with injuries.[29] Among the injuries we examine, workers with neurologic spine pain had the highest likelihood of receiving opioids, and had the highest average MME per claim (Table 3). We estimate the effects of PDMPs on MME per claim, whether any opioids were prescribed, and number of opioid prescriptions for all claims (Panels A through C); and we estimate the effects of PDMPs and initial prescribing limits on MME and number of opioid prescriptions for the subsample of claims with opioids (Panels D and E). The analysis for these outcomes helps inform the interpretation of the estimated effects of PDMPs and of initial limits on opioid prescribing.

As shown in Table 3, the effect of must-access PDMPs on MME per claim for all claims as well as on MME per claim with opioids is a statistically significant reduction (at the 10 percent level or less) for neurologic spine pain, spine sprains and strains, and other sprains and strains cases; and we find a similar reduction for MME for claims with opioids for inflammations. For these injury groups, the estimated effects are sizable—10 to 21 percent in Panel A, and 11 to 21 percent in Panel D. (For the other injury groups, the estimated effects are smaller and statistically insignificant.) Furthermore, we see little shift in the percentage of claims with opioids (Panel B) and a decrease in the number of opioid

[29] The group of "other injuries" includes heterogeneous injuries that do not fit in main groups specified in Table 3.

prescriptions among claims with prescriptions (Panel E) due to PDMPs. This evidence suggest that PDMPs lead to fewer cases with large numbers of prescriptions for neurologic spine pain, spine sprains and strains, other sprains and strains cases, and inflammations (some of which have quite high average MME).

The estimated impact of initial prescribing limits on MME for claims with opioids is nearly uniformly negative across injuries, and statistically significant for five of the eight injury categories; the estimates are sizable for most injury categories (Panel D). The negative effect of initial prescribing limits on MME per claim with opioids is apparent for fractures, inflammations, neurologic spine pain, other sprains and strains, and the group of “other injuries.” Note that the effects on initial opioid limits on number of opioid prescriptions (among those with opioids) differed across injury groups—an increase for lacerations and contusions, a decrease for other sprains and strains, and little change for most other injuries (Panel E).

Results for Problematic Opioid Prescribing

Policies to reduce opioid use were adopted in large part to reduce problematic prescribing since multiple concurrent prescriptions may indicate excessive opioid use or opioid abuse. To assess the effectiveness of the policies on this dimension, we examine the effect of PDMPs and limits on initial opioid prescriptions on the proportion of claims with opioids with three alternative measures of problematic prescriptions: longer-term opioid prescribing (prescriptions within the first three months of an injury and three or more filled prescriptions between the 7th and 12th months after an injury); more than 90 days of opioids prescribed; and a morphine equivalent daily dose (MED) exceeding 120 mg.[30] While it is difficult to distinguish between appropriate and inappropriate use of opioids, the metrics presented here reflect higher quantities of opioids that were prescribed, which may indicate potential misuse or at least dangerous use that may lead to addiction (for a review of evidence see CDC, 2016).[31]

[30] As discussed above, we only estimate the effects of initial prescribing limits for the claims with opioids. Examination of event-study graphs similar to those in Figure 2, for the problematic opioid prescribing outcomes, revealed similar concerns about pre-trends in estimating the effects of initial prescribing limits on the outcomes for all claims (results available upon request).
[31] Similar metrics were used in prior studies along with measures indicating multiple prescribers and multiple pharmacies where the opioids where dispensed (Buchmueller and Corey, 2018; Sacks et al., 2019).

We report the regression results for all claims and for all claims with opioids in Table 4. We find strong evidence that PDMPs reduced longer-term prescribing for claims with opioids; the estimate implies a 12 percent reduction, and is statistically significant. None of the other estimates in the table are statistically significant, and except for the estimated effect on daily dose greater than 120 mg, for all claims, the other estimates are small in percentage terms.

Table 5 reports estimates of the models for problematic use estimated by injury type. The results in Table 5 for all claims, in the top three panels, are almost all statistically insignificant, with one exception.

However, when we look at the results for claims with opioids (bottom three panels in Table 5), we see more evidence that PDMPs reduce problematic use. This evidence is most pronounced for the same measure—longer-term opioid prescribing—for which we found that PDMPs reduce use across all injuries combined (in Table 4). Here, we see a similar reduction in longer-term prescribing for inflammations, spine sprains and strains, other sprains and strains, and other injuries. There is also a negative effect for neurologic spine pain, but it is not statistically significant. In addition, there are a few negative and significant estimates for the other two measures of problematic prescribing, in one case (for daily dose greater than 120 mg) for neurologic spine pain. We read the evidence in Table 5 as suggesting that PDMPs had some impact in reducing problematic opioid prescribing among claims with opioids. The weaker evidence in the top three panels—for all claims—is likely attributable to the fact that PDMPs do not restrict whether one gets opioids or not. Rather, they restrict the amount of opioids for those who are prescribed opioids.

Results for Substitution toward Other Kinds of Medical Care

The evidence to here suggests that PDMPs reduce opioid prescribing, amounts prescribed, and longer-term or problematic prescribing. These results for PDMPs raise the question of whether the opioid reductions lead health care providers to substitute other kinds of care. If (as the evidence suggests) policies reduce access to opioid therapies, workers may seek other treatments to deal with their pain. This care may include non-opioid pain medications, interventional pain management, and physical therapy.[32] Note that we focus on types of care that are often recommended in treatment guidelines as potential substitutes for opioid prescriptions when dealing with acute and chronic pain (see Thumula et al., 2019). Whether the emphasis is on using non-opioid pain medications or non-pharmacological treatments, the recommendation is to approach opioid-based treatment with caution.

There are a number of possible interpretations of different types of evidence on the effects of policies restricting opioid prescribing on alternative treatments. A decrease in opioid prescribing coupled with an increase in potential substitutes for opioids may indicate that workers still had pain but were able to find other treatment that is recommended in guidelines. While workers avoided exposure to potentially addictive substances, we do not know if the alternative approaches to pain led to improved recovery and improved outcomes. It is also unclear how to interpret results indicating a decrease in opioid utilization without a corresponding increase in substitute care. One possibility is that opioid prescriptions might have been unnecessary. For instance, workers might have received opioid prescriptions for multiple days supplied but only took a few pills and did not have a need for other prescriptions. Another possibility is that workers still had pain, but they faced issues accessing appropriate providers to receive alternative care (e.g., pain specialists for pain management were not available, or they reached limits on physical therapy visits). Since we do not have detailed medical information to determine whether any of the opioid prescriptions were unnecessary, we limit this discussion to highlighting different patterns of results that we see. We do think, though, that information on management of pain in response to changes in patterns of treatment is an important research need.

For this analysis, presented in Table 6, we report results by injury type.[33] Results by injury type

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[32] It may also include chiropractic care as well as acupuncture, although this care is not widespread in some of the states we study.

[33] We also constructed event-study graphs to examine pre-policy trends for these measures across injury groups. For most policy measure and injury group combinations, we found little evidence to contradict the parallel trends assumption. We found a few cases where event-study graphs indicated that differences in trends may have emerged prior to the policy implementation, mostly for limits on initial opioid prescribing, which—for this reason, paralleling what we did earlier—we do not consider here, instead focusing only on PDMPs. For the analysis of PDMPs, we found two instances where event-study graphs indicate that trends may have emerged before the policy. For these two cases (the effects of PDMPs on the number of non-opioid pain medications and on the percentage with active

are more meaningful because different types of health care are more appropriate for one kind of injury than another. For example, the nature of treatment is well determined for someone with a fracture, and there may be little flexibility to choose different treatment approaches and different types of care. For soft-tissue injuries or for non-specific low-back injuries, the treatment approaches vary much more widely across providers and across parts of the country, so there is more flexibility in treatment approaches (e.g., Yee et al., 2015). This analysis is done for all claims, and not for claims with opioids, since the question we consider is what kind of care is provided.

We begin, in the first row, by repeating the estimated effects on MME for all claims, to provide a contrast with alternative treatments. The key question in this table is whether PDMPs lead to substitution toward other kinds of medical care. There is some evidence of this for particular injury types, but not for others. For spine sprains and strains, we see little evidence of substitution to other types of care. While the MME decreased, the use of other types of care did not change. For other sprains and strains, we see some evidence of an increase in the number of non-opioid pain medications. The most consistent evidence of substitution is for lacerations and contusions, where PDMPs lead to significant increases in non-opioid pain prescriptions, and in whether interventional pain management services were provided and the number of visits for these services. In short, for lacerations and contusions, the evidence indicates that PDMPs generate substitution toward many measures of alternative care, even though we see little change in opioid prescribing for this group of injuries (and the amount of opioids prescribed for these injuries is the lowest of the injuries we examine). To be clear, though, the absolute sizes of the effects are very small (less than a one-quarter of one percentage point change).

There is also some evidence of substitution for neurologic spine pain in response to PDMPs. For this group of injuries, we observe a 13 percent decrease in the MME, a 14 percent increase in the number of non-opioid pain medication prescriptions, and increases in whether interventional pain management services were provided and the number of visits for these services. The evidence on the management of

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physical medicine services for lacerations and contusions), we estimate the model with state-specific time trends. The event-study graphs are available upon request.

neurologic spine pain is interesting because, among the injury groups we consider, it has the highest incidence of opioid prescriptions and the highest MME.

Results for Duration of Temporary Disability

Finally, in Savych et al. (2019), we found that longer-term opioid prescribing for low-back injuries increased the duration of temporary disability benefits (which are paid, in all states, for claims with more than 7 days of lost work time). The study also found little effect of receiving any opioid prescription on the duration of temporary disability benefits. Our evidence did not pertain to the simple correlation (controlling for worker and injury characteristics) between longer-term prescribing and duration, which we would expect to be positive because longer-term prescribing is more likely for more severe injuries—even on dimensions we cannot measure. Rather, we isolated the effect of exogenous variation in longer-term prescribing based on local prescribing patterns.

In this study, given that we find some evidence that PDMPs reduce longer-term opioid prescribing, we revisit the question of longer-term prescribing and the duration of temporary disability. Here, we look at a different source of exogenous variation in longer-term prescribing—namely, the adoption of PDMPs. Given that the duration of disability outcome is potentially quite different from the outcomes studied in earlier tables, in Table 7 we repeat the three kinds of analyses reported in Table 2: estimates of the effects of both PDMPs and initial prescribing limits with and without state-specific trends, and then estimates of the effects of PDMPs only, without these trends.

A comparison of Panels A and B shows that, as in Table 2, the models without state-specific time trends generate spurious evidence of a positive effect of initial prescribing limits. With the trends included, there is no effect of these limits.[34] Regardless of the specification, there is no statistically significant evidence of effects of PDMPs, and the confidence intervals cover a wide range of possible

[34] These findings are reflected in event-study graphs paralleling those in Figure 2. There is evidence of pre-trends in the direction of increasing duration for states adopting initial prescribing limits, necessitating the inclusion of state-specific trends to obtain reliable evidence on the effects of these limits. These event-study graphs are available upon request.

estimates, including substantial positive or negative effects. This may not be surprising since the primary goal of PDMP policies is to limit excessive prescribing and simultaneous prescribing by multiple providers, so the effects of these policies on duration of disability may be a secondary effect at best.

Conclusions

We provide evidence on the effects of must-access PDMPs and regulations limiting the duration of initial opioid prescriptions on utilization of opioids, utilization of care that may be a substitute for opioid prescriptions, and duration of temporary disability benefits. We find that must-access PDMPs contributed to a decline in opioid utilization. In particular, implementation of must-access PDMPs led to a 12 percent decrease in the MME amount of opioids prescribed. This policy also decreased the number of opioid prescriptions, while there was little change in whether workers received opioids. Furthermore, we find that must-access PDMPs contributed to a 12 percent decrease in the likelihood that workers receive opioids on a longer-term basis, among those with opioids. This response was consistent with the policy's goals—reducing the amount of opioids prescribed, including longer-term prescribing. We find that limits on initial opioid prescriptions reduced opioid use among claims with opioids. These limits resulted in a 19 percent decrease in the MME amount of opioids among claims with opioids.

We find limited evidence that workers increased the use of other types of care when policies restricted access to opioid prescriptions. For most groups of injuries, we see a decrease in the amount of opioids prescribed due to must-access PDMPs, but relatively small changes in use of other services that may be a substitute for opioids for managing pain. The one exception was for neurologic spine pain cases, where we find evidence consistent with partial substitution toward alternative types of care.

Finally, we do not find evidence that either must-access PDMPs or limits on initial prescribing resulted in changes in the duration of temporary disability benefits captured at 12 months of maturity.

Our study indicates that opioid-related policies are best understood when we look at multiple dimensions of medical care and the duration of postinjury recovery. A policy-driven decrease in opioid utilization without a corresponding increase in possible substitutes suggests that some of the initial opioid prescriptions may not have been necessary, that other treatment may not be effective in managing pain, or that workers had problems getting access to other types of care. A limited change in duration of temporary disability benefits adds support for the former interpretation—while workers received fewer opioids, they did not spend more time recovering from their injuries. Unfortunately, we do not have detailed medical information to decide whether the opioids were needed, or to measure how well pain was managed with and without opioid-related policies. Further research efforts are needed to better understand the implications of different types of pain management approaches for workers' recovery.

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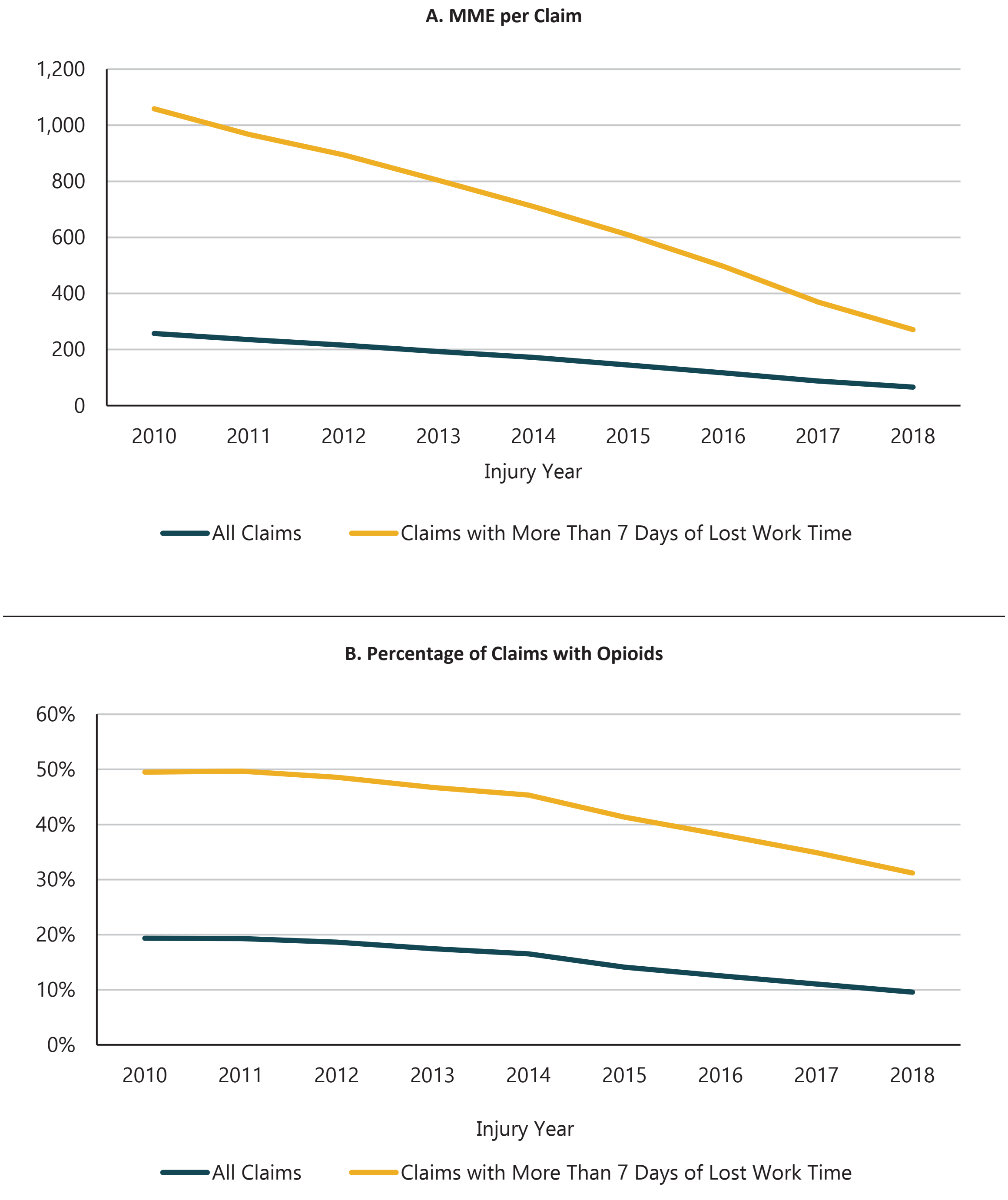
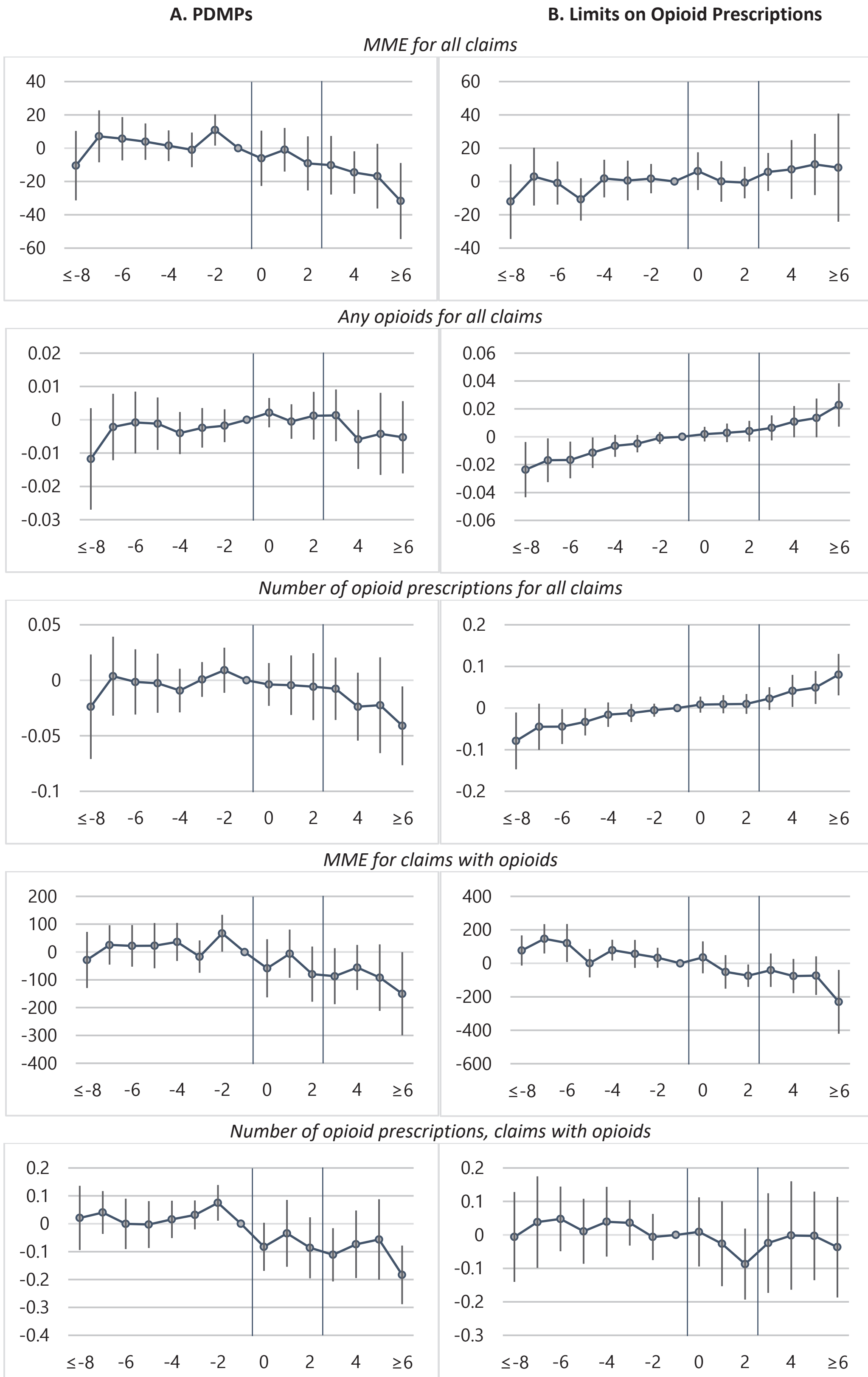
Figure 1 Descriptive Graphs for Time Series of MME and Percentage of Claims with Opioids



Notes: Average for claims at 12 months of maturity. Injury year 2010 indicates injuries that occurred between October 1, 2009, and September 30, 2010. Other years denoted similarly. 2018 includes injuries through March 31, 2018.

