The Detailing Margin: Opioid Marketing, Prescribing, and Patient Outcomes*

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Abstract

Direct-to-physician marketing ("detailing") features prominently in pharmaceutical sales strategies, yet studies leveraging geographic variation or payment disclosures provide an incomplete picture of how marketing intensity shapes individual provider behavior and patient outcomes. We link newly available Purdue Pharma detailing records to Medicare claims for Massachusetts physicians (2006–2018). Comparing physicians with varying detailing intensity, we find high exposure increases Purdue opioid prescriptions by 11% and patient-level opioid use by 4%. Prescribing responses plateau beyond moderate exposure, while downstream harms—a 22% increase in falls and fractures—concentrate at intermediate levels and among populations plausibly closest to the margin of opioid indication.

JEL classification: I11, I12, I18

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1 Introduction

Nearly three quarters of a million deaths in the US have been attributed to the ongoing opioid epidemic, 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, a central question is whether—and how—pharmaceutical marketing distorted healthcare provider behaviors in ways that fueled the crisis (Haffajee and Mello, 2017; Maclean et al., 2022; Van Zee, 2009). Among opioid manufacturers, Purdue Pharma—the manufacturer of OxyContin—stands out for the scale and intensity of its direct-to-physician marketing. From 1996 to 2000, Purdue more than doubled its sales force and spent nearly $200 million in its promotion of OxyContin in 2001 alone (Van Zee, 2009). Throughout this period and into the 2010s, Purdue sent representatives across the country to meet with healthcare providers in "detailing visits," 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 misleading claims led to a series of lawsuits resulting in Purdue's acknowledgment of its role in the opioid crisis and unprecedented financial settlements.

While a growing body of evidence links pharmaceutical marketing to increased opioid prescribing and adverse patient outcomes, our understanding of how this relationship operates remains incomplete. Studies leveraging geographic or temporal variation in marketing exposure have documented effects on prescribing and, in some cases, downstream harms including overdose mortality (Alpert et al., 2021; Arteaga and Barone, 2023; Hadland et al., 2019). By design, however, these studies operate at aggregated geographic levels and focus on the extensive margin of exposure—whether a region was targeted—rather than how variation in exposure intensity shapes individual provider behavior and patient outcomes. Other work has leveraged publicly reported financial disclosures to link within-provider variation in financial transactions to prescribing behaviors (Beilfuss and Linde, 2021; Carey et al., 2020), but these data capture only interactions involving reportable financial transfers, and miss routine detailing visits. Moreover, this literature focuses exclusively on prescribing, leaving the welfare question—whether marketing exposure harms patients—largely unaddressed.

This gap is particularly consequential given the ubiquity of direct-to-physician marketing—approximately 94% of physicians report some type of relationship with the pharmaceutical industry (Campbell et al.,

2007)—and the limited policy tools available to regulate it. While some academic medical centers and hospital systems have restricted (though not forbidden) on-site access for sales representatives, outright governmental bans on detailing face constitutional constraints on commercial speech (Sorrell v. IMS Health Inc., 2011). Existing regulations instead target marketing intensity through payment caps, disclosure requirements, and gift limitations (Grande, 2010; King and Bearman, 2017; Grassley, 2009).

We study this intensive margin directly using newly available records of tens of thousands of detailing visits between Purdue Pharma sales representatives and clinicians in Massachusetts from 2007–2017. These records were made public as part of a 2018 lawsuit in which the Commonwealth of Massachusetts accused Purdue and its executives of misleading physicians and the public about the risks of its opioid products (Office of the Attorney General of Massachusetts, 2024). We link these detailing records to administrative Medicare claims from Massachusetts to estimate the effects of marketing exposure on physicians' opioid prescribing and downstream patient outcomes, including falls, fractures, and indicators of opioid use disorder.[1] These data allow us to construct a more complete measure of marketing exposure than is available from public disclosure data and to examine how responses vary across the full distribution of detailing intensity. This heterogeneity is key to understanding the welfare implications of detailing: while prescribing increases are broadly similar across physicians with moderate to high detailing exposure, adverse patient outcomes are concentrated among physicians with intermediate exposure levels—not those most heavily detailed.

