Hassle Costs versus Information: How Do Prescription Drug Monitoring Programs Reduce Opioid Prescribing? †

Abby Alpert, University of Pennsylvania and NBER
Sarah Dykstra, University of Pennsylvania
Mireille Jacobson, University of Southern California and NBER

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Abstract

We study hassle costs versus information provision in explaining how prescription drug monitoring programs (PDMPs) decrease opioid prescribing. PDMPs are intended to affect prescribing through information provision but may also unintentionally affect prescribing through the hassle of required record checks. We analyze Kentucky's landmark PDMP to disentangle these two mechanisms. We find that although information clearly affects prescribing, hassle costs explain the majority of the decline. Hassle costs reduce prescribing across the board; however, physicians continue to prescribe opioids to patients who would benefit the most. Introducing a cost to physicians to prescribe high-risk medications improves the targeting of treatment.

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I. Introduction

In the U.S., opioid misuse, diversion, and overdoses have increased dramatically over the last two decades, creating an unprecedented public health crisis. Prescription opioids continue to play an important role in the epidemic, even as heroin and synthetic opioids fuel the most recent increase in overdose deaths. In 2018, prescription opioids were involved in nearly 15,000 overdose deaths, over 30% of all opioid overdose deaths (Wilson et al. 2020).

Since many Americans obtain opioids legally through their physicians and prescription opioids are often an entry point for opioid addiction (Compton, et al. 2016; Compton and Wargo 2018; Muhuri, et al. 2013), interventions targeted at physician prescribing play an important role in reducing opioid misuse (Schnell and Currie 2018). Prescription Drug Monitoring Programs (PDMPs), all-payer electronic databases that track prescriptions for controlled substances, are the most widely adopted physician interventions aimed at reducing inappropriate opioid prescriptions.[1] PDMPs are designed as information interventions. They provide physicians with information on patient prescription histories, enabling physicians to identify those with potentially problematic behaviors— such as "doctor shopping" or obtaining unusually large quantities or high dosages of opioids—and prescribe fewer opioids to these patients. Information could also enable providers to increase prescribing to some patients without a history of misuse if it positively updates the physician's beliefs.[2] This information may not otherwise be available since health IT systems are typically uncoordinated across providers, enabling problematic behaviors to go unchecked.

[1] Other physician interventions have included training and education initiatives, new prescribing guidelines (CDC 2016), targeted provider messages, individualized information on patient's risk of abuse, quantity defaults (Delgado et al. 2018), or limits on days supplied or dosage (Haffajee and French 2019).

[2] Baehren et al. (2010) show that prescription patterns changed in precisely this way after Ohio's adoption of a PDMP. In one-third of cases, more opioids were given than originally planned because physicians felt enabled to prescribe stronger pain medicines to those not misusing opioids.

Previous studies have examined the aggregate effects of PDMPs on opioid prescribing and overdose rates. These studies have found mixed results (e.g., Brady et al. 2014; Horwitz et al. 2018; Jena et al. 2014; Meara et al. 2016; Kilby 2015; Moyo et al. 2017; Popovici et al. 2018) at least in part because of state heterogeneity in the requirements on providers to consult the PDMP. Until recently, many states made PDMP use voluntary for doctors, meaning they were not required to query the PDMP before prescribing opioids. These types of programs are generally found to be ineffective, while states with "must access" or mandated use of PDMPs are more consistently found to reduce opioid prescribing (e.g., see Bao et al. 2018; Buchmueller and Carey, 2018; Haffajee et al. 2018; Meinhofer 2018). However, since studies of PDMP mandates have primarily relied on aggregate measures at the state-level, they provide limited evidence on *how* PDMP mandates reduce opioid prescribing. Yet, understanding mechanisms is critical to identifying what makes these programs more effective at reducing opioid prescribing than others. Understanding mechanisms can also facilitate better program targeting and more generally motivate the use of a range of different policy levers to improve opioid prescribing.

In this paper, we show the importance of hassle costs in explaining how PDMP mandates reduce opioid prescribing and examine their impact on the targeting of opioid treatment. To do so, we analyze a landmark PDMP mandate in Kentucky, which dramatically reduced opioid prescribing. We use individual-level claims data from Optum, a large commercial insurance database, to disentangle the role of hassle costs versus information provision. While information is the intended channel through which PDMPs reduce prescribing, PDMP mandates may also have an unintended effect on prescribing by introducing a hassle cost. A PDMP mandate requires physicians to log into an electronic system to do a record check before writing an opioid prescription. The effects of hassle costs are ambiguous. By raising the cost of opioid prescribing,

mandates could cause across the board reductions in opioid prescribing, even to patients who have an appropriate clinical need for opioids and no recent history of misuse. Additionally, they could lead physicians to substitute to drugs that are not monitored by the PDMP, even if they are less effective. On the other hand, mandates could also improve decision-making if physicians are only willing to incur the hassle cost of querying the PDMP when the benefit of opioids to patients is large, i.e., for medical conditions that are most appropriate for opioids. Such a mechanism would be in the same spirit as the theoretical literature showing that hassle costs can improve the targeting of social programs (Nichols and Zeckhauser 1982). Hassle costs have been cited by clinicians as a key barrier to prescriber use of PDMPs and to the widespread adoption of PDMP mandates (Deyo et al. 2013; Perrone et al. 2012; Rutkow et al., 2015) but have been largely ignored in the economics literature.[3] Another potential unintended effect of the mandate is the increased salience of government monitoring. We discuss the possible effects of salience, although we expect this factor to have more limited effects on prescribing since PDMPs were in place prior to mandated use and doctors were already likely aware of their potential for monitoring.

Our analysis focuses on prescribing in the emergency department (ED), where more than one-quarter of patients receive an opioid following their visit. The information gleaned from a PDMP may have a large impact in this setting given that doctors have little to no repeat interaction with patients and, absent a PDMP, have limited access to patient histories. Doctors in this setting are also highly time-constrained and may face significant hassle costs from the PDMP mandate. We use a difference-in-differences and event study framework to compare prescribing patterns in Kentucky with states that did not have a PDMP mandate. We find that

[3] Two recent exceptions are Sacks et al. (2019) and Buchmueller et al. (2019) discussed below, although neither study attempts to quantify the relative importance of information and hassle costs.

opioid prescriptions following an ED visit decline sharply on the extensive margin (any prescription) and more modestly on the intensive margin (days supplied) after Kentucky adopted its mandate. We also find evidence of substitution to non-opioid prescription analgesics. These changes in prescriptions appear to be driven largely by supply-side effects of the PDMP mandate and not by compositional changes in patients seeking opioids, since the volume of ED visits appears generally unaffected by the mandate.

To quantify the extent to which the reduction in opioid prescribing is driven by information or hassle costs, we test for differences in providers' prescribing responses to the mandate based on patient characteristics: opioid history (naïve vs. non-naïve status) and appropriateness for opioid pain relievers (based on patient diagnoses). Consistent with the information channel, we find that declines in prescribing are smaller for patients who have not filled an opioid prescription in the last six months (i.e., the opioid naïve) than the opioid non-naive and, among the non-naïve, largest for patients who have problematic histories that include "doctor shopping" or high daily doses or quantities of opioids. However, our finding that prescribing also declines substantially for the opioid naïve population demonstrates the important role of hassle costs since no information in the PDMP should lead a provider to reduce prescribing to this group.

Our analysis also tests for physicians' responses to the mandate based on the appropriateness of opioids for the patient's diagnosis. Prescribing is unchanged for opioid naïve patients who present in the ED with diagnoses that are most clearly appropriate for opioids, such as fractures. However, we find large declines in prescribing for opioid naïve patients presenting with conditions that are considered inappropriate for opioids, such as low back pain. Providers appear most willing to incur the hassle cost of using the PDMP for conditions where the net

benefit of treatment is high (i.e., the benefits outweigh the hassle cost) and least likely to use the PDMP when opioid treatment is inappropriate. As a result, the introduction of hassle costs from the mandate shifts opioid treatment to more appropriate diagnoses.

We combine estimates from these tests in a triple-differences framework to decompose the relative contribution of hassle costs and information. We find that although information reduces prescribing, hassle costs explain the majority of the decline. Specifically, hassle costs explain 69% of the reduction in opioid prescribing, while information explains the remaining 31%. To the extent that increased monitoring salience also reduces prescribing, it reinforces the hassle cost effects. Thus, information provision—the intended purpose of PDMPs— may play a smaller role in PDMPs' effectiveness than has been previously recognized.

Finally, we analyze longer-term outcomes of the mandate. We find that both naïve and non-naïve patients have weakly better health outcomes the year after their initial ED visit post mandate. Patients who were opioid naïve at their initial visit are less likely to have another ED visit in the following year while non-naive patients are slightly less likely to have long term use of opioids.

By adding a hurdle to writing an opioid prescription, PDMP mandates decrease opioid prescribing for high-risk populations while enabling access for those who may truly benefit from these medications. They do this because doctors are most willing to incur the PDMP hassle cost when the potential benefits of opioids are large. In short, hassle costs may be welfare enhancing in the opioid prescription context, where only a small share of patients may benefit from these drugs and a screening mechanism is needed. While hassle costs are certainly a blunt mechanism for screening patients, they may still improve prescribing in the absence of better designed policies to target opioid-appropriate individuals. Consequently, ongoing policy reforms to

remove frictions and lower the hassle cost to writing a prescription (e.g., through PDMP integration with electronic health records) may inadvertently reduce the effectiveness of PDMPs if hassle costs drive a significant share of the prescribing effects. Such policy changes should be monitored to ensure that the gains from greater access to information offset any losses from the lower costs of writing an opioid prescription.[4]

In the remainder of the paper, we first provide background on PDMP mandates, with a focus on Kentucky's program (Section II). We provide an overview of our data in Section III and present our empirical approach in Section IV. We discuss our results in Section V and offer some concluding thoughts in Section VI.

II. Background on Prescription Drug Monitoring Programs

2.1. Background on PDMPs

PDMPs are state-run programs that collect data from pharmacies on dispensed controlled substances, including opioids. The programs create databases that allow doctors to view a patient's prescription history to identify patterns of misuse. While PDMPs have been introduced in nearly every state, their design and accessibility vary widely.[5] When most states introduced PDMPs, they made prescriber use voluntary. Although pharmacies had to report prescription information for controlled substances to the state, prescribers could choose whether or not to query this information before writing prescriptions. Since 2012, most states have introduced mandates that require providers to query the database before prescribing a controlled substance.

A growing literature examines the effects of PDMPs. Much of the literature finds null

[4] We know relatively little about the impact of PDMP integration, although Wang (2020) finds that states that have PDMP integration policies experience reductions in opioid-related mortality.
[5] See Kilby (2015) for detailed background on the history of PDMPs and, more generally, state efforts to monitor the flow of controlled substances to patients.

effects (Brady et al. 2014; Horwitz et al. 2018; Jena et al. 2014; Kennedy-Hendricks et al. 2016; Li et al. 2014; McDonald et al. 2012; Meara et al. 2016; Paulozzi et al. 2011; Reifler et al. 2012). However, some studies find that PDMPs reduce opioid prescribing (Bao et al. 2016; Moyo et al. 2017; Reisman et al. 2009), substance abuse treatment admissions (Popovici et al. 2018) and overdose deaths (Kilby 2015). As a result of these mixed findings, many reviews of the literature have stated that the evidence on the effectiveness of PDMPs is inconclusive (Davis 2017; Fink et al. 2018; Haegerich et al. 2014; Horwitz et al. 2018).

Studies that distinguish explicitly between voluntary versus mandated use of PDMPs find more consistent evidence. Requiring providers to query a PDMP before writing prescriptions reduces opioid prescribing (Bao et al. 2018; Dowell et al. 2016; Haffajee et al. 2018; Meinhofer 2018; Wen et al. 2016) as well as indicators of opioid misuse (Ali et al. 2017; Buchmueller and Carey 2018; Buchmueller, Carey and Meille 2019) and overdose deaths (Dowell et al. 2016; Meinhofer et al. 2018; Pardo 2017; Patrick et al. 2016). Effects found in these studies are large, in the range of 8-18% for morphine milligram equivalents (MME) rates per state resident and 8-26% for misuse.

Existing research has primarily examined aggregate state-level effects of PDMPs without assessing how they reduce prescribing. While PDMPs are meant to provide information, hassle costs have long been cited by providers as a key barrier to PDMP use and a source of opposition to PDMP mandates (Blum et al. 2016; Deyo et al. 2013; Perrone et al. 2012; Rutkow et al. 2015). Paper-based "triplicate prescription programs,"[6] which preceded electronic PDMPs and required considerable paperwork for providers, were highly effective at reducing opioid use (Alpert et al.,

[6] Triplicate programs, which were in effect in some states through the early 2000s, required prescribers to use triplicate forms when prescribing Schedule II controlled substances. Providers kept one copy, gave one copy to the pharmacy and sent the third copy to the state monitoring agency. Alpert et al. (2019) discuss focus group evidence that doctors considered the hassle costs of these programs to be large.

2019). In contrast, the earliest electronic PDMPs, which were designed to minimize hassle costs by passively collecting prescription data from pharmacies (Fishman et al. 2004; Simoni-Wastila and Tompkins 2001), had minimal effects on prescribing. In the 2010s, the PDMP mandates re-introduced a hassle cost for providers and were again effective at reducing opioid prescribing. The correlation between the effectiveness of these programs and their hassle costs suggests that hassle costs, particularly those borne directly by the providers, may play an important role in the effectiveness of PDMPs.

Despite the potential importance of hassle costs, only two recent PDMP papers– Sacks et al. (2019) and Buchmueller et al. (2019) – discuss this mechanism. Sacks et al. (2019) use commercial claims data to study the effects of both state PDMP mandates and laws limiting the length of initial opioid prescriptions. They find reductions in the likelihood of opioid use among new patients following PDMP mandates, which they hypothesize is caused by the fixed costs of having to register with the PDMP and log in before writing a prescription. While their results are consistent with hassle costs, their work does not attempt to isolate the mechanisms for how mandates affect prescribing. Buchmueller et al. (2019) study Kentucky's PDMP mandate using administrative records from the PDMP. They find that some low-volume prescribers stopped prescribing opioids following the PDMP mandate, which they attribute to the high compliance cost of registering with and using the PDMP. Because the PDMP data do not contain clinical information or patient histories for non-opioid users, however, they are limited in their ability to isolate the relative effects of information and hassle costs. In contrast, claims data enable us to consider both prescription histories and presenting diagnoses to separate out the effects of information and hassle costs.