Key: MME: morphine milligram equivalent amount of opioids.

Figure 2 Event-Study Regressions



Notes: Coefficient estimates from the event-study analysis comparing differences between treated and control states by quarter before and after the date of policy implementation (displayed on horizontal axes) for claims at 12 months of maturity. The two full vertical lines indicate the beginning of the partial policy impact (quarter 0) and then the period when full impact begins (quarter 3). Ninety-five percent confidence intervals are shown as the vertical lines through the plotted points.

Key: MME: morphine milligram equivalent amount of opioids; PDMPs: must-access prescription drug monitoring programs.

Table 1 Effective Dates of Must-Access PDMPs and Limits on Initial Opioid Prescriptions

State	Effective Date of Must-Access PDMP	Limits on Initial Opioid Prescriptions	
		Effective Date	Days of Supply Limits
Alabama			
Arkansas	Aug-17	Aug-18	7
Arizona	Oct-17	Apr-18	5[a]
California	Oct-18		
Connecticut	Oct-15	Jul-16	7
Delaware	Mar-12	Apr-17	7
Florida	Jul-18	Jul-18	3 or 7[b]
Georgia	Jul-18		
Hawaii			
Illinois	Jan-18	--[c]	
Indiana	Jan-19	Jul-17	7
Iowa	Jul-18		
Kansas			
Kentucky	Jul-12	Jun-17	3
Louisiana	Aug-14	Aug-17	7
Maryland	Jul-18	May-17	--[d]
Massachusetts	Jan-16	Mar-16	7
Michigan	Jun-18	Jul-18	7
Minnesota		Jul-17	4[e]
Mississippi			
Missouri		Aug-18[c]	7
Nevada	Oct-15	Jun-17	14[f]
New Jersey	Nov-15	May-17	5[g]
New Mexico	Sep-12		
New York	Aug-13	Jul-16	7
North Carolina	Nov-18	Jan-18[h]	5
Oklahoma	Nov-15		
Pennsylvania	Jul-15	Jan-17	7[i]
South Carolina	May-17	May-18	7
Tennessee	Apr-13	Jul-18	--[j]
Texas	Sep-19		
Virginia	Jul-15	Mar-17	7[k]
Wisconsin	Apr-17	Apr-18	3–5

Note: We used multiple sources of information for this table, including Thumula et al. (2019), Davis et al. (2019), and online reports and articles on timing of policy adoption.

[a] In Arizona, the limit on an initial prescription for a Schedule II controlled substance that is an opioid following a surgical procedure is no more than a 14-day supply.

[b] Florida limits initial opioid prescriptions to 3 days of supply, or 7 days if the provider determines the lack of available alternative treatment.

[c] Illinois and Missouri had limits of 30 days of supply in earlier years, which were not part of the legislative trend toward limiting initial opioid prescriptions.

[d] In Maryland, the law requires prescribing opioids with the lowest effective dose, without limiting the number of days of supply.

[e] Minnesota has a four-day limit only for dental and refractive surgery pain.

[f] Nevada limits opioids to 14 days of supply and 90 MME per day.

[g] New Jersey limits prescriptions to the lowest effective dose.

[h] The "targeted controlled substances" are Schedule II and III opioids and narcotics per the North Carolina Controlled Substances Act, specifically those listed in N.C. Gen. Stat. § 90-90(1), (2) or 90-91(d). This provision does not apply to opioid prescriptions administered in a hospital, nursing home, hospice facility, or residential care facility.

[i] Pennsylvania's 7-day supply limit is for emergency department visits, urgent care, and hospital observation patient only.

[j] Tennessee has a cascading limit for initial opioid prescriptions. For pain, the limit is 10 days of supply with the maximum amount of 500 MME. For post-surgical pain, the limit is 20 days of supply with a maximum amount of 850 MME. If there is well-justified medical need, the limit is 30 days of supply, with a maximum total of 1,200 MME.

[k] Virginia limits initial opioids to 7 days of supply, or 14 days for post-surgical pain.

Key: MME: morphine milligram equivalent amount of opioids; PDMP: must-access prescription drug monitoring program.

Table 2 Estimated Effect of Policies on Opioid Utilization

	(1)	(2)	(3)	(4)	(5)
Outcome	**MME**	**Any Opioids**	**Number of Opioid Prescriptions**	**MME**	**Number of Opioid Prescriptions**
Sample	**All Claims**	**All Claims**	**All Claims**	**Claims with Opioids**	**Claims with Opioids**
A. Two policy specifications					
Must-access PDMP	-20.1374**	0.0042	-0.0093	-131.1257***	-0.1531***
	(8.5952)	(0.0063)	(0.0205)	(47.3257)	(0.0361)
Limits on initial opioid Rx	5.0133	0.0249**	0.0830***	-203.1554***	-0.0555
	(12.5930)	(0.0105)	(0.0296)	(67.9767)	(0.0556)
Observations	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	167.5	0.161	0.460	1069.0	2.849
% effect for PDMPs	-12%	3%	-2%	-12%	-5%
% effect for initial limits	3%	15%	18%	-19%	-2%
B. Two policy specifications with state-specific time trends					
Must-access PDMP	-18.8977**	0.0006	-0.0153	-106.8096**	-0.1045**
	(7.2854)	(0.0045)	(0.0140)	(43.0230)	(0.0427)
Limits on initial opioid Rx	-8.5116	0.0057	0.0065	-121.2334***	-0.0541
	(6.6762)	(0.0064)	(0.0153)	(38.6411)	(0.0677)
Observations	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	167.5	0.161	0.460	1069.0	2.849
% effect for PDMPs	-11%	0%	-3%	-10%	-4%
% effect for initial limits	-5%	4%	1%	-11%	-2%
C. Specifications for must-access PDMPs only					
Must-access PDMP	-18.9324*	0.0102	0.0107	-171.3341***	-0.1641***
	(9.6042)	(0.0076)	(0.0239)	(56.9753)	(0.0388)
Observations	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	167.5	0.161	0.460	1069.0	2.849
% effect for PDMPs	-11%	6%	2%	-16%	-6%

Notes: Estimates from state-level regressions for claims at 12 months of maturity, for injuries occurring October 2009–March 2018. Samples include all claims or claims with opioids (as indicated) aggregated to quarter of injury and state. Each observation is weighted by the number of claims represented. Controls are included for industry composition, average county-level unemployment rate, average county-level median household income, average county-level percentage disabled, and average county-level percentage without health insurance. We also control for state and (unique) quarter fixed effects. Estimates in Panel B also include controls for state-specific time trends.

*, **, *** Statistically significant at the 10 percent, 5 percent, and 1 percent level, respectively. Standard errors are clustered by state. To account for partial exposure for injuries for which the policy changed during the 12 months the data capture, we define the variable denoting the policy change as 1 if it was in effect for all 12 months of the postinjury exposure, 0.75 if it was in effect for 3 of the 4 quarters of postinjury exposure, 0.5 if it was in effect for 2 of the 4 quarters of postinjury exposure, and 0.25 if it was in effect for only the last quarter of the 4 quarters of postinjury exposure.

Key: MME: morphine milligram equivalent amount of opioids; PDMP: must-access prescription drug monitoring program; Rx: prescription.

Table 3 Estimated Effect of Policies on Opioid Utilization by Injury Group

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Fractures	Lacerations and Contusions	Inflammations	Neurologic Spine Pain	Spine Sprains and Strains	Other Sprains and Strains	Upper Extremity Neurologic	Other Injuries
A. MME for all claims								
Must-access PDMP	2.1268	3.4728	-46.3165	-160.4689**	-33.6107**	-13.3391*	3.1276	-6.6082
	(17.9836)	(3.0668)	(28.2368)	(66.0131)	(14.6385)	(7.8328)	(35.0599)	(7.9022)
Observations	1,118	1,118	1,118	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	354.586	34.341	360.143	1232.287	162.615	139.149	242.066	123.937
% effect for PDMPs	1%	10%	-13%	-13%	-21%	-10%	1%	-5%
B. Any opioids for all claims								
Must-access PDMP	0.0109	0.0129*	0.0094	0.0116	0.0154	0.0107	-0.0058	0.0063
	(0.0109)	(0.0066)	(0.0105)	(0.0131)	(0.0120)	(0.0088)	(0.0225)	(0.0044)
Observations	1,118	1,118	1,118	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	0.330	0.083	0.285	0.450	0.185	0.166	0.327	0.127
% effect for PDMPs	3%	15%	3%	3%	8%	6%	-2%	5%
C. Number of opioid prescriptions among all claims								
Must-access PDMP	0.0488	0.0226*	-0.0032	0.1020	0.0090	0.0029	0.0663	0.0135
	(0.0448)	(0.0116)	(0.0581)	(0.1181)	(0.0334)	(0.0240)	(0.0987)	(0.0177)
Observations	1,118	1,118	1,118	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	0.972	0.142	1.015	2.332	0.472	0.451	0.837	0.336
% effect for PDMPs	5%	16%	0%	4%	2%	1%	8%	4%
D. MME for claims with opioids								
Must-access PDMP	-11.9202	4.0842	-208.2098***	-368.4191**	-188.6995**	-93.2086**	-51.6436	-98.4392**
	(40.1921)	(28.3852)	(62.6083)	(140.9399)	(81.8014)	(42.7971)	(78.5488)	(44.2494)
Limits on initial opioid Rx	-284.0476***	-26.5378	-146.7429*	-418.3622***	1.9073	-140.9779***	-21.4663	-158.9752***
	(67.5311)	(40.4862)	(79.5444)	(132.5973)	(95.7819)	(39.0382)	(74.5350)	(42.2049)
Observations	1,118	1,118	1,118	1,118	1,118	1,118	1,095	1,118
Mean of outcome variable	1094.129	432.471	1289.048	2768.341	909.103	865.373	750.724	1001.242
% effect for PDMPs	-1%	1%	-16%	-13%	-21%	-11%	-7%	-10%
% effect for initial limits	-26%	-6%	-11%	-15%	0%	-16%	-3%	-16%
E. Number of opioid prescriptions among claims with opioids								
Must-access PDMP	-0.0542	-0.0151	-0.2612***	-0.1716	-0.1995***	-0.1618***	0.0227	-0.1345**
	(0.0510)	(0.0280)	(0.0732)	(0.1124)	(0.0635)	(0.0440)	(0.1120)	(0.0516)
Limits on initial opioid Rx	-0.0966	0.0913***	0.0588	0.1939	0.1115	-0.1157**	0.1218	-0.0281
	(0.0691)	(0.0264)	(0.0825)	(0.1768)	(0.0857)	(0.0520)	(0.1389)	(0.0719)
Observations	1,118	1,118	1,118	1,118	1,118	1,118	1,095	1,118
Mean of outcome variable	2.942	1.702	3.566	5.189	2.547	2.716	2.557	2.651
% effect for PDMPs	-2%	-1%	-7%	-3%	-8%	-6%	1%	-5%
% effect for initial limits	-3%	5%	2%	4%	4%	-4%	5%	-1%

Notes: Estimates from state-level regressions for claims at 12 months of maturity, for injuries occurring October 2009–March 2018. Samples include all claims or claims with opioids (as indicated) aggregated to quarter of injury and state. Each observation is weighted by the number of claims represented. Controls are included for industry composition, average county-level unemployment rate, average county-level median household income, average county-level percentage disabled, and average county-level percentage without health insurance. We also control for state and quarter fixed effects.

*, **, *** Statistically significant at the 10 percent, 5 percent, and 1 percent level, respectively. Standard errors are clustered by state. To account for partial exposure for injuries for which the policy changed during the 12 months the data capture, we define the variable denoting the policy change as 1 if it was in effect for all 12 months of the postinjury exposure, 0.75 if it was in effect for 3 of the 4 quarters of postinjury exposure, 0.5 if it was in effect for 2 of the 4 quarters of postinjury exposure, and 0.25 if it was in effect for only the last quarter of the 4 quarters of postinjury exposure.

Key: MME: morphine milligram equivalent amount of opioids; PDMP: must-access prescription drug monitoring program; Rx: prescription.

Table 4 Effect of Policies on Problematic Opioid Use Indicators

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	**Longer-Term Opioid Prescribing**	**More Than 90 Days of Opioids Prescribed**	**Daily Dose Greater Than 120 mg**	**Longer-Term Opioid Prescribing**	**More Than 90 Days of Opioids Prescribed**	**Daily Dose Greater Than 120 mg**
Sample	**All Claims**	**All Claims**	**All Claims**	**Claims with Opioids**	**Claims with Opioids**	**Claims with Opioids**
Must-access PDMP	-0.0001	-0.0002	-0.0002	-0.0070***	-0.0046	-0.0006
	(0.0007)	(0.0009)	(0.0002)	(0.0021)	(0.0046)	(0.0012)
Limits on initial opioid Rx				-0.0009	0.0005	-0.0005
				(0.0047)	(0.0076)	(0.0012)
Observations	1,118	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	0.0097	0.0111	0.0017	0.0601	0.1011	0.0159
% effect for PDMPs	-1%	-2%	-12%	-12%	-5%	-4%
% effect for initial limits				-1%	0%	-3%

Notes: Estimates from state-level regressions for claims at 12 months of maturity, for injuries occurring October 2009–March 2018. Samples include claims with opioids aggregated to quarter of injury and state. Each observation is weighted by the number of claims represented. Longer-term opioid prescriptions are defined as having prescriptions within the first three months after an injury and three or more filled opioid prescriptions between the 7th and 12th months after an injury. Controls are included for industry composition, average county-level unemployment rate, average county-level median household income, average county-level percentage disabled, and average county-level percentage without health insurance. We also control for state and quarter fixed effects. *, **, *** Statistically significant at the 10 percent, 5 percent, and 1 percent level, respectively. Standard errors are clustered by state. Data are aggregated to quarterly frequency. To account for partial exposure for injuries for which the policy changed during the 12 months the data capture, we define the variable denoting the policy change as 1 if it was in effect for all 12 months of the postinjury exposure, 0.75 if it was in effect for 3 of the 4 quarters of postinjury exposure, 0.5 if it was in effect for 2 of the 4 quarters of postinjury exposure, and 0.25 if it was in effect for only the last quarter of the 4 quarters of postinjury exposure.

Key: PDMP: must-access prescription drug monitoring program; Rx: prescription.

Table 5 Estimated Effect of Policies on Problematic Opioid Use Indicators by Injury Group

	Fractures	Lacerations and Contusions	Inflammations	Neurologic Spine Pain	Spine Sprains and Strains	Other Sprains and Strains	Upper Extremity Neurologic	Other injuries
Longer-term opioid prescribing among all claims								
Must-access PDMP	0.0019	0.0002	-0.0005	0.0045	-0.0006	-0.0002	0.0035	0.0002
	(0.0014)	(0.0001)	(0.0024)	(0.0055)	(0.0011)	(0.0006)	(0.0039)	(0.0006)
Observations	1,118	1,118	1,118	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	0.014	0.001	0.026	0.087	0.009	0.008	0.015	0.006
% effect for PDMPs	13%	20%	-2%	5%	-6%	-3%	24%	3%
More than 90 days of opioids prescribed among all claims								
Must-access PDMP	0.0019	0.0001	-0.0020	0.0061	-0.0011	0.0002	0.0023	-0.0000
	(0.0017)	(0.0002)	(0.0024)	(0.0068)	(0.0015)	(0.0007)	(0.0035)	(0.0007)
Observations	1,118	1,118	1,118	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	0.019	0.001	0.024	0.113	0.013	0.008	0.015	0.007
% effect for PDMPs	10%	8%	-8%	5%	-8%	2%	16%	0%
Daily dose greater than 120 mg among all claims								
Must-access PDMP	-0.0002	-0.0001	-0.0004	-0.0019**	-0.0002	0.0002	-0.0011	-0.0002
	(0.0005)	(0.0001)	(0.0008)	(0.0009)	(0.0001)	(0.0002)	(0.0014)	(0.0002)
Observations	1,118	1,118	1,118	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	0.005	0.001	0.005	0.006	0.001	0.002	0.005	0.002
% effect for PDMPs	-4%	-20%	-8%	-34%	-17%	11%	-21%	-13%
Longer-term opioid prescribing among claims with opioids								
Must-access PDMP	0.0003	0.0001	-0.0136***	-0.0097	-0.0094***	-0.0068**	-0.0010	-0.0055**
	(0.0031)	(0.0011)	(0.0040)	(0.0065)	(0.0028)	(0.0029)	(0.0072)	(0.0024)
Limits on initial opioid Rx	-0.0013	0.0043**	0.0089	0.0064	0.0055	-0.0030	0.0122	-0.0002
	(0.0042)	(0.0017)	(0.0054)	(0.0084)	(0.0052)	(0.0036)	(0.0097)	(0.0043)
Observations	1,118	1,118	1,118	1,118	1,118	1,118	1,095	1,118
Mean of outcome variable	0.043	0.012	0.092	0.195	0.051	0.047	0.045	0.049
% effect for PDMPs	1%	1%	-15%	-5%	-19%	-14%	-2%	-11%
% effect for initial limits	-3%	35%	10%	3%	11%	-6%	27%	0%
More than 90 days of opioids prescribed among claims with opioids								
Must-access PDMP	0.0013	-0.0007	-0.0147**	0.0049	-0.0130*	-0.0010	0.0021	-0.0077
	(0.0061)	(0.0037)	(0.0066)	(0.0125)	(0.0070)	(0.0040)	(0.0090)	(0.0048)
Limits on initial opioid Rx	-0.0015	0.0023	0.0098	-0.0015	0.0194*	-0.0013	0.0149	0.0047
	(0.0079)	(0.0041)	(0.0089)	(0.0153)	(0.0107)	(0.0052)	(0.0144)	(0.0061)
Observations	1,117	1,118	1,118	1,118	1,118	1,118	1,068	1,118
Mean of outcome variable	0.072	0.028	0.111	0.301	0.112	0.075	0.059	0.074
% effect for PDMPs	2%	-2%	-13%	2%	-12%	-1%	4%	-10%
% effect for initial limits	-2%	8%	9%	0%	17%	-2%	25%	6%
Daily dose greater than 120 mg among claims with opioids								
Must-access PDMP	-0.0007	-0.0027	-0.0009	-0.0044*	-0.0012	0.0029	-0.0012	-0.0012
	(0.0019)	(0.0019)	(0.0031)	(0.0024)	(0.0015)	(0.0019)	(0.0057)	(0.0014)
Limits on initial opioid Rx	-0.0011	-0.0015	0.0010	-0.0012	0.0019	-0.0005	-0.0059	0.0008
	(0.0027)	(0.0026)	(0.0032)	(0.0027)	(0.0019)	(0.0017)	(0.0051)	(0.0017)
Observations	1,117	1,118	1,118	1,118	1,118	1,118	1,068	1,118
Mean of outcome variable	0.019	0.012	0.023	0.015	0.011	0.017	0.021	0.017
% effect for PDMPs	-4%	-23%	-4%	-29%	-11%	18%	-6%	-7%
% effect for initial limits	-6%	-13%	4%	-8%	18%	-3%	-29%	5%

Notes: Estimates from state-level regressions for claims at 12 months of maturity, for injuries occurring October 2009–March 2018. Samples include all claims or claims with opioids (as indicated) aggregated to quarter of injury and state. Each observation is weighted by the number of claims represented. Longer-term opioid prescriptions are defined as having prescriptions within the first three months after an injury and three or more filled opioid prescriptions between the 7th and 12th months after an injury. Controls are included for industry composition, average county-level unemployment rate, average county-level median household income, average county-level percentage disabled, and average county-level percentage without health insurance. We also control for state and quarter fixed effects. *, **, *** Statistically significant at the 10 percent, 5 percent, and 1 percent level, respectively. Standard errors are clustered by state. To account for partial exposure for injuries for which the policy changed during the 12 months the data capture, we define the variable denoting the policy change as 1 if it was in effect for all 12 months of the postinjury exposure, 0.75 if it was in effect for 3 of the 4 quarters of postinjury exposure, 0.5 if it was in effect for 2 of the 4 quarters of postinjury exposure, and 0.25 if it was in effect for only the last quarter of the 4 quarters of postinjury exposure.
Key: PDMP: must-access prescription drug monitoring program; Rx: prescription.