Using an event-study framework, we estimate how detailing exposure affects the volume and intensity of Purdue opioid prescriptions at the physician-level, capturing changes in prescribing across all of a physician's patients.[2] Consistent with our focus on the intensive margin, we compare post-detailing changes in prescribing between physicians with different levels of exposure to detailing, conditional on having been detailed at least once. Importantly, exposure intensity may not be randomly assigned: if representatives return more frequently to physicians whose prescribing increased, this would create feedback between treatment intensity and outcomes. We find suggestive evidence consistent with such feedback, though we cannot observe it directly. If present, this feedback itself is a downstream consequence of

[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.

detailing—and our estimates therefore capture the total causal effect of the "detailing ecosystem," inclusive of any dynamics that detailing initiates, rather than the marginal effect of an additional visit. An adaptation of the parallel trends test from Rambachan and Roth (2023) suggests that similar forces may operate in the post-period, reinforcing this interpretation.

Physicians with high detailing exposure experience an average increase of 0.762 Purdue opioid prescriptions per quarter (11.2% relative to the pre-detailing mean), driven primarily by a 6.8% increase in the number of patients receiving an opioid prescription rather than an increase in prescription intensity. The prescribing response emerges at moderate exposure and remains stable across higher levels: effects are small and imprecisely estimated at the lowest exposure level (2 visits, 5.2% increase), but substantial and broadly similar from moderate through high exposure (3–4 visits, 14.5%; 5–7 visits, 10.1%; 8–37 visits, 13.7%). As we show below, this relatively uniform prescribing response produces sharply divergent effects on patient welfare, raising questions not only about which physicians are most sensitive to marketing, but about which patients are most vulnerable to marketing-induced prescribing.

We complement this with a beneficiary-level analysis that holds the patient panel fixed, isolating changes in the treatment of patients already in a physician's practice prior to detailing. We find a statistically significant 7.4% increase in the probability of filling a Purdue opioid prescription at intermediate levels of detailing exposure. The welfare implications of increased opioid prescribing are *a priori* ambiguous: opioids carry risks of dependence and misuse, but can also be clinically appropriate for pain management.[3] To assess welfare directly, we examine opioid-related adverse events. We find no evidence that detailing affects indicators of opioid misuse, such as high-dosage prescribing or overlapping prescriptions.

However, we find substantial increases in falls and fractures—though these responses vary across the exposure distribution in ways that do not directly track prescribing effects. In the physician-level analysis, the largest effect occurs at 5-7 visits (33% relative increase), with smaller and insignificant effects at other exposure levels—even those with significant prescribing increases. The beneficiary-level analysis reveals a more nuanced pattern. Heterogeneity by exposure intensity does not show a clear correspondence between opioid use and harm; however, along other dimensions—particularly patient age and provider specialty—the subgroups exhibiting the largest increases in opioid use also experience the largest increases in falls and fractures.

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

Given this heterogeneity, we caution against drawing uniform conclusions about the welfare implications of detailing. However, the divergence between prescribing and harm across the exposure distribution sheds light on which patients are most vulnerable to marketing-induced prescribing. Physicians with the highest detailing exposure (8–37 visits) show significant increases in prescribing (13.7%) but no corresponding increase in falls and fractures. Descriptive evidence suggests these physicians treat patients with clearer clinical indications for opioid therapy—their patients are more likely to be disabled and less likely to have prior opioid-specific adverse events—offering one explanation for why additional prescribing at the top of the exposure distribution does not translate into harm. By contrast, physicians with intermediate exposure (5–7 visits) exhibit the largest increase in patient harm (33.2%) alongside a 10.1% increase in prescribing that is imprecisely estimated. This pattern is consistent with an interpretation in which even modest increases in prescribing among this group disproportionately affect patients at the margin of clinical indication—though we cannot rule out alternative explanations. At the beneficiary level, where patient composition is held fixed, this pattern is clearer along specific dimensions: younger beneficiaries and patients of non-primary-care physicians exhibit the largest increases in both opioid use and falls and fractures—suggesting that the welfare consequences of detailing depend critically on which patients receive the marginal prescription.

Our study makes several contributions to the literature. First, we extend work linking opioid manufacturers' marketing strategies to prescribing and health outcomes. Prior studies have linked Purdue's geographically targeted marketing of OxyContin to increased prescription opioid use, overdose mortality, and adverse birth outcomes in more heavily marketed regions (Alpert et al., 2021; Arteaga and Barone, 2023). By operating at the geographic level, however, these studies cannot explore heterogeneity in the individual provider- and patient-level responses to marketing—heterogeneity that proves central to understanding which patients are harmed by marketing-induced prescribing. We extend this work by directly measuring physician-level exposure to marketing and estimating its effects on individual provider behavior and downstream patient outcomes.

