The Impact of Opioid Pharmaceutical Representative Interactions and Provider Prescribing Practices: Evidence from Purdue Detailing Records*

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Abstract

The aggressive marketing strategies of pharmaceutical companies are regarded as key contributors to the current opioid epidemic that has resulted in the deaths of nearly 750,000 individuals in the United States. However, due to lack of access to marketing records, it has been difficult to directly tie these marketing strategies to individual providers' prescribing behaviors and resulting patient outcomes. Using newly-available marketing records documenting Purdue Pharma's sales representatives visits with healthcare providers in Massachusetts from 2007 to 2017, we directly tie "detailing" visits of providers to changes in Purdue-specific prescribing activity and downstream health outcomes among Medicare beneficiaries. Using an event study framework, we find that shifting a physician from a below-median volume of detailing visits (1 visit per year) to an above-median volume of detailing visits (2–10 visits per year) increases physicians' prescriptions of Purdue Pharma opioids by an average of **0.595** prescriptions per quarter (**8.9**% relative to the mean). Given that this may in part be driven by unobservable compositional changes in the patients the physicians treats, we further explore within-beneficiary changes in the probability of filling a Purdue opioid prescription after one's primary ("key") provider is detailed. We find that, relative to patients whose key provider receives a below-median volume of detailing visits, patients of a key provider with an above-median detailing volume are **0.6** percentage points (**5.4**% relative to the mean) more likely to fill a prescription for a Purdue opioid in any given quarter following the first year of detailing visits. We further find evidence that patients of detailed providers in the top tercile of detailing volume (5–10 visits in the first year) are 45% more likely to experience a fall or fracture following detailing visits. Together, these results imply that Purdue's detailing of physicians has negative welfare implications for patients.

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1 Introduction

Nearly three quarters of a million deaths in the US have been attributed to the ongoing opioid epidemic that is one of the defining public health crises of the early 21^{st} century (Centers for Disease Control and Prevention, 2024). While a variety of supply- and demand-side factors have contributed to the epidemic, the aggressive marketing tactics of opioid manufacturers in the late 1990's and early 2000's are regarded as key contributors (Haffajee and Mello, 2017; Maclean et al., 2022; Van Zee, 2009). Among these firms, Purdue Pharma, the manufacturer of OxyContin, stands out as a leading aggressor. In 2001 alone Purdue spent nearly $200 million in its promotion of Oxycontin, and more than doubled its sales force (from 318 to 671) from 1996 to 2000 (Van Zee, 2009). During this time and continuing into the 2010's, it conducted "detailing visits", sending representatives across the country to meet with healthcare providers, with the explicit goal of downplaying the addictive nature of opioids and offering payments to "induce" providers to write more prescriptions (United States Attorney's Office for the District of New Jersey, 2020). These tactics, and the misleading claims that Purdue's products were less addictive (and therefore less dangerous) than other opioids, have lead to a series of lawsuits that have resulted in Purdue's acknowledgment of their role in the opioid crisis and in unprecedented payments by Purdue for this established wrongdoing [**CITE**].

However, establishing a clear empirical relationship between Purdue's marketing activities and harms to the public has not necessarily been straight-forward. While prior work has used variation in state-level policies to indirectly measure the impact of such marketing practices on provider opioid prescribing behavior and subsequent patient welfare (Alpert et al., 2021; Arteaga and Barone, 2023), the ability to directly quantify this relationship has been limited by lack of access to marketing records. To address this gap, we leverage newly available data containing in-depth information on tens of thousands of visits between Purdue Pharma representatives and clinicians in Massachusetts from 2007–2017. Records of these "detailing" visits were made available as part of a 2019 lawsuit brought against Purdue by the Commonwealth of Massachusetts, alleging that Purdue and its executives and directors (including members of the Sackler family who owned Purdue) "deceived doctors and the public to get more people on addictive opioids, at higher doses, for longer periods of time," resulting in the deaths and suffering of thousands of Massachusetts residents (Office of the Attorney General of Massachusetts, 2024).

In this paper, we link Purdue detailing records to administrative Medicare claims from Massachusetts to

quantify the impact of detailing visits on physicians' opioid prescription patterns and plausibly related patient outcomes.[1] We evaluate the impact of visits at both the physician and patient level to explore two different but related implications of detailing. First, we implement a physician-level approach to determine how detailing impacts physician prescriptions of the target drug. However, this approach does not account for underlying changes in demographic and clinical characteristics of a physician's patient panel that may be correlated with both opioid use and detailing. To gain more insight on patient impact, our second approach explores within-patient changes in opioid prescription fills among patients whose primary physician is detailed.

Our first empirical strategy seeks to answer the question: how does detailing influence physician prescribing? Using an event study framework, we estimate the relationship between the volume of detailing visits a physician receives and changes in the volume and dosage strength of Purdue opioid prescriptions filled by the patients they treat.[2] We recognize that never-detailed physicians are likely to be different from ever-detailed in ways that are important to our empirical setting (which we discuss more below). Thus, our main strategy leverages differences in degree of exposure to detailing, conditional on having been detailed at all. We define exposure based on the first four calendar quarters beginning with the quarter in which a physician is first detailed.[3] We then compare differences in the change in the opioid prescription volume attributed to physicians with above-median exposure (**2-10** visits in the first four quarters, with an average [SD] of **4.3** [2.3] visits) to physicians with below-median exposure (**1** visit in the first four quarters).

Compared to below-median exposure physicians, we observe a **0.595** (**8.9**% on the pre-detailed mean) average increase in the number of Purdue opioid prescriptions filled by patients of physicians with above-median exposure in the six quarters following the four-quarter exposure definition period. We find that this increase is driven by a **6.5**% increase in the number of a physician's patients with an opioid prescription. We find a marginally significant **9.1**% increase in the dosage strength in morphine

[1] While non-physician clinicians, such as nurse practitioners and physician assistants, were also targeted by pharmaceutical representatives, we cannot reliably identify them in our data, and thus restrict our analysis to Doctors of Medicine (MDs) and Doctors of Osteopathy (DOs).

[2] We circumvent a key limitation of the data—our prescription records do not contain the identifier of the provider who wrote them—by attributing patients to physicians in each quarter that a physician submits a reimbursement claim for an evaluation and management (E&M) visit with a patient. This approach attributes patients to physicians who treat them in a non-acute context and have some oversight and influence over their opioid use, even if not writing prescriptions directly.

[3] For example, if a physician is detailed on June 20, 2008 (during quarter 2 of 2008), we define their exposure as the number of detailing visits (in days) they receive between quarter 2 of 2008 to quarter 1 of 2009—that is, from April 1, 2008 through March 31, 2009. We use this quarterly definition, rather than an exact 365-day window (June 20, 2008 and June 19, 2009), to maintain consistency with the quarterly empirical framework described below.

milligram equivalents (MME) of opioid prescriptions per patient.

We next turn to a related but separate question about the impact of detailing on patients: how does their physician being detailed impact a patient's opioid use? We first assign patients to "key providers" who are responsible for the plurality of their office visits. Using an event study framework, we compare changes in the probability that a beneficiary fills a Purdue opioid prescription in a given quarter, before versus after their key provider is first detailed, among beneficiaries whose key providers have above-median exposure versus those whose key providers have below-median exposure. We find a **0.6** percentage point (**5.4% relative to pre-treatment mean**) relative increase in the likelihood of filling a Purdue opioid prescription among beneficiaries with above-median exposure key providers. This finding is particularly notable given a key feature of this analysis: unless there was a contemporaneous change in a beneficiary's clinical profile that made them more appropriate for opioids, this change in probability is likely due to the influence of detailing on their key provider's beliefs around opioids.

While our results suggest that detailing visits induce the prescription of Purdue drugs—even without any change in patient clinical condition—the impact on patient welfare is ambiguous. On the one hand, opioid use is associated with a risk of developing opioid dependence; prescribing patients more opioids without any clinical indication may result in an unnecessary increase in the risk of opioid abuse or misuse. On the other hand, opioids can be useful in the management of pain.[4] To more explicitly determine implications for patient welfare, we re-estimate our model for several measures of opioid abuse/misuse. While we find no evidence that detailing visit volume impacts the probability of behavior indicating opioid misuse, such as high-dosage opioid prescriptions or overlapping prescriptions, we find strong evidence of a **45**% relative increase in the likelihood of a patient having a fall or fracture for patients whose key providers are in the top tercile (5 - 10 visits) of exposure. Prior work has established falls and fractures as an adverse consequence of opioid use among the elderly (Solomon et al., 2010; Yoshikawa et al., 2020). We find similar effects in the physician-level analysis: there is a **25**% relative increase in the number of patients attributed to physicians with above-median exposure who experience a fall or fracture in the post-period. This effect is strongest in the top-tercile exposure (5 - 10 visits) of detailed physicians.

Our study makes several contributions to the literature. First, we extend the existing literature on opioid manufacturers' marketing strategies and provider prescribing behavior by estimating the direct

[4]Though an extensive volume of literature suggests that chronic pain is *not* well managed by long-term opioid use (Busse et al., 2018).

relationship between detailing visits and provider prescribing behavior (and associated patient health outcomes). Two prior studies have used unsealed court documents to show that Purdue's targeted marketing of OxyContin to areas with more lax opioid prescribing policies essentially protected states with stricter policies against some of the more substantial adverse outcomes associated with the opioid epidemic. Specifically, Alpert et al. (2021) found that "triplicate states" in which healthcare providers had to create three copies of any opioid prescription experienced less adverse impacts of the opioid crisis than other states without such policies due to being subject to less aggressive marketing by Purdue. Arteaga and Barone (2023) similarly use these records to link targeted OxyContin marketing to increases in prescription opioid levels as well as increases in opioid mortality and inter-generational effects via impacts on fertility and birth outcomes. We extend this work by directly examining the impacts of marketing on provider behavior and patient outcomes.

Second, a robust existing literature documents responses in provider behavior to the receipt of financial or in-kind incentives from industry, which has also been tied to health consequences in patients (Bergman et al., 2021; Carey et al., 2020; David et al., 2010; DeJong et al., 2016; Larkin et al., 2017; Orlowski and Wateska, 1992; Sharma et al., 2018; Wazana, 2000). Two studies most closely related to our work similarly use detailing records to document the direct link between direct-to-physician marketing and providers' (and their peers') prescriptions of anticoagulants and antipsychotics (Agha and Zeltzer, 2022; Shapiro, 2018). Another closely related study explores the impact of educational outreach ("academic detailing") on provider prescribing of opioids in the Veterans Affairs healthcare system, finding that such outreach can induce providers to adopt more protective approaches to opioid prescribing (such as also prescribing medication to reverse overdoses) (Zhang, Forthcoming). To our knowledge, no other study has used detailing records to look at the direct relationship between marketing and provider prescription of opioids. Opioids are a particularly interesting context in which to study this relationship due to the unique possibility that the drug being marketed can lead to addiction and/or use of illicit drugs, while also having (potential) clinical value.

Third, our work contributes to the literature on the role of supply-side drivers of opioid use, particularly via provider prescribing behaviors. Patients who are treated by providers who prescribe more opioids tend to also use more opioids, even accounting for selection and differences in patient population (Barnett et al., 2017; Currie and Schwandt, 2020; Eichmeyer and Zhang, 2022, 2023; Staiger et al., 2022). More broadly, Finkelstein et al. (2022) show that the nature of the environment in which a patient lives—particularly with regards to risky opioid use—accounts for a large degree of the regional variation

in opioid addiction.

The paper proceeds as follows. Section 2 provides additional detail on the background of the Purdue detailing records used in this study, as well an exploration of the characteristics of detailed providers. Section 3 provides additional detail on the data and methods we use. Section 4 reports our main physician- and patient-level results examining Purdue opioid prescriptions and fills, respectively. Section 5 discusses the impacts of detailing on patient welfare. Section 6 presents robustness checks on our main results. Finally, Section 7 concludes.

2 Background

2.1 Purdue Marketing Strategies

Detailing visits describe marketing practices in which a sales representative from a pharmaceutical or medical device company meets with healthcare providers to "educate" them on their company's particular drug. The explicit goal of the detailing visit is to "influence the physician's prescribing choices so they opt for [their company's] products" (Platforce, 2024). For Purdue, detailing visits represented an opportunity to directly reach healthcare providers and address their concerns about opioids [**CITE**]. Prior to the 1990s, anxiety around the addictive nature of opioids led to low use outside of treatment for acute pain or pain related to cancer (Food and Drug Administration, 2018) [**CITE**]. However, systematic efforts during this decade to encourage healthcare providers to address untreated pain (led in part by opioid manufacturers) caused opioid prescriptions to rise rapidly (Centers for Disease Control and Prevention, 2024; Maclean et al., 2022; McGreal, 2018) [**CITE**]. Among the most popular opioid prescriptions was Purdue's OxyContin, launched in 1996 and marketed as a less-addictive alternative to previous opioids (Maclean et al., 2022; Van Zee, 2009). From 1996 to 2001, sales of OxyContin increased from $48 million to $1.1 billion, with profits reaching $3.1 billion in 2010 (Crofts and van Rijswijk, 2024; Van Zee, 2009). Purdue-manufactured opioids accounted for 16% of the national market for prescription opioids (adjusted for dosage and potency) from 2006–2012 (Armstrong and Ernsthausen, 2019).[5]

[5]In the 2019 lawsuit brought against Purdue by Massachusetts, Purdue's defense team contended that Purdue was not responsible for the opioid epidemic because its prescriptions accounted for only 3.3% of pill volume in the US from 2006–2012. However, an analysis by ProPublica and STAT of the same data used by Purdue to generate this figure noted that without accounting for potency, this number is quite misleading: "It's analogous to measuring alcohol sales by equating a 12-ounce glass of 100 proof whiskey with a similar-sized can of light beer." After accounting for the dose and potency of the prescription (since "[h]igher doses of opioids are associated with greater risk of overdose"), Armstrong and Ernsthausen (2019) found that Purdue's market share of 16% was more than five times that of the 3.3% share they

At that same time, Purdue was heavily investing in their marketing efforts, increasing their sales force from 318 in 1996 to 671 in 2000 (Van Zee, 2009). Documents from the Purdue Pharma House Oversight Committee Investigation provide insights into the detailed and sophisticated marketing strategies used by the company to shape the detailing efforts of their drug representatives. Internal documents from 2013 illustrate rapid changes in marketing approaches, appearing to be driven by data on past drug representatives targeting activities as well as prescribing practices of providers. For example, in May 2013, Purdue instructed representatives to target existing OxyContin prescribers: "Make all effort to reach existing high and medium/low OxyContin and ERO prescribers as there is higher responsiveness from existing prescribers of OxyContin" (Purdue Pharma L.P., 2013).

These aggressive marketing campaigns became the focus of several lawsuits filed by cities, counties, and states. In 2001, West Virginia filed one of the earliest state lawsuits against Purdue in which they alleged aggressive and deceptive marketing tactics of OxyContin to West Virginians (Thomas Jr., 2004); Purdue paid the state $10 million in settlement. In 2007, Purdue pled guilty to criminal charges related to misleading regulators, physicians, and patients regarding OxyContin's addictive nature, as well as its potential to be abused. Purdue's resulting payment of approximately $635 million in fines was, at the time, the third-highest ever payment amount by a pharmaceutical manufacturer for such a case (Meier, 2007; United States Attorney's Office, Western District of Virginia, 2007). The lawsuit filed by Massachusetts in 2018 took a unique approach by naming not only the manufacturer, but also members of the family, accusing them of "misleading doctors and concealing the true risks of Purdue's opioids" via the micromanagement of a "deceptive sales campaign" (Commonwealth of Massachusetts, 2019).

In 2021, Purdue Pharma was dissolved in a bankruptcy settlement that included two landmark conditions. First, it was to pay over $4.3 billion to communities nation-wide for opioid misuse prevention, treatment, and recovery efforts (Hoffman, 2021).[6] Second, it required "unprecedented and complete disclosure about the role Purdue and the Sacklers played in the opioid crisis," making more than 30 million documents—including attorney-client privileged communications about "the original FDA approval of OxyContin and tactics to promote opioids"—publicly available (Office of the Attorney General, 2021).

Prior to this, however, the Massachusetts lawsuit forced Purdue to make public in 2018 the records of over 150,000 individual detailing visits between Purdue representatives and physicians occurring

reported.

[6]The terms of this settlement are ongoing, with total payments growing to $7.4 billion by June 2025 (Stempel, 2025), of which $900 million are from Purdue Pharma and $6.5 billion are from the Sackler family.

in Massachusetts from 2007 to 2017.[7] An archive of these records was subsequently created by the University of California, San Francisco (UCSF) and Johns Hopkins University in 2021, and is maintained online (UCSF-JHU Opioid Industry Documents Archive, 2021).[8] In the next section, we use these records to explore where and when Massachusetts providers were detailed by Purdue Pharma representatives during this time period.

2.2 Who Gets Detailed?

The detailing records contain rich information about sales visits including the date of the visit, the name of the Purdue representative conducting the visit, the "target" provider (first and last name), and the street and city address of the target. Targets include individual clinicians and organizations (such as pharmacies); in this analysis, we focus on detailed physicians.

We first explore variation in the volume of detailing visits across years. The time period represented in these records spans three "waves" of the opioid epidemic (Humphreys et al., 2022; Maclean et al., 2022):

Wave 1. The misuse of prescription opioids (mid 1990's through 2010);

Wave 2. The substitution to illegal opioids, such as heroin, in response to policies aimed at reducing access to prescription opioids (2010 —2013); and

Wave 3. The introduction of synthetic opioids, such as fentanyl (2013 and after).

While prescribed opioids have become less of a focus in later waves, they remained the leading cause of overdose deaths through 2015 (Humphreys et al., 2022; Maclean et al., 2022).

Figure A1 plots the number of detailing visits performed (left y-axis) and the number of distinct physicians detailed (right y-axis) by year-month. We observe an increasing volume of detailing visits through waves 1 and 2 (2007 - 2012), which then began to decline around the beginning of wave 3 (2013). Specifically, detailing visits in Massachusetts grew from between **500–1,000** per month in the beginning of 2007 to a peak of approximately **2,500** per month in 2011, before declining back to 2007-levels by the end of 2018. Similarly, distinct physicians detailed increased from approximately **300** per month in 2007 to a peak of more than **600** per month in **2011**, before falling below **200** per month by 2018. These

[7] "Exhibit 1 - Sales Visit By Purdue in Massachusetts" can be accessed online at https://www.mass.gov/files/documents/2019/07/11/43_02%20First%20Amended%20Complaint%20Exhibit%201%20filed%2001-31-2019%20.pdf.

[8] Records of these sales visits are available here: https://www.industrydocuments.ucsf.edu/opioids/research-tools/sales-visit/

trends suggest a **25-150**% growth in the average number of detailing visits per physician per year-month, from **1.7-3.3** visits per physician in 2007 to **4.2** visits per physician in 2011, before declining in 2018.

Next, we documented geographical variation in detailing visits. Appendix Figure A2 maps the number of detailing visits (panel a) and the number of physicians treating Medicare patients (though not necessarily detailed; panel b) for counties in Massachusetts in 2011. The overlap of the density in detailing visits with the density of physicians reflects the strategy stated in the marketing documents of targeting provider-dense areas [**CITE**].[9] Appendix Figure A3 maps the share of Medicare-participating physicians detailed in 2011.

Appendix Table A1 explores differences in the characteristics of physicians who were ever detailed during our study period (**3,322**), to those who never received a detailing visit (**29,980**).[10] Detailed providers were more likely to be male (69% versus 60%, respectively)—a gender gradient observed in other studies examining direct-to-physician marketing [**CITE**]—and have more years of experience, as indicated by their average year of graduation from medical school (1991 and 1998, respectively). Detailed physicians were more likely to have a specialty associated with primary care (53% internal medicine, 18.5% family medicine) than non-detailed physicians (33% and 5%, respectively), which aligns with the stated focus on primary care providers discussed in Purdue marketing documents. Medicare patient volume appears equivalent between detailed and non-detailed providers: on average, physicians treat approximately 9 patients per month.

Appendix B contains additional details on the data cleaning steps undertaken to prepare these records for use in the study.

3 Methods

3.1 Data

We use two main data sources in our study. The first is the Purdue Pharma detailing records discussed in Section 2, which span May 2007 through December 2017. The second data source is a 20% sample of

[9] This is similar to strategies observed in the marketing of other products, as documented in (Grennan et al., 2024).

[10] Below, we discuss the transition from one type of healthcare provider identifier ("UPIN") to another ("NPI") in 2007; we matched names in detailing records to NPIs, requiring a UPIN-NPI crosswalk in our claims data to facilitate matching for earlier years. However, if our UPIN-NPI crosswalk is incomplete, these counts may be a lower-bound on the actual number of physicians detailed and/or participating in Medicare in Massachusetts.

Fee-For-Service (FFS) Medicare administrative billing claims records from 2006 to 2018 from the Centers for Medicare and Medicaid Services (CMS).[11] These data contain the universe of Medicare claims for a randomly selected 20% sample of all Medicare beneficiaries, including administrative claims generated by professionals and facilities as well as records of prescription fills. To match the information available in the Purdue records, we focused on all claims generated by physicians and patients providing and receiving services (respectively) in Massachusetts.

One notable challenge relates to CMS' Part D Event (PDE) files, which contain records of prescriptions filled by Medicare beneficiaries.[12] Prior to 2014, the healthcare provider who wrote the prescription was not recorded with the standard National Provider Identifier (NPI) used to identify providers in the other claims files.[13] This prohibits us from observing which provider wrote the prescription. To circumvent this limitation, we attribute patients to any provider with whom they had an outpatient evaluation & management (E&M) visit in a particular quarter. We attribute any opioid prescription(s) filled by that patient (in that quarter) to that provider.[14]

Notably, while it is possible to observe the NPI associated with the prescription fill in PDE records starting in 2014, we apply the attribution approach across all years for consistency. We test the extent to which the approach is appropriate by comparing the frequency with which the NPI associated with a PDE record after 2014 matches the NPI assigned to the patient via our attribution method, finding a match rate of **XX**%. **ADDITIONAL JUSTIFICATION**. We discuss limitations of this approach, as well as any bias it may introduce to our estimates, below. Appendix C provides additional details.

To combine the Purdue detailing records with the CMS datasets, we used the National Provider and

[11] During our study period, **XX**% of Medicare beneficiaires were enrolled in Traditional (FFS) Medicare, as compared to (managed care) Medicare Advantage.

[12] A second, more minor challenge is in how clinicians are identified over time. Midway through 2007, CMS adopted the National Provider Identifier (NPI) as a standardized approach to identifying clinicians. Each clinician treating individuals enrolled in Medicare or Medicaid (which includes the vast majority of clinicians in the US) received a unique NPI that can be used to follow them across time and space. Our study period begins prior to 2007, during which time Unique Physician Identification Numbers (UPINs) were used to identify clinicians in administrative records instead of NPIs. To standardize provider identifiers throughout our study period, we created a crosswalk between UPINs and NPIs to fill in missing NPI's for earlier time periods. In doing so, we were able to assign NPIs to **XX**% of clinicians with a missing NPI (and non-missing UPIN) in earlier years.

[13] From 2006 – 2013, the NPI associated with each Part D event was masked by CMS, and a unique identifier assigned by the Chronic Condition Warehouse (CCW) was used instead. After 2013, NPI was used as the provider identifier, and a "bridge" file was created that provided researchers with a crosswalk between the CCW identifier and the NPI. As of the writing of this draft, we do not have access to this crosswalk. Additional details can be found online at: https://resdac.org/cms-data/variables/prescriber-identification-number.

[14] Note that this can result in multiple physicians being associated with the same patient in a given quarter. **DEVELOP?**

Patient Enumeration System publicly available downloadable files [**CITE**] to link physicians named on marketing records to their National Provider Identifier (NPI's) via their first and last names. We then merged Purdue records to Medicare claims using NPI. Additional details on name-to-NPI linkages are available in Appendix B.

3.2 Sample Construction

Physician-Level Analysis. Our final physician-level analytic sample is comprised of two main physician cohorts: a "never-detailed" cohort, and an "ever-detailed" cohort. The never-detailed cohort is the set of physicians that never receive a detailing visit, and who are affiliated with groups (as identified by the tax identification number [TIN] on the claim record) whose members are either never detailed or are not yet detailed.[15] In the empirical strategy we discuss shortly, never-detailed physicians (who do not have a pre- and post-period) contribute to the estimation of event-time effects through estimates of calendar-time effects. In an attempt to create a set of "secular trends" that are as relevant as possible to our treated physicians, we select a matched sample of never-detailed physicians based on several provider-level covariates.[16] This reduces the never-detailed sample from **19,761** distinct providers to **5,184**. Table A5 compares the matched and unmatched control sample.

