Opioids for the masses: Welfare tradeoffs in the regulation of narcotic pain medications*

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Abstract

Use of prescription opioid pain relievers to manage pain has increased fourfold since 1999, as medical guidelines have increasingly emphasized that appropriate pain management is required for an acceptable standard of care. However, a concomitant rapid rise in opioid abuse, addiction, overdose, and death has led to recent efforts to crack down on opioid prescribing. This paper sheds light on the tradeoffs of public policies that reduce the supply of medical opioids by investigating their health, labor, and welfare ramifications. I exploit state-level variation in the introduction of Prescription Monitoring Program (PMP) laws, and make use of several rich data sources, documenting that PMPs reduce the distribution of opioids, and achieve a key policy goal by reducing opioid overdose deaths by about 12%. I also find substantial costs resulting from these policies, including increased pain in the hospital setting, more missed days for injured and disabled individuals, and substitution towards more expensive medical care. A rough back-of-the-envelope welfare calculation suggests the welfare losses and gains from regulation are on the same order of magnitude - approximately $12.1 billion per year in increased costs from inpatient and outpatient medical spending plus lost wages, compared to $7.3 billion per year in benefits from lives saved from opioid and heroin overdose.

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1 Introduction

Prescription opioid pain relievers are a highly effective medical technology for the general relief of pain, but they are also notoriously addictive, and their use can lead to dependence, abuse, overdose, and death. Until the mid-1990s, prescription opioids were utilized primarily for acute pain and pain related to terminal cancer, but rarely for chronic, non-cancer pain, and fears of abuse meant that even for acute pain and cancer patients, pain was often undertreated. In the face of concerns that undertreatment of pain was a "serious public health issue," medically indicated use of these drugs over the past 15 years has increased dramatically, and attitudes have liberalized towards the use of opioids for chronic non-cancer pain.

However, in recent years, there has been a considerable backlash against the use of opioids, as a concomitant, and largely unanticipated, rise in adverse events associated with opioid pain relievers has raised alarm among the medical and public health communities (Kolodny et al., 2015).[1] As can be seen in Figures 1 and 2, a fourfold increase since 1999 in Morphine Milligram Equivalent (MME) of opioids distributed has coincided with a fourfold increase in drug overdose deaths linked to opioid pain relievers during that same time period. The 25,117 opiate-linked overdose deaths in 2013 rival the number of deaths due to automobile accidents (which caused 35,369 deaths in 2013).[2] Policymakers including the Secretary of Health and Human Services have declared the situation with prescription opioid drug abuse an 'epidemic' (US Department of Health and Human Services, 2015).

In this paper I exploit state-level variation in the timing of one major category of efforts to crack down on the distribution of opioids inside the medical care system - the implementation of statewide Prescription Drug Monitoring Programs (PMPs or PDMPs) - to identify the causal effects of a reduction in opioid supply on health outcomes, health spending, work output, and death, and attempt to integrate these results in order to better understand the welfare tradeoffs inherent in regulating these controlled substances. PMPs are state-run databases used to track prescribing and dispensing of controlled prescription drugs to patients, and the data collected are accessible by physicians, pharmacists, and sometimes law enforcement officials. By monitoring the behavior of both doctors and

[1] See, for example, "A Pain Drug Champion Has Second Thoughts" (Catan and Perez, 2012), in which Russell Portenoy, the author of one of the most highly-cited studies in favor of using opioids for chronic, long-term noncancer pain (Portenoy and Foley, 1986) publicly reversed course after decades promoting increased opioid use: "Dr. Portenoy and other pain doctors who promoted the drugs say they erred by overstating the drugs' benefits and glossing over risks. 'Did I teach about pain management, specifically about opioid therapy, in a way that reflects misinformation? Well, against the standards of 2012, I guess I did,' Dr. Portenoy said in an interview with The Wall Street Journal. 'We didn't know then what we know now.' "

[2] Total drug overdoses as a category, of which there were 44,113 in 2013, recently exceeded car accidents as the leading cause of accidental death in the United States.

patients, PMPs aim to reduce bad doctor behavior (i.e. so-called "pill mills") as well as bad patient behavior (i.e., doctor-shopping), and also are intended to increase physician caution, improve clinical decision-making, and reduce the overall propensity of doctors to prescribe and refill opioids for pain management (PDMP Center of Excellence, 2014). PMPs are considered to be one of the most important policy levers for reducing opioid abuse, overdose, and death. For example, in September 2015 the Centers for Disease Control announced a $20 million grant funding program for states to combat opioid abuse, and the area of focus for these grants listed first is "enhancing prescription drug monitoring programs (PDMPs)." (Centers for Disease Control and Prevention, 2015) Similarly, the American Medical Association recently convened a Task Force to Reduce Opioid Abuse, and their first recommendations, released in July 2015, were that they would focus "on efforts that urge physicians to register for and use state-based prescription drug monitoring programs (PDMPs) as part of the decision-making process when considering treatment options." (Johnson, 2015)

The welfare considerations for determining optimal policy in the domain of opioids are complex. Early understanding of the 'opioid epidemic' conceptualized it as resulting from diversion and theft of opioids from the medical care system for the purpose of illicit abuse, and emphasized that legitimate medical use of opioids for pain is safe and effective. A welfare calculation under this scenario would entail valuing costs and benefits of increased regulation that fall largely on different groups of people - legitimate pain patients bear the costs, while people at risk of becoming illicit drug users experience the benefits. Optimal policy would focus on reducing diversion and theft.[3]

However, recent concerns have shifted to whether opioids are being appropriately used in the course of medical care. A high-profile medical literature review published in 2015 by Chou et al. stated that "accumulating evidence supports the increased risk for serious harms associated with long-term opioid therapy, including overdose, opioid abuse, fractures, myocardial infarction, and markers of sexual dysfunction." The study also noted that no controlled studies on the effectiveness of opioids for pain control have been conducted for use that extends past one year, and concluded that "reliable conclusions about the effectiveness of long-term opioid therapy for chronic pain are not possible." As such, efforts to reduce opioid-related adverse events have shifted towards targeting the pain management system, and patients seen as likely to be at-risk for these opioid-related adverse effects, more directly. Under this newer understanding, it is possible that doctors and patients are not jointly and optimally trading off the

[3]Early efforts to rein in the negative effects of the expanded use of prescription opioids focused on this channel - shutting down illicit 'pill mills' via DEA busts, preventing pharmacy theft, and reducing the supply of unused pills in medicine cabinets that might be stolen by friends or family members via drug "take back" programs (Kolodny et al., 2015; SAMHSA, 2012; Smith, 2013).

costs and benefits of opioid therapy. On the patient side, time inconsistency/present bias or incomplete information might lead to an irrational use of and the development of a dependence or addiction to these drugs. On the doctor side, agency problems with respect to pharmaceutical company perks, insurer reimbursement systems, or hospital satisfaction surveys might incentivize over-prescription of opioids, even as it may be counter to the long-term welfare of most individual patients (Thomas et al., 2015).[4,5]

This paper makes several contributions to this debate. First, I establish that an increasingly prevalent policy lever for addressing societal problems brought about by increased opioid use - the implementation, and push towards greater use, of state-level Prescription Monitoring Programs - achieves a key policy goal, by reducing opioid overdose deaths. However, I also document that there have been unintended consequences with considerable welfare costs from the reduction in opioid availability that has been the result of these efforts. First, I find suggestive evidence that pain levels rise, by examining self-reported pain in the hospital setting. Second, I find that the reduction in opioid availability alters the medical practice of pain management, as patients and their doctors substitute towards alternative pain management approaches like surgery that are more costly and possibly inferior. Third, I study workers with workers' compensation injuries and those on short term disability with pain-related diagnosis codes, and find that they miss more days of work. This suggests that an increase in pain and a reduction in function has resulted from reduced access to pain management with opioids, and that substitution towards alternate pain management has not been sufficient to fully compensate. Finally, I document important substitution behavior by drug abusers. I find that heroin deaths temporarily rise, as users of opioids shift towards heroin, which is a pharmacologic substitute for prescription opioids that is also much more dangerous. The resulting increase is large enough to offset the reduction in opioid overdose deaths for the first year after PMP implementation.

I use these empirical estimates as inputs to a rough welfare calculation, finding that reducing opioid use results in a tradeoff wherein $12.1 billion per year in increased costs from inpatient and outpatient

[4]Legislative attention has been focused on both pharmaceutical company perks and hospital patient satisfaction surveys. In 2014 Senators Grassley and Feinstein sent a letter to Centers for Medicare and Medicaid Services complaining that the use of HCAHPS satisfaction surveys in Value Based Purchasing, which have 3 questions out of 20 on pain control, was distorting incentives for hospitals and physicians towards over-prescription of pain medications. In 2012, Senator Grassley requested information from Medicaid which demonstrated that the highest prescribers of certain abusable prescription drugs were also those receiving large payments from the pharmaceutical companies that make them. Finally, the City of Chicago and two California counties are currently suing the major pharmaceutical manufacturers of prescription opioids, alleging that they deceptively marketed opioids to doctors.

[5]Insurance limitations on reimbursements for e.g. physical therapy and mental health are well known; CMS recently made cuts to the reimbursement structure for some interventional pain management procedures due to lack of evidence for their efficacy and concerns about overuse. The American Society of Interventional Pain Physicians responded by stating that "this may end up driving patients to receive more opiates, drive patients to expensive points of care (the hospital) and have patients [undergo] unnecessary spinal surgery, which is much more expensive than the care offered by interventional pain physicians."

medical spending plus lost wages are traded off against $7.3 billion per year in benefits from reduced addiction and lives saved from opioid and heroin overdose. Further, by examining patterns of benefit and harm more closely, I show that the same demographic subgroups largely appear to experience both the costs of regulation, in terms of increased pain and lost work, and the benefits, in terms of reduced death. This suggests that significant tradeoffs of opioid use do occur at the private level. While opioids may be more effective than is currently suggested in Chou et al., individual pain patients must weigh the private benefits of opioid therapy against the costs in terms of increased chance of addiction, overdose, and death.

As discussed in Garthwaite (2012), an increasing enthusiasm for comparative and cost effectiveness research, and in particular a shift towards its use in deciding reimbursement rates and insurance coverage (e.g. "value based care") has typically neglected to include important broader nonmedical and economic benefits of medical technologies, especially labor supply. This is in part due to a paucity of evidence on those effects stemming from the fact that it is difficult to identify exogenous variation in the use of medical technology; as such, few studies have directly estimated the economic or labor supply benefits of medical technologies or pharmaceuticals. (Exceptions include Garthwaite (2012) and Berndt et al. (2000).) By focusing on a plausibly exogenous source of variation in an individual's opioid supply I am able to shed light on the tradeoffs between economic and medical costs and benefits, for an important and widely-used medical technology: pain medications are the second largest by-volume class of drug sold in the United States, after antihypertensives, and are dispensed to about 75 million unique patients per year, with 15-16 million patients on therapy at a given time (Aitken, 2013).[6]

Additionally, I contribute to the medical and public health literature on the health, wellbeing, and life outcomes of pain patients, and whether they are improved or hindered by long term use of opioids, with an identification strategy that utilizes plausibly exogenous variation in individual opioid supply to study individual outcomes. Because long-term randomized controlled trials of greater than one year have not been conducted on the use of opioids for chronic pain - and almost all studies on opioids have only lasted between 3 and 16 weeks - studies in this literature have been observational or used matched-control techniques, and have been hampered by endogenous selection into chronic opioid therapy which is likely to be correlated with pain, functionality, health, work output, and other life outcomes. My results are generally of the opposite sign to what is often found in these studies. For example, there has been substantial concern in the workers' compensation insurance literature

[6] At peak levels in 2010, sufficient opioids were distributed annually in the US to medicate every person in the country with a standard dose of Vicodin every day for one month.

that utilizing opioid therapy causes injured workers to miss more days of work.[7] This concern arises from a positive correlation between length of opioid therapy and days missed; by studying a plausibly exogenous reduction in opioid supply, I show that there is actually a *negative* relationship between use of opioids and days missed in workers' compensation.

Finally, I provide evidence that the alarming rise in heroin overdose deaths - a 40% year-on-year increase since 2010 (see Figure 3) - is indeed causally linked to the recent crack down in prescription opioid supply. This is a link that has been widely speculated on in the media (see e.g., The Economist, 2015), and for which a small but suggestive medical literature exists (Cicero et al., 2012; Jones, 2013), but which has not yet been empirically demonstrated.

The paper proceeds as follows. Section 2 provides background on the medical literature on opioids, and documents the changing landscape of opioid use over the past 15 years. Section 3 describes my data, and Section 4 describes the empirical framework. Section 5 presents results on the policy successes of PMPs, while Section 6 presents results on the unintended consequences of PMPs. Finally, Section 8 integrates these results into a back-of-the-envelope welfare calculation. Section 9 concludes.

2 Background

The first research to suggest that opioids could be used successfully and safely in the management of chronic, non-cancer pain, without generating significant problems with addiction or abuse, was a non-randomized study of 38 patients who were on opioid therapy for as long as 7 years, published by Portenoy et al. in 1986. Several similar follow-up studies, in combination with a prominent article entitled "The Tragedy of Needless Pain," (Melzack, 1990) which stated that "contrary to popular belief, morphine taken solely to control pain is not addictive," led to a shifting perception among doctors during the 1990s that pain was widely undertreated and that opioids could provide effective relief. A small number of randomized clinical trials on the effectiveness of opioids for chronic non-cancer pain were conducted during this period, indicating opioids could be used for chronic pain, but they answered a narrow question - over a period of 3-16 weeks, are prescription opioids effective at reducing chronic pain and improving function, compared to a placebo drug? (Furlan et al., 2006).

Guidelines and incentives for physicians and hospitals during this period, as well as the overall

[7] See, for example, NY Times, *Pain Pills Add Cost and Delays to Job Injuries*: "Workers who received high doses of opioid painkillers to treat injuries like back strain stayed out of work three times longer than those with similar injuries who took lower doses, a 2008 study of claims by the California Workers' Compensation Institute found. When medical care and disability payments are combined, the cost of a workplace injury is nine times higher when a strong narcotic like OxyContin is used than when a narcotic is not used, according to a 2010 analysis by Accident Fund Holdings, an insurer that operates in 18 states."

regulatory landscape, dramatically shifted towards more liberal pain management across the board. In 1995 the FDA approved Oxycontin for the management of moderate to severe pain, specifically mentioning its use for chronic pain such as back pain. In 1998, The Federation of State Medical Boards Model Pain Policy was rewritten, stating that "the Board recognizes that controlled substances including opioid analgesics may be essential in the treatment of acute pain due to trauma or surgery and chronic pain, whether due to cancer or non-cancer origins" (Federation, 1998). The policy also articulated that "inadequate pain control may result from physicians' lack of knowledge about pain management or an inadequate understanding of addiction. Fears of investigation or sanction by federal, state, and local regulatory agencies may also result in inappropriate or inadequate treatment of chronic pain patients." This policy was strengthened further in 2004, stating that "the state medical board will consider inappropriate treatment, including the undertreatment of pain, a departure from an acceptable standard of practice" (Federation, 2004). Similarly, for hospitals, revised 2001 standards from the Joint Commission on Accreditation of Healthcare Organizations emphasized "a patient's right to pain management (AAACN, 2001)," and introduced the "Pain Scale," a scale of smiling to frowning faces ranging from 0 ("no hurt") to 10 ("hurts worst"). The Hospital Consumer Assessment of Healthcare Providers and Systems (HCAHPS) patient satisfaction survey, which affects hospital reimbursement rates as of October 2012, includes 3 questions out of 20 on pain control, such as whether "the hospital staff [did] everything they could to help with your pain (Zusman, 2012)."

While in the early 2000s medical consensus continued to on-balance support the notion that opioids were under-prescribed and "the medical use of opioids does not create drug addicts, and restrictions on this medical use hurt patients" (McQuay, 1999), by the mid-2000s two concerns about the rapid expansion in the use of opioids had emerged. First, a concomitant and largely unanticipated sharp rise in opioid abuse, addiction, overdose, and death suggested that the *safety* of widespread opioid use had been overstated (Ballantyne, 2006; Catan and Perez, 2012). Although the initial focus of opioid harm mitigation was on preventing diversion and abuse of opioids by non-medical abusers (e.g., through stealing drugs from others' medicine cabinets, pharmacy theft, diversion via pill mills, etc.) increasing attention has been paid to abuse, "iatrogenic addiction," and overdose among medical users (Kaplovitch et al., 2015; Kolodny et al., 2015). However, attempts to estimate how many addicts are created via legitimate medical use are hampered by the fact that a clear consensus on the definition of problematic opioid use and abuse among prescribed patients does not exist, and in practice the distinction can be very blurry (Ballantyne and Shin, 2008; Jones CM et al., 2014).[8] A recent study (Vowles et al., 2015)

[8]Nonmedical prescription drug use is most frequently defined as "use without a prescription, or use that occurs simply

noted that estimated rates of problematic opioid use across studies are quite broad, ranging from <1% to 81%.

Second, the *effectiveness* of long-term opioid therapy also began to be called into question. An observational epidemiological study on opioid use among Danish adults was published in 2006 (Eriksen et al., 2006); this study is still widely cited as some of the highest-quality evidence on long-term opioid use due to the absence of clinical trial evidence. Among patients who reported chronic pain, non-opioid users were utilized as a control group for opioid users. Self-reported pain, work capacity, and total medical utilization were compared between the two groups, and opioid users fared worse on all outcomes. Although the study cautioned that "because of the cross-sectional nature causative relationships cannot be ascertained," it also concluded that "it is remarkable that opioid treatment of long-term/chronic non-cancer pain does not seem to fulfil any of the key outcome opioid treatment goals: pain relief, improved quality of life and improved functional capacity." Several other studies assessing opioid efficacy utilize a similar observational methodology, and find that opioid use is associated with increased medical expenditure and more missed work on workers' compensation and disability (Mahmud et al., 2000; Manchikanti et al., 2011; Morris et al., 2015; Vogt et al., 2005). However, these studies all suffer from major concerns that selection into opioid therapy is endogenous to the severity and unmanageability of pain, as well as correlated with other characteristics such as poor mental health that might determine outcomes, and hence that the negative association between opioid therapy and health and work outcomes may not be causal.

A 2008 review of the use of opioids for analgesia summarized the clinical situation as follows: "The past several decades in the United States have been characterized by attitudes that have shifted repeatedly in response to clinical and epidemiological observations, and events in the legal and regulatory communities. The interface between the legitimate medical use of opioids to provide analgesia and the phenomena associated with abuse and addiction continues to challenge the clinical community, leading to uncertainly about the appropriate role of these drugs in the treatment of pain" (Rosenblum et al., 2008). The 2015 review (Chou et al., 2015) was more pointed, stating "the lack of scientific evidence on effectiveness and harms of long-term opioid therapy for chronic pain is clear and is in striking contrast to its widespread use for this condition . . . Reliable conclusions about the effectiveness of long-term opioid therapy for chronic pain are not possible due to the paucity of research to date." This uncertainty is reflected in increasing controversy among doctors, pain practitioners, and public health professionals about the role of opioids in society; some doctors advocate reconsideration about whether

for the experience or feeling the drug causes."

opioids should be indicated for most chronic pain patients (McCance-Katz et al., 2012). In September 2015, the CDC released draft opioid prescribing guidelines which cited this lack of evidence, stating that non-opioid therapies for chronic pain are preferred and that opioid use should be limited in duration and in quantity when they are utilized for chronic pain conditions; the guidelines provoked immediate controversy (Anson, 2015).

The result of this shifting understanding of the most appropriate role of opioids in society has been a series of recent regulatory changes that have resulted in a decline since 2010 in total opioids dispensed in the US, as seen in Figure 1. A primary policy lever has been the introduction of Prescription Monitoring Program laws at the state level, which are detailed below and which are studied in this paper.[9] Amidst optimism that these policy levers are reducing opioid abuse, overdose, and death, there have also been increasing reports of difficulty accessing pain medications by chronic pain patients (Gleason et al., 2014).

3 Data

3.1 Drug Enforcement Administration Automated Reports and Consolidated Orders System

Data for this study comes from four main sources. First, I utilize data from the Drug Enforcement Administration's Automated Reports and Consolidated Orders System (ARCOS) from 2000 to 2013, obtained by a Freedom of Information Act request. The ARCOS system tracks the flow of DEA controlled substances from their point of manufacture, through commercial distribution channels, to the point of sale/distribution (by hospitals, retail pharmacies, practitioners, mid-level practitioners, and teaching institutions). The ARCOS reports obtained via FOIA contain data on all Schedule II and select Schedule III substance sales at the quarter-state level; in particular, these reports cover the sale of all major opioid pain relievers broken out by active ingredient (codeine, fentanyl, hydrocodone, hydromorphone, meperidine, methadone, morphine, and oxycodone).[10]

[9] Several other regulatory changes have contributed. In 2010, Purdue Pharma switched to a new abuse-deterrent formulation of Oxycontin, intended to provide equivalent pain relief to the old formulation, but prevent street abuse by being resistant to crushing and snorting. In 2011, the DEA engaged in a series of crackdowns on pill mills, especially in Florida. In 2013, the FDA blocked the introduction of non-abuse-deterrent generic versions of Oxycontin from entering the market.

[10] Schedule II and III drugs are both defined as having "a currently accepted medical use in treatment in the United States," and a "potential for abuse and dependence." Fentanyl, hydromorphone, meperidine, methadone, morphine, and oxycodone are Schedule II, while codeine and hydrocodone are Schedule III. Schedule III drugs are distinguished from Schedule II as less abusable, and less likely to produce physical and psychological dependence. In practice, the distinction is important because Schedule III drugs are easier to prescribe and obtain: they can be refilled and the prescription can be called in, while Schedule II drugs cannot. They also have been less of a focus on enforcement by the DEA, leading to greater practitioner comfort with their use.

The ARCOS data received from the DEA is very noisy for several reasons, and this is likely a significant source of measurement error in my ARCOS analysis. First, the ARCOS system has some internal inconsistencies.[11] Additionally, in the course of my research, I gathered ARCOS reports from a variety of sources aside from my FOIA request: ARCOS reports from 1997-2006 that were originally hosted on the DEA's website, and are now archived on the Internet Archive's Wayback Machine, as well as data aggregated by a reporter based on his own FOIA request to the DEA. In each case discrepancies between what was provided to me by the DEA and these other reports exist.[12]

3.2 Truven Marketscan Commercial Claims and Encounters / Health and Productivity Management

Second, I utilize the Truven Marketscan Commercial Claims and Encounters (CCAE) plus linked Health and Productivity Management (HPM) database from 2003 to 2012. This individual-level panel dataset is constructed from data obtained from large employers, and has grown from 457,696 enrollees in 2003 to 3,964,364 enrollees in 2012.[13] Broadly, the individuals in a given year of the Truven data can be considered to represent the population of working Americans who have employer-sponsored insurance, or about 59% of the total population. The CCAE dataset contains rich medical claims data: detailed prescription claims, clinical utilization, and expenditures. Additionally, the CCAE data is linkable via enrollee ID to the HPM databases, which contain workplace absence, short-term disability, long-term disability, and workers' compensation data. The average enrollee appears in the dataset for about 11 quarters.