Second, we contribute to the literature documenting how financial and in-kind incentives from pharmaceutical firms shape provider behavior (Bergman et al., 2021; Carey et al., 2020; David et al., 2010; DeJong et al., 2016; Hadland et al., 2019; Larkin et al., 2017; Orlowski and Wateska, 1992; Sharma et al., 2018; Wazana, 2000). Most closely related are studies using detailed marketing records to estimate the effects of direct-to-physician detailing on providers' and their peers' prescribing of anticoagulants and antipsychotics (Agha and Zeltzer, 2022; Shapiro, 2018). We bring comparable data to the opioid setting,

where the stakes differ fundamentally: the marketed product in this context carries risks of addiction and downstream harms that extend well beyond the prescribing decision itself, even as it retains legitimate clinical uses. To our knowledge, no prior study has used physician-level detailing records to estimate the relationship between opioid marketing exposure and both provider prescribing and patient health outcomes.[4]

Third, our findings contribute to a growing literature on supply-side drivers of opioid use, particularly via provider prescribing behaviors. Patients treated by higher-prescribing providers tend to also use more opioids, even after accounting for selection (Barnett et al., 2017; Currie and Schwandt, 2020; Eichmeyer and Zhang, 2022, 2023; Staiger et al., 2022), and place-based factors—particularly local prescribing environments—account for substantial regional variation in opioid addiction (Finkelstein et al., 2022). Our findings identify pharmaceutical marketing as a specific, policy-relevant mechanism shaping the provider behavior that drives this variation.

The paper proceeds as follows. Section 2 describes the background of the Purdue detailing records used in this study, as well as an exploration of the characteristics of detailed providers. Section 3 describes the data and methods we use. Section 4 reports our main physician- and patient-level results examining Purdue opioid utilization and patient welfare, as well as associated robustness tests. Finally, Section 5 concludes.

2 Background

2.1 Purdue Marketing Strategies

Detailing visits—meetings between pharmaceutical sales representatives and healthcare providers—are designed to influence prescribing in favor of the manufacturer's products (Platforce, 2024). For Purdue, detailing visits represented an opportunity to directly reach healthcare providers and address their concerns about opioids while reshaping prevailing beliefs about the appropriateness and risks of opioid prescribing (Van Zee, 2009). Prior to the 1990s, concerns about addiction limited opioid use to acute pain or pain related to cancer (Food and Drug Administration, 2018; Meldrum, 2016). However, systematic

[4]Several studies (Beilfuss and Linde, 2021; Fleischman et al., 2019; Hadland et al., 2018, 2019) use Open Payments data from the Centers for Medicare & Medicaid (CMS), which capture only marketing interactions involving reportable financial transfers. As we discuss above, these data miss routine detailing visits—the primary physician-facing marketing channel (Donohue et al., 2007; Gagnon and Lexchin, 2008). We quantify the extent of unobserved interactions below.

efforts during this decade to encourage healthcare providers to address untreated pain—efforts led in part by opioid manufacturers—coincided with a rapid increase in opioid prescribing (Campbell, 1996; Centers for Disease Control and Prevention, 2024; Maclean et al., 2022; McGreal, 2018).

Purdue's OxyContin, launched in 1996 and marketed as a less-addictive alternative, became one of the most widely prescribed 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]

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.[6] In the early 2000s, Purdue's marketing practices became the subject of extensive legal scrutiny by states, counties, and municipalities.[7]

The 2018 Massachusetts lawsuit was unique in naming not only Purdue, 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). The lawsuit forced Purdue to make public the records of over 150,000 individual detailing visits between Purdue representatives and physicians occurring in Massachusetts from 2007 to 2017.[8] These records

[5]In the 2018 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 reported.

[6]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).

[7]In 2001, West Virginia sued Purdue for aggressive and deceptive marketing tactics of OxyContin, settling for $10 million (Thomas Jr., 2004). In 2007, Purdue pled guilty to criminal charges for misleading regulators, physicians, and patients about OxyContin's addictive potential, paying $635 million in fines—then 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).

[8]"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.

are archived online by the University of California, San Francisco (UCSF) and Johns Hopkins University (UCSF-JHU Opioid Industry Documents Archive, 2021).[9] In the next section, we use these data to characterize the timing and intensity of Purdue's detailing activity across Massachusetts providers.

The records capture marketing activity unavailable in public data. While the Physician Payments Sunshine Act requires manufacturers to report certain financial transactions made to physicians, it does not require disclosure of detailing visits that do not generate such transactions. In Appendix C, we compare Purdue detailing records to Open Payments and find that less than 40% of detailed physicians received a reportable Purdue payment in the same year, suggesting that detailing visits frequently occur without accompanying financial transfers.