2.2. Kentucky’s PDMP

To study the role of information versus hassle costs, we focus on Kentucky's PDMP. The state's comprehensive law is considered the gold standard for PDMPs and is often held up as a model for other states (SAMHSA 2013). Consistent with its robust policy, Kentucky experienced the largest decline in opioid prescribing among mandate states. As shown in Figure 1, the likelihood of receiving an opioid prescription following an ED visit fell sharply (by about 20%) in Kentucky after the state implemented its mandate in July 2012. Changes in prescriptions in other mandate states were much smaller (see Appendix Figure A1).[7] Since Kentucky had one of the earliest mandates, this also allows for longer follow-up.[8] Consequently, Kentucky provides the best opportunity to disentangle the mechanisms underlying PDMP mandate effects.

The Kentucky All Schedule Prescription Electronic Reporting System (KASPER) was started in 1999. At the outset, prescriber use of the PDMP was voluntary. As a result, provider take-up of the system was low. In 2009, for example, fewer than 28% of DEA-licensed prescribers were registered and could make queries to KASPER (Freeman et al. 2011).

Beginning in July 2012, pharmacists and physicians in Kentucky were *required* to register with KASPER and query the system before prescribing any Schedule II controlled substances or Schedule III controlled substances containing hydrocodone (Freeman et al. 2015). In addition, pharmacists had to report new prescriptions to KASPER within one day of dispensing in contrast to the previous 7-day requirement.[9] Within one year of implementing the mandate, 95% of DEA-licensed prescribers registered with the system (Freeman et al. 2015). Prescriber queries increased dramatically, from under 100,000 in the month just prior to the

[7] Haffajee et al. (2018) also show that of the four states with the most robust mandate PDMPs, only Kentucky experienced a decline in the proportion of individuals receiving opioids.
[8] In addition, in most mandate states, we lack adequate post-period data to credibly evaluate mandate effects. The states of CT, MA, NH, NJ, NV, OH, OK, PA, RI, VA, VT adopted mandates in 2015 or later.
[9] Kentucky also participates in an inter-state sharing agreement, such that KASPER reports include data from out-of-state prescribers. This sharing began in 2011, with a pilot program with Ohio and the Bureau of Justice Assistance (see http://ci.uky.edu/kentuckyhealthnews/2011/08/08/kentucky-ohio-now-exchanging/) but quickly expanded to other states.

mandate to over 350,000 in the month just after (see Figure 2 in Freeman et al. 2015). The 2012 law that updated KASPER, HB1, also strengthened pain clinic regulations and reportedly led to the closure of several clinics (Freeman et al. 2015).[10] In principle, these closures might have increased drug-seeking in the ED as a primary source of opioids disappeared. Our analysis of visit patterns discussed below, however, finds no evidence to support this type of behavior.

Kentucky's mandate allows an exemption to PDMP queries in the event of a true medical emergency or when administering a controlled substance immediately prior to, during or within 14 days of a surgery or other invasive procedure.[11] While a typical emergency department (ED) visit would not constitute a medical emergency warranting an exemption, a gunshot wound treated in the ED might. In addition, and unlike many other states, Kentucky does not allow any exemptions to either reporting or querying the system based on the number of days supplied or MME.[12] These strong features of the mandate likely contributed to Kentucky's success.

2.3. Conceptual Framework for Potential Mechanisms

Our central interest is understanding *how* Kentucky's mandate, and similar laws modeled on this policy, reduce opioid prescribing. A reduction in prescribing could reflect provider use of the information in the PDMP to target opioid prescriptions. It could also reflect an across the board decrease due to hassle costs, which could even affect those who would benefit from opioids and have no recent history of misuse. The predicted effects of information and hassle costs provide guidance on how we can disentangle these mechanisms.

We begin with hassle costs since, by requiring physicians to log into an electronic system

[10] See http://www.khpi.org/dwnlds/2015/KentuckyHB1ImpactStudyReport03262015.pdf

[11] Some of these exemptions were written directly into HB1 while others came a few months later in Kentucky HB 217. According to Van Ingram, Executive Director of Kentucky's Office of Drug Control Policy, HB 217 was a 2013 "clean up bill" to address things like stakeholder issues with HB1, which was passed in a special legislative session. See https://apps.legislature.ky.gov/record/13rs/hb217.html

[12] Kentucky's mandate also allows physicians to access institutional (e.g., emergency department) PDMP accounts and assign delegates to perform data queries (Haffajee et al. 2018; SAMHSA 2013).

to check records before writing an opioid prescription, a mandate necessarily imposes a hassle cost. Hassle costs should weakly decrease opioid prescribing. When deciding whether to write an opioid prescription, a physician first needs to decide whether the expected net benefit to the patient is greater than the hassle cost of accessing the PDMP. We assume that physicians place some value on the benefit of opioids to patients, so they should be more likely to bear the cost to access the PDMP when a patient's presenting diagnosis is appropriate for opioids (e.g., the pain relief benefit for fractures would typically be greater than for lower back pain). Thus, all else equal, we predict that hassle costs should cause larger decreases in opioid prescribing to patients with conditions that are inappropriate for opioids.

Conditional on bearing the hassle cost, a physician will have access to the information in the PDMP when making prescription decisions. For patients with a recent history of prescription opioid fills, i.e., non-naïve patients, the information reinforces any hassle cost effect such that opioid prescribing should decline. On the other hand, for patients *without* a recent history of prescription opioid fills, i.e., opioid naïve patients, information should work in the opposite direction of hassle costs. Among opioid naïve patients with conditions appropriate for opioids, information about their (lack of) recent opioid history should fully counter the hassle cost effect leading to no change or even an increase in opioid prescribing rates if beliefs about past opioid use are positively updated. For patients who are opioid naïve and have conditions inappropriate for opioids, the hassle cost effect likely dominates, meaning prescriptions decline.

These predictions based on presenting conditions and opioid histories motivate our empirical approach to isolating the independent effects of information and hassle costs. Specifically, using a triple difference framework, within each condition category (appropriate or inappropriate), differences in the mandate's effect on prescribing for non-naïve relative to naïve patients should

isolate the effect of information on prescribing. This approach nets out the effects of hassle costs since hassle costs are constant across these groups while information could only reduce prescribing for the non-naïve.

III. Data

We use claims data from Optum's Clinformatics Data Mart for 2006 to 2016 to conduct our analysis. These data contain commercial claims from a large health insurer covering over 13 million annual enrollees across the United States. Health care claims allow the identification of the opioid history and case severity of patients presenting to an ED. They also provide the state of the facilities where care is delivered.[13]

We create a dataset of all ED visits[14] and identify all opioid prescriptions within a window of 180 days prior through 3 days following an ED visit.[15] We limit our analysis to individuals who are continuously enrolled during this period. This allows us to identify a patient's recent opioid history and whether they obtained an opioid within the 3 days following the visit. We also identify whether they obtained any non-opioid analgesics.[16] Finally, we limit the sample to non-elderly adults ages 18 to 64. Of the 42 million ED visits we identify between 2006 and 2016, we restrict to 16 million visits using the above inclusion criteria.

We classify individuals as "opioid naïve" or "non-naïve" based on prescription fills in the 6 months prior to an emergency department visit. Individuals with at least one fill are "non-

[13] Our Optum extract identifies the state of the facility, but not the patient's residence. We obtain state identifiers by linking the hospital, facility or provider identifiers from the ED claims to Optum's provider dataset. State of the facility takes precedence if there is disagreement between the states of different providers on the claim. We drop 4.75 million visits (14% of visits) where no state can be assigned.
[14] ED visits are identified by place of service codes and revenue, procedure or type of service codes.
[15] Opioids are identified using a list of national drug codes from the CDC. The CDC compilation of opioid analgesics is available at https://www.cdc.gov/drugoverdose/resources/data.html.
[16] AHFS pharmacologic therapeutic classifications for nonsteroidal anti-inflammatory agents (28:08.04) and analgesics and antipyretics, miscellaneous (28:08.92) identify non-opioid analgesics.

naïve", while those without any fills are "opioid naïve."[17] A potential limitation of the naïve measure is that it does not include opioid prescriptions paid for by secondary insurers, although coverage by multiple insurers is less common among those under 65 years old. This measure also does not include opioids purchased with cash at "pill mills" or on the black market. However, since these types of purchases would also not be observed by doctors in the PDMP, they would be unlikely to affect behaviors resulting from the mandate. We believe that the Optum data provides a close approximation to what providers would observe when querying the PDMP.

We also classify whether individuals present to the ED with diagnoses that are considered "appropriate" or "inappropriate" for opioids. Most conditions fall in a gray zone, so we limit these categories to conditions where opioid use is clearly indicated in order to obtain a clean comparison. A widely-used medical decision support tool (UpToDate) characterizes kidney stones and fractures as conditions generally requiring an opioid prescription and headache, sprains, strains and low back pain as conditions generally not requiring an opioid prescription (see Appendix Table A1 for specific diagnosis codes). We label the remaining conditions as "unclassified." Some important limitations of these categories are worth noting. First, because we cannot fully capture severity in claims, we are unable to differentiate between sub-categories of "appropriate conditions," some of which (e.g., minor fractures) may not require an opioid.[18] Second, because inappropriate conditions are less easily verified than appropriate conditions, which often are often diagnosed via X-ray, CT scan or ultrasound, the share of people falling into this category (vs. the unclassified category) could respond to the prescribing regime. In practice in our data, the share inappropriate remains quite stable before and after the mandate.

[17] Although definitions vary in the literature, we follow a commonly used definition for opioid naïve of 6-months (e.g., Barnett et al. 2017). Robustness checks using a 9-month lookback are qualitatively similar to our main results (shown below).

[18] This may partially explain why rates of opioid prescribing for "appropriate" conditions, while much higher than that for inappropriate and unclassified conditions, are well below 100 percent.

Because the Optum data are from a commercially insured population, enrollees in these plans may differ in other ways from the general population. Our estimates should provide a clear understanding of the impact of a strong mandate on opioid prescribing to commercially insured populations, the vast majority of insured individuals in the US, but may not translate fully to, for example, Medicaid-covered populations. The richness of the Optum CDM data, however, allow us to characterize individual opioid histories and medical conditions, which is simply not feasible with Medicaid data (e.g., the State Drug Utilization Data) or nationally representative data (e.g., Medical Expenditure Panel Survey, ARCOS).

IV. Empirical Approach

To understand the mechanisms through which PDMPs reduce opioid prescribing, we use both event-study and difference-in-differences approaches to compare prescribing in Kentucky versus non-mandate states before and after the PDMP mandate went into effect. We analyze these patterns separately by the patient's past history of opioid use and appropriateness of the diagnosis for opioid pain medications.

We begin by estimating aggregate effects for the entire population, using event-study models of the following basic form to assess how prescribing evolved before and after the mandate in Kentucky relative to 34 comparator (non-mandate) states:

$$Y_{st} = \alpha_s + \gamma_t + \delta_t * KY_s + X'_{st}\beta + \varepsilon_{st} \qquad (1)$$

where Y_{st} is the outcome of interest (e.g., share of patients filling an opioid prescription within 3 days of visit) for patients visiting an ED in state s, in quarter-year t. Our regression includes both state α_s and quarter-year γ_t fixed effects. We include controls for demographics and state policy

variables that may influence prescribing and opioid-seeking behavior.[19] Estimates are weighted by the number of ED visits in each cell. Our interest is in δ_t, the coefficients on the quarter-year fixed-effects interacted with the Kentucky indicator. We omit the interaction term for the second quarter of 2012 such that estimates are normalized to the quarter before the PDMP mandate took effect. These models allow us to assess whether our difference-in-differences estimates capture a change in prescribing behavior that is credibly related to Kentucky's mandate.

To summarize the impact of the mandate on prescribing, we estimate a difference-in-differences model of the following form:

$$Y_{st} = \alpha_s + \gamma_t + \delta Post_t * KY_s + \rho Qtr_t * KY_s + X'_{st}\beta + \varepsilon_{st} \qquad (2)$$

where Y_{st}, α_s and γ_t are defined as above. The key coefficient of interest is δ, the interaction between a post-mandate indicator, which equals one beginning in Quarter 3 of 2012, and an indicator for Kentucky. In our preferred specification, we also include a Kentucky-specific linear time trend ($Qtr_t * KY$). As both the time series trends (see Figure 1) and our event study models show, the rate of opioid prescribing in Kentucky in general and relative to most other states was on a downward trend even prior to the mandate. Not including a Kentucky-specific trend may overstate the effect of the mandate on reductions in opioid prescriptions and related outcomes.

To quantify the mechanisms through which mandates affect our outcomes of interest, we first estimate equation (2) separately for patients who are opioid naïve and non-naïve at the time of their ED visit. We also estimate this equation separately for patients who have presenting diagnoses that would or would not be medically appropriate for opioid treatment. The

[19] We include demographic controls for the share of enrollees with some college or more, share white, share male, and the share ages 50 to 64. We also control for several state policies including pill mill laws, medical marijuana laws, active and legal medical marijuana dispensaries, naloxone laws and ACA Medicaid expansion. Data on marijuana laws and dispensaries are from the RAND Marijuana Policy database (see Powell et al. 2018 and Williams et al. 2019). Pill mill and naloxone laws (see Abouk et al. 2019) are coded using the Prescription Drug Abuse Policy System (PDAPS). While the health insurer represented in Optum's database is large, its participation on the ACA exchanges was very limited, particularly in Kentucky where it did not participate until 2016.

information provided in the PDMP alone could not generate reductions in prescribing for opioid naïve patients or generate differential changes in prescribing for appropriate and inappropriate diagnoses conditional on naïve-status. Thus, comparing changes across appropriateness categories and by naïve status serves as a key test for hassle cost effects, as discussed in Section 2.3. Finally, we estimate a triple difference model that fully interacts an indicator for non-naïve with all of the terms in equation (2). This model, which we also estimate separately by opioid appropriateness category, differences out any reduction in prescribing for naïve patients—which is due to hassle costs—isolating the effects of information for non-naïve patients.

To address potential serial correlation in our outcomes, we cluster standard errors by state in our main results. However, because we have only one treated unit (Kentucky), cluster-robust standard errors may be too small and thus lead us to over-reject the null (Conley and Taber 2011). To assess the possibility of over-rejection, we also generate p-values using a variant of Fisher's (1935) permutation test. Specifically, we compare our difference-in-differences estimates to the distribution of placebo estimates where each non-mandate state (34 states) is assigned as the "treated" unit. Since theory predicts that the PDMP mandate *decreases* opioid prescribing, we present p-values from a one-tailed test of the null hypothesis that the mandate weakly increases prescribing. While this test is somewhat less demanding than the two-tailed test, it is still very conservative because with only 34 comparator states, 5% statistical significance requires that Kentucky is ranked at the bottom of the placebo distribution. In robustness tests, we show p-values estimated from two-tailed tests and also estimate p-values using another permutation approach that adjusts placebo estimates based on the variance of the residuals to account for heteroscedasticity due to differences in state population size (Ferman and Pinto 2019). We generally arrive at similar conclusions across the inference methods.