The ever-detailed cohort is the set of physicians who receive a detailing visit at some point during the study period. Appendix Figure A4 plots the distribution of the volume of detailing visits an in-sample physician received in the year following their first visit. Note that while 95% of providers have ten or fewer visits in the first year, 5% have between 11 and 54 visits.[17] In our main analyses, we drop these top 5% of physicians, since preliminary analyses suggest that their pre-detailing prescribing patterns are notably different than other detailed physicians and never-detailed physicians.[18]

In our empirical approach, we leverage a physician's first detailing visit as the key treatment event. Figure A5 shows the distribution of the timing of first detailing visits, by calendar quarter, among treated physicians. The mass of first visits occurring in the first part of 2007 is at least in part mechanical,

[15] This group restriction is motivated by Agha and Zeltzer (2022), who found spillovers on prescribing behaviors from detailed physicians to their peers.

[16] **UPDATE** Specifically, we match ever-detailed physicians to up to ten never-detailed providers based on: specialty, gender, average number of patients seen per quarter, and pre-detailed average Purdue-specific opioid volume associated with patients treated per quarter.

[17] **PLACEHOLDER:** ***Describe top 5%.***

[18] This aligns with known marketing strategies in which sales representatives systematically target specific providers based on prescribing information. We address this in further detail in Section 3.5. We also test the robustness of our results to the inclusion of these physicians.

as they also corresponding to the start of the data. Thus, it is likely that many of the "first visits" represented in the 2007 mass are not in fact first visits; we drop the **519** physicians whose first detailing visit was in the first half (i.e., January through June) of 2007. We also test the robustness of our results to excluding all first visits that occur in 2007. Our final ever-detailed sample includes **2,168** physicians. Appendix Table **XX** reports the number of physicians at each restriction point.

We further subset our ever-detailed physicians into cohorts with above- and below-median exposure to detailing. Specifically, we define a physician's detailing exposure as the number of detailing visits (in distinct days) they received in the four quarters after their first detailing visit occurs. In this context, the quarter of the first detailing visit is quarter 1 of this definitional period. We define exposure at the quarterly level (as opposed to a 365-day outlook) in order to be consistent with our quarterly empirical framework.[19] In our final sample, the **1,198** physicians with above-median exposure have **2–10** visits in the definitional four quarters, while the **970** physicians with below-median exposure have **one** visit in those four quarters (Appendix Table A3).

Notably, the Purdue detailing records contain information on visits that took place with physician and non-physician clinicians (e.g., Advanced Practice Clinicians; APC). Scope of practice laws in Massachusetts allowed APCs to prescribe controlled substances, such as opioids, during our study period. However, a well-known limitation of administrative medical claims data is that in many instances (and even in states with non-restrictive scope of practice laws), APCs submit reimbursement claims using a physician's NPI, a practice referred to as "incident to billing."[20] For empirical integrity, our main analysis focuses on the impact of detailing visits on physicians.

Additional details on constructing the physician-level sample are provided in Appendix E.

Beneficiary-Level Analysis. Our construction of the beneficiary-level analytical sample is guided by the requirement of our empirical strategy that we observe all of a beneficiary's healthcare utilization records. To that end, we include only those individuals who are enrolled in Medicare Parts A (hospital insurance), B (medical insurance), and D (drug insurance), and exclude those enrolled in Part C (Medicare Advantage), as our data only includes claims for beneficiaries enrolled in traditional fee-for-service

[19] **PLACEHOLDER**: *robustness for 365-day definition.*

[20] This practice allows the service to be reimbursed at the higher physician rate than at the rate for a service provided by an APC.

Medicare (and not Medicare Advantage).[21]

As in the physician-level analyses, we identify a set of "never-treated" and "ever-treated" beneficiaries. Never-treated beneficiaries are those who are never treated by a detailed physician *or* are never treated by a physician in a TIN that has ever ("never-detailed") or yet ("not-yet detailed") had members who were detailed. To be included in the final analytic sample, never-treated beneficiaries must have a continuous enrollment period of at least **15** quarters, i.e. the full length of the event study panel. For computational reasons, we take a random sample of beneficiaries with the goal of having a 4:1 ratio of never-treated to ever-treated beneficiaries. We construct a matched sample of never-treated beneficiaries, matching on mean age, SSDI receipt, chronic pain diagnosis, and opioid use (additional details provided in Appendix **XX**). Our final matched sample includes **43,803** never-treated beneficiaries.

Ever-treated beneficiaries are individuals who were treated by a detailed physician prior to the physician's first detailing visit. Specifically, for each detailed physician, we capture all patients with whom that physician had an E&M visit in the **four** quarters prior to the first detailing visit. If that physician was the provider responsible for the plurality of E&M visits during that time, we label the physician the patient's "key provider," and consider that patient to be ever-treated. On average, key providers have **4.10** (SD: **XX**) E&M visits with these patients, and are responsible for **56**% (SD: **XX**) of their total E&M visits during the key provider identification period. Of note, while we restrict our primary analysis to patients whose key providers are physicians, we define a patient's key provider based on the clinician—physician or non-physician—responsible for the plurality of E&M visits in the pre-period. As opposed to restricting the provider pool to physicians prior to capturing the key provider, we consider this approach to be more conservative and empirically honest. Specifically, the provider selected as the key provider is the clinician who was actually responsible for the majority of that patient's E&M visits.

We identify two sub-populations of empirical relevance with respect to their likelihood of being prescribed an opioid. First, we flag whether a beneficiary was a Social Security Disability Insurance recipient. Such individuals, who receive supplemental income through the Social Security Administration, are considered to be unable to work due to a physical or intellectual disability and can qualify for Medicare coverage even if they are younger than 65 (the standard age for Medicare eligibility). Other research has characterized this population as being both highly likely to have a prescription opioid and more likely to have opioid-related overdose relative to the general population (Meara et al., 2016; Finkelstein et al.,

[21]Note that our physician-level analysis does not require such a restriction, as our empirical strategy does not rely on continuous representation at the beneficiary level.

2021).

Second, we flag beneficiaries with chronic pain. Purdue promoted the use of opioids in this patient population in particular, referencing an excess of "untreated pain" that could be better managed with long-term opioid use (Van Zee, 2009) [**CITE**]. Notably, later research would show that opioids had little to no benefit in managing chronic (non-cancer) pain (Busse et al., 2018). We identify beneficiaries with a chronic pain diagnosis following guidelines published by the Chronic Condition Warehouse [**CITE**]; additional details on this approach are available in Appendix F. In our beneficiary-level analyses, we focus on the ever-detailed sample of beneficiaries who had a chronic pain diagnosis *prior* to their physician's first detailing visit; detailing itself may influence a physician's likelihood to diagnose a patient with chronic pain, introducing endogeneity into the capture of the "opioid-appropriate" samples we seek to identify.

Additional details on constructing the patient-level sample are provided in Appendix E.

3.3 Empirical Approach

Our empirical goal is to identify the causal relationship between Purdue pharmaceutical representative's detailing activities and opioid prescribing and use. To do this, we use a two-way fixed effects model allowing for dynamic treatment effects that vary over time. Analyses are at the physician-quarter level for physician-level assessments, and at the beneficiary-quarter level for beneficiary-level assessments.

Our main estimating equation for the physician-level analysis takes the form of an event study in which the event of interest is a provider's first visit with a Purdue representative occurring at time r. We define outcome y associated with provider p in calendar time t as:

$$y_{pt} = \sum_{t'=-6, t' \neq -1}^{9} \beta_{t'} \mathbb{1}[t - r = t'] \times \tau_p + \sum_{t'=-6, t' \neq -1}^{9} \gamma_{t'} \mathbb{1}[t - r = t'] + \delta_t + \delta_p + \varepsilon_{pt} \tag{1}$$

where τ_p equals 1 for physicians whose detailing volume in the first year is above-median and 0 otherwise; δ_t and δ_p are calendar year-quarter and physician fixed effects, respectively; and ε_{pt} is an idiosyncratic error term. The event study period includes 6 quarters in both the pre- and post-periods; relative quarters 0 through 3 are used in defining physician detailing volume, and are considered to be separate from the post-period.

Our event study compares two treated groups with different degrees of exposure. This is motivated

by the fact that physicians who are detailed may be different in important ways that are relevant to our empirical setting as compared to physicians who are never detailed. Due to the non-standard event study form we use, we pause here to build intuition around the interpretation of the estimated effects generated by this specification. Specifically, the three cohorts of physicians in our study—never-treated, below-median-exposure treated, and above-median-exposure treated—inform the estimates of our coefficients in three different ways. As noted above, with no relative time fixed effects, the never-treated cohort informs the estimates of δ_t, the "secular trends" in opioid prescribing. This in turn informs the estimates of the relative time terms, i.e. the βs and γs. The γs can be understood as describing the average treatment effect of receiving an average exposure of detailing visits on y. The βs can be interpreted as the additional effect of receiving above-median exposure.

Given this non-standard event study form, there are two ways to interpret the βs. The most straightforward interpretation is simply that β represents the change in y relative to the quarter before the first detailing visit, among physicians with above-median exposure relative to physicians with below-median exposure. The second interpretation is less straightforward, but potentially informative as a "dose-response" translation. Specifically, we can scale the coefficients by the difference between the above-median annual detailing volume (μ_d^H) and the overall annual detailing volume (μ_d), where $\mu_d^H - \mu_d = \Delta_d$. The rescaled effects can be interpreted as the impact of a one-visit increase in detailing exposure on y. One important caveat here is that this second approach implies a dose-response relationship that is linear; we will empirically test whether this assumption (and thus, interpretation) holds.

Appendix G provides additional details on this empirical specification. We also build intuition around the interpretation of the empirical form by viewing it through the lens of recent developments in difference-in-differences with continuous treatment Callaway et al. (2024a,b).[22] Additionally, we include a discussion of the assumptions required for the interpretation of our results as causal.

Our analytic sample is constructed with balance for treated units across the pre- and post-period.[23] The estimating equation for the beneficiary-level analyses is analogous to Equation 1, where outcomes are the at the patient-time level, i and t respectively.

[22] ***Not presented in this draft.***

[23] We allow for ever-treated physicians to have treated no Medicare beneficiaries in quarters within this range (and we fill in outcome values with 0 in this instance); the restriction is met when a provider's first and last observed treatment date meet these requirements.

3.4 Outcomes of Interest

We estimate our model for two broad classes of outcomes: (i) opioid prescriptions; and (ii) patient harms. Changes in volume of opioids prescribed (physician-level) or probability of filling an opioid prescription (beneficiary-level) act as a "first stage" estimation of the effect of detailing. The "reduced form" relationship we estimate between detailing and associated adverse health events provides a measure of the downstream effects of detailing on patient welfare. We seek to demonstrate that any changes that occur do so through the change in opioid prescribing. All outcomes are defined at the level of analysis (i.e., physician-quarter or beneficiary-quarter).

Opioid Prescriptions. *A priori*, the predicted impact of detailing visits on physicians' prescription of opioids is relatively intuitive: if detailing visits from Purdue induce providers to prescribe more opioids manufactured by Purdue, then we would expect to observe an increase in the total number of Purdue-manufactured opioids prescribed. We construct a measure of "Purdue opioids" using a set of generic names associated with Purdue-brand opioids.[24] This approach is particularly relevant for our empirical setting: Massachusetts is a "mandatory [generic] substitution" state in which generic drugs *must* be substituted for brand name drugs when available (unless the doctor indicates otherwise). For example, it's possible that a provider would prescribe a Purdue drug (Oxycontin), but not specify that a brand name be given, resulting in the patient receiving the generic version (Oxycodone ER).[25] This approach does not however guarantee that the drug dispensed was a generic drug, as **all** PDE records include a generic name.[26]

At the physician-level, detailing may induce an increase in Purdue opioid prescriptions through the extensive margin, in which providers expand their patient panel and prescribe more opioids to this expanded panel; and/or through the intensive margin, in which providers increase the share of patients for whom they prescribe opioids. We may also capture provider response to detailing visits via a change in the Morphine Milligram Equivalent (MME) associated with their prescriptions (MME is a standardized measure of the strength of a prescription).

[24] Specifically, we follow Neprash et al. (2021) and flag any drugs with generic names that include the following: butorphanol, codeine, dihydrocodeine, fentanyl, hydrocodone, hydromorphone, levorphanol, meperidine, methadone, morphine, opium, oxycodone, oxymorphone, pentazocine, propoxyphene, tapentadol, and tramadol.

[25] *In subsequent drafts, we will explore prescriptions with an indicator that the physician requested the drug be "dispensed as written," i.e. as a brand name product and not the generic equivalent.*

[26] *Prior research has shown it is difficult to identify brand-name prescriptions in the PDE files (**CITE**). In future drafts, we will use the set of National Drug Codes (NDC) associated with Purdue opioids to check the robustness of our results against the generic-name approach.*

To explore these dimensions, we construct several extensive- and intensive-margin measures of Purdue opioid prescriptions. Our primary extensive-margin measure is the total number of Purdue opioid prescriptions filled by patients attributed to a given physician in that quarter. We capture the total intensity of the prescriptions filled using log-transformed MME,[27] as well as the number of patients attributed to a provider with at least one Purdue opioid prescription in a given quarter. On the intensive margin, we create measures of the number of Purdue opioid prescriptions per patient, total MME per patient (log-transformed), and the share of patients with at least on Purdue opioid prescription).

Similarly, at the beneficiary-level, detailing of one's key provider may impact an individual's opioid use versus the extensive margin—a change in the likelihood of filling an opioid prescription—or the intensive margin—a change in (log) MME of existing prescriptions. To capture the extensive margin effect, we construct a binary variable indicating that a beneficiary filled a Purdue opioid prescription in a given quarter.

Adverse Health Events. The predicted impact of detailing on patient health is more theoretically ambiguous. On the one hand, opioids have been shown to be an effective pain management tool for patients with acute and/or cancer-related pain, though evidence of their effectiveness in managing chronic non-cancer-related pain is much less compelling (Maclean et al., 2022; Van Zee, 2009). Thus, opioid prescriptions to manage pain may result in fewer pain-specific visits to healthcare providers in the outpatient, ED, or inpatient setting. On the other hand, opioids are associated with several adverse side effects, including addiction and even death. This may manifest in claims data as treatment for opioid use disorder (OUD), opioid-related ED or inpatient visits.

To capture the reduced-form relationship between detailing visits and patient health outcomes, we construct two composite measures of adverse health events that have been linked to opioid use and/or abuse in prior literature. First, we flag any claim that originated in the emergency department (ED)[28] or inpatient[29] setting with a diagnosis code associated with a non-fatal opioid overdose or injection drug-use related infection based on the set of codes used in Barnett et al. (2023) (additional details given in Appendix F). Second, we flag any ED or inpatient claim associated with a fall or fracture (Barnett et al., 2017). Prior literature has demonstrated an increased risk of falls and fractures specifically among

[27] We took the log transformation (plus 1 to accommodate 0s) of MME due to the long right tail in the distribution of total MME per quarter; see Figure A6

[28] We identified emergency department claims using the revenue code associated with a record in the outpatient files. Additional details (including the values of the revenue codes we used) can be found in Appendix F.

[29] We used the Medicare PRovider Analysis and Reivew (MedPAR) files to capture inpatient utilization

elderly patients who use opioids (Solomon et al., 2010; Yoshikawa et al., 2020).

We also construct a flag for opioid use disorder (OUD). While OUD can be flagged using diagnosis codes, the diagnosis itself is relatively subjective,[30] raising endogeneity concerns. Specifically, a provider's willingness to prescribe opioids may be correlated with their propensity to diagnose OUD. As an alternative, we define OUD based on opioid utilization patterns proposed in other studies. First, we flag patients with a daily MME of (i) 90 mg or greater; or (ii) 120 mg or greater in any given quarter (Finkelstein et al., 2022; Meara et al., 2016). Second, we flag patients with overlapping opioid prescriptions, i.e. with at least one opioid prescription that starts before a prior prescription's days supply ends (**CITE**). Note that these measures are based on all opioid prescriptions, not just prescriptions for Purdue opioids. Specific codes for each of these measures are given in Appendix Table **XX**.

Patient Characteristics and Balance Tests. In the physician-level analysis, it's possible that patient composition changes after a physician is detailed. This may be motivated in part by the detailing itself, *i.e.,* if a physician is more open to treating patients with characteristics associated with opioid use. **DEVELOP**. We test for this explicitly by estimating our main event study for the following patient characteristic outcomes constructed at the physician-quarter level: patient volume (total patients), patient demographics (male patients, White patients, patients under age 65, patients aged 65–74, patients aged 75–84, and patients older than 85), enrollment characteristics (patients dually enrolled in Medicare and Medicaid as well as SSDI recipients), and clinical characteristics (patients with cancer and patients with any chronic condition).

3.5 Threats to Identification and Sources of Bias

There are two key features of the empirical setting that raise concerns around our identification strategy. First, it's unlikely that providers were randomly targeted by pharmaceutical representatives. Prior investigations into marketing plans for OxyContin revealed that pharmaceutical representatives identified and targeted providers who prescribed the largest volume of opioids nationally (Van Zee, 2009). This approach is echoed in the Purdue documents released by the lawsuit. Other studies of marketing and provider prescribing behavior have recognized the selection concerns in this empirical setting (which

[30] OUD diagnosis is made on the basis of a "checklist" of diagnostic criteria from DSM-5, and includes flags that the patient is "taking opioids in larger amounts or over a longer period of time than intended" to the patient is "exhibiting withdrawal symptoms."Additional details can be found here: https://www.cdc.gov/overdose-prevention/hcp/clinical-care/opioid-use-disorder-diagnosis.html.

span broadly across a range of pharmaceutical and medical device products) as an empirical challenge, and have identified alternative empirical approaches (namely, instrumental variables and two-stage least squares) to circumvent issues that arise from such selection (Grennan et al., 2024; Larkin et al., 2017).[31]

Our empirical strategy accounts for differences in non-time-variant levels in opioid prescribing via physicians fixed effects. However, if pharmaceutical representatives targeted physicians as these clinicians were increasingly prescribing more opioids over time, we would not be able to differentiate between any increasing trends in opioid prescriptions that are due to detailing visits versus a physician's own prescribing behavior. We address these concerns in two ways: First, any systematic increases in opioid prescriptions prior to the first detailing visit will be visible as violations of the parallel trends assumption; we test for this empirically.[32] Second, our comparison leverages the intensive (number of detailing visits) instead of the extensive (any detailing visit) margin of detailing exposure. Given the substantial evidence that pharmaceutical representatives did employ strategic targeting of physicians with particular ex-ante prescribing behaviors, we include never-detailed physicians only to inform calendar trends in opioid prescriptions during this time.

The second key feature is that we can only observe detailing visits, and not any engagements that providers may have had with Purdue Pharma outside of these recorded visits. Thus, while a provider's first detailing visit is the event we leverage in our empirical design, it's possible that providers had prior engagement with the pharmaceutical company. This, in turn, may explain any pre-visit trends in opioid prescriptions. In forthcoming drafts, we will use other available data sources (such as industry payments recorded in the publicly available CMS database Open Payments[33]) to evaluate the extent to which interactions between physicians and the pharmaceutical company might be captured through other settings, such as conferences or meals, that are not recorded in the detailing records.

We also consider any bias that may be introduced by attributing patients' opioid prescriptions records to providers who may not have prescribed them. **CLASSICAL MEASUREMENT ERROR → ATTENUATING BIAS?** As noted above, prescription records after 2013 contain the NPI's of the prescribing physicians; using these years, we determine the match rate between our physician attribution

[31] Notably, Grennan et al. (2024) have a different empirical goal of evaluating the impact of policies that restrict or eliminate industry payments to physicians. The authors note this as a meaningful difference to motivate a different two-stage least squares strategy instead of relying on the event study.

[32] Other marketing-to-physician studies have used the same empirical checks to defend an event study approach (Agha and Zeltzer, 2022; Carey et al., 2020; Shapiro, 2018).

[33] Available: https://openpaymentsdata.cms.gov/.

approach and the physician on the patient's prescription record.

4 Purdue Opioid Prescribing and Fills

4.1 Physician-Level Results

The physician-level analysis seeks to answer the question, "how does pharmaceutical detailing impact a physician's prescribing behaviors?" by comparing changes in outcomes, such as volume of Purdue opioid prescriptions, among physicians with above-median detailing exposure to physiciasn with below-median exposure, where the event of interest is the first detailing visit a physician receives from a Purdue Pharma representative.

4.1.1 Descriptive Statistics

Table 1 reports characteristics of ever- and never-detailed physicians and the patients they treat. Time-varying measures (such as total patients per quarter) are calculated for ever-detailed physicians based on the four quarters prior to their first detailing visit, and are calculated across all quarters for never-detailed providers. Our final analytical sample includes **5,184** never-detailed and **1,912** ever-detailed physicians, among which **1,070** have above- and **84** have below-median detailing exposure, respectively.

By design, we construct a sample of never-detailed providers who are more similar to ever-detailed providers than the full, unmatched population of never-detailed providers. Nevertheless, some differences between treatment classes remain. Providers who were ever-detailed are more likely to be men (**69**%) than providers who are never-detailed (**63**%), and the likelihood of being a man increases across detailing volume, with **71**% of above-median and **67**% of below-median providers being men.

Relative frequency of specialties are similar across treatment classes (by design). The most common specialty for all provider types is internal medicine, associated with over half of all physicians. Other common specialties include family medicine, orthopaedic surgery, anesthesiology, and physical medicine & rehabilitation. These specialties describe over 80% of both ever- and never-detailed providers.

In terms of patient volume, physicians with above-median exposure tended to see slightly more patients per quarter (**27**, SD: **20**) than below-median physicians (**23**, SD: **18**), and physicians who were ever detailed treated substantially more patients per quarter (**25**, SD: **19**) than providers who were never detailed (**16**, SD: **15**), even after imposing a match based on this characteristic. Notably, these patients

can be "shared" across physicians (*i.e.*, a beneficiary may have an E&M visit with physicians in a given quarter); total "exclusive" patients refers to the number of beneficiaries a physician treats exclusively in a given quarter. The volume gradient is in the same direction; above-median physicians treat on average more patients exclusively per quarter (mean: **9**; SD: **9**) than below-median physicians (mean **7**; SD: **7**). Similarly, ever-detailed physicians treat more patients exclusively (mean: **8**; SD: **8**) than never-detailed physicians (mean: **3**; SD: **4**). All told, patient volume appears to be correlated with detailing exposure, and may reflect part of the strategic targeting discussed in the Purdue documents.

Pre-detailing Purdue-opioid prescription volume is similar across detailed physicians; these physicians prescribed between **6** and **7** Purdue opioids per quarter, with total prescription strength being slightly higher in the above-median cohort (**169** MME) than in the below-median cohort (**164**). Physicians in the never-detailed sample tended to prescribe fewer Purdue opioids (**4.45**, SD: **5.04**) with lower total intensity (**119** MME, SD: **187**). We also compare pre-detailing patterns in general (including non-Purdue) opioid prescriptions, finding that ever-detailed physicians were prescribing more opioids in general per quarter (**9**–**11**) relative to never-detailed physicians (approximately **6**), with a similar gradient for total strength associated with all prescriptions (approximately **183** MME for ever-detailed physicians, and **135** MME for never-detailed physicians).

Finally, the first-year detailing intensity for above-median physicians was approximately **4.3** (SD: **2.3**) visits, and **1.0** visit (SD: **0.0**) for below-median physicians. Overall first-year detailing intensity across all in-sample physicians who were ever detailed was **2.8** (SD: **2.4**). In the interpretation of our results (as noted above), we take the difference between above-median (**4.3**) and overall (**1.8**) detailing intensity to scale the coefficients, standardizing them by a one-visit increase. The value of this difference is **1.5**; we will reference this in our discussion of the event studies.

Additionally, to explore whether above-median exposed physicians remain detailed at a high volume over time, relative to physicians with below-median exposure, Figure A31 plots trends in detailing visits by relative (panel a) and calendar quarter (panel b), for above- and below-median detailed physicians. In both plots, physicians who received an above-median volume of detailing visits in the year after the first detailing visit receive, on average, more detailing visits in subsequent quarters than below-median physicians. **IMPLICATIONS.** This suggests that our measure of detailing exposure is a good proxy for subsequent detailing exposure. We will test the robustness of our results to ongoing exposure versus short-term exposure in future drafts.