Table 6 Estimated Effect of Opioid Policies on Other Care Overall and by Injury Group, All Claims

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Fractures	**Lacerations and Contusions**	**Inflammations**	**Neurologic Spine Pain**	**Spine Sprains and Strains**	**Other Sprains and Strains**	**Upper Extremity Neurologic**	**Other Injuries**
MME for all claims								
Must-access PDMP	2.1268	3.4728	-46.3165	-160.4689**	-33.6107**	-13.3391*	3.1276	-6.6082
	(17.9836)	(3.0668)	(28.2368)	(66.0131)	(14.6385)	(7.8328)	(35.0599)	(7.9022)
Observations	1,118	1,118	1,118	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	354.6	34.3	360.1	1232.3	162.6	139.1	242.1	123.9
% effect for PDMPs	1%	10%	-13%	-13%	-21%	-10%	1%	-5%
Number of non-opioid pain medication prescriptions								
Must-access PDMP	0.0319	0.0163**	0.1173*	0.3401***	0.0870	0.0719*	0.2045	0.0413**
	(0.0282)	(0.0076)	(0.0603)	(0.1173)	(0.0525)	(0.0367)	(0.1304)	(0.0183)
Observations	1,118	1,118	1,118	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	0.533	0.2662	1.1958	2.4916	0.9211	0.702	0.9732	0.4039
% effect for PDMPs	6%	6%	10%	14%	9%	10%	21%	10%
Any active physical medicine services								
Must access PDMP	0.0151	0.0022	0.0114	0.0131	0.0057	0.0111	0.0201	0.0039
	(0.0134)	(0.0016)	(0.0128)	(0.0219)	(0.0105)	(0.0086)	(0.0136)	(0.0036)
Observations	1,118	1,118	1,118	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	0.4148	0.0805	0.5688	0.7192	0.4259	0.3902	0.4866	0.1164
% effect for PDMPs	4%	3%	2%	2%	1%	3%	4%	3%
Number of visits for active physical medicine services								
Must-access PDMP	0.2882	0.0237	0.2397	-0.2028	0.0488	0.1646	-0.0123	0.1075
	(0.3407)	(0.0228)	(0.3232)	(0.4847)	(0.1173)	(0.1614)	(0.2415)	(0.0769)
Observations	1,118	1,118	1,118	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	7.4671	0.6944	10.4126	12.0179	3.9673	5.2183	6.2638	1.8177
% effect for PDMPs	4%	3%	2%	-2%	1%	3%	0%	6%
Any interventional pain management services								
Must-access PDMP	0.0048	0.0018***	0.0103	0.0217*	0.0004	0.0023	0.0043	0.0024
	(0.0036)	(0.0005)	(0.0075)	(0.0115)	(0.0016)	(0.0028)	(0.0133)	(0.0017)
Observations	1,118	1,118	1,118	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	0.1638	0.0178	0.4138	0.4616	0.0568	0.132	0.2783	0.0537
% effect for PDMPs	3%	10%	2%	5%	1%	2%	2%	4%
Number of visits for interventional pain management services								
Must-access PDMP	0.0029	0.0022***	0.0221	0.0624*	-0.0050	0.0030	0.0143	0.0024
	(0.0060)	(0.0007)	(0.0178)	(0.0352)	(0.0056)	(0.0047)	(0.0245)	(0.0039)
Observations	1,118	1,118	1,118	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	0.2296	0.0233	0.6995	1.0663	0.1098	0.2003	0.4191	0.0923
% effect for PDMPs	1%	9%	3%	6%	-5%	1%	3%	3%

Notes: Estimates from state-level regressions for claims at 12 months of maturity, for injuries occurring October 2009–March 2018. Sample includes all claims aggregated to quarter of injury and state. Each observation is weighted by the number of claims represented. Controls are included for industry composition, average county-level unemployment rate, average county-level median household income, average county-level percentage disabled, and average county-level percentage without health insurance. Specifications for number of non-opioid pain medications and any active physical medicine services for lacerations and contusions also include state-specific time trends. We also control for state and quarter fixed effects. *, **, *** Statistically significant at the 10 percent, 5 percent, and 1 percent level, respectively. Standard errors are clustered by state. To account for partial exposure for injuries for which the policy changed during the 12 months the data capture, we define the variable denoting the policy change as 1 if it was in effect for all 12 months of the postinjury exposure, 0.75 if it was in effect for 3 of the 4 quarters of postinjury exposure, 0.5 if it was in effect for 2 of the 4 quarters of postinjury exposure, and 0.25 if it was in effect for the last quarter of the 4 quarters of postinjury exposure.

Key: MME: morphine milligram equivalent amount of opioids; PDMP: must-access prescription drug monitoring program.

Table 7 Effect of Opioid Policies on Duration of Temporary Disability

	(1)	(2)	(3)
Outcome	**Weeks of Temporary Disability Payments**	**Weeks of Temporary Disability Payments**	**Claims with More Than 7 Days of Lost Work Time**
Sample	**Claims with More Than 7 Days of Lost Work Time**	**All Claims**	**All Claims**
A. Two policy specifications			
Must-access PDMP	0.0284	0.0053	-0.0011
	(0.1702)	(0.0580)	(0.0026)
Limits on initial opioid Rx	0.4422*	0.2366**	0.0101**
	(0.2260)	(0.0866)	(0.0046)
Observations	1,118	1,118	1,118
Mean of outcome variable	13.3	2.830	0.212
% effect for PDMPs	0%	0%	-1%
% effect for initial limits	3%	8%	5%
B. Two policy specifications with state-specific time trends			
Must-access PDMP	0.0648	0.0254	-0.0001
	(0.1546)	(0.0528)	(0.0022)
Limits on initial opioid Rx	0.2609	0.1085	0.0038
	(0.2077)	(0.0905)	(0.0037)
Observations	1,118	1,118	1,118
Mean of outcome variable	13.3	2.830	0.212
% effect for PDMPs	0%	1%	0%
% effect for initial limits	2%	4%	2%
C. Specifications for PDMPs only			
Must-access PDMP	0.1455	0.0622	0.0013
	(0.1605)	(0.0638)	(0.0029)
Observations	1,118	1,118	1,118
Mean of outcome variable	13.3	2.830	0.212
% effect for PDMPs	1%	2%	1%

Notes: Estimates from state-level regressions for claims at 12 months of maturity, for injuries occurring October 2009–March 2018. Samples include all claims or claims with more than 7 days of lost work time (as indicated) aggregated to quarter of injury and state. Each observation is weighted by the number of claims represented. Controls are included for industry composition, average county-level unemployment rate, average county-level median household income, average county-level percentage disabled, and average county-level percentage without health insurance. We also control for state and quarter fixed effects. *, **, *** Statistically significant at the 10 percent, 5 percent, and 1 percent level, respectively. Standard errors are clustered by state. To account for partial exposure for injuries for which the policy changed during the 12 months the data capture, we define the variable denoting the policy change as 1 if it was in effect for all 12 months of the postinjury exposure, 0.75 if it was in effect for 3 of the 4 quarters of postinjury exposure, 0.5 if it was in effect for 2 of the 4 quarters of postinjury exposure, and 0.25 if it was in effect for only the last quarter of the 4 quarters of postinjury exposure.
Key: PDMP: must-access prescription drug monitoring program; Rx: prescription.

Online Appendix:

Effects of Opioid-Related Policies on Opioid Utilization, Nature of Medical Care, and Duration of Disability

Descriptive Figures by State

Figures A.1-A.5 provide graphical depictions of the evidence on how opioid prescribing changed in the states that adopted PDMPs (shorthand for must-access PDMPs) or limits on initial opioid prescriptions, relative to other states. These graphs are only descriptive. They do not account for other sources of changes in outcomes (including federal policies or regulations), nor do they lend themselves to precise estimation of the effects of the policy changes, or statistical inference (assessing whether the changes associated with policies are statistically significant). Perhaps even more important, they do not separately estimate the effects of one policy (e.g., PDMPs) controlling for the effects of the other policy (e.g., initial prescribing limits).

Panel A of Figure A.1 shows average MME for all claims, for the pre- and post-policy change period for each state that adopted a PDMP.[1] We also show a pair of bars averaging across the control states (labeled "Controls"), defining the pre- and post-periods based on the median month of the policy change for the states that adopted the policy. The states are ranked based on the change in opioid prescribing between these two periods (smallest to largest). Consistent with Figure 1 in the main paper, the graph shows MME per claim declining in all treated states, as well as in the control states where the policy did not change. The decrease in the control states may reflect the effect of federal regulations that are common across all states. For most of the states that adopted PDMPs, though, the decline was larger than in the control states that did not adopt PDMPs (the states to the right of the control states in the figure), suggesting that PDMPs reduced MME.[2]

Panel B shows similar evidence, but for limits on initial opioid prescriptions. Again, we see that MME per claim fell in all states that adopted these limits. But MME per claim also fell in the control states, and the decline in about one-half of the treated states was smaller than the decline in the control states, indicating a lack of clear evidence that limits on initial opioid prescriptions reduced MME for all claims.

Figure A.2 shows the same kind of analysis, but for the outcome of whether any opioids were prescribed. In Panel A, for the effects of PDMPs, we again see declines for all treated states and the control states, although the decline in most of the treated states was smaller than in the control states. In Panel B, for initial prescribing limits, there is again no clear evidence of a difference in the change in the outcome—in this case, any opioid prescribing—associated with adopting initial prescribing limits. All states show a decline, but the decline is smaller in most of the treated states than in the control states. Thus, there is little indication that must-access PDMPs or initial prescribing limits reduced the likelihood that opioids were prescribed.

Figure A.3 shows the number of opioid prescriptions, still for all claims. In Panel A, for the effects of PDMPs, we again see that the outcome declined for all states, with the decline smaller in most of the treated states than the control states. In Panel B, for initial prescribing limits, the decline is also smaller in most of the treated states. Thus, there is little indication that PDMPs or prescribing limits reduced the number of opioid prescriptions.

Figure A.4 turns to evidence on claims with opioids, in this case for MME. In Panel A, for the effects of PDMPs, there is rather clear evidence of larger declines in the treated states. Compared with the

[1] For the pre-policy period, we show information for injuries that occurred between 24 and 12 months prior to the policy implementation, to avoid showing estimates for claims with partial policy exposure.

[2] Cross-referencing with Table 1 in the main paper, one can also see that the states that were early adopters of must-access PDMPs tended to have larger declines in average MME, a result confirmed in the statistical analysis presented in this online appendix.

control states, the decline in MME per claim with opioids is larger for all treated states except one. In Panel B, for initial prescribing limits, there is some heterogeneity, but the decline is generally larger in the treated states. Thus, this evidence suggests that both policies may have reduced MME for claims with opioids.

Figure A.5 reports evidence for the number of opioid prescriptions for claims with opioids. In Panel A, for the effects of PDMPs, we see evidence that the decline was larger in most treated states than in the control states. In Panel B, for initial prescribing limits, we see a similar finding, so both panels suggest that the policies reduced the number of prescriptions for claims with opioids.

We next present similar figures for measures of problematic opioid prescribing. Panel A of Figure A.6 reports evidence for the proportion of claims with longer-term prescribing among claims with opioids. The graph shows that for most states, the decline in longer-term prescribing was larger in states that adopted PDMPs. Panel B shows similar evidence, but for limits on initial opioid prescriptions. We see no clear evidence that these initial limits affected longer-term prescribing. For initial prescribing limits, the change in the control states is roughly in the middle of the distribution of changes between the two periods.

Figure A.7 presents the evidence for the second measure of problematic use—more than 90 days of opioids prescribed. In Panel A, the decline in this problematic use measure is generally larger in the states that adopted PDMPs. In Panel B, for initial prescribing limits, the comparison between treated and control states also suggests that the decline tends to be larger in the treated states.

Figure A.8 presents the evidence for the third measure of problematic use—daily dose exceeding 120 mg. In Panel A, this measure fell more in states that adopted PDMPs, relative to the control states, although the evidence is fairly balanced as there are a number of treated states where this measure fell by less. In Panel B, there is less evidence that initial prescribing limits reduced this measure, as it actually increased in some treated states but fell in the control states—overall suggesting no clear evidence of an effect of initial prescribing limits.

Figures A.9-A.11 show similar information, but for all claims (and looking only at PDMPs). None of these figures paints a crystal clear picture. There were more treated states with larger declines in problematic use than in the control states, but in each case there were a number of states with smaller declines. This heterogeneity might give us some pause in drawing strong conclusions about the effects of PDMPs on problematic opioid prescribing for the set of all claims.

As just noted, these graphs are useful in providing evidence on how heterogeneous effects might be across states, which is relevant to the question of how meaningful an estimate of the “average” effect of an opioid prescription policy across states is. We read the preceding figures as indicating that there is not too much heterogeneity, although there are some exceptions that need to be kept in mind when interpreting our results in the main paper.

Timing of Other Policies

Table A.1 provides information for the other policies that we use in robustness checks of our analyses. We list information on whether states had treatment guidelines and whether those treatment guidelines were mandatory. In our robustness analysis, we include controls for mandatory treatment guidelines, mandatory treatment guidelines and/or utilization review for opioid prescriptions or chronic pain, and whether states introduced a mandatory drug formulary.

Duration of Initial Prescriptions Before and After Implementation of Days of Supply Limits

Since overall prescribing per claim is not the immediate target of policies limiting initial opioid prescriptions, it is informative to consider changes in the measure most directly targeted by limits on initial opioid prescriptions—the number of days of supply for the first opioid prescription after an injury. Figure A.12 shows the percentage of initial opioid prescriptions that were above the days of supply limits before and after the limits were implemented, ranked by the days of supply limits (3, 4, 5, 7, 10, or 14 days of supply). For each state, we show the percentage of first opioid prescriptions that were for more days than the limit set in the regulations. A higher bar means that more prescriptions had days supplied beyond the limit. We find that limits on initial opioid prescriptions were likely to affect a sizable number of prescriptions in most states, although the effect varies across states—the states that imposed shorter days' supply limits had a higher percentage of prescriptions that would be affected by such limits. Consider Florida, Kentucky, and Wisconsin, which adopted the most stringent (3 days) limits (see Table 1 in the main paper). In these states, more than 60 percent of opioid prescriptions were for more than 3 days. In states with a 7 days of supply limit, between 25 and 52 percent of prescriptions prior to the policy implementation were for more than 7 days of supply. In the two analysis states with 10 or 14 days of supply limits (Tennessee and Nevada), only about 10 percent of prescriptions were above that limit, suggesting that days of supply limits above 10 or 14 days are likely to affect only a small number of prescriptions.

The figure also shows that prescriptions above the limit were common before the limits were introduced and remained common after the effective date of the regulations. This likely reflects exceptions to the limits discussed in the main text. At the same time, the policy does appear to have had an effect—the percentage of prescriptions above the limit decreased. In most states, the decrease in the percentage of prescriptions above the limit was greater than in the control states.

Robustness Checks

We conducted a number of robustness checks and estimated a number of alternative specifications to verify that our main conclusions hold up under reasonable alternative ways to alter our statistical models, which they do. Many of those estimates are presented in Tables A.2-A.4 (along with the original estimates from the main paper). We summarize our robustness analyses here.

Sensitivity to changing partial policy exposure weights

In our main specification, we estimate effects of the policies using one way of capturing the gradual increase in policy exposure depending on timing of the injury relative to when the policy became effective. Here, we consider an alternative approach, computing the partial exposure weights or values from data on the timing of opioid prescriptions. We estimate the percentage of prescriptions that were exposed to the policy if the policy was implemented in each of the quarters within a year after an injury, and use those values to indicate partial implementation (using the data on pre-treatment observations). We find that 53 percent of prescriptions were subject to the policy when the policy was in effect for 3 out of the 4 quarters after an injury, 30 percent when the policy was in effect for 2 out of the 4 quarters after an injury, and 14 percent when the policy was in effect for 1 out of the 4 quarters postinjury. Estimates using these weights, rather than 75, 50, and 25 percent, are provided in column (2) of Tables A.2-A.4. (In either case, the weight is 1 (100 percent) when the policy was in effect for all 4 quarters.) The estimates are very similar to the estimates from the main specification, which are repeated in column (1) of Tables A.2-A.4.

Estimates using 6 months of maturity data

Shifting to using data at 6 months of maturity allows us to extend the injury dates that we include by 6 months through September 2018 (with the latest evaluation date of March 21, 2019), and to explore sensitivity of the results to a different window. This allows us to observe the full implementation of the policies for a few more states that adopted must-access PDMPs and limits on initial opioid prescriptions between April and September 2018. Estimates that use 6-month maturity data are presented in column (3) of Tables A.2-A.4. For most measures, the implied percentage change effects are very similar to what we find in our main specification (the absolute effects are smaller, as we would expect from the shorter maturity). Note that we do not estimate models for longer-term opioid prescribing in the 6-month maturity data since this measure is defined using 12 months of postinjury experience.