V. Results

5.1 Descriptive Statistics

In Table 1, we provide descriptive statistics for our analytic sample of individuals ages 18-64 with an ED visit in Kentucky and comparator non-mandate states, before and after Kentucky's mandate (July 2012). The top panel shows demographic and patient characteristics. The age distributions are roughly similar across states, but Kentucky has lower levels of education and a higher proportion of the population that is white. Prior to the mandate, 62% of individuals were opioid naïve at the time of their ED visit in Kentucky, compared to 67% in non-mandate states. Across both sets of states, roughly 6% of visits in our sample have diagnosis codes clearly appropriate for opioids (as defined in section III) while about 14% do not. As discussed previously, we focus on the diagnoses that are considered unambiguously appropriate or inappropriate for opioids. The remaining 80% of visits are unclassified, reflecting the substantial gray area in determining the appropriateness of opioid prescriptions.

In the bottom panel of Table 1, we show means for our main outcomes. Prior to the mandate, roughly the same share of patients entering the ED in Kentucky and non-mandate states received an opioid prescription within 3 days of their visit (28% in Kentucky vs. 26% in non-mandate states). Opioid prescription rates declined in *all* states after July 2012, although the decline was steeper in Kentucky. Non-opioid analgesic prescription rates increased slightly in Kentucky but were unchanged in non-mandate states. Conditional on receiving an opioid prescription, the average dosage, measured as total morphine milligram equivalents (MME), declined in both Kentucky and comparator states. Days supplied actually increased slightly in

both groups, although the increase was smaller for Kentucky.[20]

5.2 Opioid Prescribing on the Extensive and Intensive Margins

We first analyze overall opioid prescribing in the ED following the mandate. In addition to analyzing changes on the extensive margin, i.e., the rate of opioid prescribing, we analyze intensive margin changes such as the number of days supplied and dosage. This initial analysis is analogous to prior studies showing the aggregate effects of PDMPs, although we hone in on the specific effects for Kentucky.

Figure 2 shows the event study estimates of the rate of receiving an opioid prescription after an ED visit by quarter in Kentucky relative to non-mandate states. The graph shows a sharp decline in opioid prescription rates in Kentucky relative to non-mandate states right after the mandate took effect in quarter 2 of 2012. The sharp decline is preceded by a more gradual relative decline in prescriptions in Kentucky, which motivates the inclusion of a Kentucky-specific linear trend in our preferred regression specifications. Following the mandate, there is a partial reversion in the estimates towards zero. However, this pattern is driven by a faster pace of decline in non-mandate states beginning in 2013 rather than an increase in prescribing rates in Kentucky, as can be seen in the raw trends of prescription rates in Appendix Figure A3.

We show difference-in-differences estimates summarizing the magnitude of this extensive margin change in the first row of Table 2. Column (1) presents the difference-in-differences estimate without controls. This estimate implies a decline in opioid prescription rates by 5.2 percentage points post-mandate in Kentucky relative to non-mandate states. The estimate is cut roughly in half to 2.7 percentage points when we include a Kentucky-specific linear trend

[20] The mean change masks differential changes in the distribution of days supplied across treatment and control states. As shown in Appendix Figure A2, in Kentucky the distribution of days supplied hollows out, as the share using 3 to 7 days supplied shifts to either 1 or 2 days supplied or more than 7 days supplied. The pattern is quite different in non-mandate states, where the right tail increases after July 2012.

in column (2) but is stable thereafter when we add demographic and policy controls in columns (3) and (4), respectively. Based on our preferred estimate with the full set of controls and Kentucky-specific linear trend, opioid prescription rates following an ED visit decline by 2.3 percentage points or almost 9% off a baseline prescription rate of 26%.

We show changes in opioid prescriptions on the intensive margin in rows 2-6. We focus on results from our preferred specification in column (4). Days supplied, conditional on filling an opioid prescription, decline by about 2.9%. Off a base of about 4.8 days supplied, this is a decline of about 0.14 days. The decline in the mean number of days supplied is primarily driven by a shift from prescriptions with 3 to 7 days supplied towards 1 to 2 days supplied. In contrast, we find no clear change in dosage, as measured by log MME or other measures of dosage (see Appendix Table 2). Thus, the overall rate of prescribing and the number of days of medication supplied for prescriptions declined but dosages remain unchanged.

5.3 Inside the Black Box: Understanding the Mechanisms for Prescribing Changes

Next, we analyze both the intended and unintended mechanisms driving the estimated decline in opioid prescriptions. To isolate the role of information versus hassle costs, we analyze changes in prescribing based on patient characteristics observable to the physician: 1) recent opioid history and 2) the appropriateness of an opioid prescription, which is determined based on the patient's presenting diagnosis.

5.3.1. Opioid Prescription History

In Panels A and B of Table 3, we compare opioid prescribing responses across opioid naïve and non-naïve patients. We find that the rate of opioid prescribing following an ED visit declines after the mandate for both patients with and without a history of opioid prescriptions, consistent with the raw data patterns (see Appendix Figure A4). After the mandate, we estimate a

1.5 percentage point or 6.8% reduction in opioid prescription rates for naïve patients and a 3.5 percentage point or 10.6% decline for non-naïve patients. The sizeable reduction among the opioid naïve is evidence of an unintended hassle cost effect since no information in the PDMP should lead a provider to reduce prescribing to this group. This suggests that some physicians reduce opioid prescribing across the board to avoid the costs of logging into the PDMP.

Declines in prescription rates are larger in both absolute and proportional terms for non-naïve patients relative to naïve patients, suggesting an information effect as well. Conditional on bearing the hassle cost, physicians appear to use the information in the PDMP to distinguish between patients who are at higher (non-naïve) versus lower (naïve) risk of misuse. This is further supported by our finding that, among the non-naïve population, declines in opioid prescriptions are much larger for patients with histories of problematic opioid behaviors. Specifically, in Panels C-E of Table 3, we show prescribing responses for patients who had in the past 6 months: 1) prescriptions from three or more prescribers or pharmacies (Panel C), 2) an average daily dose above 120 MME (Panel D) or 3) more than 30 days of overlapping prescriptions (Panel E).[21] We estimate a nearly 9 percentage point or 20% reduction in opioid prescription rates to both patients who previously received opioid prescriptions from 3 or more prescribers or pharmacies, a potential indication of "doctor shopping", and patients who had a very high average daily MME. Those with more than 30 days of overlap in prior prescriptions experience a smaller decline in prescription rates, suggesting either that this measure disproportionately captures individuals with serious conditions needing pain management or that

[21] We follow Buchmueller and Carey (2018) in defining measures of potential opioid misuse. As in their paper, we use an indicator for high daily dosage, greater than 120 daily MME. We adapt two of their measures of "doctor shopping," 5+ prescribers or 5+ pharmacies, to our setting by defining an indicator for individuals with 3+ prescribers or pharmacies, using a lower threshold given how few individuals in our sample receive prescriptions from more than 5 prescribers. Finally, they define an indicator for overlapping or concurrent prescriptions with the same ingredient. We extend this definition to capture the degree of overlap, creating an indicator equal to 1 for individuals with greater than 30 days of overlapping prescriptions during the previous 6 months.

doctors may not consider overlapping prescriptions an indication of problematic behavior. Overall, we find that both information and hassle costs contribute to the decline in prescribing from the PDMP mandate. We parse out the relative effects of these channels in Section 5.3.3.

5.3.2. Appropriateness of Opioids for Presenting Diagnosis

We also test for prescribing responses to the mandate based on the appropriateness of a patient's presenting diagnosis for opioid treatment. Since the decision to check the PDMP, i.e., incur the hassle cost, is a function of diagnosis and not opioid history (which is revealed only *after* checking the PDMP), physicians should be more likely to incur the hassle costs of accessing the PDMP when medical conditions are most appropriate for opioids (i.e., the benefits exceed the costs). Hence, finding a larger decrease in prescribing for patients with conditions that are *not* appropriate for opioids is also evidence of hassle cost effects.

In Panel A of Table 4, we show the effect of the mandate for individuals presenting with conditions that are most clearly appropriate for opioids (Column 2) and most clearly inappropriate for opioids (Column 3). We also show estimates for individuals with an "unclassified" condition (Column 4).[22] The decline in opioid prescriptions is largely driven by those who are inappropriate for opioids. Specifically, we find a 6-percentage point (or 16%) decline in opioid prescription rates for those presenting with conditions, such as low back pain, that are considered clearly inappropriate for opioids, and a 2-percentage point (10%) decline among the unclassified sample. In contrast, we find no statistically significant decline in prescription rates for patients who are most clearly appropriate for opioids, such as fractures. The

[22] Unclassified conditions are probably closer to inappropriate given recent guidelines that recognize that the risks of opioids often outweigh the benefits and that non-opioid analgesics are equivalent or superior in many cases (CDC 2016; Schug and Goddard 2014; White 2017). The opioid prescription rate for those with an "unclassified" diagnosis (21%) is closer to the rate for those with an "inappropriate" (38%) than an "appropriate" diagnosis (64%). We suspect that the prescribing rate is higher in the inappropriate than unclassified group because conditions currently deemed inappropriate were previously targeted by pharmaceutical companies for an expanded role of opioids (e.g., low back pain).

point estimate is a precisely estimated 0.35 percentage points off a baseline of 64%. The lower 95% confidence interval implies that we can reject declines larger than 1.44 percentage points. Thus, the mandate does not prevent patients appropriate for opioids from getting necessary prescriptions since physicians appear willing to access the PDMP when the benefits of treatment are large enough to exceed the hassle costs. Meanwhile, the additional hassle cost to accessing the PDMP provides an effective hurdle against prescribing for inappropriate conditions.

Panels B and C of Table 4 stratify the sample further by patients who are opioid naïve versus non-naïve. These results show the combined effects of information and hassle costs. Among patients appropriate for opioids, the opioid naïve have a precisely estimated zero change in opioid prescriptions. The lower 95 percent confidence interval rules out declines in prescribing of more than 0.63 percentage points off a base rate of 65%. In contrast, non-naïve patients experience a decline of 3.3 percentage points or about 5%. This is consistent with our predictions that hassle costs reinforce information effects for the non-naïve and counter these effects for the naïve. Among those inappropriate for opioids, both those who are naïve and non-naïve experience large declines in opioid prescribing rates, suggesting hassle cost effects dominate. The asymmetric treatment of naïve versus non-naïve patients and the fact that naïve patients appropriate for opioids experience no change in prescription rates show that providers are using the information in the PDMP in cases where the benefits of treatment outweigh the hassle costs.

5.3.3. Separating the Effects of Information from Hassle Costs

The above evidence demonstrates that both information and hassle costs contribute to prescribing changes. To isolate the effect of information from hassle costs, we estimate a triple differences model in Panel D of Table 4 that compares changes in opioid prescription rates in Kentucky relative to non-mandate states before versus after the mandate for non-naïve relative to

naïve patients. In other words, we are effectively subtracting estimates in Panel B from Panel C. We estimate these effects for the full sample and separately by whether or not the patient is appropriate for opioids. This model nets out reductions in prescribing for naïve patients—which are due to hassle costs—isolating the effects of information for non-naïve patients.[23]

For the full sample and all subgroups, the triple difference estimates are negative and statistically significant, implying a role for information. In particular, for non-naïve relative to naïve patients, information leads to a 4.2 percentage point decline in opioid prescribing rates for those with diagnoses appropriate for opioids, a 3.9 percentage point decline for those with diagnoses inappropriate for opioids and a 1.3 percentage point decline for those with unclassified diagnoses. Dividing these effects by the estimates in Panel C shows the proportion of the mandate's effect for the non-naive due to information. For the non-naïve who are appropriate for opioids, information explains more than the full decline in prescribing, meaning prescription rates would have slightly increased absent the hassle cost. In contrast, information explains only about 46% of the decline for the non-naive with inappropriate or unclassified diagnoses. Thus, for appropriate conditions, when the doctor is most likely to access the PDMP, information dominates but for inappropriate and unclassified conditions hassle costs play the largest role in discouraging opioid prescribing. This fits our intuition that doctors are less willing to check the PDMP for inappropriate cases. For the opioid naïve, hassle costs may explain the entire decline in prescribing.

Overall, for the full sample in column (1), the triple difference estimate implies that the information from the PDMP explains 31% of the decline in prescribing across all patients

[23] We assume that doctors view diagnoses within appropriateness categories the same pre and post-mandate relative to non-mandate states and that the distribution of diagnoses is similar by naïve and non-naïve status within appropriateness groups before and after the mandate. This latter assumption is supported by our later evidence that ED visit counts did not change after the mandate.

following the mandate, with the remaining 69% due to hassle costs.[24] Intuitively, information explains a larger share of the decline among non-naïve patients, roughly 54% of the effect, as they have a history of opioid prescriptions in the PDMP. However, because the naïve population accounts for a larger share of ED visits, the prescribing reductions for this population— which are due to hassle costs— explain a larger share of the overall decline in opioid prescribing.

One important caveat to interpreting the reductions (net of information) as hassle costs is that the mandate may also unintentionally reduce prescribing by increasing the salience of government monitoring.[25] Since PDMPs were in place as repositories of prescription data for many years prior to the mandate, doctors were likely already aware of the monitoring. Although a mandate may heighten the salience of monitoring, we expect any prescribing responses to changes in salience to be relatively limited. In contrast, hassle costs increased significantly after the mandate (indeed, this is the main difference between mandate and non-mandate PDMPs) and information provision also increased as many more providers accessed the PDMP. While it is difficult to fully separate hassle costs from salience since these effects are reinforcing, our results still show that information provision—the intended mechanism of PDMPs— is less important for reducing opioid prescribing than the unintended mechanisms.

5.3.4. Robustness Tests

We take several approaches to testing the robustness of the main conclusions from Table 4. First, we assess whether the changes in prescribing that we attribute to providers' decisions can instead be explained by demand-side mechanisms. Monitoring of patient behavior through

[24] We observe a decline due to information of 0.019 for the non-naïve which is 54.3% (0.019/0.035) of the total decline in prescribing for the non-naïve. Since 38% of ED visits are for non-naïve patients, the decline due to information accounts for 31% (.38*(.035/0.023)*(0.019/0.035)) of the decline in prescribing for the full sample. Most of the decline comes from the naïve sample, which reflects hassle costs.