As an intuitive check of whether detailing visits have any impact on opioid prescription volume, Figure 1 plots unadjusted trends in mean Purdue-manufactured opioid prescriptions per quarter by physician detailing cohort, with never-detailed physicians assigned a placebo event time. Visually, it appears that trends in opioid prescriptions among physicians of all detailing types were more or less in parallel in the pre-period, with some indication of increasing trends among the ever-detailed cohorts relative to the never-detailed cohort. Following a physician's first detailing visit, and particularly in the second half of the post-period, there is a visual suggestion that opioid prescriptions among above-median detailed physicians increase more than the increase in below-median detailed and never-detailed physicians. We next formally test whether these apparent differences in trends are statistically meaningful, and whether there are any significant pre-trends that would violate the parallel trends assumption.

4.1.2 Provider Prescribing Behavior

Figure 2 plots the estimated coefficients of interest (βs) obtained from estimating Equation 1 on the number of Purdue opioids attributed to a physician each quarter, our main first stage outcome. The estimated effects in the pre-period are relatively flat and not significantly different from zero, supporting a parallel trends assumption and suggesting that effects can be interpreted as causal. The grayed out period (relative quarters 0–3) represent the time period during which we define detailing intensity; the gray is a visual cue that the relevant post-period begins in relative quarter 4. In the post-period, we observe a relative increase in total Purdue opioid prescriptions after a provider's first detailing visit, suggesting that moving from a below-median volume of detailing visits to an above-median volume increases the number of Purdue opioids a physician prescribes. This effect increases over time, from a non-significant increase of **0.394** prescriptions in event time 4, to a highly significant **0.993**-prescription increase in event time 8, before declining slightly (to **0.560** additional prescriptions) and becoming insignificant in the final relative quarter.

Re-scaling these coefficients by Δ_d (1.5) and dividing by the pre-period mean for above-median physicians, these coefficients translate to a **4**%, **10**%, and **6**% relative increase in prescriptions per quarter, respectively, for every additional detailing visit a physician receives in a given year. Note that this interpretation assumes a linear relationship between detailing visits and the outcome; we examine the linear nature of the dose-response relationship more explicitly shortly.

Figure 3 plots estimated effects for other extensive margin measures. Similar to our main results, we

observe a relative increase in both log MME and the number of a physician's attributed patients with a Purdue opioid prescription, though these effects are less precisely estimated. The imprecision of these estimates may be due to relatively small sample size; to increase power and obtain one summary effect size, we next turn to a pooled version of these results.

Panel A of Table 3 summarizes the extensive margin impacts as one post-detailing-visit average effect per quarter. In this estimation, we pool the individual pre-period quarters (event time -6 through -1) and post-period quarters (event time 4 through 9) together into one pre- and post-period, omitting event time 0–3 as part of the detailing-intensity definition period. Column (1) shows that, relative to below-median detailed physicians, total Purdue opioid prescriptions associated with ever-detailed providers increased by an average of **0.595** additional prescriptions per quarter in the post-period. Rescaling implies that one additional detailing visit in the first year post-first visit increases the volume of Purdue opioids by **0.397** or, based on a pre-treated mean of **6.66** prescriptions per quarter, by an additional **6.0**% in prescriptions. Put differently, we estimate that a one-standard deviation increase in detailing visits in the first year post-detailing—an additional **2.4** visits—would result in a **14**% relative increase in Purdue opioids prescribed.

Relative to below-median detailed physicians, we estimate a **15%** increase in the total MME associated with a physician's prescriptions in a given quarter in the post-period (column 2); however, as in the event study results, this is not significant. We find a marginally significant average relative increase of 0.155-patients with a Purdue opioid prescription associated with a one-visit increase in detailing visits, or a 6.5% relative increase based on the pre-detailing mean of 2.39 patients with such a prescription (column 3).

Taken together, these extensive margin results suggest that additional detailing visits increases physicians' prescription of the target drug, though not necessarily the strength of these prescriptions. As noted previously, detailed physicians might increase their prescription volume by increasing the number of patients they treat, or by increasing the number of opioids they prescribe per patient, holding fixed the number of patients in their panel. Both of these scenarios would lead us to estimate the significant effects we observe in columns (1) and (2). To gain further insight on which scenario is more relevant in our setting, we next turn to our intensive margin outcomes.

If additional detailing visits lead physicians to prescribe more opioids to their existing patient panel, this should be reflected as significant (relative) increases in the number of opioid prescriptions per patient

and/or in the share of patients with an opioid prescription in the post-period. Columns (4) and (6) of Panel B (Table 3) suggest that this scenario does not explain the increase in overall opioid prescription volume observed in Panel A. Instead, we observe a small and not significant 0.008-prescription increase per patient (2.6% on the pre-treatment mean of 0.311), and a small and not significant 0.004 increase (3.8% on the pre-treatment mean of 0.105) in the share of patients with an opioid prescription. The strength (in MME) of prescriptions increases by approximately 9.1% per patient. Figure A7 supports the causal interpretation of these results with pre-trends that appear relatively flat and not significantly different from 0. The figures also illustrate that effects of additional detailing visits on the intensive margin are homogeneously non-significant over time, with a slight increase in log MME in the post-period indicated by panel (b).

Alternatively, detailing visits may induce physicians to treat more patients with greater demand (or clinical relevance) for opioids, leading to the observed increase in opioid prescription volume. We will test this shortly in Section 4.1.5.

4.1.3 Dose-Response

To gain more intuition on the "dose-response" nature of detailing visits and Purdue opioid prescriptions, we next estimate our model on separate cuts of the above-median detailed cohort. Specifically, we split the above-median sample into terciles by detailing volume in the first year, and estimate the model on each tercile separately (still comparing to below-median detailing-volume physicians). Figure 4 plots the coefficients estimated for each cohort, with tercile detailing volume and physician count given on the y-axis, and average treatment effect (ATE) on the x-axis. Percent change relative to tercile-specific pre-treatment mean is reported in text above the ATE marker.

The relatively small sample sizes in each bin limits the extent to which we can estimate these results with precision. However, even without imprecision, it appears that at least for the bottom and middle terciles, the effect increases in the expected direction: physicians with more detailing visits tend to prescribe more opioids. Specifically, physicians with 2 visits per year (the bottom tercile) tend to increase the volume of their opioid prescriptions by an average **7.6**% relative to a pre-treatment mean of **5.83** prescriptions per quarter, while physicians with 3-4 detailing visits per year tend to increase opioid prescription volume by **12.2**% relative to a pre-treatment mean of **6.85** prescriptiosn per quarter.

Interestingly, we observe the smallest relative increase in Purdue opioid prescriptions among physicians in

the top tercile of above-median visits (5-10 visits a year). Among these physicians, opioid prescriptions increase by **7.2**%, relative to a pre-treatment mean of **7.17**. Notably, while the dose-response isn't necessarily linear, the relationship between detailing tercile and pre-mean opioid prescriptions is. This pattern points to the selection associated with detailing: Purdue's marketing strategy was to target high-prescribers in an effort to get them to prescribe more. Notably, Figure 4 suggests that this strategy may have benefited from additional nuance: the largest "return on investment" in terms of detailing came from mid-tier opioid prescribers (i.e., those in the middle tercile), introducing a "flat of the curve" possibility for detailing. Specifically, it may be more efficient from the perspective of the pharmaceutical company to target not the top or bottom prescribers, but those in the middle.

4.1.4 Heterogeneity by Physician Characteristics

Detailing visits may impact physician prescribing behavior differently based on physician characteristics, particularly to the extent that Purdue was targeting particular types of physicians. For example, Purdue specifically targeted primary care providers in their marketing efforts (Van Zee, 2009); thus, evaluating whether providers with a specialty commonly associated with primary care (namely, internal medicine and family medicine) responded differently to detailing visits as compared to physicians with non-primary care specialties is informative as to whether these specific targeting efforts were uniquely successful. We examine this in the context of our main outcome (total Purdue opioid prescriptions per quarter).

Detailing has a similar effect across physician gender (Figure A8, panels a and b), though the smaller sample sizes result in less precisely-estimated effects. The response among non-PCPs is similar in trend to the response among PCPs (panels c and d), with a larger (though less precisely estimated) response among non-PCPs. Finally, despite growing evidence of the harms of opioids and burgeoning epidemic of opioid misuse, the response to detailing appeared similar regardless of whether the physician was detailed in the first part of our study period (2007–2011) or in our last (2012–2016) (panels e and f).[34]

4.1.5 Balance Checks

In the presentation of our main results, we described two scenarios that might explain the observed increase in prescription (Purdue) opioid volume. First, it's possible that physicians alter their treatment

[34]Because our empirical strategy requires physicians to be observed for 9 quarters after the first detailing visit, the latest year in which a first detailing visit can occur is 2016.

of existing patients; Panel B of Table 3 suggests that this is not the case. Second, it's possible that physicians change the patients they treat in response to detailing. We explore this further in Figures A9–A12. Specifically, we estimate Equation 1 for patient characteristics to assess balance in patient panel.

In general, the volume of patients a physician treats is relatively unchanged across the event time horizon (Figure A9, panel a). Total male paitents (panel b) and total white patients (panel c) remain relatively unchanged. There is a slight suggestion of an increase in the volume of younger patients (Figure A10, panel a) and a decrease in older patients (panel b), but these effects are not significant. Additionally, there is an insignificant decline in the number of dually-enrolled patients (Figure A11, panel a) a physician treats, but no change in the volume of SSDI recipients.

Overall, it does not appear as though physicians meaningfully change the volume or composition of the patients they treat. We caveat this interpretation with two considerations: First, we only test for balance across a select set of observable characteristics; there may be other observable or unobservable characteristics that change around detailing visits. Second, the non-significant change in the volume of patients overall is somewhat puzzling. Specifically, the increase in opioid prescription volume and nonsignificant change in opioids per patient would lead us to anticipate an increase in patient volume to explain the increased opioid prescription volume. Instead, it's possible that our attribution method introduces a degree of imprecision to the estimates that masks some of the per-patient effects. To address this and other concerns, we next explore an empirical setting in which beneficiary characteristics are (more or less) held constant: within-beneficiary changes in opioid use.

4.2 Beneficiary-Level Results

The beneficiary-level analysis seeks to answer the question, "how does one's physician being detailed impact one's own drug access?" by comparing within-beneficiary changes in outcomes among beneficiaries who are treated by physicians with above-median detailing intensity to below-median detailing intensity, where the event of interest is the first detailing event.

4.2.1 Descriptive Statistics

Table 4 presents characteristics of the beneficiaries and their key providers in the beneficiary-level analyses. Panel A reports beneficiary characteristics. Our final sample includes **43,803** never-treated beneficiaries and **14,601** ever-treated beneficiaries, of which **8,989** have key providers whose first year

detailing intensity was above-median, and **5,612** have key providers with a below-median detailing intensity. Beneficiaries are very similar across detailing cohorts. The majority of beneficiaries in this sample are female, with ever-treated beneficiaries equally as likely to be male as never-treated (**37%**). Ever-treated beneficiaries are also approximately the same age (mean age: **68**, SD: **15**) as never-treated beneficiaries. The vast majority (approximately **90**%) of beneficiaries are White, with similar compositions of Black (**3-5**%), Hispanic (**2**%), and Asian (**1-2**%) across all detailed cohorts.

Beneficiaries remain similar on dimensions related to sub-populations. Nearly a quarter of beneficiaries are SSDI recipients, and nearly all (**99**%) have a chronic pain diagnosis at some point in the study. Notably, approximately **90**% of ever-treated beneficiaries have a chronic pain diagnosis before their key provider's first detailing visit.[35]

Among ever-treated beneficiaries, approximately 59% have a record of an opioid prescription fill from any manufacturer (not just Purdue) in the pre-period. Slightly less (**43**–**45**%) have a record of a Purdue opioid prescription fill in the pre-period. There are no substantial differences in opioid fills between beneficiaries treated by above-median and below-median detailed key providers. Of never-detailed beneficiaries, **67**% and **44**% have a prescription fill record for any and Purdue-specific opioids during the study period. Notably, the much higher share of never-detailed beneficiaries with an opioid prescription relative to ever-detailed beneficiaries is almost certainly due to mechanical reasons; namely, never-treated beneficiaries are observed for an average of **34** quarters (SD: **13**) in our study period, and opioid use is reported across this entire period. The ever-treated beneficiaries are observed for only 16 quarters, and opioid use is based on observations in the 6-quarter pre-period.

Panel B of Table 4 describes characteristics of the 1,723 key providers treating the ever-treated beneficiaries, 980 of whom have above-median detailing volume in the year post-first visit, and 743 of whom have below-median detailing volume. On average, beneficiaries have approximately **XX** E&M visits with their key providers in the pre-period, and key providers are responsible for over **60**% of all E&M visits in the pre-period. The majority (89%) of beneficiaries have a key provider with a primary care provider specialty, with a slightly higher percent (91%) among beneficiaries whose key provider was above-median in detailing volume than beneficiaries with a below-median key provider (87%). On average, beneficiaries are treated by key providers with 3 (SD: 3) detailing visits in the year after first

[35]Unless chronic pain is in fact nearly universal among Medicare beneficiaries in Massachusetts, or our physician sample is selected along this dimension, this suggests that the set of diagnosis codes identified by the Chronic Condition Warehouse includes codes that are so ubiquitous that they may not include valuable signal for capturing chronic pain. **FINISH**

detailing visit. By design, the key providers of beneficiaries with above-median detailing key providers receive 5 detailing visits (SD: 2) on average in the year after first detailing visit, while the key providers of beneficiaries with below-median key providers have 1 visit in the first year on average.

4.2.2 Probability of Purdue Opioid Use

Figure 5 plots coefficients obtained from estimating Equation 1 for three populations: the full sample of all beneficiaries (panel a); the subset of beneficiaries with a chronic pain diagnosis prior to the first detailing visit (panel b); and the subset of beneficiaries who are SSDI recipients (panel c). In all populations, estimated effects in the pre-period are positive though not significantly different from zero.[36] As in the physician-level analyses, estimated effects in the post-period are dynamic: for the full beneficiary sample, there is a non-significant relative increase of slightly less than **1** percentage point in the probability of a Purdue opioid prescription fill for beneficiaries with above-median key providers, relative to those with below-median key providers, in event time 4. By event time 6, the probability of filling a Purdue opioid prescription is nearly **2** percentage points higher and strongly significant, declining again to just under a **1** percentage point increase by the end of the even time horizon. Similar patterns occur for beneficiaries with a pre-existing chronic pain diagnosis, as well as those who receive SSDI.

Table 5 summarizes these effects in a difference-in-differences (pooled pre- and post-period) framework as described above, for both the probability of filling a Purdue opioid prescription as well as the strength of Purdue opioid prescriptions a beneficiary fills in a given quarter in the post-period. Relative to beneficiaries whose key provider has below-median detailing intensity, beneficiaries whose key provider has an above-median detailing intensity experience a small but significant relative increase in the probability of filling a Purdue opioid prescription of approximately **0.6** percentage points per every additional detailing visit in the second part of the post period. Relative to a pre-period mean of 11.2%, this represents an increased likelihood of approximately 5.4% per additional detailing visit a key provider receives. Patients with pre-existing chronic pain similarly experience a 0.7 percentage point (5.7%) relative increase in the likelihood of filling a prescription in the post-period; this effect is similar in magnitude (1.2 percentage

[36]The persistently positive coefficients in the pre-period relative to event time -1 is puzzling. In robustness checks (Figure A22), we re-estimate our event study omitting event time -2 to determine whether this issue is mechanical with regards to our event study specification, or a feature of the data. We find that the coefficient associated with event time -1 is negative (though not significant) relative to event time -2. While the estimated effects in the post-period remain positive, they are not significant, though pooling pre- and post-periods returns significance, as we discuss later. We will explore this anomaly further in future drafts.

points; 5.4%) for the SSDI population, but not significant across the pooled post-period. We observe no significant impact on the strength of the prescription, as represented by MME, for either the full sample or populations of interest.

Figure A13 plots the results of estimating the model on the three terciles of detailing visit intensity represented in the above-median key provider cohort (as with the dose-response exercise above). We observe a similar non-linear dose-response effect that is similar to that estimated in the physician-level analysis: the smallest (a non-significant **2.2**% increase, relative to a pre-treatment mean of **11**%) change in likelihood of filling a Purdue opioid prescription occurs among beneficiaries whose key providers are in the bottom tercile (2 detailing visits in the first year). Beneficiaries whose key providers are in the middle tercile (3-4 detailing visits in the first year) experience an increased likelihood of approximately **7.2**% relative to a pre-treatment mean of **10**%, while beneficiaries with key providers in the top tercile (5-10 visits per year) experience an increased likelihood of approximately **6.1**% relative to a pre-treatment mean of **12**%. Notably, the pre-treatment means are much more similar than in the physician-level analysis above, suggesting substantially less sorting of patients to physicians in terms of pre-treatment opioid use.

4.2.3 Heterogeneity by Patient and Key Provider Characteristics

UNDER CONSTRUCTION

5 Health Outcomes

Assuming the beneficiary did not experience a contemporaneous health shock that made them more clinically appropriate for opioid treatment, the increased probability of filling a Purdue opioid prescription while holding all else constant—observable and unobservable—about a beneficiary suggests that detailing visits increase opioid prescriptions through updating physicians' perceptions about their clinical appropriateness. However, it is not yet clear whether these additional prescriptions are welfare improving or welfare reducing. To explore this, we next estimate our model on a set of indicators for adverse health events related to opioid use.

To assess the impact of detailing on patient welfare, we first look at within-patient changes in utilization-based indicators for health harms. Figures A14 and A15 plot the coefficients obtained from estimating Equation 1 at the beneficiary level for outcomes related to opioid misuse and opioid-related harms.

Figure A14 shows little to no change in the probability that a beneficiary has at least one day in a given quarter that includes total MME greater than 90 (panel a), total MME greater than 120 (panel b), or overlapping opioid prescriptions (panel c). If anything, the likelihood of having at least one day with an MME above a critical threshold appeared to decline for patients with above-median detailed key providers following the first year of detailing visits. **DEVELOP**. Similarly, Figure A15 does not suggest that there was any relative change in the likelihood of a hospitalization or ED visit due to an overdose or infection (panel a) or a fall or fracture (panel b) for patients with above-median detailed key providers. Table 6 summarizes these effects in our pooled pre- and post-treatment model, and confirms that in the aggregate, detailing does not have any significant impact on the opioid-related adverse health of interest.

In contrast, Table 7 reports a highly significant increase in the number of patients that are hospitalized or have an ED visit for a fall or fracture among physicians with above-median detailing volume. Specifically, we observe a relative increase of 0.039 (25% on the pre-treatment mean of 0.156) patients with a fall or fracture among above-median detailed physicians in the post-detailing period, relative to below-median detailed physicians in the post-detailing period. Panel (b) of Figure A16 shows a similar pattern: following a pre-period with estimated effects that are not significantly different from 0, we see an overall increase in magnitude in the number of patients with a fall or fracture in event times 4–9. While these estimated effects are not significant, they are consistently positive; pooling the pre- and post-period quarters into one pre- and post-period allows us to detect the significant effect. [***PLACEHOLDER FOR MME90, MME120, OVERLAPPING RX***]

Motivated by this finding, and by the heterogeneity previously observed in the "dose-response" exercises, Figure 6 reports estimated effects by detailing-volume tercile for physician-level (panel a) and beneficiary-level (panel b) analyses. In both, estimated effects are largest and most significant in the top tercile in which physicians had 5-10 detailing visits in the first year. Specifically, the number of patients with a fall or fracture event increased by **33.9**% (on a pre-treatment mean of **0.156**), for above-median physicians, relative to the below-median physicians. Meanwhile, the likelihood of experiencing a fall or fracture increased by **44.9**% (on a pre-treatment mean of **0.5%**) among beneficiaries with top-tercile detailed key providers, relative to beneficiaries with below-median key providers.

To develop intuition in the pathway between detailing exposure and patient outcomes that generates these results, we consider two mechanisms. First, detailing exposure may induce physicians to increase

the strength/dosage of existing prescriptions, leading to an increased risk of falls among patients with pre-existing opioid use. We test this using our dose-response exercise, focusing on the estimated (relative) effect for patients with physicians with top tercile exposure, given their strongest increased risk of fractures and falls. Figure A17 plots the estimated effect of detailing exposure on (log) MME per patient, at the physician level, and log MME of prescription, at the beneficiary level, by tercile of exposure. Focusing on the top tercile, these results do not suggest that an increase in the strength of the prescription is driving the increased risk of falls and fractures.

Second, detailing exposure may induce a physician to prescribe opioids to the marginal patient who did not previously have a prescription. We can test whether this mechanism is driving our results by revisiting Figure A13. Pre-existing patients of top-tercile physicians experience a precisely-estimated increase in the likelihood of filling a Purdue opioid prescription following their key provider's high degree of detailing exposure. Notably, so do patients of middle-tercile key providers; in future drafts, we will more closely evaluate observable differences between these patient population that might explain why an increase in opioid use led to an increased risk of falls and fractures in one patient population, and not the other.

6 Robustness Checks

We test the robustness of our results to a variety of different considerations. First, we consider an alternative comparison to the above- and below-median comparison we use in our main approach. We estimate Equation 1 at both the physician- and beneficiary-level for two additional comparisons: (i) above-median detailed physicians (beneficiaries) versus never-detailed physicians (beneficiaries); and (ii) ever-detailed physicians (beneficiaries) versus never-detailed physicians (beneficiaries). Figures A18 and A19 plot the resulting estimated effects for physician- and beneficiary-level analyses, respectively. In all plots, the comparison to the never-detailed physicians (beneficiaries) results in event studies that violate assumptions necessary for a causal interpretation (Figure A18), and/or generates estimated effects that do not support an increase in Purdue opioid prescriptions (Figure A19).

Together, these event studies provide support to concerns around selection; namely, and as indicated by the marketing documents, physicians targeted by detailers were likely to be different on important dimensions related to opioid prescriptions than physicians not targeted by representatives. As further support, Figure A20 plots the estimated coefficients from models comparing below-median detailed

physicians (beneficiaries) to never-detailed physicians (beneficiaries). We observe largely the same trends (particularly in the pre-period) as with comparisons using the above-median cohorts, suggesting that detailed physicians are likely systematically different in their prescribing behaviors than non-detailed physicians. This also raises an important limitation to the interpretation of our results: we are not able to comment on the effect of detailing itself in a binary sense, but rather on the effect of an increasing intensity in detailing. However, given that **XX**% of physicians are ever detailed, or who work with colleagues who are ever detailed,[37] we believe that this comparison is relevant on both a policy and practice landscape.

We also test the robustness of our results to excluding any providers whose first detailing visit was in the first year of our detailing database (2007).[38] This exercise drops **XX** ever-detailed providers from our sample. Figure A21 suggests that our main finding is robust to excluding these physicians.

PLACEHOLDER: We test the integrity of our empirical approach by estimating our model on a set of negative control outcomes. For example, if there is some other contemporaneous event that changes a physician's overall propensity to prescribe any prescription (including opioids), then we might observe a relative increase in the volume of seemingly unrelated prescriptions in our event study framework. Figure A23 plots the results of estimating Equation 1 for a set of plausibly negative control outcomes: prescriptions of antihypertensives, β-blockers, diabetes agents (non-insulin), glaucoma medications, and proton pump inhibitors (PPIs). We see no comparable increases in these (unrelated) medications.

PLACEHOLDER: ***Years > 2013, NPI on prescription***

PLACEHOLDER: ***Include top 5%***

PLACEHOLDER: ***Patients exclusive to physician***

PLACEHOLDER: ***Accounting for staggered treatment timing***

PLACEHOLDER: ***Continuous treatment*** (Callaway et al., 2024a,b)

PLACEHOLDER: ***More appropriate consideration of pre-trends*** (Rambachan and Roth, 2023)

[37] The number of control physicians that are truly never-detailed (as opposed to not-yet detailed, from a TIN standpoint) is much smaller than our sample size would suggest. Specifically, when we restrict our never-detailed physician sample to physicians who themselves are never detailed, and who are never associated with TINs that have detailed provider-members, we are left with **XX** physician controls.

[38] Recall that we exclude providers detailed in the first half (i.e., quarters 1 and 2) of 2007 in our main analyses.

PLACEHOLDER: ***Short-term versus long-term exposure***

PLACEHOLDER: ***Key provider-patient relationship (e.g., number of visits in pre-period, continued relationship after detailing event)***

7 Conclusion

In this paper, we establish a direct connection between detailing visits from pharmaceutical representatives, physician prescribing behavior, and patient opioid use. Specifically, we find that for every additional detailing visit from a Purdue representative that a physician experiences in a given quarter, their total Purdue opioid prescriptions increase by **XX** (**XX**%), and their patients experience a **XX** (**XX**%) relative increase in filling a Purdue opioid prescription starting in the year after the first detailing visit takes place. Furthermore, we find that increased opioid prescribing and fills following detailing visits can result in negative impacts on patient welfare: we observe a **XX**% relative increase in the number of patients a detailed physician treats with a fall or fracture, and a within-patient relative **XX**% increase in the likelihood of experience a fall or fracture event after one's key provider is detailed. While we cannot rule out that increased access to opioids improved patient pain, we also present compelling evidence that any benefits were not without costs.

