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

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Abstract

We study hassle costs versus information provision in explaining how prescription drug monitoring programs (PDMPs) decrease opioid prescribing. PDMPs aim to reduce opioid 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. Hassle costs reduce opioid prescribing across the board, including to opioid-naïve patients; however, physicians continue to prescribe opioids to patients who would benefit the most. Although information also affects prescribing, hassle costs explain the majority of the decline. Introducing a cost to prescribing high-risk medications improves the targeting of treatment.

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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 (Muhuri, Gfoerer, and Davies 2013; Compton, Jones, and Baldwin 2016; Compton and Wargo 2018), 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. This information may not otherwise be available since health IT systems are typically uncoordinated across providers, enabling problematic behaviors to go unchecked.

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; Jena et al. 2014; Kilby 2015; Meara et al. 2016; Moyo et al. 2017; Horwitz et al. 2018; Popovici et al.

[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).

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 other interventions and can shed light on the potential impact of efforts to further improve PDMP performance.[2]

In this paper, we study the importance of hassle costs in explaining how PDMP mandates reduce opioid prescribing and how they impact 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.[3] While information is the intended channel through which PDMPs reduce prescribing (SAMHSA 2017), PDMP mandates may also have an unintended effect on prescribing by introducing a hassle cost.[4] A PDMP mandate requires physicians to log into an electronic system to do a record check before writing an opioid prescription. The welfare effects of hassle costs are ambiguous. By raising the cost of

[2] See, for example, https://www.pewtrusts.org/~/media/assets/2016/12/prescription_drug_monitoring_programs.pdf.
[3] We use Optum's de-identified Clinformatics® Data Mart Database (2006-2021).
[4] In Kentucky, the focus of our study, the PDMP use requirement aimed "to assist prescribers in making appropriate treatment decisions, to identify patients potentially in need of substance abuse treatment interventions and to identify possible doctor shopping" (Freeman et al 2015, p. 3). Administrative hassles and their potential chilling effect on opioid prescribing were identified as unintended consequences and studied as part of the policy's impact evaluation (Freeman et al. 2015).

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.[5] On the other hand, mandates could improve decision-making if physicians are more 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 initial adoption of PDMP mandates (Perrone, DeRoos, and Nelson 2012; Deyo et al. 2013; Rutkow et al. 2015) but have been largely ignored in the economics literature.[6]

Our work also relates to a growing literature on the impact of administrative hassles in U.S. health care. Just as PDMP search may be a barrier to opioid prescribing, prior work shows that the administrative burden created by prior authorization requirements raises the costs of prescribing (Ketcham and Epstein 2008) and reduces the likelihood of prescribing both inappropriate and appropriate drugs (Epstein and Ketcham 2014; Dillender 2018). The administrative burden of medical billing and revenue collection also reduces the likelihood that providers accept Medicaid patients (Dunn et al. 2021). More broadly, administrative hassle is an important barrier to both public (Deshpande and Li 2019) and private benefit receipt (e.g., see Madrian and Shea 2001) as well as optimal benefit choices (Abaluck and Gruber 2011; Handel

[5] 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.
[6] Two recent exceptions are Buchmueller, Carey, and Meille (2020) and Sacks et al. (2021) discussed below, although neither study attempts to quantify the relative importance of information and hassle costs.

and Kolstad 2015). Reducing hassle costs can meaningfully increase take-up of social programs (e.g., see Bhargava and Manoli 2015; Finkelstein and Notowidigdo 2019; Shepard and Wagner 2021). Together these prior studies suggest that, although the intent of a PDMP mandate is to improve informed decision-making, the hassle cost of search may have a sizeable influence on opioid prescribing decisions.

Our analysis focuses on prescribing in the emergency department (ED). In our sample, more than one-quarter of adult patients receive an opioid following an ED visit. The ED setting is well suited for understanding the impact of the PDMP on provider behavior for several reasons. First, unlike a pre-scheduled primary care appointment, patients are generally unable to select providers that they think are amenable to opioid prescribing in the ED since provider assignment is handled through an administrative triage process. Thus, we are able to isolate provider behavior from patient selection. Second, the impact of both information and hassle costs from PDMPs are potentially large in these settings. Providers in the ED setting generally have limited ex-ante information about their patients given that repeat visits are uncommon. Likewise, outside of integrated health systems, providers in the ED generally do not have access to patient prescription histories or records more generally and have little scope to request a transfer of records prior to a visit. Thus, the PDMP provides new information which could have a large impact on their clinical decisions. At the same time, doctors in the ED setting are highly time-constrained and may face significant hassle costs from complying with the PDMP mandate. These features make the ED setting advantageous for studying the impact of information versus hassle costs, although we note that our results may not fully generalize to other care settings.

Our analysis uses 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

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 are 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 assess whether 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 larger for patients who have filled an opioid prescription in the last six months (i.e., the opioid non-naïve) than the opioid naïve and, among the non-naïve, largest for patients who have problematic histories that include "doctor shopping" or high daily doses or quantities of opioids. That prescribing also declines for the opioid naïve population, however, 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.

Based on a simple framework of provider prescribing decision-making, we combine estimates from these tests in a triple-differences framework to quantify the relative contribution of hassle costs and information. The key assumption is that the marginal impact of hassle costs is the same for naïve and non-naïve patients within narrowly defined clinical diagnosis categories. Using this framework, we show 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%.[7] 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. These findings suggest that the mandate weakly improves long-term patient outcomes. An important caveat, however, is that we do not have any direct measures of pain and so cannot assess the impact of the mandate on pain management.

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. Opioid prescribing remains unchanged for

[7] To the extent that increased monitoring salience also reduces prescribing, it reinforces the hassle cost effects.

opioid naïve patients whose conditions are appropriate for opioids but declines for patients whose diagnoses would be inappropriate for opioids. In short, hassle costs serve as a screening mechanism that, although a blunt tool, can improve prescribing in the absence of better designed policies to target opioid-appropriate individuals. In the opioid prescription context, where only a small share of patients may benefit from these medications and some prescribers do not follow guidelines, hassle costs can be welfare enhancing. 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 reductions in inappropriate prescribing and other features are not added to improve guideline adherence. 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.[8]

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

I. Background on Prescription Drug Monitoring Programs

A. 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

[8] There is relatively little evidence about the impact of PDMP integration, although Wang (2021) finds that states that have PDMP integration policies experience reductions in opioid-related mortality.

in nearly every state, their design and accessibility vary widely.[9] 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 effects (Paulozzi, Kilbourne, and Desai 2011; McDonald, Carlson, and Izrael 2012; Reifler et al. 2012; Brady et al. 2014; Jena et al. 2014; Li et al. 2014; Kennedy-Hendricks et al. 2016; Meara et al. 2016; Horwitz et al. 2018). However, some studies find that PDMPs reduce opioid prescribing (Reisman et al. 2009; Bao et al. 2016; Moyo et al. 2017), 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 (Haegerich et al. 2014; Davis 2017; Fink et al. 2018; 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 increases PDMP queries (Buchmueller, Carey, and Meille 2020; Carey, Meille, and Buchmueller 2021), reduces opioid prescribing (Dowell et al. 2016; Wen et al. 2016; Bao et al. 2018; Haffajee et al. 2018; Meinhofer 2018) as well as indicators of opioid misuse (Ali et al. 2017; Buchmueller and Carey 2018; Buchmueller, Carey, and Meille 2020) and overdose deaths (Dowell et al. 2016; Patrick et al. 2016; Pardo 2017; Meinhofer 2018). 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%

[9] 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.

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 (Perrone, DeRoos, and Nelson 2012; Deyo et al. 2013; Rutkow et al. 2015; Blum, Nelson, and Hoffman 2016). Paper-based "triplicate prescription programs,"[10] which preceded electronic PDMPs and required considerable paperwork for providers, were highly effective at reducing opioid use (Alpert et al. 2022). In contrast, the earliest electronic PDMPs, which were designed to minimize hassle costs by passively collecting prescription data from pharmacies (Simoni-Wastila and Tompkins 2001; Fishman et al. 2004), 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– Buchmueller, Carey, and Meille (2020) and Sacks et al. (2021)– discuss this mechanism. Both papers find suggestive evidence of hassle costs but neither decomposes the relative effects of hassle and information nor is their data and/or sample geared towards isolating these effects. Sacks et al. (2021) use commercial claims data to study the effects of both state PDMP mandates and laws limiting the length of initial opioid prescriptions for the sample of new opioid users. They find reductions in the likelihood of opioid use among new patients following PDMP

[10] 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. (2022) discuss focus group evidence that doctors considered the hassle costs of these programs to be large.

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 mechanisms. In this work, we introduce the comparison of opioid naïve with opioid non-naïve to tease out and quantify the effects of information from hassle costs.

Buchmueller, Carey, and Meille (2020) 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 patient histories for non-opioid users, however, they are unable to isolate the relative effects of information and hassle costs on opioid receipt. In contrast, we use claims data to consider prescription histories for all those eligible for an opioid prescription. As a result, we can decompose changes in the opioid prescribing rate on the extensive margin. Furthermore, we restrict our analysis to emergency department (ED) visits. Because patients cannot choose their provider in the ED, patient selection into an opioid prescription is mitigated in this setting relative to prior studies. This is important for decomposing the supply-side response to the mandate from any demand-side responses. Finally, we introduce a classification of appropriate and inappropriate prescribing to assess the impact of hassle costs on the targeting of opioid treatment. Thus, our contribution, relative to prior work, is to make explicit the role of both hassle costs and information and to empirically isolate their contributions to the PDMP-induced change in opioid prescribing in Kentucky.[11]

B. Kentucky's PDMP

[11] Another recent paper, Ukert and Polsky (2021), builds on our framework to analyze the role of information versus hassle costs on prescribing and longer-term outcomes in states with strict PDMP requirements.

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 one of the largest declines 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 typically much smaller (see Appendix Figure A1).[12] Since Kentucky had one of the earliest mandates, this also allows for longer follow-up.[13] 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, about 28% of DEA-licensed prescribers were registered and could make queries to KASPER and this proportion remained stable over time (see Figure 2 in Freeman et al. 2015).

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.[14] The mandate led to an immediate

[12] 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.
[13] 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.
[14] 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.

jump in the number of prescribers who were registered with the PDMP and the number of queries made. Within just a few months of implementing the mandate, close to 95% of DEA-licensed prescribers registered with the system, up from about 33% in the month prior to the mandate (see Figure 2 in Freeman et al. 2015). Prescriber queries increased dramatically, from under 100,000 in the month just prior to the mandate to over 350,000 in the month just after (see Figure 3 in Freeman et al. 2015). The share of opioid prescriptions that involved a query increased nearly five-fold, including among providers with a surgical or emergency medicine specialty (Carey, Meille, and Buchmueller 2021). 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).[15] 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.[16] 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.[17] These strong features of the mandate likely contributed to Kentucky's success.