Because the Truven sample is obtained from large employers, individuals usually move into and out of the sample in groups based on whether their employer is providing data to Truven for that year. This generates concerns that non-random selection of individuals into and out of the sample could bias my estimates. In order to handle this concern, my baseline empirical specification will utilize individual-level fixed effects, thus estimating only within-person variation. In order to evaluate the appropriateness of the assumptions underlying a fixed-effects model (that omitted variables that may bias my estimates

[11] For example, Report 2 contains quarterly drug distribution in each state in *grams*, and Report 3 contains quarterly drug distribution in each state in *grams per 100k population*. Despite the fact that each quarter-state observation should only differ by a constant (100k population as a divisor), these numbers in Reports 2 and 3 - which were generated for me simultaneously by the DEA - do not always agree.

[12] For most discrepancies, I utilized the value from one data source because the other option was an obvious error. No one data source was obviously superior overall; each data source in some cases had obvious inaccuracies or errors. Cleaning files which detail how I reconcile the various ARCOS data sources in order to generate my analysis dataset are available on request.

[13] Overall, 7,361,805 total individuals appear in the database.

are fixed at the individual level over time, and that bias only arises from different individuals with different characteristics moving into and out of my sample over time), I will present robustness checks for the inclusion or exclusion of the individual fixed effects on a balanced subsample of individuals who are present in my dataset for longer periods of time.

For most analyses, I extract an outcome variable from the CCAE claims or HPM data and aggregate it to the individual-quarter level. For example, a main outcome variable, "quantity of oxycodone consumed by an individual in a quarter," is derived from the prescription drug flat file, which contains a list of every prescription filled, identified by National Drug Code (NDC) number (including date filled, intended days supply, and quantity). Truven produces a datafile called Redbook to map NDC numbers onto active ingredients, number of dosage units, and strength per dosage unit. To identify oxycodone prescriptions, I first process the Redbook datafile to identify all NDC codes with oxycodone as the active ingredient. I then merge Redbook onto the CCAE prescription drug flatfile to identify oxycodone prescriptions. Next, I standardize the unit of measurement, strength per unit, and quantity measures in order to calculate the milligrams of active ingredient in each prescription. (For combination prescriptions with multiple active ingredients, only milligrams of the opioid in the prescription were included.) Finally, I total the active milligrams of oxycodone in all oxycodone scripts for each individual in each quarter. A similar procedure is utilized for outcome variables derived from inpatient and outpatient claims, as well as workers' compensation and disability data.

As can be seen in Table 2, in a given year approximately 25% of enrollees in the Truven sample will fill a prescription for an opioid.[14] This is in conformance with national data, which indicates that about 75 million patients receive narcotics prescriptions in a given year (Aitken, 2013). In this study I focus primarily on the prescribing of Schedule II opioids, because they are used for moderate to severe pain and because the reduction in their use has been a primary target of opioid crackdowns. Oxycodone, a Schedule II drug, is the highest-sold opioid by volume, and is also the drug most frequently targeted by these crackdowns, due to the fact that it has typically been considered the most abused and abuseable commonly-prescribed opioid. The highest-volume Schedule III opioid is hydrocodone; it is the opioid patients are most commonly exposed to in a given year, as can be seen in Table 2. This difference in frequency versus total volume is because hydrocodone is more typically used for acute, less-severe, incidental, and shorter-term pain, and thus is prescribed in low and short doses, whereas oxycodone is typically used for chronic or more-severe pain, and thus is prescribed in higher doses and for longer-term

[14] Note that these sample statistics are reported only for the sample of enrollees in the 38 states that are in my study sample, which is described in detail in Section 4.1 below.

use.[15]

Opioids naturally generate tolerance in their users, and most long-term opioid patients will experience escalating doses, such that a typical dose for an opiate-naive user will be far lower than a typical dose for an experienced user (and in fact a dose for an opiate-experienced user would often be fatal to an opiate-naive user). There is not a medical consensus on whether doses should be allowed to escalate to very high levels, or rather should be capped at some maximum.[16] In the Truven prescription claims dataset, I group prescriptions into therapy episodes, where multiple prescription claims are considered to be part of the same episode of opioid therapy if they are for drugs with the same generic name and commence within 3 times the number of days supply of the last prescription filled with that same generic name. Most prescriptions for oxycodone and hydrocodone - over 80% - are initial prescriptions with no follow-on prescriptions, and last less than 15 days. Only a small fraction of opioid therapy episodes (4.5% for oxycodone, 3.3% for hydrocodone) last longer than 90 days. As shown in Table 2, the average Morphine Milligram Equivalent (MME) per day prescribed of oxycodone escalates over time as the duration of the therapy episode increases; the same increase is not observed for hydrocodone because in all formulations available during this period the maximum safe dose was constrained by the maximum safe dose of other active ingredients, usually acetaminophen.

The end result of these prescribing patterns is an extremely right-skewed distribution, where most observations in an enrollee-quarter for oxycodone or hydrocodone are zero, but the tail of the distribution is also very long. My baseline specification will utilize a concave transformation of this outcome variable (specifically, ln(MME in quarter + 1)), which preserves zeros but reduces the influence of outliers. As can be seen in Tables 1 and 5, the average MME per enrollee per quarter in the Truven sample (61 mg)

[15] Although oxycodone is a slightly stronger opioid than hydrocodone, with a MME of 1.5 compared to a MME for hydrocodone of 1, this difference in utilization patterns is mostly due to regulatory factors. Hydrocodone formulations were, until recently, only approved in low doses and in combination with other drugs such as acetaminophen, which, due to its liver toxicity, is considered to provide some abuse deterrence. The presence of other ingredients such as acetaminophen means that hydrocodone products are less safe for long-term use, and cannot be escalated to higher dosages. All hydrocodone formulations (e.g. Vicodin, Lortab) were Schedule III throughout the study period, whereas all oxycodone formulations (as well as morphine, fentanyl, hydromorphone, etc.) were Schedule II, and due to being in Schedule III, hydrocodone was easier to prescribe and obtain than oxycodone. Hydrocodone was rescheduled to Schedule II in August 2014, in large part due to the controversial 2014 FDA approval of Zohydro, a hydrocodone formulation which comes in much higher doses, does not contain any additional active ingredients such as acetaminophen, and which does not have any abuse deterrent technology.

[16] Older pain management guides state that "because of intrapatient variability in physical dependence, opioid tolerance, subjectivity of pain, and biopsychosocial influences, exact opioid dosing with consistently accurate equivalency tables (for conversion among opioids) are lacking. Accordingly, opioids have no well-defined maximum dosage to achieve appropriate therapeutic benefit. Dosage escalation is commonly used until adequate pain relief is achieved or adverse effects preclude further escalation. One panel has stated that a 'reasonable definition for high dose opioid therapy is >200 mg daily of oral morphine [sulfate] (or equivalent), based on maximum opioid doses studied in randomized trials.' (Nerenberg and Fudin, 2010) More recently, in 2012 Washington State set a threshold of 120 MME per day, above which primary care physicians are required by law to consult with a pain management specialist. In Massachusetts, recently-released (May 2015) guidelines recommend that any patient receiving over 100 MME per day begin tapering to a lower dose. Finally, the recently-released draft CDC guidelines (September 2015) recommend a maximum dose of 90 MME per day, with increased prescriber caution above 50 MME per day.

is lower than the MME per resident per quarter in the ARCOS data (102 mg), with the lower figure likely reflecting the fact that the Truven sample does not represent the Medicare-covered elderly or the Medicaid-covered poor, who have greater utilization of these drugs, and also would not capture any illegal or suspicious high-volume diversion, e.g., the operation of pill mills, which do not usually accept insurance (Ailes et al., 2015; Hackbarth et al., 2015).

For my analyses of changes in inpatient and outpatient spending patterns, I focus on individuals who are likely to need pain management due to chronic pain. In order to do this, I construct a subsample of the Truven dataset of enrollees who are likely to need opioid therapy. Using 2003-2004 data, I train a model that isolates the ICD-9 diagnostic codes on inpatient or outpatient claims that are predictive of opioid therapy, and then I identify enrollees from 2005 onwards who ever have an inpatient or outpatient claim using one of these codes as "predictive enrollees." These predictive enrollees can be thought of broadly as individuals who are predicted to need pain management given their underlying health state. Although endogeneity in sample selection might present a concern - a crackdown on opioid prescribing might result in enrollees being more likely to seek inpatient or outpatient care and thus having a claim mentioning these diagnostic codes - as seen in Table A16, having any claim mentioning these diagnostic codes does not appear to be influenced by whether a crackdown has occurred. Although there is evidence presented in Section 6 that total outpatient spending is influenced by decreased access to opioids, all that is needed to select into the sample is the presence of even one encounter mentioning a predictive diagnostic code, so endogeneity seems less likely to be a concern. In some analyses, I combine these enrollees with enrollees injured under workers' compensation, and enrollees on short term disability with a pain-associated case diagnosis code. This "combined predictive subsample" is predicted to have high need for pain management due to underlying chronic pain conditions, and I use this sample to examine several outcomes of interest.

3.3 Hospital Consumer Assessment of Healthcare Providers and Systems (HCAHPS)

Additionally, I utilize the Hospital Consumer Assessment of Healthcare Providers and Systems' Patients' Perspectives of Care (HCAHPS) Survey, a survey sent to a random sample of hospital patients after a hospital visit. HCAHPS contains 3 questions (out of a total of 20) on pain management during the patient's visit, and the CMS Hospital Compare website reports aggregate statistics for a composite measure based on those questions. I utilize data at the state-quarter level from 2008Q3 to 2013Q4. I

utilize as an outcome variable the percent of patients answering that their pain was "sometimes" or "never" well-controlled during their hospital stay.[17]

3.4 National Vital Statistics System Multiple Causes of Death Microdata

Finally, I utilize CDC National Vital Statistics System (NVSS) Multiple Causes of Death Microdata. This dataset contains a record of every death from 1999 to 2013, including underlying cause of death and up to 20 causes of death listed on the death certificate, demographic data such as race, gender, age, and education, and geographic identifiers at the county level. I follow the public health literature in identifying drug overdoses caused by prescription opioids and heroin using both the underlying and multiple cause of death fields.[18] Heroin-related drug overdose deaths are identified if the multiple causes of death fields contain T40.1, and opioid-related drug overdose deaths are identified if they contain T40.2 (morphine, oxycodone, hydrocodone), T40.3 (methadone), or T40.4 (synthetic narcotics - Fentanyl, Propoxyphene, Meperidine, Buprenorphine). For my prescription overdose death results, I will in most cases present estimates for T40.2 alone, because it is comprised of the Schedule II drugs for which I find a first-stage prescribing effect. In appendices I will present results for T40.3 and T40.4 as well. For welfare calculations I will combine all opioids and heroin, i.e. T40.1-T40.4, in order to estimate the aggregate impact on opiate and opioid deaths, which will include any offsetting dynamics as users substitute between different types of opioids/opiates.[19]

4 Empirical Framework

I exploit state-level variation in the timing of the introduction of Prescription Monitoring Program (PMP) laws to study the impact of PMP introduction on opioid prescribing, as well as establish a causal linkage between a reduction in opioid prescribing and a range of other health and labor outcomes of interest.

[17] The current version of the HCAHPS survey instrument can be found at http://www.hcahpsonline.org/surveyinstrument.aspx.

[18] I identify drug overdoses using underlying cause of death codes X40-X44, X60-X64, X85, and Y10-Y14.

[19] Note that multiple cause of death categories are not mutually exclusive - it is possible to have both heroin and prescription opioids contribute to a death, and for both codes to be listed in the 20 multiple cause of death fields. In practice, this overlap is relatively small - over all 15 years in my sample, about 5% of opioid overdoses also had heroin listed on the death certificate.

4.1 Institutional Details of Prescription Monitoring Programs

As discussed in Section 3.1, the Drug Enforcement Administration (DEA) monitors the flow of controlled substances in the United States, but this monitoring only extends to the second-to-last step in the distribution chain: the sale of those substances from manufacturers/distributors to pharmacies, hospitals, and practitioners. Monitoring the last step of the chain, from pharmacies to patients at the retail level, is left to the states. State-level programs to monitor the flow of these substances date back as early as 1973, and a common approach during this early period used special triplicate prescription pads to report the prescribing of every controlled substance. Between 1973 and and 2003, 12 states implemented some kind of controlled substances monitoring; these efforts focused on providing information to law enforcement or licensing boards for the purposes of detecting illicit behavior among patients and doctors.

As opioid prescribing began to grow rapidly in the early 2000s after physician and hospital standards on pain management were liberalized, concerns about greater oversight spurred increasing attention to state-level monitoring. In 2002, the DOJ started a grant program to states to support the implementation of state Prescription Monitoring Programs through the Harold Rogers Prescription Drug Monitoring Program (HRPDMP), and began cooperation with the National Alliance for Model State Drug Laws (NAMSDL), which published the first Model Prescription Monitoring Program Act for adoption by state legislatures in 2003.

The HRPDMP maintained a concern with law enforcement, but during this period states also began to recognize that PMPs could also serve to improve pain management practice and the coordination of care by providing information to physicians and pharmacists about their patients. Nevada introduced the first PMP that was partly intended to improve the practice of pain management via providing access to the database to physicians and prescribers in 1997. The 2003 NAMSDL Model PMP Act included language that the PMP database should be accessible to prescribers. In 2005, this shift in focus towards using PMPs in the practice of medicine (rather than primarily facilitating law enforcement) was codified in federal guidelines passed as a part of the National All Schedules Prescription Electronic Reporting Act (NASPER), which also included grant funding for the implementation of state PMPs. Due to the NAMSDL model law, most states adopting after 2005 adopted similar legislation, including an electronic database and online access for physicians and pharmacists at the point of care, and the focus of those PMPs was on altering the practice of medicine.[20,21]

[20] These later-adopting states even often utilize the same software, from one of a small handful of vendors.

[21] In some recently-adopting states the relationship between law enforcement and the PMP is actively non-cooperative,

As such, states implementing PMPs during this period were implementing relatively standardized laws, and the introduction of a PMP can be thought of as a natural experiment. In this natural experiment, physicians simultaneously gain access to information about their patients, and also begin undergoing monitoring themselves. PMPs can thus be thought of as a bundled intervention which might affect opioid prescribing and distribution in several ways. First, PMPs provide information on patients which can be used to improve care, such as if a patient is being prescribed multiple medications by multiple doctors which are in combination contraindicated due to a drug interaction. Second, by increasing oversight of the last point in the distribution chain, they should reduce obviously illegal diversion by pharmacists, physicians (pill mills) and patients (doctor shoppers). (Patients might be caught by law enforcement if they are engaging in illegal behavior, but there are other behaviors, such as a violation of a patient contract specifying the patient may only visit one doctor, which the PMP can facilitate a doctor uncovering.) Finally, in conjunction with shifting standards of care towards greater caution in medical prescribing, the fact of being monitored by PMPs should induce doctors to move their own prescribing practices more in alignment with "best practice," and in general err on the side of prescribing less rather than more.[22] In practice, exactly how PMPs affect prescribing practices is an empirical question that will be explored in Section 6.

I exclude the 12 states that implemented some kind of controlled substances monitoring program prior to 2003 from my analysis (see Table A2). Institutionally, the highly variable nature of early controlled substances monitoring approaches at the state level suggests that states with early PMPs are likely to be qualitatively different and are thus poor controls for the study of opioid prescribing during the period of study. In particular, assigning a "date of implementation of PMP" to these states is difficult; early controlled substances monitoring programs usually did not meet all the criteria of a PMP that this study considers (especially physician access), and states with early programs tended to phase in the characteristics associated with a PMP that states in the 38-state sample usually adopted all at once, while simultaneously experimenting with more aggressive policies like proactively reaching out to doctors about their patients.[23] Empirically, Figure A2 demonstrates that these institutional

and access requires probable cause or a subpoena; see e.g. Oregon PDMP v. United States Drug Enforcement Administration.

[22]The tension between evolving, more restrictive, opioid standards and what some doctors still consider to be acceptable opioid prescribing, is highlighted by the fact that a prominent pain physician and former head of the American Academy of Pain Management, Lynn Webster, was the subject of clinic raid and four-year DEA investigation. Although his pain clinic closed due to the allegations and investigation, all charges were eventually dropped.

[23]For example, NAMSDL lists Illinois as having implemented its controlled substances program variously in 1961 or 1999; the state lists the date of implementation as 1984. And in a 2008 press release announcing reforms that brought its controlled substances program into conformance with national guidelines and thus finally meeting the definition of a PMP, Illinois also called itself a leader in the aggressive monitoring of controlled substances.

differences indeed translated into notably different prescribing patterns in early-adopting and late-adopting states, both in level and in trend. Early controlled substances monitoring states prescribe significantly less opioids and experience a slower growth in opioid prescribing during the study period, and appear to have partially substituted for lower opioid usage with greater hydrocodone usage, which is higher in level and in growth rate in early adopting states.[24] Focusing on the period after 2002 provides a relatively clean experiment.[25]

Public health studies evaluating the effectiveness of PMPs thus far have been mixed. Studies on early PMPs found weak support that PMPs altered prescribing patterns, but did not affect overdose deaths: for example, Paulozzi et al. (2011) found that between 1999 and 2005, for states with operational PMPs, Schedule II prescribing declined, hydrocodone prescribing increased, and overdoses were not affected. Twillman (2006) documented similar patterns for Schedule II and Schedule III prescribing, and also found no impact on measures of abuse, overdose, and death. Reifler et al. (2012) found that between 2003 and 2009 states with PMPs had a lower rate of increase in indicators of opioid abuse and misuse (but did not consider deaths). More recently, Rutkow et al. (2015) found that the introduction of Florida's PMP in 2011 was associated with significant declines in prescribing compared to a neighboring state, Georgia, and Delcher et al. (2015) found this reduction in prescribing corresponded with a reduction in overdose deaths.

The identifying assumption of my empirical strategy is parallel trends in adopting (treatment) and non-adopting (control) states before the implementation of the law; for all my results I will rely heavily on event-study style specifications, described below in Section 5, which estimate leads and lags of the law's implementation, and thus allow for visual verification that the parallel trends assumption holds. For my health, work absence, and death outcomes, I will additionally utilize an instrumental variables approach. I use the introduction of a PMP as an instrument for a reduction in individual opioid consumption, and interpret my results on e.g., work absences as causally linked to a reduction in opioid availability. The identifying assumption for this interpretation is that there is no other channel by which the introduction of a PMP might affect work and functionality other than through the availability of

[24] That early adopting states have a lower level of opioid use and experience a slower growth in opioid prescribing is evidence that early PMPs did have the intended effect of reducing prescribing; current public health literature on PMPs, which mostly studied the early-adopting states, has been somewhat mixed/inconclusive on this point. (Baehren et al., 2010; Pacula et al., 2015; Paulozzi et al., 2011; Reifler et al., 2012)

[25] Starting in 2012/2013 many of these states did begin to strengthen their PMPs, in particular by mandating that physicians access the PMP prior to dispensing any controlled substance, a reform that has been advocated as a "best practice" for PMPs. I also conduct robustness checks to the choice of 2003 as a cutoff, shown in Table A5. In two states (OH and NM), several phases of implementation did occur during my sample period. In both cases I code using the latter date of implementation, but results are robust to alternate coding using the first date, or to dropping those states from the sample.

opioids.[26]

A final consideration in viewing PMPs as an experiment on the effects of reducing opioid prescribing is whether state-level PMPs have spillovers to other states. In the ideal experiment, the implementation of a PMP would not affect any relevant outcomes in neighboring states, as spillovers could bias the estimated treatment effect. For PMPs, the concern is that patients or doctor shoppers/drug seekers may travel across state lines to states without PMPs to obtain a prescription, and/or bring those drugs back to their home state to sell illicitly. Spillover effects would overstate the effect of PMPs on reducing actual amounts of opioids consumed by state residents, and by overstating the actual reduction in opioid availability, would thus understate the IV-style interpretation of the effect of a reduction in opioid supply on outcomes like overdose deaths.

5 Empirical Analysis: Policy Successes of PMPs

5.1 Opioid distribution decreases

I first consider the impact of the introduction of a PMP on the amount of opioids distributed, utilizing the DEA ARCOS monitoring data for the sale of prescription opioids.

I estimate the following regression models:

$$Y_{st} = \alpha + \gamma_s + \lambda_t + \sum_{\tau} \sigma_\tau D_{\tau,st} + \beta X_{st} + \epsilon_{st} \tag{1}$$

$$Y_{st} = \alpha + \gamma_s + \lambda_t + \beta_1 \ 1(PMP_{st}) + \beta_2 X_{st} + \epsilon_{st} \tag{2}$$

where s is state and t is time (quarter), and $D_{\tau,st}$ are dummy variables for each year before and after the policy is introduced.[27] τ is normalized to 0 in the year before physicians gain access to the PMP. Y_{st} is milligrams of morphine equivalent sold per person. γ_s and λ_t are state and time fixed effects. X_{st} are state controls including unemployment rate and population over 60, and population. Robust standard errors, clustered at the state level, are reported.

[26] A slight nuance when considering this identifying assumption is that because PMPs are a bundled intervention, we might expect that the channels by which opioid supply are reduced are several. In particular, a patient may find his or her opioid supply reduced by their doctor. Alternatively, even if they are not a pain patient, if they are an illicit drug user, they may find their opioid use reduced because the overall supply of illicit opioids has been reduced post-PMP. For these outcomes, I cannot directly detangle whether an increase in absences is due to a a reduction in *legitimate* versus *illegitimate* opioid use. In Section 5 I will consider some distributional results which shed light onto the channels in operation, but I am for the most part not able to observe this directly.

[27] The full specification only includes dummies for 4 years before and after; for early and late adopting states in which more than 4 years before or after are observed, I also include dummies for "more than 4 years before/after" which are not reported.

Specification 1 utilizes an event-study approach that allows visualization of the impact of the law over time through the estimated coefficients on leads and lags, σ_τ. This specification, versions of which I will utilize heavily throughout this paper, allows visualization of the law's impact over time, and also facilitates verification of the parallel trends identifying assumption, by examining coefficients on dummies for the period prior to the law's implementation. Specification 2 is a less-flexible specification which summarizes the mean effect of the introduction of a PMP using β_1, the estimated coefficient on a dummy indicator for presence of the law, $1(PMP_{st})$.