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Tables

Table 1: Summary Statistics of the Physician-Level Sample with Matched Controls, Provider Characteristics

	First Year Detailing Intensity			
	Above Median	Below Median	Ever Detailed	Never Detailed
Panel A. Provider demographic and specialty characteristics				
Men (%)	71	67	69	63
Specialty (%)				
Internal Medicine	60	56	58	73
Family Medicine	18	18	18	9
Orthopaedic Surgery	8	8	8	4
Anesthesiology	2	2	2	0
Physical Medicine & Rehabilitation	2	2	2	1
Panel B. Treatment activity				
Total patients per quarter, mean (SD)	27 (20)	23 (18)	25 (19)	16 (15)
Total exclusive patients per quarter, mean (SD)	9 (9)	7 (7)	8 (8)	3 (4)
Total Purdue rx per quarter, mean (SD)	6.66 (7.18)	6.29 (7.03)	6.50 (7.12)	4.51 (5.04)
Total Purdue MME per quarter, mean (SD)	169 (452)	164 (342)	167 (407)	119 (187)
Total any opioid rx per quarter, mean (SD)	10.69 (10.48)	9.71 (10.02)	10.26 (10.29)	6.19 (6.67)
Total any opioid MME per quarter, mean (SD)	188 (508)	177 (357)	183 (448)	135 (203)
Detailing intensity, mean (SD)	4.3 (2.3)	1.0 (0.0)	2.8 (2.4)	
Distinct physicians (N)	1070	842	1912	5184

For time-variant measures, means and standard deviations (SD) were calculated based on the four quarters in the pre-period for ever-detailed providers, and were calculated based on all quarters a provider was observed for never-detailed providers. Detailing intensity refers to the number of detailing visits a provider receives within the first year of their first detailing visit.

Table 2: Summary Statistics of the Physician-Level Sample with Matched Controls, Patient Characteristics

	First Year Detailing Intensity			
Patient Characteristics	Above Median	Below Median	Ever Detailed	Never Detailed
Male (%)	38	38	38	42
Race (%)				
White	92	91	92	86
Black	4	5	4	7
Hispanic	1	1	1	2
Asian	1	1	1	2
Native	0	0	0	0
Age, mean (SD)	69 (12)	68 (15)	68 (13)	64 (16)
Deceased (%)	1	0.61	0.83	0.49
SSDI recipient (%)	15	16	16	22
Medicare-Medicaid dually eligible (%)	27	30	28	60
Has chronic condition(s) (%)	92	91	91	90
Has cancer (%)	12	12	12	13
Has other chronic or potentially disabling condition(s) (%)	91	91	91	89
Has leukemia/lymphoma (%)	5	5	5	5
Has opioid use disorder (%)	4	4	4	5
Has Parts A/B/D, not C (%)	55	56	55	67
Ever OUD event (%)	0.91	0.96	0.93	1.54
Ever fall or fracture (%)	0.66	0.68	0.67	0.62
Ever OUD event, excl (%)	0.15	0.14	0.14	0.14
Ever fall or fracture, excl (%)	0.15	0.13	0.14	0.09

For time-variant measures, means and standard deviations (SD) were calculated based on the four quarters in the pre-period for ever-detailed providers, and were calculated based on all quarters a provider was observed for never-detailed providers. Detailing volume tercile is based on the volume of detailing visits a provider receives within the first year of their first detailing visit. "Has chronic condition(s)" refers to the beneficiary having a record of one of the following chronic conditions: acute myocardial infarction (AMI), Alzheimers disease, Alzheimers disease and related disorders or senile dementia, atrial fibrillation, cataract, chronic kidney disease, chronic obstructive pulmonary disease (COPD), heart failure (CHF), diabetes, glaucoma, hip/pelvic fracture, ischemic heart disease, depression, osteoporosis, rheumatoid arthritis/osteoarthritis, stroke/transient ischemic attack, breast cancer, colorectal cancer, prostate cancer, lung cancer, endometrial cancer, anemia, asthma, hyperlipidemia, benign prostatic hyperplasia, hypertension, or hypothyroidism. "Has other chronic or potentially disabling condition(s) refers to the beneficiary having a

Table 3: Differences-in-Differences, Purdue Opioid Prescriptions, Physician Level

Panel A. Extensive Margin

	Total Opioid Rx	Ln(MME)	Num. Pts w/ Opioid Rx
	(1)	(2)	(3)
Above Median × Post	0.595**	0.138	0.155*
	(0.299)	(0.086)	(0.087)
Observations	229,828	229,828	229,828
Below Median Mean	6.29	164.4	2.26
Above Median Mean	6.66	168.7	2.39

Panel B. Intensive Margin

	Num. Opioid Rx per Pt	Ln(MME per Pt)	Share(Pts) w/ Opioid Rx
	(4)	(5)	(6)
Above Median × Post	0.008	0.087*	0.004
	(0.014)	(0.045)	(0.004)
Observations	229,828	229,828	229,828
Below Median Mean	0.304	8.01	0.106
Above Median Mean	0.311	8.02	0.105

Standard errors are clustered at the provider level and are reported in parentheses. All outcomes are defined at the provider-quarter level. Means are based on pre-treatment values for ever ever-detailed observations, and overall values for never detailed observations. Means for log-transformed variables are reported in the untransformed form. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Summary Statistics of the Beneficiary-Level Sample with Matched Controls, Beneficiary and Key Provider Characteristics

	Key Provider's First Year Detailing Intensity			
	Above Median	Below Median	Ever Treated	Never Treated
Panel A. Beneficiaries				
Male (%)	37	36	37	37
Mean age (SD)	68 (15)	68 (14)	68 (15)	68 (14)
Race (%)				
White	90	89	90	90
Black	3	5	4	4
Hispanic	2	2	2	2
Asian	2	1	2	2
SSDI Recipient (%)	24	24	24	23
Chronic pain ever (%)	99	99	99	99
Chronic pain before first detailing (%)	89	91	90	
Any opioid Rx ever (%)	58	59	59	67
Any generic Purdue opioid Rx ever (%)	43	45	44	44
Mean quarters observed (SD)	16 (0)	16 (0)	16 (0)	34 (13)
Panel B. Key Providers				
Mean share of visits in pre-period (SD)	0.64 (0.25)	0.61 (0.26)	0.63 (0.26)	
Primary care (%)	91	87	89	
Mean detailing intensity (SD)	5 (2)	1 (0)	3 (3)	
Total Distinct Beneficiaries	8989	5612	14601	43803
Total Distinct Key Providers	980	743	1723	

Table 5: Differences-in-Differences, Purdue Opioid Prescriptions, Beneficiary Level

	Pr(Opioid Rx) (1)	Ln(MME) (2)
Panel A. Full Sample		
Above Median × Post	0.006*	0.013
	(0.003)	(0.012)
Observations	1,645,679	1,645,679
Below Median Mean	0.119	0.167
Above Median Mean	0.112	0.152
Panel B. Chronic Pain Population		
Above Median × Post	0.007*	0.015
	(0.004)	(0.013)
Observations	1,617,144	1,617,144
Below Median Mean	0.130	0.182
Above Median Mean	0.124	0.168
Panel C. SSDI Population		
Above Median × Post	0.012	-0.016
	(0.009)	(0.035)
Observations	421,247	421,247
Below Median Mean	0.216	0.311
Above Median Mean	0.223	0.341

Standard errors clustered at the beneficiary level; outcomes are at the beneficiary–quarter level. Means are calculated on pre-treatment values for treated beneficiaries and all periods for control beneficiaries. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Differences-in-Differences, Opioid-Related Adverse Health Events, Beneficiary Level

	MME $\geq$ 90	MME $\geq$ 120	Overlapping Opioid Rx	Pr(Overdose or Infection)	Pr(Fall or Fracture)
	(1)	(2)	(3)	(4)	(5)
Above Median $\times$ Post	-0.002	-0.002	0.002	-0.0001	0.001
	(0.001)	(0.001)	(0.003)	(0.0010)	(0.0008)
Observations	1,645,538	1,645,538	1,645,538	1,645,538	1,645,538
Below Median Mean	0.010	0.006	0.068	0.007	0.005
Above Median Mean	0.009	0.007	0.067	0.008	0.005

Standard errors clustered at the beneficiary level; outcomes are at the beneficiary–quarter level. Means are calculated on pre-treatment values for treated beneficiaries and all periods for control beneficiaries. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

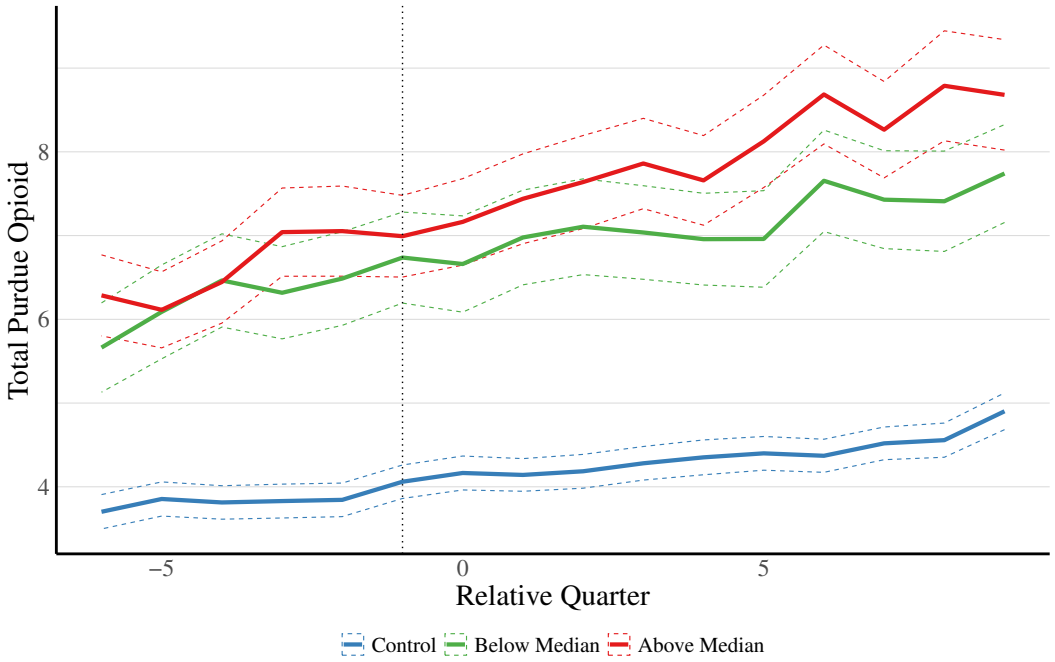
Table 7: Differences-in-Differences, Opioid-Related Adverse Health Events, Physician Level

	Num. Patients w/ Overdose or Infection	Num. Patients w/ Fall or Fracture
	(1)	(2)
Above Median $\times$ Post	0.009	0.039***
	(0.015)	(0.015)
Observations	229,641	229,641
Below Median Mean	0.213	0.154
Above Median Mean	0.215	0.156

Standard errors are clustered at the provider level and are reported in parentheses. All outcomes are defined at the provider-quarter level. Means are based on pre-treatment values for ever ever-detailed observations, and overall values for never detailed observations. Means for log-transformed variables are reported in the untransformed form. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

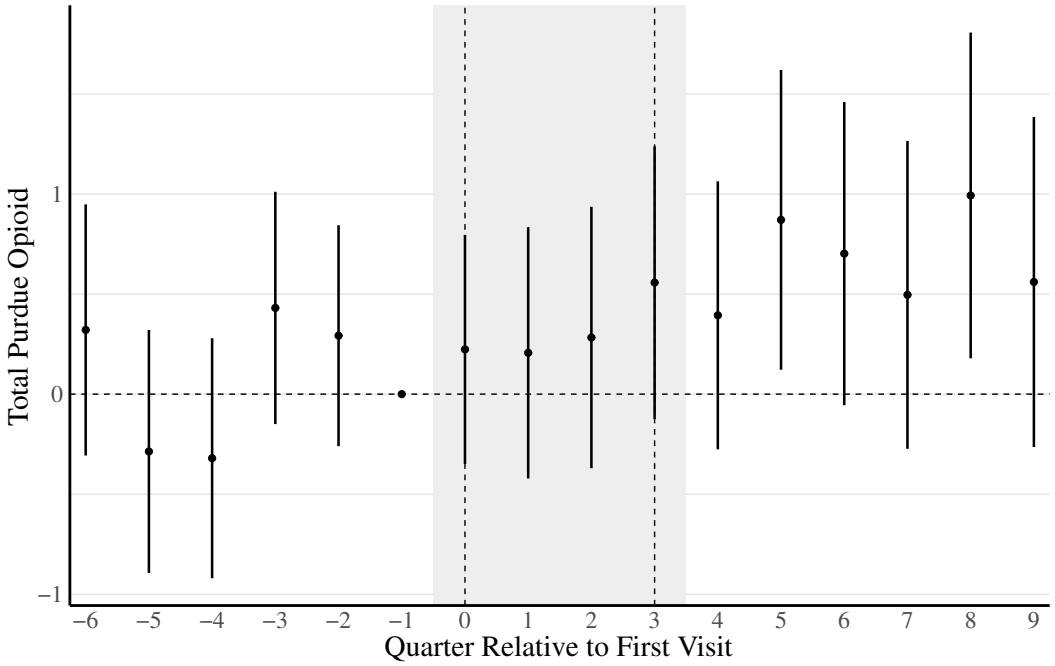
Figures

Figure 1: Unadjusted Trends in Purdue Opioid Prescriptions Relative to First Detailing Visit, Physician Level



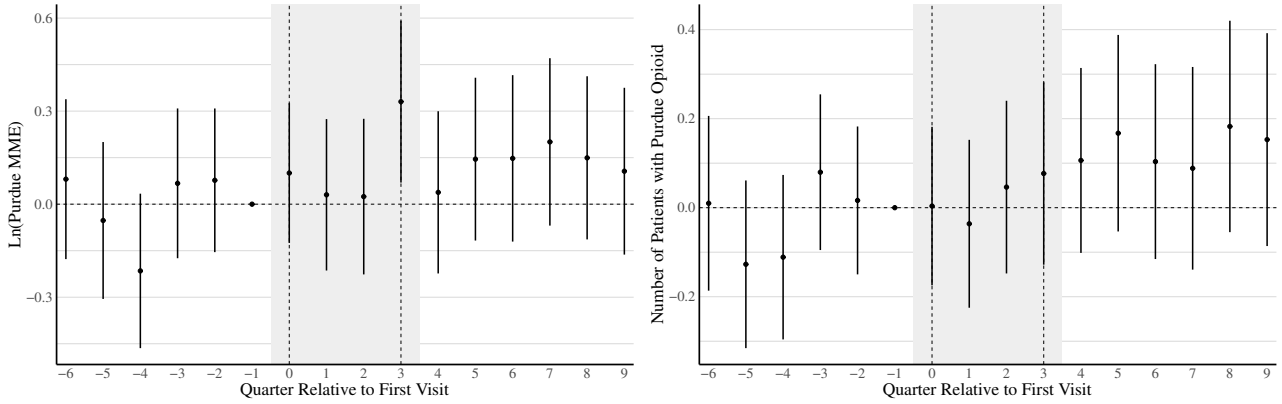
Never-detailed physicians are randomly assigned a placebo treatment date for purposes of comparison.

Figure 2: Estimated Effects of Additional Detailing Visits on the Number of Purdue Opioid Prescriptions per Physician-Quarter, Physician Level



This figure plots the estimated βs from Equation 1. All outcomes are at the distinct physician-quarter level. Standard errors are clustered at the physician level.

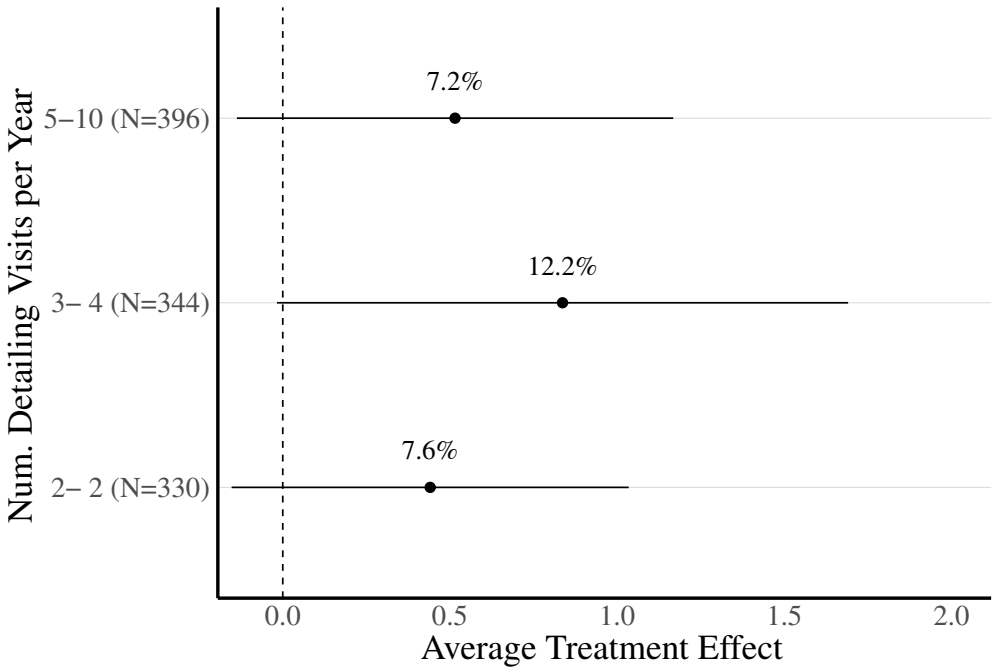
Figure 3: Estimated Effects of Additional Detailing Visits, Other Extensive Margin Measures, Physician Level



(a) Ln(MME) (b) Num. Pts w/ Opioid Rx

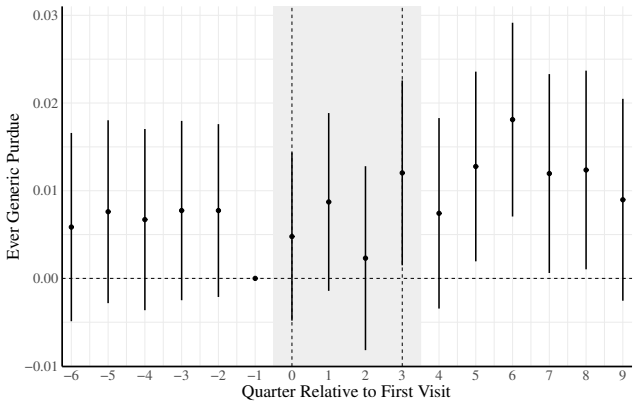
This figure plots the estimated βs from Equation 1. All outcomes are at the distinct physician-quarter level. Standard errors are clustered at the physician level.

Figure 4: Effect Size by Detailing Visits per Year, Total Purdue Opioids Prescribed, Physician Level

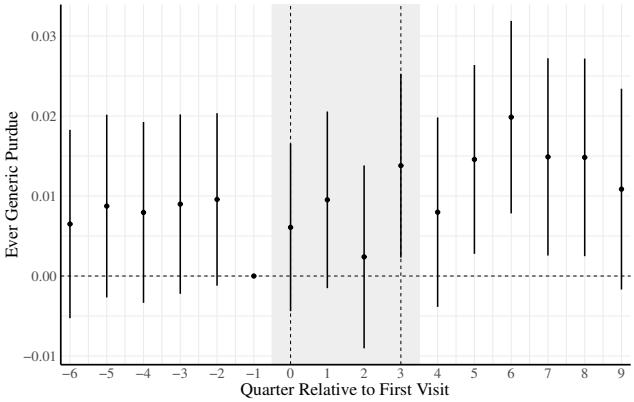


90% confidence intervals are given by solid black lines.

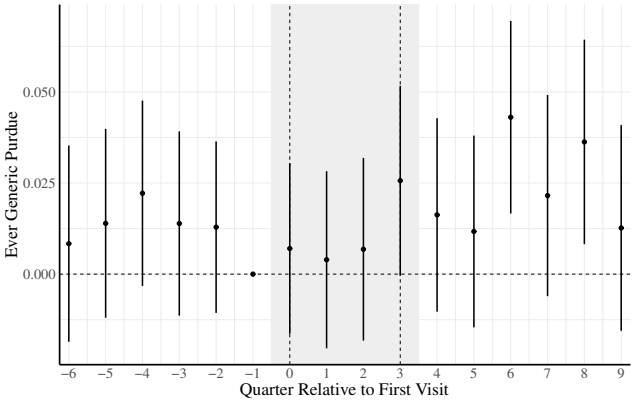
Figure 5: Estimated Effects of Additional Detailing Visits, Probability of Filling a Purdue Opioid Prescription, Beneficiary Level



(a) Full Sample

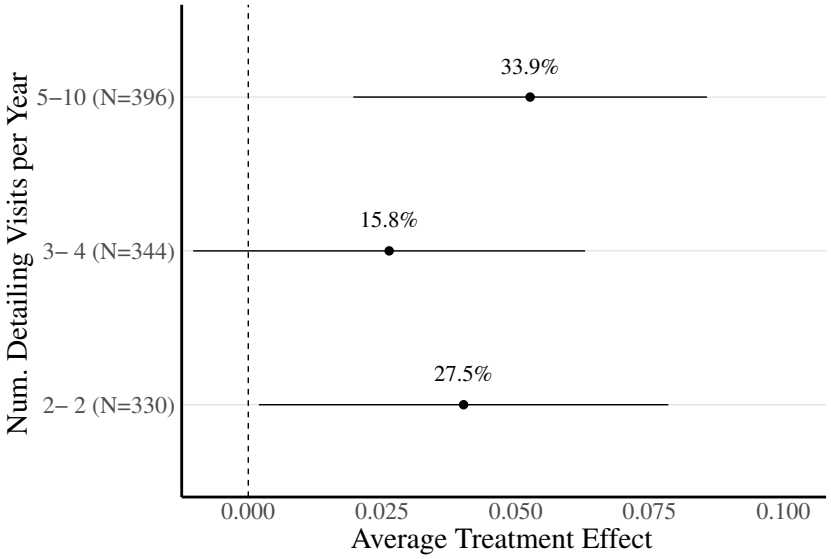


(b) Chronic Pain Sample

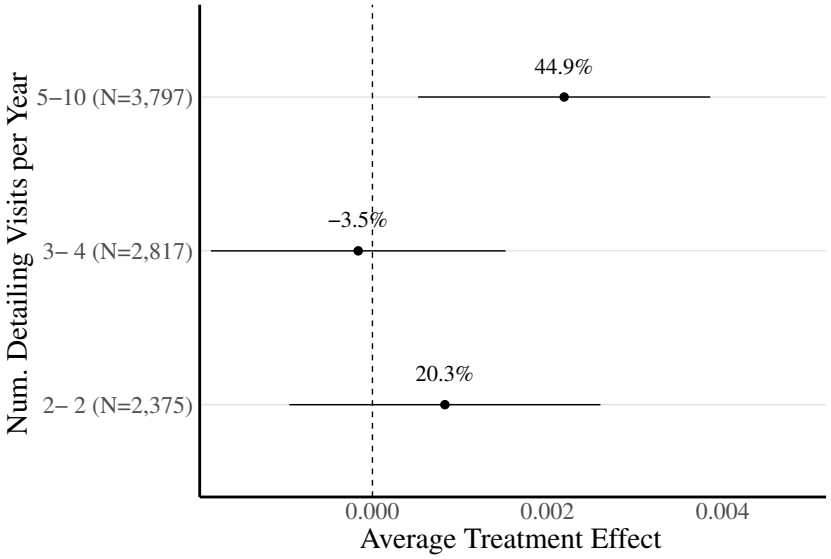


(c) SSDI Sample

Figure 6: Dose-Response to Detailing, Falls or Fractures



(a) Physician Level



(b) Beneficiary Level

90% confidence intervals are given by solid black lines.