C. Conceptual Framework for Potential Mechanisms

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

[16] 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

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

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.

To understand how the PDMP mandate affects prescribing, we outline a simple model of provider decision making before and after the mandate. Although there are a multitude of factors that can influence physician prescribing, such as education and training (e.g., Schnell and Currie 2018), intrinsic motivation (e.g., Kolstad 2013), or insurance benefit design and choice architecture (e.g., Epstein and Ketcham 2014), we focus on how PDMP's affect prescribing through both hassle costs and information. Other factors impacting prescribing are not expected to change systematically across states before and after the mandate.[18]

In this framework, we focus on the majority of providers who were not voluntarily searching the PDMP pre-mandate since the mandate is only binding for this group. Providers who incurred the cost to search the PDMP pre-mandate for a given patient type should also incur the cost to search post-mandate and their prescribing should be unchanged. Prior to the mandate, providers choosing not to use the PDMP prescribe opioids if the marginal benefit of an opioid prescription (weakly) exceeds the marginal cost of writing the prescription. The marginal benefit can be expressed as $v_i = g(a_i - n_i)$ which is a function of the patient's appropriateness for opioids based on their diagnosis, a_i, net of the potential adverse consequences of misuse, n_i.[19] The marginal cost is c_i. For concreteness, Panel A of Appendix Figure B1 shows an illustrative

[18] More recently PDMPs were integrated into electronic health records and could, in principle, be integrated in ways that affect choice architecture as in Epstein and Ketcham (2014). This was not the case during our study period.
[19] The appropriateness of opioids will be determined by both the diagnosis and severity of the condition.

probability distribution of pre-mandate benefits v_i, $f_v(v_i)$, and the pre-mandate costs of prescribing, c_i. Without a mandate, providers prescribe opioids where $v_i \geq c_i$.

After the mandate, providers are required to search the PDMP prior to prescribing which introduces a hassle cost (e.g., the time cost of logging into the PDMP) that raises the marginal cost of prescribing to c_i'. This reduces the share of patients prescribed opioids by $\int_{c_i}^{c_i'} f_v(u)du$ (denoted by the area labeled A in Panels B and C of Figure B1). In addition to increasing hassle costs, the mandate, by requiring that prescribers search the PDMP, can also provide new information about a patient's opioid history. To the extent search provides new information, providers can update their beliefs about the net benefits of treatment and thus the distribution of v_i can shift. Whether the PDMP mandate provides new information that affects prescribing will depend on the patient's revealed opioid prescription history— i.e., whether they are opioid naïve or non-naïve.[20] We show in detail how information gained from the PDMP mandate impacts prescribing in Appendix B.1 and briefly summarize these effects in the section below.

For opioid-naïve patients not previously subject to search, the mandate provides no new information that affects prescribing. As shown in Panel B of Figure B1, only patient types with ex-ante values of v_i above the new search threshold, c_i', will be subject to PDMP search. For these opioid naïve patients, search only confirms the provider's initial beliefs that the value of an opioid for the patient exceeds the cost of prescribing and information does not affect prescribing.[21] For opioid naïve patients with ex-ante values of v_i below the new search threshold,

[20] For simplicity, we categorize opioid history as naïve or non-naïve in our framework, but we recognize that within the non-naïve category there are patterns of prescription fills that would be classified as more or less problematic. It would not change the basic predictions from our model to take these more nuanced classifications into account.

[21] In practice, even though prescribing is unchanged, patients with v_i above c_i' may have their information updated in some cases. Specifically, if the provider would have misclassified a naïve patient as non-naïve prior to the mandate but still would prescribe to that patient because the provider believed the net benefit for that patient was very high based on her diagnosis then the provider may update v_i post-mandate upon learning her actual naïve status. While the distribution of v_i could change in this case such that there is more mass at higher values of v_i, no

c_i', the provider does not search because she believes that relative to the cost of prescribing, including the hassle cost of search, these patients are either too high risk for misuse (based on observable characteristics) or inappropriate for opioids based on their diagnosis. Although some of these patients will be misclassified as non-naïve, the provider will not learn their true naïve status post-mandate and the provider's initial beliefs about the distribution of net benefits and prescribing will be unchanged. For these reasons, Panel B of Figure B1 shows the distribution of v_i for naïve patients as unchanged after the mandate. Thus, the only impact of the mandate on prescribing to naïve patients will be a reduction in the share of patients receiving opioids due to the added hassle cost of search (by $\int_{c_i}^{c_i'} f_v(u)du$, or area A in Panel B of Figure B1).

On the other hand, for non-naïve patients, prescribing changes will occur both due to the increase in hassle costs (i.e., increase in the marginal cost of prescribing from c_i to c_i') and also because of new information from search (i.e., shift in distribution of v_i to the left). Specifically, when the PDMP search reveals that a patient with ex-ante $v_i \geq c_i'$ is non-naïve, the doctor will receive (weakly) negative information. She will update her beliefs about the potential adverse consequences of an opioid prescription for the patient and (weakly) increase n_i, thereby decreasing v_i. This update will mean that some patients will now be below the threshold, c_i', for receiving an opioid. How this new information affects the distribution of net benefits depends on specific assumptions about how n_i gets updated. In Panel C of Figure B1, we show the case where the higher n_i shifts the distribution of net benefits downwards by a constant amount to $\widetilde{f_v}(v_i)$ (denoted by the dashed line).[22] Irrespective of the specific change in the distribution, the

individuals who were above the threshold for prescribing will be shifted to below the threshold and vice versa. Thus, the *area* under the distribution (above c_i') will remain unchanged pre- and post-mandate. For simplicity we represent this as no change in the distribution since it has no impact on prescribing.

[22] There is a discontinuity in the post-mandate distribution of benefits $\widetilde{f_v}(v_i)$ (denoted by the dashed line) at c_i'. This occurs because doctors check the PDMP and obtain information for patients with ex-ante benefits $v_i \geq c_i'$ and

mandate will reduce the share of non-naïve patients receiving opioids by both the hassle cost, $\int_{c_i}^{c_i'} f_v(u)du$ (denoted as area A) and by the change in the distribution of net benefits for patients whose benefits exceed the hassle cost c_i' (where providers will search the PDMP and obtain information), $\int_{c_i'}^{\bar{v}_i} f_v(u)du$-$\int_{c_i'}^{\bar{v}_i'} \tilde{f}_v(u)du$ (denoted as area B). This last term is the reduction in prescribing due to new information.

Based on this framework, we predict that prescribing (weakly) decreases to all patient types as a result of the mandate, even for opioid naïve patients for whom there is no information gained from the PDMP that will affect prescribing. While the reduction in prescribing due to information occurs only for non-naïve patients, the reduction due to hassle cost occurs for both naïve and non-naïve patients. Assuming naïve and non-naïve patients have the same ex-ante distribution of net benefits at the margin for search (i.e., between the pre and post mandate PDMP search thresholds, c_i and c_i'), then the reduction due to hassle costs is the same for the two groups. Since we condition on diagnosis categories in our empirical analysis, in practical terms this means we assume that the impact of hassle costs is the same for naïve and non-naïve patients with the same clinical diagnosis category.[23] We can then difference out the effect of the mandate on naïve patients (area A) from its effect on non-naïve patients (area A+B), conditioning on

update their beliefs about the distribution of benefits for only this subset of patients. In some cases, doctors will confirm their beliefs and prescribe where $v_i' \geq c_i'$. In other cases, doctors will negatively update their beliefs about patients, which will "move" the distribution of benefits to the left of c_i'. The distribution of "true" benefits to the left of c_i' is not fully known since doctors only check the PDMP for patients with ex-ante beliefs about benefits of opioids exceeding costs. While the distribution of benefits to the left of c_i'could take many forms, we depict this part of the distribution as a shift upwards (relative to the pre-mandate distribution) to illustrate the case where the higher n_i shifts the distribution of net benefits to the left by a constant amount.

[23] After conditioning on diagnosis category, any differences observable to the physician (but not in our data) that lead to ex-ante differences in net benefits between naïve and non-naïve patients (e.g., diagnosis severity or ex-ante signals of opioid misuse) are likely to be small near the margin for search. This is because both naïve and non-naïve patients above the threshold for prescribing who are affected by hassle costs (i.e., are between c_i and c_i') must have similar ex-ante signals of a low probability of misuse and/or high clinical benefit (or severity) in order to be considered for a prescription.

diagnosis category, to isolate the PDMP's information effect on prescribing behavior (area B = [A+B] – A). This model forms the conceptual basis of our triple difference empirical approach to isolating the independent effects of information and hassle costs (discussed in Section III).

Below, we separately calculate the impact of information versus hassle costs for patients who are clearly appropriate or clearly inappropriate for opioid treatment based on narrowly defined presenting diagnoses. Since the benefits of a prescription are higher for patients who are appropriate for opioids (e.g., the clinical benefit for fractures is considered greater than for lower back pain), we predict that hassle costs should cause larger proportional decreases in opioid prescribing to patients with conditions that are inappropriate for opioids.

II. 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.[24]

We create a dataset of all ED visits[25] and identify all opioid prescriptions within a window of 180 days prior through 3 days following an ED visit.[26] 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

[24] 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.
[25] ED visits are identified by place of service codes and revenue, procedure or type of service codes.
[26] 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.

the visit. We also identify whether they obtained any non-opioid analgesics.[27] 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-naïve," while those without any fills are "opioid naïve."[28] 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 outside of insurance coverage or without a valid prescription. However, cash prescriptions not billed to insurance constitute only about 8% of the Kentucky PDMP data (see Buchmueller, Carey, and Meille 2020; Carey, Meille, and Buchmueller 2021). Black market purchases made without a valid prescription would also not be observed by doctors using the PDMP. Thus, they would be unlikely to affect prescribing behaviors resulting from the mandate. As such, 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 or discouraged 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

[27] AHFS pharmacologic therapeutic classifications for nonsteroidal anti-inflammatory agents (28:08.04) and analgesics and antipyretics, miscellaneous (28:08.92) identify non-opioid analgesics.
[28] Although definitions vary in the literature, we follow a commonly used definition for opioid naïve of 6-months (e.g., Barnett, Olenski, and Jena 2017). Robustness checks using a 9-month lookback are qualitatively similar to our main results (shown below).

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.[29] 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.

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).

III. Empirical Approach

To understand the mechanisms through which PDMPs reduce opioid prescribing, we use both event-study and difference-in-differences approaches that compare prescribing in Kentucky versus non-mandate states before and after the PDMP mandate went into effect. We analyze

[29] 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.