2.2 Who Gets Detailed?

The detailing records include the name of the Purdue representative conducting the visit, the "target" provider (first and last name), the date the visit occurred, and the visit location. Targets include individual clinicians and organizations (such as pharmacies); in this analysis, we focus on detailing visits targeting individual physicians.

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 in Massachusetts. Detailing visits increased from 2007 - 2012, beginning to decline around the beginning of 2013.[10] Specifically, detailing visits in Massachusetts grew from between 500–1,000 per month in early 2007 to a peak of over 2,000 per month in 2011, before declining back to 2007-levels by the end of 2017. Similarly, distinct physicians detailed increased from 200-400 per month in 2007 to a peak of nearly 700 per month in 2011, before falling below 150 per month by late 2017.

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 geographic concentration of detailing visits mirrors the distribution of physicians, consistent with the

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

[10] These time periods correspond to three "waves" of the opioid epidemic (Humphreys et al., 2022; Maclean et al., 2022): the misuse of prescription opioids (wave 1: mid 1990's through 2010); the substitution to illegal opioids, such as heroin, in response to policies aimed at reducing access to prescription opioids (wave 2: 2010 —2013); and the introduction of synthetic opioids, such as fentanyl (wave 3: 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).

strategy of targeting provider-dense areas described in Purdue's marketing documents.[11] Appendix Figure A3 maps the share of Medicare-participating physicians detailed in 2011.

Appendix Table A2 compares characteristics of physicians who were ever detailed during our study period (3,322), compared with those who never received a detailing visit (29,980).[12] Detailed physicians were more likely to be male than never-detailed physicians (69% versus 60%, respectively)—a gender gradient observed in other studies examining direct-to-physician marketing (Rose et al., 2015)—and had more experience (average medical school graduation year: 1991 and 1998). 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. Detailed physicians treated approximately twice as many patients per month (14.8) as never-detailed physicians (8.8), on average.

Appendix B describes the detailing data cleaning process.

3 Methods

3.1 Data

We use two main data sources. Purdue Pharma detailing records, as discussed in Section 2, span May 2007 through December 2017. Our other primary data source is administrative billing claims records from a random 20% sample of Medicare beneficiaries enrolled in Traditional (fee-for-service) Medicare from 2006–2018.[13] To match the geographic scope of the Purdue records, we restrict our Medicare data to claims from Massachusetts.

One notable challenge relates to CMS' Part D Event (PDE) files, which contain records of prescriptions filled by Medicare beneficiaries. 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

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

[12] 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.

[13] Specifically, we use the Medicare 20%, Outpatient files (doi: 10.57761/qgsq-tj66); Provider Analysis and Review files (MedPAR; doi: 10.57761/nc92-5272); Enrollment/Summary, Master Beneficiary Summary Files (MBSF; 10.57761/wnn9-b060) files; Carrier files (doi: 10.57761/d1kp-xa44); and Part D files (doi: 10.57761/gd2g-8879).

claims files.[14] This prevents 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 all opioid prescriptions filled by that patient in that quarter to that provider.[15]

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. To assess how well this approach captures the prescribing provider, we compare the NPI on PDE records to the NPI assigned via attribution, finding a match rate of 19% for never-detailed physicians and 38% for ever-detailed physicians in our final analytic sample. We test the robustness of our results to using the prescriber NPI recorded in Part D claims instead of the attributed NPI, and discuss limitations of this approach below. Appendix E provides additional details.

To combine the Purdue detailing records with the CMS datasets, we used the National Plan and Provider Enumeration System publicly available downloadable files[16] to link physicians named on marketing records to their National Provider Identifier (NPIs) 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. We discuss how we handle the transition between physician identifiers (UPIN to NPI) in 2007 in Appendix E.

3.2 Sample Construction

Physician-Level Analysis. Our physician-level sample comprises two cohorts: "never-detailed" and "ever-detailed" physicians. The never-detailed cohort includes physicians who never receive a detailing visit and are affiliated with groups (as identified by the tax identification number [TIN] on the claim record) whose members are either never detailed or not yet detailed.[17] In our empirical strategy, never-detailed physicians contribute to the estimation of event-time effects through calendar-time effects. To construct secular trends relevant to our treated physicians, we select a matched sample of never-detailed

[14] 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. The organization through which we access this data does not have this crosswalk. Additional details can be found online at: https://resdac.org/cms-data/variables/prescriber-identification-number.

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

[16] See: https://download.cms.gov/nppes/NPI_Files.html

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

physicians based on provider-level covariates (see Appendix E), reducing the never-detailed sample from 19,761 to 5,362 physicians. Table A3 compares the matched and unmatched control samples.