Appendix

A Additional Tables and Figures

Appendix Table A1: Detailed vs. Non-detailed Providers in Medicare, 2006–2018

Characteristic	Not detailed	Detailed
Male (%)	60.1	69.3
Mean year of graduation, medical school (SD)	1998 (11)	1991 (10)
Specialty		
Anesthesiology (%)	6.2	4.7
Family medicine (%)	5.3	18.5
Internal medicine (%)	33.1	53.4
Physical medicine & rehabilitation (%)	0.7	3.5
Orthopaedic surgery (%)	3.0	7.1
Mean Medicare patients per month (SD)	9.4 (12)	9.4 (12)
Total	29980	3322

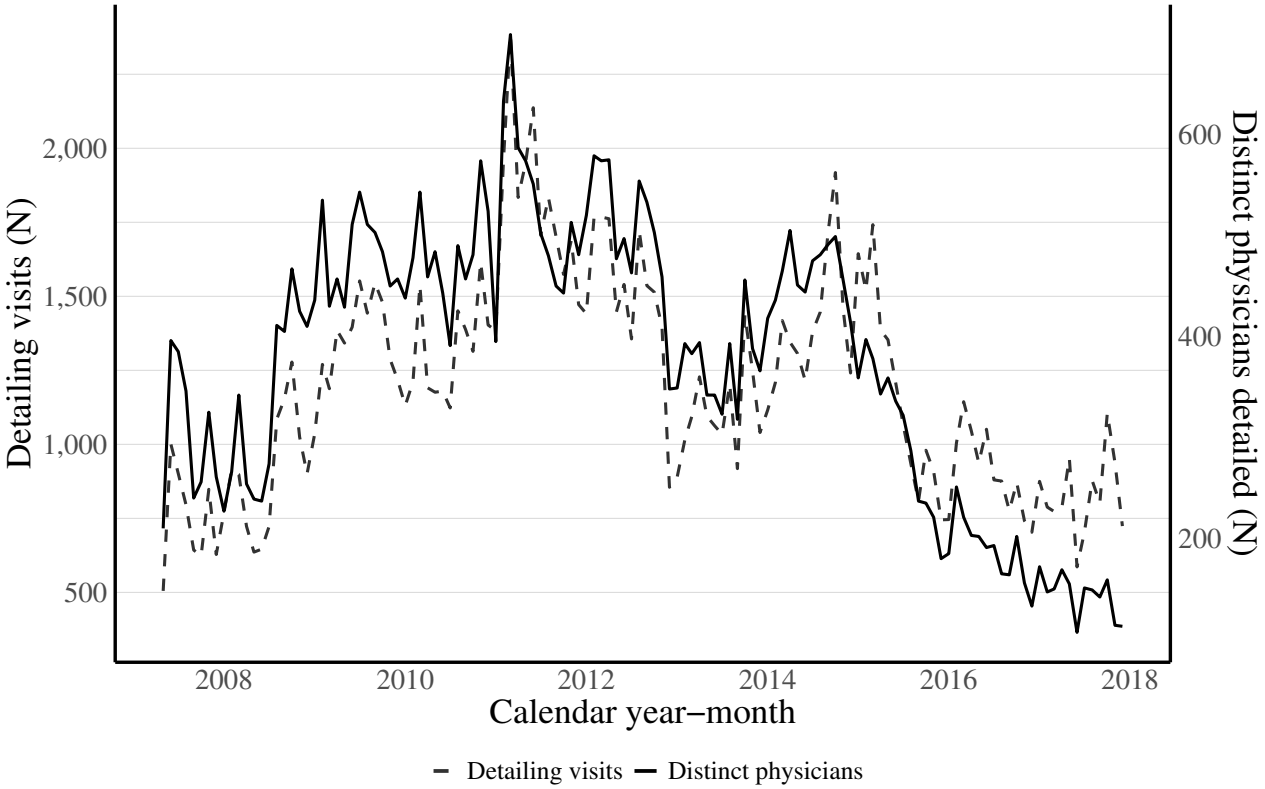
Appendix Table A2: Compare Characteristics of Matched and Unmatched Controls

Characteristic	Unmatched Sample	Matched Sample
Men (%)	59.1	63.3
MD/DO (%)	100	100
Specialty (%)		
Internal Medicine (%)	39	73.5
Family Medicine (%)	6.4	9.2
Orthopaedic Surgery (%)	3.7	4.5
Nurse Practitioner (%)	0	0
Anesthesiology (%)	1.3	0.4
Physician Assistant (%)	0	0
Physical Medicine & Rehabilitation (%)	0	0
Total Patients, Mean (SD)	10.3 (13.8)	15.9 (15.2)
Total Purdue Rx, Mean (SD)	3 (4.4)	4.5 (5)
Total Purdue MME, Mean (SD)	80.3 (173.8)	119.4 (187.2)
Total Opioid Rx, Mean (SD)	4.1 (5.9)	6.2 (6.7)
Total MME, Mean (SD)	91 (191.1)	134.9 (203.3)
Distinct Physicians (N)	19761	5184

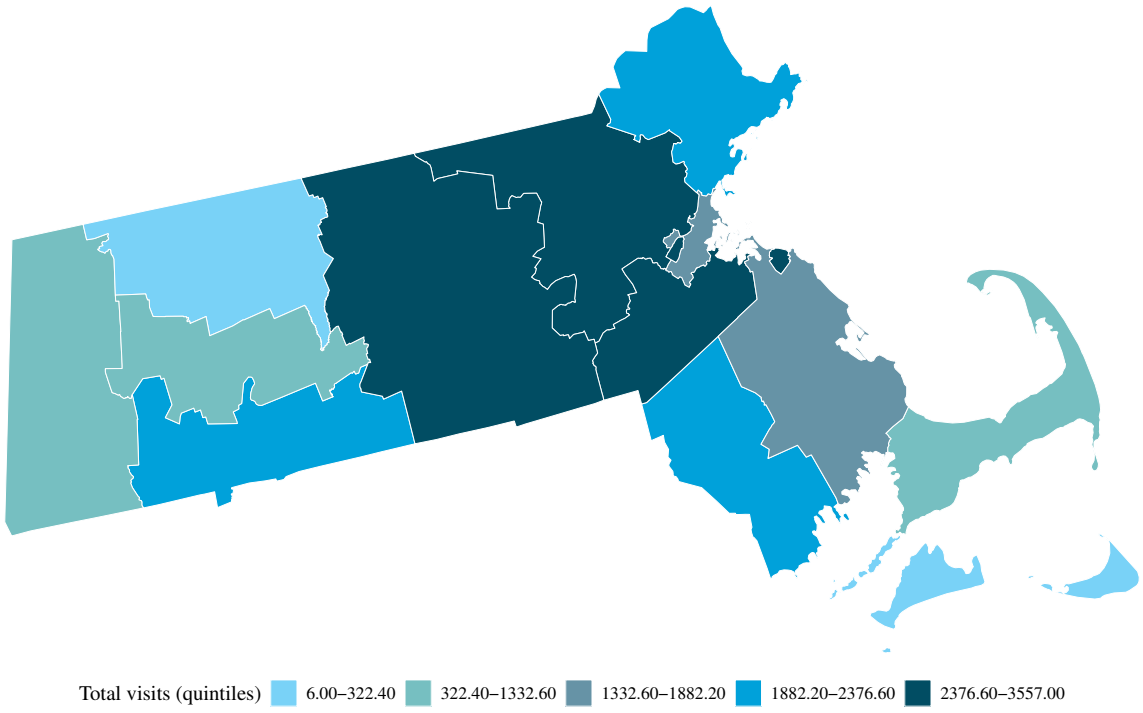
Appendix Table A3: Provider Counts by Treatment Status

Treatment Status	N	%	Range of visits
Ever detailed	2168	7.60	
Above median	1198	4.20	2-10
Below median	970	3.40	1-1
Never detailed	26183	92.40	
Total NPI	28351	100.00	

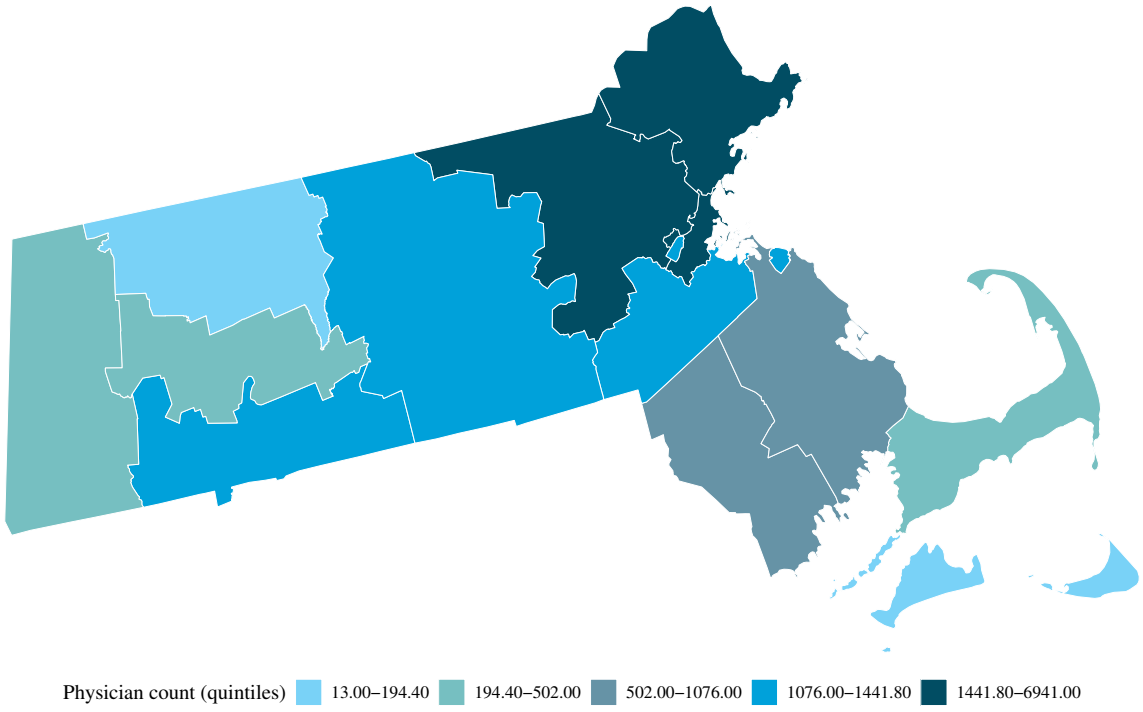
Appendix Figure A1: Volume of Detailing Visits and Detailed Physicians in Massachusetts, 2007–2017



Appendix Figure A2: Detailing Activity, County Level, 2011

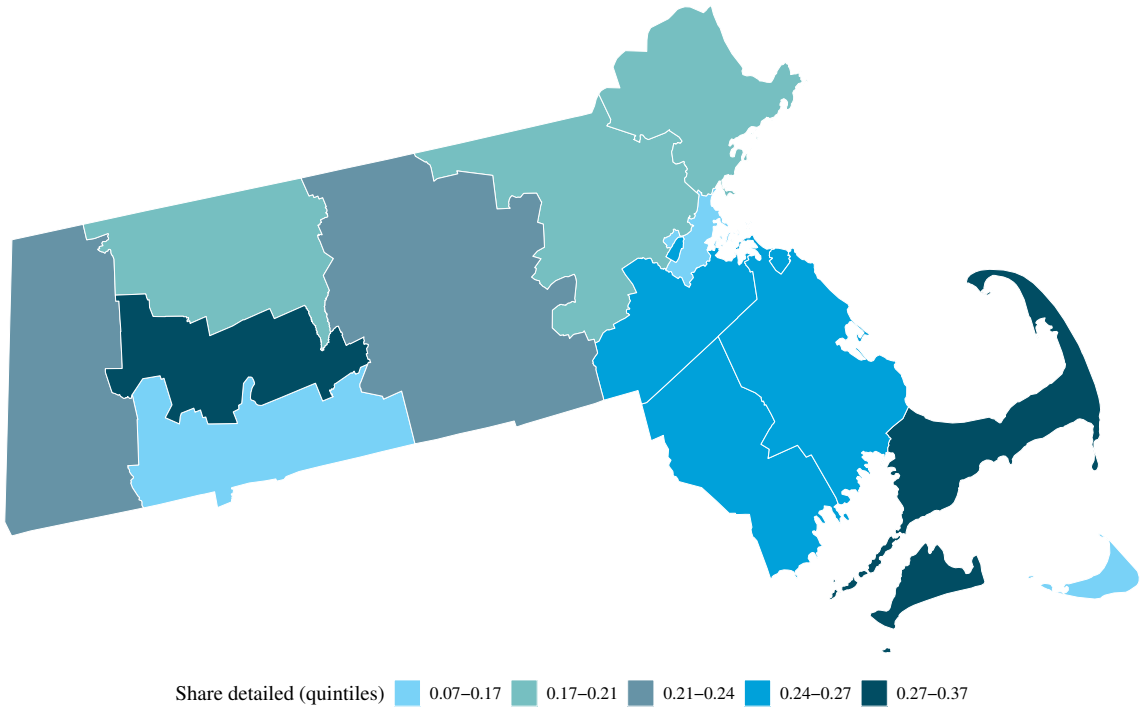


(a) Detailing Visits

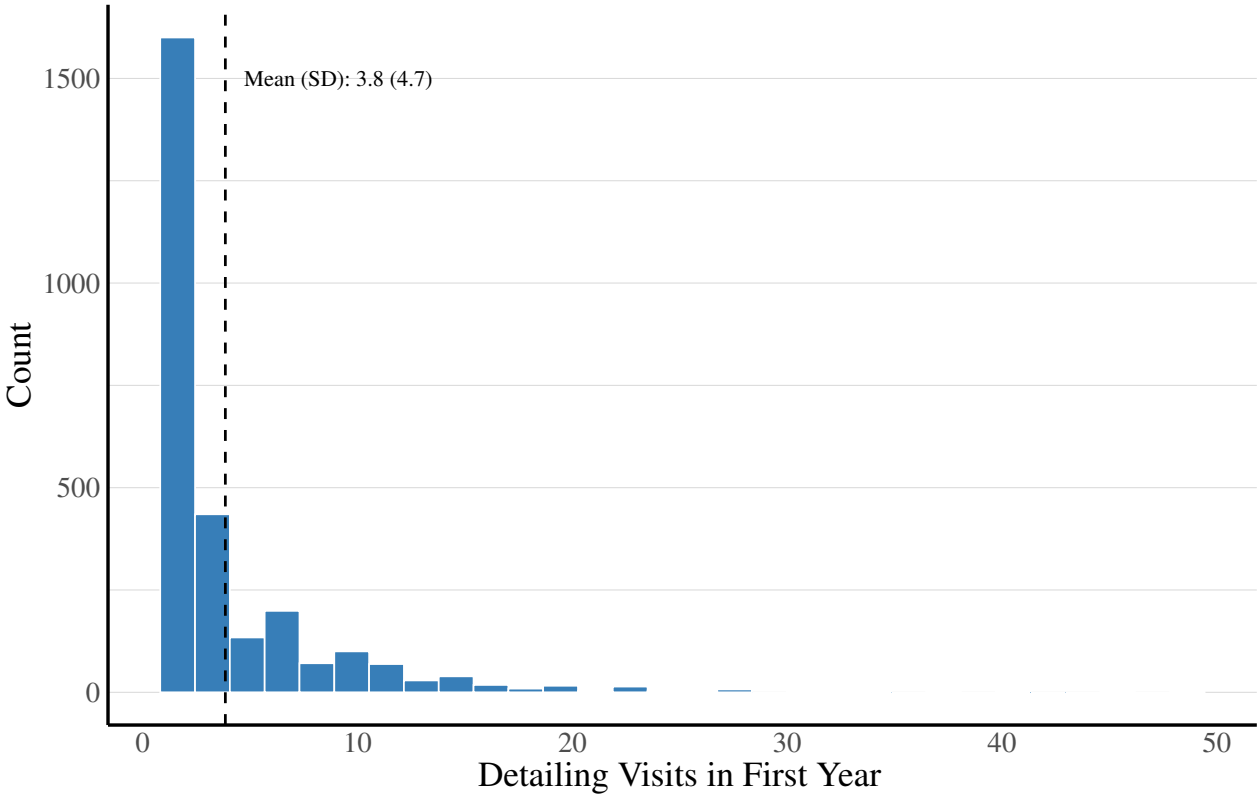


(b) Medicare-Participating Physicians

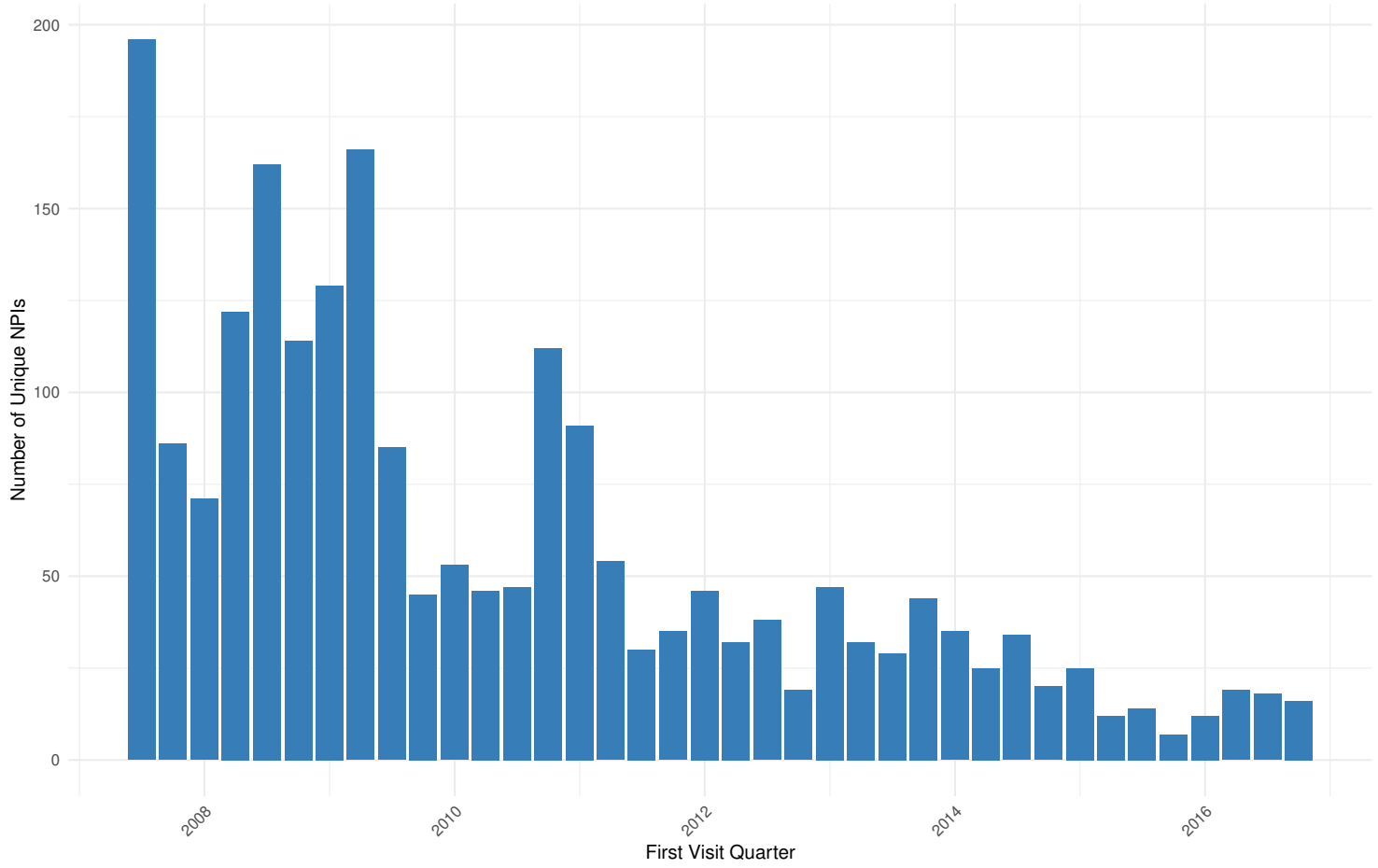
Appendix Figure A3: Share of Detailed Physicians, County Level, 2011



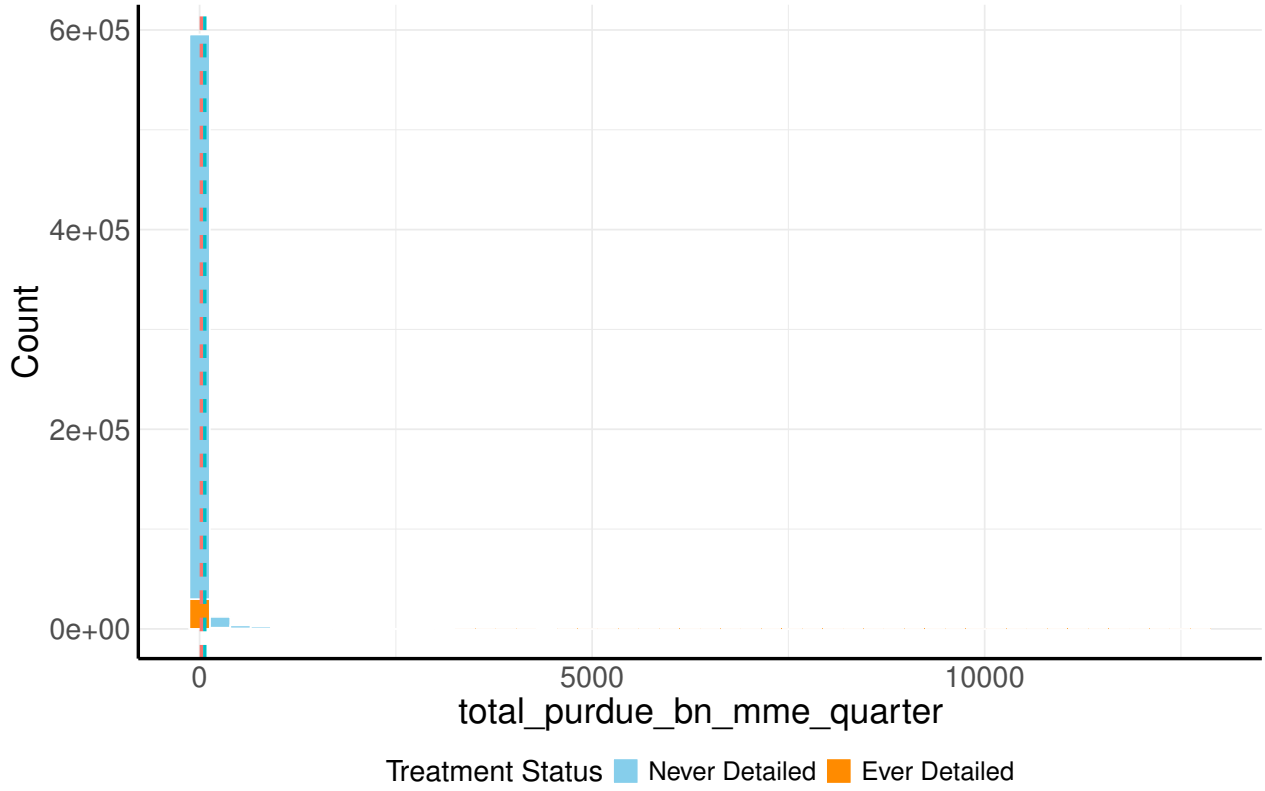
Appendix Figure A4: **UPDATE** Distribution of Volume of Detailing Visits in the Year Following First Visit



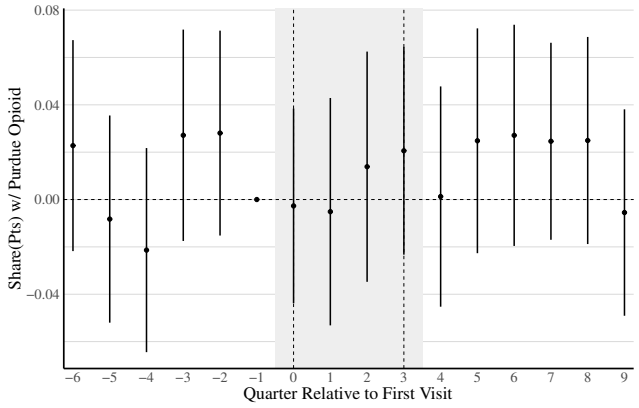
Appendix Figure A5: Distribution of the Timing of First Detailing Visits



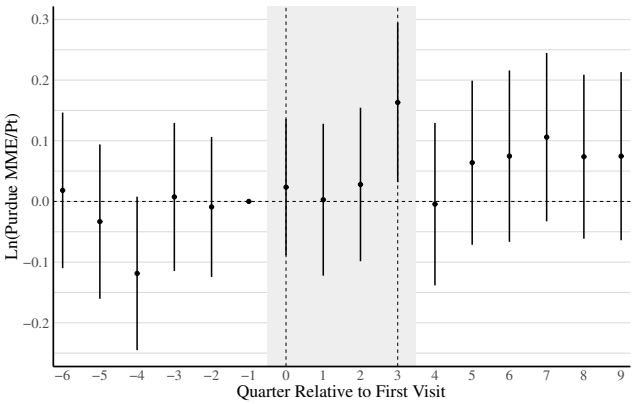
Appendix Figure A6: Distribution of Total MME per Provider-Quarter Associated with Purdue-Brand Opioids