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:

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

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.[30] 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:

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

where Y_{st}, α_s and γ_t are defined as above. The key coefficient of interest is δ, the interaction

[30] 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, Pacula, and Jacobson 2018 and Williams, Pacula, and Smart 2019). Pill mill and naloxone laws (see Abouk, Pacula, and Powell 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.

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 information provided in the PDMP alone should 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 narrowly defined appropriate versus inappropriate diagnosis categories and by naïve status serves as key tests for hassle cost effects, as discussed in Section I. 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 the naïve—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. For comparison, we also show p-values estimated from two-tailed tests and 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 these inference methods.

IV. Results

A. 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 II) 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.[31]

B. 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

[31] 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.

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.[32]

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 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,

[32] We show event study estimates including a Kentucky-specific linear trend in Appendix Figure A4. As expected, the de-trended event study removes the negative pre-trend but continues to show a sharp drop in prescriptions immediately after Kentucky's mandate. As further expected, the overall effect of the mandate is reduced slightly since this specification amplifies the bounce-back caused by the faster decline in opioid prescriptions in non-mandate states after 2013. These patterns are consistent with our difference-in-differences results below showing that the estimated effect of the mandate is smaller when linear trends are included. In a robustness test, we use a synthetic control approach that eliminates both the pre-trend and the bounce-back by using a weighted average of comparison states that is a close match to Kentucky. The synthetic control results are similar to our main results suggesting that they are not driven by pre-existing trends.

we find no clear change in dosage, as measured by log MME or other measures of dosage (see Appendix Table A2). Thus, the overall rate of prescribing and the number of days of medication supplied for prescriptions decline but dosages remain unchanged.

Our estimates should be interpreted in the context of how the PDMP mandate changed the behavior of prescribers. Freeman et al. (2015) report that the proportion of prescribers registered with the PDMP increased from 33% to 95% as a result of the mandate. Thus, we can recover the local average treatment effect (LATE) among compliers by scaling our estimates by the increase in PDMP compliance of 0.62. This represents the prescribing change resulting from not just additional querying (i.e., information) but also through hassle costs that create a hurdle to prescribing when physicians comply with the mandate.

C. 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.

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 A5). 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 or otherwise interacting with 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).[33] 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 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

[33] 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.

from the PDMP mandate. We parse out the relative effects of these channels below.

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).[34] 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 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.

[34] 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 (Schug and Goddard 2014; CDC 2016; 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).

Thus, the mandate does not prevent patients appropriate for opioids from getting necessary prescriptions; physicians appear willing to access the PDMP when the benefits of treatment are large enough to exceed the hassle costs. That prescribing declines for inappropriate conditions suggests that the additional hassle cost to accessing the PDMP provides a hurdle against prescribing for inappropriate conditions. We next turn to estimating how much of this decline is due to hassle versus information about patient opioid history.

Panels B and C of Table 4 stratify the sample not just by appropriateness but also by opioid naïve versus non-naïve status. 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 the importance of hassle cost effects. The same basic patterns are found for patients with conditions that cannot be classified as appropriate or inappropriate: prescribing declines for both the naïve and non-naïve patients with unclassified diagnoses. Since information contained in the PDMP should not affect prescribing for naïve patients, hassle costs must be at play in the decline for the naïve inappropriate and naïve unclassified patients. On the other hand, 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.

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 (as discussed in Section I) 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, conditioning on diagnosis category. 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.[35]

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

[35] 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.

discouraging opioid prescribing. This fits our framework 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 since the PDMP provides no actionable information for this group.

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 following the mandate, with the remaining 69% due to hassle costs.[36,37] Intuitively, information contributes to the decline only among non-naïve patients, roughly 54% of the effect. Information cannot account for the decline for opioid-naïve patients. 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.[38] 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

[36] 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*(0.035/0.023)*(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. n

[37] If instead we use a weighted average of the estimates in columns 2-4 that condition on diagnosis category, the estimates imply that information explains 49.6% of the effect (0.38*(0.06*(0.042/0.0035) + 0.14*(0.039/0.061) + 0.8*(0.013/0.021)), with the remaining 50.4% due to hassle costs. This is consistent with the estimate from the full sample, which shows that information is not the only or even primary channel through which the PDMP induced a reduction in prescribing.

[38] 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).

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.

D. 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 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.[39] 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

[39] 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.

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 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.[40]

Third, we test the sensitivity of our results to the empirical specification. In Appendix Figure A7 we use a synthetic control method (Abadie, Diamond, and Hainmueller 2010) to account for pre-existing differences in trends across Kentucky and non-mandate states. The synthetic control weights are shown in Appendix Table A5. Figure A7 shows that the rate of opioid prescribing in Kentucky and "synthetic Kentucky" track each other closely prior to

[40] 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 Figure A6 replicates the event study results comparing estimates for Kentucky with the 5th and 95th percentile of coefficients from the placebo distributions.

Kentucky's mandate but then diverge immediately after 2012. The differences persist until the end of our study period. In Appendix Table A6, we compute the average difference between the outcomes in the post-period. We find that the opioid prescription rate declines by 3.8 percentage points (or 15%) post-mandate in Kentucky relative to synthetic Kentucky, which is slightly larger than our main estimate. For inference, we conduct a permutation test, randomly assigning treatment to each control state. We report the ranking of the actual Kentucky estimate relative to the 34 placebo estimates. For the full sample and inappropriate diagnoses subsample, Kentucky ranks first or second in the distribution of estimates. The ranking is lower for patients appropriate for opioids, implying that, consistent with the main results, we cannot rule out a null effect for the appropriate subgroup.

In Appendix Table A7, we return to our main difference-in-differences results and 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 A8, 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 2021). For this reason, we focus on the cleaner control group of non-mandate states in our main results.

In Appendix Table A9, 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.[41]

Fourth, in Appendix Table A10 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.

Finally, we combine the full set of robustness tests for the difference-in-differences results (Appendix Tables A7-A10) simultaneously in Appendix Table A11. The patterns of results are similar to the main results in Table 4 and to each of the robustness tests on their own. Overall, we find that the results are not sensitive to these specific modeling decisions.

E. Intensive Margin Responses

While less informative about mechanisms than extensive margin changes, 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%.[42] Naïve patients experience a roughly

[41] 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.
[42] Dosages, however, are unaffected (see Table 2 and Appendix Table A2).

5% decline in days supplied. 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 not statistically significant. This change is driven by an increase in the 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. These patterns could also reflect a shift towards high dose prescribers. Data limitations make it difficult to measure severity within a diagnosis code or track prescribers (as opposed to patients) over time, precluding us from formally testing for compositional changes or drawing strong conclusions about which channels drive the intensive margin changes shown here.

F. 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.

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 an increase for both naïve and non-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 a 2.3 percentage point reduction in opioids. This increase is likely understated since many NOAs are available over-the-counter (OTC) and not observable in claims data. The increase in NOAs is of similar magnitude for both naïve and non-naïve patients overall. However, among the non-naïve, the increases seem to be driven largely by patients with inappropriate diagnoses. The decrease in NOA receipt among naïve patients with inappropriate diagnoses is surprising and suggests some caution in overinterpreting these results, particularly in light of common OTC use which we do not observe.

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.

V. 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.

Post-mandate, 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. Opioid prescriptions decline to both naïve and non-naïve patients who are 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. 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.

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.

These results are important for ongoing efforts to improve physician prescribing, including efforts to lower PDMP hassle costs. Our work suggests that efforts to reduce hassle costs could inadvertently undermine some of the positive effects of PMDPs on prescribing. Key to understanding this effect is that, despite dramatic reductions in opioid prescribing in the past decade, many clinicians do not follow appropriate prescribing guidelines. As a result, although eliminating hassle costs would lead more physicians to query and view the information in the PDMP, it could also partially undo the effects hassle costs have on deterring inappropriate use.

For opioid-naïve patients, inappropriate prescribing would increase since the PDMP provides no information that would discourage inappropriate prescribing absent hassle costs. For the non-naïve, inappropriate prescribing would likely remain unchanged or increase, since removing the hassle should be at least partially, if not fully, offset by the information gained from additional PDMP queries. Using our estimates, a back-of-the-envelope calculation indicates that a complete elimination of hassle costs could increase inappropriate prescribing by at least 2.8 percentage points or 7%.[43] Using estimates from Barnett, Olenski, and Jena (2017) on the effect of being prescribed an opioid in the ED on long-term opioid use, this would imply a 0.058 percentage point increase in long-term users or one more long-term user for every additional 1,724 patients receiving an opioid.[44] On the other hand, prescribing for diagnoses that are clearly appropriate for opioids would not change based on our estimates because we do not find that this was reduced by hassle costs.[45]

This estimate does not account for the possibility that states that streamline their PDMPs

[43] Using results from Table 4, we find that, for the opioid-naive, the PDMP mandate reduces inappropriate prescribing by 4.5 percentage points, which is entirely due to hassle costs. Eliminating hassle costs would then increase inappropriate prescribing for this group by 4.5 percentage points. For the non-naïve, we find a reduction of 8.3 percentage points, about half of which is due to hassle and the other half is due to information. For this group, removing hassle will be counteracted by the additional information gained from new queries, which will confirm the patient's non-naïve status. In our most conservative calculation, we assume that doctors would not write an opioid prescription if they learn that the patient is non-naïve. Thus, the increase in information perfectly counteracts the reduction in hassle costs and there is no change in prescribing for this group when hassle is reduced. This implies that overall inappropriate prescribing increases by about 4.5*.62+0*.38= 2.8 percentage points (given that the opioid-naïve is 62% of the sample), or a 7% increase relative to the baseline mean of 38 percentage points. This is a lower bound estimate given our strong assumption that doctors do not write prescriptions when they learn that the patient is non-naive. More realistically, some doctors will write prescriptions for the non-naïve who previously did not receive a prescription because of hassle and we will see a larger increase in prescribing.

[44] This calculation divides our 2.8 percentage point increase in prescribing by the estimate from Barnett, Olenski, and Jena (2017) that out of 48 patients prescribed a new opioid in the emergency department, 1 will become a long-term user. That number is based on Table 2 and described in more detail in the appendix. See https://www.nejm.org/doi/suppl/10.1056/NEJMsa1610524/suppl_file/nejmsa1610524_appendix.pdf

[45] Prescribing for diagnoses where the appropriateness of opioids is unclassified follows a similar logic as for inappropriate diagnoses. We would also expect that prescribing would be unchanged or would increase if hassle was eliminated.

to reduce hassle costs also adopt other interventions, such as clinical PDMP alerts,[46] to increase guideline concordant opioid prescribing. It also does not account for the gains to physicians from reducing the burdens from the PDMP. Recent estimates suggest annual time costs on the order of $1,664 (or 13.5 hours) for family medicine physicians to query PDMP patient reports (Bachhuber et al. 2018)—with higher costs likely for emergency medicine physicians. While a full welfare analysis is outside the scope of this paper, if the benefits of reduced inappropriate opioid prescribing exceed these time costs, then maintaining some cost to prescribing may be beneficial. To the extent that hassle costs can be lowered while enhancing PDMP features that promote guideline adherent prescribing, however, policymakers may facilitate additional improvements in the targeting of opioid prescriptions.