[25] The effects of increased salience on opioid prescribing will depend on the specific model considered. The mandate may increase the salience of the government's monitoring or make salient the harms of opioids generally and thus lead to greater caution in prescribing opioids for all patients. It could also make providers more cautious in the prescriptions they do write (e.g., by reducing days supplied or daily doses).

PDMPs may deter drug-seeking individuals from coming to the ED, either due to the fear of detection or knowledge that doctors are less likely to prescribe opioids when they are mandated to use the PDMP. To test for demand-side factors we examine changes in the composition of ED visits after the mandate.[26] Figure 3 shows the quarterly count of ED visits in Kentucky (Panel A) and non-mandate states (Panel B) for naïve and non-naïve patients. In Kentucky, the trend for naïve patients is upward and smooth for the entire study period. For non-naïve patients, the group more likely to be drug-seeking, we see a slight downward trend after the mandate. While some of this may be due to a reduction in the stock of non-naïve patients in Kentucky given the mandate's effects on prescribing, such a change would, all else equal, be offset by an increase in visits by naïve patients, which we do not observe. In non-mandate states, we find little evidence of a trend break for naïve or non-naïve patients. In Table 5, we test more formally for demand-side changes by estimating the relationship between the mandate and log ED visit counts overall, by naïve status and by diagnostic appropriateness. The estimates are small and not statistically distinguishable from zero. Overall, these results suggest that demand-side responses to the mandate, at least in the ED setting, are minimal. Therefore, we interpret the changes in prescription patterns found here as largely supply-side or provider-driven.

Second, in Appendix Table A3, we explore alternative approaches to statistical inference. Specifically, we estimate p-values for the results in Table 4 using both one and two-tailed permutation tests and the Ferman-Pinto modified permutation test. Overall, we find similar patterns of results. For the full sample, we can reject zero change in opioid prescription rates in Kentucky relative to non-mandate states in all but the two-tailed permutation test. Given that we are conducting permutation tests using only 34 control states, the two-tailed tests (and even the

[26] At the extreme, for example, the decline in the opioid prescribing rate could be driven by a reduction in the number of people coming to the ED seeking opioids—who receive opioids at a higher rate than non-opioid seeking patients—rather than any change in the propensity of clinicians to prescribe opioids.

one-tailed tests) may be overly conservative. In contrast, the Ferman-Pinto method uses a bootstrap approach that more effectively accounts for the small number of units. The decline in prescribing overall is driven by those with inappropriate diagnoses, where we can reject zero change using both one and two-tailed permutation tests and the Ferman-Pinto method. For patients who are appropriate for opioids, we cannot reject zero change in opioid prescription rates, irrespective of the inference method. These results suggest our general conclusions are not due to over-rejecting the null hypotheses.[27]

Third, we test the sensitivity of our results to the empirical specification. In Appendix Table A5, we show estimates allowing the slope of the linear Kentucky-specific trend to vary before versus after the mandate to isolate the change at the quarter of mandate adoption. This estimate, like a regression discontinuity estimate, isolates the change in outcomes more locally to the policy change. We obtain estimates quite similar to our main estimate– about a 3-percentage point or 12% decline in the likelihood of receiving a prescription after the mandate.

In Appendix Table A6, we show results that include all states as controls instead of only states that did not adopt a mandate during the study period. Including all states as controls and separately estimating the effects of the mandate for Kentucky and for other mandate states, supports our general conclusions. This specification suggests that hassle costs may play an even bigger role than our main specification indicates – as evidenced by the decline in prescribing among the appropriate opioid naïve sample. However, we are cautious in interpreting these results, because using other treated states as controls may confound our estimates due to the staggered adoption of treatment (Goodman-Bacon 2019). For this reason, we focus on the

[27] Appendix Table A4 shows the ranking of Kentucky's mandate effects across all states in the sample from the permutation test. Kentucky ranks near the bottom of the distribution for all samples. Appendix Figures A5 and A6 replicate the event study results comparing estimates for Kentucky with the 5th and 95th percentile of coefficients from the placebo distributions.

cleaner control group of non-mandate states in our main results.

In Appendix Table A7, we show results using a 9-month instead of a 6-month lookback period to identify opioid histories. These results are very similar to those in Table 4, with declines across opioid naïve and non-naïve patients and differential treatment of the naïve appropriate versus non-naïve appropriate groups.[28]

Finally, in Appendix Table A8 we exclude patients with a benzodiazepine prescription in the 6 months prior to their ED visit. Benzodiazepines are a scheduled class of drugs subject to the PDMP in Kentucky (Freeman et al. 2015) that interact with opioids and increase the risk of overdose. We conduct this check to rule out the alternative explanation that a history of benzodiazepines explains the decline in opioid prescriptions, particularly among opioid naïve patients. The results are very similar to the main results in Table 4, bolstering the evidence for the role of hassle costs.

5.3.5. Intensive Margin Responses

While less informative about mechanisms than changes on the extensive margin, changes on the intensive margin are of interest in their own right. In Table 6, we show that conditional on an opioid prescription, days supplied declines by about 3%.[29] Naïve patients experience a roughly 5% decline in days supplied. The decline is driven by a reduction in the share of prescriptions with 3 or more days of supply. Since writing a prescription implies that the hassle costs have already been borne, these intensive margin changes cannot be directly due to hassle costs. Instead, this reduction could be due to a composition change in the type of patients receiving opioids due to hassle costs or to a salience effect that causes providers to be more cautious. In contrast, non-naïve patients see a 2.5% *increase* in days supplied, although this is

[28] Requiring a 12-month lookback, consistent with Kentucky's law, reduces the sample by nearly 20% and in a non-random way. Dropped patients are more likely to be non-naïve.
[29] Dosages, however, are unaffected (see Table 2 and Appendix Table A2).

not statistically significant. This change is driven by an increase in the right tail (share of prescriptions with more than 7 days supplied). These patterns may reflect compositional changes if, for example, providers reduce prescriptions generally but not to patients who are already using opioids because of a pre-existing, high severity condition that requires more days of supply to manage. However, these patterns could also reflect a shift towards high dose prescribers if other prescribers are less likely to write prescriptions. Data limitations make it difficult to measure severity within a given diagnosis code or track prescribers (as opposed to patients) in the data over time, precluding us from formally testing for compositional changes. Since it is difficult to separate the above mechanisms, we are cautious in concluding which channels drive the results on the intensive margin.

5.4 Consequences of Reduced Opioid Supply

Another key question, particularly in light of the importance of hassle costs in reducing opioid prescriptions, is how the mandate impacted patient outcomes. We consider whether the mandate increased substitution to non-opioid analgesics. We also examine outcomes in the year following the index ED visit, including the likelihood of a follow-up ED visit and long-term opioid use, a measure of possible opioid dependence.

5.4.1. Non-opioid Analgesic Prescriptions

As providers reduce opioid prescribing, they may substitute to non-opioid analgesics (NOAs) as an alternative source of pain management. At the same time, NOAs may be co-prescribed with opioids for pain relief and could decrease after the mandate. Figure 4 shows event study estimates for NOA prescription rates in Kentucky relative to non-mandate states. The graph shows a sharp relative increase in NOA prescribing in Kentucky just after the mandate took effect. Panel B, which shows event studies by naïve status, suggests that the increase was

slightly larger for naïve patients.

Table 7 quantifies these effects. The increase in NOA prescription rates after the mandate is much smaller than the decrease in opioid prescriptions, suggesting that substitution is not one-for-one. NOAs increase by just 0.72 percentage points (about 10%) compared to the 2.3 percentage point reduction in opioids. This increase is likely understated since many NOAs are available over-the-counter and not observable in claims data. The increase in NOAs is of similar magnitude for both naïve and non-naïve patients overall. However, the increases seem to be driven largely by patients with inappropriate or unclassified diagnoses where hassle costs play the largest role. This suggests that it is hassle costs (i.e., doctors prescribe NOAs to avoid checking the PDMP) and not information that drives this substitution. Since NOAs are most likely to be prescribed when opioids are inappropriate, it is unlikely this substitution has negative health consequences. However, given that doctors may recommend and patients use over the counter NOAs not observed in our data, we urge some caution in interpreting these results.

5.4.2. *Patient Outcomes*

We also examine how the prescribing changes due to the mandate affected patient outcomes over the longer term. In Table 8 we analyze the number of ED visits in the year following the index visit (col (1)) and long-term opioid use, defined as any opioid use between 180 and 365 days after the ED visit (col (2)). We exclude the first 180 days to allow time for the resolution of a medical issue that brought the patient to the index ED visit and may have necessitated opioids. In columns (3) and (4) of Table 8 we analyze the log total MME and log total days supplied of opioids conditional on having filled at least one opioid within the 180 to 365 day period. For the full sample, in Panel A, the number of return ED visits declines by 0.07 days or almost 7%, although this estimate is not statistically different from zero. The overall

decline is driven by the opioid naïve population. Those patients who were opioid naïve at the index ED visit were less likely to get an opioid prescription at that visit and have fewer ED visits in the following year. The decline is about 0.08 visits or almost 15% off a mean of 0.5 visits. The estimated change for the non-naïve sample is negative but small in magnitude (0.03 days or 1.3% off a mean of 2.1 days) and statistically indistinguishable from zero.

We find no evidence that the likelihood of long-term opioid use changes for the full sample or for those who were opioid naïve at the initial ED visit. However, long-term use declines by 0.66 percentage points or almost 3% off a base rate of 23% for the non-naïve sample. Conditional on long-term opioid use, total MME declines by nearly 30% for the naïve sample and about 16% for the non-naïve sample. Days supplied declines by about 12% or almost 3 days for the naïve filling prescriptions in the long term but is unchanged for the non-naïve sample. These outcomes reflect both the effects of the initial reduced likelihood of receiving an opioid prescription at the index ED visit and the continued effect of the mandate for opioid prescriptions at each subsequent visit. On net, these results suggest that outcomes do not get worse and weakly improve for both naïve and non-naïve patients.

VI. Conclusions

This paper is among the first to delve inside the black box of PDMPs to isolate the role of information versus hassle costs in improving opioid prescribing. To do so, we focus on Kentucky's landmark PDMP mandate, which dramatically reduced opioid prescribing in the state. Using data for commercially insured adults in the ED setting, we examine changes in opioid prescribing by patient prescription history and presenting diagnoses.

Our results imply that opioid prescribing declines through multiple supply-side channels.

We find lower opioid prescription rates after an ED visit for patients with and without a history of opioid prescriptions, evidence of a hassle cost effect that decreases prescribing generally. Declines in prescription rates are larger for patients with a recent history of opioid prescriptions, particularly those with problematic histories, suggesting an information effect that enables physicians to distinguish between naïve and non-naïve clinically appropriate patients.

The reduction in prescriptions is largely driven by patients presenting with conditions inappropriate for opioids. We find smaller effects among patients who come in with conditions considered clinically appropriate for these medications, with no reduction in prescribing among opioid naïve patients presenting with appropriate conditions. These results suggest that hassle costs do not deter physicians from accessing the PDMP in cases where patients may benefit the most from opioids.

Using estimates from a triple difference model we quantify the relative contribution of hassle costs and information. We find that the hassle costs from the mandate explain 69% of the decline in prescribing, concentrated among patients presenting with conditions that are inappropriate for opioids. The information provided by the PDMP mandate explains the remaining 31% of the decline, driven by patients who have a history of opioid prescriptions.

Analysis of outcomes at 1 year of follow-up suggests modest improvement in patient health as measured by return ED visits and subsequent prescription opioid use. These results are important for ongoing efforts to improve physician prescribing, including efforts to lower PDMP hassle costs. Our work suggests these efforts could inadvertently undermine some of the positive effects of PMDPs. When combined with improved information, hassle costs can improve the targeting of opioid treatment.

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Table 1: Descriptive Statistics

	Kentucky		Non-Mandate States	
	Before	After	Before	After
Demographic Variables				
% Some College or More	0.43	0.44	0.66	0.66
	(0.02)	(0.01)	(0.12)	(0.12)
% White	0.88	0.87	0.67	0.65
	(0.01)	(0.01)	(0.11)	(0.12)
% Male	0.44	0.45	0.42	0.42
	(0.01)	(0.02)	(0.02)	(0.02)
% Age 18-34	0.36	0.35	0.33	0.32
	(0.02)	(0.02)	(0.04)	(0.05)
% Age 35-49	0.36	0.32	0.35	0.31
	(0.02)	(0.01)	(0.02)	(0.02)
% Age 50-64	0.27	0.33	0.32	0.37
	(0.02)	(0.02)	(0.04)	(0.05)
Patient Characteristics				
Share of Visits Opioid Naive	0.62	0.68	0.67	0.65
	(0.01)	(0.03)	(0.06)	(0.07)
Share of Visits Opioid Appropriate	0.06	0.06	0.05	0.05
	(0.01)	(0.00)	(0.01)	(0.01)
Share of Visits Opioid Inappropriate	0.14	0.15	0.14	0.15
	(0.01)	(0.01)	(0.01)	(0.02)
Outcome Variables				
Any Opioid Rx	0.28	0.19	0.26	0.23
	(0.02)	(0.02)	(0.03)	(0.03)
Days Supply	4.67	5.02	4.79	5.46
	(0.26)	(0.35)	(0.51)	(0.74)
Share <=2 Days Supply	0.33	0.38	0.25	0.24
	(0.05)	(0.03)	(0.06)	(0.08)
Share 3-7 Days Supply	0.58	0.51	0.65	0.63
	(0.05)	(0.04)	(0.05)	(0.07)
Share >7 Days Supply	0.09	0.11	0.09	0.12
	(0.01)	(0.02)	(0.02)	(0.03)
MME	83.86	75.33	93.03	81.94
	(5.45)	(2.71)	(7.54)	(7.15)
Any Non-Opioid Analgesic Rx	0.07	0.09	0.08	0.08
	(0.01)	(0.01)	(0.02)	(0.02)
Observations	70,244	54,938	6,433,491	5,529,598

Note: Means and percentages are calculated using Optum data from 2006-2016 using the ED visit as the unit of observation. Means are pooled before and after Q3 of 2012 (the introduction of the Kentucky mandate). See section III for definitions of the variables. Days supply and MME are conditional on receiving a prescription following the ED visit. For long term outcomes, total days supply and total MME are conditional on having at least one prescription between 60 and 365 days after the ED visit.