The event study for impact of the introduction of a PMP on Schedule II distribution, estimated according to Equation 1, is depicted in Figure 4. For Schedule II opioids, there is a decline in dispensing that lasts for about $\tau = 3$ to 4 years, before trending somewhat back towards zero. There is no evidence of clear differential pretrends that would indicate policy endogeneity or threaten the empirical framework's internal validity.[28] The corresponding point estimate, estimated according to Equation 2 and displayed in Table 3, indicates that the quantity of opioids prescribed per resident per quarter in a state declines by about 11.35 MME, compared to an overall sample mean of 102.3 MME. Table A3 shows that while a substantial fraction of Schedule II prescribing is for drugs other than oxycodone, the entirety of the main effect is explained by a reduction in oxycodone, which is, as discussed above in Section 3.2, the primary target of these laws.[29] Finally, I subject the Schedule II opioid distribution results to a battery of robustness checks, depicted in Tables A5 and A6.[30] As an additional robustness

[28]There does appear to be a slight anticipatory decline in opioid prescription in the year prior to the law's implementation. There are several reasons why this might be the case, and why the same effect is not apparent in the Truven prescribing data, as will be seen below. Although PMPs are not usually high-profile policy initiatives with long lead-times before enactment, they do often have a phase-in period in which pharmacists begin reporting filled prescriptions to the database but before physicians gain access. Following the NAMSDL national legal database, I code the initiation of a PMP according to date of physician access, but it is likely that pharmacists became aware of, and felt monitored by, the PMP's implementation before physicians. Assuming that some diversion occurs at the pharmacy level, this could explain the anticipatory decline in the year prior. Supporting evidence for this story is depicted in Figure A3; this graph utilizes the NAMSDL database coding for the date PMP begins collection from pharmacists, rather than date physicians gain access, and the pre-trend shrinks almost to zero. (I do not generally utilize this specification in part because date of collection is not available for every state; for this event study I utilize date of physician access if date of collection is missing.) A related but separate story would be that physicians operating pill mills - who would show up in ARCOS data but not Truven because pill mills usually operate cash-only - pay more attention to the passage of PMP laws, and thus adjust their behavior in advance of the date of physician access, as soon as they are aware of being monitored.

[29]The relative size of the non-oxycodone Schedule II drugs category, with a sample mean of 46 MME as shown in Table A3, is possibly somewhat overstated. This category is dominated by Fentanyl, which is a very potent opioid used for the treatment of cancer in highly opioid tolerant individuals. Because its MME is 75, conversion to morphine equivalents makes fentanyl look higher-volume than it is in practice.

[30] The only robustness check in Table A5 yielding a substantively different result is the population-weighted regression. This is a result of the fact that Florida, a large state, had an extremely large decline in opioid prescribing coinciding with the introduction of its state PMP. This was likely due in part to reasons not fully attributable to the PMP itself: the DEA and state law enforcement undertook a concerted effort during the same time period to raid and shut down pill mills that had been operating prolifically, notoriously supplying much of the southeast's supply of illicit pills (Meinhofer, 2015). Florida is an outlier in regards to the level of prescribing attained prior to crackdown as well as the law enforcement scrutiny it received coinciding with PMP introduction, which made national headlines; this kind of contamination of the PMP 'experiment' by other law enforcement efforts is not present in other states. Further, while obtaining a population-weighted treatment effect might be desirable, it is unclear from the perspective of statistical significance whether the weighted or unweighted regression is superior. Considering each state as a separate experiment means there is no inherent

check, I engage in a placebo exercise by randomly assigning a date of implementation to my 38-state sample, then estimating β_1 for the placebo PMP laws 1000 times. The results are depicted in Figure A5. My baseline point estimate is lower than 99.2% of estimated β_1s.

The result that total Schedule II dispensing declines post-PMP indicates that these laws have had a clear policy success. I will explore in more detail the ways in which Schedule II opioid prescribing and dispensing changed below, in Section 6.1, where I can exploit rich prescription claims to better understand the nature of this decline.

5.2 Opioid overdose deaths decline

Momentum for the passage of Prescription Monitoring Programs came from increasing alarm about rising opioid overdose deaths, as depicted in Figure 2, and so I next consider whether deaths indeed declined after the introduction of a PMP. I estimate the following specifications on the CDC mortality data:

$$Y_{ct} = \alpha + \gamma_c + \lambda_t + \sum_{\tau} \sigma_\tau D_{\tau,cst} + \beta X_{ct} + \epsilon_{ct} \tag{3}$$

$$Y_{ct} = \alpha + \gamma_c + \lambda_t + \beta_1 \ 1(PMP_{cst}) + \beta X_{ct} + \epsilon_{ct} \tag{4}$$

where Y_{ct} is county-level opioid overdose deaths (T40.2), specified as $\ln(\text{overdose deaths}_{ct}+1)$ in the baseline OLS specification. These specifications mirror those used for the ARCOS opioid distribution data, Equations 2 and 1, described above.[31]

I document that the reduction in opioid prescribing brought about by the introduction of a PMP has a substantial and lasting effect on opioid-related overdose deaths. As is shown in Table 4 and Figure 5, the introduction of a PMP resulted in an approximate decline in overdose deaths of 12.5%, with the effect persisting 4 years after the introduction of the law. The number of lives saved per year can be roughly approximated: a 12.5% reduction in 2,273 counties with a mean of 0.57 opioid deaths per quarter implies 648 deaths averted per year in my 38-state sample; scaling up for the entire US population, this translates to 912 deaths averted per year via the reduced opioid prescribing that comes from PMP monitoring. I also conduct a 2SLS analysis using the introduction of a PMP as an instrument for reduced overall opioid distribution. The results are depicted in Table A7; point estimates for the

reason to place additional weight on larger states. This is particularly the case in light of the data quality issues discussed in Section 3. Dropping Florida from the sample reduces the point estimate to -7.26 (standard error = 2.73) for the weighted regression and -7.11 (standard error = 3.69) MME per quarter for the unweighted regression, significant at the 5 and 10% levels respectively.

[31] I conduct robustness checks, and in particular consider robustness to this form of coding of the outcome variable, in Table A8.

effect of reducing the distribution of opioids on overdose deaths are very close for the OLS and 2SLS specification, indicating that the tight correlation between levels of opioid prescribing and number of opioid deaths is indeed causal.

5.3 Heroin overdose deaths temporarily increase

I also find empirical evidence that cracking down on prescription opioids is contributing to the alarming recent increase in heroin overdose deaths - a result that temporarily mitigates the gains achieved in reducing opioid overdoses. As prescription opioid overdose deaths have leveled off, beginning in 2010, heroin overdose deaths have risen rapidly (Figures 2 and 3). Prescription opioids and heroin, both derived from the opium poppy, are pharmacologic substitutes: a user of one can stave off withdrawal, relieve pain, or get high, using the other. Substitution away from opioids and towards heroin may be of particular concern because heroin is more potent and more variable than prescription opioids, sometimes containing deadly adulterants such as illicitly synthesized fentanyl; for these reasons heroin abusers experience much higher mortality rates.[32] I consider the impact of reduced opioid prescribing on heroin overdoses using Specifications 3 and 4 above. The results are depicted in Figure 6: I find an approximate 7% increase in heroin overdose deaths in the first year after the introduction of a PMP. After several years the sign of the effect reverses and becomes negative (though insignificant), suggesting that heroin may be a short-run substitute but long-run complement for prescription opioids.[33]

Given their substitutability, considering heroin and all opioid overdose deaths jointly is policy-relevant for assessing the effectiveness of Prescription Monitoring Programs at achieving their primary goals. As shown in Figure 7, I find the net result of these opposing effects is that the increase in heroin overdoses is enough to offset the decrease in opioid overdoses for the first 1-2 years.[34] After 4 years, the reduction in deaths is actually strengthened when considering the two categories together. Utilizing the point estimate in Table 4, Column (3), in a similar calculation as Section 5.2 above, I approximate

[32] Utilizing the National Survey on Drug Use and Health to estimate the total number of heroin users, the overdose death rate per heroin abuser is nearly 1%, far higher than any other drug including prescription opioids.

[33] I consider robustness of this result in Table A9; the heroin overdose results are more sensitive to alternative specifications than the prescription opioid overdose results. Nonetheless, the broad pattern tends to hold: an increase in year 1, and a decrease by year 4. Demographic breakdowns, not shown, indicate that different subpopulations may experience heterogeneous effects.

[34] Alongside prescription opioids overdose deaths under T40.2 that I considered above, there are two other categories of prescription opioids included in this analysis of the total effect: methadone (T40.3), and fentanyl and other synthetics, (T40.4). Both are smaller categories than prescription opioids under T40.2. Methadone is used for the treatment of pain as well as the treatment of opioid addiction, and thus the expected effect on methadone overdoses post-PMP is ambiguous, as more people may be seeking opioid addiction treatment. Fentanyl is used for cancer pain, but is also an illicit and extremely dangerous additive to heroin that has been partly blamed for the recent rise in heroin overdoses. I display the event studies for each independently in Figure A4; while both are noisy, of note is that the graph for fentanyl appears to mirror the graph for heroin.

that the net average effect is a reduction in 1,103 opioid and opiate deaths per year.

That the substitution towards heroin is significant but temporary suggests that there are important tradeoffs when cracking down on opioids when they have been liberally utilized previously. There is a stock of addicts who substitute towards a much more dangerous substance as they lose access to opioids (whether illicitly or licitly obtained) - and those addicts are more likely to die because it is easier to overdose on heroin. But after several years, the reduction in availability of opioids means that the flow of individuals into becoming addicts has been reduced, and the net result is a reduction in overdose deaths after several years.

6 Empirical Analysis: Consequences of Reduced Opioid Prescribing

Next, I consider the consequences of reduced opioid prescribing on the treatment of pain. Pain management is a classic 'grey area of medicine' - the evidence base is mixed, and patient preferences should be important to determining a course of treatment (Chandra et al., 2012). Although opioid therapy has become controversial in recent years, and the evidence base is very inconclusive on safety and efficacy, as is explored in sections 1 and 2, other approaches to pain management, especially interventional pain management (e.g., epidural steroid injections), and surgery are also controversial, as evidence on their effectiveness has been mixed.[35] Heavy utilization of opioid therapy to manage pain, rather than physical therapy, interventional pain management approaches, or surgery, has likely found favor in part due to the fact that prescribing opioids is far cheaper for the payer and less time-intensive for the provider.[36] But if there are agency issues between the insurer and patient, or doctor and patient, reducing opioid availability and shifting towards more expensive therapies which may treat the root of the pain problem rather than the symptom may be preferable for some patients, especially if they consider their private risks of addiction or overdose (Thomas et al., 2015).

[35] As noted in a previous footnote, in 2014 Medicare cut reimbursement rates to many kinds of interventional pain management for this reason. Certain kinds of surgery, such as spinal fusion surgery for back pain, are also considered questionably evidence-based (Christensen, 2004).

[36] Note that other developed countries, particularly in Europe, favor an integrative approach to pain management, and use far fewer opioids per capita.

6.1 Medical treatment of pain

I exploit rich claims data on healthcare utilization (the Truven databases) to uncover how medical treatment of pain changes in response to the introduction of a PMP. For these datasets, I aggregate raw claim-level data to observations at the individual-quarter as described in Section 3.2, and estimate:

$$Y_{it} = \alpha + \gamma_i + \lambda_t + \sum_{\tau} \sigma_\tau D_{\tau,ist} + \beta X_{it} + \epsilon_{it} \tag{5}$$

$$Y_{it} = \alpha + \gamma_i + \lambda_t + \beta_1 \ 1(PMP_{ist}) + \beta_2 X_{it} + \epsilon_{it} \tag{6}$$

where i is individual, s is state, and t is quarter, and Y_{it} is an outcome variable constructed from Truven raw data such as ln(morphine milligram equivalent [MME] per quarter + 1).

6.1.1 Opioid Prescribing

I first document that the overall effect of PMP introduction on opioid prescribing for the Truven medical claims dataset, which represents the experience of approximately 175 million American individuals with employer-sponsored health insurance, comports well with the results from the DEA ARCOS controlled substance monitoring dataset, which in theory contains a complete accounting of all opioids that are distributed. The results of estimating Equations 5 and 6 are presented in Figure 8 and Table 5.

The results show a statistically significant decline in Morphine Milligram Equivalent (MME) of Schedule II opioid prescribed for all individuals in the sample, post PMP introduction. I present results for ln(MME prescribed in quarter + 1) as the dependent variable in Column 1.[37] The point estimate from Panel 1 indicates that there is a 0.5% decrease in the *geometric* mean of the distribution of MME + 1 for the full sample.[38] Column 2 (the binary outcome variable) indicates that the introduction of a PMP reduces the overall probability of an enrollee receiving opioids in a quarter by .0595 percentage points, i.e., from about 2.5 percent overall, to about 2.44 percent.[39] Finally, considering the results in Column 3 assists in understanding the change in overall prescribing levels, using the raw level of MME

[37] As detailed above in Section 3.2, due to the fact that individuals tolerate physically to opioids very quickly, the opioid distribution for the overall Truven sample is highly skewed, with a large number of zeros as well as a very long right tail. I utilize ln(MME + 1) as the outcome variable in my baseline specification because it is the most straightforward transformation with which to deal with outliers, while also retaining the large number of zeros in the sample and information about the quantity/level consumed in quarter. I consider several alternate concave transformations in Table A12 and Figure A6.

[38] $(e^{\beta_1} - 1) * 100$ is the percent change in the geometric mean of MME + 1 associated with going from no PMP to PMP. The geometric mean captures the central tendency of the distribution and is in this case very different from the arithmetic mean - 60.8 MME per quarter (arithmetic) versus 0.15 MME per quarter (geometric).

[39] This estimate only captures reductions on the extensive margin, i.e., patients entering or exiting opioid therapy, and does not capture changes in the level of prescribing.

dispensed in the quarter as the dependent variable. Although the ln(MME+1) specification is more appropriate for assessing statistical significance due to the skew of the dependent variable, as OLS on the raw quantity of MME is too influenced by outliers, the raw MME per quarter OLS specification gives an unbiased estimate of the average reduction in per capita opioids dispensed per quarter in the Truven claims data, and thus is useful for comparison to the ARCOS result. The result demonstrates that the introduction of a PMP is associated with an average of 6.3 MME fewer Schedule II opioids prescribed per patient per quarter (from a mean of 60.8 MME, so a decrease of about 10% in the total quantity). As discussed in Section 3.2, the average MME per capita in the ARCOS dataset is somewhat higher than in the Truven claims data (102.3 MME versus 60.8 MME per capita per quarter) likely because Truven does not include the elderly or the poor, and does not capture most illegal or high-volume suspicious diversion. The point estimate of the reduction in MME per capita for the Truven prescription claims dataset represents a 10% decline, compared to an 11% decline in MME per capita for the ARCOS controlled substance monitoring data.

That these reductions are so similar across the two datasets is suggestive that PMPs are affecting prescribing and dispensing roughly proportionally for disparate U.S. populations, including those with employer-sponsored insurance. It also demonstrates that the reduction in overall opioid availability is not simply due to a reduction in high-volume diversion through pill mills or pharmacy theft, and is occurring for medical users as well. The next section considers the characteristics of medical users who experience the sharpest declines in opioid access.

6.1.2 Margins along which opioid prescribing is reduced

As described in Section 2, doctors and public health practitioners are increasingly questioning the role of opioids in the treatment of long-term, chronic non-cancer pain, while use for acute pain or terminal cancer is less under debate. Yet recent legal initiatives have also targeted prescribing for acute conditions, based on the concern that excess prescribing in the acute setting can lead to abuse among those patients, or that left-over pills might be diverted (Miller, 2015).

I attempt to better understand the margins along which the PMP-induced reduction in opioid prescribing is occurring, using several approaches. First, as detailed in Section 3.2, I group opioid prescription claims into 'opioid therapy episodes' based on temporal proximity and generic ingredient, allowing me to observe whether opioid therapy is *initiated* in a given quarter when the enrollee is not currently taking an opioid. Recall from Section 3 that most prescriptions for opioids (around 80%)

end after one prescription. Table 6 demonstrates that there is a reduction in whether opioid therapy is initiated at all, and it appears from Column 2 that the effect is stronger when considering the amount prescribed in that script as well as whether an initial script is prescribed, indicating new prescriptions are less frequent and contain less morphine equivalent. Given that the majority of opioid therapy episodes are short-term therapy for acute pain conditions like broken bones, it is likely that this reduction in initiation of opioid therapy is occurring both for acute pain conditions (like broken bones) and chronic pain conditions.

Table A14 considers whether doctors switch established patients from more potent opioid prescriptions to less potent opioid prescriptions, such as from oxycodone to hydrocodone, or vice versa. I classify opioids as 'more potent' if they are in a higher schedule, and 'less potent' if they are in a lower schedule; I also classify fentanyl, hydromorphone, morphine, and oxymorphone as more potent than oxycodone due to their greater equianalgesic potency. In both cases the coefficient is a precise zero, indicating this behavior is not an important component of changes in prescribing patterns post-PMP.[40]

In Table 7, I disaggregate the distribution of enrollee-quarter observations for MME opioid into bins (where thresholds are set in order to divide the distribution of nonzero observations into 10 groups with roughly equal numbers of observations), and define the outcome variables as binary (0/1) for whether or not a quantity of opioid within the thresholds for that decile were observed in a given quarter. What this exercise demonstrates is that there is shrinkage at the highest deciles of the distribution (which is patients receiving approximately 6,074 MME opioid in the quarter or greater). It is unclear from this exercise whether the other bins shrink or not; the insignificant results for most lower deciles are consistent with a story where individuals in Deciles 7-10 move to Deciles 4-6, individuals from Deciles 4-6 move to Deciles 1-3, etc., i.e. a world where the introduction of a PMP induces doctors to reduce prescribing for everyone. It is also consistent with a story where individuals move from Deciles 7-10 to "no opioids;" this would be a story where PMPs primarily function to catch high-volume "doctor shoppers," while the rest of the distribution is unaffected. Recall from Table 2 that average MME per day of opioids ranges from 40 to 100 depending on the length of time on opioid therapy; the threshold for Decile 10, 6,074 MME per quarter, translates to 70 MME per day, so people in Decile 10 are receiving enough opioids in that quarter for a moderate dose of opioids every day. The significant reduction from Decile 10 suggests that people on chronic opioid therapy are receiving a reduction in dose, or being discontinued entirely.

[40] I do find some suggestive but inconclusive evidence that there is overall substitution towards hydrocodone and Schedule III drugs as Schedule II drugs are restricted. As shown in Figure A7, there is possibly a weak increase in overall dispensing and prescribing of these drugs post-PMP.

Medical literature suggests that individuals in Decile 10 are at relatively high risk for adverse events based on their medical opioid use, including overdose death. Other studies have estimated that the death rate for chronic/heavy opioid users is 205 per 100,000 users (Kaplovitch et al., 2015). In Table 7, I find that individuals go from a .23% to a .2% chance of being in the heaviest-prescribed Decile 10 in a given quarter, post-PMP. Scaling up for the portion of the US population represented by the Truven dataset, that represents about 52,500 fewer individuals in a given quarter in that category, which would imply about 431 fewer deaths of chronic medical opioid users in the population of working aged individuals with employer sponsored health insurance, per year. This suggests that a little under half of opioid overdose deaths averted by the introduction of a PMP could be among heavy medical opioid users of working age who are employed (as opposed to teenagers, recreational users, the elderly, etc.).

Taking these results together, I conclude that the reduction in opioid prescribing is likely occurring at all parts of the prescribing distribution - for initial prescriptions (most of which will be for acute pain), and existing, high-volume users (likely, individuals with chronic pain).

6.1.3 Robustness and specification checks

As discussed above in Section 3, the Truven data is not a representative national sample, but rather is obtained from employers (meaning that individuals, whose characteristics are likely to be correlated, enter or leave the sample in groups as their employers join or exit). In Table A13, I consider robustness of the baseline specification to the inclusion of individual fixed effects, which I utilize throughout my analysis. I do this by considering a balanced panel in individuals - individuals who are present in my data for 7 years continuously (i.e. 2006-2012) - and estimating the treatment effect with and without individual fixed effects. As shown in Table A13, the point estimates for all these groups are consistently of the same magnitude, which is approximately 2/3 the magnitude of the baseline point estimate for the full sample. That the inclusion of individual FE in this sample does not materially affect the magnitude or significance of the point estimate compared to no individual FE indicates that there are not individual-level unobserved characteristics that might bias the estimate; the individual FE are serving in regressions on the full sample only to deal with changes in the composition of the sample that might bias the estimate. I do not generally utilize this smaller sample of continuous enrollees, but instead opt for the full sample with individual fixed effects, because most individuals are present in my data only for a few years, so there is a considerable cost in terms of power and also representativeness to excluding the modal employee. It also is likely that individuals employed continuously for many years

are different (on observables and unobservables) from employees employed for a shorter duration. They differ on observable demographics - they are on average four years older (46 versus 42), more male (65% versus 60%), are more likely to be in a union (23% versus 17%), and consume fewer opioids on average (60.8 versus 52.4 MME per quarter).

Robustness checks presented in Tables A10 and A11 indicate that the main Truven results for prescribing are robust to a variety of other alternate specifications, including state specific linear time trends and a balanced panel in states.[41]

6.1.4 Self-reported pain in hospital setting increases

A first-order consideration for assessing the welfare costs of reducing opioid use is whether patients experience increased pain. Unfortunately, a high-quality, yearly, population-based survey, with available geographic identifiers, that measure pain levels in the general population, does not exist to the author's knowledge. In order to partially assess whether pain increases or decreases post-PMP, I utilize the Hospital Consumer Assessment of Healthcare Providers and Systems (HCAHPS) patient satisfaction survey, reported on Medicare's Hospital Compare website, which assesses whether a patient's pain was adequately controlled during their hospital visit, grouping responses into "Always," "Usually," "Sometimes/Never."

I estimate the following specifications:

$$Y_{st} = \alpha + \gamma_s + \lambda_t + \sum_{\tau} \sigma_\tau D_{\tau,st} + \beta X_{st} + \epsilon_{st} \tag{7}$$

$$Y_{st} = \alpha + \gamma_s + \lambda_t + \beta_1 \ 1(PMP_{st}) + \beta_2 X_{st} + \epsilon_{st} \tag{8}$$

where Y_{st} is percent of patients answering that their pain was "Sometimes/Never" well-controlled. The event study for the impact of a PMP on the percent of patients reporting their pain was "sometimes or never" well-controlled is presented in Figure 9, and the corresponding point estimates in Table 8. The mean percent reporting their pain was not well controlled for this period is 7.4, and the estimated percentage point increase is around 0.2 in Year 1, rising to around 0.6 in Year 4.[42]

[41] For the balanced panel, the σ_τ coefficients are estimated according to the full specification in Equation 5, but only reported if estimated by a balanced panel (i.e. for the 1 year balanced panel, I only report coefficients for four quarters pre- and post.) I do not estimate the 2-year balanced panel for Truven, whereas I do for ARCOS as reported in Table A6, because I lose too many states from the sample; I am able to do so for ARCOS because I have data for 2013 and thus a 2 year balanced panel includes more states.