[46] PDMP alerts are clinical notifications sent to providers based on pre-specified parameters such as patient risk scores. State are increasingly adopting alerts: https://info.apprisshealth.com/en/whitepaper-risk-scoring-pdmp

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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

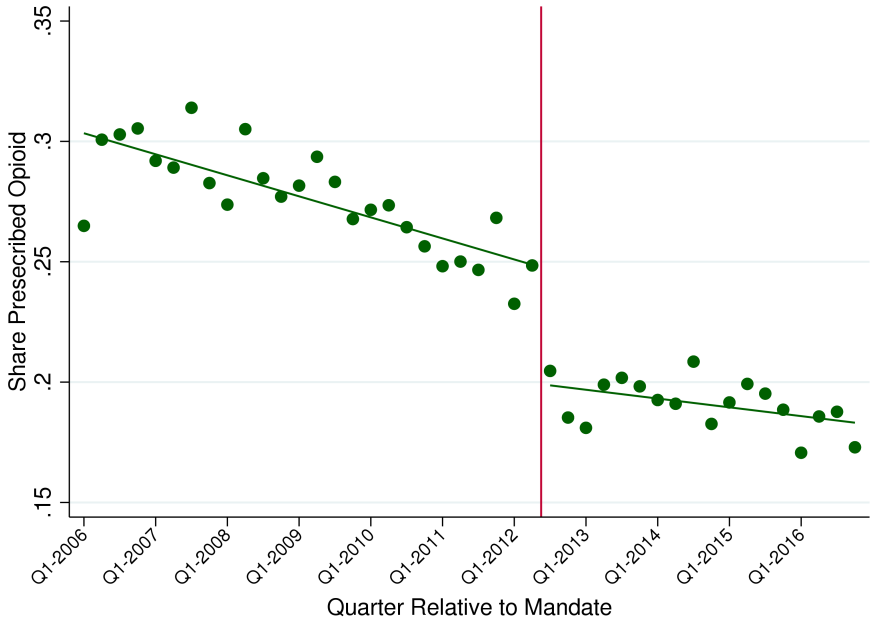
Note: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are clustered at the state level. Each estimate shows the coefficient on the difference-in-differences term (post x KY) from a separate regression. All specifications include state and year fixed effects, KY specific linear trend, and full set of 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.

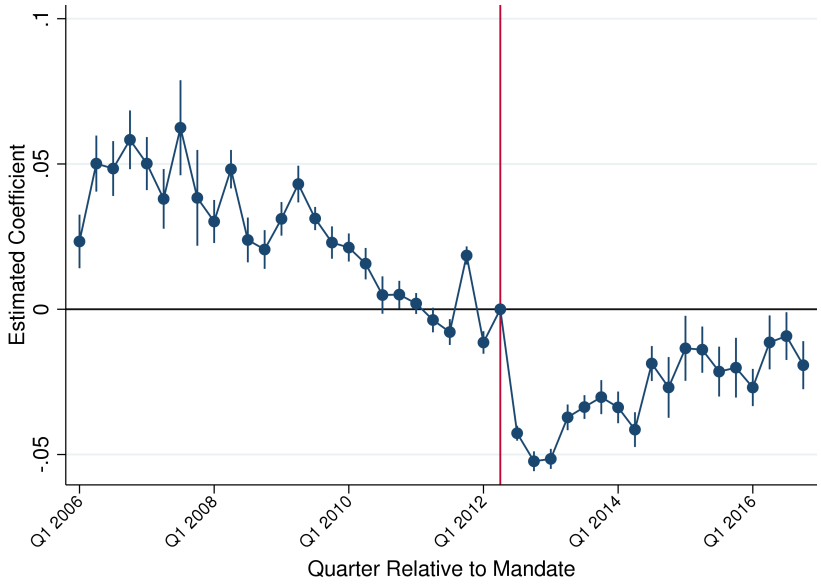
Figure 1: Rate of Opioid Prescriptions following ED Visits in Kentucky



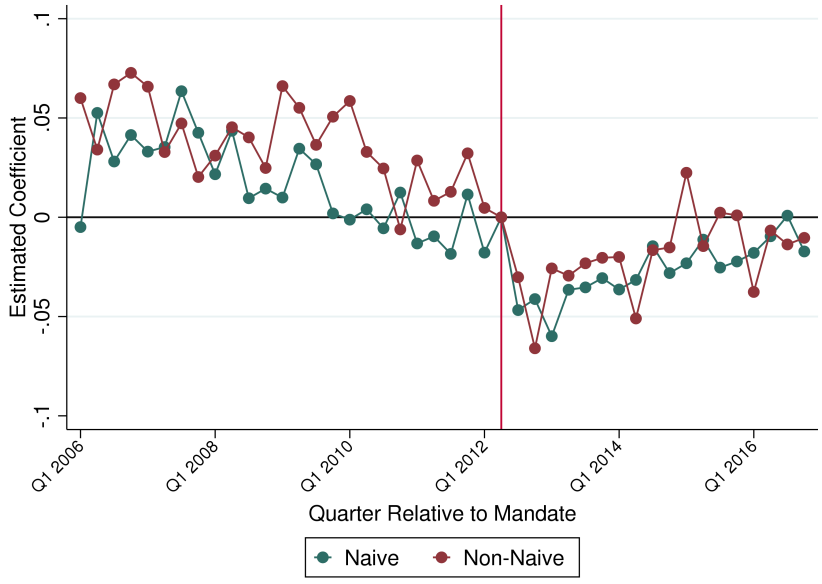
Note: Share of patients receiving an opioid following an ED visit in KY using Optum data from 2006-2016. Vertical line represents introduction of KY mandate in Q3 of 2012.

Figure 2: Event Study: Rate of Opioid Prescription

(a) Panel A: All



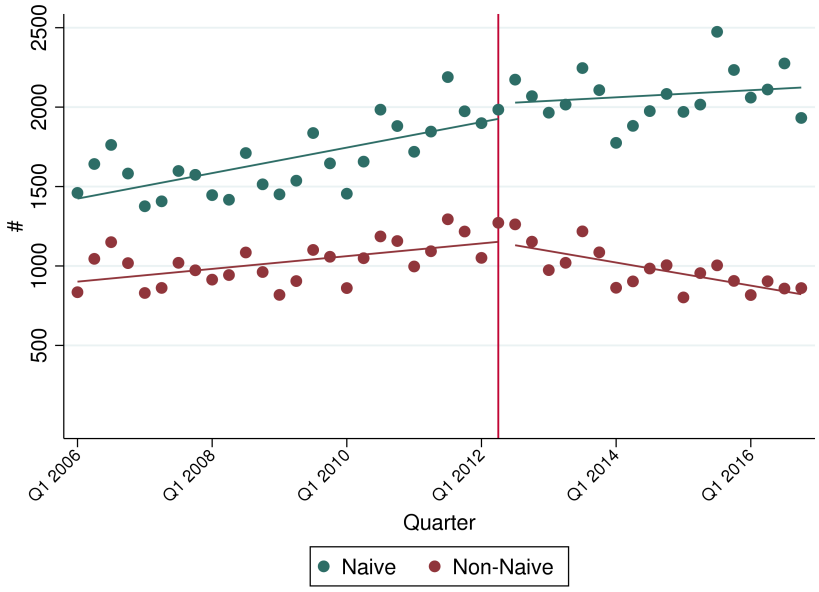
(b) Panel B: By Naive Status



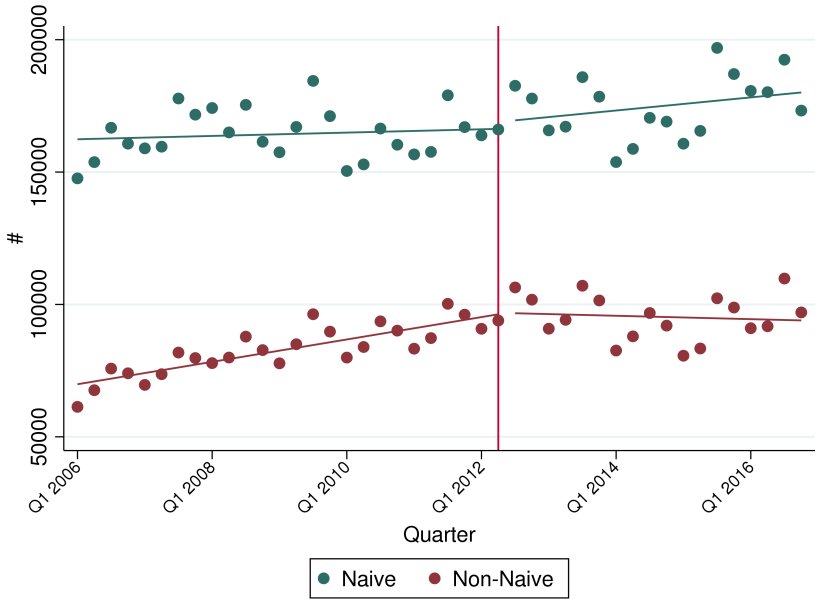
Note: Each graph includes point estimates from the event study (normalized to 0 in Q2:2012) and 95% confidence intervals which are adjusted for within-state clustering. Outcome is the share of patients receiving an opioid following an ED visit. Panel A shows the full sample, Panel B shows separate event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