Table 2: The Effect of PDMP Mandates on the Supply of Opioids

	(1)	(2)	(3)	(4)
Outcome Variable				
Any Opioid Rx	-0.052***	-0.027***	-0.024***	-0.023***
	(0.0022)	(0.0022)	(0.0023)	(0.0025)
Dep. Var. Mean	0.26	0.26	0.26	0.26
Log Days	-0.088***	0.00036	-0.022***	-0.029**
	(0.012)	(0.0047)	(0.0035)	(0.0081)
Dep. Var. Mean	4.79	4.79	4.79	4.79
<=2 Days Supply	0.067***	0.013**	0.019***	0.025**
	(0.0080)	(0.0041)	(0.0032)	(0.0081)
Dep. Var. Mean	0.25	0.25	0.25	0.25
3-7 Days Supply	-0.056***	-0.029***	-0.026***	-0.030***
	(0.0063)	(0.0039)	(0.0041)	(0.0080)
Dep. Var. Mean	0.65	0.65	0.65	0.65
>7 Days Supply	-0.011**	0.016***	0.0065***	0.0047*
	(0.0039)	(0.0013)	(0.0018)	(0.0022)
Dep. Var. Mean	0.094	0.094	0.094	0.094
Log MME	-0.025	0.0068	-0.018**	-0.0077
	(0.013)	(0.0045)	(0.0054)	(0.0097)
Dep. Var. Mean	235.5	235.5	235.5	235.5
N	1540	1540	1540	1540
Trend	No	Yes	Yes	Yes
Demographic Controls	No	No	Yes	Yes
Policy Controls	No	No	No	Yes

Note: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are clustered at the state level. Each estimate is the coefficient on the difference-in-differences term (post x KY) from a separate regression. All specifications include state and year fixed effects. Each row represents a different outcome variable and each column represents a different specification. Col (1) is the difference-in-differences regression without controls, Col (2) adds a KY specific linear trend, Col (3) adds demographic controls, Col (4) adds policy controls.

Table 3: PDMP Mandate Effects on Opioid Prescriptions by Opioid History

		(1)
Panel A: Naive		
Post x Kentucky		-0.015***
		(0.0022)
	Dep. Var. Mean	0.22
Panel B: All Non-Naive		
Post x Kentucky		-0.035***
		(0.0032)
	Dep. Var. Mean	0.33
Panel C: >2 Prescribers or Pharmacies		
Post x Kentucky		-0.087***
		(0.0044)
	Dep. Var. Mean	0.43
Panel D: >120 Average Daily MME		
Post x Kentucky		-0.087***
		(0.0035)
	Dep. Var. Mean	0.43
Panel E: >30 Days Overlap		
Post x Kentucky		-0.018*
		(0.0079)
	Dep. Var. Mean	0.44
	N	1538

Note: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are clustered at the state level. Each estimate shows the coefficient on the difference-in-differences term (post x KY) from a separate regression. All specifications include state and year fixed effects, KY specific linear trend, and full set of controls. Outcome is the share of patients receiving an opioid following an ED visit. Panel A repeats the estimate from Table 3 (Panel C, Col (1)). Panels B, C and D are subsets of the opioid non-naive sample of Panel A.

Table 4: PDMP Mandate Effects on Opioid Prescriptions by Presenting Diagnosis

	All	Appropriate	Inappropriate	Unclassified
Panel A: All				
Post x Kentucky	-0.023***	-0.0035	-0.061***	-0.021***
	(0.0025)	(0.0056)	(0.0044)	(0.0022)
Dep. Var. Mean	0.26	0.64	0.38	0.21
Panel B: Naive				
Post x Kentucky	-0.015***	0.0088	-0.045***	-0.015***
	(0.0022)	(0.0077)	(0.0037)	(0.0018)
Dep. Var. Mean	0.22	0.65	0.34	0.17
Panel C: Non-Naive				
Post x Kentucky	-0.035***	-0.033***	-0.083***	-0.028***
	(0.0032)	(0.0081)	(0.0061)	(0.0028)
Dep. Var. Mean	0.33	0.63	0.45	0.29
N	1540	1532	1540	1540
Panel D: Triple Difference				
Post x KY x Non-Naive	-0.019***	-0.042**	-0.039***	-0.013***
	(0.0024)	(0.012)	(0.0044)	(0.0022)
N	3080	3072	3080	3080

Note: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are clustered at the state level. Each estimate shows the coefficient on the difference-in-differences term (post x KY) from a separate regression. All specifications include state and year fixed effects, KY specific linear trend, and full set of controls. In the triple differences specification we fully interact the non-naive indicator with fixed effects and controls. Outcome is the share of patients receiving an opioid following an ED visit. Each panel and column represent a different sample. Col (1) shows estimates from the full sample of diagnosed conditions. Col (2) contains ED visits with diagnosis codes for opioid appropriate conditions, Col (3) contains visits for opioid inappropriate conditions, Col (4) contains visits that are unclassified (neither appropriate nor inappropriate).

Table 5: Demand-Side Effects of PDMP Mandates on ED Visits

	All	Appropriate	Inappropriate
Panel A: All			
Post x Kentucky	-0.0098	0.035	0.049
	(0.094)	(0.098)	(0.096)
Dep. Var. Mean	19154.5	1018.2	2655.8
Panel B: Naive			
Post x Kentucky	0.014	0.046	0.088
	(0.10)	(0.099)	(0.10)
Dep. Var. Mean	12822.7	715.3	1721.4
Panel C: Non-Naive			
Post x Kentucky	-0.035	0.014	-0.019
	(0.09)	(0.11)	(0.09)
Dep. Var. Mean	6331.8	302.8	934.4
N	1540	1532	1540

Note: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are clustered at the state level. Each estimate shows the coefficient on the difference-in-differences term (post x KY) from a separate regression. All specifications include state and year fixed effects, KY specific linear trend, and full set of controls. Outcome is the log count of ED visits. Each panel and column represent a different sub-sample.

Table 6: PDMP Mandate Effects on Days Supplied by Naïve Status

	Log Mean Days	<=2 Days	3-7 Days	>7 Days
Panel A: All				
Post x Kentucky	-0.029**	0.025**	-0.030***	0.0047*
	(0.0081)	(0.0081)	(0.0080)	(0.0022)
Dep. Var. Mean	4.79	0.25	0.65	0.094
Panel B: Naive				
Post x Kentucky	-0.050***	0.033***	-0.027**	-0.0060***
	(0.0090)	(0.0090)	(0.0081)	(0.0016)
Dep. Var. Mean	3.78	0.27	0.68	0.044
Panel C: Non-Naive				
Post x Kentucky	0.025	0.011	-0.042***	0.031***
	(0.012)	(0.0078)	(0.0077)	(0.0050)
Dep. Var. Mean	6.18	0.23	0.61	0.16
N	1540	1540	1540	1540

Note: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are clustered at the state level. Each estimate shows the coefficient on the difference-in-differences term (post x KY) from a separate regression. All specifications include state and year fixed effects, KY specific linear trend, and full set of controls. Each column represents a different outcome variable. Panel A shows estimates from the full sample, Panel B from the opioid naive sample, and Panel C from the opioid non-naive sample.

Table 7: Effects of PDMP Mandates on the Rate of Non-Opioid Analgesic Prescriptions

	All	Appropriate	Inappropriate
Panel A: All			
Post x Kentucky	0.0072***	0.015***	0.0050
	(0.0014)	(0.0024)	(0.0030)
Dep. Var. Mean	0.076	0.13	0.17
Panel B: Naive			
Post x Kentucky	0.0069***	0.019***	-0.012***
	(0.0013)	(0.0030)	(0.0033)
Dep. Var. Mean	0.078	0.14	0.19
Panel C: Non-Naive			
Post x Kentucky	0.0064***	0.0026	0.027***
	(0.0016)	(0.0037)	(0.0030)
Dep. Var. Mean	0.071	0.11	0.14
N	1540	1532	1540

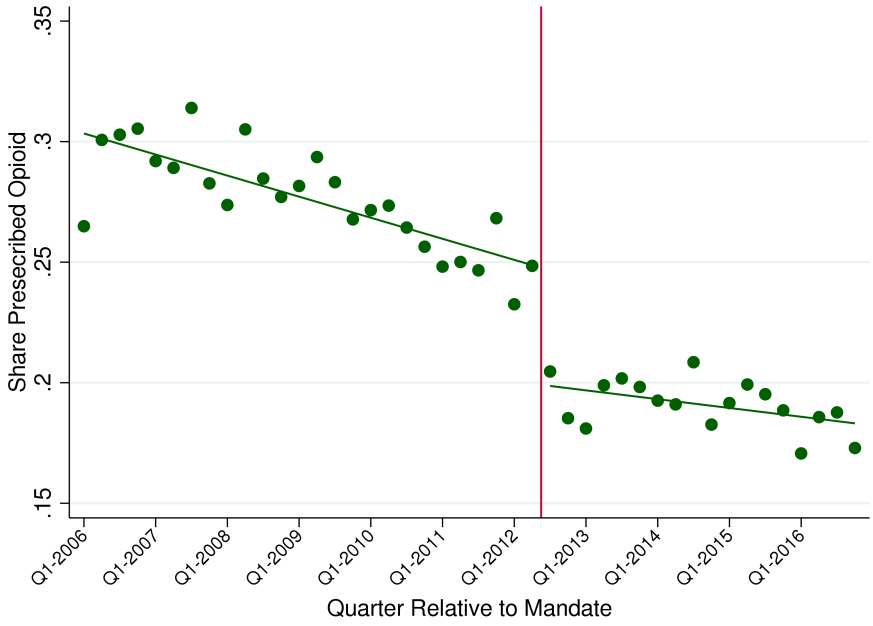
Note: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are clustered at the state level. Each estimate shows the coefficient on the difference-in-differences term (post x KY) from a separate regression. All specifications include state and year fixed effects, KY specific linear trend, and full set of controls. Outcome is the share of patients receiving a non-opioid analgesic prescription following an ED visit. Each panel and column represent a different sub-sample.

Table 8: Effects of PDMPs on Long-Term Outcomes

	ED Visit Count	Long-Term Opioid Use	Log Total MME	Log Total Days
Panel A: All				
Post x Kentucky	-0.073	-0.0027	-0.11*	0.0056
	(0.047)	(0.0014)	(0.044)	(0.037)
Dep. Var. Mean	1.05	0.11	2986.9	39.4
Panel B: Naive				
Post x Kentucky	-0.077***	-0.0019	-0.32***	-0.24***
	(0.018)	(0.00096)	(0.070)	(0.053)
Dep. Var. Mean	0.52	0.057	622.6	13.2
Panel C: Non-Naive				
Post x Kentucky	-0.027	-0.0066**	-0.17**	-0.050
	(0.12)	(0.0020)	(0.053)	(0.044)
Dep. Var. Mean	2.14	0.23	4127.2	52.8
N	1505	1505	1061	1061

Note: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are clustered at the state level. Each estimate shows the coefficient on the difference-in-differences term (post x KY) from a separate regression. All specifications include state and year fixed effects, KY specific linear trend, and full set of controls. Each panel represents a different sample and each column represents a different outcome variable. Any long term use is defined as the share of patients with an opioid prescription between 180 and 365 days after the ED visit. Log MME and log days supply are the sum of MME and days supply between 180 and 365 days after the ED visit, conditional on filling at least one prescription during that time period.

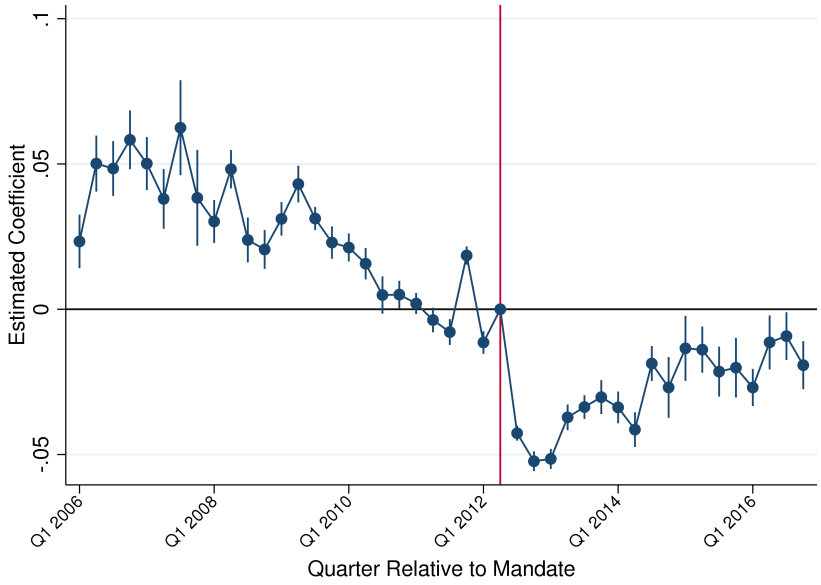
Figure 1: Rate of Opioid Prescriptions following ED Visits in Kentucky



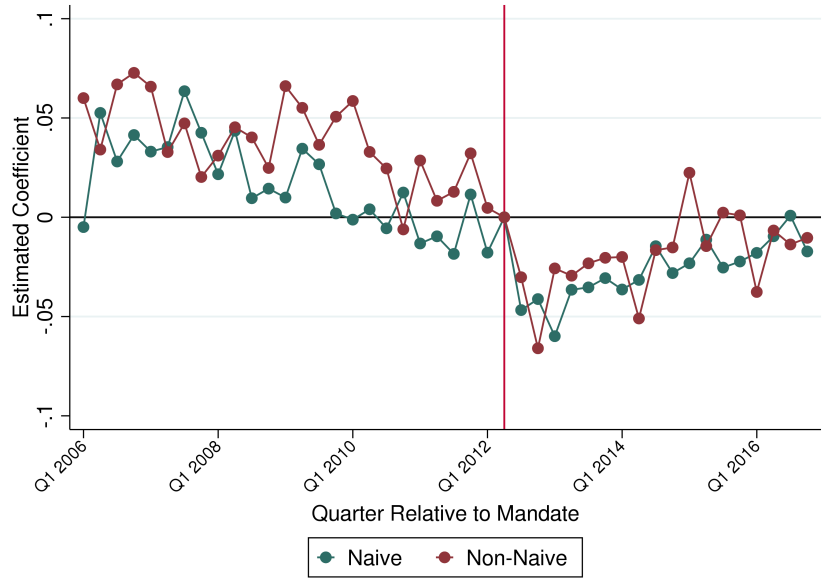
Note: Share of patients receiving an opioid following an ED visit in KY using Optum data from 2006-2016. Vertical line represents introduction of KY mandate in Q3 of 2012.