[42] Note that as depicted in Table 8, the point estimate as reported in Column (1), based on Equation 8 is smaller, and insignificant, when compared to any of the point estimates for the lags from Equation 7. This discrepancy is due to (mis-?) estimation of the baseline time trends using the less-flexible pre-post model. Controlling for state-specific linear trends brings the two estimates closer into alignment, but there is still a substantial discrepancy. Given evidence in Sections 5.1

6.1.5 Substitution towards more expensive inpatient and outpatient spending

As established in Section 6.1, opioid prescribing declines for both initial and high quantity prescriptions, and thus likely affects patients with both acute and chronic pain. If a reduction in opioid prescribing falls on legitimate chronic pain patients (rather than solely removing access for doctor shoppers, nonmedical abusers, etc.), we might expect to observe a compensating increase in other kinds of medical spending to manage their pain.

As discussed in Section 3.2, for this analysis I utilize a subset of Truven enrollees who are predicted, based on diagnosis codes in their inpatient and outpatient medical claims, to need chronic pain management. I additionally consider, alongside this group, employees ever injured enough to miss days of work under workers' compensation. Having a workers' compensation injury is an effective predictive characteristic because the nature of workers' compensation injuries means that they almost always are accompanied by pain: about 70% of workers injured enough to miss days of work received an opioid prescription.[43] Finally, I include all employees who take short term-disability using a pain-associated case diagnosis code.[44] This "combined predictive subsample" is a group who have high predicted need for chronic pain management based on observable characteristics; these characteristics have been selected because they are predictive, in the training sample, of a need for opioids, and not endogenous to the introduction of a PMP. (Tables 11 and A16 displays the results of tests for whether these characteristics are endogenous.)

In Table 9, I display estimates of the impact of a PMP on overall inpatient and outpatient spending, considering separately all subsets of enrollees, and then finally the "the combined predictive subsample," which combines the three groups. The event study for the impact of PMPs on inpatient and outpatient spending for this combined subsample is depicted in Figure 10. Post-PMP, there is a discontinuous jump in medical spending that is persistent and ranges from approximately 5% initially to 10% in later years.[45]

and 6.1 that the impact of PMPs does in fact evolve over time, with the effect growing for a few years post introduction, the more flexible model with leads and lags is likely more appropriate. See Table A15 for robustness checks for the event study specification; it is robust to alternate specifications.

[43] Examples of some of the most expensive causes of injuries severe enough to miss work in my dataset include "slip and fall", "caught in/between machinery", "continuous trauma", "strain or injury by lifting", "struck/injured by falling or flying object", etc.

[44] I associate diagnosis codes with a need for pain management using a similar approach as the predictive subsample; I train a model using early years of the Truven data, associating short term disability case diagnosis codes with inpatient, outpatient, and drug spending that indicates the patient is utilizing pain management. Unlike the workers' compensation sample, this is necessary because people take short term disability for many reasons that may not have to do with injury or pain. In particular, the most common reason for taking short term disability is pregnancy. For this exercise, I use four years of training data, as the lower frequency of short term disability claims and the dominance of pregnancy in case diagnoses requires more years of data.

[45] Checks for robustness to alternate specifications can be found in Table A10, Panel 2.

Workers' compensation is in itself an interesting setting in which to study changes in medical utilization after a reduction in opioid access, because of the prevalence of opioid use. Workers' compensation patients typically visit special doctors for all medical care that is linked to the injury that occurred on the job, and those claims are submitted to the workers' compensation payer rather than the employee's employer-provided health insurance. Workers' compensation insurers have historically reimbursed more readily for prescriptions such as opioids than alternatives such as physical therapy or surgery; many have limitations, for example, on types of procedures covered, which doctors a patient may visit for the injury, the number of visits for treatments like physical therapy, time coverage will last for an injury, etc., depending on state law and other factors. Further, unlike employer health insurance, claims for spending on an injury must be approved and disputes over coverage frequently arise. Although workers' compensation insurers historically preferred prescribing pain medications over other procedures, recently a great amount of attention has been focused on reducing opioid pain relievers in workers' compensation, as studies have emerged showing an association between use of opioid pain relievers and higher medical spending. Some state PMPs specifically target workers' compensation doctors.

One disadvantage of the Truven dataset is that while I do observe detailed medical claims made on their employer-sponsored insurance, I cannot observe the detailed medical claims *made under the workers' compensation claim;* I can only observe total medical payments over the full life of the claim.[46]

As can be seen in, Column 3, workers who are ever injured on workers' compensation badly enough to miss work are observed to have significantly higher inpatient and outpatient spending after a PMP-induced reduction in opioid availability, *under their employer-sponsored health insurance.* It is important to note that this increase in spending is observed for the entire duration of the injured worker's time in my sample - not just while they are injured. Thus this result should be interpreted first as a result for a group of workers that is particularly likely to need pain management, similar to the predictive enrollees sample. Nonetheless, this result is also suggestive that there may be cost offsets/financial externalities from reducing the prescribing of opioids inside workers' compensation (e.g., similar to Chandra et al. (2010)). In Table A18 I also display the equivalent result for the workers' compensation insurer's total medical spending, pre- and post-PMP. Because this data is very inexact, as discussed, this evidence is only suggestive; but indicates that the increased spending accrues to both types of insurer.[47]

For the remainder of this section, I consider the combined predictive subsample. In Table 10, I consider substitution to alternate pain management more closely, by disaggregating this result for

[46] Because of the nature of the dataset, this data is also not particularly temporal: if a claim lasts for 2 years, I only observe the total amount spent on medical care over those two years.

[47] I do include the total medical spending observed for the workers' compensation insurer in my final welfare calculations.

inpatient and outpatient spending based on rough categories of procedures that might provide pain management as a substitute for opioid therapy. For example, under interventional pain management, I include procedures such as epidural steroid injections and spinal cord stimulation; under surgery I include procedures such as spinal fusion surgery and joint replacement. A complete list of how categories were constructed appears in the Online Appendix. Table 10 shows particularly notable substitution towards spending on surgery - a 1.3% increase, and office visits - a 5% increase. (The surgery category has mean spending of about $331 per enrollee per quarter, and office visits about $135 per enrollee per quarter.) As discussed above in the introduction to this section, this substitution towards surgery as an alternative form of pain management may be welfare-decreasing or increasing, depending on patient preferences over pain management inputs, many of which have a questionable evidence base, and whether there were agency problems between individuals and their insurer in getting more expensive procedures like surgery approved.

The increase in spending on office visits is somewhat more difficult to interpret. The spending categories as constructed are rough; because most outpatient spending occurs at an office visit, this category may be capturing many miscellaneous kinds of substitute pain management spending. Alternatively, this increase also may represent transaction costs associated with PMPs and crackdowns on opioid prescribing. Guidelines for long-term opioid therapy recommend frequent checkups, pill counts, urine testing, etc., and many states increased the frequency of doctors' visits required to obtain hydrocodone formulations when they implemented their PMP.[48]

These results, in combination with the results in Section 5.1, demonstrate that the reduction in opioid prescribing is occurring not only for non-medical users, people engaging in diversion, or pill mills, but also for medical users with legitimate needs for pain management. Because opioid medications are cheap, these results show that overall medical costs for chronic pain patients will *increase* with a reduction in opioid availability. Given past medical studies, detailed in Section 2, have indicated that higher opioid use is associated with higher medical spending, this result contextualizes those findings, indicating the observed associations in those studies are likely not causal. Nonetheless, it is not clear from this result alone that patients are being harmed by an opioid crackdown, from a private welfare perspective, given insured patients do not bear the majority of the cost of this increase in spending. The substitution towards new and more expensive kinds of pain management care is suggestive, but not definitive, that their pain may not be properly managed after the reduction in opioid availability.

[48] Hydrocodone was schedule III during this period, in theory allowing patients to receive refills from their doctor, but many states implemented specific regulations not allowing refills for hydrocodone only.

7 Empirical Analysis: The Effect of Reduced Opioid Prescribing on Work

I exploit a unique feature of the Truven claims dataset - its linkage to workers' compensation claims and short term disability data - to examine the work ramifications of reducing opioids for workers that are likely to need them to manage pain. As discussed above, about 70% of workers missing days of work under workers' compensation received an opioid prescription. There has also been increasing alarm among workers' compensation payers that narcotics use is contributing to increases in total medical and indemnity (lost wages) costs for injured workers. A typical study concluded that "workers prescribed even one opioid had average total claims costs 4-8 times greater than claimants with similar claims who didn't get opioids... the cost of a workplace injury is nine times higher when a strong narcotic like OxyContin is used than when a narcotic is not used (Rosenblum, 2012)." As discussed in the introduction, these studies usually use matched controls, (see e.g., Franklin et al. (2008); Webster et al. (2007)) and are plagued by endogeneity problems. Additionally as above, I study workers missing work under short term disability who have pain-associated case diagnosis codes. As with workers' compensation, the literature on the role of opioids in short term disability has been observational, and found that medical opioid use is associated with *decreased* work and function (Ashworth et al., 2013; Franklin et al., 2008; Mahmud et al., 2000).

I document a strong and persistent increase in days missed under workers' compensation and short term disability that coincides with the introduction of a Prescription Monitoring Program. The point estimates are displayed in Table 12 and the equivalent event study is depicted in Figure 11; the introduction of a PMP coincides with a persistent increase in days missed of about 3.8% on average. Robustness checks can be found in Table A10, Panel 3. The change in days missed seems to occur on the intensive margin; as is shown in Table 11, the increase does not come from an increase in the frequency of a worker being injured, nor does the introduction of a PMP seem to affect whether a workers' compensation injury results in having some days lost versus none.

To consider more precisely the nature of the causal relationship between opioids and work output, I conduct an analysis using two-stage least squares, utilizing the introduction of a PMP an an instrument for access to opioids, and consider the impact of opioid availability on work outcomes. The causal relationship of interest (eq. 9) is:

$$Y_{it} = \alpha_1 + \gamma_i + \lambda_t + \beta_1 X_{it} + \rho \ \ln(MME_{it} + 1) + \epsilon_{1,it} \tag{9}$$

and first stage (eq. 10) and reduced form (eq. 11) relationships are:

$$\ln(MME_{it}+1) = \alpha_2 + \gamma_i + \lambda_t + \beta_2 X_{it} + \Pi \cdot Z_{it} + \epsilon_{2,it} \tag{10}$$

$$Y_{it} = \alpha_3 + \gamma_i + \lambda_t + \beta_3 X_{it} + \Pi \cdot Z_{it} + \epsilon_{3,it} \tag{11}$$

where Y_{it} is $\ln(\text{days absent}_{it}+1)$ and $\Pi \cdot Z_{it}$ is a vector of instruments for $\ln(MME_{it}+1)$ comprised of lags of time elapsed since PMP implementation:

$$\Pi \cdot Z_{it} = \sum_{\tau=1}^{16} \pi_\tau D_{\tau,ist} + \pi_{17} D_{\tau \geq 17,ist} \tag{12}$$

The results of this exercise are found in Table 14, Columns (1) and (4). While the OLS association between days missed and opioids suggest that increasing opioid use by 100% would increase days missed by 16%, using PMPs as an instrument reverses the sign: increasing opioid use by 100% *decreases* days missed by 74%.

The decline in work output for workers on workers' compensation and disability after an opioid crackdown is important because it is a clear demonstration of direct welfare losses due to the reduction in availability of opioids and subsequent increase in pain. Aside from being of direct policy relevance to workers' compensation payers and policymakers, the IV results linking reduced opioid use to increased days missed provide evidence that opioid use is causally linked to *improved* ability to function in the long term for people needing pain management, a result with implications for individuals beyond those directly studied in the workers' compensation and disability subsamples. As discussed above, evidence on long term opioid use and overall function is limited to observational studies because no randomized trials of opioids have been conducted that last more than one year; this quasi-experimental approach uncovers a result that is of the opposite sign to the observational approach.

In conjunction with the results on increased inpatient and outpatient spending in section 6.1.5, these results indicate that pain and function might be worsening for pain patients after an opioid crackdown, suggesting important welfare losses for individuals with pain beyond workers' compensation and short term disability claimants per se. Losing access to opioid pain management may reduce chronic pain patients' work output, and harm overall functionality, freedom from suffering, life satisfaction, and happiness.

8 Empirical Analysis: Quantifying Tradeoffs of Reducing Opioid Use

I have documented four main effects of introducing a Prescription Monitoring Program. First, opioid prescribing declines. Second, opioid overdose deaths decline. Third, substitute spending on alternative (and more expensive) inpatient and outpatient medical care increases, and pain levels in the hospital setting increase. Finally, days missed for short-term disabled and injured workers increase.

I use these four results to undertake a rough, and incomplete, social welfare calculation, attempting to quantify the costs and benefits of a broad reduction opioid prescribing. First, using the Truven claims data, most of the costs and benefits to society of a reduction in opioid prescribing are directly monetized. For the 418,212 combined predictive subsample enrollees, I can sum spending on prescriptions (including the decrease in spending on opioids, as well as any changes in spending for other drugs) and inpatient and outpatient utilization, thus encompassing all observed medical spending for the employer-sponsored insurance. I then add lost wages (through an imputed daily wage based on the total indemnity payment for a claim), and medical payments under workers' compensation, and thus consider the change in total spending on healthcare and lost wages, post-PMP. The result of this exercise is found in Table 13 (with robustness checks in Table A10, Panel 4). There is a 5.2% increase in total costs, and the mean total cost for this group is $4,035 per quarter, so this increase represents about $211 in increased spending per quarter for these enrollees. Assuming the Truven dataset represents the population of employees with employer provided health insurance as discussed in Section 3, these 418,212 individuals represent roughly 14.3 million people nationally, and an increase in spending of $12.1 billion annually for their health care and lost wages.[49]

These costs are traded off against the value of a 7.7% overall reduction in opioid + heroin overdose deaths in 2,273 counties with a mean of 1.12 deaths per quarter: approximately 1,103 lives saved per year. To make a rough comparison, I employ a commonly-utilized number for the Value of a Statistical Life (VSL), $6.4 million; this implies the benefits in lives saved according to this metric are approximately $7.1 billion. Further, in order to capture reductions in other costs to society of substance abuse and addiction, I use estimates from other literature on rates of treatment admissions, emergency department visits and incarcerations that are benchmarked against opioid overdose death rates as

[49]Note that the last line of Table 13 contains the implied increase in costs if I were to only consider each subsample independently. Though the point estimate is noisily estimated due to the large numbers of individuals who have inpatient and outpatient spending that are not affected by a reduction in opioids, the estimate for all 5,113,535 enrollees in Column (1) is very close to the estimate for the combined sample enrollees in Column (5), indicating that my combined predictive subsample may mostly capture the individuals in the Truven dataset who have needs for pain management.

a proxy for levels of underlying abuse. These estimates suggest that one overdose death represents approximately 10 treatment admissions and 32 emergency department visits, and incarceration costs per opioid user are estimated to be approximately 3 times hospital and ED costs (Center for Integrated Behavioral Health Policy, 2011; HCCI, 2013; Kassed et al., 2007). As such, my estimates of 1,103 deaths averted suggest 11,030 treatment admissions were prevented at an average cost of $7,230 per admission. This figure totals to $79.7 million. Similarly, there are approximately 32 fewer emergency department visits for every overdose death, so 35,296 ED visits were prevented at an average cost per ED visit of $1200. This figure totals to $42.4 million. Finally, incarceration costs per user, estimated at approximately 3 times hospital and ED costs, total to $127.1 million. In total, the benefits from reduced opioid addiction, overdose, and death can be roughly valued at $7.3 billion. This is a smaller figure, but on the same order of magnitude, as the $12.1 billion figure for costs derived above.

The above two numbers are subject to a number of major limitations. Costs may be underestimated for many reasons. First, I calculate costs of restricting opioids based on a subpopulation that is most in need of pain management; I exclude the lost work of people in acute (temporary) pain, and all other individuals who need pain management but do not fall into my group of predictive enrollees. Second, I do not measure lost work output for any individuals other than those injured in workers' compensation or on short term disability. Third, I cannot observe the direct psychological costs of increased pain. As explored in Case and Deaton (2015a) and Case and Deaton (2015b), increasing pain among certain subsets of the US population, especially middle-aged white men, is associated with increased distress, declining mental health, and increased mortality, including suicide.

Benefits may also be underestimated. Costs to society of drug abuse, addiction, overdose, and death accrue in many forms, such as increased need for family services. There are also non-monetary costs in the form of suffering by addicted individuals, their families, and their community. Additionally, by considering overdose deaths from opioids and heroin in aggregate, I neglect any differentially worse effects of heroin use versus opioid use which appear in the short run as individuals temporarily substitute towards heroin in the first 1-2 years. Of particular concern are the societal costs associated with injection drug use, especially transmission of HIV and Hepatitis C.[50,51]

Finally, I consider what these estimates suggest about the *private* welfare tradeoffs facing pain patients. As discussed above, there is controversy in the medical and public health communities over

[50] Opioid abuse can also be associated with injection drug use; for example, the recent spread of HIV in rural Indiana due to injection of the prescription drug Opana.

[51] The cost of one additional Hepatitis C infection can be approximately monetized; it costs approximately $80,000 for a course of treatment to cure the infection.

how to best understand opioid abuse and opioid harms. Some practitioners believe that opioids can be used safely by individuals with legitimate medical need, and opioid abuse is primarily a result of diversion and theft from the legitimate medical care system. However, an emerging alternative viewpoint is that widespread legitimate medical opioid use is the primary driver of opioid abuse, addiction, overdose, and death - that the medical care system is producing "iatrogenic addiction" and is thus largely responsible for opioid-related harms.

If the first story dominates, there would be a tradeoff in the incidence of costs and benefits of reduced overall access to opioids, from the perspective of social welfare. If we reduce access to opioids to people in need (mostly the middle-aged), their welfare may go down as they are unable to work and as they substitute towards alternate, possibly less effective, pain management. The benefits of regulation accrue to the individuals (perhaps their children or grandchildren) who are less able to access diverted drugs, and consequently are less likely to experience addiction and death. If the second story dominates, the middle-aged are experiencing both the costs of regulation, in terms of lost work and functionality, and the benefits, in that they are themselves less likely to be come addicted, overdose, and die.

In Tables 15, A19, and A20, I provide suggestive evidence primarily in support of the latter story. I disaggregate each of my main results according to demographic variables; for all datasets I can consider results by sex and age group. (There is considerable evidence that pain is closely linked to poverty; to assess this I utilize coarse employment variables in Truven, including union/non-union and salaried/hourly, and race and education variables in the CDC death data.) I find suggestive evidence that, broadly speaking the middle aged and blue-collar workers are both the primary beneficiaries of reduced opioid use, through reduced overdose deaths, and primarily bear the costs, in terms of reduced work and functionality.

Though the costs and benefits of a reduction in opioid prescribing appear to mostly accrue to the same groups - the middle aged - this result does not speak to whether doctors and patients are currently making optimal tradeoffs over the use of prescription opioids. The overall overdose death rate for prescription opioids is roughly 26 per 100,000 past-year users (where the denominator is all individuals prescribed an opioid in the past year, and the numerator is overdose deaths), or .026%; studies attempting to estimate this number for chronic (long-term) opioid users have found that the overdose death rate is approximately 0.29% for men and 0.12% for women (Kaplovitch et al., 2015). And, as documented in Section 7, the costs to an individual of not using opioids, in terms of increased pain and reduced function, may be large.

9 Conclusion

This study exploits a natural policy experiment, state-level variation in the timing of the introduction of Prescription Monitoring Programs, to examine the costs and benefits of reducing access to opioids. The findings of this study are several. First, PMPs, an increasingly important policy lever for reducing access to opioids, do indeed reduce the use of opioids, and achieve a major policy success in reducing opioid overdose deaths. However, they have had several unintended consequences. Heroin overdose deaths temporarily rise before falling, as some individuals substitute from prescription opioids to heroin. This compensatory effect is strong enough to offset the gains from reducing prescription opioid overdose deaths for the first one to two years after a crackdown. Second, pain management is altered. Individuals experience more pain in the hospital setting. As access to prescription opioids is curtailed, chronic pain patients substitute towards alternate and more costly forms of medical pain management, especially surgery. Finally, functionality and ability to work decline: days absent for injured and disabled workers increase, creating clear welfare losses for those individuals, and suggesting that other, unobserved, welfare losses attributable to losing access to pain management (in terms of functionality and overall life satisfaction) are also likely.

Back-of-the-envelope calculations suggest that the regulation of prescription opioids involves costs and benefits that are both large: welfare losses from a crackdown on opioids are on the order of $12.1 billion in increased medical spending and lost wages, whereas welfare gains are on the order of $7.3 billion, from reduced addiction, overdose, and death.

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Table 1: Summary statistics: ARCOS

MME per Resident per Quarter	Mean	Standard Deviation	Minimum	Median	Maximum
Oxycodone	56.2	31.5	8.5	50.1	285.5
Other Schedule II :					
Fentanyl	25.8	9.2	5.0	26.3	56.0
Hydromorphone	3.5	2.2	0.3	3.0	16.3
Meperidine	1.2	0.9	0.1	1.0	5.3
Morphine	15.5	7.1	3.6	14.5	41.4
Total Schedule II	102.3	44.3	24.2	97.6	344.7
Hydrocodone	22.1	13.6	3.3	19.6	76.5
Other Schedule III-V :					
Codeine	1.4	0.4	0.6	1.4	4.5
Total Schedule III-V	23.5	13.5	4.1	20.9	77.6

Notes: Source is Drug Enforcement Administration Automated Reports and Consolidated Orders System (ARCOS). Summary statistics for 38 states in sample depicted in Table A1. Data spans years 2000 to 2013. Number of observations: 2128. Oxycodone is Schedule II, while hydrocodone is Schedule III. Schedule II and III drugs are both defined as having " a currently accepted medical use in treatment in the United States," and a "potential for abuse and dependence." Schedule III drugs are distinguished from Schedule II as less abusable, and less likely to produce physical and psychological dependence. Schedule III drugs are easier to prescribe and obtain.

Table 2: Summary Statistics, Truven

Panel A: Percentage of enrollees by year with prescription for opioid pain relievers

Year	Oxycodone	Hydrocodone	Schedule II excl. oxy	Schedule III & below excl. hydro	All opioids	Total enrollees
2003	5.0	13.8	0.8	8.8	22.5	281,637
2004	5.5	14.9	1.0	9.5	24.0	599,017
2005	6.0	15.9	1.0	9.6	25.3	822,167
2006	5.8	15.4	0.9	8.6	24.2	1,057,487
2007	5.9	15.7	0.9	8.6	24.5	1,221,408
2008	6.2	15.7	0.8	9.7	25.4	1,579,009
2009	6.5	15.6	0.8	9.7	25.6	2,079,528
2010	6.4	14.9	0.8	8.7	24.2	2,527,267
2011	6.5	15.3	0.9	8.1	24.3	2,686,926
2012	6.2	14.7	0.8	7.8	23.5	2,774,767
All Years	14.2	29.7	2.0	18.7	42.2	5,113,535

Panel B: Dosage by time elapsed in opioid therapy episode

Days elapsed	Hydrocodone (average MME per day in script)	Oxycodone (average MME per day in script)
Days 0 to 15	33.3	39.6
Days 15 to 30	27.8	40.2
Days 30 to 60	28.4	45.6
Days 60 to 90	31.1	56.3
Days 90 to 120	32.8	65.1
Days 120 to 150	33.7	69.3
Days 150 to 180	34.3	73.3
Days 180 to 365	36.0	83.0
Days 365 and after	37.9	103.4

Notes: Panel A displays the percent of enrollees in 38-state sample with an oxycodone, hydrocodone, etc. prescription in each year, as well as over all years spent in the Truven Marketscan prescription drug claims claims dataset. Panel B displays the average MME prescribed for prescriptions that are earlier or later in an opioid therapy episode, where an opioid therapy episode is as defined in the text.