The ever-detailed cohort includes physicians who receive a detailing visit at some point during the study period. Appendix Figure A4 plots the distribution of detailing visits received in the year following a physician's first visit. Our empirical approach uses the first detailing visit as the treatment event. Figure A5 shows the distribution of first visit timing, by calendar quarter. The concentration of first visits in early 2007 is partly mechanical, coinciding with the start of our data; many "first visits" in this period likely reflect left-censoring rather than true first visits. We therefore drop the 458 physicians whose first visit occurred in the first half of 2007. We also test robustness to excluding all physicians with first visits in 2007. Our final ever-detailed sample includes 2,076 physicians. Appendix Table A4 reports sample sizes at each restriction step.

We further divide our ever-detailed physicians into high- and low-exposure cohorts based on the number of detailing visits (on distinct days) received during a four-quarter "washout period" following the first visit (illustrated in Figure A6).[18] We define exposure at the quarterly level to maintain consistency with our empirical framework. In our final sample, 1,234 high-exposure physicians received 2–37 visits during the washout period, while 842 low-exposure physicians received one visit (Appendix Table A5).

The Purdue detailing records include visits with both physicians and non-physician clinicians (e.g., nurse practitioners, physician assistants). We restrict our analysis to physicians (MD/DO) given challenges in attributing care delivered by non-physicians in administrative claims data (Patel et al., 2022). Additional details on physician-level sample construction are provided in Appendix E.

Beneficiary-Level Analysis. Our beneficiary-level analysis requires observing all of a beneficiary's healthcare utilization during the study period. We therefore restrict our sample to beneficiaries continuously enrolled in Medicare Parts A (hospital insurance), B (medical insurance), and D (drug insurance), and not enrolled in Part C (Medicare Advantage), as our data include claims only for traditional fee-for-service beneficiaries.[19]

As in the physician-level analysis, we identify "never-treated" and "ever-treated" beneficiaries. Never-

[18] In our empirical framework, relative time r is defined with respect to the quarter of the first detailing visit ($r = 0$). The pre-period spans $r \in [-6, -1]$, the washout period $r \in [0, 3]$, and the post-period $r \in [4, 9]$, yielding six-quarter pre- and post-periods with a four-quarter washout.

[19] The physician-level analysis does not require this restriction, as that empirical strategy does not rely on continuous beneficiary-level observation.

treated beneficiaries are those whose providers are affiliated only with practices that have never had a detailed member or are not yet detailed at the time outcomes are measured. To be included in the final sample, never-treated beneficiaries must have continuous enrollment for at least 15 quarters (the full event study window). We construct a matched sample of never-treated beneficiaries (see Appendix E), yielding a final sample of 48,081 never-treated beneficiaries.

Ever-treated beneficiaries are those who received care from a detailed physician in the four quarters prior to that physician's first detailing visit.[20] For each such beneficiary, we assign a "key provider"—the clinician (physician or non-physician) responsible for the plurality of E&M visits during this period. Key providers average 4.10 E&M visits (SD: 3.43) with their assigned patients, and account for 56% (SD: 26) of total E&M visits during the identification period. We restrict our primary analysis to patients whose key provider is a physician; patients whose key provider is a non-physician are omitted.

We identify two subpopulations with elevated clinical indication for opioid use. First, we flag Social Security Disability Insurance (SSDI) recipients, who qualify for Medicare coverage due to disability. This population has been characterized as both more likely to receive opioid prescriptions and more likely to experience opioid-related overdose (Meara et al., 2016; Finkelstein et al., 2021). Second, we flag beneficiaries with a chronic pain diagnosis. Purdue promoted opioids for chronic pain management, referencing an excess of "untreated pain" that could benefit from long-term opioid therapy (Campbell, 1996; Van Zee, 2009), though subsequent research found opioids provided little benefit for chronic (non-cancer) pain (Busse et al., 2018). We identify chronic pain diagnoses using Chronic Condition Warehouse guidelines (Chronic Conditions Data Warehouse, 2025). To avoid endogeneity from detailing-induced diagnoses, we restrict this cohort to those who were diagnosed with chronic pain *prior* to their key provider's first detailing visit.

Additional details on sample construction are provided in Appendix E.