Estimates that exclude California

Thumula et al. (2019) documented that California had one of the largest decreases in opioid utilization between 2012 and 2016 for claims at 24 months of maturity. The authors attribute this change to the strengthening of the independent medical review (IMR) process for resolution of medical disputes, and the California Workers' Compensation Institute (CWCI) estimated that about one-third of the pharmaceutical IMRs were about opioids (David and Bullis, 2019). Columns (4) and (5) of Tables A.2-A.4 provide estimates that exclude California from the analysis to see if the results are sensitive to an exclusion of a large state that represents 20 percent of the workers' compensation benefits paid in the country. We find that some of the estimates that we present in our main analysis are sensitive to whether we exclude California, and we find that estimates are less precise when the samples include California. However, our main findings are unchanged, although we find stronger effects of PDMPs on longer-term opioid prescribing for all claims.[3]

Estimates without controls for population weights

In our main specification, we use weights reflecting the number of claims with opioid prescriptions in each state. This gives a larger weight to more populous states and makes the estimates representative of workers (or their claims). As an alternative, we also present specifications that do not weight by number of prescriptions, giving each state an equal weight and making the estimates representative of states. These estimates are presented in column (6) of Tables A.2-A.4. Again, the estimates are robust.

Specifications that control for other policies

We performed a number of other analyses testing whether our results hold when we add more policy controls. Column (7) of Tables A.2-A.4 shows estimates for our main policy measures when we add controls for three other opioid-related policies: mandatory treatment guidelines, mandatory pain treatment guidelines, and drug formularies. The estimates for the main policies do not change much when we include additional policy controls.

In addition, in Panel A of Tables A.5 through A.7, we show the estimates for these additional policies. We generally find little evidence of effects of treatment guidelines and opioid treatment guidelines. While we find strong negative estimates for drug formularies, the data for this measure often do not satisfy the parallel trends assumption necessary to interpret the estimates as causal; when we look at the event-study graphs for this measure, we find substantial divergence in pre-policy trends between states that adopted this policy and states that did not. As an example, see Figure A.13 for the substantial divergence in pre-policy trends for drug formulary policy for MME for all claims.[4]

Heterogeneous effects by year of policy adoption

Panels B and C of Tables A.5-A.7 show estimates from specifications that allow the effects of policies to vary by year of policy adoption. Panel B shows results by year of adoption of PDMPs while Panel C shows results by year of adoption of limits of initial opioid prescription. For both policies, we find stronger effects in states that were the first ones to implement policies, and sometimes a different direction of the effect in more recent years. For example, we find that PDMPs introduced in 2012-2013

[3] We study the effects of must-access PDMPs but often refer to these as simply "PDMPs."
[4] The event-study graphs for other policy measures are available upon request.

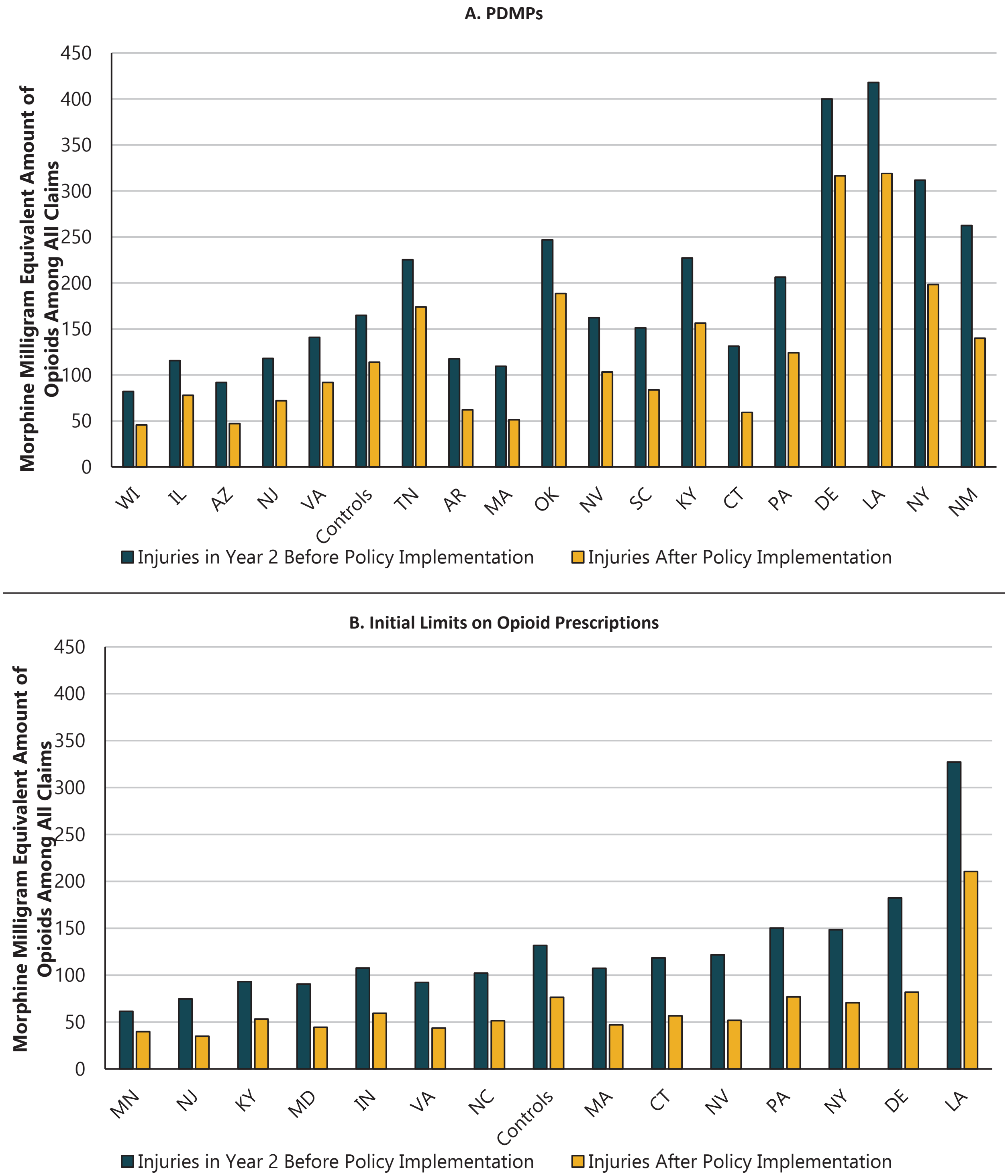
resulted in a 37 percent decrease in MME per claim for all claims,[5] while PDMPs implemented in 2014-2015 resulted in a 3 percent decrease in MME per claim for all claims, and PDMPs implemented in 2016-2018 resulted in an 8 percent increase in MME per claim for all claims. The estimates for claims with opioids are similar—26, 4, and 1 percent decreases, respectively. This is also consistent with the finding discussed for event studies that showed that estimates of policy effects strengthen as more time passes after policy implementation. This may also suggest that the states that were the first adopters had the biggest problems with opioid prescribing, which contributed to a bigger response to a policy change.

Robustness to leaving out one state at a time

We also verified that our estimates of the effects of PDMPs and initial prescribing limits on our measures of opioid utilization do not change substantially when we change the sample to exclude one state at a time. This suggests that our estimates are not driven by experience in a single state. Figures displaying these results are available upon request.

[5] We estimate this percentage effect by dividing the coefficient presented in Table A.5 by the mean of the dependent variable: 61.64/167.48 = 37 percent. The other percentage changes are estimated similarly.

Figure A.1 MME for All Claims

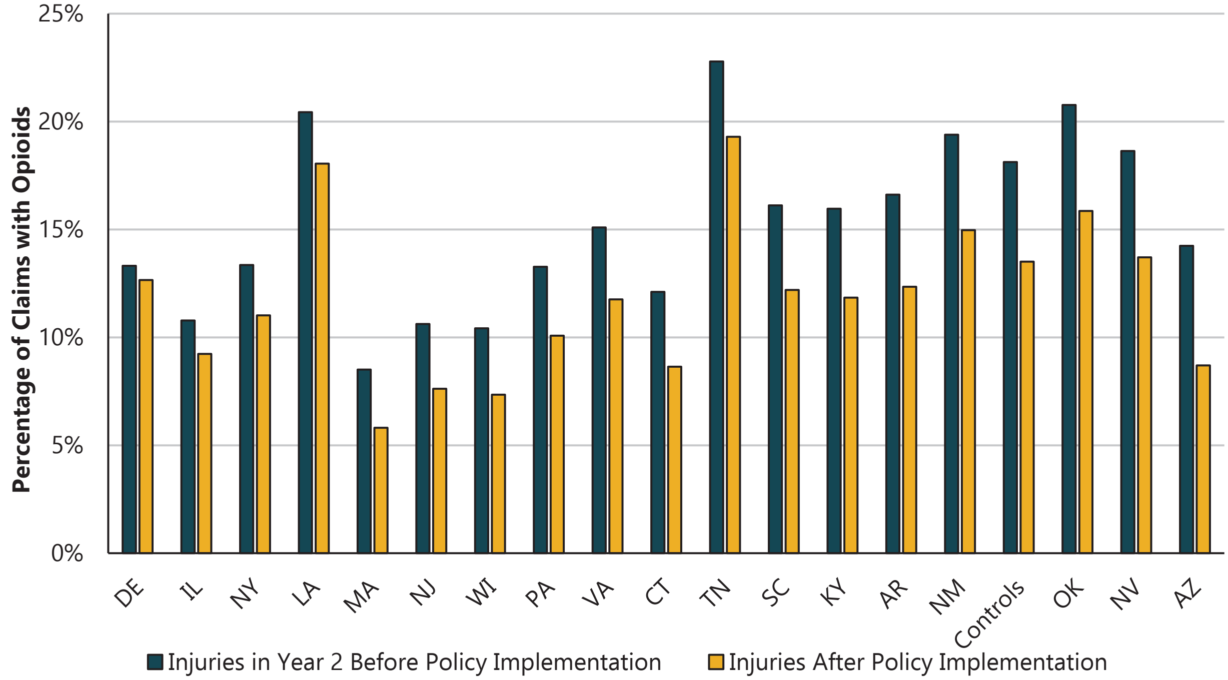


Notes: Average for claims at 12 months of maturity. We show measures for injuries that occurred in months 12 to 24 before policy implementation and for injuries that occurred within 12 months after policy implementation. For the control group, we use the median policy implementation month in the sample. States are sorted by change in measure from before to after policy implementation.

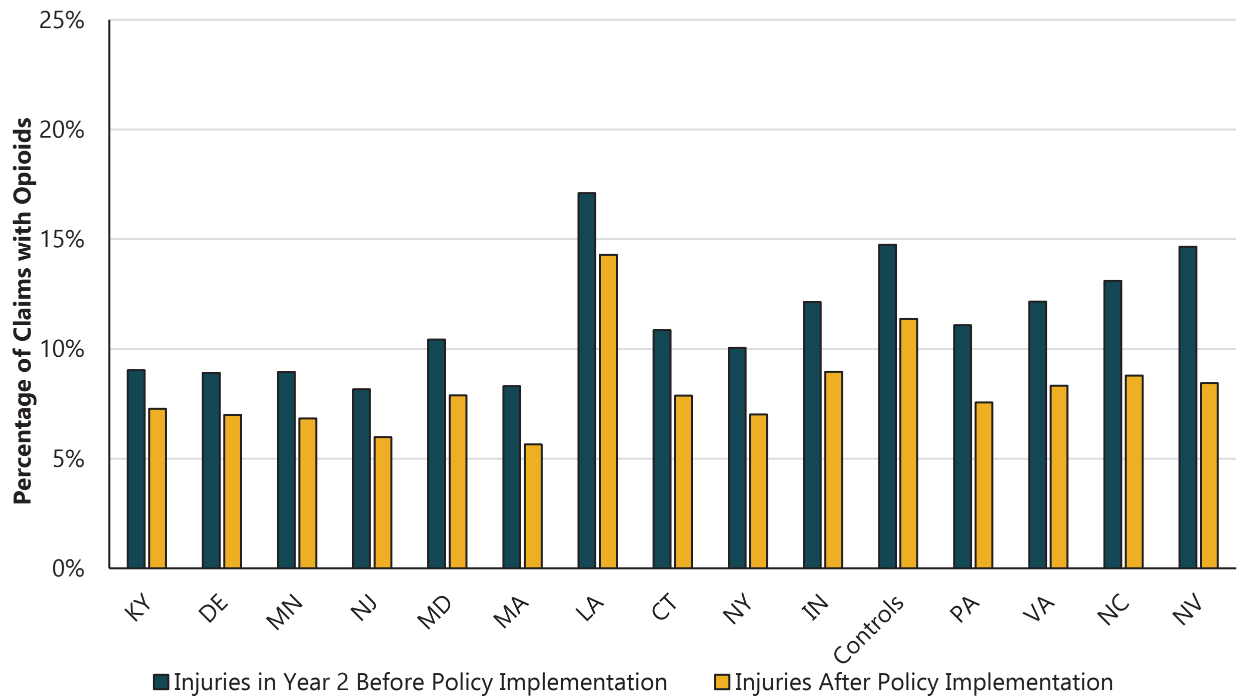
Key: MME: morphine milligram equivalent amount of opioids; PDMPs: must-access prescription drug monitoring programs.

Figure A.2 Any Opioids (all claims)

A. PDMPs



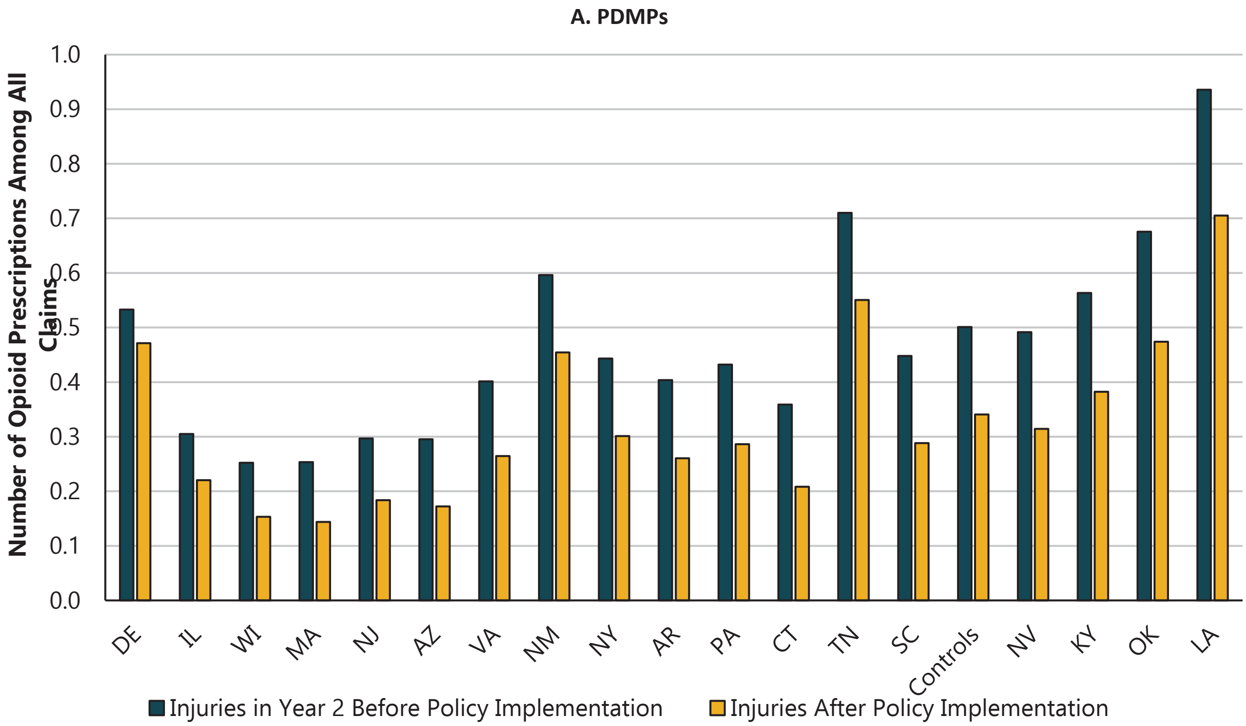
B. Initial Limits on Opioid Prescriptions

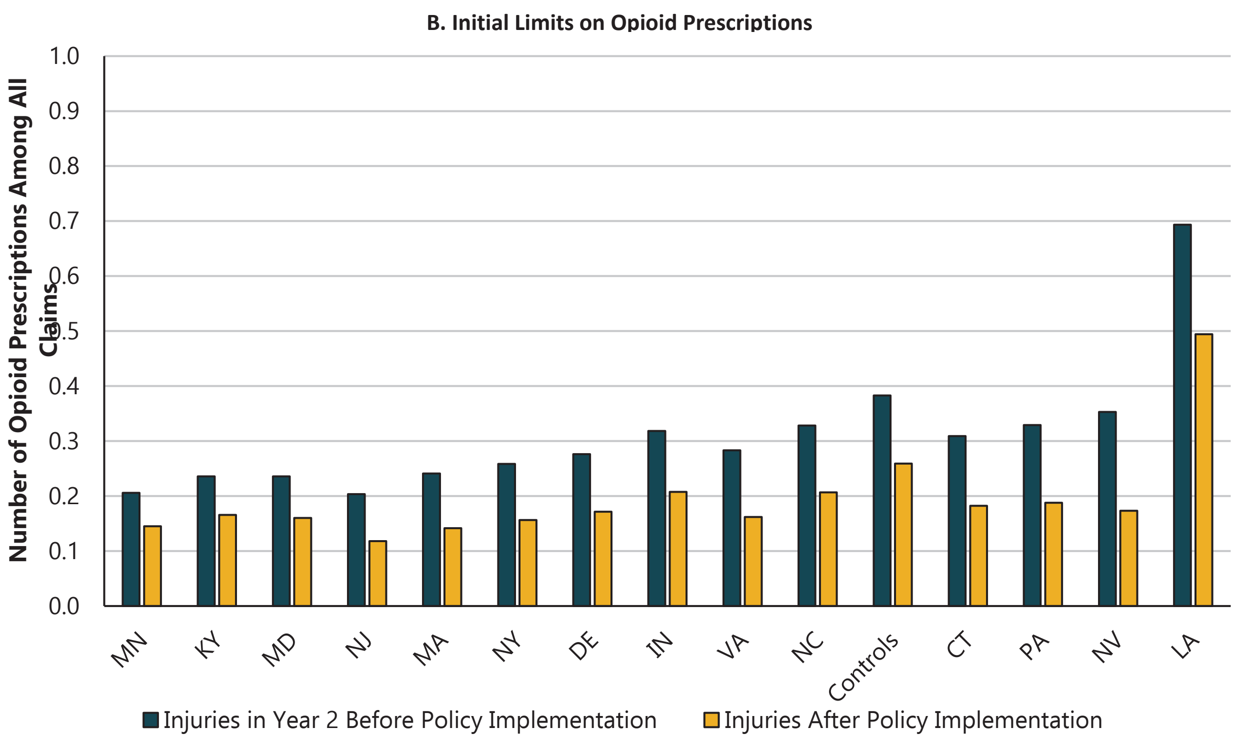


Notes: Average for claims at 12 months of maturity. We show measures for injuries that occurred in months 12 to 24 before policy implementation and for injuries that occurred within 12 months after policy implementation. For the control group, we use the median policy implementation month in the sample. States are sorted by change in measure from before to after policy implementation.