Table 3: The Effect of PMP Introduction on Schedule II Opioids Distributed (ARCOS)

	(1) MME per person per quarter (all Sch. II)
1(PMP)_*st*	-11.35**
	(5.484)
Controls:	
State Controls	Y
State FE	Y
Quarter FE	Y
State Linear Trends	N
Adjusted R^2	0.890
Clusters	38
Observations	2,128
Mean	102.26

Notes: The dependent variable is the sum of Morphine Milligram Equivalent (MME) per resident per quarter for all Schedule II drugs: oxycodone, fentanyl, hydromorphone, meperidine, and morphine, as measured in the ARCOS dataset. State controls include state-level population, unemployment rate, and percentage of population over 60. Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: The Effect of PMP Introduction on Opioid and Heroin Overdose Deaths

	(1) Opioid Overdose Deaths (T40.2)	(2) Heroin Overdose Deaths (T40.1)	(3) Combined Opiate Deaths (T40.1-T40.4)
1(PMP)_st	-0.125**	0.0130	-0.0769
	(0.0533)	(0.0670)	(0.0600)
Controls:			
County Controls	Y	Y	Y
County FE	Y	Y	Y
Quarter FE	Y	Y	Y
State Linear Trends	N	N	N
Adjusted R^2	0.822	0.804	0.851
Clusters	38	38	38
Observations	131,604	131,604	131,604
Counties	2,268	2,268	2,268
Mean	0.6	0.2	1.1

Notes: Dependent variables are ln(opioid overdose deaths + 1) in a county for a given quarter. Opioid and heroin overdose deaths are coded according to standard definitions. Drug overdoses are defined as deaths with underlying causes of death codes X40-X44, X60-X64, X85, Y10-Y14. Prescription opioid overdoses are defined as deaths with multiple cause of death codes T40.2. Heroin overdoses are defined as deaths with multiple cause of death code T40.1. Combined Heroin and Opioid Overdose deaths are defined as deaths with multiple cause of death codes T40.1-T40.4. The baseline specification for CDC overdose deaths results includes county-level controls for population, unemployment rate, and percentage of population over 60, and observations are weighted according to population. Means are unweighted and listed in levels (deaths). Robust standard errors, clustered at the state level, in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: The Effect of PMP Introduction on Schedule II Opioids Distributed (Truven)

	(1) ln(MME in quarter + 1)	(2) binary (0/1) in quarter	(3) MME in quarter
1(PMP)_ist	-0.00496***	-0.000595**	-6.288**
	(0.00168)	(0.000234)	(2.875)
Controls:			
Ind. Controls	Y	Y	Y
State Controls	N	N	N
Individual FE	Y	Y	Y
Quarter FE	Y	Y	Y
State FE	N	N	N
State Linear Trends	N	N	N
Adjusted R^2	0.324	0.209	0.649
Clusters	38	38	38
Observations	56,668,472	56,668,472	56,668,439
Individuals	5,113,535	5,113,535	5,113,535
Mean	0.154	0.025	60.79

Notes: The dependent variables are ln(Morphine Milligram Equivalent (MME) for Schedule II drugs + 1) per quarter for each individual enrollee in Column 1, a binary indicator (0/1) for whether the enrollee was prescribed a prescription containing a Schedule II drug in that quarter in Column 2, and raw MME prescribed in levels in Column 3. For Truven data, Schedule II drugs are oxycodone, fentanyl, hydromorphone, meperidine, and morphine. The baseline specification includes individual and quarter fixed effects, and individual-level controls (age, employment type/status (salaried, union, full time, etc.), and industry (oil and gas, manufacturing, etc.). Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: The Effect of PMP Introduction on First Prescriptions

	(1) Schedule II drugs, initial script (0/1)	(2) Schedule II drugs, initial script (ln mme + 1)
1(PMP)_ist	-0.000353*	-0.00273**
	(0.000181)	(0.00116)
Controls:		
Ind. Controls	Y	Y
State Controls	N	N
Individual FE	Y	Y
Quarter FE	Y	Y
State FE	N	N
State Linear Trends	N	N
Adjusted R^2	0.090	0.106
Clusters	38	38
Observations	56,668,472	56,668,472
Individuals	5,113,535	5,113,535
Mean	0.021	0.121

Notes: Scripts are grouped into a single episode according to whether they have the same generic ingredient and if they follow a previous script that was within a period not exceeding three times the days supply of the previous script. An initial script is defined as the first script in that episode. Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: The Effect of PMP Introduction on the Opioid Prescribing Distribution

	(1) Binary (0/1) for enrollee dispensed amount betw. thresholds in quarter	
Panel 1: No Sch. 2 opioids	0.00060**	(0.00023)
Panel 2: > 0 to 119 MME	0.00003	(0.00005)
Panel 3: 120 to 149 MME	-0.00000	(0.00002)
Panel 4: 150 to 225 MME	-0.00009**	(0.00004)
Panel 5: 226 to 298 MME	0.00004	(0.00005)
Panel 6: 299 to 419 MME	-0.00001	(0.00006)
Panel 7: 420 to 562 MME	-0.00003	(0.00004)
Panel 8: 563 to 898 MME	-0.00005	(0.00004)
Panel 9: 899 to 2,024 MME	-0.00010	(0.00006)
Panel 10: 2,025 to 6,073 MME	-0.00014*	(0.00007)
Panel 11: 6,074 to 129,600 MME	-0.00025**	(0.00009)
Controls:		
Ind. Controls	Y	
State Controls	N	
Individual FE	Y	
Quarter FE	Y	
State FE	N	
State Linear Trends	N	
Clusters	38	
Observations	56,668,472	
Individuals	5,113,535	

Notes: The dependent variable is a binary (0/1) indicator for whether enrollee had a quantity of oxycodone falling in a given bin, defined by the listed thresholds, in each quarter. Thresholds are set roughly according to the deciles of *positive* observations of oxycodone in each quarter. Each bin contains captures approx. 130,000 positive observations. For scale, 6,074 MME per quarter would represent approximately 70 MME per day for the entire quarter; thus, the threshold for Bin 10 represents an individual on chronic daily opioid therapy, with moderately high dosage. Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: The Effect of PMP Introduction on Percent of Hospital Patients Reporting Pain was Sometimes/Never Well-Controlled (HCAHPS)

	(2) Pre-post Specification	(3) Event Study Specification
1(PMP)_st	0.129	
	(0.109)	
4 years before		-0.0250
		(0.211)
3 years before		-0.0233
		(0.193)
2 years before		-0.0999
		(0.147)
1 year before		0.0647
		(0.0935)
Year of implementation		0
		(.)
1 year after		0.181*
		(0.0940)
2 years after		0.299**
		(0.140)
3 years after		0.381**
		(0.185)
4 years after		0.597**
		(0.263)
Controls:		
State Controls	Y	Y
State FE	Y	Y
Quarter FE	Y	Y
State Linear Trends	Y	N
Adjusted R^2	0.938	0.915
Clusters	38	38
Observations	912	912
Mean	7.4	7.4

Notes: The dependent variable is percent of survey respondents answering that their pain was "sometimes" or "never" well-controlled during their hospital visit on an HCAHPS patient satisfaction survey, for a given state in a given quarter. Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: The Effect of PMP Introduction on Inpatient and Outpatient Spending

	(1) All Enrollees	(2) Predictive enrollees	(3) WC injured, days absent	(4) Short Term Disabled	(5) Combined Sample
1(PMP)_ist	0.0177	0.0577**	0.0806***	0.0676**	0.0567**
	(0.0181)	(0.0214)	(0.0283)	(0.0299)	(0.0220)
Controls:					
Ind. Controls	Y	Y	Y	Y	Y
State Controls	N	N	N	N	N
Individual FE	Y	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y	Y
State FE	N	N	N	N	N
State Linear Trends	N	N	N	N	N
Adjusted R^2	0.325	0.249	0.317	0.275	0.278
Clusters	38	38	38	38	38
Observations	56,668,472	4,973,246	892,245	2,452,826	6,368,111
Individuals	5,113,535	301,774	44,418	157,772	418,212
Mean	859.5	3586.3	1035.51	2726.17	2913.04

Notes: Dependent variable is ln(total $ inpatient and outpatient spending + 1). Predictive enrollee subsample is obtained using a model to identify chronic pain patients from inpatient and outpatient diagnosis codes, as is described in the text. WC and short term disabled samples are as defined for Table 12. The combined predictive subsample includes all enrollees from Models (2), (3), and (4). Some enrollees appear in multiple predictive groups. Means are listed in levels ($). Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: The Effect of PMP Introduction on Inpatient and Outpatient Spending by Type (Combined Predictive Sample)

	(1) PT	(2) Interven-tional	(3) Surgery	(4) Chiropractic	(5) Office Visits	(6) Non-opioid drugs
1(PMP)_*ist*	0.00897	-0.00435	0.0126**	0.0111*	0.0500**	0.0201
	(0.00802)	(0.0101)	(0.00607)	(0.00601)	(0.0229)	(0.0162)
Controls:						
Ind. Controls	Y	Y	Y	Y	Y	Y
State Controls	N	N	N	N	N	N
Individual FE	Y	Y	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y	Y	Y
State FE	N	N	N	N	N	N
State Linear Trends	N	N	N	N	N	N
Adjusted R^2	0.217	0.183	0.202	0.398	0.269	0.603
Clusters	38	38	38	38	38	38
Observations	6,368,111	6,368,111	6,368,111	6,368,111	6,368,111	6,368,111
Individuals	418,212	418,212	418,212	418,212	418,212	418,212
Mean	82.79	272.16	331.34	20.5	135.03	446.4

Notes: Dependent variable is ln(total $ inpatient and outpatient spending + 1), except for Model 6, where dependent variable is ln($ drug spending +1) for every prescription drug claim except those identified as prescription opioids. Coding of type of spending is detailed in the online appendix, and is based on details on each claim, including listed diagnostic codes, CPT procedure codes, physician codes, procedure groups, etc. Enrollees are from the combined predictive sample, as is defined in Table 9. Means are listed in levels ($). Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 11: The Effect of PMP Introduction on Takeup of Workers' Compensation and Short Term Disability

	(1) Whether worker takes WC (0/1)	(2) Whether WC injury results in days lost (0/1)	(3) Whether worker takes disability (0/1)
1(PMP)_*ist*	-0.000223	-0.000131	0.000170
	(0.000308)	(0.000708)	(0.000450)
Controls:			
Ind. Controls	Y	Y	Y
State Controls	N	N	N
Individual FE	Y	Y	Y
Quarter FE	Y	Y	Y
State FE	N	N	N
State Linear Trends	N	N	N
Adjusted R^2	0.034	0.027	0.050
Clusters	38	38	38
Observations	56,668,472	4,053,277	56,668,472
Individuals	5,113,535	218,667	5,113,535
Mean	0.005	0.019	0.014

Notes: Dependent variables are binary (0/1) for whether worker makes a workers' compensation (Model (1)) or short term disability (Model (3)) claim, and binary (0/1) for whether a workers' compensation injury results in days missed (Model (2)). Model 1 considers the full Truven sample, and is 1 if an enrollee makes a workers' compensation claim under WC in the quarter. Model 2 considers the subsample of enrollees ever injured under workers' compensation, and is 1 if a worker makes a WC claim in the quarter which ever results in days absent. Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: The Effect of PMP Introduction on Days Absent in Workers' Compensation and Short Term Disability

	(1) WC injured, days absent	(2) Short Term Disabled	(3) WC plus Short Term Disabled
1(PMP)_*ist*	0.0651**	0.0324***	0.0377***
	(0.0312)	(0.0108)	(0.0128)
Controls:			
Ind. Controls	Y	Y	Y
State Controls	N	N	N
Individual FE	Y	Y	Y
Quarter FE	Y	Y	Y
State FE	N	N	N
State Linear Trends	N	N	N
Adjusted R^2	0.257	0.193	0.207
Clusters	38	38	38
Observations	947,070	2,501,778	3,230,567
Individuals	44,475	158,521	193,320
Mean	8.17	5.06	5.57

Notes: The dependent variable is ln(days absent in quarter under workers' compensation or short term disability + 1) for Models 1-3. Model 1 considers the subsample of enrollees who ever miss a day under workers' comp. Model 2 considers the subsample of enrollees who take disability using a case dx that is predicted to mean they require pain management. Model 3 combines these two samples. Note that while most enrollees in the workers' compensation sample are accruing days missed under a workers' compensation claim (and similarly for disability) there are some enrollees that miss work under both programs. Means are listed in levels (days). Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 13: The Effect of PMP Introduction on Total Costs (Inpatient, Outpatient, Drug, Lost Wages, WC Medical)

	(1) All Enrollees	(2) Predictive enrollees	(3) WC injured, days absent	(4) Short Term Disabled	(5) Combined Sample
1(PMP)_ist	0.0137	0.0443**	0.0971**	0.0718**	0.0522**
	(0.0145)	(0.0168)	(0.0424)	(0.0272)	(0.0209)
Controls:					
Ind. Controls	Y	Y	Y	Y	Y
State Controls	N	N	N	N	N
Individual FE	Y	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y	Y
State FE	N	N	N	N	N
State Linear Trends	N	N	N	N	N
Adjusted R^2	0.483	0.359	0.390	0.372	0.375
Clusters	38	38	38	38	38
Observations	56,668,472	4,973,246	892,245	2,452,826	6,368,111
Individuals	5,113,535	301,774	44,418	157,772	418,212
Mean	1201.91	4726.51	2716.74	3895.34	4035.18
Implied increased costs (billions)	11.5	8.7	1.6	6.0	12.1

Notes: Dependent variable is ln(total inpatient spending, outpatient spending, drug spending, imputed lost wages under workers' compensation and short term disability, and imputed medical costs accrued to workers' compensation insurer +1), for the same samples as described for Table 9. Means are listed in levels ($). Implied total costs in billions are calculated as described in text. Robust standard errors, clustered at the state level, in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 14: 2SLS Estimates for the Effect of Availability of Schedule II Opioids on Work Output, Inpatient and Outpatient Spending, and Total Costs

	OLS			IV		
	(1) ln(days abs+1)	(2) ln(spending +1)	(3) ln(tot costs+1)	(4) ln(days abs+1)	(5) ln(spending +1)	(6) ln(tot costs+1)
$\ln(MME\_it+1)$	0.166***	0.509***	0.430***	-0.740**	-2.228**	-1.990**
	(0.00739)	(0.00968)	(0.00799)	(0.376)	(1.042)	(0.969)
Instruments:						
Dummies for 16 Quarters Post-PMP				Y	Y	Y
Controls:						
Ind. Controls	Y	Y	Y	Y	Y	Y
State Controls	N	N	N	N	N	N
Individual FE	Y	Y	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y	Y	Y
State FE	N	N	N	N	N	N
State Linear Trends	N	N	N	N	N	N
Adjusted R^2	0.278	0.329	0.419			
Clusters	38	38	38	38	38	38
Observations	6,147,880	6,147,880	6,147,880	6,144,184	6,144,184	6,144,184
Individuals	405,621	405,621	405,621	401,925	401,925	401,925
F-Statistic (excl. instruments)				7.8	7.8	7.8
Mean	3.6	2898.7	4035.2	3.6	2898.7	4035.2

Notes: The dependent variables are ln(days absent in quarter under workers' compensation or short term disability + 1), ln(inpatient and outpatient spending+1), or ln(total costs + 1) per quarter for each individual enrollee, and the independent variable is ln(MME schedule II opioids + 1) per quarter for each individual enrollee. Models (4-6) use 2SLS with dummies for the 16 quarters post-PMP implementation as instruments for MME Schedule II opioids dispensed. Means are listed in levels ($) for spending/costs, and days for days absent. Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 15: The Effect of PMP Introduction on Total Costs and Overdose Deaths, by Age and Gender

Panel A: Costs

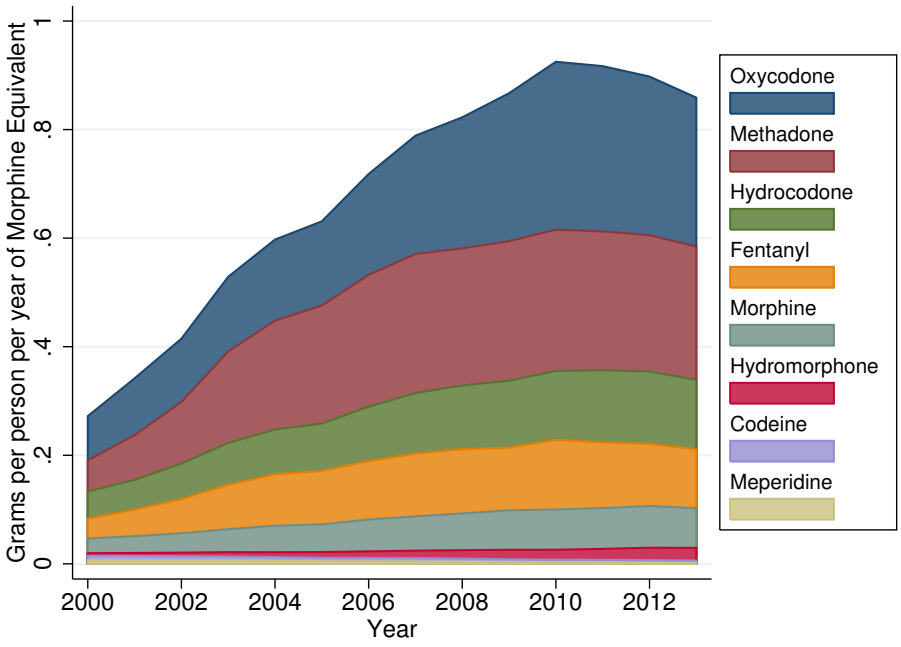
			Mean	Individuals
Panel 1: Men, 17-28	-0.023	(0.045)	2,818	20,215
Panel 2: Men, 29-40	0.062*	(0.032)	3,037	60,388
Panel 3: Men, 41-52	0.052**	(0.023)	3,738	108,813
Panel 4: Men, 53-64	0.046**	(0.018)	5,067	91,318
Panel 5: Women, 17-28	0.061	(0.040)	3,232	26,130
Panel 6: Women, 29-40	0.065***	(0.022)	3,564	71,829
Panel 7: Women, 41-52	0.047*	(0.025)	4,007	93,306
Panel 8: Women, 53-64	0.038	(0.029)	5,055	63,773

Panel B: Deaths

			Mean
Panel 1: Men, 17-28	-0.03284	(0.026)	0.061
Panel 2: Men, 29-40	-0.07829**	(0.035)	0.091
Panel 3: Men, 41-52	-0.08929**	(0.036)	0.123
Panel 4: Men, 53-64	-0.04318	(0.033)	0.053
Panel 5: Women, 17-28	-0.04022**	(0.019)	0.021
Panel 6: Women, 29-40	-0.04743***	(0.016)	0.049
Panel 7: Women, 41-52	-0.09391**	(0.038)	0.087
Panel 8: Women, 53-64	-0.06293**	(0.028)	0.046

Notes: The above specifications are as in baseline specifications shown in Tables 13 (costs) and 4 (deaths). Costs specification is run on split sample, by characteristics, and dependent variable is ln(total costs + 1). Deaths specification utilizes ln(opioid overdose deaths [T40.2] for the demographic group listed + 1) as the dependent variable. Means are in levels ($ and deaths). Robust standard errors, clustered at the state level, in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 1: Grams Morphine Equivalent Distributed Per Person



Notes: Source: DEA ARCOS. Total distributed of oxycodone, methadone, hydrocodone, fentanyl, morphine, hydromorphone, codeine, and meperidine, summed according to equianalgesic potency (morphine equivalent). For years 2000-2002, total grams for methadone are partially imputed, because only methadone distributed for pain relief was reported by the ARCOS system, while in subsequent years methadone numbers include methadone distributed to narcotic addiction treatment programs.