Figure 2: Event Study: Rate of Opioid Prescription

(a) Panel A: All



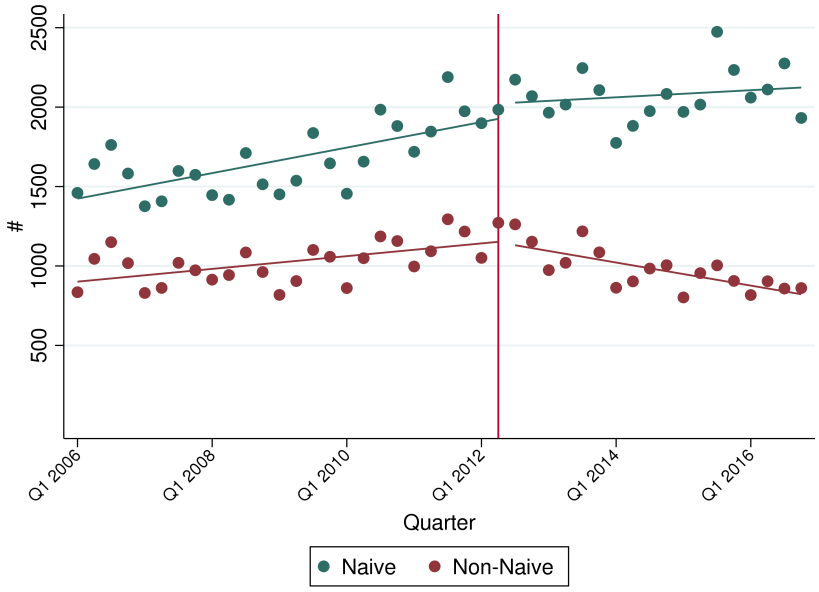
(b) Panel B: By Naive Status



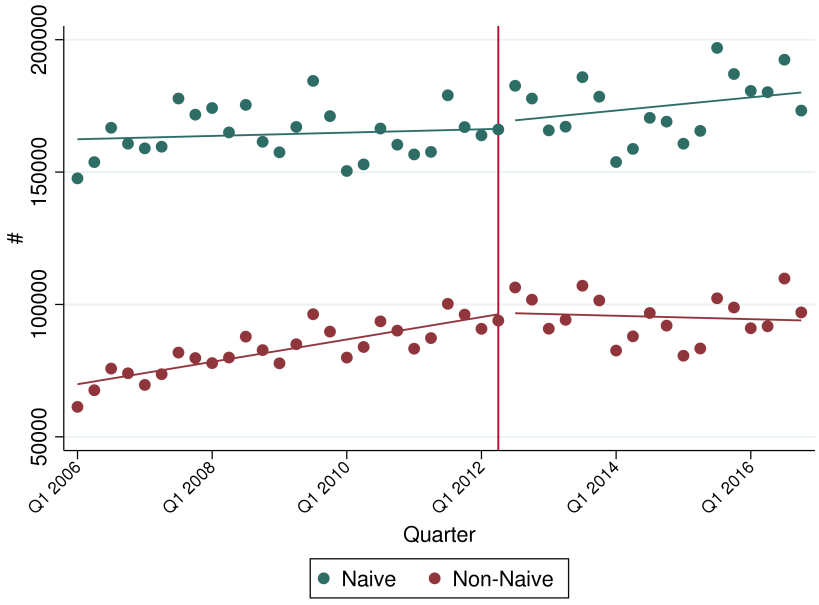
Note: Each graph includes point estimates from the event study (normalized to 0 in Q2:2012) and 95% confidence intervals which are adjusted for within-state clustering. Outcome is the share of patients receiving an opioid following an ED visit. Panel A shows the full sample, Panel B shows separate event study coefficients for opioid naive and non-naive samples.

Figure 3: Count of Emergency Department Visits

(a) Panel A: Kentucky



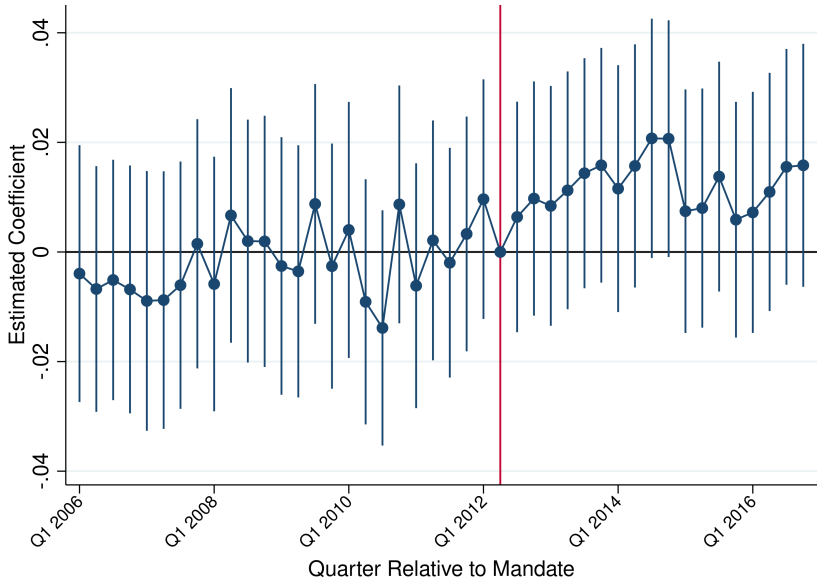
(b) Panel B: Non-Mandate States



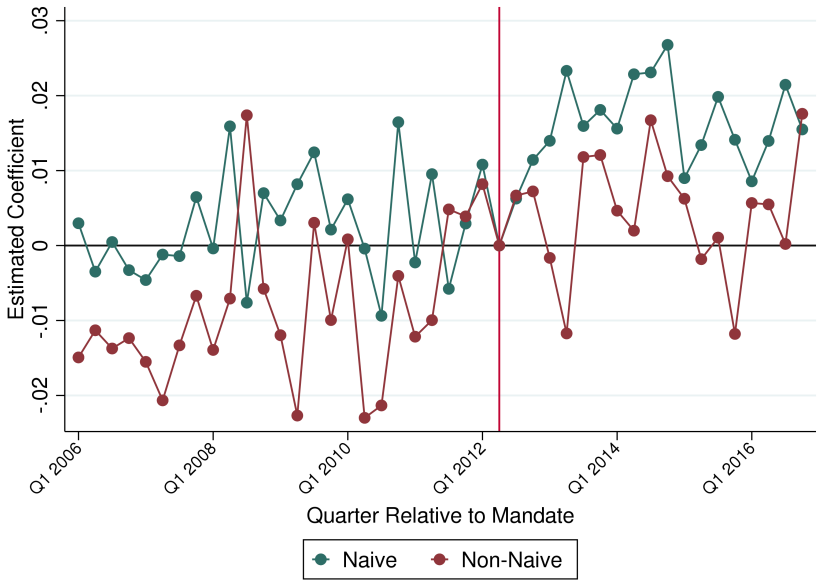
Note: Total number of ED visits in Optum data from 2006-2016. Each graph shows number of ED visits where patient is opioid naive and non-naive. Panel A shows ED visits in KY and Panel B shows ED visits in non-mandate states. Vertical line represents introduction of KY mandate in Q3 of 2012.

Figure 4: Event Study: Rate of Non-Opioid Analgesic Prescriptions

(a) Panel A: All



(b) Panel B: By Naive Status



Note: Each graph includes point estimates from the event study (normalized to 0 in Q2:2012) and 95% confidence intervals which are adjusted for within-state clustering. Outcome is the share of patients receiving a non-opioid analgesic following an ED visit. Panel A shows the full sample, Panel B shows separate event study coefficients for opioid naive and non-naive samples.

Appendix

Table A1: Appropriate and Inappropriate Condition Definitions

Category	Condition	Diagnosis Codes
Appropriate	Kidney Stones	ICD-9 Codes: 592X ICD-10 Codes: N20X
	Fractures	ICD-9 Codes: 800X-830X ICD-10 Codes: M84X, M80X, SX2X
Inappropriate	Headache	ICD-9 Codes: 784X ICD-10 Codes: G44X, R51X
	Sprains/Strains	ICD-9 Codes: 840X-848X, S93X ICD-10 Codes: SX3X, SX6X, SX9X
	Lower Back Pain	ICD-9 Codes: 7242X ICD-10 Codes: M545X, S399X

Note: Opioid-appropriate and -inappropriate conditions were identified using prescribing recommendations in *UpToDate*, which identifies headache, sprains, strains and lower back pain as generally not requiring opioids for treatment. Kidney stones and fractures are identified as causing more severe pain and requiring a prescription of opioids. The remainder of conditions are unclassified. Source: Pino, C. A., & Covington, M. (2019) Prescription of opioids for acute pain in opioid naïve patients. *UpToDate*. Available online. Accessed February, 11.

Table A2: PDMP Effects on the Intensive Margin

	Daily MME>50	Log MME | 1-2 Days	Log MME | 3-7 Days	Log MME | >7 Days
Panel A: All				
Post x Kentucky	0.00037	-0.0059	-0.017	0.047
	(0.0067)	(0.011)	(0.0089)	(0.037)
Dep. Var. Mean	0.29	4.41	4.91	6.25
Panel B: Naive				
Post x Kentucky	-0.000098	0.0026	-0.015	0.10**
	(0.0075)	(0.012)	(0.0100)	(0.034)
Dep. Var. Mean	0.25	4.39	4.87	5.49
Panel C: Non-Naive				
Post x Kentucky	0.0058	-0.016	-0.0087	0.0020
	(0.0064)	(0.012)	(0.0086)	(0.028)
Dep. Var. Mean	0.33	4.44	4.98	6.54
N	1540	1539	1540	1519

Note: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are clustered at the state level. Each estimate shows the coefficient on the difference-in-differences term (post x KY) from a separate regression. All specifications include state and year fixed effects, KY specific linear trend, and full set of controls. Each column represents a different outcome variable. For Cols 2-4, the outcome is Log MME conditional on the number of days supply (e.g., Col (2) is Log MME conditional on receiving a prescription for 1-2 days supply). Panel A shows estimates from the full sample, Panel B from the opioid naive sample, and Panel C from the opioid non-naive sample.

Table A3: P-values from Different Inference Methods

	All	Appropriate	Inappropriate
Panel A: All			
Coefficient	-0.023	-0.0035	-0.061
Cluster Robust	<0.001	0.54	<0.001
One-Tailed			
Ferman Pinto	0.032	0.41	<0.001
Permutation Test	0.057	0.46	0.029
Two-Tailed			
Ferman Pinto	0.059	0.79	<0.001
Permutation Test	0.14	0.86	0.057
Panel B: Naive			
Coefficient	-0.015	0.0088	-0.045
Cluster Robust	<0.001	0.26	<0.001
One-Tailed			
Ferman Pinto	0.071	0.71	0.004
Permutation Test	0.14	0.57	0.057
Two-Tailed			
Ferman Pinto	0.13	0.60	0.0044
Permutation Test	0.23	0.71	0.086
Panel C: Non-Naive			
Coefficient	-0.035	-0.033	-0.083
Cluster Robust	<0.001	<0.001	<0.001
One-Tailed			
Ferman Pinto	0.024	0.053	<0.001
Permutation Test	0.029	0.31	0.029
Two-Tailed			
Ferman Pinto	0.027	0.17	<0.001
Permutation Test	0.029	0.46	0.057
Panel D: Triple Difference			
Coefficient	-0.019	-0.042	-0.039
Cluster Robust	<0.001	0.001	<0.001
One-Tailed			
Ferman Pinto	0.087	0.061	0.028
Permutation Test	0.17	0.31	0.17
Two-Tailed			
Ferman Pinto	0.16	0.19	0.072
Permutation Test	0.26	0.54	0.26

Note: Each panel and column presents the coefficient from our preferred specification followed by p-values from 5 separate methods of correcting for clustered errors. The first is the standard Huber-White cluster robust adjustment. The following four p-values are obtained using one- and two-sided tests from the Ferman & Pinto (2019) inference method and a permutation test procedure.

Table A4: State Rankings by Coefficient Magnitude from Permutation Tests

Full			Naïve			Non-Naïve		
	b	p-value		b	p-value		b	p-value
SOUTH DAKOTA	-0.04	0.00	SOUTH DAKOTA	-0.03	0.00	WASHINGTON	-0.04	0.00
WASHINGTON	-0.02	0.00	WYOMING	-0.03	0.00	KENTUCKY	-0.03	0.00
KENTUCKY	-0.02	0.00	ALASKA	-0.03	0.02	DELAWARE	-0.03	0.00
DELAWARE	-0.02	0.02	GEORGIA	-0.02	0.00	HAWAII	-0.03	0.00
GEORGIA	-0.02	0.00	DELAWARE	-0.02	0.07	OREGON	-0.03	0.00
WYOMING	-0.02	0.02	KENTUCKY	-0.02	0.00	GEORGIA	-0.02	0.00
IDAHO	-0.01	0.00	WASHINGTON	-0.01	0.00	DC	-0.01	0.01
HAWAII	-0.01	0.14	NEBRASKA	-0.01	0.01	IDAHO	-0.01	0.00
NEBRASKA	-0.01	0.01	TEXAS	-0.01	0.08	ARKANSAS	-0.01	0.00
ARKANSAS	-0.01	0.00	NORTH CAROLINA	-0.01	0.20	MONTANA	-0.01	0.16
ALASKA	-0.01	0.45	ARKANSAS	0.00	0.18	NEBRASKA	-0.01	0.05
OREGON	-0.01	0.05	IOWA	0.00	0.41	MINNESOTA	-0.01	0.09
NORTH CAROLINA	0.00	0.38	INDIANA	0.00	0.51	SOUTH DAKOTA	-0.01	0.41
MINNESOTA	0.00	0.38	MISSOURI	0.00	0.49	KANSAS	0.00	0.21
MISSOURI	0.00	0.21	CALIFORNIA	0.00	0.60	MISSISSIPPI	0.00	0.25
TEXAS	0.00	0.59	MINNESOTA	0.00	0.84	ARIZONA	0.00	0.34
MONTANA	0.00	0.86	COLORADO	0.00	0.65	NORTH CAROLINA	0.00	0.53
IOWA	0.00	0.86	SOUTH CAROLINA	0.00	0.10	MICHIGAN	0.00	0.73
CALIFORNIA	0.00	0.95	MONTANA	0.00	0.50	WISCONSIN	0.00	0.64
COLORADO	0.00	0.85	MARYLAND	0.00	0.13	IOWA	0.00	0.81
ARIZONA	0.00	0.80	ARIZONA	0.01	0.21	COLORADO	0.00	0.99
SOUTH CAROLINA	0.00	0.23	KANSAS	0.01	0.16	MISSOURI	0.00	1.00
KANSAS	0.00	0.38	WISCONSIN	0.01	0.02	SOUTH CAROLINA	0.00	0.92
WISCONSIN	0.00	0.24	FLORIDA	0.01	0.00	TEXAS	0.00	0.62
INDIANA	0.00	0.31	IDAHO	0.01	0.00	CALIFORNIA	0.00	0.52
MARYLAND	0.01	0.08	UTAH	0.01	0.00	WYOMING	0.01	0.37
DC	0.01	0.19	ILLINOIS	0.01	0.00	MARYLAND	0.01	0.14
MISSISSIPPI	0.01	0.00	HAWAII	0.01	0.04	UTAH	0.01	0.01
MICHIGAN	0.01	0.10	OREGON	0.01	0.00	INDIANA	0.01	0.01
UTAH	0.01	0.00	MAINE	0.01	0.00	ALABAMA	0.01	0.01
FLORIDA	0.01	0.00	MICHIGAN	0.01	0.01	ALASKA	0.02	0.05
ILLINOIS	0.01	0.00	MISSISSIPPI	0.01	0.00	ILLINOIS	0.02	0.00
MAINE	0.02	0.00	NORTH DAKOTA	0.02	0.00	FLORIDA	0.02	0.00
ALABAMA	0.02	0.00	DC	0.02	0.00	NORTH DAKOTA	0.02	0.00
NORTH DAKOTA	0.02	0.00	ALABAMA	0.03	0.00	MAINE	0.03	0.00