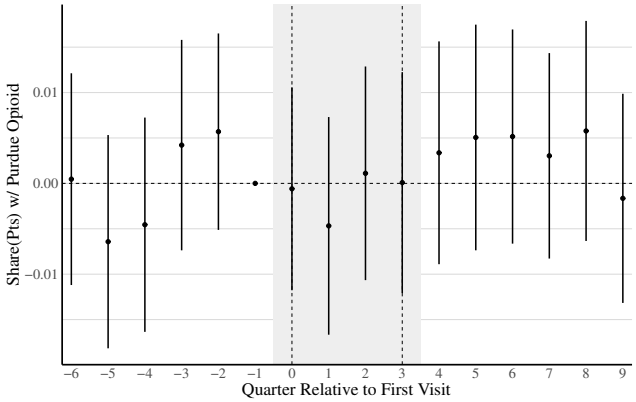
Appendix Figure A7: Estimated Effects of Additional Detailing Visits, Intensive Margin Measures, Physician Level



(a) Num. Opioid Rx per Pt



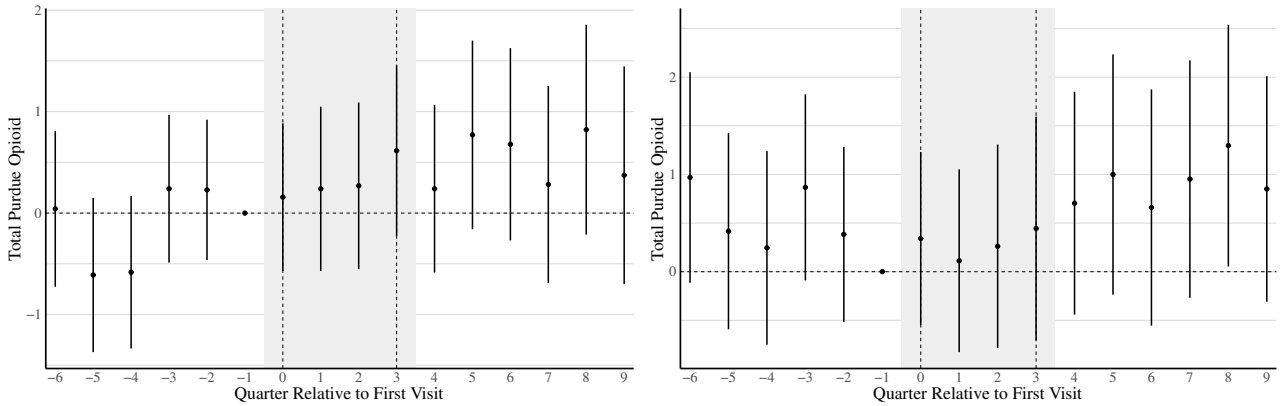
(b) Ln(MME/Pt)



(c) Share Pts w/ Opioid Rx

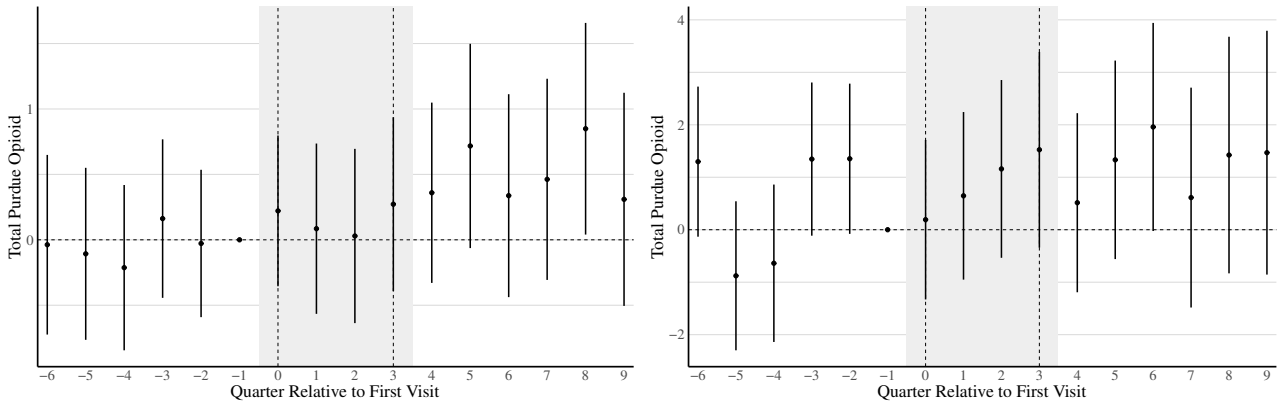
This figure plots the estimated βs from Equation 1. All outcomes are at the distinct physician-quarter level. Standard errors are clustered at the physician level.

Appendix Figure A8: Heterogeneity by Provider Characteristics, Total Purdue Opioid Prescriptions, Physician Level



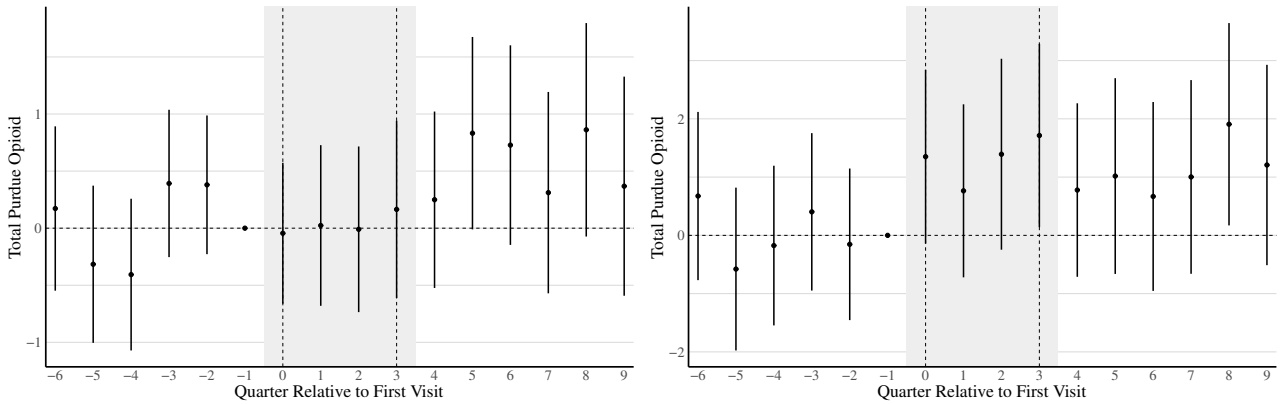
(a) Men (N=1,336)

(b) Women (N=608)



(c) IM or FM (N=1,482)

(d) Not IM or FM (N=462)

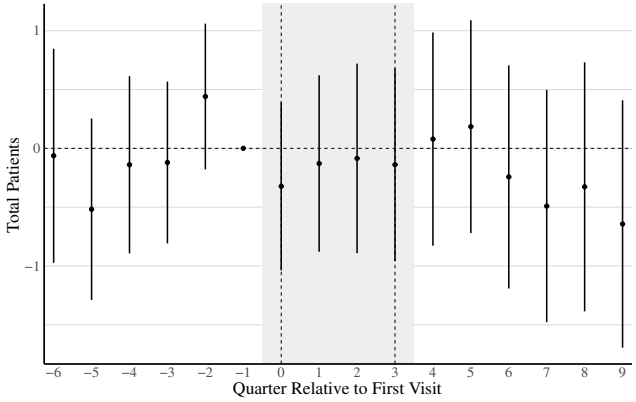


(e) 2007 – 2011 ($N = 1,494$)

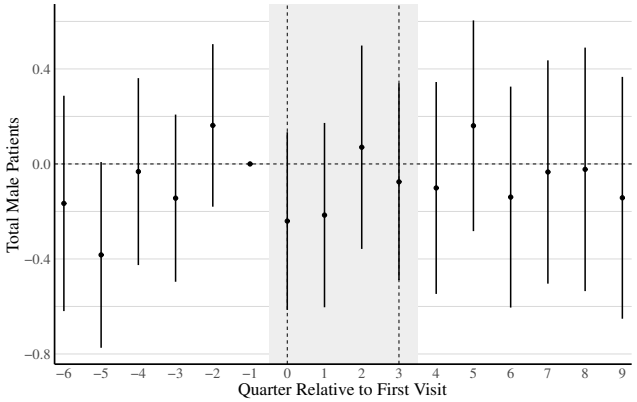
(f) 2012 – 2016 ($N = 450$)

These figures plot the βs estimated from Equation 1 on several different populations of providers. Sample size (N) for the "ever treated" provider population is given in parentheses. "IM" and "FM" abbreviate Internal Medicine or Family Medicine. The top five most common specialties in the "Not IM or FM" sample are: orthopaedic surgery, psychiatry & neurology, anesthesiology, physical medicine & rehabilitation, and surgery.

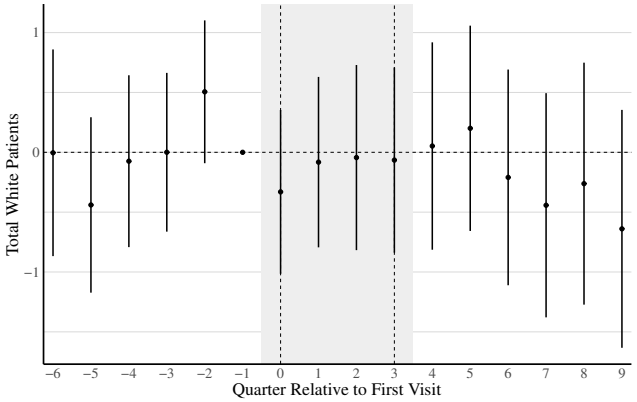
Appendix Figure A9: Balance in Patient Demographics



(a) Total Patients

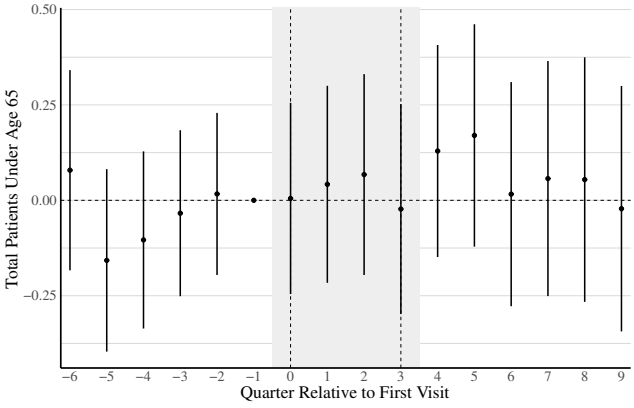


(b) Total Male Patients

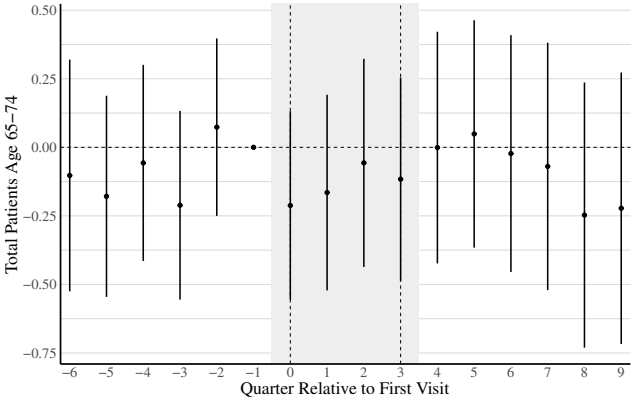


(c) Total White Patients

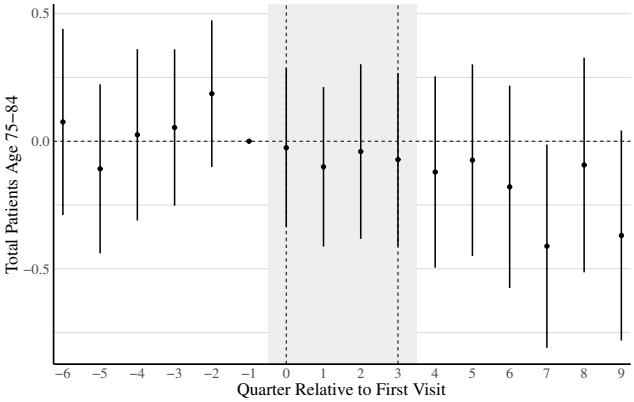
Appendix Figure A10: Balance in Patient Age



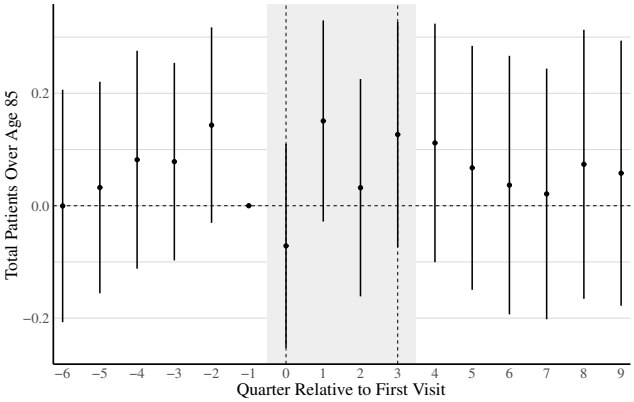
(a) Age < 65



(b) Age 65-74

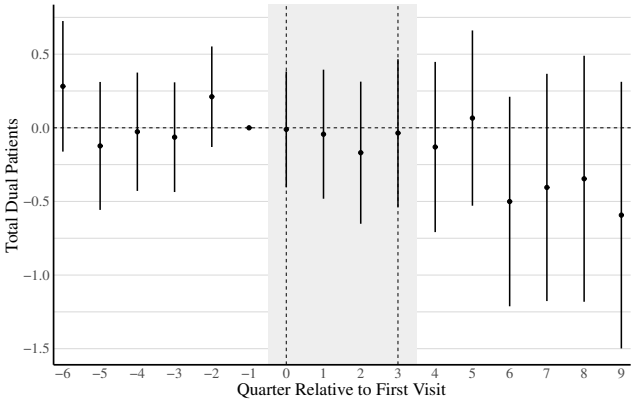


(c) Age 75-84

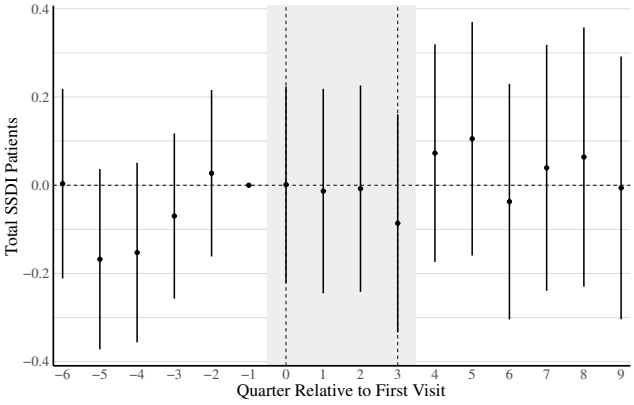


(d) Age > 85

Appendix Figure A11: Balance in Enrollment Characteristics

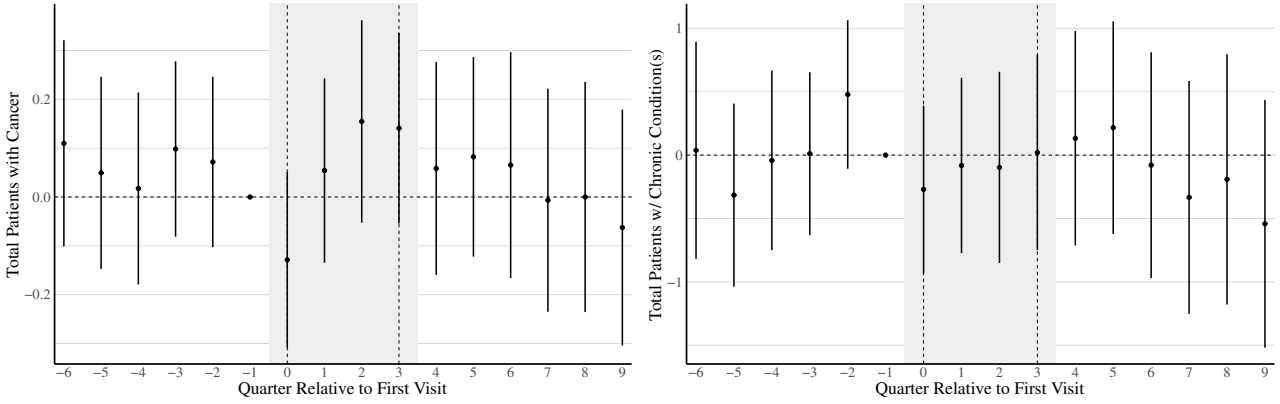


(a) Total Duals



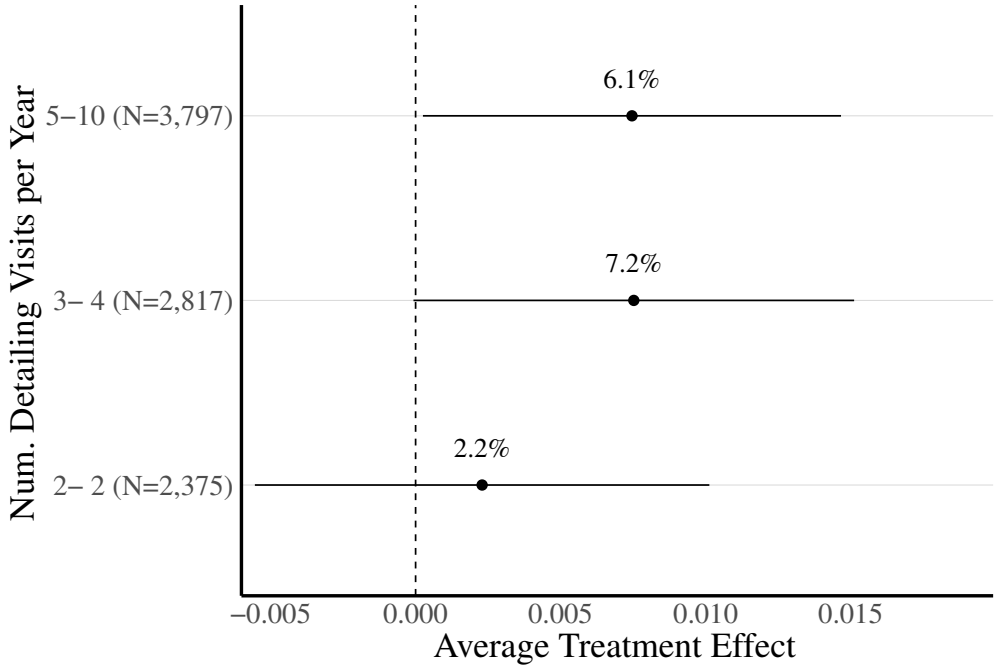
(b) Total SSDI Recipients

Appendix Figure A12: Balance Test in Clinical Characteristics



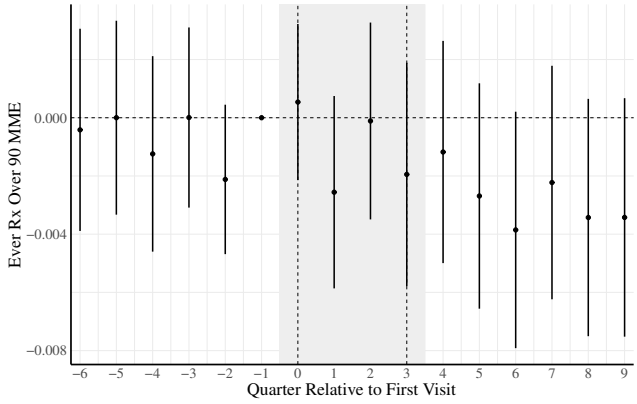
(a) Total Patients w/ Cancer (b) Total Patients w/ Any Chronic Condition

Appendix Figure A13: Dose-Response to Detailing, Pr(Purdue Opioid), Beneficiary Level

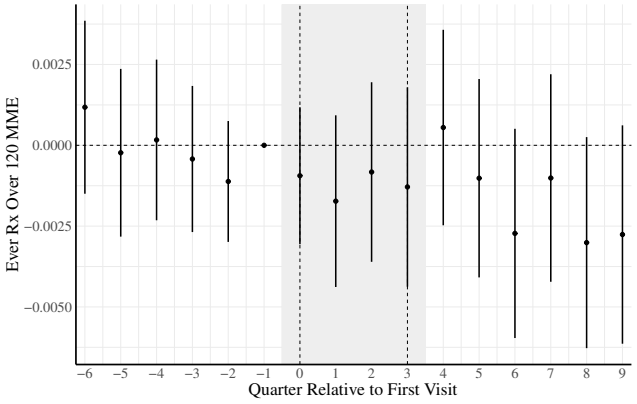


90% confidence intervals are given by solid black lines.