Figure 2: Prescription Opioid Overdose Deaths

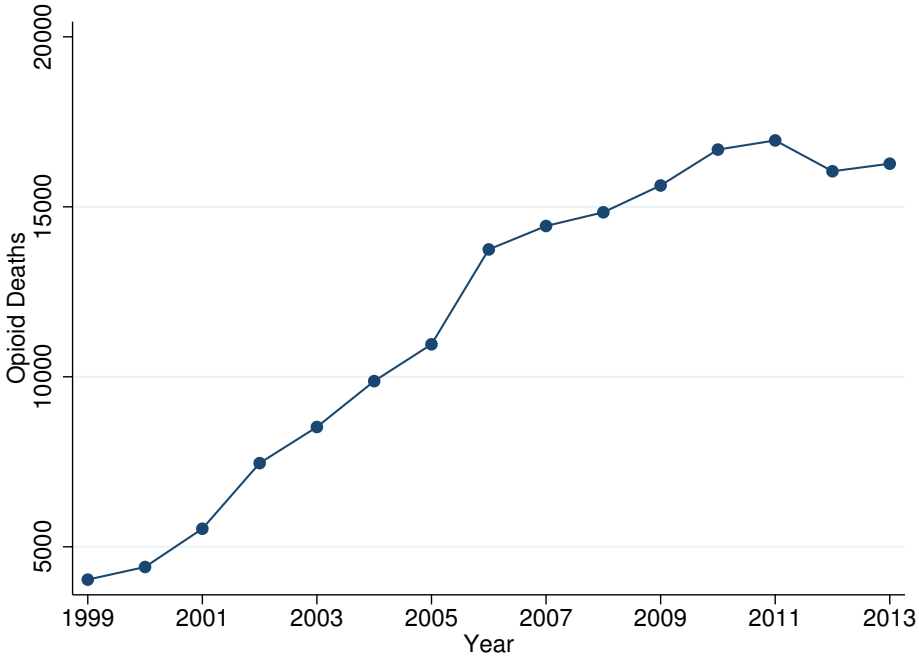


Figure 3: Heroin Overdose Deaths

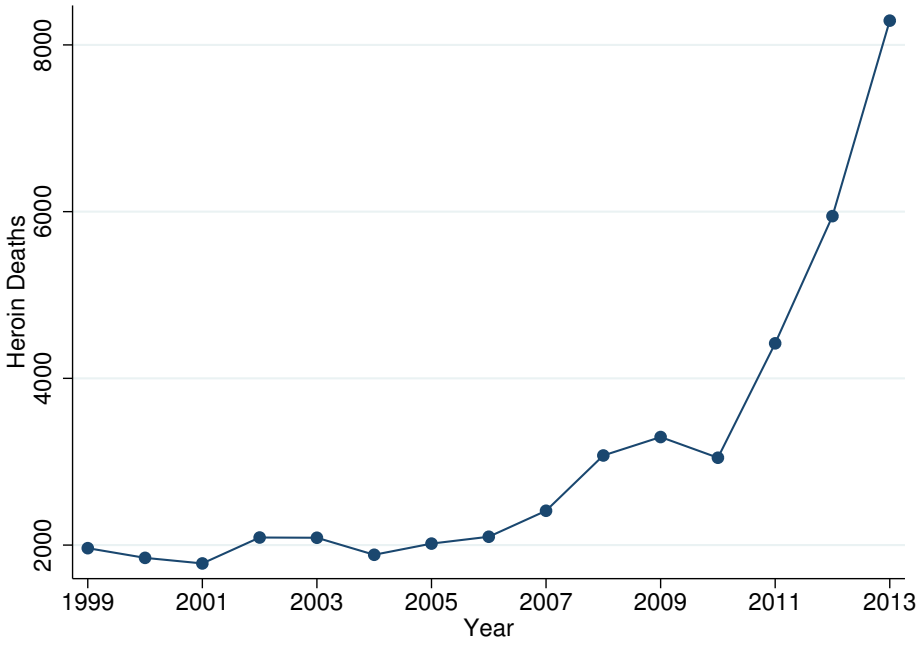
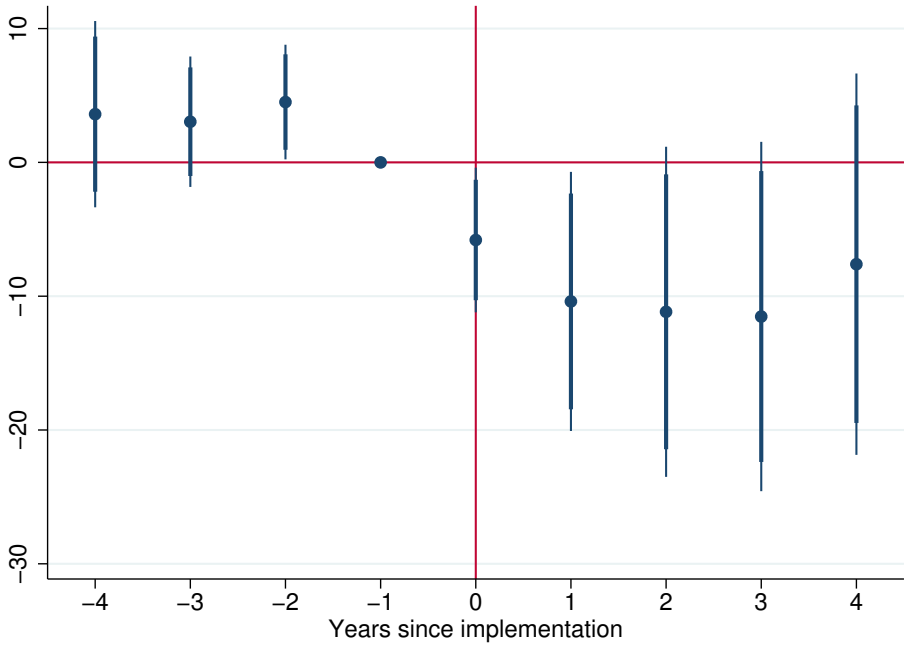
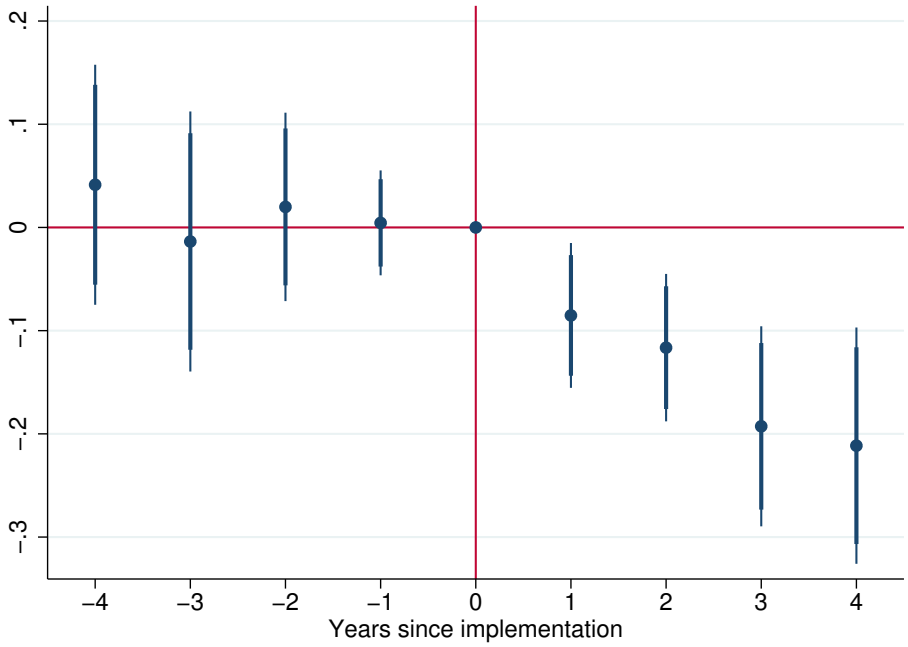


Figure 4: The Effect of PMP Introduction on Schedule II Opioids Distributed (ARCOS)



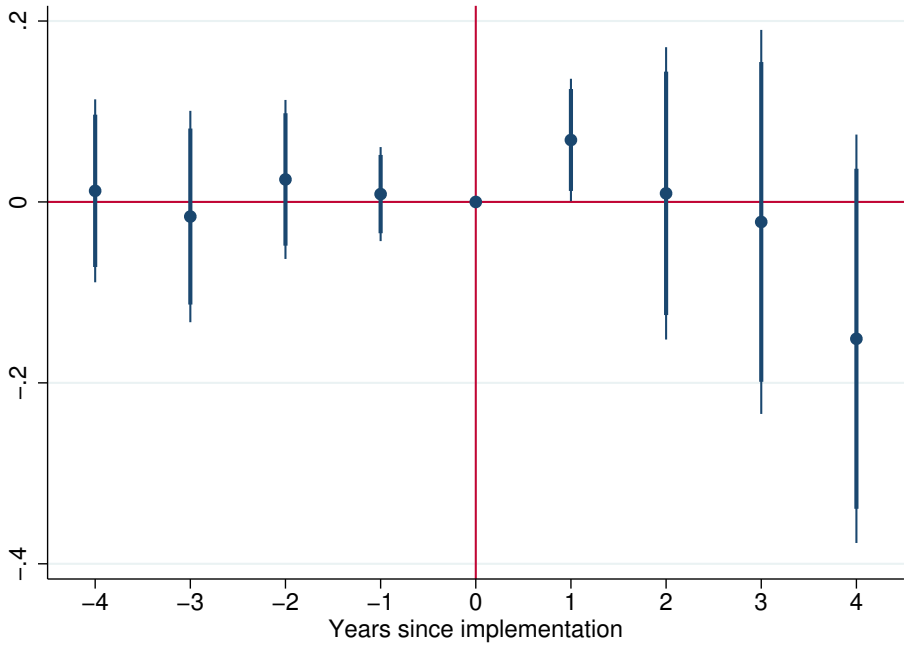
Notes: ARCOS event study analogue of baseline specification for Schedule II opioids displayed in Table 3, Model (1). Robust standard errors clustered at state level. Thin bars represent 95% confidence intervals and thick bars represent 90% confidence intervals.

Figure 5: The Effect of PMP Introduction on Opioid Overdose Deaths



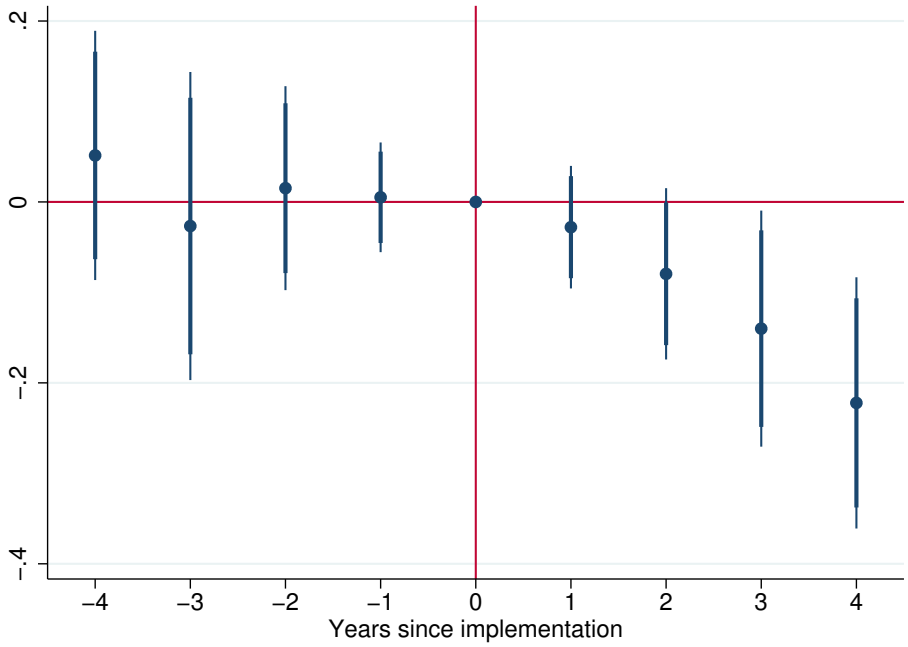
Notes: CDC event study analogue for Model (1) in Table 4, where dependent variable is ln(opioid [T40.2] overdose deaths + 1). Robust standard errors clustered at state level. Thin bars represent 95% confidence intervals and thick bars represent 90% confidence intervals.

Figure 6: The Effect of PMP Introduction on Heroin Overdose Deaths



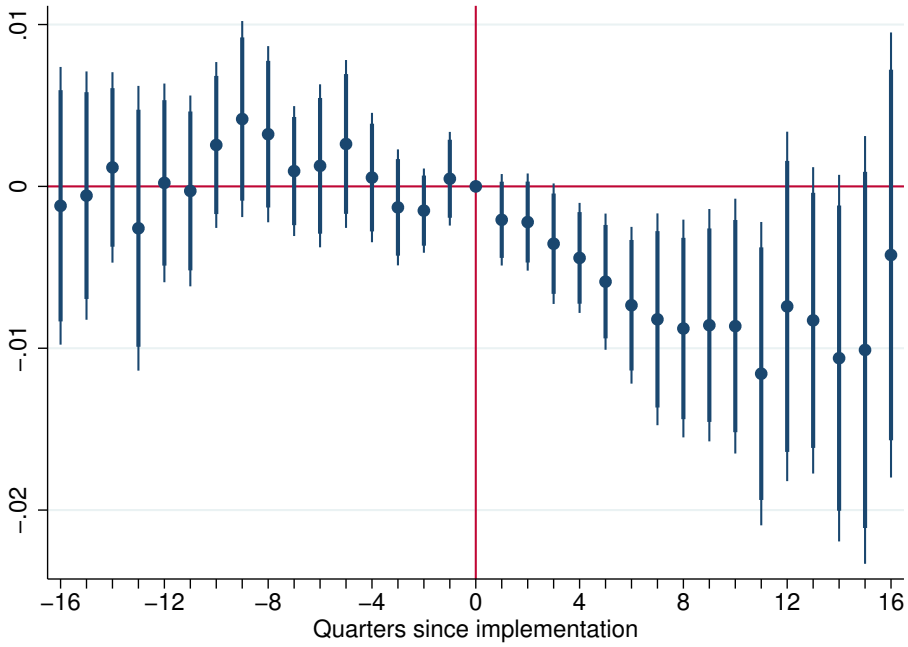
Notes: CDC event study analogue for Model (2) in Table 4, where dependent variable is ln(heroin [T40.1] overdose deaths + 1). Robust standard errors clustered at state level. Thin bars represent 95% confidence intervals and thick bars represent 90% confidence intervals.

Figure 7: The Effect of PMP Introduction on All Opioid/Opiate Overdose Deaths



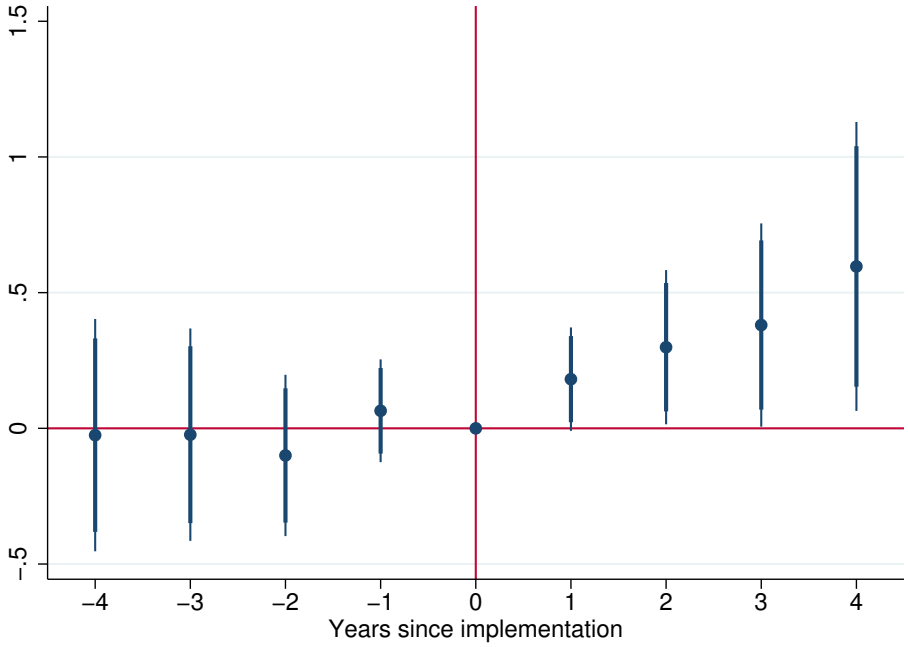
Notes: CDC event study analogue for Model (3) in Table 4, where dependent variable is ln(all opiate [T40.1-T40.4] overdose deaths + 1). Robust standard errors clustered at state level. Thin bars represent 95% confidence intervals and thick bars represent 90% confidence intervals.

Figure 8: The Effect of PMP Introduction on Schedule II Opioids Distributed (Truven)



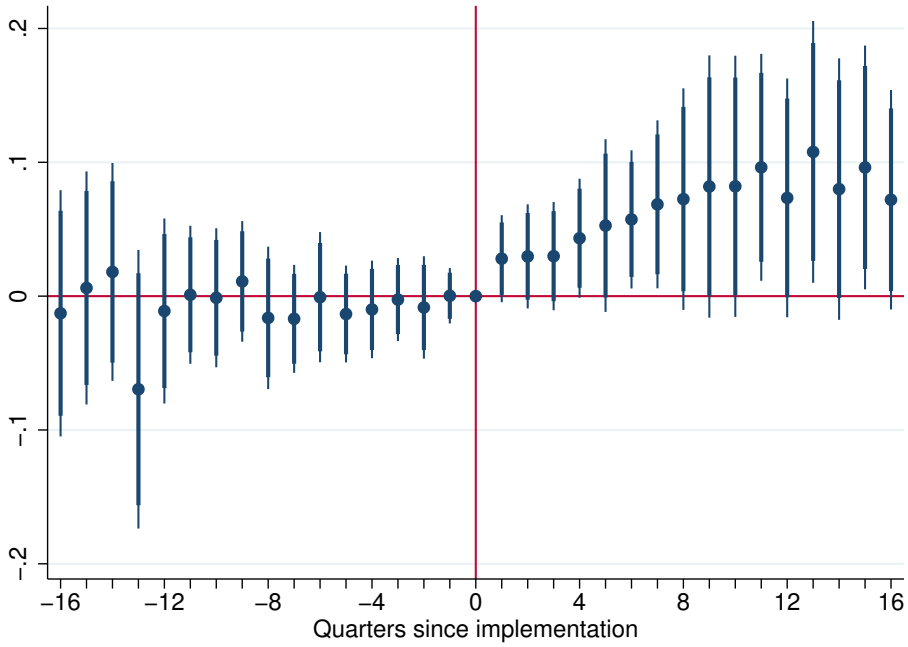
Notes: Truven event study analogue of baseline specification for oxycodone displayed in Table 5, Model (1). Robust standard errors clustered at state level. Thin bars represent 95% confidence intervals and thick bars represent 90% confidence intervals.

Figure 9: The Effect of PMP Introduction on Percent of Hospital Patients Reporting Pain was Sometimes or Never Well-controlled (HCAHPS)



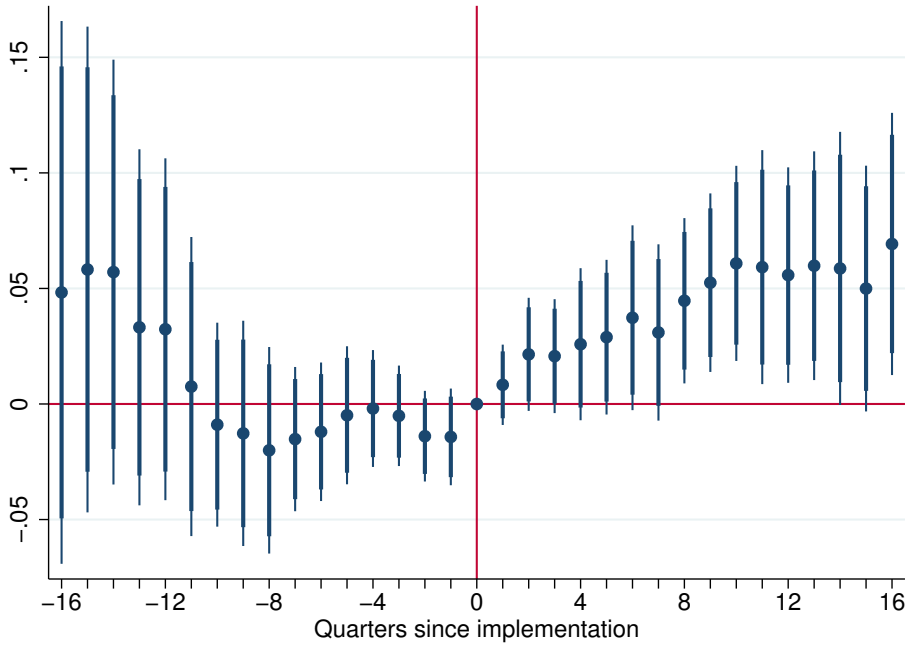
Notes: Event study analogue for HCAHPS patient satisfaction survey question: "During your hospital stay, how often was your pain sometimes or never well-controlled?" Robust standard errors clustered at state level. Thin bars represent 95% confidence intervals and thick bars represent 90% confidence intervals.

Figure 10: The Effect of PMP Introduction on Total Inpatient and Outpatient Spending, Combined Predictive Sample



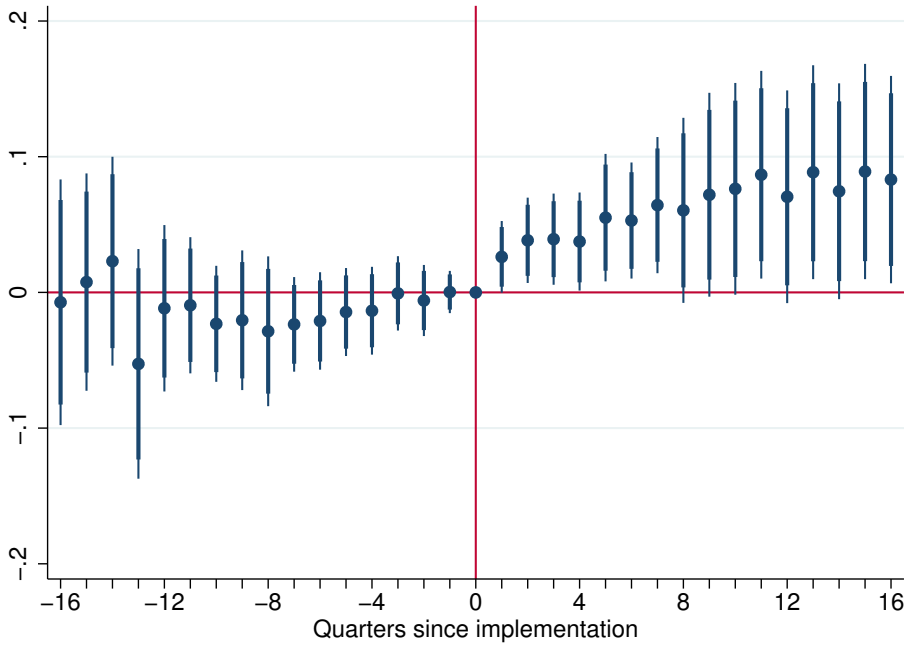
Notes: Truven event study analogue for Model (5) in Table 9. Combined predictive subsample. Robust standard errors clustered at state level. Thin bars represent 95% confidence intervals and thick bars represent 90% confidence intervals.

Figure 11: The Effect of PMP Introduction on Days Absent Under Workers' Compensation and Short Term Disability



Notes: Truven event study analogue for Model (3) in Table 12. Sample is workers injured under workers' comp and short term disabled as is described for Table 12, Model (3). Robust standard errors clustered at state level. Thin bars represent 95% confidence intervals and thick bars represent 90% confidence intervals.

Figure 12: The Effect of PMP Introduction on Total Costs, Combined Predictive Sample



Notes: Truven event study analogue for Model (5) in Table 13. Combined predictive subsample. Robust standard errors clustered at state level. Thin bars represent 95% confidence intervals and thick bars represent 90% confidence intervals.

10 Appendix

Table A1: Date of Implementation of State PMP (Main Sample)

Wyoming	Jan-2004	California	Jan-2009	South Dakota	Mar-2012
Maine	Jan-2005	Louisiana	Jan-2009	New Mexico	Aug-2012
Mississippi	Dec-2005	Iowa	Mar-2009	Delaware	Aug-2012
Virginia	Jun-2006	Vermont	Apr-2009	Montana	Oct-2012
Oklahoma	Jul-2006	Minnesota	Apr-2010	Arkansas	Mar-2013
Indiana	Jan-2007	Massachusetts	Aug-2010	Wisconsin	May-2013
North Dakota	Jan-2007	Kansas	Apr-2011	Georgia	Jul-2013
Alabama	Apr-2007	Oregon	Sep-2011	Maryland	Jan-2014
North Carolina	Oct-2007	Florida	Oct-2011	Nebraska	
Colorado	Feb-2008	Ohio	Oct-2011	Pennsylvania	
South Carolina	Jun-2008	Alaska	Jan-2012	New Hampshire	
Connecticut	Jul-2008	New Jersey	Jan-2012	Missouri	
Arizona	Dec-2008	Washington	Jan-2012		

Table A2: Year of Implementation of Early State Controlled Substances Monitoring

New York	1973
Texas	1989
Tennessee	1990
West Virginia	1995
Hawaii	1996
Utah	1997
Nevada	1997
Idaho	1998
Michigan	1998
Kentucky	1999
Illinois	1999
Rhode Island	2001

Notes: Source: National Alliance for Model State Drug Laws (NAMSDL).