Figure 3: Count of Emergency Department Visits

(a) Panel A: Kentucky



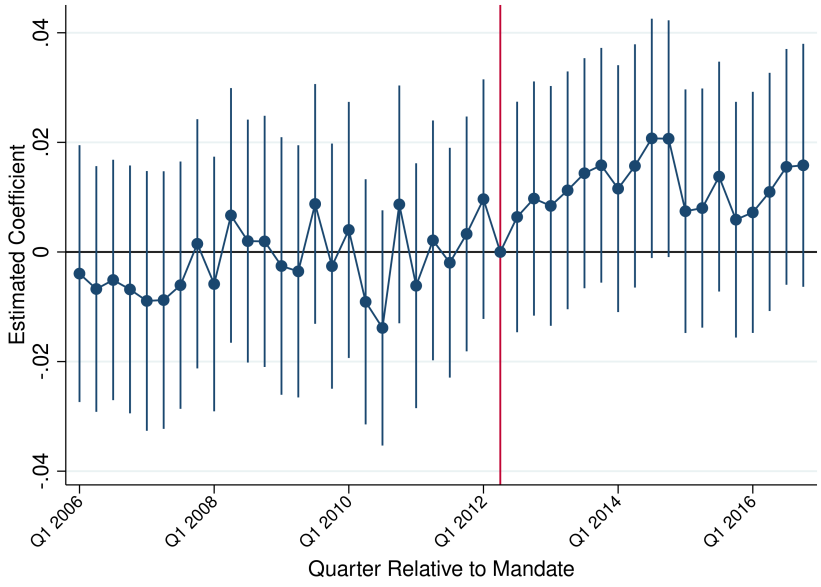
(b) Panel B: Non-Mandate States



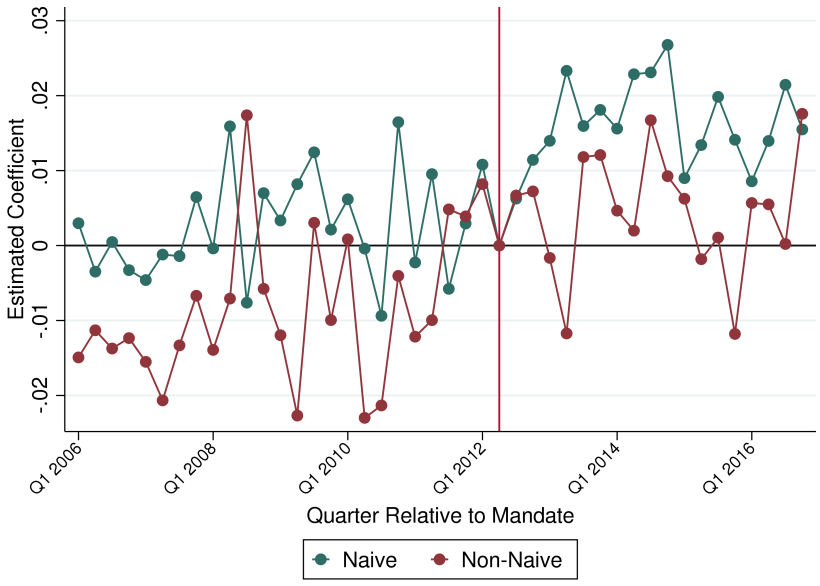
Note: Total number of ED visits in Optum data from 2006-2016. Each graph shows number of ED visits where patient is opioid naive and non-naive. Panel A shows ED visits in KY and Panel B shows ED visits in non-mandate states. Vertical line represents introduction of KY mandate in Q3 of 2012.

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.

Online Appendix

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

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November 2022

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 Mandate 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: Synthetic Control State Weights

State	Weight
WASHINGTON	.497
MARYLAND	.178
INDIANA	.174
FLORIDA	.093
ALABAMA	.035
MONTANA	.023

Note: Table displays weights for states with non-zero weights from the synthetic control model used to construct the synthetic control unit for the estimates in Appendix Table A5. Weights were calculated using the opioid prescription rate in control states during pre-mandate quarters. States allocated a zero weight include: AK, AZ, AR, CA, CO, DE, DC, GA, HI, ID, IL, IN, IA, KS, ME, MI, MN, MO, MS, NE, NC, ND, OR, SC, SD, TX, UT, WI, WY.

Table A6: PDMP Mandate Effects on Opioid Prescriptions: Synthetic Control Method

	All	Appropriate	Inappropriate
Panel A: All			
Post x Kentucky	-0.038	-0.018	-0.075
RMSPE ratio rank	[2]	[6]	[1]
Dep. Var. Mean	0.26	0.64	0.38
Panel B: Naive			
Post x Kentucky	-0.033	-0.031	-0.070
RMSPE ratio rank	[2]	[5]	[2]
Dep. Var. Mean	0.22	0.65	0.34
Panel C: Non-Naive			
Post x Kentucky	-0.045	-0.039	-0.078
RMSPE ratio rank	[2]	[3]	[1]
Dep. Var. Mean	0.33	0.63	0.45
N	1540	1532	1540

Note: Each estimate shows the coefficient from a synthetic control model. Each panel and column represents a different sample.Root mean squared prediction error (RMSPE) ratio rank is calculated using the method proposed by Abadie, Diamond, and Hainmueller (2010). All ranks are out of 35, with the exception of the non-naive, appropriate sample which is out of 31. For this sample, states without observations across all quarter-years were dropped (AK, DE, SD, WY).

Table A7: Alternate Trend Specification: PDMP Mandate Effects on Opioid Prescriptions

	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 A8: PDMP Mandate Effects on Opioid Prescriptions 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 A9: 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 A10: 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

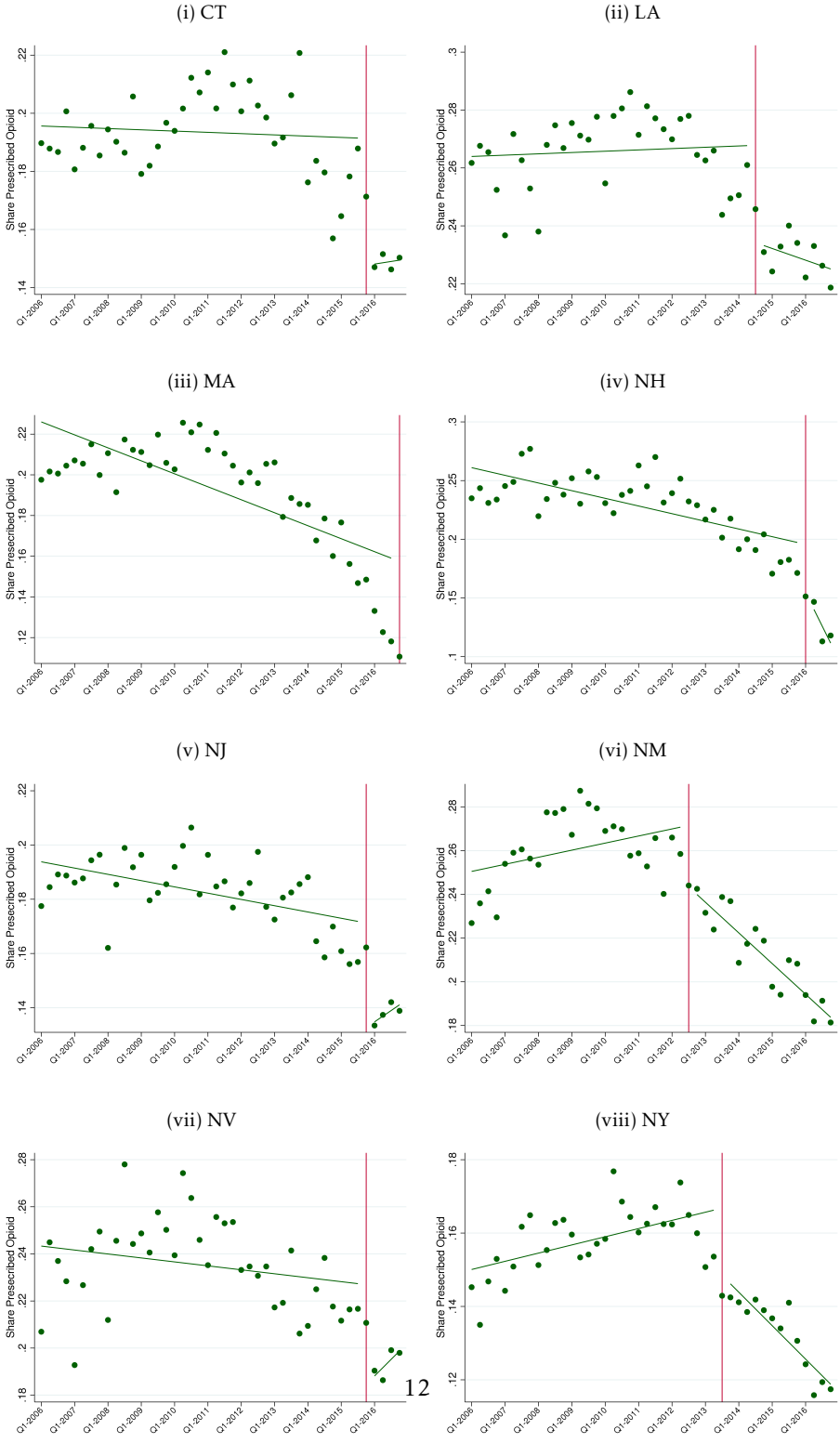
Note: *** p<0.001, ** p<0.01, * p<0.05. Standard errors are clustered at the state level. Each estimate shows the coefficient on the difference-in-differences term (post x KY) from a separate regression. All specifications include state and year fixed effects, KY specific linear trend, and full set of 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).

Table A11: PDMP Mandate Effects on Opioid Prescriptions: Integration of All Robustness Tests

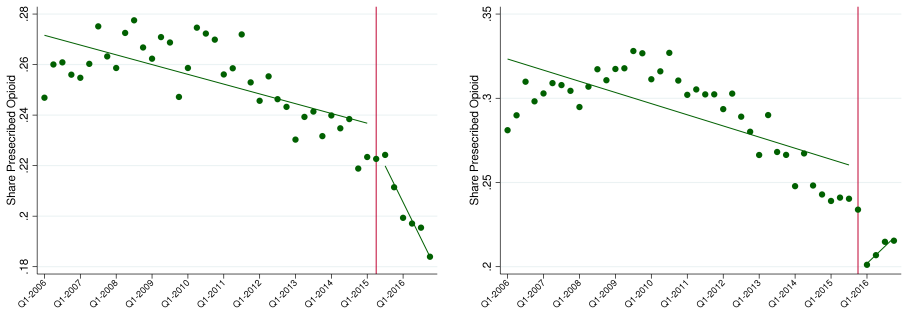
	All	Appropriate	Inappropriate	Unclassified
Panel A: All				
Post x Kentucky	-0.031***	-0.023***	-0.071***	-0.027***
	(0.0014)	(0.0042)	(0.0032)	(0.0012)
Dep. Var. Mean	0.25	0.64	0.38	0.21
Panel B: Naive				
Post x Kentucky	-0.024***	-0.020**	-0.059***	-0.021***
	(0.0014)	(0.0060)	(0.0029)	(0.0012)
Dep. Var. Mean	0.21	0.64	0.33	0.16
Panel C: Non-Naive				
Post x Kentucky	-0.037***	-0.028***	-0.082***	-0.031***
	(0.0022)	(0.0059)	(0.0042)	(0.0020)
Dep. Var. Mean	0.31	0.64	0.44	0.27
N	2244	2235	2244	2244
Panel D: Triple Difference				
Post x KY x Non-Naive	-0.012***	-0.012	-0.023***	-0.0097***
	(0.0025)	(0.0092)	(0.0035)	(0.0025)
Dep. Var. Mean	0.25	0.64	0.38	0.21
N	4488	4479	4488	4488

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. The sample excludes patients with a benzodiazepine prescription and includes all states as controls. All specifications include state and year fixed effects, KY specific linear trend, the interaction with a linear trend (post x KY x trend), and the full set of controls. Outcome is the share of patients receiving an opioid following an ED visit, defining naive status using a 9-month lookback period. 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

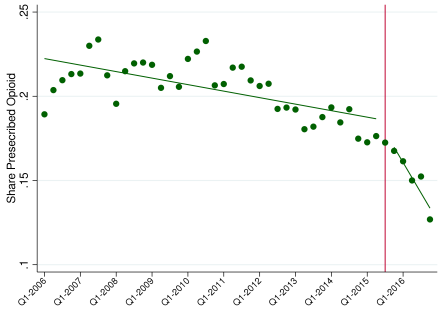


Note: Share of patients receiving an opioid following an ED visit using Optum data from 2006-2016 for all other mandate states. Vertical line represents introduction of mandate in each state.