Key: PDMPs: must-access prescription drug monitoring programs.

Figure A.3 Number of Opioid Prescriptions (all claims)



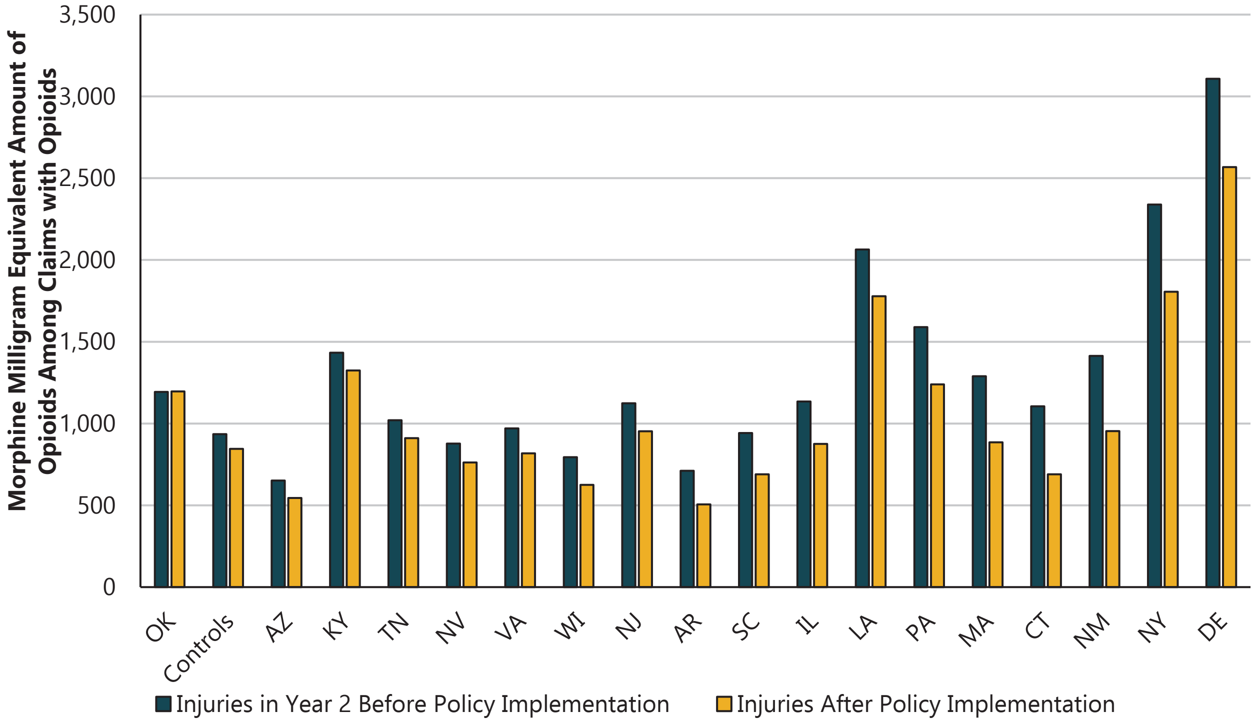


Notes: Average for claims at 12 months of maturity. We show measures for injuries that occurred in months 12 to 24 before policy implementation and for injuries that occurred within 12 months after policy implementation. For the control group, we use the median policy implementation month in the sample. States are sorted by change in measure from before to after policy implementation.

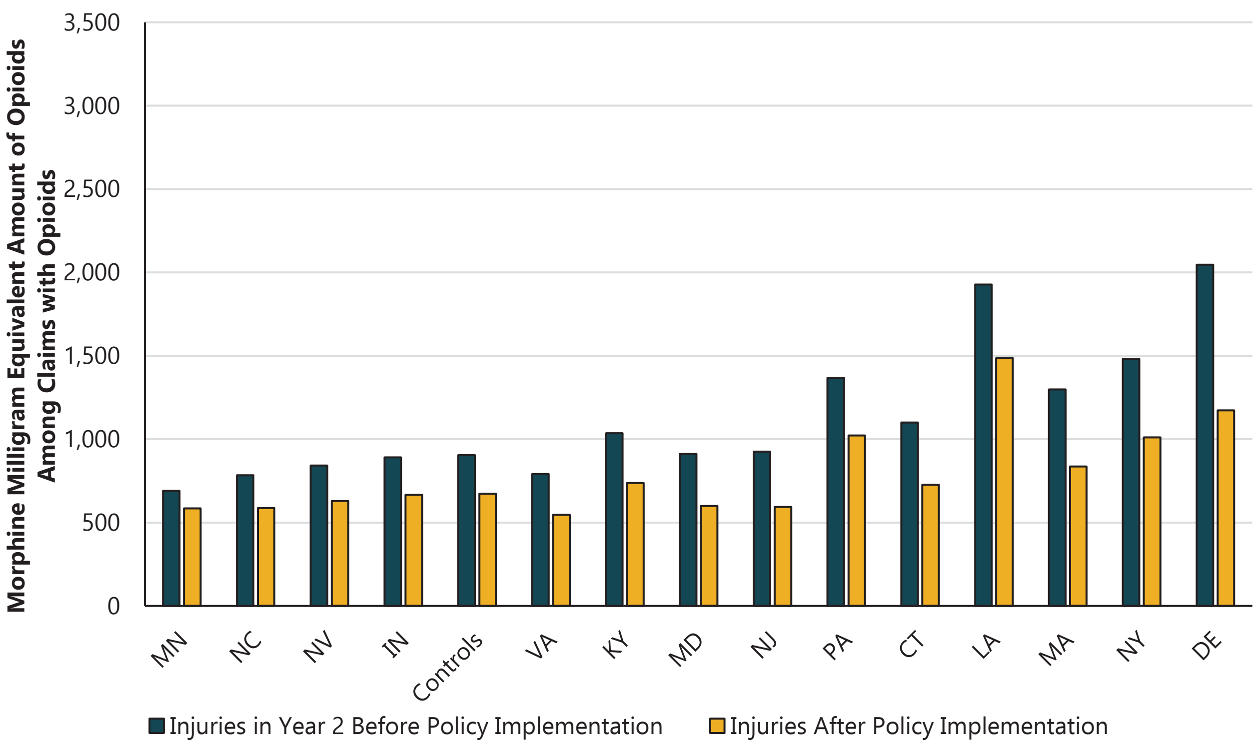
Key: PDMPs: must-access prescription drug monitoring programs.

Figure A.4 MME (claims with opioids)

A. PDMPs



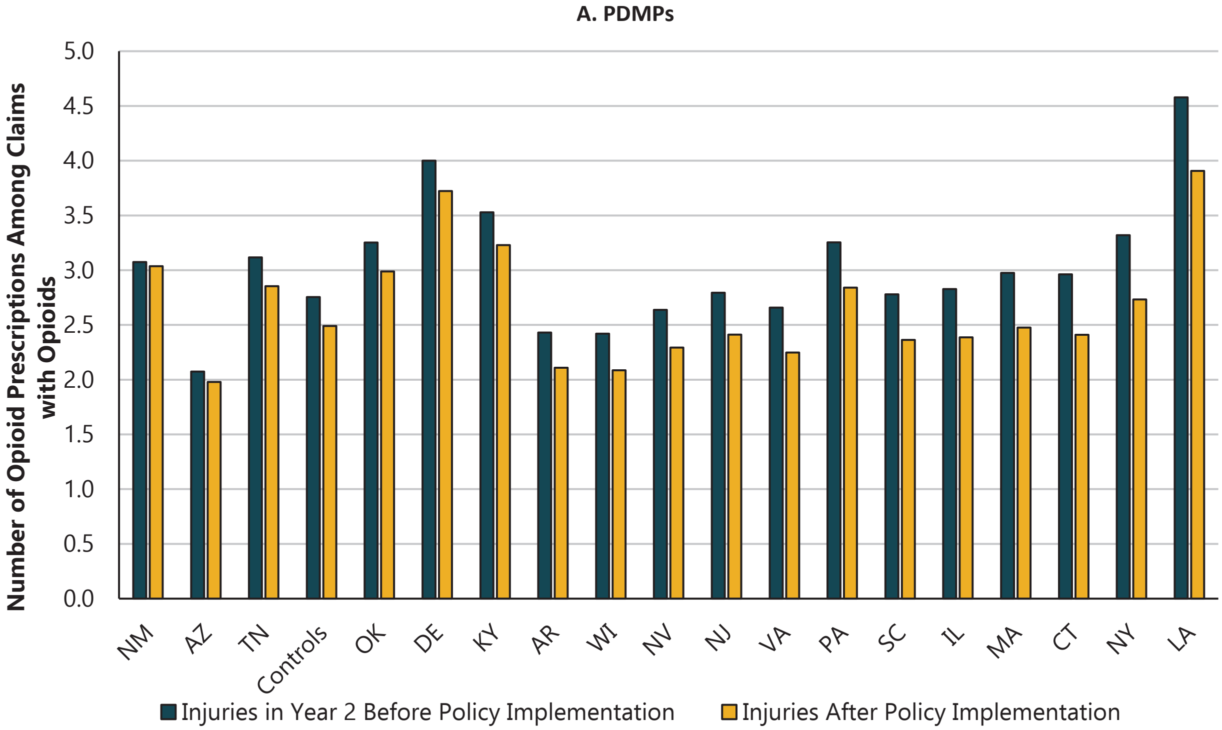
B. Initial Limits on Opioid Prescriptions

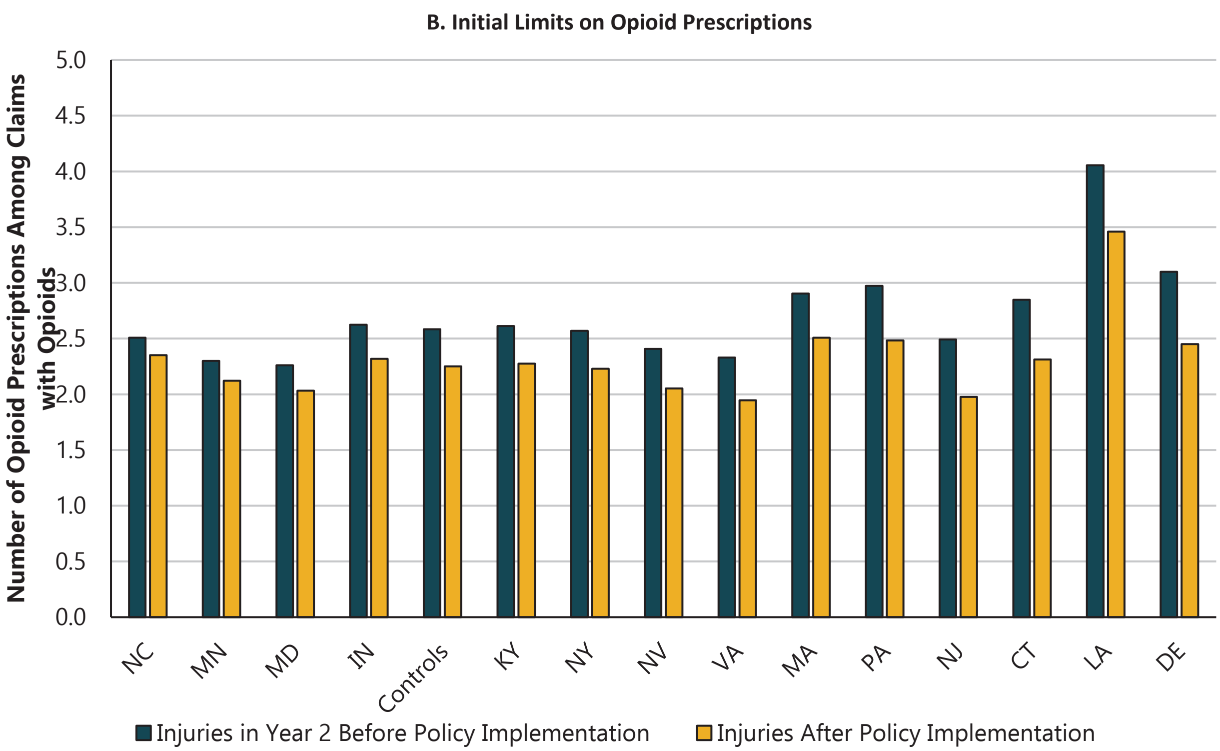


Notes: Average for claims at 12 months of maturity. We show measures for injuries that occurred in months 12 to 24 before policy implementation and for injuries that occurred within 12 months after policy implementation. For the control group, we use the median policy implementation month in the sample. States are sorted by change in measure from before to after policy implementation.

Key: MME: morphine milligram equivalent amount of opioids; PDMPs: must-access prescription drug monitoring programs.

Figure A.5 Number of Opioid Prescriptions (claims with opioids)

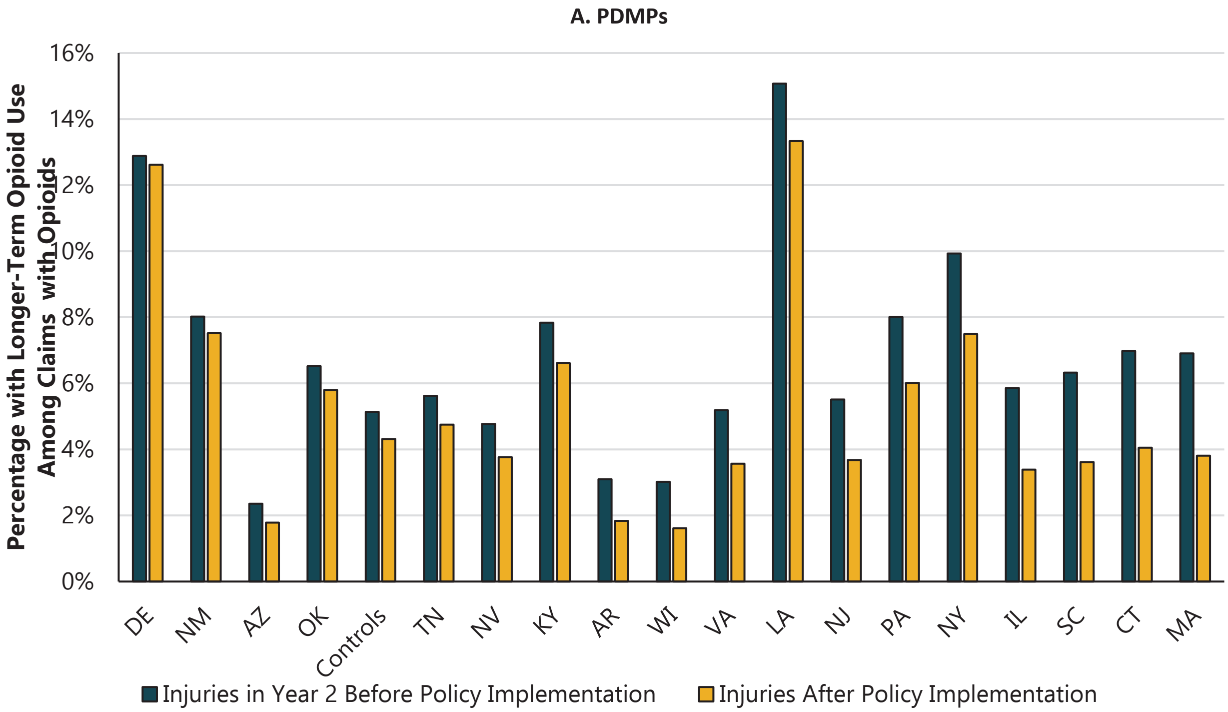


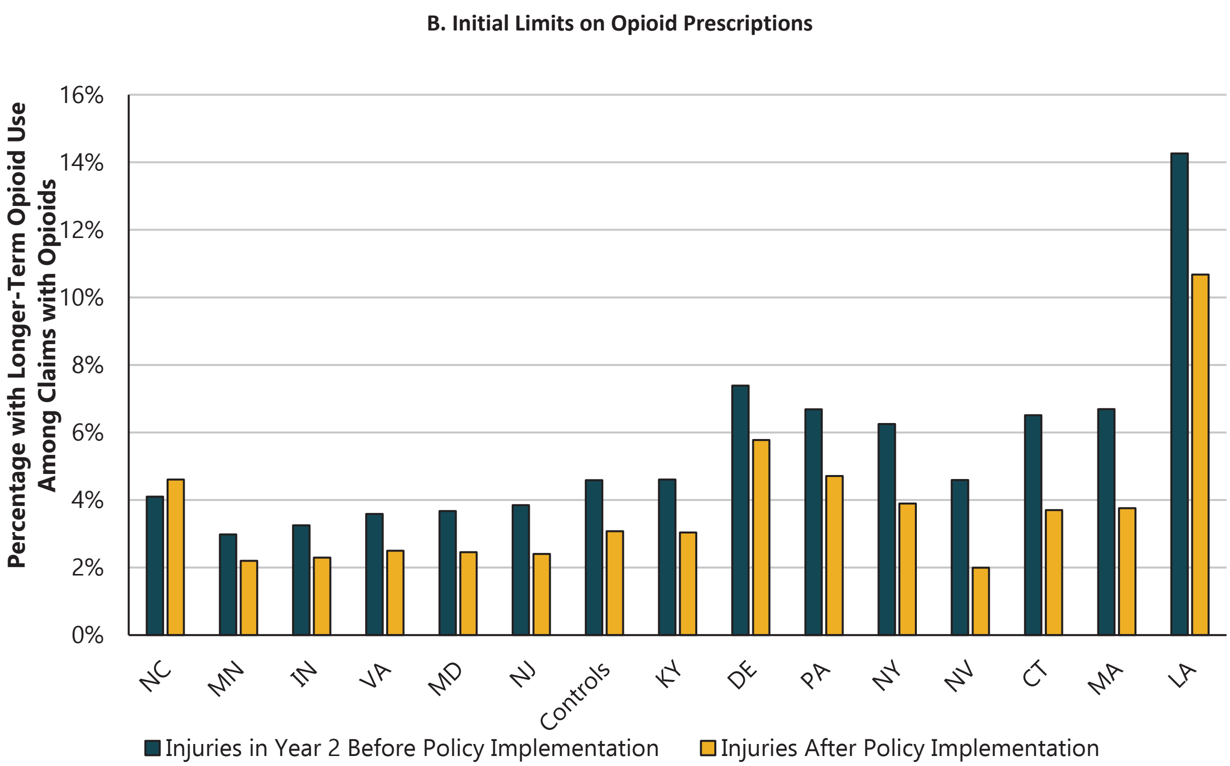


Notes: Average for claims at 12 months of maturity. We show measures for injuries that occurred in months 12 to 24 before policy implementation and for injuries that occurred within 12 months after policy implementation. For the control group, we use the median policy implementation month in the sample. States are sorted by change in measure from before to after policy implementation.

Key: PDMPs: must-access prescription drug monitoring programs.