Table A3: The Effect of PMP Introduction on Opioids Distributed by Drug (ARCOS)

	(1) Oxycodone	(2) Total Schedule II excl. Oxycodone	(3) Hydrocodone
1(PMP)_st	-11.06**	-0.288	3.247*
	(5.191)	(1.298)	(1.710)
Controls:			
State Controls	Y	Y	Y
State FE	Y	Y	Y
Quarter FE	Y	Y	Y
State Linear Trends	N	N	N
Adjusted R^2	0.836	0.920	0.873
Clusters	38	38	38
Observations	2,128	2,128	2,128
Mean	56.25	46.01	22.1

Table A4: The Effect of PMP Introduction on Opioids Distributed by Drug (Truven)

	(1) Oxycodone	(2) Total Schedule II excl. Oxycodone	(3) Hydrocodone	(4) Total Schedule III-VI excl. Hydrocodone
1(PMP)_ist	-0.00547***	-0.00000666	-0.00386	0.000947
	(0.00162)	(0.000707)	(0.00481)	(0.00203)
Controls:				
Ind. Controls	Y	Y	Y	Y
State Controls	N	N	N	N
Individual FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
State FE	N	N	N	N
State Linear Trends	N	N	N	N
Adjusted R^2	0.293	0.417	0.283	0.273
Clusters	38	38	38	38
Observations	56,668,472	56,668,472	56,668,472	56,668,472
Individuals	5,113,535	5,113,535	5,113,535	5,113,535
Mean	44.75	16.25	32.18	12.63

Notes: This table breaks out oxycodone from Schedule II and hydrocodone from Schedule III for baseline ARCOS and Truven specifications found in Tables 3 and 5. Oxycodone drives the Schedule II result. Means are in levels (MME in quarter). Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: The Effect of PMP Introduction on Schedule II Opioids Distributed (ARCOS): Robustness Checks

	(1) No controls	(2) State-specific linear time trends	(3) States impl. after 1997	(4) States impl. after 2005	(5) Alternate coding for OH/NM	(6) Obs. Weighted by population
1(PMP)_*st*	-10.66*	-11.49**	-8.697*	-12.61**	-10.26*	-21.07*
	(5.581)	(4.671)	(4.694)	(5.882)	(5.699)	(10.77)
Controls:						
State Controls	N	Y	Y	Y	Y	Y
State FE	Y	Y	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y	Y	Y
State Linear Trends	N	Y	N	N	N	N
Adjusted R^2	0.881	0.949	0.893	0.887	0.889	0.878
Clusters	38	38	44	36	38	38
Observations	2,128	2,128	2,464	2,016	2,128	2,128
Mean	102.26	102.26	100.53	101.92	102.26	102.86

Notes: This table displays robustness checks corresponding to baseline ARCOS specification for Schedule II opioids found in Table 3. Robust standard errors, clustered at the state level, in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Balanced Panel for Event Study (ARCOS)

	(1) Sch. II, unbalanced (baseline specification)	(2) Sch. II, 1 year balanced	(3) Sch. II, 2 years balanced
4 years before	3.603		
	(3.436)		
3 years before	3.038		
	(2.405)		
2 years before	4.509**		5.539
	(2.114)		(3.422)
1 year before	0	0	0
	(.)	(.)	(.)
Year of implementation	-5.801**	-7.264**	-5.817
	(2.668)	(3.146)	(3.964)
1 year after	-10.39**		-10.31
	(4.778)		(6.550)
2 years after	-11.17*		
	(6.088)		
3 years after	-11.52*		
	(6.442)		
4 years after	-7.608		
	(7.033)		
Controls:			
State Controls	Y		
State FE	Y		
Quarter FE	Y		
State Linear Trends	N		
Adjusted R^2	0.891	0.889	0.898
Clusters	38	34	27
Observations	2,128	1,904	1,512

Notes: For each specification the full event study (dummies for 4 years before and after PMP implementation) was estimated, but the sample was restricted to states which had observations for a full 1 or 2 years before and after the law was implemented, i.e. states implementing between 2002 and 2011 for the 2 year balanced panel. Only the coefficients for 1 or 2 years before and after are displayed; others are suppressed. Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: 2SLS Estimates for the Effect of Availability of Schedule II Opioids on Opioid Overdose Deaths

	(1) OLS Opioid overdose deaths per 1,000,000	(2) 2SLS Opioid overdose deaths per 1,000,000
MME_st	0.0371***	0.0356**
	(0.00399)	(0.0176)
Instruments:		
Dummies for 16 Quarters Post-PMP		Y
Controls:		
State Controls	Y	Y
State FE	Y	Y
Quarter FE	Y	Y
State Linear Trends	N	N
Adjusted R^2	0.784	0.605
Clusters	38	38
Observations	2,128	2,128
F-Statistic (excl. instruments)		11.8
Mean	6.1	6.1

Notes: Dependent variable is prescription opioid overdose deaths [T40.2] per 1,000,000 residents in a state for a given quarter, and independent variable is MME Schedule II opioids per resident in a state for a given quarter. Model (2) uses 2SLS with dummies for the 16 quarters post-PMP implementation as instruments for MME Schedule II opioids dispensed.

Table A8: The Effect of PMP Introduction on Opioid Overdose Deaths: Robustness Checks

	(1) ln(deaths+1)	(2) ln(deaths+1)	(3) Poisson spec	(4) Poisson spec	(5) Deaths per capita	(6) Deaths per capita	(7) ln(deaths+1)
4 years before	0.0413	0.00861	0.0494	0.0204	0.0194	0.116	0.0121
	(0.0574)	(0.0597)		(0.0832)	(0.657)	(0.603)	(0.0637)
3 years before	-0.0136	-0.0391	-0.0201	-0.0416	-0.338	-0.257	-0.0405
	(0.0622)	(0.0681)		(0.0798)	(0.581)	(0.546)	(0.0686)
2 years before	0.0199	0.00916	0.0402	0.0365	0.175	0.263	-0.00327
	(0.0451)	(0.0540)		(0.0691)	(0.517)	(0.499)	(0.0467)
1 year before	0.00442	0.00151	0.0220	0.0232	0.116	0.168	-0.0136
	(0.0251)	(0.0297)		(0.0438)	(0.323)	(0.317)	(0.0235)
Year of implementation	0	0	0	0	0	0	0
	(.)	(.)	(.)	(.)	(.)	(.)	(.)
1 year after	-0.0853**	-0.0772**	-0.109	-0.111**	-0.858*	-0.918**	-0.0809**
	(0.0346)	(0.0345)		(0.0564)	(0.425)	(0.439)	(0.0348)
2 years after	-0.116***	-0.0861**	-0.121	-0.116*	-1.022**	-1.121**	-0.0983***
	(0.0352)	(0.0397)		(0.0678)	(0.492)	(0.482)	(0.0348)
3 years after	-0.193***	-0.156***	-0.162	-0.178**	-1.373*	-1.578**	-0.143**
	(0.0478)	(0.0453)		(0.0906)	(0.682)	(0.733)	(0.0550)
4 years after	-0.211***	-0.182***	-0.150	-0.210	-1.202	-1.526	-0.129*
	(0.0565)	(0.0634)		(0.142)	(0.839)	(0.947)	(0.0719)
Controls:							
County Controls	Y	Y	Y	Y	Y	Y	Y
County FE	Y	N	Y	N	Y	N	Y
State FE	N	Y	N	Y	N	Y	N
Quarter FE	Y	Y	Y	Y	Y	Y	Y
State Linear Trends	N	N	N	N	N	N	Y
Observations	135349	135349	116583	135349	135349	135349	135349

Notes: This table displays robustness of the CDC event study specification for prescription opioids (T40.2) displayed in Figure 5 to alternate specifications. All models except (3) and (4) are OLS. Standard errors for Model (3) forthcoming. Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9: The Effect of PMP Introduction on Heroin Overdose Deaths: Robustness Checks

	(1) ln(deaths+1)	(2) ln(deaths+1)	(3) Poisson spec	(4) Poisson spec	(5) Deaths per capita	(6) Deaths per capita	(7) ln(deaths+1)
4 years before	0.0123	-0.00462	-0.0127	-0.0384	0.288	0.357	-0.0985
	(0.0499)	(0.0519)		(0.133)	(0.456)	(0.457)	(0.0797)
3 years before	-0.0161	-0.0302	-0.146	-0.165	-0.00585	0.0526	-0.105
	(0.0577)	(0.0598)		(0.139)	(0.427)	(0.430)	(0.0788)
2 years before	0.0249	0.0147	0.0125	0.00182	0.143	0.188	-0.0421
	(0.0434)	(0.0438)		(0.0767)	(0.293)	(0.295)	(0.0572)
1 year before	0.00868	0.00334	-0.0290	-0.0332	-0.0489	-0.0277	-0.0320
	(0.0257)	(0.0260)		(0.0607)	(0.198)	(0.201)	(0.0347)
Year of implementation	0	0			0	0	0
	(.)	(.)			(.)	(.)	(.)
1 year after	0.0685**	0.0740**	0.0136	0.0110	0.380	0.334	0.109***
	(0.0334)	(0.0327)		(0.0596)	(0.416)	(0.407)	(0.0375)
2 years after	0.00949	0.0193	-0.0136	-0.0192	-0.179	-0.298	0.0585
	(0.0797)	(0.0773)		(0.132)	(0.534)	(0.544)	(0.0675)
3 years after	-0.0221	-0.00336	-0.162	-0.227	-1.153	-1.337	0.0547
	(0.105)	(0.100)		(0.221)	(0.758)	(0.879)	(0.0769)
4 years after	-0.151	-0.123	-0.403	-0.510*	-2.225**	-2.444**	0.00846
	(0.111)	(0.106)		(0.262)	(1.006)	(1.161)	(0.105)
Controls:							
County Controls	Y	Y	Y	Y	Y	Y	Y
County FE	Y	N	Y	N	Y	N	Y
State FE	N	Y	N	Y	N	Y	N
Quarter FE	Y	Y	Y	Y	Y	Y	Y
State Linear Trends	N	N	N	N	N	N	Y
Observations	135349	135349	67590	135349	135349	135349	135349

Notes: This table displays robustness of the CDC event study specification for heroin (T40.1) displayed in Figure 6 to alternate specifications. All models except (3) and (4) are OLS. Standard errors for Model (3) forthcoming. Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A10: The Effect of PMP Introduction: Robustness Checks for Main Truven Claims Data Results

	(1) Baseline Specification	(2) No individual controls	(3) State-level controls	(4) State FE	(5) State-specific linear time trends
Panel 1: Schedule II Drugs, ln(MME + 1)					
1(PMP)_*st*	-0.00496***	-0.00492***	-0.00473***	-0.00465**	-0.00468***
	(0.00168)	(0.00162)	(0.00161)	(0.00173)	(0.00161)
Adjusted R^2	0.293	0.292	0.293	0.293	0.293
Clusters	38	38	38	38	38
Observations	56,668,472	56,668,472	56,668,472	56,668,472	56,668,472
Individuals	5,113,535	5,113,535	5,113,535	5,113,535	5,113,535
Panel 2: Inpatient and Outpatient Spending for Combined Predictive Sample, ln($ + 1)					
1(PMP)_*st*	0.0567**	0.0574**	0.0586***	0.0587**	0.0554**
	(0.0220)	(0.0231)	(0.0188)	(0.0230)	(0.0225)
Adjusted R^2	0.278	0.273	0.278	0.278	0.278
Clusters	38	38	38	38	38
Observations	6,368,111	6,368,111	6,368,111	6,368,111	6,368,111
Individuals	418,212	418,212	418,212	418,212	418,212
Panel 3: Days Absent for WC Injured and Short Term Disability Enrollees, ln(days abs + 1)					
1(PMP)_*st*	0.0377***	0.0372***	0.0373***	0.0394***	0.0363***
	(0.0128)	(0.0129)	(0.0131)	(0.0127)	(0.0130)
Adjusted R^2	0.207	0.138	0.207	0.207	0.207
Clusters	38	38	38	38	38
Observations	3,230,567	3,230,567	3,230,567	3,230,567	3,230,567
Individuals	193,320	193,320	193,320	193,320	193,320
Panel 4: Total Costs (Inpatient, Outpatient, Drug, Lost Wages, WC Medical) for Combined Predictive Sample, ln($ + 1)					
1(PMP)_*st*	0.0522**	0.0535**	0.0532***	0.0536**	0.0479**
	(0.0209)	(0.0215)	(0.0194)	(0.0215)	(0.0216)
Adjusted R^2	0.375	0.366	0.375	0.375	0.375
Clusters	38	38	38	38	38
Observations	6,368,111	6,368,111	6,368,111	6,368,111	6,368,111
Individuals	418,212	418,212	418,212	418,212	418,212
Controls:					
Ind. Controls	Y	N	Y	Y	Y
State Controls	N	N	Y	N	N
Individual FE	Y	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y	Y
State FE	N	N	N	Y	N
State Linear Trends	N	N	N	N	Y

Notes: This table displays robustness checks corresponding to baseline specifications found in Table 5 Model (1), Table 9 Model (4), Table 12 Model (3), and Table 13 Model (5). Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Balanced Panel for Event Study (Truven)

	(1) Unbalanced (baseline specification)	(2) Two quarters balanced	(3) Four quarters balanced
4 Quarters Before	0.000546		0.00212*
	(0.00197)		(0.00121)
3 Quarters Before	-0.00130		0.000299
	(0.00177)		(0.00178)
2 Quarters Before	-0.00150	-0.00145	-0.000965
	(0.00128)	(0.00148)	(0.00172)
1 Quarter Before	0.000472	0.000601	-0.000336
	(0.00143)	(0.00151)	(0.00153)
Year of Implementation	0	0	0
	(.)	(.)	(.)
1 Quarter After	-0.00206	-0.00198	-0.00227
	(0.00139)	(0.00139)	(0.00134)
2 Quarters After	-0.00220	-0.00244*	-0.00214
	(0.00148)	(0.00143)	(0.00167)
3 Quarters After	-0.00354*		-0.00418**
	(0.00184)		(0.00173)
4 Quarters After	-0.00442**		-0.00402**
	(0.00168)		(0.00157)
Adjusted R^2	0.324	0.324	0.323
Clusters	38	31	27
Observations	56,668,472	50,554,976	43,711,252
Individuals	5,113,535	4,561,598	3,998,287

Notes: For each specification the full event study (dummies for 16 quarters before and after PMP implementation) was estimated, but the sample was restricted to states which had observations for a full 2 or 4 quarters before and after the law was implemented, i.e. states implementing between 2004 and 2011 for the 4 quarter balanced panel. Only the coefficients for the quarters with a balanced panel are displayed; others are suppressed. Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A12: The Effect of PMP Introduction on Schedule II Opioids Distributed: Alternate Concave Transformations for MME Taken in Quarter

	(1) ln(MME + 1) (baseline specification)	(2) sqrt(MME)	(3) ln(MME + 0.01)	(4) ln(MME + 100)
1(PMP)_*ist*	-0.00496***	-0.0450**	-0.00771***	-0.00221***
	(0.00168)	(0.0168)	(0.00272)	(0.000710)
Controls:				
Ind. Controls	Y	Y	Y	Y
State Controls	N	N	N	N
Individual FE	Y	Y	Y	Y
Quarter FE	Y	Y	Y	Y
State FE	N	N	N	N
State Linear Trends	N	N	N	N
Adjusted R^2	0.324	0.603	0.275	0.502
Clusters	38	38	38	38
Observations	56,668,472	56,668,472	56,668,472	56,668,472
Individuals	5,113,535	5,113,535	5,113,535	5,113,535
Mean	0.154	0.81	-4.338	4.65

Notes: Dependent variables are alternate concave transformations of Schedule II Morphine Milligram Equivalent. Means are means of the transformed variable. Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A13: The Effect of PMP Introduction on Schedule II Opioids Distributed: Full Sample and Continuous Enrollees (2006-2012) With and Without Individual FE

	(1) Ind/Quarter FE Full Sample	(2) Quarter/State FE Full Sample	(3) Ind/Quarter FE Cont. 2006-2012	(4) Quarter/State FE Cont. 2006-2012
1(PMP)_ist	-0.00496***	-0.00231	-0.00371*	-0.00367*
	(0.00168)	(0.00216)	(0.00188)	(0.00199)
Controls:				
Ind. Controls	Y	Y	Y	Y
State Controls	N	N	N	N
Individual FE	Y	N	Y	N
Quarter FE	Y	Y	Y	Y
State FE	N	Y	N	Y
State Linear Trends	N	N	N	N
Adjusted R^2	0.324	0.010	0.281	0.009
Clusters	38	38	38	38
Observations	56,668,472	56,668,472	9,209,571	9,209,571
Individuals	5,113,535	5,113,535	335,435	335,435
Mean	60.79	60.79	52.38	52.38

Notes: This table examines the robustness of the point estimates to the exclusion / inclusion of individual fixed effects for the full sample as well as a restricted subsample of enrollees that provide a balanced panel in 7 years. Dependent variable is ln(MME Sch. 2 + 1). Means are in levels (MME). Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A14: The Effect of PMP Introduction on Whether Patient is Switched to Alternate Opioid

	(1) Switch to more potent opioid	(2) Switch to less potent opioid
1(PMP)_ist	-0.0000576	-0.0000138
	(0.0000748)	(0.0000673)
Controls:		
Ind. Controls	Y	Y
State Controls	N	N
Individual FE	Y	Y
Quarter FE	Y	Y
State FE	N	N
State Linear Trends	N	N
Adjusted R^2	0.058	0.059
Clusters	38	38
Observations	56,668,472	56,668,472
Individuals	5,113,535	5,113,535
Mean	0.006	0.006

Notes: Dependent variable is binary (0/1) for whether a patient switches to a more or less potent opioid in a quarter. Scripts are classified as 'more potent' if they are in a higher schedule, and 'less potent' if they are in a lower schedule. Fentanyl, Hydromorphone, Morphine, and Oxymorphone are additionally classified as more potent than Oxycodone. Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A15: The Effect of PMP Introduction on Percent of Hospital Patients Reporting Pain was Sometimes/Never Well-Controlled (HCAHPS): Robustness Checks

	(1) Baseline Specification	(2) No controls	(3) State-specific linear time trends
4 years before	-0.0250	-0.0103	-0.116
	(0.211)	(0.205)	(0.275)
3 years before	-0.0233	-0.00941	-0.102
	(0.193)	(0.185)	(0.217)
2 years before	-0.0999	-0.0886	-0.158
	(0.147)	(0.147)	(0.161)
1 year before	0.0647	0.0701	-0.00405
	(0.0935)	(0.0945)	(0.0852)
Year of implementation	0	0	0
	(.)	(.)	(.)
1 year after	0.181*	0.178*	0.229**
	(0.0940)	(0.0937)	(0.0904)
2 years after	0.299**	0.295**	0.339**
	(0.140)	(0.140)	(0.128)
3 years after	0.381**	0.372**	0.388**
	(0.185)	(0.179)	(0.168)
4 years after	0.597**	0.581**	0.605**
	(0.263)	(0.247)	(0.267)
Controls:			
State Controls	Y	N	Y
State FE	Y	Y	Y
Quarter FE	Y	Y	Y
State Linear Trends	N	N	Y
Adjusted R^2	0.915	0.915	0.939
Clusters	38	38	38
Observations	912	912	912
Counties			
Mean (Levels)	7.4	7.4	7.4

Notes: This table displays robustness of the HCAHPS event study specification displayed in Figure 9 to alternate specifications. Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A16: Endogeneity in Predicted Enrollee Sample

	(1) Has any claim mentioning predictive diagnostic codes (0/1)
1(PMP)_ist	-0.000115
	(0.000199)
Controls:	
Ind. Controls	Y
State Controls	N
Individual FE	Y
Quarter FE	Y
State FE	N
State Linear Trends	N
Adjusted R^2	0.131
Clusters	38
Observations	53,520,057
Individuals	4,944,277
Mean	0.023

Notes: This table displays the relationship between the introduction of a PMP and whether an enrollee ever has a claim mentioning a predictive diagnostic code, for years 2005 to 2012. Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A17: First Stage for Combined Predictive Enrollees: The Effect of PMP Introduction on Schedule II Opioids Distributed

	(1) ln(MME in quarter + 1)	(2) binary (0/1) in quarter	(3) MME in quarter
1(PMP)_ist	-0.0153**	-0.00158	-25.75**
	(0.00684)	(0.000992)	(12.56)
Controls:			
Ind. Controls	Y	Y	Y
State Controls	N	N	N
Individual FE	Y	Y	Y
Quarter FE	Y	Y	Y
State FE	N	N	N
State Linear Trends	N	N	N
Adjusted R^2	0.345	0.238	0.606
Clusters	38	38	38
Observations	6,368,111	6,368,111	6,368,108
Individuals	418,212	418,212	418,212
Mean	0.503	0.077	225.9

Notes: This table displays the point estimate for the effect of the introduction of a PMP on Schedule II opioid prescribing for the combined predictive enrollee sample. Robust standard errors, clustered at the state level, in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A18: The Effect of PMP Introduction on Total Workers' Comp Medical Spending, by Insurer

	(1) Employer health insurance inpatient + outpatient spending	(2) WC insurer total medical spending	(3) Combined medical spending
1(PMP)_ist	0.0806***	0.131*	0.130***
	(0.0283)	(0.0651)	(0.0463)
Controls:			
Ind. Controls	Y	Y	Y
State Controls	N	N	N
Individual FE	Y	Y	Y
Quarter FE	Y	Y	Y
State FE	N	N	N
State Linear Trends	N	N	N
Adjusted R^2	0.317	0.173	0.279
Clusters	38	38	38
Observations	892,245	892,245	892,245
Individuals	44,418	44,418	44,418
Mean	1035.51	618.6	1679.91

Notes: This table displays estimates of the effect of PMP introduction on medical spending on individuals injured under workers' compensation. Direct medical costs associated with the injury should be paid for not by the worker's health insurance, but by the worker's compensation insurer. This exercise is only suggestive due to data limitations on the f's medical spending. Robust standard errors, clustered at the state level, in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A19: The Effect of PMP Introduction on Total Costs and Overdose Deaths, by Demographics