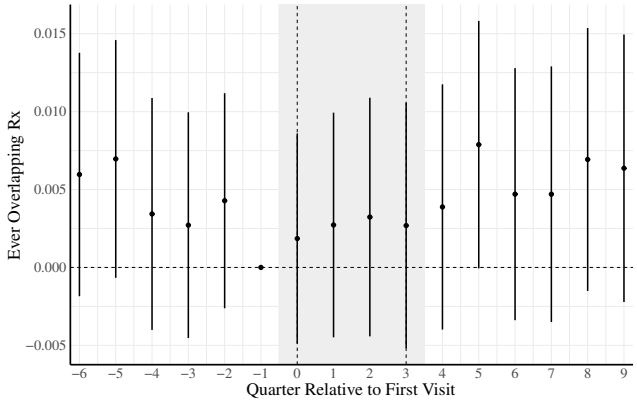
Appendix Figure A14: Probability of ≥ 1 Day in a Quarter with Utilization-Based Indicator for Opioid Use Disorder, Beneficiary Level



(a) MME ≥ 90

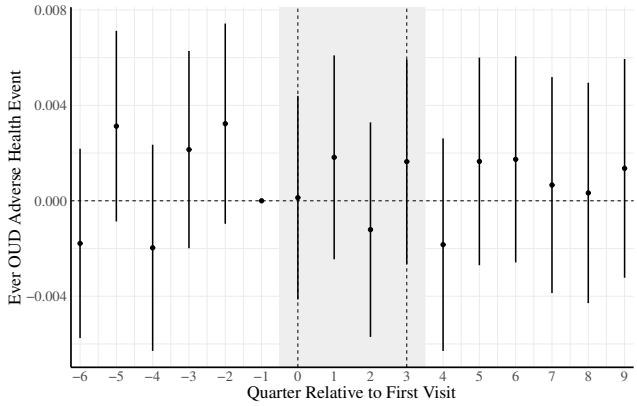


(b) MME ≥ 120

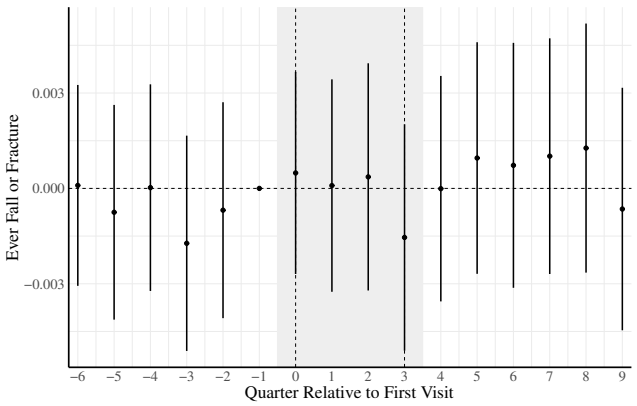


(c) Overlapping Opioid Rx

Appendix Figure A15: Probability of Adverse Health Events, Beneficiary Level

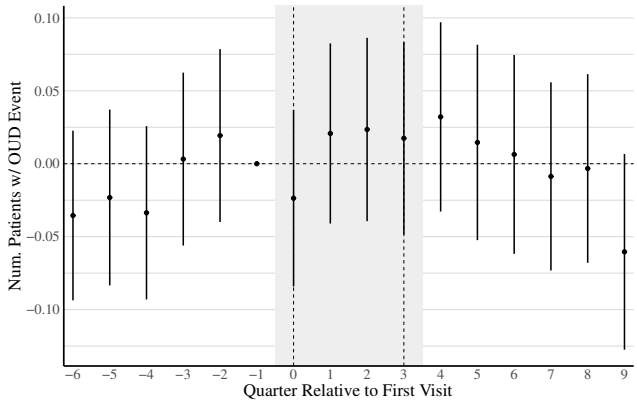


(a) Overdose or Infection

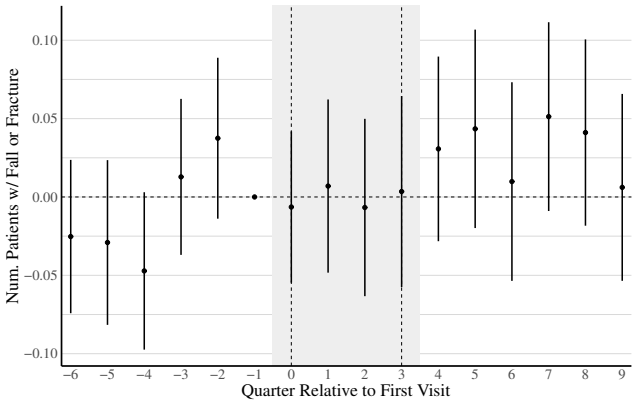


(b) Fall or Fracture

Appendix Figure A16: Probability of Adverse Health Events, Physician Level

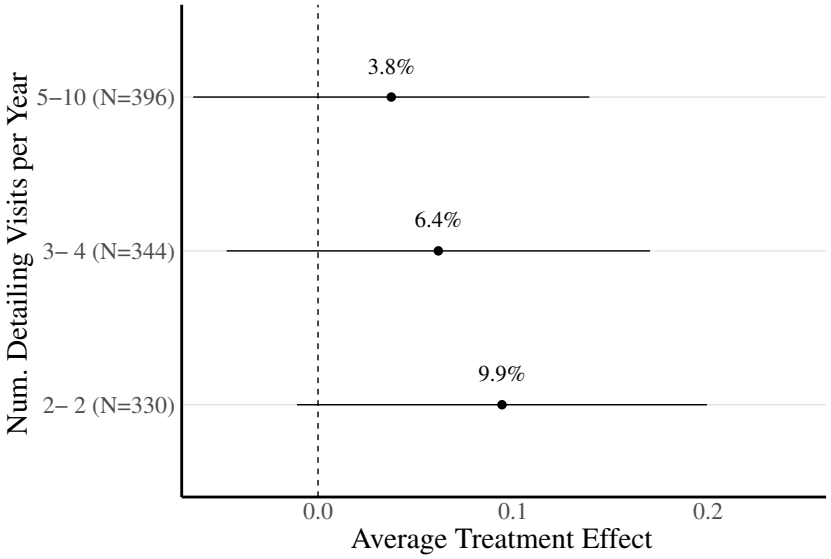


(a) Number of Patients with Overdose or Infection

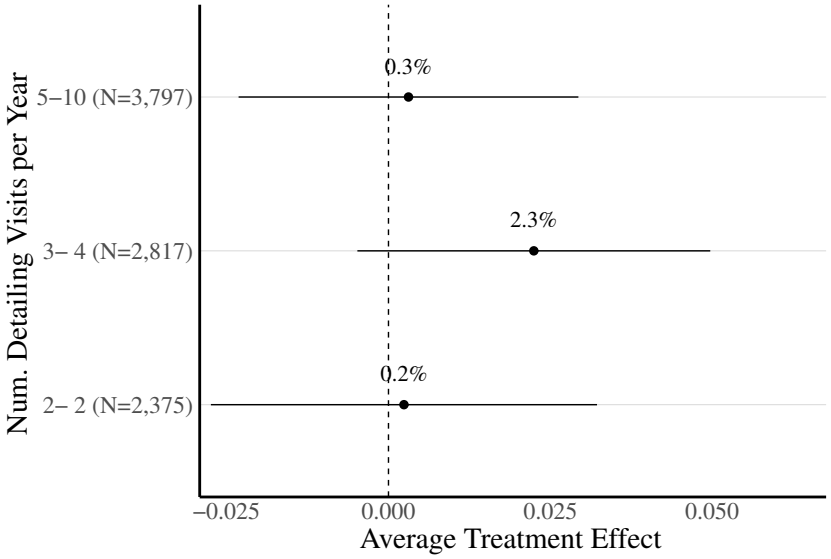


(b) Number of Patients with Fall or Fracture

Appendix Figure A17: Exploration of Mechanisms

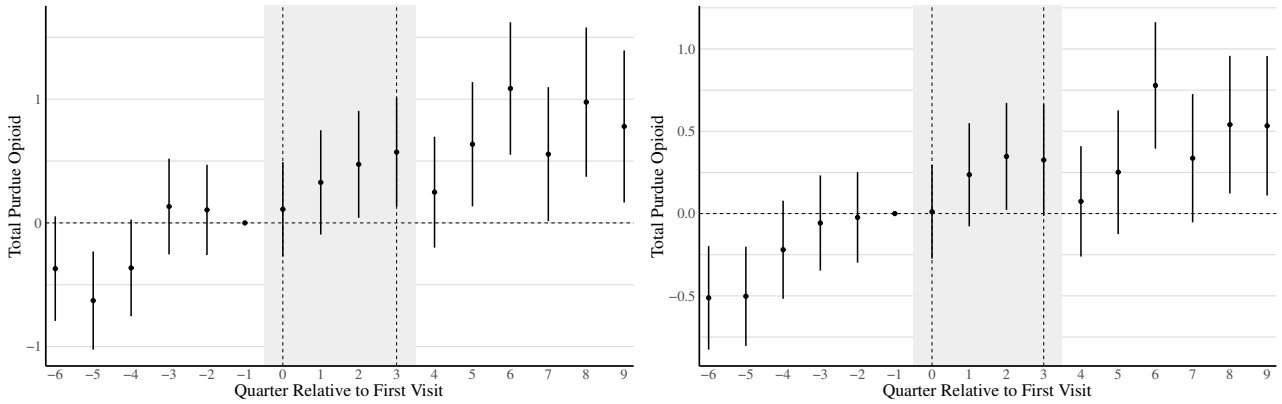


(a) Ln MME per Patient (Physician Level)



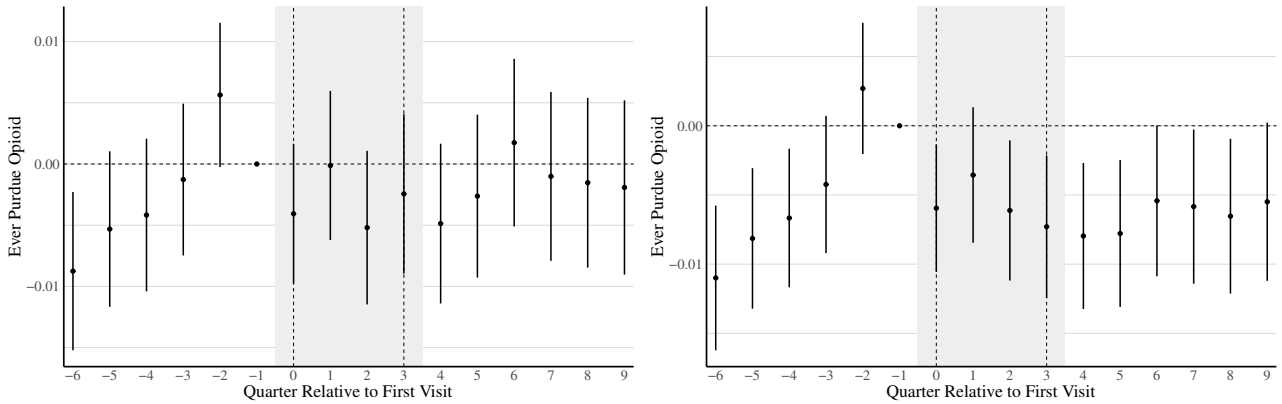
(b) Ln MME (Beneficiary Level)

Appendix Figure A18: Alternative Comparisons, Physician Level



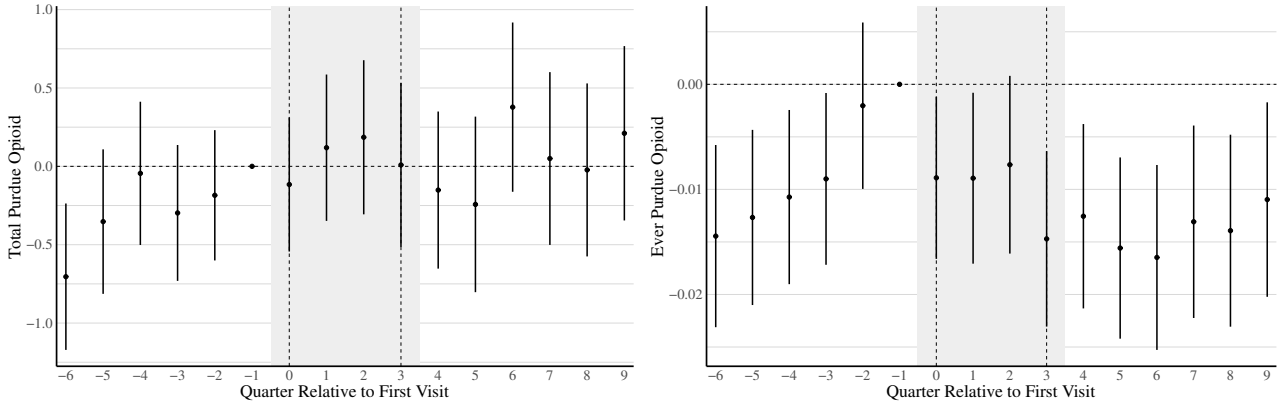
(a) Above-Median v Never-Detailed (b) Ever- v Never-Detailed

Appendix Figure A19: Alternative Comparisons, Physician Level



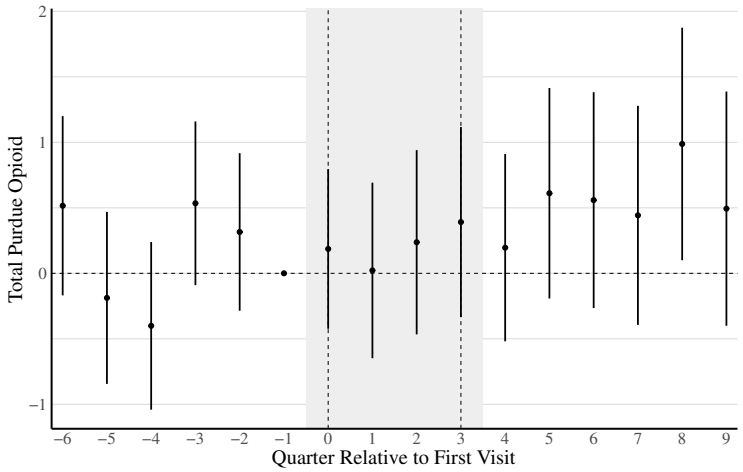
(a) Above-Median v Never Detailed (b) Ever- v Never-Detailed

Appendix Figure A20: Below-Median versus Never-Detailed Physicians



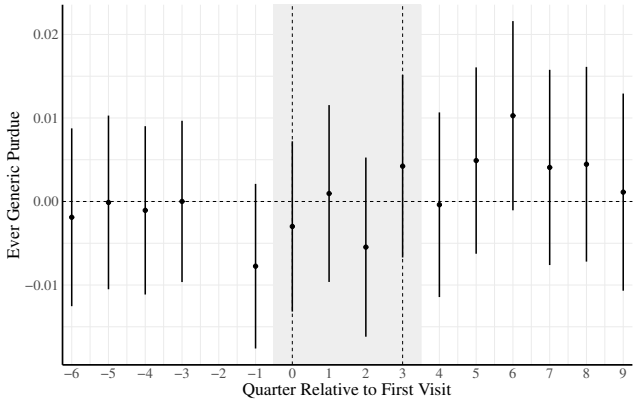
(a) Physician Level (b) Beneficiary Level

Appendix Figure A21: Drop First Detailing in 2007

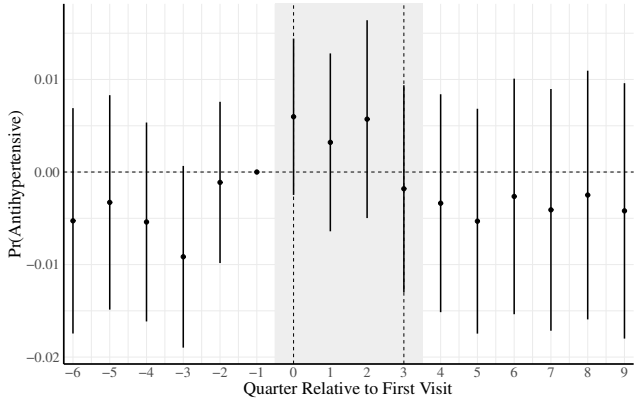


This figure plots the βs estimated from Equation 1 after dropping **XX** any ever-detailed providers whose first detailing visit was in 2007. The final sample includes **XX** ever-detailed providers.

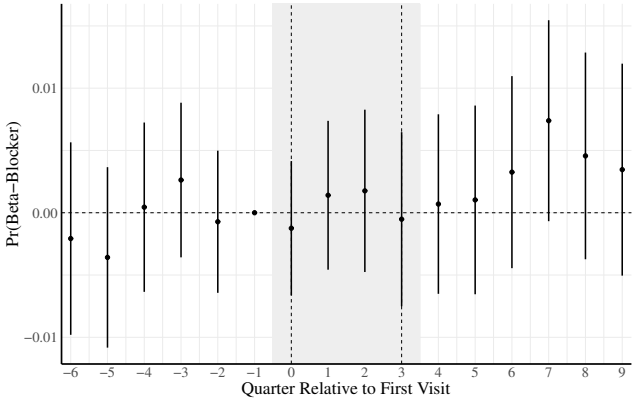
Appendix Figure A22: Beneficiary-Level Event Study, Full Sample, Omit Relative Quarter -2



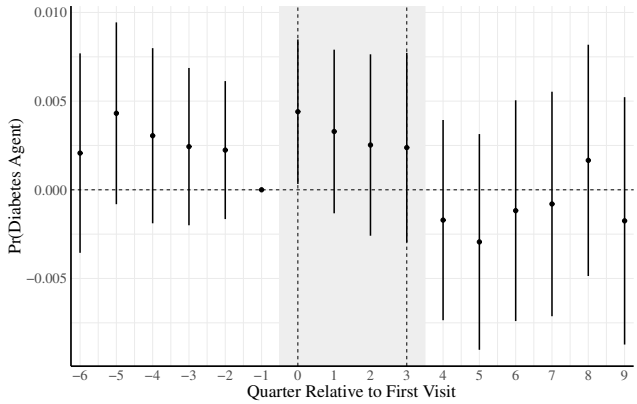
Appendix Figure A23: Negative Controls



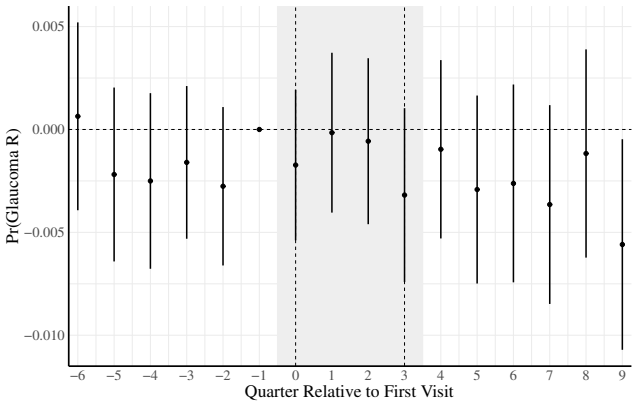
(a) Antihypertensives



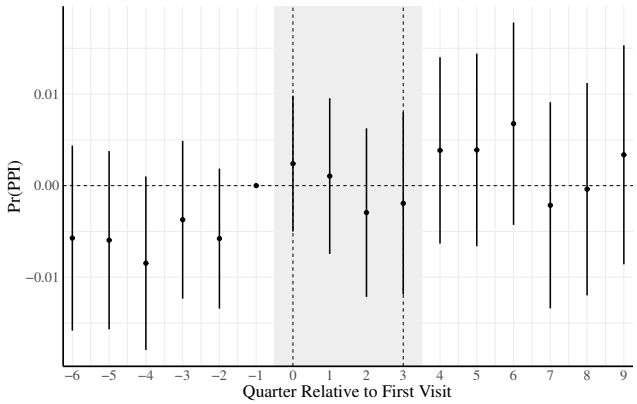
(b) β-blocker



(c) Diabetes



(d) Glaucoma



(e) PPI

B Purdue Pharma Detailing Records

Purdue Pharmaceuticals employed a highly sophisticated data-driven approach to monitoring the growth of their products, including OxyContin, as well as the marketing and promotion of their products. In addition to their internal efforts, they hired McKinsey to analyze and report on quarterly activity. They used internal data to track the detailing calls and visits of their drug representatives in conjunction with prescribing information of clinicians using IMS data. Together this data enabled them to understand the impact of their detailing efforts on the specific MMEs of providers and to observe how prescribing varied by type of clinician (e.g. MD, NP, PA); clinician characteristics such as age and specialty; availability in pharmacies; small geographical areas (e.g. company territories within states), etc.. Using this information, their reports describe efforts to segment the market and target prescribers that would have the largest impact on overall sales and growth. Generally, speaking efforts were made to target providers that were already prescribing a higher volume compared to their peers. In a 2013 company report of declines in OxyContin growth, they attribute 75% of the decline to providers that Purdue is not targeting. The company intentionally integrated science and data into their messaging to staff, prescribers, and consumers to promote their products and they were skilled at influencing the public narrative about opioid therapy advocating for physician organizations and government committees to frame pain assessment and management as necessary and best clinical practice.

By August 2013, internal reports suggest that Purdue was asking representatives to focus their targeting efforts more exclusively on high-prescribers, with the report citing practices in the previous quarter: "Today Purdue spends as much effort detailing the lesser value prescribers (decile 0-4) as it does on the higher value prescibers (decile 5-10). To put this in perspective, the average prescriber in decile 5-10 writes 25 times as many OxyContin scripts as a prescriber in decile 0-4. In Q1 2013 the majority (52%) of OxyContin primary calls were made to decile 0-4 prescribers (page 39-40)" (Committee on Oversight and Reform, U.S. House of Representatives, 2020a).

By September of 2013, Purdue advocated for a shift from decile targeting to physician workload targeting, considering not only deciles but additional factors such as generic prescriptions, first-time brand prescriptions, specialty, and managed care access(Committee on Oversight and Reform, U.S. House of Representatives, 2020b). While the available documents capture only a snapshot of Purdue marketing of OxyContin across the years, they suggest that the targets were specific but also variable over time and with differential adherence across representatives.

The Opioid Industry Documents Archive at the UCSF Industry Documents Library (IDL), created in 2021, contains millions of documents created by opioid manufacturers and related companies. The documents were internal company documents and made publicly available through litigation and other sources. The Archive is hosted by UCSF in collaboration with Johns Hopkins University and all documents are publicly available. For the current project, we obtained the pharmaceutical representative detailing data from the archive that was made available following litigation between Purdue Pharma and the Commonwealth of Massachusetts. These data were accessed via the UCSF IDL API in November 2021. The data are available here.

In the original Purdue data file, there are 153,039 rows, where each row represents an individual detailing visit by a Purdue representative to a "target" where the target can be a specific provider or the name of an entity such as a pharmacy, store, or clinical practice. In the original data, there are 8,776 unique targets of opioid detailing from May 16, 2007 through December 22, 2017. More than 99% of the visits occurred in Massachusetts; the remaining visits occurred in neighboring states.

We used unique addresses listed in the raw Purdue data and queried the Google Maps Geocoding API (https://developers.google.com/maps/documentation/geocoding/overview) to obtain a standardized formatted address, latitude and longitude, and additional point-of-interest information (e.g., type of building) for each location. Nineteen typographic errors were manually fixed by investigators and geocoded a second time.

In the first step of the data cleaning process, we identified targets of detailing that were an entity rather than an individual provider. We identified non-individual target values through a series of rules that we applied to the string values. The order of implementation of the rules was based on the number of non-individual targets that would be identified for exclusion. In the original 153,039 rows, we identified five conditions:

1. 407 instances of "CVS pharmacies" accounting for 10% of all detailing visits
2. 504 instances of "prescription, pharmacy drug, or apothecary" in the name accounting for 7% of all detailing visits
3. 751 instances of "medical centers, hospitals, clinics, or other group practices" accounting for 7% of all detailing visits
4. 410 instances of stores (e.g., "costco", "conley", "kmart") accounting for 6% of all detailing visits

5. and 118 instances of specific geographic locations (e.g., "fall river", "new england") in the name accounting for 1% of all detailing visits.

For any row that met at least one of the previous conditions or the name did not contain a comma to delineate the first and last name, we assumed they were not an individual provider and removed them from our analytic data set. We made exceptions for 6 names that were either missing a common or whose name contained a word or phrase matching a string that was previously flagged. After cleaning, the final analytic data set contained 113,292 detailing visits across 6,631 unique targets .

We linked providers in the detailing data to NPI in the NPPES data through the following steps. The NPPES NPI registry data is publicly available and available for online download https://download.cms.gov/nppes/NPI_Files.html). For each individual provider with an NPI, the data includes NPI, taxonomy description, first name, last name, city, state, country, postal code, and address type. We linked providers listed in the cleaned Purdue data to providers in the NPPES NPI registry using a matching algorithm developed and described by Enamorado et al. (2019). The algorithm returns a probability of an exact match when a potential match is found. We used the open-source R package fastLink: Fast Probabilistic Record Linkage with Missing Data package (version 0.6.0) to implement the algorithm.

We conducted 4 rounds of matching in an attempt to link providers to an NPPES NPI. At each round, we created a flag in the data to indicate if a provider was matched during that round. This will enable us to conduct sensitivity analyses in which we exclude matches from particular rounds to manipulate the certainty of the match. In the first round of matching we used NPPES data for all U.S. states; matches were based on first name, last name, city, and state using string distance matching where the first name could be a partial match (i.e., "Matt" and "Matthew"). The posterior values are listed in the posterior column of the analytic dataset.

In the second round of matching, we restricted NPPES to Massachusetts and matched the remaining providers to an NPI based on first name and last name. The third round of matching was done manually by our team; we looked up unmatched names in the Massachusetts NPPES. In this round we were able to link providers when differences in string values exceed a simple typo (e.g. identify Bob Xyz and Robert Xyz as the same provider). In the fourth round, we used the historic NPPES files for Massachusetts to link remaining providers. The historic NPPES files are accessible via NBER (https://data.nber.org/npi/backfiles/). While the NPPES data includes deactivated providers, it lists

only the state in which an active provider is currently practicing or the state in which a deactivated provider last practiced. Therefore, if a provider was practicing in Massachusetts during the study period 2007-2017 but is currently practicing in another state, they would not be linked. Hence, the historic data files increase our ability to link providers that currently practice outside of Massachusetts.

Of the 7,593 providers with a unique first name, last name, and city combination, we matched 2,459 providers with a probability 99.9, and 3,272 with a probability $\geq$ 67.5 and <99.9. We manually reviewed the remaining 1862 provider names from the Purdue data that were not matched, and matched an additional 186 providers. We assigned a posterior value of 10 to providers who we matched in the manual check, and a posterior value of -10 to providers that we were unable to match in the manual check. The type of match, that is at which round the match occurred or whether the match was manual, is noted in the match_type column. Reasons for failed matches include insufficient information, misspelled or inaccurate provider names, and multiple providers with the same name.