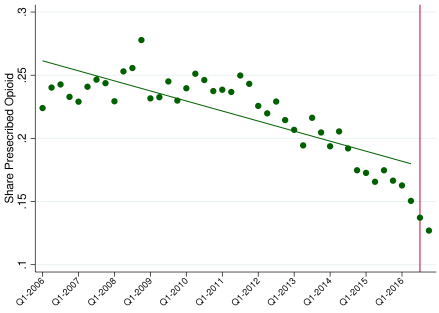


(i) OH

(ii) OK

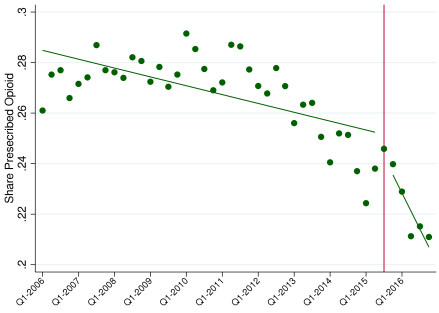


(iii) PA

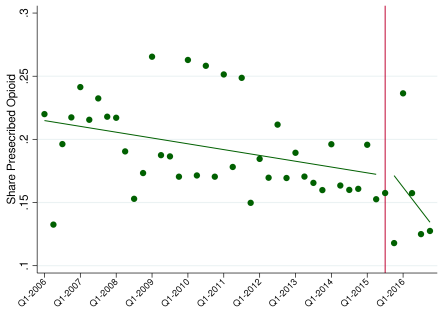


(iv) RI

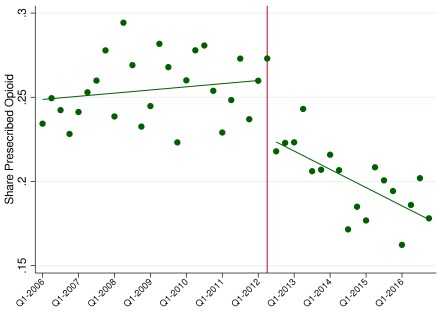
(v) TN



(vi) VA



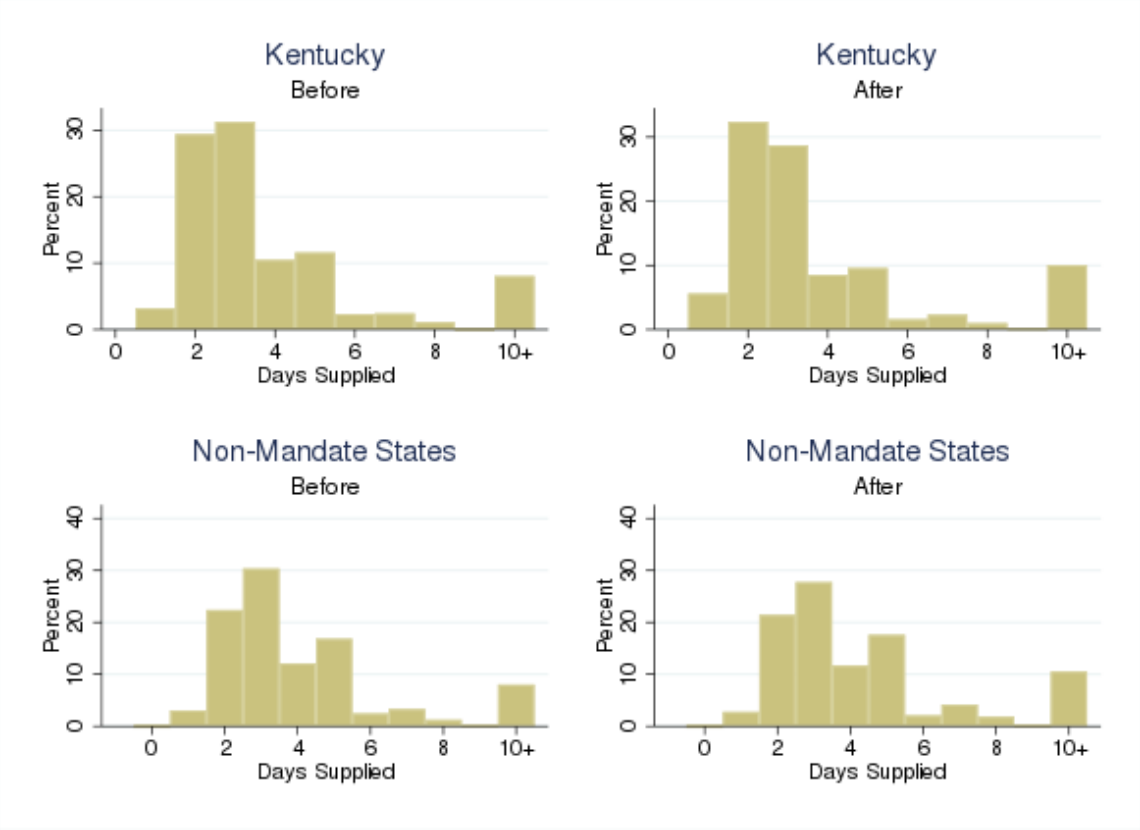
(vii) VT



(viii) WV

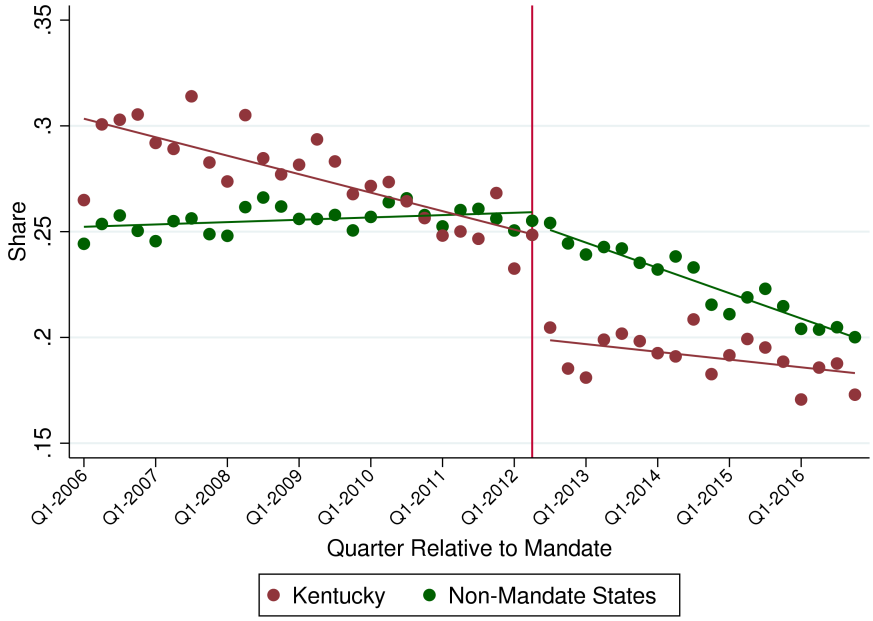
Note: Share of patients receiving an opioid following an ED visit in KY vs. non-mandate states using Optum data from 2006-2016. Vertical line represents introduction of KY mandate in Q3 of 2012.

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.

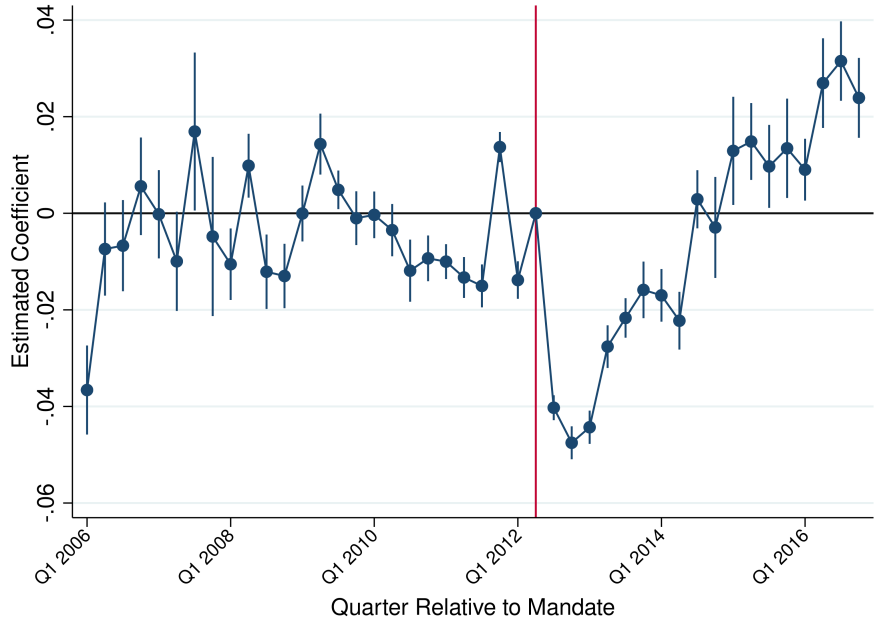
Figure A3: Rate of Opioid Prescriptions in Kentucky and Non-Mandate States



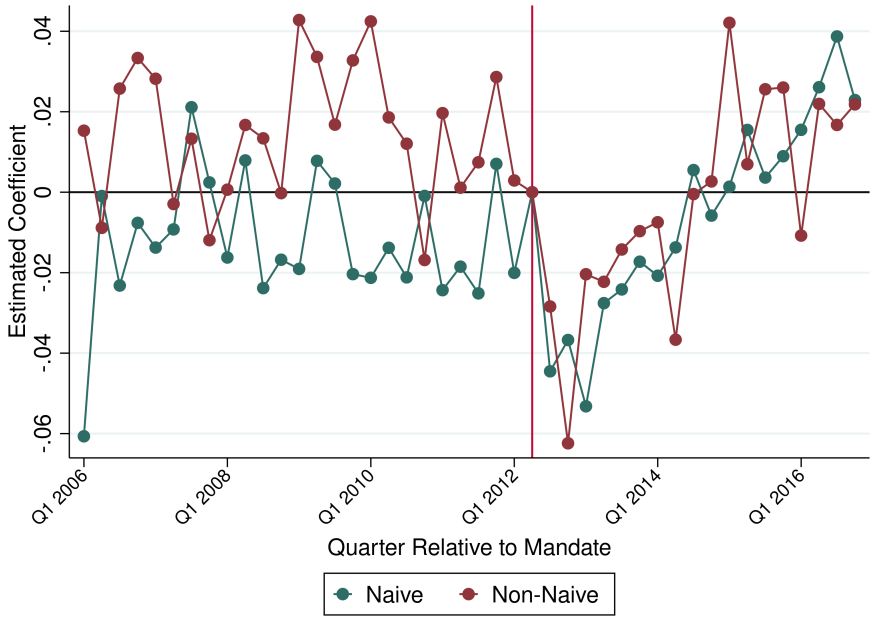
Note: Share of patients receiving an opioid following an ED visit in KY and non-mandate states using Optum data from 2006-2016. Vertical line represents introduction of KY mandate in Q3 of 2012.