3.3 Empirical Approach

Our empirical goal is to estimate the causal effects of Purdue detailing on opioid prescribing and patient outcomes. Detailing may operate through two distinct but empirically intertwined channels. First, it may alter physicians' willingness to treat or retain patients with higher opioid demand, such as those with chronic pain or complex clinical profiles—a behavioral response operating through endogenous

[20] This approach is similar to Staiger (2022).

patient selection. Second, it may directly affect prescribing intensity, leading physicians to prescribe opioids more frequently or at higher doses to a given set of patients. Both pathways are policy-relevant and may operate simultaneously.

Our empirical strategy is designed to detect the presence and relative importance of these channels. We examine extensive-margin outcomes (total prescriptions, total MME, and the number of patients receiving opioids), intensive-margin outcomes (prescriptions and MME per patient), and changes in observable patient characteristics following a physician's first detailing visit. Together, these analyses allow us to assess whether increased opioid exposure arises primarily through changes in patient volume, shifts in patient mix, or changes in prescribing intensity conditional on a physician's patient panel. Accordingly, we do not interpret post-detailing changes in patient composition as confounding, but rather as a potential mechanism through which detailing affects prescribing.

We use an event study strategy that allows for dynamic treatment effects. We conduct analyses at the physician–quarter level for physician-level outcomes and at the beneficiary–quarter level for beneficiary-level outcomes. Our main estimating equation takes the form:

$$y_{pt} = \sum_{r=-6, r\neq -1}^{9} \beta_r \mathbb{1}[r_{pt} = r] \times \tau_p + \sum_{r=-6, r\neq -1}^{9} \gamma_r \mathbb{1}[r_{pt} = r] + \delta_t + \delta_p + \varepsilon_{pt} \tag{1}$$

where r_{pt} denotes relative time (the number of quarters since physician p's first detailing visit); τ_p equals 1 for high-exposure physicians (above-median detailing visits during the washout period) and 0 otherwise; δ_t and δ_p are calendar quarter and physician fixed effects, respectively; and ε_{pt} is an idiosyncratic error term. The pre-period spans $r \in [-6, -1]$, the washout period $r \in [0, 3]$, and the post-period $r \in [4, 9]$.

Our event study compares two treated groups with different exposure levels. Given this non-standard specification, we briefly discuss interpretation. The three physician cohorts—never-detailed, low-exposure (below-median detailing visits during the washout period), and high-exposure (above-median)—inform the estimates in distinct ways. Never-detailed physicians, lacking relative time variation, inform the calendar-time fixed effects δ_t, which capture secular trends in opioid prescribing. The γ_r coefficients capture the change in y at relative time r, relative to $r = -1$, for low-exposure physicians, net of secular trends. The β_r coefficients capture the additional change for high-exposure relative to low-exposure physicians at each relative time r. These β_r coefficients are our primary parameters of interest.

While our main specification uses OLS, we assess robustness to using Poisson estimation given that our

primary physician-level outcomes are non-negative count variables.[21] Our sample is balanced, requiring treated physicians to be observed throughout the event study window.[22] The beneficiary-level estimating equation is analogous to Equation 1 with outcomes at the beneficiary-quarter level.

To obtain average treatment effects (ATEs) across all post-period quarters, we also estimate a pooled specification:

$$y_{pt} = \alpha_1 \mathbb{1}[r \in [4,9]] \times \tau_p + \alpha_2 \mathbb{1}[r \in [4,9]] + \alpha_3 \mathbb{1}[r \in [0,3]] \times \tau_p + \alpha_4 \mathbb{1}[r \in [0,3]] + \delta_t + \delta_p + \varepsilon_{pt} \quad (2)$$

where r denotes relative time and the pre-period $r \in [-6,-1]$ is the omitted reference category. The coefficient α_1 captures the average differential effect of high versus low detailing exposure in the post-period. Pooling quarters increases precision relative to estimating separate quarter-specific effects. In all physician-level analyses, we cluster standard errors at the physician level; similarly, for all beneficiary-level analyses, we cluster standard errors at the beneficiary level.

3.4 Outcomes of Interest

We estimate our model for two broad classes of outcomes: opioid prescribing and patient harms. Changes in the volume of opioids prescribed (physician-level) or the probability of filling an opioid prescription (beneficiary-level) provide a "first stage" estimate of detailing's effect. The relationship between detailing and adverse health events provides a "reduced form" measure of downstream effects on patient welfare.