Table A5: Alternate Trend Specification: Rate of Opioid Prescription

	All	Appropriate	Inappropriate	Unclassified
Panel A: All				
Post x Kentucky	-0.030***	-0.017**	-0.069***	-0.027***
	(0.0020)	(0.0047)	(0.0037)	(0.0018)
Dep. Var. Mean	0.26	0.64	0.38	0.21
Panel B: Naive				
Post x Kentucky	-0.022***	-0.0043	-0.052***	-0.021***
	(0.0018)	(0.0068)	(0.0032)	(0.0015)
Dep. Var. Mean	0.22	0.65	0.34	0.17
Panel C: Non-Naive				
Post x Kentucky	-0.040***	-0.044***	-0.090***	-0.032***
	(0.0027)	(0.0075)	(0.0052)	(0.0023)
Dep. Var. Mean	0.33	0.63	0.45	0.29
N	1540	1532	1540	1540
Panel D: Triple Difference				
Post x KY x Non-Naive	-0.018***	-0.040**	-0.039***	-0.011***
	(0.0020)	(0.011)	(0.0039)	(0.0018)
Dep. Var. Mean	0.26	0.64	0.38	0.21
N	3080	3072	3080	3080

Note: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are clustered at the state level. Each column shows coefficient on the difference-in-differences term (post x KY) and the interaction with a linear trend (post x KY x trend). All specifications include state and year fixed effects, KY specific linear trend (KY x trend) and the full set of demographic and policy controls. Outcome is the share of patients receiving an opioid following an ED visit.

Table A6: PDMP Effects using All States as Controls

	All	Appropriate	Inappropriate	Unclassified
Panel A: All				
Post x Kentucky	-0.030***	-0.044***	-0.069***	-0.024***
	(0.0016)	(0.0049)	(0.0027)	(0.0015)
Dep. Var. Mean	0.25	0.64	0.38	0.21
Panel B: Naive				
Post x Kentucky	-0.027***	-0.032***	-0.050***	-0.024***
	(0.0014)	(0.0059)	(0.0025)	(0.0013)
Dep. Var. Mean	0.22	0.64	0.34	0.17
Panel C: Non-Naive				
Post x Kentucky	-0.035***	-0.061***	-0.092***	-0.025***
	(0.0024)	(0.0061)	(0.0047)	(0.0024)
Dep. Var. Mean	0.33	0.63	0.45	0.28
N	1540	1532	1540	1540
Panel D: Triple Difference				
Post x KY x Non-Naive	-0.0080**	-0.028***	-0.042***	-0.00072
	(0.0024)	(0.0077)	(0.0053)	(0.0023)
Dep. Var. Mean	0.25	0.64	0.38	0.21
N	3080	3072	3080	3080

Note: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are clustered at the state level. Each estimate shows the coefficient on the difference-in-differences term (post x KY) from a separate regression. All specifications include state and year fixed effects, KY specific linear trend, and full set of controls. Outcome is the share of patients receiving an opioid following an ED visit. Each panel and column represent a different sample. Col (1) shows estimates from the full sample of diagnosed conditions. Col (2) contains ED visits with diagnosis codes for opioid appropriate conditions, Col (3) contains visits for opioid inappropriate conditions, Col (4) contains visits that are unclassified (neither appropriate nor inappropriate).

Table A7: PDMP Mandate Effects on Opioid Prescriptions: Nine-Month Lookback

	All	Appropriate	Inappropriate	Unclassified
Panel A: All				
Post x Kentucky	-0.024***	-0.0074	-0.064***	-0.021***
	(0.0026)	(0.0057)	(0.0044)	(0.0023)
Dep. Var. Mean	0.26	0.64	0.38	0.21
Panel B: Naive				
Post x Kentucky	-0.018***	-0.0050	-0.054***	-0.016***
	(0.0022)	(0.0087)	(0.0038)	(0.0018)
Dep. Var. Mean	0.22	0.65	0.34	0.17
Panel C: Non-Naive				
Post x Kentucky	-0.032***	-0.015*	-0.076***	-0.027***
	(0.0033)	(0.0073)	(0.0058)	(0.0029)
Dep. Var. Mean	0.32	0.64	0.44	0.27
N	1540	1533	1540	1540
Panel D: Triple Difference				
Post x KY x Non-Naive	-0.013***	-0.011	-0.023***	-0.010***
	(0.0023)	(0.012)	(0.0046)	(0.0019)
Dep. Var. Mean	0.26	0.64	0.38	0.21
N	3080	3073	3080	3080

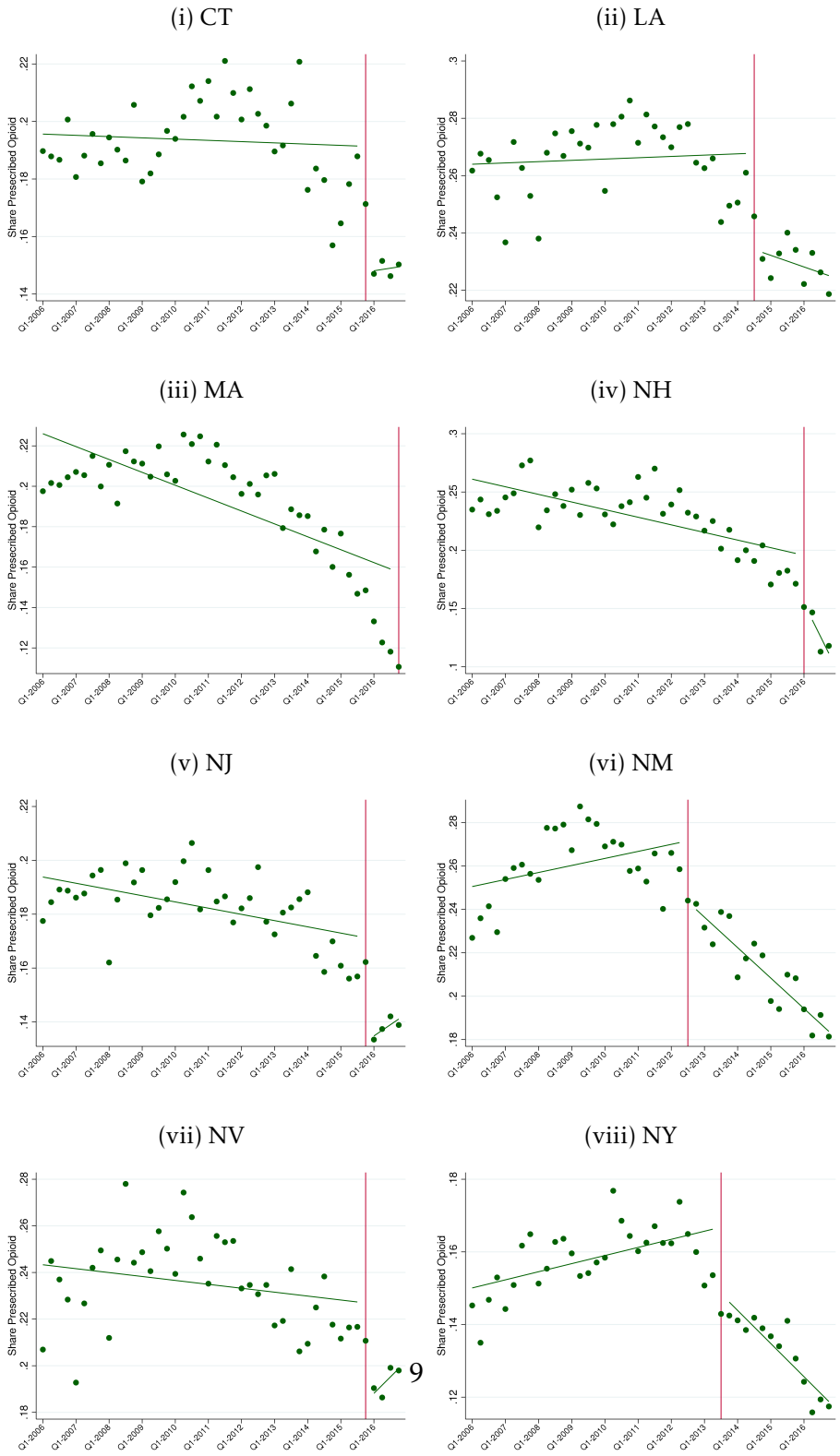
Note: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are clustered at the state level. Each estimate shows the coefficient on the difference-in-differences term (post x KY) from a separate regression. All specifications include state and year fixed effects, KY specific linear trend, and full set of controls. Outcome is the share of patients receiving an opioid following an ED visit. Each panel and column represent a different sample. Col (1) shows estimates from the full sample of diagnosed conditions. Col (2) contains ED visits with diagnosis codes for opioid appropriate conditions, Col (3) contains visits for opioid inappropriate conditions, Col (4) contains visits that are unclassified (neither appropriate nor inappropriate).

Table A8: PDMP Mandate Effects on Opioid Prescriptions: Excluding Patients with a Benzodiazepine Prescription

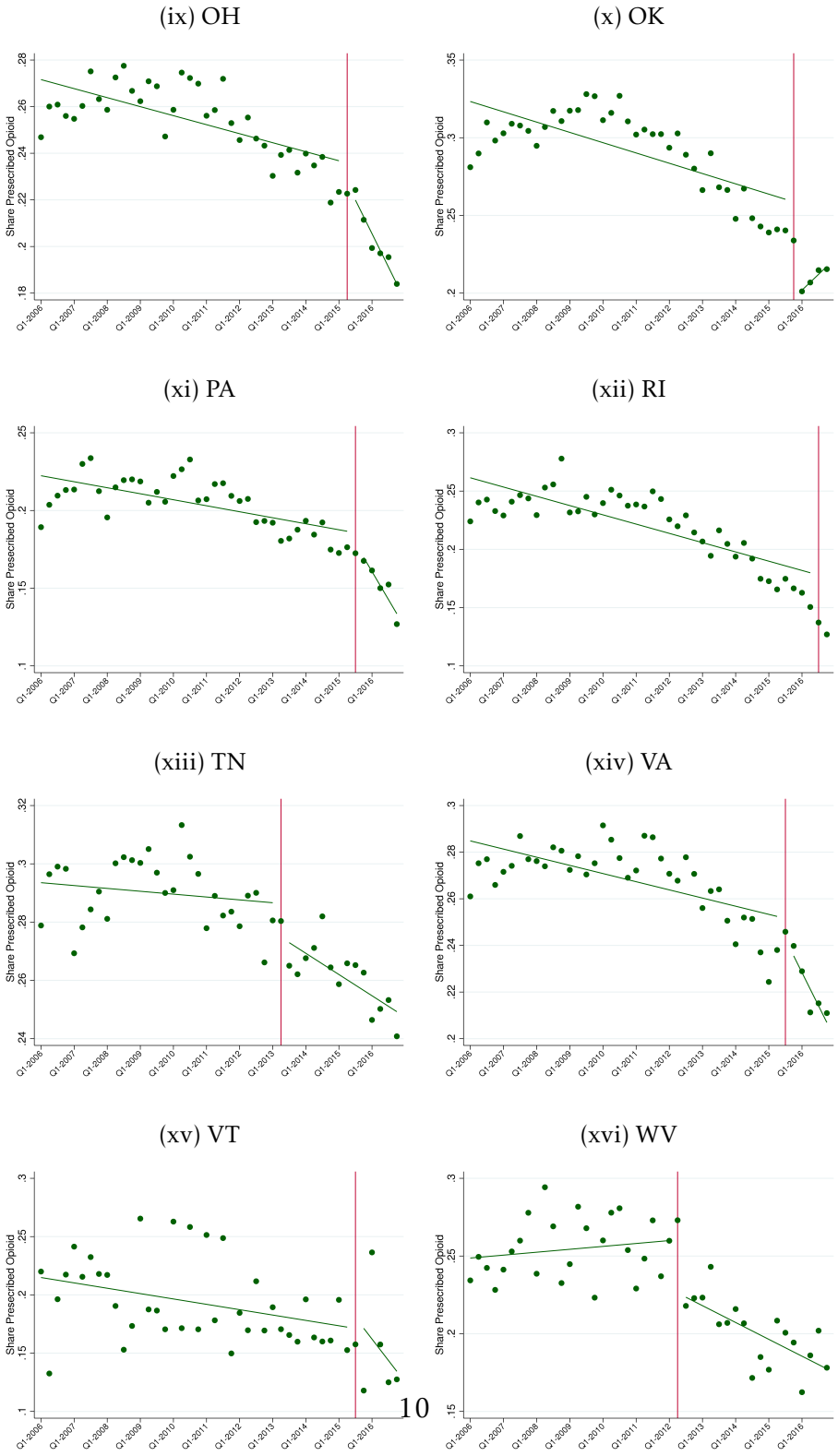
	All	Appropriate	Inappropriate	Unclassified
Panel A: All				
Post x Kentucky	-0.023***	0.00017	-0.058***	-0.021***
	(0.0025)	(0.0056)	(0.0043)	(0.0021)
Dep. Var. Mean	0.25	0.64	0.38	0.21
Panel B: Naive				
Post x Kentucky	-0.015***	0.012	-0.040***	-0.016***
	(0.0022)	(0.0078)	(0.0037)	(0.0018)
Dep. Var. Mean	0.22	0.65	0.34	0.17
Panel C: Non-Naive				
Post x Kentucky	-0.035***	-0.027**	-0.085***	-0.029***
	(0.0032)	(0.0084)	(0.0061)	(0.0030)
Dep. Var. Mean	0.32	0.63	0.45	0.28
N	1540	1532	1540	1540
Panel D: Triple Difference				
Post x KY x Non-Naive	-0.020***	-0.040**	-0.045***	-0.013***
	(0.0026)	(0.013)	(0.0048)	(0.0025)
Dep. Var. Mean	0.25	0.64	0.38	0.21
N	3080	3072	3080	3080

Note: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are clustered at the state level. Each estimate shows the coefficient on the difference-in-differences term (post x KY) from a separate regression. All specifications include state and year fixed effects, KY specific linear trend, and full set of controls. Outcome is the share of patients receiving an opioid following an ED visit. Each panel and column represent a different sample. Col (1) shows estimates from the full sample of diagnosed conditions. Col (2) contains ED visits with diagnosis codes for opioid appropriate conditions, Col (3) contains visits for opioid inappropriate conditions, Col (4) contains visits that are unclassified (neither appropriate nor inappropriate).