Figure A.6 Longer-Term Opioid Prescribing (claims with opioids)

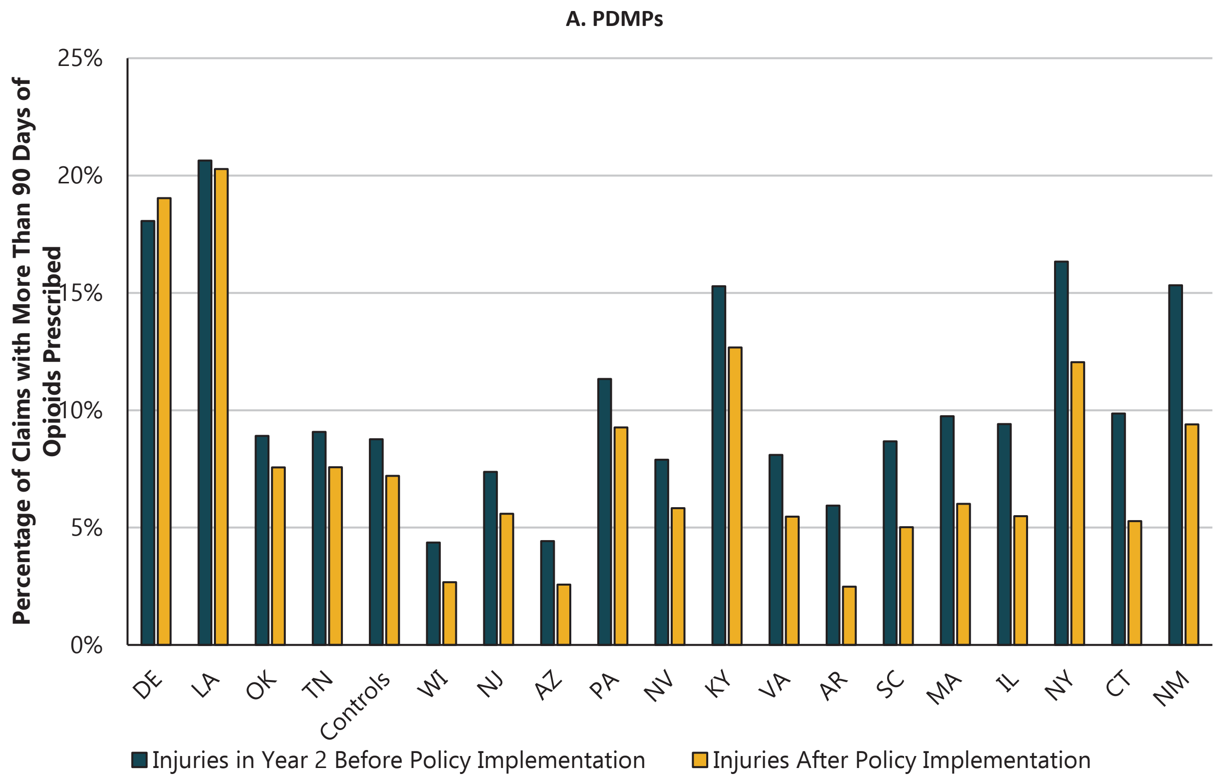


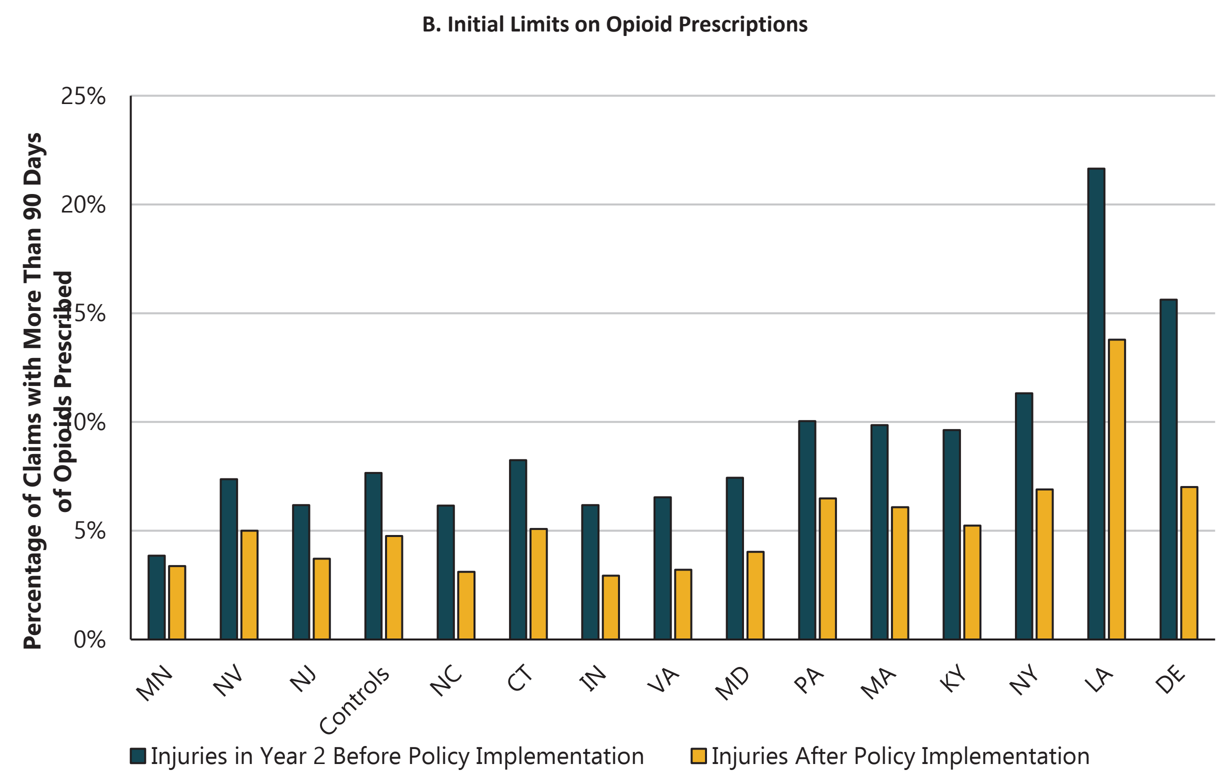


Notes: Average for claims at 12 months of maturity. We show measures for injuries that occurred in months 12 to 24 before policy implementation and for injuries that occurred within 12 months after policy implementation. For the control group, we use the median policy implementation month in the sample. States are sorted by change in measure from before to after policy implementation. Longer-term opioid prescriptions are defined as having prescriptions within the first three months after an injury and three or more filled opioid prescriptions between the 7th and 12th months after an injury.

Key: PDMPs: must-access prescription drug monitoring programs.

Figure A.7 More Than 90 Days of Opioids Prescribed (claims with opioids)

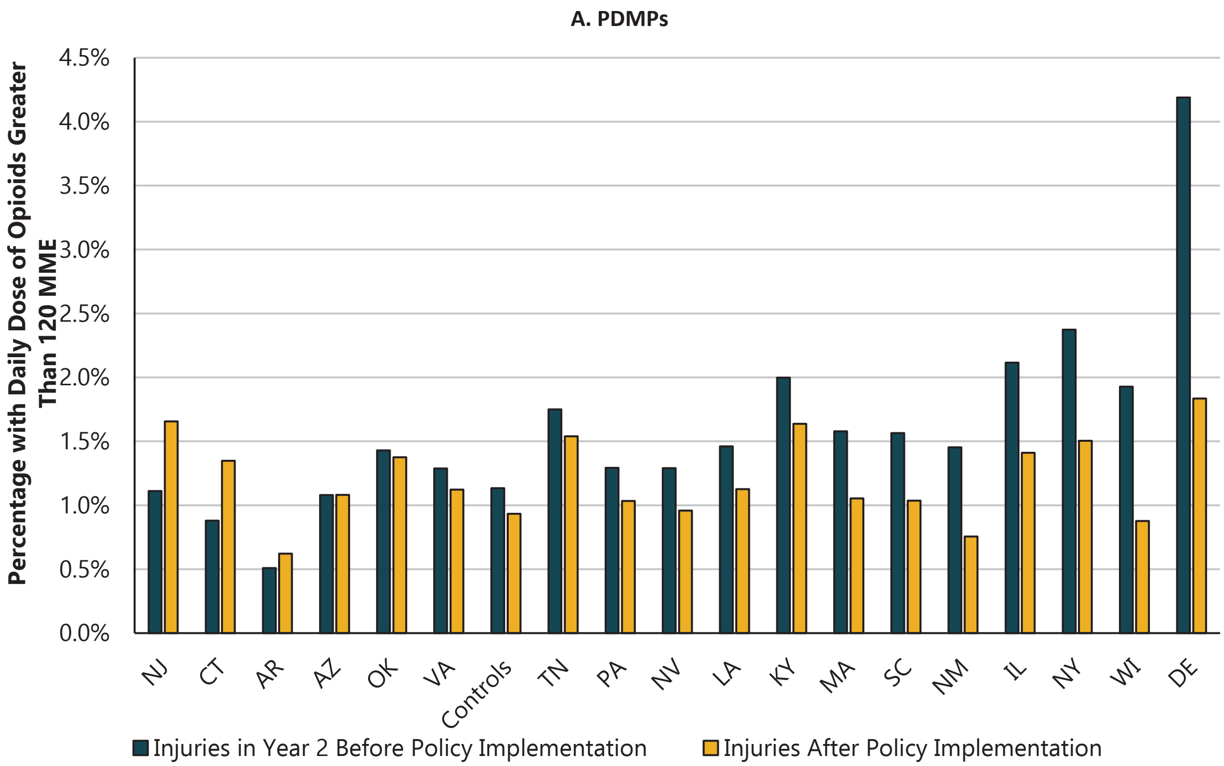


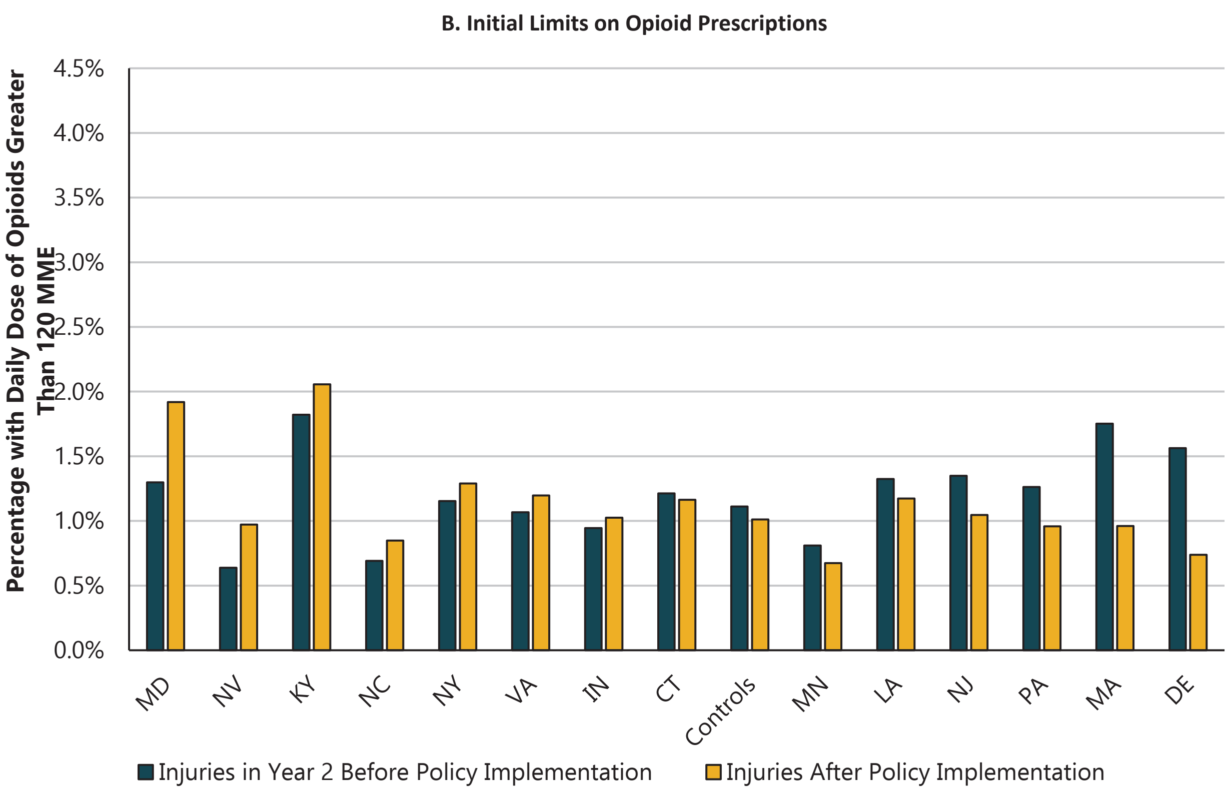


Notes: Average for claims at 12 months of maturity. We show measures for injuries that occurred in months 12 to 24 before policy implementation and for injuries that occurred within 12 months after policy implementation. For the control group, we use the median policy implementation month in the sample. States are sorted by change in measure from before to after policy implementation.

Key: PDMPs: must-access prescription drug monitoring programs.

Figure A.8 Daily Doses Greater Than 120 mg (claims with opioids)

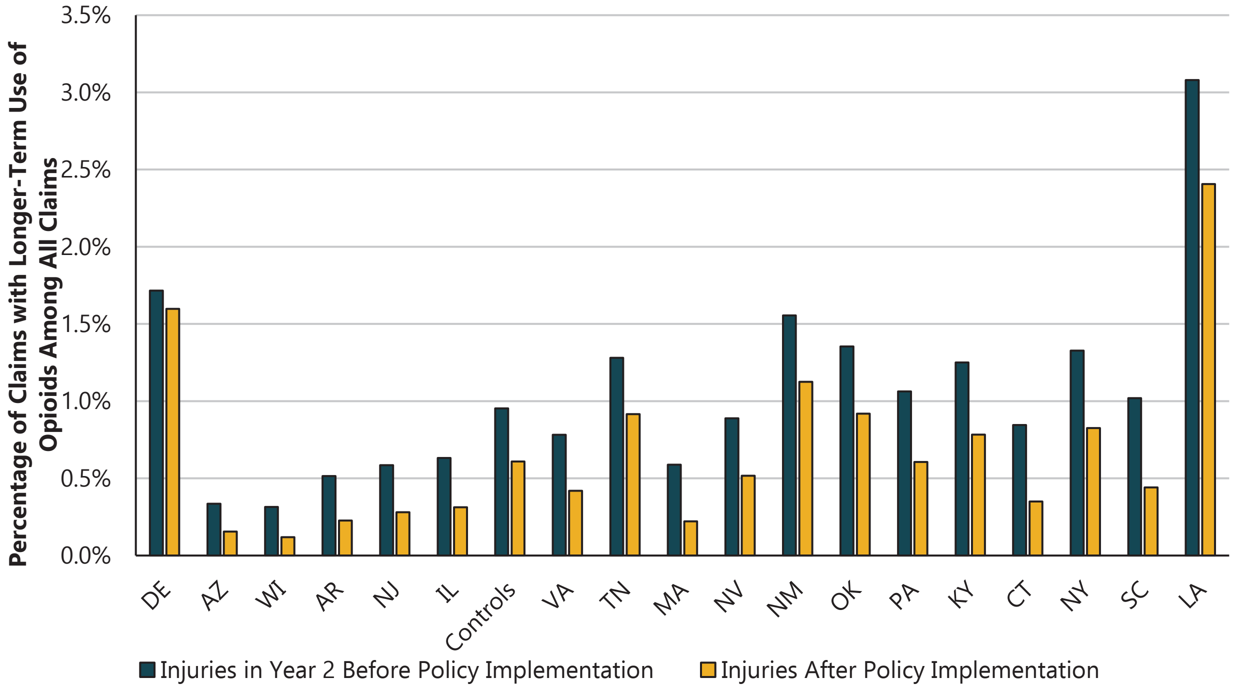




Notes: Average for claims at 12 months of maturity. We show measures for injuries that occurred in months 12 to 24 before policy implementation and for injuries that occurred within 12 months after policy implementation. For the control group, we use the median policy implementation month in the sample. States are sorted by change in measure from before to after policy implementation.

Key: MME: morphine milligram equivalent amount of opioids; PDMPs: must-access prescription drug monitoring programs.

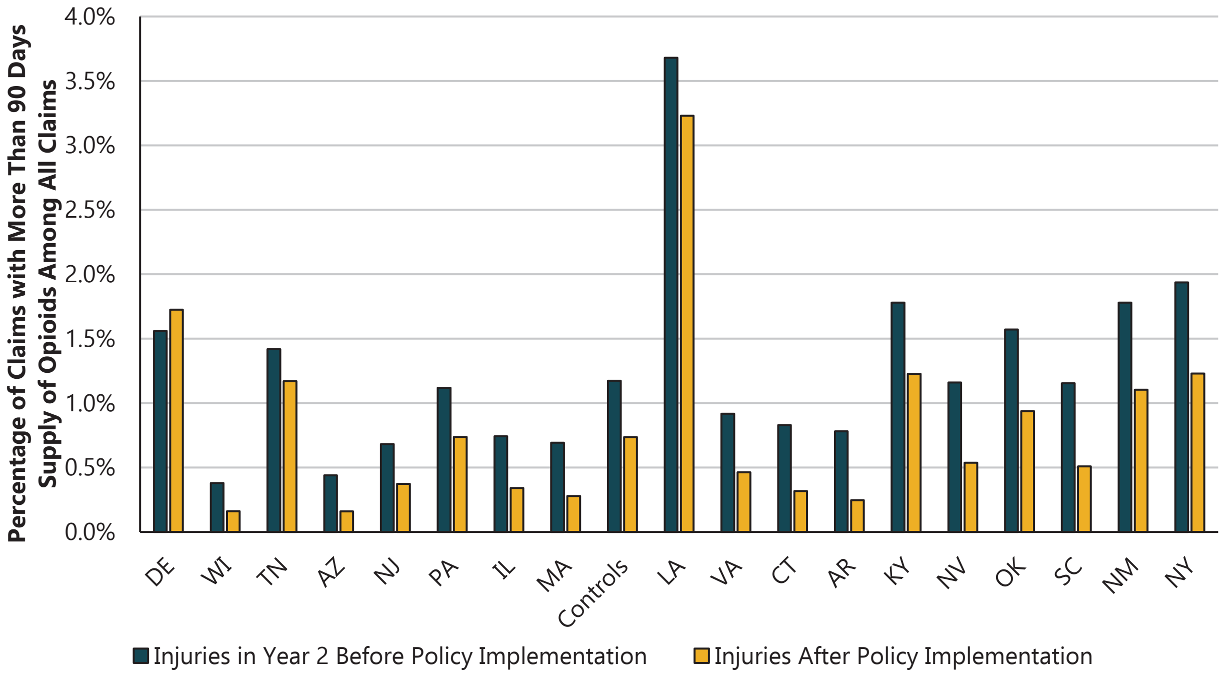
Figure A.9 Longer-Term Opioid Prescribing (all claims), PDMPs



Notes: Average for claims at 12 months of maturity. We show measures for injuries that occurred in months 12 to 24 before policy implementation and for injuries that occurred within 12 months after policy implementation. For control group, we use the median policy implementation month in the sample. States are sorted by change in measure from before to after policy implementation. Longer-term opioid prescriptions are defined as having prescriptions within the first three months after an injury and three or more filled opioid prescriptions between the 7th and 12th months after an injury.