Figure A4: Event Study of of Opioid Prescriptions Relative to KY Mandate Including a Kentucky-Specific Time Trend

(i) All



(ii) 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. Specification includes a Kentucky-specific linear trend.

Figure A5: Rate of Opioid Prescriptions in Kentucky by Naive Status

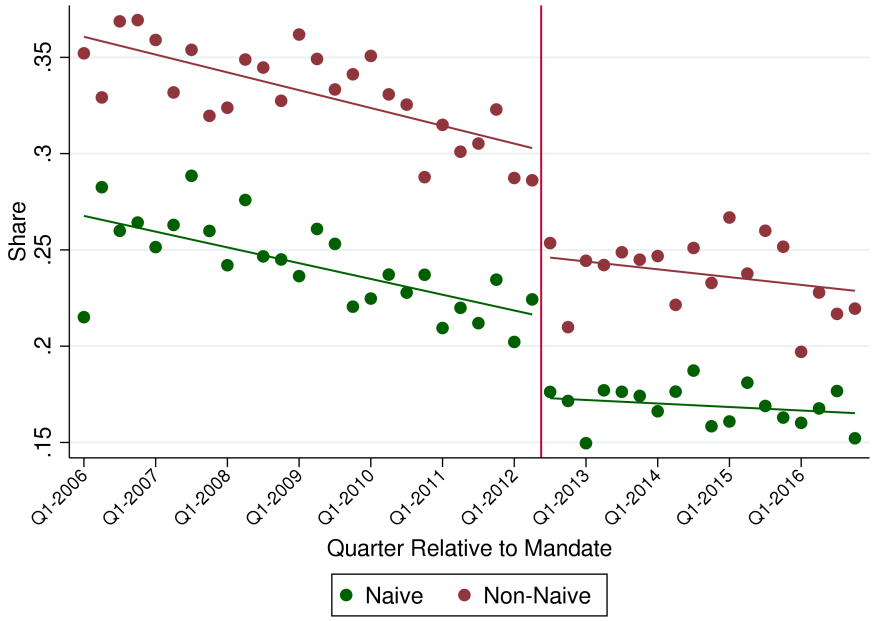
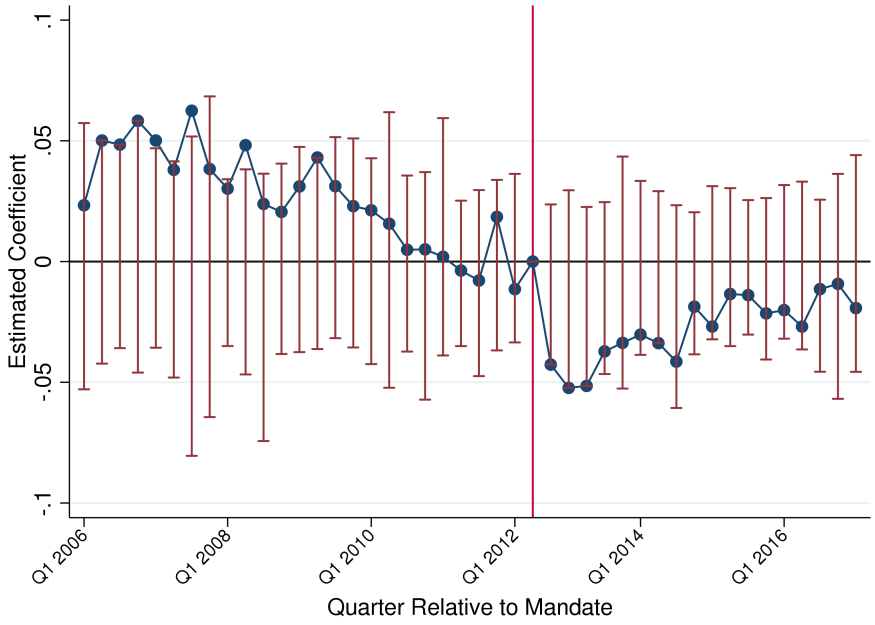


Figure A6: Event Study of Opioid Prescriptions Relative to KY Mandate. Confidence Intervals from 5th and 95th Percentile of Coefficients from Permutation Test

(i) All



(ii) By Naive Status

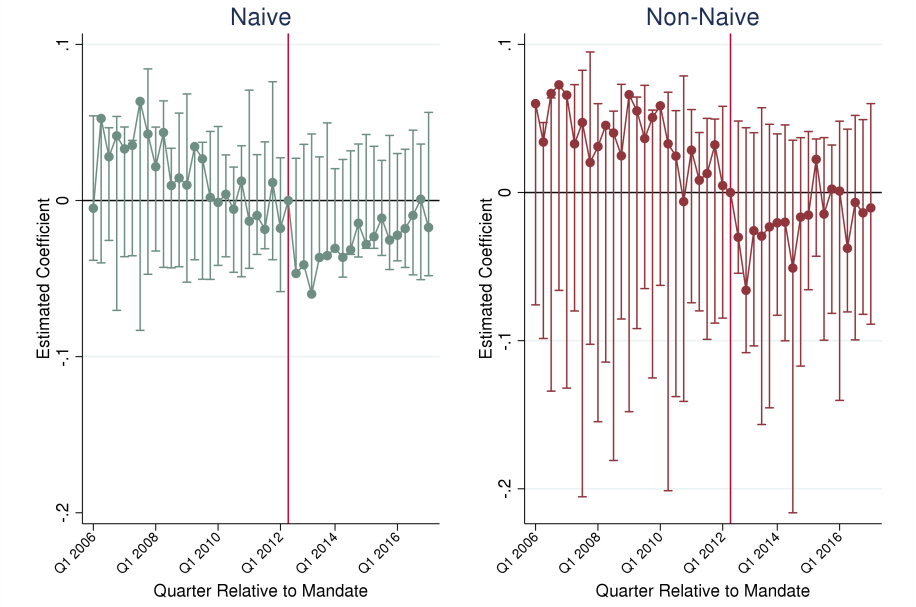
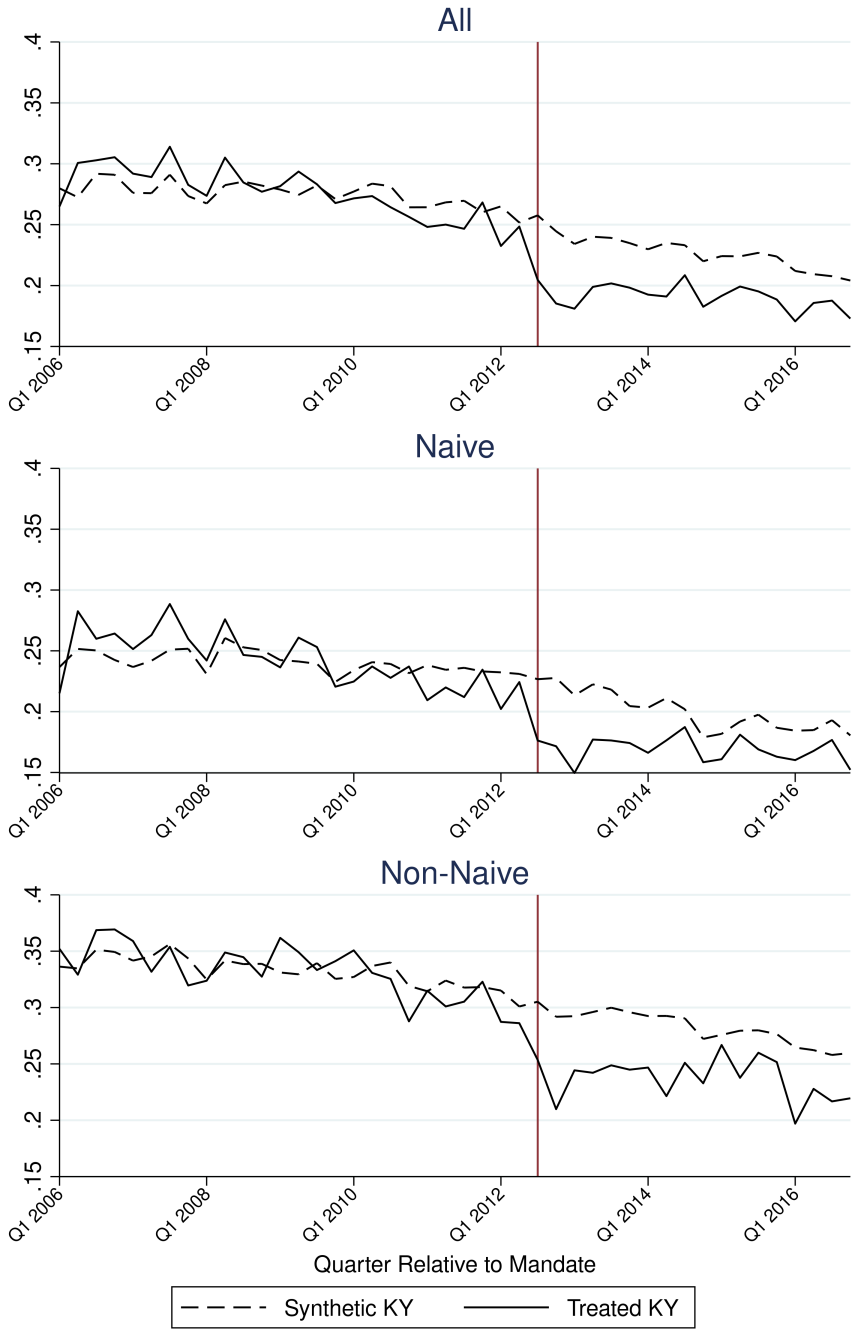