Opioid Prescriptions. Our outcome of interest is prescriptions for Purdue-manufactured opioids. However, direct identification of Purdue products in Part D Event files is not feasible for two reasons. First, brand identifiers do not reliably distinguish branded from generic products. Second, and more fundamentally, Massachusetts is a mandatory generic substitution state: pharmacies must dispense generic equivalents when available unless the prescriber specifies "dispense as written." As a result, even when a physician prescribes a Purdue-branded product (e.g., OxyContin), the dispensed product—and thus the National Drug Code recorded in claims data—will typically reflect a generic formulation. We therefore construct a measure of "Purdue-associated opioids" by flagging prescriptions for molecules

[21] Mullahy and Norton (2024) emphasize that common transformations of skewed, non-negative outcomes involve non-trivial tradeoffs, particularly in the treatment of zeros. Consistent with their recommendations, we retain outcomes in levels and demonstrate robustness across OLS and Poisson specifications.

[22] We allow physicians to have zero Medicare beneficiaries in some quarters within this range (filling outcome values with zeros); the balance restriction requires only that a physician's first and last observed quarters span the full window.

marketed by Purdue during our study period (Appendix Table A11). This approach captures the prescribing behavior that detailing aimed to influence, regardless of which manufacturer's product was ultimately dispensed. Additional details are provided in Appendix F.1.

At the physician level, we construct several extensive- and intensive-margin measures. Our primary extensive-margin outcomes are total Purdue-associated opioid prescriptions filled by attributed patients in a quarter, log-transformed MME[23], and the number of patients with at least one Purdue-associated opioid prescription. On the intensive margin, we measure prescriptions per patient, MME per patient (log-transformed), and the share of patients with at least one Purdue-associated opioid prescription. At the beneficiary level, we construct a binary indicator for filling a Purdue-associated opioid prescription in a given quarter and total MME (log-transformed).

Adverse Health Events. The impact of detailing on patient health is theoretically ambiguous. Opioids are effective for treating acute and cancer-related pain, though evidence for chronic non-cancer pain is less compelling (Maclean et al., 2022; Van Zee, 2009). At the same time, opioids are associated with addiction, overdose, and other adverse effects.

We construct two measures of adverse health events linked to opioid use. First, we flag emergency department (ED) or inpatient claims with diagnosis codes for non-fatal opioid overdose or injection drug use-related infection, following Barnett et al. (2023). Second, we flag ED or inpatient claims for falls or fractures, which prior literature links to opioid use among elderly patients (Solomon et al., 2010; Yoshikawa et al., 2020). Details on diagnosis codes and claim identification are provided in Appendix F.2.

Negative Control Outcomes. We use a set of negative control outcomes to test the validity of our empirical approach (Danieli et al., 2025). If an unobserved contemporaneous shock were driving changes in physicians' general prescribing behavior—rather than detailing-specific responses—we might expect to observe relative increases in prescriptions of unrelated medications within our event study framework. To isolate prescribing behavior from potential changes in patient volume, we define negative control outcomes as the *share* of patients receiving a given medication class, paralleling one of our main outcomes of interest (share of patients receiving Purdue opioids).

[23] We apply $\ln(MME + 1)$ due to the long right tail in the distribution; see Figure A8. While Mullahy and Norton (2024) caution against log-transforming variables with many zeros, we adopt this approach for ease of interpretation of this non-primary outcome.

Patient Characteristics. To assess whether detailing affects patient composition, we estimate our event study for physician-quarter patient characteristics: total patients, demographics (sex, race, age categories), enrollment characteristics (dual eligibility, SSDI receipt), and clinical characteristics (cancer, any chronic condition). We do not control for patient composition in the physician-level analysis, as we consider compositional changes to be part of the treatment effect.

3.5 Threats to Identification

Selection into Treatment. A key challenge facing studies that seek to identify the impact of opioid detailing on opioid use is that pharmaceutical representatives did not randomly target physicians. Industry documents and prior investigations into marketing plans for OxyContin revealed that pharmaceutical representatives identified and targeted providers who prescribed the largest volume of opioids (Van Zee, 2009). This selection concern primarily affects comparisons between ever-detailed and never-detailed physicians. Our empirical strategy sidesteps this issue: we compare physicians with high versus low detailing exposure, conditional on being detailed. Never-detailed physicians enter our analysis only to inform calendar-time fixed effects, not as a primary comparison group.

Our identifying assumption is that, absent differential exposure to detailing, opioid prescribing among high-exposure physicians would have evolved similarly to prescribing among low-exposure physicians. While this assumption is inherently untestable, we assess its plausibility by examining pre-period coefficients in our event study; we discuss this in Section 4.