Figure A1: Rate of Opioid Prescriptions in All Other Mandate States

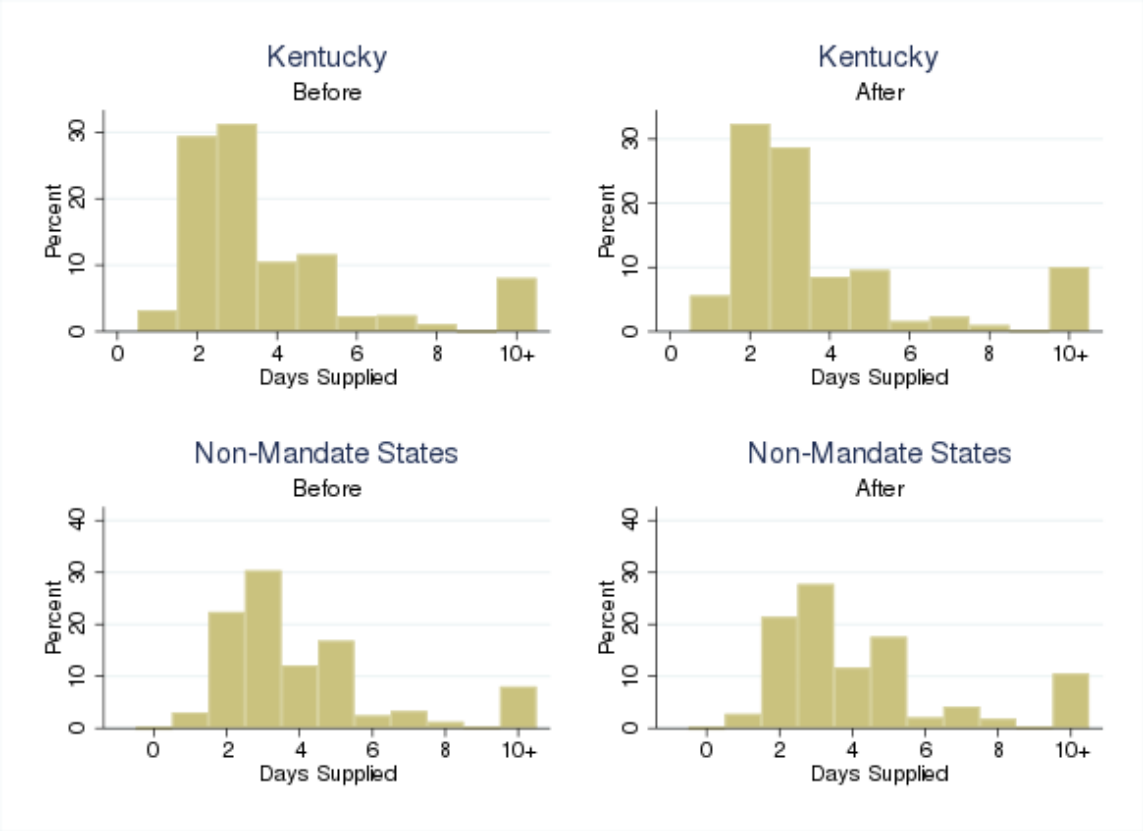


Note: Share of patients receiving an opioid following an ED visit using Optum data from 2006-2016 for all other mandate states. Vertical line represents introduction of mandate in each state.



Note: Share of patients receiving an opioid following an ED visit in KY vs. non-mandate states using Optum data from 2006-2016. Vertical line represents introduction of KY mandate in Q3 of 2012.

Figure A2: Histograms of Opioid Days Supplied



Note: Histogram shows distribution of days supplied for opioid prescriptions in Kentucky relative to non-mandate states before and after Q3 of 2012, the introduction of the Kentucky mandate.

Figure A3: Rate of Opioid Prescriptions in Kentucky and Non-Mandate States

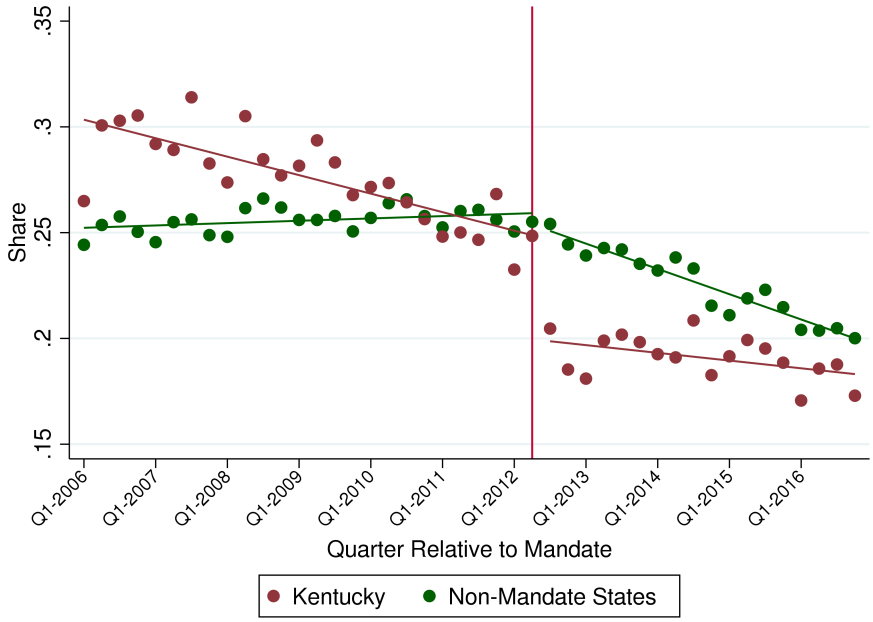


Figure A4: Rate of Opioid Prescriptions in Kentucky by Naive Status

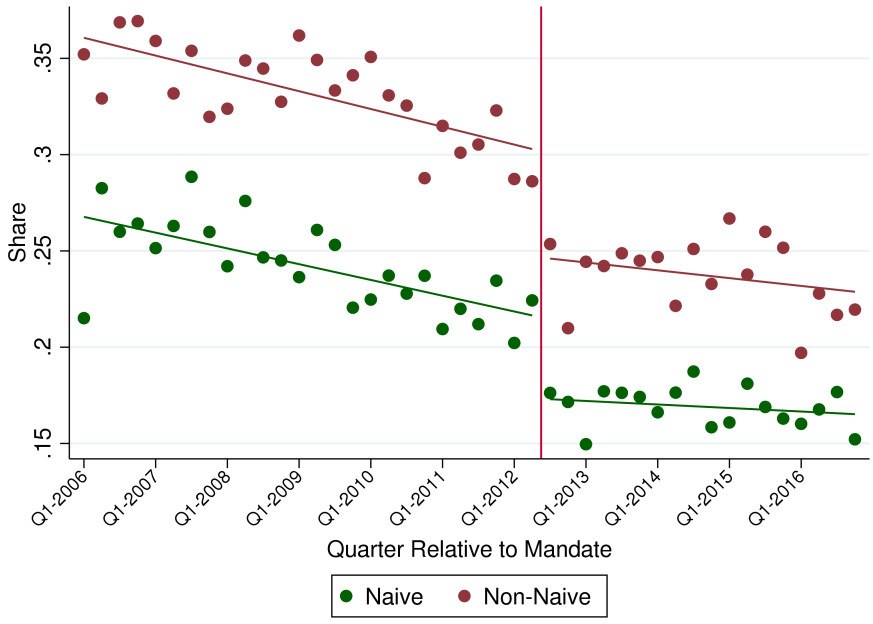


Figure A5: Event Study of Opioid Prescriptions Relative to KY Mandate. Confidence Intervals from 5th and 95th Percentile of Coefficients from Permutation Test

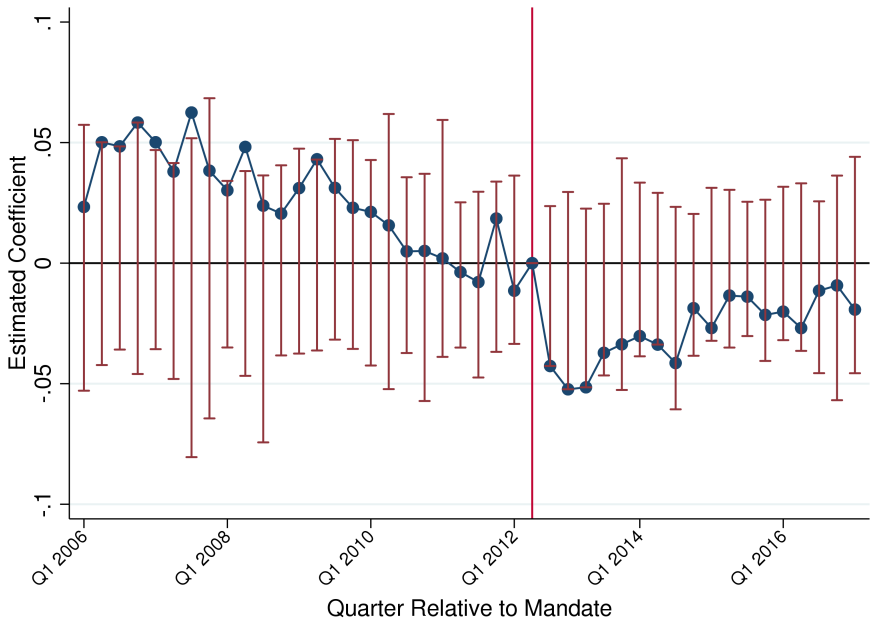


Figure A6: Event Study of Opioid Prescriptions Relative to KY Mandate by Naive Status. Confidence Intervals from 5th and 95th Percentile of Coefficients from Permutation Test

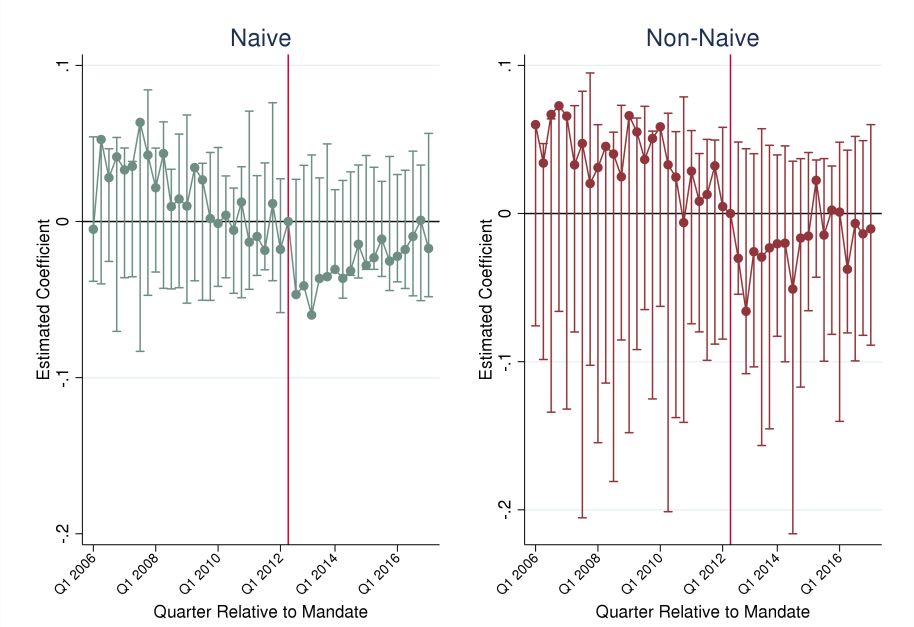
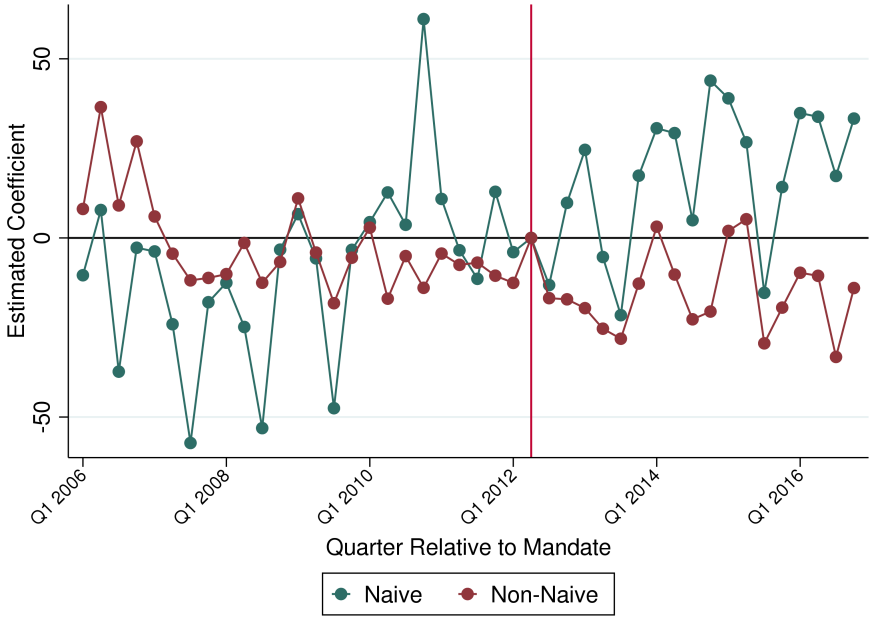


Figure A7: Event Study of ED Visits for Opioid-Appropriate and -Inappropriate Conditions

(i) Appropriate Conditions



(ii) Inappropriate Conditions