Figure A7: Trends in Opioid Prescriptions in Kentucky and Synthetic Kentucky, Overall and by Naive Status

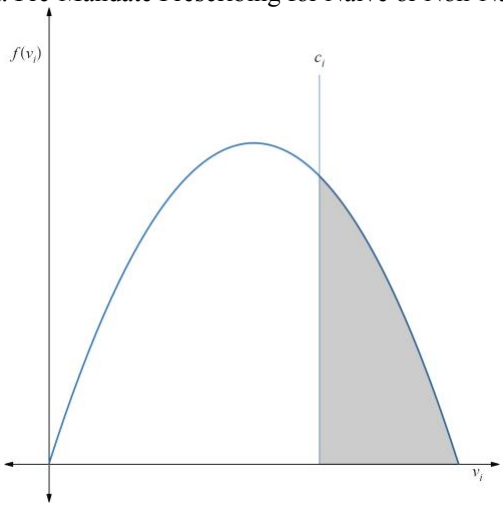


Appendix B

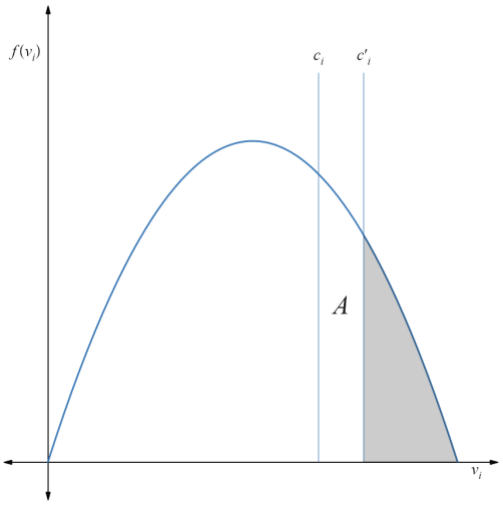
B.1 Modelling Information and Hassle Costs

Figure B1: Decomposing Information and Hassle Cost Effects

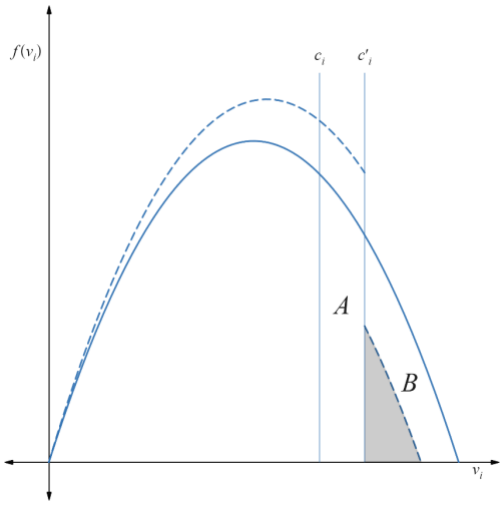
Panel A. Pre-Mandate Prescribing for Naïve or Non-Naïve Patient



Panel B. PDMP Mandate Effect for Naïve Patient



Panel C. PDMP Mandate Effect for Non-Naïve Patient



Note: These figures show information and hassle cost effects for the providers who would not voluntarily search the PDMP pre-mandate for a given patient type, since the mandate is only binding for this group. The solid line in each panel is the ex-ante distribution of provider beliefs about net benefits v_i; the dashed line is a potential ex-post distribution of updated provider beliefs. For naïve patients, the ex-ante and ex-post distribution of beliefs are effectively the same since any updating of beliefs will not affect prescribing (see Appendix B.2 cases 2 and 3). The shaded area under each density function represents the share of patients receiving opioid prescriptions. In Panel A, patients receive an opioid in the pre-mandate period if $v_i \geq c_i$. In Panel B, the reduction in prescribing to naïve patients is denoted by the area labeled A and is due entirely to hassle costs. In Panel C, the reduction due to hassle costs for non-naïve patients is the same as for naïve patients (A). The reduction due to information is denoted by the area labeled B. In this hypothetical case, provider updating shifts the distribution of net benefits down by a constant amount. For a given distribution of ex-ante provider beliefs, the information effect is recovered by subtracting the reduction in prescribing to the naïve from the reduction to the non-naïve ([A+B] – A = B).

B.2 Predicted Information Effects for Naïve and Non-Naïve Patients

In this section, we provide predictions for how prescribing adjusts in response to *information* obtained as a result of the PDMP mandate. To understand how Kentucky's mandate could have affected opioid prescribing through the information channel, we must consider PDMP search and prescribing in the absence of the mandate. Prior to the mandate, providers could search the PDMP and get information or prescribe solely based on the observable characteristics of the patient (i.e., signals that are correlated with opioid history), independent of search. How prescribing changes after the mandate depends on the provider's initial PDMP search behavior and the provider's initial beliefs about the value of prescribing an opioid to the patient, e.g., the non-naïve status of a patient based on observable characteristics and the clinical benefit based on the diagnosis. We show that in all cases, prescribing weakly decreases after the mandate.

PDMP search behavior and prescribing decisions across provider-patient pairs in the pre-mandate period can be grouped into three cases: 1) providers who searched the PDMP for a given patient, 2) providers who did not search and did not prescribe because they believed the patient had a low net benefit from an opioid prescription (based on beliefs that they were non-naive and/or had low clinical benefit), and 3) providers who did not search but prescribed because they believed the patient had high net benefit from an opioid prescription (based on beliefs that they were naïve and/or had a high clinical benefit). We present these three pre-mandate scenarios and the subsequent search/prescribing behavior in the post-mandate period that would follow from these initial behaviors.

Case 1. Provider Searches Pre-mandate: We first consider providers who searched the PDMP pre-mandate for a given patient type. Providers who incurred the cost to search for a given type of patient pre-mandate will also incur the cost to search post-mandate. Thus, the mandate

provides no new information to the provider and their prescribing should be unchanged. For simplicity, we do not include these providers in Figure B1 since their behavior is unchanged by the mandate.

Case 2. Provider does not search and does not prescribe pre-mandate because she believes the patient has low net benefit, $v_i < c_i$: In the second case, we consider providers who did not search the PDMP pre-mandate for patient types they believed had a low net benefit from receiving an opioid. The reasons for this low benefit could be that the provider believes the patient is non-naive based on ex-ante observable characteristics, such as exhibiting problematic behaviors (e.g., showing up intoxicated or clearly under the influence of opioids) or clinical information provided in other electronic health records indicating opioid abuse. Furthermore, the provider may believe the patient has low clinical benefit from an opioid (based on their diagnosis) regardless of opioid history. In this case, which corresponds to $v_i < c_i$ in Figure B1, providers would not prescribe an opioid to this patient type prior to the mandate.

After the mandate, patients of the type believed ex-ante to have low net benefit will continue to be viewed as such (i.e., $v_i < c_i'$ given that $v_i < c_i$) and the provider will continue to be unwilling to prescribe an opioid. Since the provider does not intend to prescribe an opioid for this patient type, they will not search the PDMP for these patients and there will be no updating of information about non-naïve status. The key insight here is that a patient type not even considered for an opioid in the absence of the mandate should not, once a mandate is in place, be considered for an opioid prescription. This implies that an opioid naïve patient believed to be low benefit because they are misclassified as non-naïve will not suddenly receive an opioid once the mandate goes into effect.

Case 3. Provider does not search but prescribes pre-mandate because she believes patient has high net benefit, $v_i \geq c_i$: In the third case, we consider providers who did not search the PDMP pre-mandate for a patient type believed to have a high net benefit from receiving an opioid. Pre-mandate this patient type received an opioid because they were believed to be naïve and/or have high clinical benefit based on their diagnosis. This case corresponds to $v_i \geq c_i$ in Figure B1.

For this patient type, the mandate will now require that the doctor searches the PDMP before prescribing them an opioid. How the information in the PDMP affects ex-post prescribing depends on whether the patient is opioid naive or non-naive.[1]

3a. Patient is opioid naïve: For patients who are opioid naïve, the information gained in the PDMP could be positive but there is no effect on prescribing. Some patients were initially believed to be naïve and search simply confirms this ex-ante belief. Other patients were mistakenly considered non-naïve but still prescribed opioids because providers deemed them to have high clinical benefit based on their diagnosis. For these patients, PDMP search causes the provider to positively update their beliefs about the net value of opioids to the patient. But because they would have received an opioid pre-mandate, this information has no effect on prescribing. For this reason, we represent the distribution of v_i as unchanged after the mandate for naïve patients in Figure B1.[2]

3b. Patient is opioid non-naïve: For patients who are opioid non-naïve, PDMP search could provide negative information and prescribing will weakly decline. Specifically, based on

[1] For simplicity, we categorize opioid history as naïve or non-naïve in our framework, but we recognize that within the non-naïve category there are patterns of prescription fills that would be classified as more or less problematic. It would not change the basic predictions from our model to take these more nuanced classifications into account.
[2] In practice, the distribution of v_i could change such that there is more mass at higher values of v_i. However, since no naïve individuals who were above the threshold for prescribing will be shifted to below the threshold and vice versa, the area under the distribution (above c_i') will remain the same pre- and post-mandate. Thus, for simplicity we represent this as no change in the distribution since it has no impact on prescribing.

search post-mandate, the provider will discover that some patient types who they believed to be naïve are actually non-naïve. They will negatively update their beliefs which will shift the distribution of v_i in Figure B1. Thus, the information in the PDMP will weakly reduce prescribing to the non-naive.

The above scenarios clarify that prescribing weakly decreases as a result of the information provided after the PDMP mandate.[3] Information causes prescribing to weakly decrease for the non-naïve and does not affect prescribing to the naïve. With the mandate, doctors will only get new information through search for patient types that absent the mandate would not have been subject to search but would have been prescribed an opioid (Case 3). Furthermore, this information will only negatively affect beliefs and thereby prescribing for those who are non-naïve (Case 3b).

[3] The above framework is based on a rational model of provider decision making. Large psychological costs of searching or other behavioral biases could generate different predictions. For example, if the mandate enables some prescribers to overcome psychological barriers to searching such that the mandate pushes them to consider some patients for opioids who were never even considered previously, then prescribing could increase to opioid naïve patients. In practice, however, we do not observe an increase in opioid prescribing suggesting this type of model may be unlikely.