Key: PDMPs: must-access prescription drug monitoring programs.

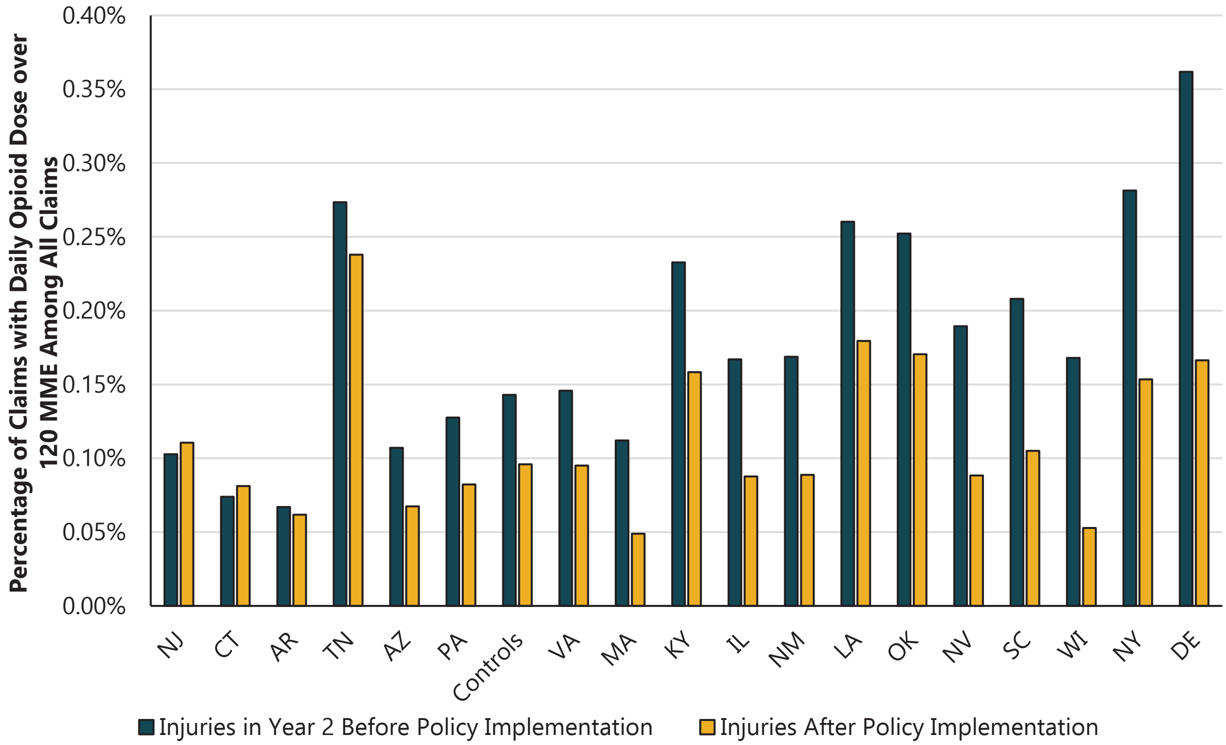
Figure A.10 More Than 90 Days of Opioids Prescribed (all claims), PDMPs



Notes: Average for claims at 12 months of maturity. We show measures for injuries that occurred in months 12 to 24 before policy implementation and for injuries that occurred within 12 months after policy implementation. For the control group, we use the median policy implementation month in the sample. States are sorted by change in measure from before to after policy implementation.

Key: PDMPs: must-access prescription drug monitoring programs.

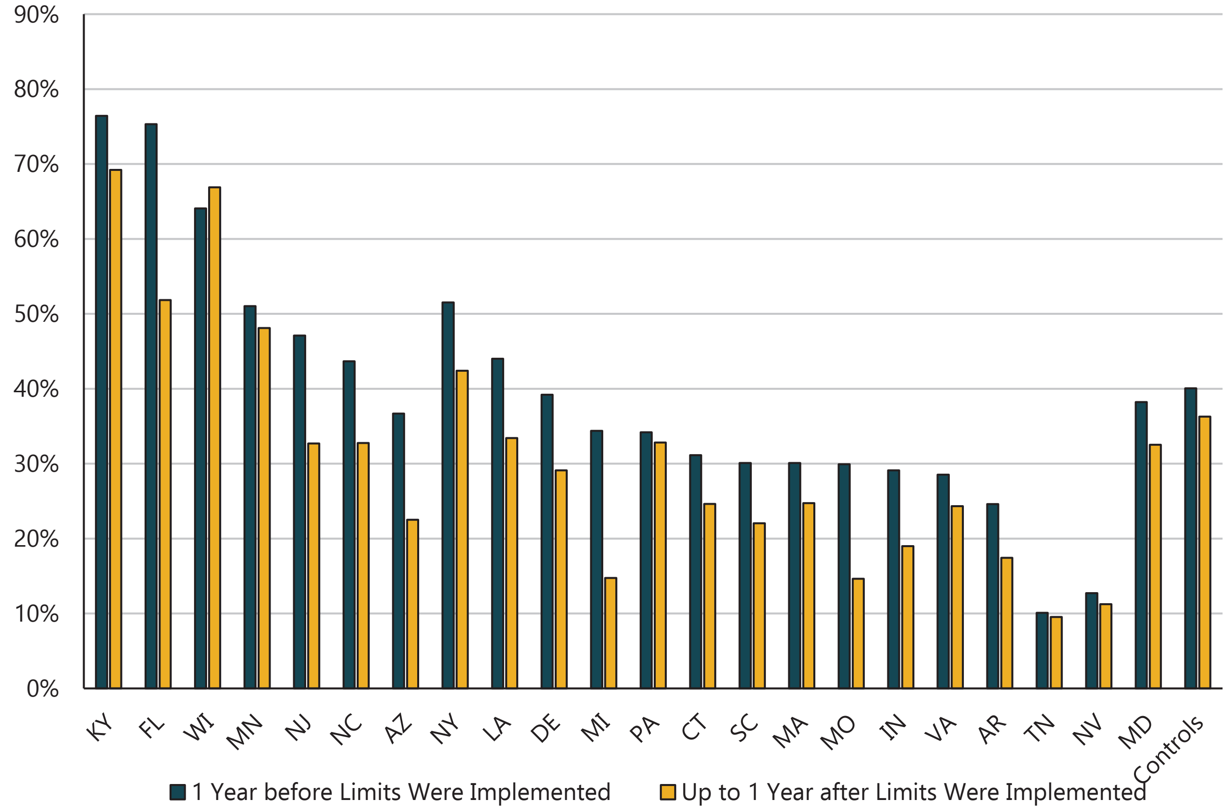
Figure A.11 Daily Doses Greater Than 120 mg (all claims), PDMPs



Notes: Average for claims at 12 months of maturity. We show measures for injuries that occurred in months 12 to 24 before policy implementation and for injuries that occurred within 12 months after policy implementation. For the control group, we use the median policy implementation month in the sample. States are sorted by change in measure from before to after policy implementation.

Key: MME: morphine milligram equivalent amount of opioids; PDMPs: must-access prescription drug monitoring programs.

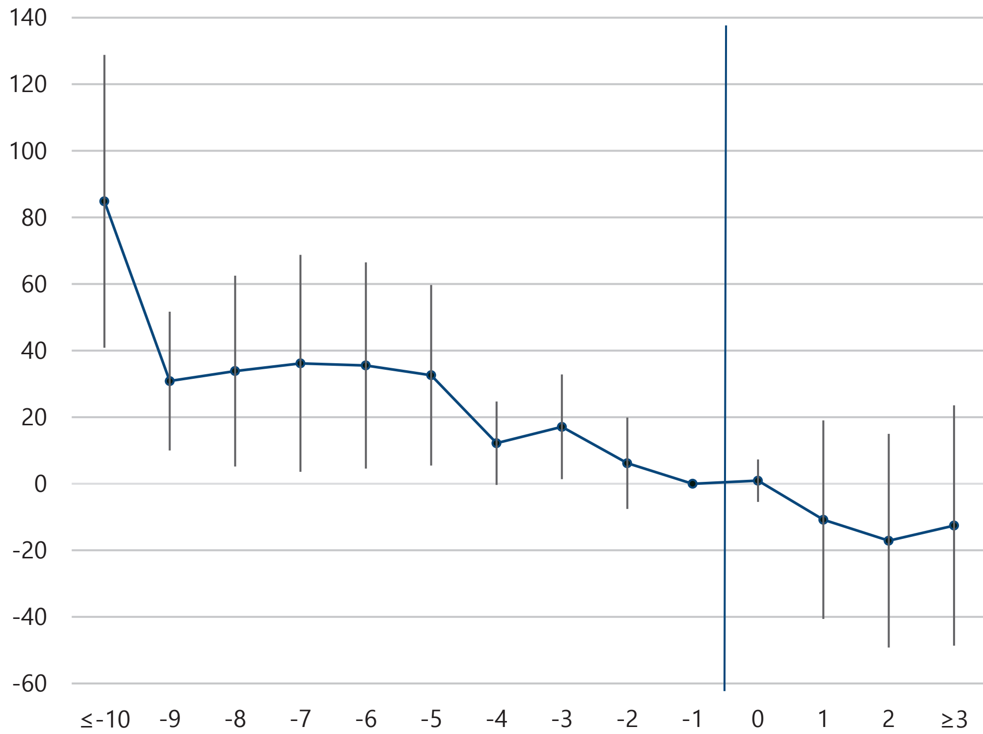
Figure A.12 Percentage of Initial Opioid Prescriptions Above Days of Supply Limit, by State



State	KY	FL	WI	MN	NJ	NC	AZ	NY	LA	DE	MI	PA	CT	SC	MA	MO	IN	VA	AR	TN	NV	MD	Controls
Limits on initial supply	3	3	3	4	5	5	5	7	7	7	7	7	7	7	7	7	7	7	7	10	14	N/A	N/A

Note: Sample includes initial opioid prescriptions filled one year before and up to one year after the limits on initial opioid prescriptions were implemented. In eight states (Arizona, Arkansas, Florida, Michigan, Missouri, South Carolina, Tennessee, and Wisconsin), we have less than one year of post-implementation data since the limits on initial opioid prescriptions were implemented after April 1, 2018. In Maryland, the law requires the lowest effective dose, without limiting the number of days supplied; for this state, we show the percentage of claims with more than seven days of supply of opioids. The states are ranked based on the days of supply limits for initial prescriptions. For the control state, we show the percentage of initial prescriptions with more than seven days of supply within one year before and after July 1, 2017.
Key: N/A: not applicable.

Figure A.13 Event-Study Regression for Drug Formularies for MME for All Claims



Notes: Average for claims at 12 months of maturity. We report estimates on quarterly leads and lags from date of implementation. The vertical lines indicate the beginning of the partial policy impact (quarter 0) and then the period when full impact begins (quarter 3). Ninety-five percent confidence intervals are shown as the vertical lines through the plotted points.

Key: MME: morphine milligram equivalent amount of opioids.

Table A.1 Effective Dates of Treatment Guidelines, Pain Treatment Guidelines, and Drug Formularies

State	Treatment Guidelines			Treatment Guides and/or Utilization Review for Opioid Prescriptions or Chronic Pain		Drug Formularies	
	Any Guideline?	Is It Mandatory?	Effective Date	Any Guideline?	Effective Date	Any Formulary?	Effective Date
Alabama	No	No		UR		No	
Arkansas	No	No		No		Yes	Jul 1, 2018
Arizona	Yes	Yes	Oct 1, 2018	Yes	Oct 1, 2016	Yes	Oct 1, 2018
California	Yes	Yes	Dec 1, 2004	Yes	Jul 18, 2009	Yes	Jan 1, 2018
Connecticut	Yes	No	Jan 1, 1996	Yes, no mandatory UR	Jul 1, 2012	No	
Delaware	Yes	No	May 23, 2008	Yes	May 23, 2008	Yes	Jan 4, 2013
Florida	Yes	Yes	Jan 1, 2003	No		No	
Georgia	No	No		No		No	
Hawaii	No	No		No		No	
Illinois	No	No				No	
Indiana	No	No		No		Yes	Jan 1, 2019
Iowa	No	No		No		No	
Kansas	Yes	No	Jan 1, 2008	Yes	Jan 1, 2008	No	
Kentucky	Yes	Yes	1996	No		Yes	Jul 1, 2019
Louisiana	Yes	Yes	Jul 13, 2011	Yes	Jul 13, 2011	No	
Maryland	No	No		No		No	
Massachusetts	Yes	No	1993	Yes	Jun 2016	No	
Michigan	No	No		No		No	
Minnesota	Yes	Yes	Jun 11, 2008	Yes	Jul 16, 2015	No	
Mississippi	No	No		Yes	Jun 14, 2017	No	
Missouri	No	No		No		No	
Nevada				Yes	1998	Yes, not mandatory	Jan 1, 2015
New Jersey	No	No		No		No	
New Mexico	Yes	No	Jul 1, 2013	No		No	
New York	Yes	Yes	Dec 1, 2010	Yes	Dec 15, 2014	Yes	Dec 5, 2019
North Carolina	No	No		Yes	May 1, 2018	No	
Oklahoma	Yes	No	Mar 1, 2012	Yes	Mar 1, 2012	Yes	Feb 1, 2014
Pennsylvania	No	No		Yes, not mandatory	Jul 16, 2018	No	
South Carolina	No	No		No		No	
Tennessee	Yes	Yes	Jan 1, 2016	Yes	Feb 28, 2016	Yes, but optional	Feb 28, 2016
Texas	Yes	Yes	May 1, 2007	Yes	May 1, 2007	Yes	Sep 1, 2011
Virginia	No	No		No		No	
Wisconsin	No	No		No		No	

Note: Source of guidelines information is Wang et al. (2019, Chapter 4) and Rothkin (2018, Tables 9 and 10).
Key: UR: utilization review.

Table A.2 Alternative Specifications for Our Preferred Estimates in Table 2

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Alternative Specifications	**Main Specification**	**Partial Policy Exposure Based on Number of Prescriptions**	**Specification Using 6 Months of Maturity Data**	**Specification Excluding California**	**Specification Excluding CA without Population Weights**	**Specification without Population Weights**	**Specification with Controls for Other Policies**
A. MME for all claims							
Must-access PDMP	-18.9324*	-19.9325*	-6.7541	-26.4308***	-27.6001***	-25.4814***	-18.7138**
	(9.6042)	(9.8383)	(4.9222)	(8.3879)	(7.3012)	(7.7279)	(8.5603)
Observations	1,118	1,118	1,184	1,084	1,084	1,118	1,118
Mean of outcome variable	167.5	167.5	101.6	166.7	170.9	171.0	167.5
% effect for PDMPs	-11%	-12%	-7%	-16%	-16%	-15%	-11%
B. Any opioids for all claims							
Must-access PDMP	0.0102	0.0103	0.0096	0.0022	-0.0026	-0.0006	0.0040
	(0.0076)	(0.0076)	(0.0069)	(0.0058)	(0.0054)	(0.0058)	(0.0064)
Observations	1,118	1,118	1,184	1,084	1,084	1,118	1,118
Mean of outcome variable	0.2	0.2	0.1	0.2	0.2	0.2	0.2
% effect for PDMPs	6%	6%	7%	1%	-2%	0%	2%
C. Number of opioid prescriptions among all claims							
Must-access PDMP	0.0107	0.0106	0.0130	-0.0147	-0.0260	-0.0194	-0.0092
	(0.0239)	(0.0237)	(0.0164)	(0.0172)	(0.0172)	(0.0189)	(0.0206)
Observations	1,118	1,118	1,184	1,084	1,084	1,118	1,118
Mean of outcome variable	0.5	0.5	0.3	0.4	0.5	0.5	0.5
% effect for PDMPs	2%	2%	4%	-3%	-6%	-4%	-2%
D. MME for claims with opioids							
Must-access PDMP	-131.1257***	-139.4607***	-61.6130**	-136.2057***	-120.7370***	-117.8070***	-122.0477***
	(47.3257)	(50.0434)	(23.0927)	(48.3782)	(37.4161)	(37.7056)	(39.6998)
Limits on initial opioid Rx	-203.1554***	-205.5581***	-99.8518***	-228.0317***	-180.1472***	-171.9636***	-186.3750***
	(67.9767)	(71.6092)	(23.1877)	(63.2968)	(59.4812)	(58.8492)	(47.4234)
Observations	1,118	1,118	1,184	1,084	1,084	1,118	1,118
Mean of outcome variable	1069.0	1069.0	714.1	1099.1	1106.4	1099.3	1069.0
% effect for PDMPs	-12%	-13%	-9%	-12%	-11%	-11%	-11%
% effect for initial limits	-19%	-19%	-14%	-21%	-16%	-16%	-17%
E. Number of opioid prescriptions among claims with opioids							
Must-access PDMP	-0.1531***	-0.1579***	-0.0936***	-0.1663***	-0.1315***	-0.1262***	-0.1461***
	(0.0361)	(0.0356)	(0.0181)	(0.0338)	(0.0280)	(0.0292)	(0.0338)
Limits on initial opioid Rx	-0.0555	-0.0512	-0.0242	-0.1131***	-0.0941**	-0.0759*	-0.0573
	(0.0556)	(0.0547)	(0.0245)	(0.0393)	(0.0427)	(0.0444)	(0.0551)
Observations	1,118	1,118	1,184	1,084	1,084	1,118	1,118
Mean of outcome variable	2.8	2.8	2.3	2.9	2.9	2.8	2.8
% effect for PDMPs	-5%	-6%	-4%	-6%	-5%	-4%	-5%
% effect for initial limits	-2%	-2%	-1%	-4%	-3%	-3%	-2%

Notes: Estimates from state-level regressions for claims at 12 months of maturity (except column (3)), for injuries occurring October 2009–March 2018. Samples includes all claims, claims with opioids, or claims with more than seven days of lost work time (as indicated) aggregated to quarter of injury and state. Each observation is weighted by the number of claims represented. Controls are included for industry composition, average county-level unemployment rate, average county-level median household income, average county-level percentage disabled, and average county-level percentage without health insurance. We also control for state and quarter fixed effects.

*, **, *** Statistically significant at the 10 percent, 5 percent, and 1 percent level, respectively. Standard errors are clustered by state. To account for partial exposure for injuries for which the policy changed during the 12 months the data capture, we define the variable denoting the policy change as 1 if it was in effect for all 12 months of the postinjury exposure, 0.75 if it was in effect for 3 of the 4 quarters of postinjury exposure, 0.5 if it was in effect for 2 of the 4 quarters of postinjury exposure, and 0.25 if it was in effect for the last quarter of the 4 quarters of postinjury exposure. (The definition is different in column (2), as explained in the text.)

Key: MME: morphine milligram equivalent amount of opioids; PDMP: must-access prescription drug monitoring program; Rx: prescription.