NPPES data includes codes for provider taxonomy. We joined the Health Care Provider Taxonomy Code Set (Version 23.0, released on 2023-01-01) to obtain the grouping, classification, and specialization associated with each taxonomy code. This code set is accessible through that National Uniform Claim Committee and is available for public use: https://www.nucc.org/index.php/code-sets-mainmenu-41/provider-taxonomy-mainmenu-40/csv-mainmenu-57

C Patient Attribution to Physicians

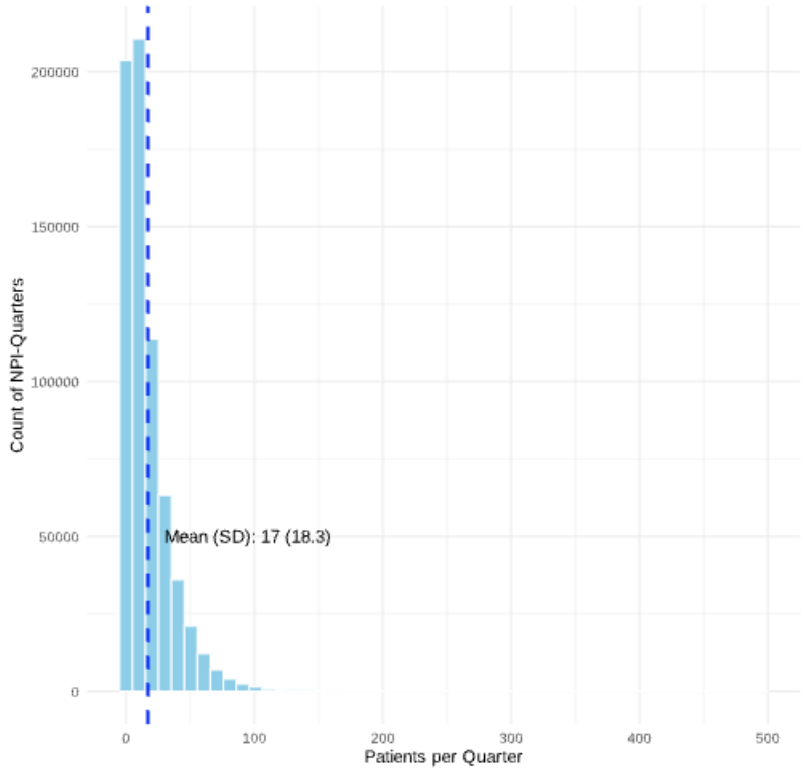
Because our version of the Part D Event (PDE) files does not contain the NPI of the provider who wrote the prescription for a given fill record, we attribute patients to providers that they had an outpatient evaluation & management (E&M) visit with during the calendar quarter. To do this, we create a distinct list of NPIs and patients for each calendar quarter (derived from the calendar month of visit). We then filter claims to include only E&M visits, based on the Current Procedural Terminology (CPT) code recorded as the procedure for that claim. Table A4 reports the specific codes used.

Appendix Table A4: CPT Values for E&M Visit

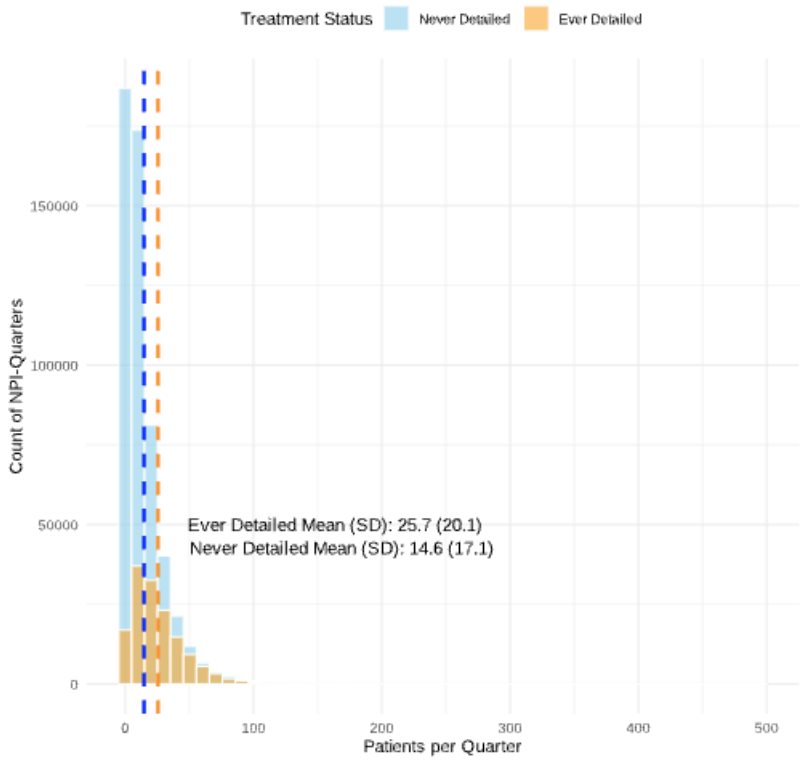
CPT Code	Description
99201 – 99205	Office/other outpatient visit for new patient
99211 – 99215	Office/other outpatient visit for established patient

Figure A24 plots the distribution of the number of patients a provider treats per quarter, overall and by providers detailing status.

Appendix Figure A24: Number of Patients per Quarter (Provider-Level)



(a) Overall



(b) By Detailing Status

TO ADD: number of providers per patient per quarter

TO ADD: how often does physician on the rx record after 2014 match up to the attributed physician?

D Opioid and OxyContin Use in Medicare, 2006 – 2018

This section provides additional context on prescribing/fill patterns and prescription drug coverage of opioids in Medicare during our study period.

The prescription drug benefit for Medicare, Part D, was added to Medicare on January 1, 2006, as part of the Medicare Prescription Drug, Improvement, and Modernization Act of 2003. The take-up of this benefit was gradual among both beneficiaries and providers over time [**CITE**]. Appendix Figure A25, panel a shows this empirically, with the number of distinct beneficiaires enrolled in Part D increasing from **XX** in 2006 Q1 to **XX** in 2018 Q4.

The number of beneficiaries with an opioid prescription increased over this same period (panel b). Coverage of opioids by Part D formularies (**DEFINE**) similarly increased from 2006 to 2015, though strategies like quantity limits and prior authorization were increasingly applied to restrict daily allowable prescribed dosing (Samuels et al., 2017).

Over this same period, opioid use among this population was drawing more scrutiny. By 2016 (at which point approximately one third of Part D beneficiaries received a prescription opioid), the Office of Inspector General reported that "protect[ing] Medicare beneficiaries[. . .]from prescription drug abuse" was a top priority in fighting the opioid crisis. While the most commonly prescribed opioids in 2016 were tramadol, hydrocodone-acetaminophen (which includes Vicodin), and oxycodone-acetaminophen (which includes Percocet), the most commonly prescribed opioid for the 501,008 beneficiaries who were flagged as receiving high amounts of opioids through Part D was oxycodone 30 mg, of which OxyContin is the brand name for a specific extended-release formulation Office of Inspector General (2014).

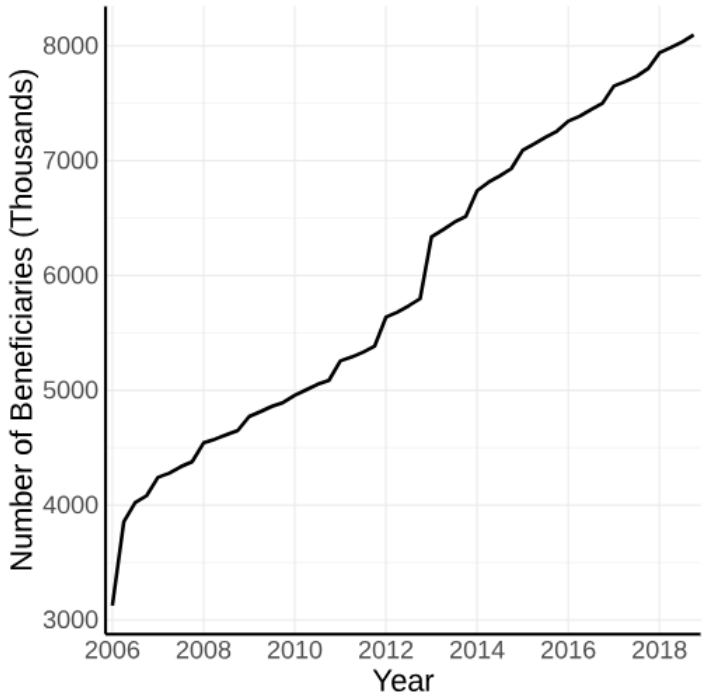
Opioid use in disabled Medicare benes (Morden et al., 2014)

E Sample Construction

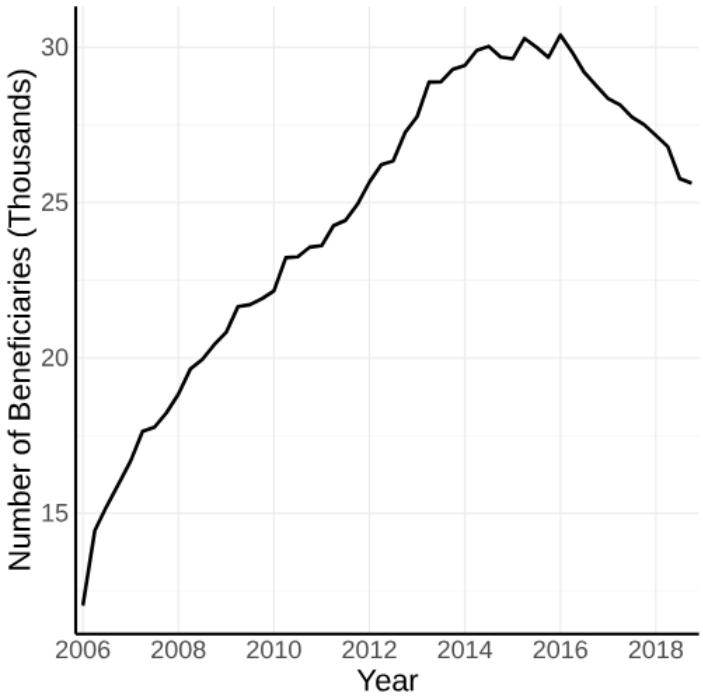
E.1 Physician-Level Sample

UNDER CONSTRUCTION

Appendix Figure A25: Beneficiaries in Part D by Quarter



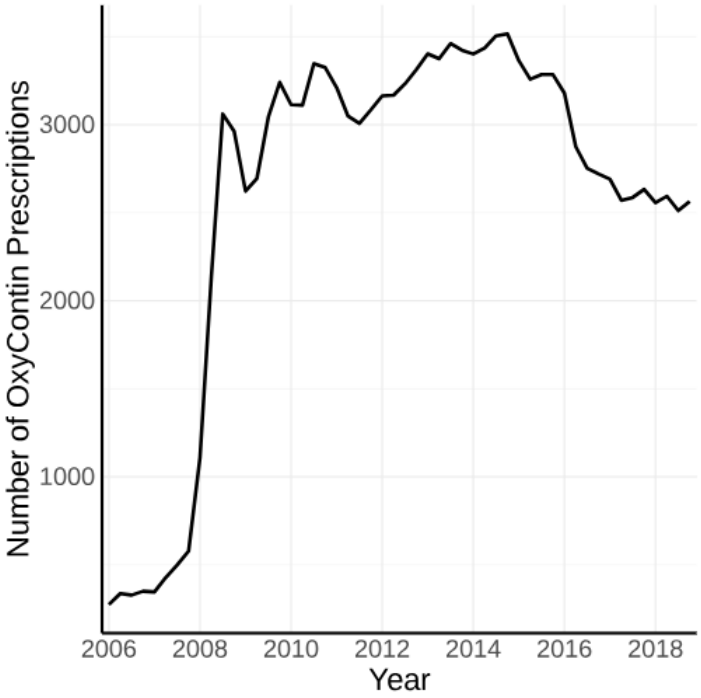
(a) Distinct Beneficiaries (20% National Sample)



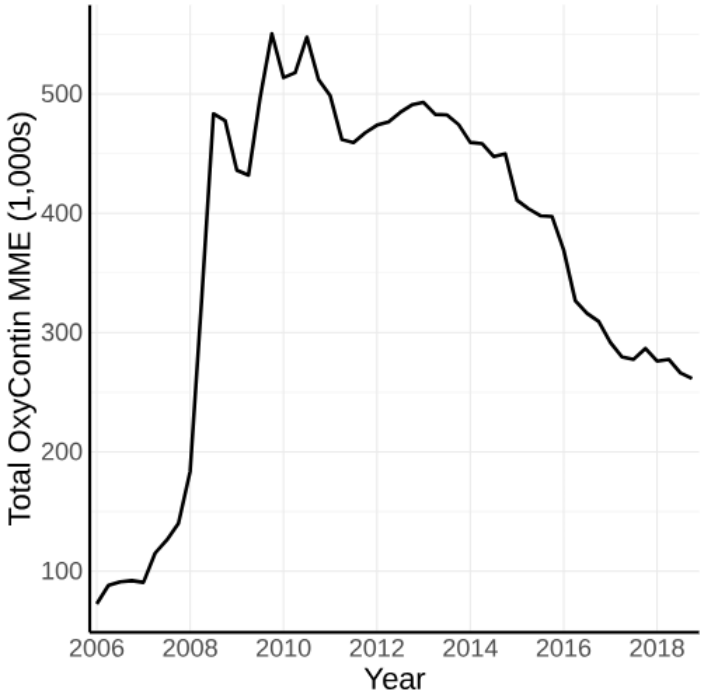
(b) Distinct Beneficiaries with Any Opioid Prescription (Study Sample)

Beneficiary counts in panel a are derived from the unrestricted 20% Part D Event file. Beneficiary counts in panel b represent in-sample Medicare beneficiaries assigned to providers in our sample. All measures are at the year-quarter level.

Appendix Figure A27: OxyContin Use Over Time



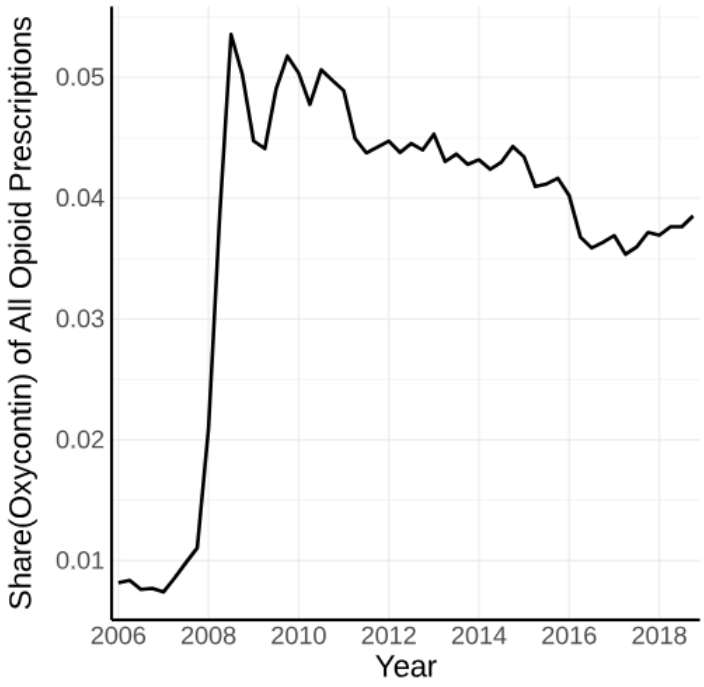
(a) Total Rx



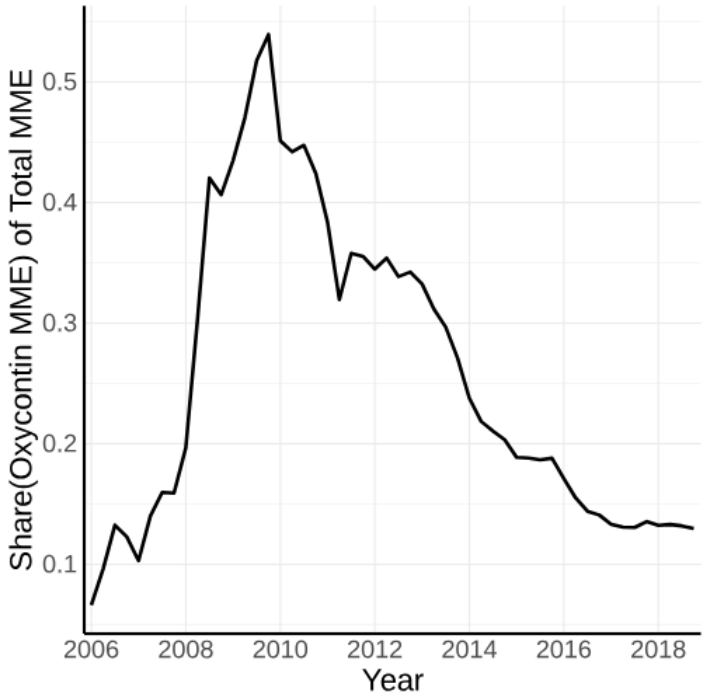
(b) Total MME

All measures are at the year-quarter level.

Appendix Figure A29: Share of Oxycontin Out of All Opioid Use Over Time



(a) Total Rx



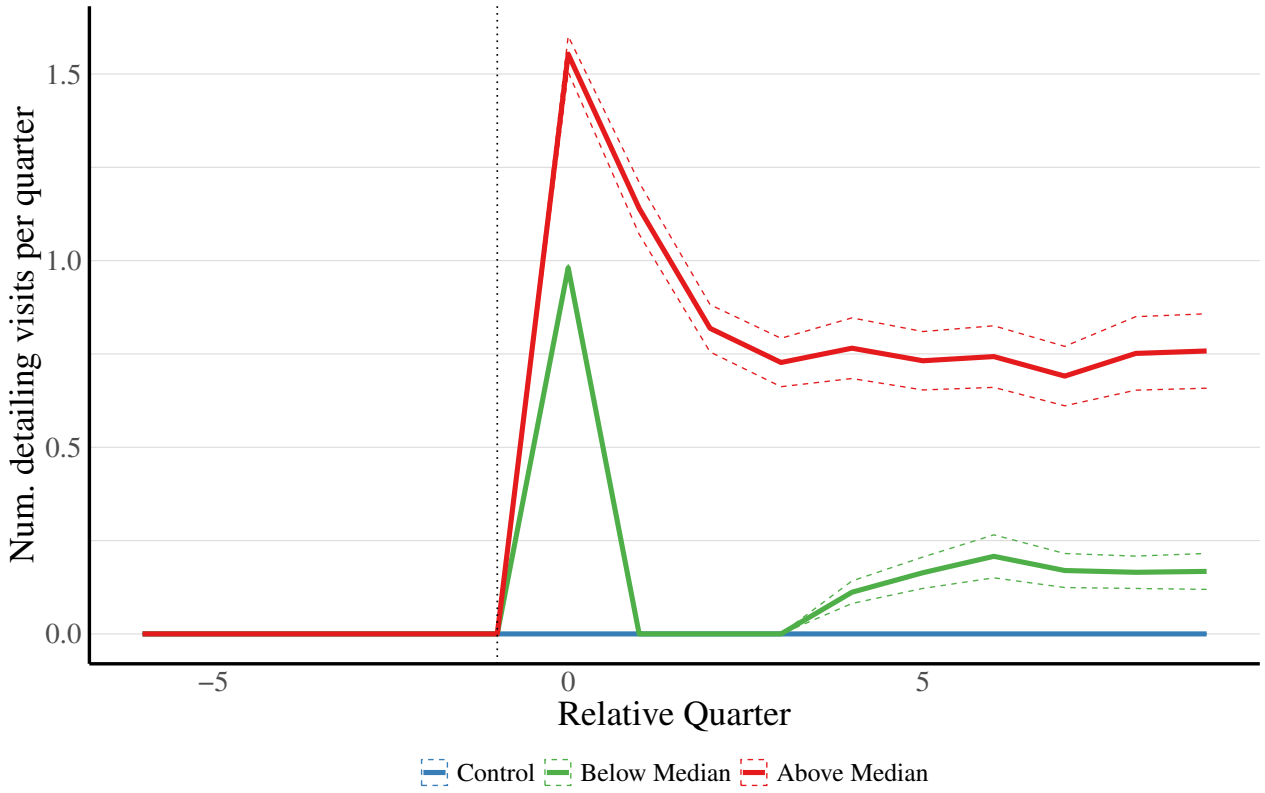
(b) Total MME

All measures are at the year-quarter level.

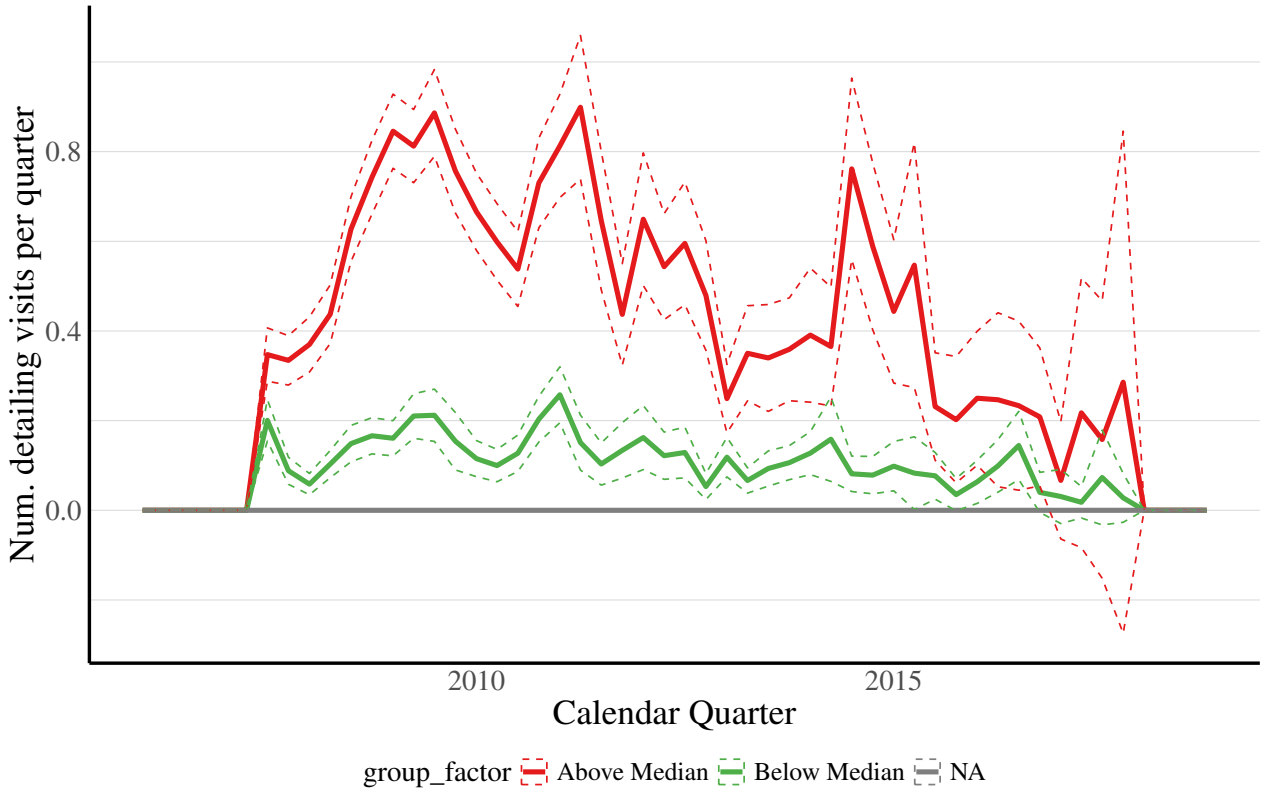
Appendix Table A5: Compare Characteristics of Matched and Unmatched Controls

Characteristic	Unmatched Sample	Matched Sample
Men (%)	59.1	63.3
MD/DO (%)	100	100
Specialty (%)		
Internal Medicine (%)	39	73.5
Family Medicine (%)	6.4	9.2
Orthopaedic Surgery (%)	3.7	4.5
Nurse Practitioner (%)	0	0
Anesthesiology (%)	1.3	0.4
Physician Assistant (%)	0	0
Physical Medicine & Rehabilitation (%)	0	0
Total Patients, Mean (SD)	10.3 (13.8)	15.9 (15.2)
Total Purdue Rx, Mean (SD)	3 (4.4)	4.5 (5)
Total Purdue MME, Mean (SD)	80.3 (173.8)	119.4 (187.2)
Total Opioid Rx, Mean (SD)	4.1 (5.9)	6.2 (6.7)
Total MME, Mean (SD)	91 (191.1)	134.9 (203.3)
Distinct Physicians (N)	19761	5184

Appendix Figure A31: Trends in Detailing Visits



(a) Relative to First Visit



(b) Trends over Time

E.2 Beneficiary-Level Sample

UNDER CONSTRUCTION

Beneficiaries

Key Providers

F Outcomes Construction

UNDER CONSTRUCTION

Total prescriptions per provider per quarter represents the total number of prescriptions filled by attributed patients in that quarter.

Appendix Table A6: Purdue-Brand Drugs

Brand Name	Generic Name	NDC
Butrans	Buprenorphine	
Hysingla ER	Hydrocodone Bitartrate	
Dilaudid	Hydromorphone Hydrochloride	
MS Contin	Morphine Sulfate	
Oxycontin	Oxycodone Hydrochloride	
Ryzolt	Tramadol Hydrochloride	

CHRONIC PAIN: https://share.google/zSchQdMYajHWnBcYd. Instead of three-year lookback period, we just allow all years

G Event Study Details

Callaway et al. (2024a)