Panel A: Costs by Employment

			Mean	Individuals
Panel 1: Union	0.057**	(0.028)	3,795	98,474
Panel 2: Nonunion	0.030*	(0.017)	4,049	262,182
Panel 3: Salaried	0.033*	(0.018)	4,464	154,441
Panel 4: Hourly	0.063***	(0.023)	3,704	218,664

Panel B: Costs by Industry

			Mean	Individuals
Panel 1: Oil and Gas Extraction, Mining	-0.003	(0.067)	4,521	3,533
Panel 2: Manufacturing, Durable Goods	0.084***	(0.026)	4,067	125,605
Panel 3: Manufacturing, Nondurable Goods	0.014	(0.030)	3,709	60,947
Panel 4: Transportation, Communications, Utilities	-0.003	(0.029)	3,678	88,600
Panel 5: Retail Trade	0.061	(0.045)	4,851	11,004
Panel 6: Finance, Insurance, Real Estate	0.068*	(0.036)	4,382	83,631
Panel 7: Services	0.052	(0.052)	4,817	35,175
Panel 8: Construction	-0.252	(0.230)	7,952	309
Panel 9: Wholesale	0.583	(0.541)	7,583	112

Panel C: Deaths by Race

			Mean
Panel 1: Non-hispanic white	-0.12614**	(0.047)	0.471
Panel 2: Hispanic	-0.02546	(0.029)	0.043
Panel 3: Non-hispanic black	-0.07738***	(0.022)	0.031
Panel 4: Non-hispanic other races	0.02636	(0.021)	0.010

Panel D: Deaths by Education

			Mean
Panel 1: No high school diploma	-0.10784**	(0.047)	0.112
Panel 2: High school but no 4-year college degree	-0.15406***	(0.045)	0.364
Panel 3: College or post-grad degree	-0.03189*	(0.018)	0.055

Notes: The above specifications are as in baseline specifications shown in Tables 13 (costs) and 4 (deaths). Costs specification is run on split sample, by characteristics, and dependent variable is ln(total costs + 1). Deaths specification utilizes ln(opioid overdose deaths [T40.2] for the demographic group listed + 1) as the dependent variable. Means are in levels ($ and deaths). Robust standard errors, clustered at the state level, in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A20: The Effect of PMP Introduction on Total Costs and Overdose Deaths, by Geography

Panel A: Costs

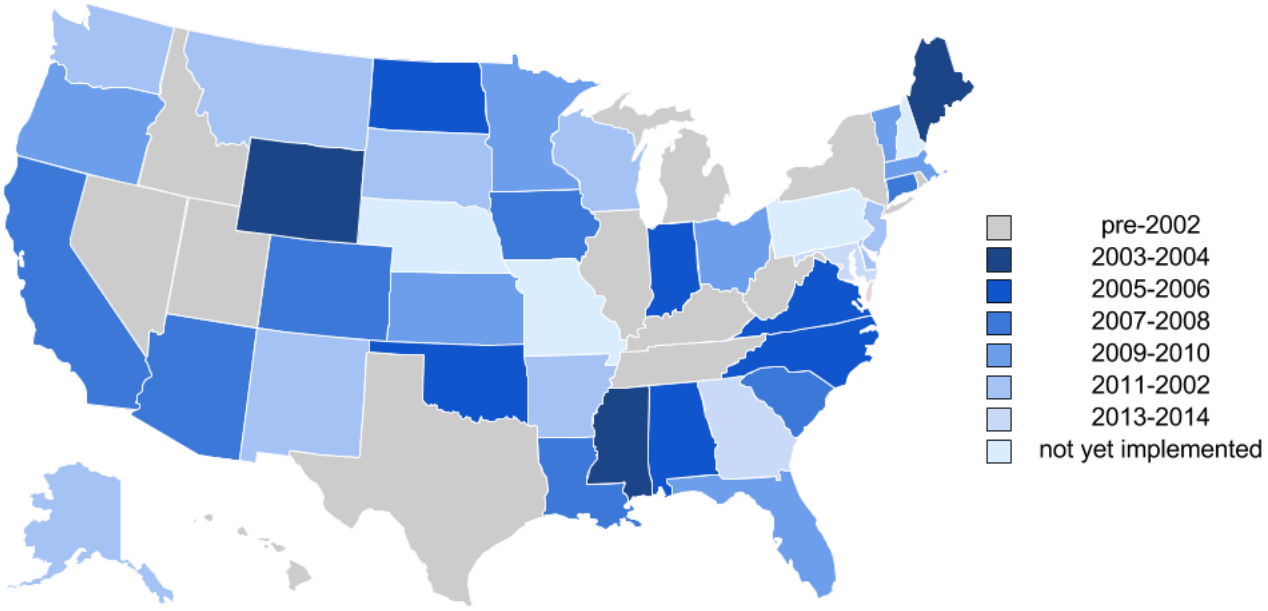
			Mean	Individuals
Panel 1: Urban	0.051**	(0.021)	4,041	377,091
Panel 2: Rural (non-MSA)	0.062**	(0.025)	3,983	46,473

Panel B: Deaths

			Mean
Panel 1: Urban	-0.15438**	(0.061)	1.261
Panel 2: Rural	0.00722	(0.024)	0.130

Notes: The above specifications are as in baseline specifications shown in Tables 13 (costs) and 4 (deaths). Costs specification is run on split sample, by characteristics, and dependent variable is ln(total costs + 1). Deaths specification utilizes ln(opioid overdose deaths [T40.2] for the demographic group listed + 1) as the dependent variable. Means are in levels ($ and deaths). Robust standard errors, clustered at the state level, in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

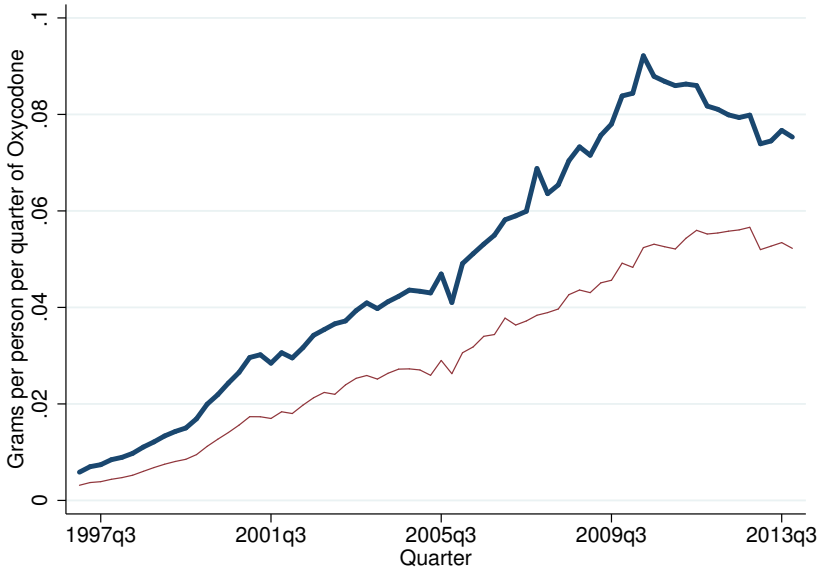
Figure A1: Date of Implementation of State PMPs



Notes: Source: National Alliance for Model State Drug Laws (NAMSDL)

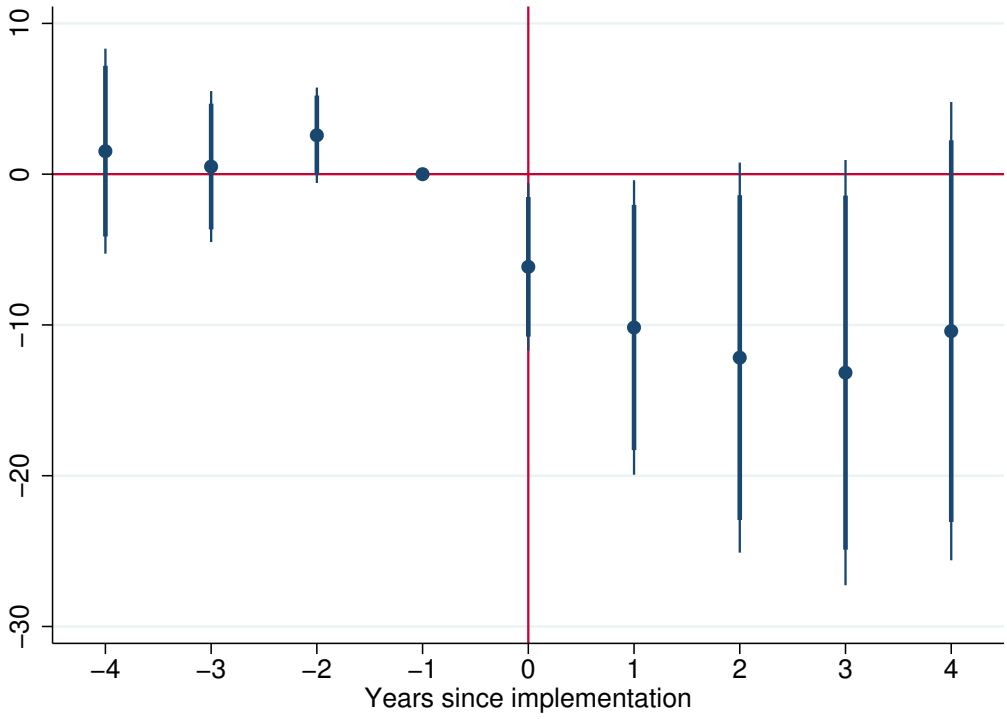
Figure A2: Grams Morphine Equivalent Distributed Per Person in 38 Sample States versus States with Early Controlled Substances Efforts

Panel A: MME oxycodone per resident per quarter



Panel B: MME hydrocodone per resident per quarter

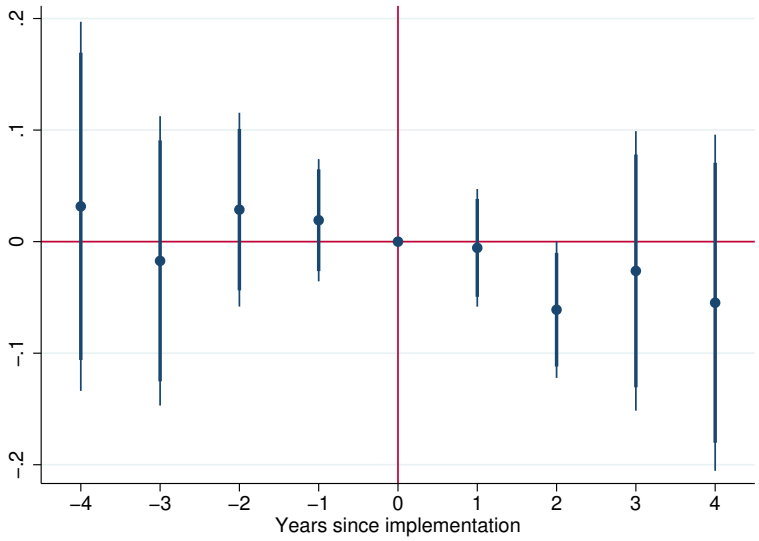
Figure A3: Event Study, ARCOS: Using Date of Collection Rather than Date of Access



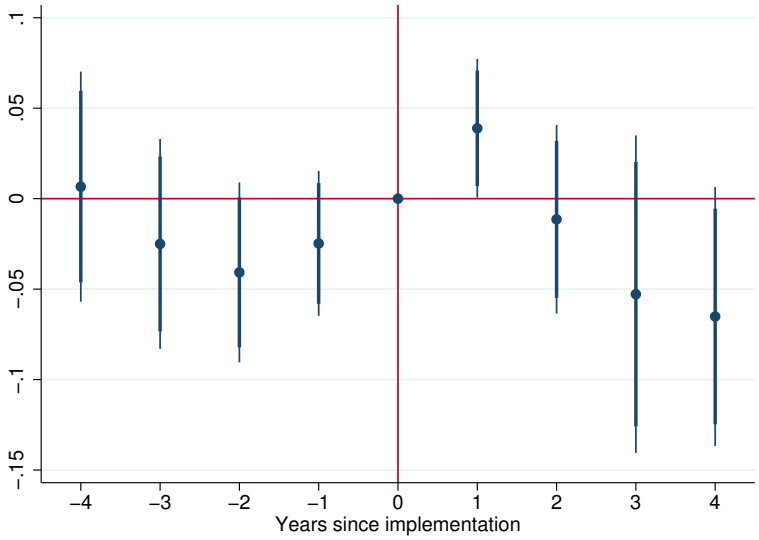
Notes: ARCOS event study, utilizing date of first data collection on prescriptions rather than date of access for physicians. Note that not all states have date of collection in NAMSDL data; for those states date of implementation was used. Robust standard errors clustered at state level. Thin bars represent 95% confidence intervals and thick bars represent 90% confidence intervals.

Figure A4: Other Opioid Overdoses

Panel A: Methadone [T40.3] Overdose Deaths

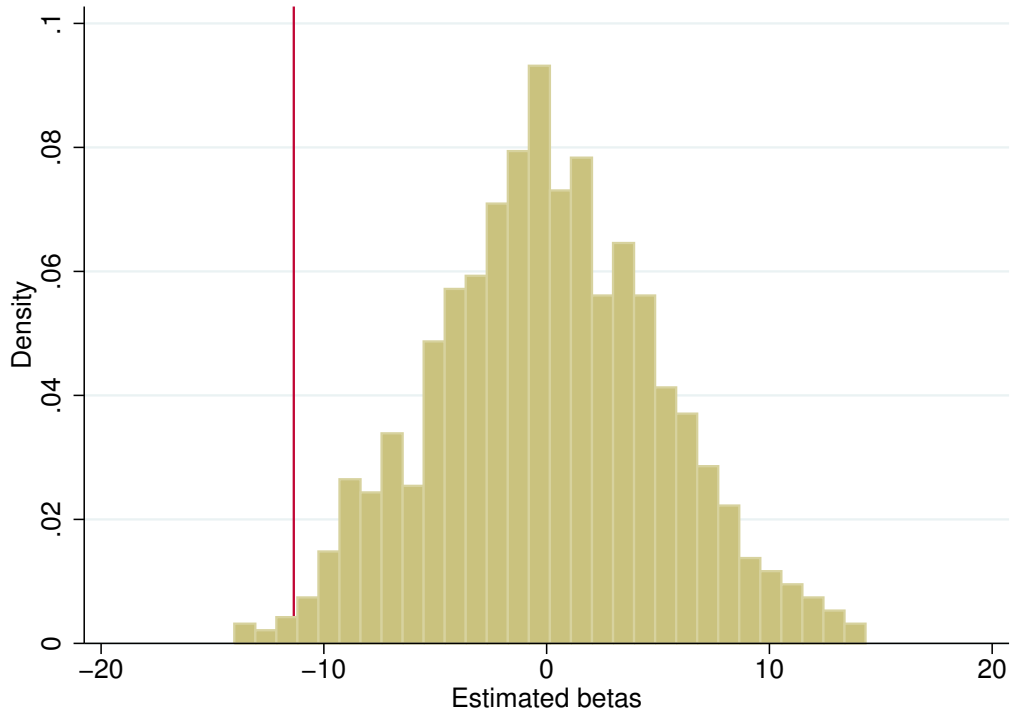


Panel B: Fentanyl and Other Synthetic Opioids [T40.4] Overdose Deaths



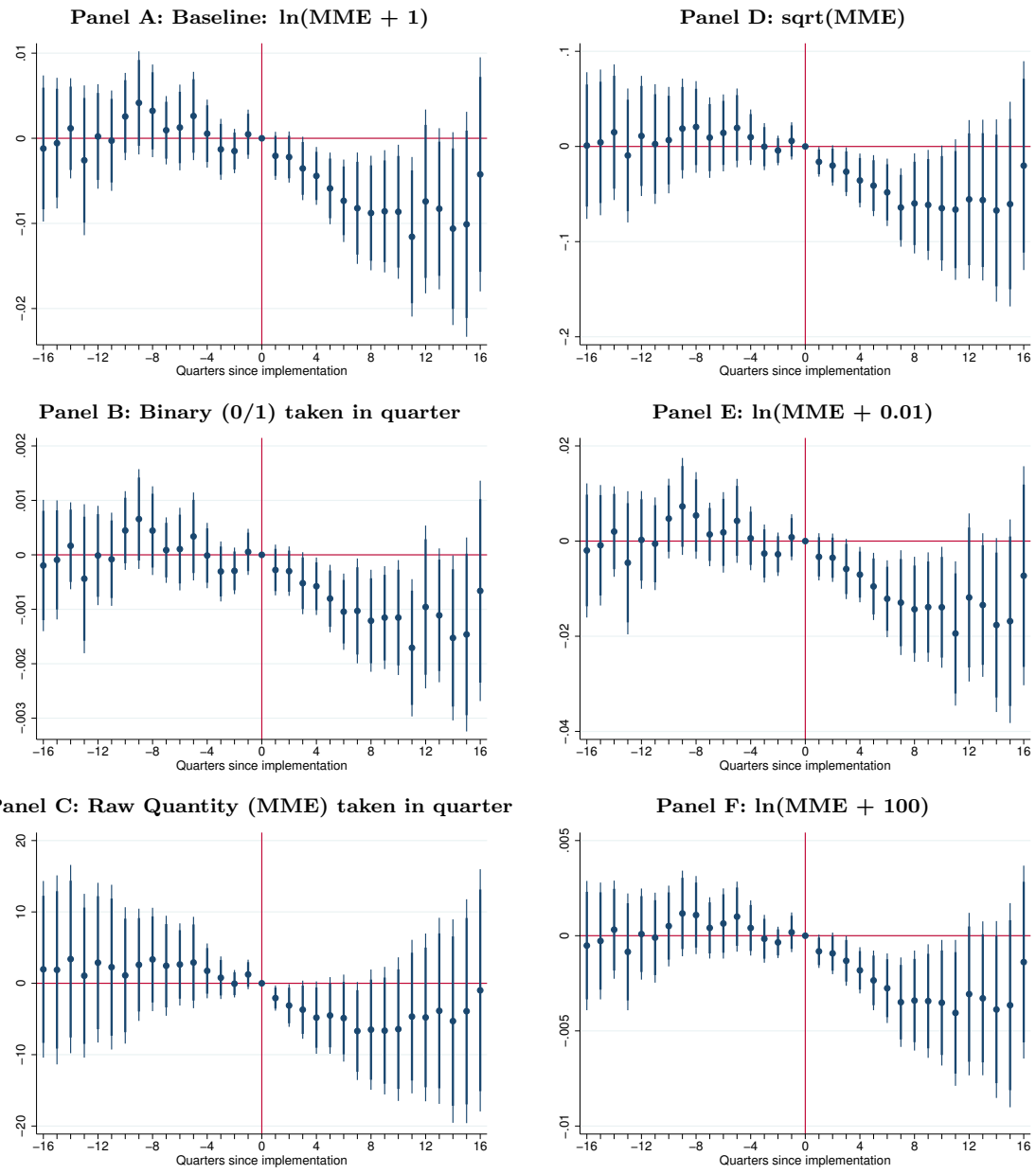
Notes: CDC event study for other opioids. Robust standard errors clustered at state level. Thin bars represent 95% confidence intervals and thick bars represent 90% confidence intervals.

Figure A5: Placebo Laws (ARCOS)



Note: This figure plots the distribution of coefficients from the placebo exercise. The red line represents the baseline coefficient from Table 3, $\hat{\beta_1} = -11.35$. 8 out of 1000 placebo point estimates are below this baseline coefficient.

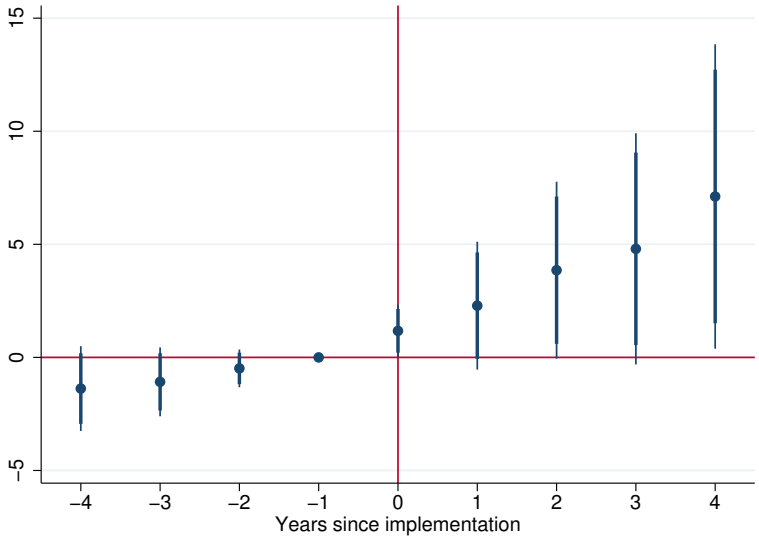
Figure A6: Alternate Transformations for MME Schedule II Opioids Taken in Quarter



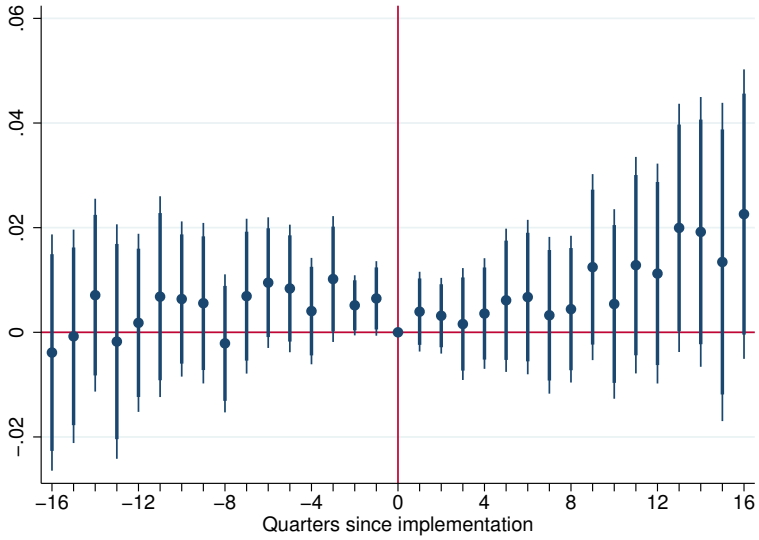
Notes: Truven event study. Dependent variables are alternate transformations of Schedule II Morphine Milligram Equivalent. Robust standard errors clustered at state level. Thin bars represent 95% confidence intervals and thick bars represent 90% confidence intervals.

Figure A7: Substitution Towards Lower Schedules

Panel A: MME Sch. III-VI opioids per resident per quarter, ARCOS



Panel B: ln(MME Schedule III-VI in quarter+1), Truven



Notes: Lower schedules in ARCOS include codeine and hydrocodone. Lower schedules in Truven include codeine, dihydrocodeine, hydrocodone, butorphanol, nalbuphine, pentazocine, propoxyphene, and tramadol. Robust standard errors clustered at state level. Thin bars represent 95% confidence intervals and thick bars represent 90% confidence intervals.