Table A.3 Alternative Specifications for Estimates in Table 4

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Alternative Specifications	**Main Specification**	**Partial Policy Exposure Based on Number of Prescriptions**	**Specification Using 6 Months of Maturity Data**	**Specification Excluding California**	**Specification Excluding CA without Population Weights**	**Specification without Population Weights**	**Specification with Controls for Other Policies**
A. Longer-term opioid prescribing among all claims							
Must-access PDMP	-0.0001	-0.0001		-0.0010**	-0.0010**	-0.0008	-0.0007
	(0.0007)	(0.0007)		(0.0004)	(0.0004)	(0.0005)	(0.0006)
Observations	1,118	1,118		1,084	1,084	1,118	1,118
Mean of outcome variable	0.0097	0.0097		0.0092	0.0094	0.0095	0.0097
% effect for PDMPs	-1%	-1%		-11%	-11%	-8%	-7%
B. More than 90 days of opioids prescribed among all claims							
Must-access PDMP	-0.0002	-0.0002	-0.0000	-0.0009	-0.0010	-0.0008	-0.0006
	(0.0009)	(0.0009)	(0.0005)	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Observations	1,118	1,118	1,184	1,084	1,084	1,118	1,118
Mean of outcome variable	0.0111	0.0111	0.0058	0.0107	0.0108	0.0108	0.0111
% effect for PDMPs	-2%	-2%	0%	-8%	-9%	-7%	-5%
C. Daily dose greater than 120 mg among all claims							
Must-access PDMP	-0.0002	-0.0002	-0.0000	-0.0002	-0.0001	-0.0001	-0.0002
	(0.0002)	(0.0002)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Observations	1,118	1,118	1,184	1,084	1,084	1,118	1,118
Mean of outcome variable	0.0017	0.0017	0.0016	0.0018	0.0018	0.0018	0.0017
% effect for PDMPs	-12%	-12%	0%	-11%	-6%	-6%	-12%
D. Longer-term opioid prescribing among claims with opioids							
Must-access PDMP	-0.0070***	-0.0070***		-0.0080***	-0.0063***	-0.0058***	-0.0070***
	(0.0021)	(0.0021)		(0.0020)	(0.0020)	(0.0020)	(0.0020)
Limits on initial opioid Rx	-0.0009	-0.0010		-0.0055*	-0.0046*	-0.0029	-0.0006
	(0.0047)	(0.0049)		(0.0031)	(0.0026)	(0.0031)	(0.0043)
Observations	1,118	1,118		1,084	1,084	1,118	1,118
Mean of outcome variable	0.0601	0.0601		0.0590	0.0575	0.0575	0.0601
% effect for PDMPs	-12%	-12%		-14%	-11%	-10%	-12%
% effect for initial limits	-1%	-2%		-9%	-8%	-5%	-1%
E. More than 90 days of opioids prescribed among claims with opioids							
Must-access PDMP	-0.0046	-0.0046	-0.0032	-0.0074	-0.0033	-0.0021	-0.0048
	(0.0046)	(0.0046)	(0.0029)	(0.0044)	(0.0050)	(0.0051)	(0.0043)
Limits on initial opioid Rx	0.0005	0.0000	0.0001	-0.0089**	-0.0109*	-0.0074	0.0007
	(0.0076)	(0.0078)	(0.0029)	(0.0039)	(0.0054)	(0.0062)	(0.0071)
Observations	1,118	1,118	1,184	1,084	1,084	1,118	1,118
Mean of outcome variable	0.1011	0.1011	0.0587	0.0939	0.0912	0.0921	0.1011
% effect for PDMPs	-5%	-5%	-5%	-8%	-4%	-2%	-5%
% effect for initial limits	0%	0%	0%	-9%	-12%	-8%	1%
F. Daily dose greater than 120 mg among claims with opioids							
Must-access PDMP	-0.0006	-0.0007	0.0007	-0.0006	-0.0002	-0.0002	-0.0010
	(0.0012)	(0.0012)	(0.0011)	(0.0013)	(0.0009)	(0.0009)	(0.0012)
Limits on initial opioid Rx	-0.0005	-0.0004	-0.0014	-0.0007	0.0008	0.0008	-0.0004
	(0.0012)	(0.0013)	(0.0011)	(0.0013)	(0.0019)	(0.0019)	(0.0011)
Observations	1,118	1,118	1,184	1,084	1,084	1,118	1,118
Mean of outcome variable	0.0159	0.0159	0.0162	0.0159	0.0153	0.0153	0.0159
% effect for PDMPs	-4%	-4%	4%	-4%	-1%	-1%	-6%
% effect for initial limits	-3%	-3%	-9%	-4%	5%	5%	-3%

Notes: Estimates from state-level regressions for claims at 12 months of maturity (except column (3)), for injuries occurring October 2009–March 2018. Samples include all claims, claims with opioids, or claims with more than seven days of lost work time (as indicated) aggregated to quarter of injury and state. Each observation is weighted by the number of claims represented. Longer-term opioid prescriptions are defined as having prescriptions within the first three months after an injury and three or more filled opioid prescriptions between the 7th and 12th months after an injury. Controls are included for industry composition, average county-level unemployment rate, average county-level median household income, average county-level percentage disabled, and average county-level percentage without health insurance. We also control for state and quarter fixed effects. *, **, *** Statistically significant at the 10 percent, 5 percent, and 1 percent level, respectively. Standard errors are clustered by state. To account for partial exposure for injuries for which the policy changed during the 12 months the data capture, we define the variable denoting the policy change as 1 if it was in effect for all 12 months of the postinjury exposure, 0.75 if it was in effect for 3 of the 4 quarters of postinjury exposure, 0.5 if it was in effect for 2 of the 4 quarters of postinjury exposure, and 0.25 if it was in effect for the last quarter of the 4 quarters of postinjury exposure. (The definition is different in column (2), as explained in the text.)
Key: PDMP: must-access prescription drug monitoring program; Rx: prescription.

Table A.4 Alternative Specifications for Estimates in Table 7

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Alternative Specifications	**Main Specification**	**Partial Policy Exposure Based on Number of Prescriptions**	**Specification Using 6 Months of Maturity Data**	**Specification Excluding California**	**Specification Excluding CA without Population Weights**	**Specification without Population Weights**	**Specification with Controls for Other Policies**
A. Weeks of temporary disability payments among all claims							
Must-access PDMP	0.0622	0.0676	0.0353	0.0436	0.0091	0.0148	0.0114
	(0.0638)	(0.0646)	(0.0325)	(0.0628)	(0.0718)	(0.0714)	(0.0509)
Observations	1,118	1,118	1,184	1,084	1,084	1,118	1,118
Mean of outcome variable	2.8300	2.8300	1.8349	2.6805	2.6395	2.6708	2.8300
% effect for PDMPs	2%	2%	2%	2%	0%	1%	0%
B. Weeks of temporary disability payments among claims with more than 7 days of lost time							
Must-access PDMP	0.1455	0.1673	0.0794	0.0748	0.0491	0.0676	0.0359
	(0.1605)	(0.1615)	(0.0721)	(0.1633)	(0.1653)	(0.1625)	(0.1611)
Observations	1,118	1,118	1,184	1,084	1,084	1,118	1,118
Mean of outcome variable	13.3415	13.3415	9.1871	12.8909	12.6983	12.7857	13.3415
% effect for PDMPs	1%	1%	1%	1%	0%	1%	0%
C. Claims with more than 7 days of lost work time among all claims							
Must-access PDMP	0.0013	0.0014	0.0014	0.0011	-0.0015	-0.0014	-0.0009
	(0.0029)	(0.0030)	(0.0026)	(0.0030)	(0.0033)	(0.0033)	(0.0022)
Observations	1,118	1,118	1,184	1,084	1,084	1,118	1,118
Mean of outcome variable	0.2115	0.2115	0.1988	0.2075	0.2037	0.2046	0.2115
% effect for PDMPs	1%	1%	1%	1%	-1%	-1%	0%

Notes: Estimates from state-level regressions for claims at 12 months of maturity (except in column (3)), for injuries occurring October 2009–March 2018. Samples include all claims, claims with opioids, or claims with more than seven days of lost work time (as indicated) aggregated to quarter of injury and state. Each observation is weighted by the number of claims represented. Controls are included for industry composition, average county-level unemployment rate, average county-level median household income, average county-level percentage disabled, and average county-level percentage without health insurance. We also control for state and quarter fixed effects. *, **, *** Statistically significant at the 10 percent, 5 percent, and 1 percent level, respectively. Standard errors are clustered by state. To account for partial exposure for injuries for which the policy changed during the 12 months the data capture, we define the variable denoting the policy change as 1 if it was in effect for all 12 months of the postinjury exposure, 0.75 if it was in effect for 3 of the 4 quarters of postinjury exposure, 0.5 if it was in effect for 2 of the 4 quarters of postinjury exposure, and 0.25 if it was in effect for the last quarter of the 4 quarters of postinjury exposure. (The definition is different in column (2), as explained in the text.)

Key: PDMP: must-access prescription drug monitoring program.

Table A.5 Specifications with Additional Controls for Preferred Specifications in Table 2

	(1)	(2)	(3)	(4)	(5)
Outcome	**MME**	**Any Opioids**	**Number of Opioid Prescriptions**	**MME**	**Number of Opioid Prescriptions**
Sample	**All Claims**	**All Claims**	**All Claims**	**Claims with Opioids**	**Claims with Opioids**
A. Specifications with controls for other policies					
Must-access PDMP	-17.6218*	0.0091	0.0075	-122.0477***	-0.1461***
	(8.7960)	(0.0074)	(0.0229)	(39.6998)	(0.0338)
Limits on initial opioid Rx				-186.3750***	-0.0573
				(47.4234)	(0.0551)
Opioid treatment guidelines	-12.2263	0.0114	0.0337	-110.8029	-0.0306
	(18.1062)	(0.0094)	(0.0334)	(92.2759)	(0.0707)
Treatment guidelines	1.2952	-0.0109*	-0.0377*	49.6674	-0.0204
	(12.4744)	(0.0060)	(0.0208)	(61.9138)	(0.0653)
Drug formulary	-31.6755**	-0.0094	-0.0566	-63.3542**	-0.0745
	(15.4570)	(0.0154)	(0.0552)	(28.2738)	(0.0496)
Observations	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	167.48	0.161	0.460	1,069.00	2.85
B. Effects of must-access PDMPs by year of implementation					
Must-access PDMPs implemented in 2012–2013	-61.6351***	-0.0075	-0.0439	-274.7042*	-0.2243***
	(15.9740)	(0.0083)	(0.0263)	(138.8510)	(0.0726)
Must-access PDMPs implemented in 2014–2015	-5.4561	0.0175**	0.0261	-37.9700	-0.1257**
	(8.7476)	(0.0075)	(0.0260)	(48.8077)	(0.0552)
Must-access PDMPs implemented in 2016–2018	12.8660	0.0201	0.0548	-7.7418	-0.0453
	(12.0901)	(0.0122)	(0.0383)	(50.5397)	(0.0698)
Limits on initial opioid Rx				-231.3960***	-0.0638
				(73.2590)	(0.0589)
Observations	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	167.48	0.161	0.460	1,069.003	2.849
C. Effect of limits on initial opioid Rx by year of implementation					
Must-access PDMP				-124.4286**	-0.1503***
				(48.3440)	(0.0363)
Limits on initial opioid Rx implemented in 2016				-410.8691***	-0.1446**
				(137.8188)	(0.0657)
Limits on initial opioid Rx implemented in 2017–2018				-91.1106*	-0.0075
				(47.7725)	(0.0643)
Observations				1,118	1,118
Mean of outcome variable				1,069.00	2.85

Notes: Estimates from state-level regressions for claims at 12 months of maturity, for injuries occurring October 2009–March 2018. Samples include all claims or claims with opioids (as indicated) aggregated to quarter of injury and state. Each observation is weighted by the number of claims represented. Controls are included for industry composition, average county-level unemployment rate, average county-level median household income, average county-level percentage disabled, and average county-level percentage without health insurance. We also control for state and quarter fixed effects. *, **, *** Statistically significant at the 10 percent, 5 percent, and 1 percent level, respectively. Standard errors are clustered by state. To account for partial exposure for injuries for which the policy changed during the 12 months the data capture, we define the variable denoting the policy change as 1 if it was in effect for all 12 months of the postinjury exposure, 0.75 if it was in effect for 3 of the 4 quarters of postinjury exposure, 0.5 if it was in effect for 2 of the 4 quarters of postinjury exposure, and 0.25 if it was in effect for the last quarter of the 4 quarters of postinjury exposure.
Key: MME: morphine milligram equivalent amount of opioids; PDMP: must-access prescription drug monitoring program; Rx: prescription.

Table A.6 Specifications with Additional Controls for Preferred Specifications in Table 4

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome	**Longer-Term Opioid Prescribing**	**More Than 90 Days of Opioids Prescribed**	**Daily Dose Greater Than 120 mg**	**Longer-Term Opioid Prescribing**	**More Than 90 Days of Opioids Prescribed**	**Daily Dose Greater Than 120 mg**
Sample	**All Claims**	**All Claims**	**All Claims**	**Claims with Opioids**	**Claims with Opioids**	**Claims with Opioids**
A. Specifications with controls for other policies						
Must-access PDMP	-0.0002	-0.0003	-0.0002	-0.0070***	-0.0048	-0.0010
	(0.0007)	(0.0009)	(0.0002)	(0.0020)	(0.0043)	(0.0012)
Limits on initial opioid Rx				-0.0006	0.0007	-0.0004
				(0.0043)	(0.0071)	(0.0011)
Opioid treatment guidelines	0.0009	0.0006	-0.0002	-0.0021	-0.0000	-0.0020
	(0.0011)	(0.0014)	(0.0002)	(0.0045)	(0.0088)	(0.0022)
Treatment guidelines	-0.0003	-0.0013	0.0003	0.0019	0.0020	0.0054***
	(0.0009)	(0.0011)	(0.0003)	(0.0032)	(0.0062)	(0.0019)
Drug formulary	-0.0018	-0.0033***	-0.0023***	-0.0029*	0.0012	-0.0066***
	(0.0014)	(0.0011)	(0.0006)	(0.0015)	(0.0022)	(0.0014)
Observations	1,118	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	0.010	0.011	0.002	0.060	0.101	0.016
B. Effects of must-access PDMPs by year of implementation						
Must-access PDMPs implemented in 2012–2013	-0.0019***	-0.0038***	-0.0002	-0.0132***	-0.0213***	0.0001
	(0.0007)	(0.0008)	(0.0004)	(0.0045)	(0.0068)	(0.0027)
Must-access PDMPs implemented in 2014–2015	0.0003	0.0013	-0.0001	-0.0033	0.0070*	-0.0005
	(0.0007)	(0.0008)	(0.0003)	(0.0029)	(0.0038)	(0.0015)
Must-access PDMPs implemented in 2016–2018	0.0016	0.0018	-0.0003	-0.0009	0.0080	-0.0027
	(0.0014)	(0.0013)	(0.0003)	(0.0048)	(0.0087)	(0.0020)
Limits on initial opioid Rx				-0.0020	-0.0030	-0.0006
				(0.0049)	(0.0075)	(0.0014)
Observations	1,118	1,118	1,118	1,118	1,118	1,118
Mean of outcome variable	0.010	0.011	0.002	0.060	0.101	0.016
C. Effect of limits on initial opioid Rx by year of implementation						
Must-access PDMP				-0.0067***	-0.0041	-0.0006
				(0.0021)	(0.0046)	(0.0013)
Limits on initial opioid Rx implemented in 2016				-0.0118**	-0.0135	-0.0022
				(0.0052)	(0.0096)	(0.0021)
Limits on initial opioid Rx implemented in 2017–2018				0.0050	0.0080	0.0004
				(0.0044)	(0.0079)	(0.0015)
Observations				1,118	1,118	1,118
Mean of outcome variable				0.060	0.101	0.016

Notes: Estimates from state-level regressions for claims at 12 months of maturity, for injuries occurring October 2009–March 2018. Samples include all claims or claims with opioids (as indicated) aggregated to quarter of injury and state. Each observation is weighted by the number of claims represented. Longer-term opioid prescriptions are defined as having prescriptions within the first three months after an injury and three or more filled opioid prescriptions between the 7th and 12th months after an injury. Controls are included for industry composition, average county-level unemployment rate, average county-level median household income, average county-level percentage disabled, and average county-level percentage without health insurance. We also control for state and quarter fixed effects. *, **, *** Statistically significant at the 10 percent, 5 percent, and 1 percent level, respectively. Standard errors are clustered by state. To account for partial exposure for injuries for which the policy changed during the 12 months the data capture, we define the variable denoting the policy change as 1 if it was in effect for all 12 months of the postinjury exposure, 0.75 if it was in effect for 3 of the 4 quarters of postinjury exposure, 0.5 if it was in effect for 2 of the 4 quarters of postinjury exposure, and 0.25 if it was in effect for the last quarter of the 4 quarters of postinjury exposure.

Key: PDMP: must-access prescription drug monitoring program; Rx: prescription.

Table A.7 Specifications with Additional Controls for Preferred Specifications in Table 7

	(1)	(2)	(3)
Outcome	**Weeks of Temporary Disability Payments**	**Weeks of Temporary Disability Payments**	**Claims with More Than 7 Days of Lost Work Time**
Sample	**Claims with More Than 7 Days of Lost Work Time**	**All Claims**	**All Claims**
A. Specifications with controls for other policies			
Must-access PDMP	0.1351	0.0498	0.0004
	(0.1502)	(0.0519)	(0.0023)
Opioid treatment guidelines	0.0784	0.1791***	0.0114***
	(0.1382)	(0.0634)	(0.0028)
Treatment guidelines	-0.1330	-0.2518***	-0.0147***
	(0.1121)	(0.0533)	(0.0035)
Drug formulary	-0.3457	-0.3008***	-0.0166***
	(0.3543)	(0.0679)	(0.0029)
Observations	1,118	1,118	1,118
Mean of outcome variable	13.34	2.83	0.21
B. Effects of must-access PDMPs by year of implementation			
Must-access PDMPs implemented in 2012–2013	0.1223	0.0534	0.0018
	(0.2048)	(0.0847)	(0.0052)
Must access PDMPs implemented in 2014–2015	0.0769	-0.0025	-0.0024
	(0.2551)	(0.0745)	(0.0032)
Must-access PDMPs implemented in 2016–2018	0.2836	0.2010	0.0079
	(0.1959)	(0.1187)	(0.0063)
Observations	1,118	1,118	1,118
Mean of outcome variable	13.34	2.830	0.212

Notes: Estimates from state-level regressions for claims at 12 months of maturity, for injuries occurring October 2009–March 2018. Samples include all claims or claims with more than seven days of lost work time (as indicated) aggregated to quarter of injury and state. Each observation is weighted by the number of claims represented. Controls are included for industry composition, average county-level unemployment rate, average county-level median household income, average county-level percentage disabled, and average county-level percentage without health insurance. We also control for state and quarter fixed effects. *, **, *** Statistically significant at the 10 percent, 5 percent, and 1 percent level, respectively. Standard errors are clustered by state. To account for partial exposure for injuries for which the policy changed during the 12 months the data capture, we define the variable denoting the policy change as 1 if it was in effect for all 12 months of the postinjury exposure, 0.75 if it was in effect for 3 of the 4 quarters of postinjury exposure, 0.5 if it was in effect for 2 of the 4 quarters of postinjury exposure, and 0.25 if it was in effect for the last quarter of the 4 quarters of postinjury exposure.
Key: PDMP: must-access prescription drug monitoring program.