A related concern is that patients may sort to detailed physicians based on unobservable characteristics—for instance, if opioid-seeking patients learn which physicians are more willing to prescribe. This would bias our physician-level estimates if patient composition shifts endogenously in response to (or anticipation of) changes in prescribing behavior. Our beneficiary-level analysis partially addresses this concern by restricting attention to patients already attributed to a physician at baseline, holding the patient panel fixed. Additionally, we examine whether detailing is associated with changes in observable patient characteristics (Section 4); significant compositional shifts would suggest that unobservable sorting may also be present.

Reverse Causality. In our intensive-margin comparison, the primary identification concern is reverse causality; namely, that detailing intensity may respond endogenously to prescribing behavior. If repre-

sentatives return more frequently to physicians who prescribe more opioids, our estimates would reflect both the causal effect of detailing on prescribing and feedback from prescribing to detailing intensity.

We address this concern through two complementary steps. First, we define detailing exposure based on visits during a four-quarter washout period following the first visit, then estimate effects in a separate post-period. This design concentrates the most direct feedback dynamics in the washout period rather than the period used for our primary estimates. Empirically, exposure is persistent: physicians with high exposure in the washout period tend to remain high-exposed thereafter (Figure A7). We cannot rule out that feedback dynamics continue into the post-period, but separating the exposure-defining period from the estimation period mitigates the most immediate reverse causality concerns.

Second, we apply the sensitivity analysis framework of Rambachan and Roth (2023) to assess the extent to which the same forces driving the effects estimated in the washout period can account for the effects estimated in the post-period. Under the assumption of no deviation from parallel trends in the post-period ($\bar{M} = 0$), the average post-period effect remains statistically significant (95% CI: [0.31, 1.52]). However, allowing post-period deviations of just 25% of the largest washout-period deviation ($\bar{M} = 0.25$) yields confidence intervals that include zero. Details are provided in Appendix G. We interpret this sensitivity as reflecting a fundamental challenge in settings where treatment intensity responds to outcomes: we cannot fully isolate the causal effect of detailing from feedback-driven dynamics. Accordingly, the policy-relevant effect we estimate may be more appropriately interpreted as reflecting a "treatment ecosystem" in which both causal and feedback-driven mechanisms coexist, rather than a point-identified structural effect of detailing alone. Importantly, this conclusion reflects a challenge of point identification rather than evidence of a null effect.

Measurement Error in Prescription Attribution. Prior to 2014, prescriber NPIs are not recorded in Part D Event files, requiring us to attribute prescriptions to physicians based on E&M visit patterns. As discussed in Section 3.1, this attribution approach yields modest match rates between attributed physicians and recorded prescribers in the post-2014 period (19–38%). This low concordance likely reflects both true misattribution and the prevalence of team-based care, in which multiple clinicians prescribe for the same patient.

The implications for our estimates depend on whether misattribution patterns are stable over time within exposure groups. In a difference-in-differences framework, level differences in attribution quality

across groups do not bias estimates provided that these differences are constant over time—the within-group differencing removes time-invariant measurement differences. However, if attribution quality changes differentially over time across exposure groups, this could bias our estimates. For instance, if high-exposure physicians' prescriptions become more likely to be correctly attributed in the post-period (relative to low-exposure physicians), we could overstate the treatment effect.

To assess whether trends in misattribution differ by exposure group, we compare match rates across high-exposure, low-exposure, and never-detailed physicians in the post-2014 period (Appendix Table A1). Match rates are highest for high-exposure physicians (43%) and lowest for never-detailed physicians (19%), with low-exposure physicians in between (34%). This pattern is consistent across years. We interpret the stability of the match rates across exposure as suggesting that systematic changes in attribution quality are unlikely to drive our main findings. To further assess robustness, we re-estimate our main specifications using prescriber NPIs recorded directly in Part D claims, bypassing the attribution procedure; we discuss this exercise in Section 4.4.

Staggered Treatment Timing. Physicians receive their first detailing visit at different calendar times. As a result, conventional two-way fixed effects designs may implicitly compare newly-treated physicians to already-treated physicians, potentially biasing estimated effects (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). We assess sensitivity to this concern using a cohort-based approach in Section 4.4.

4 Results

4.1 Physician-Level Analysis

The physician-level analysis compares changes in prescribing outcomes among physicians with high exposure to detailing relative to physicians with low exposure, where the event of interest is the first detailing visit a physician receives from a Purdue Pharma representative.

4.1.1 Sample Characteristics

Table 1 reports characteristics of physicians in our analytical sample by detailing exposure. There are 5,362 matched never-detailed and 2,076 ever-detailed physicians, of which 842 have low exposure and

1,234 have high exposure. Time-varying measures for ever-detailed physicians are calculated over the four quarters prior to their first detailing visit; measures for never-detailed physicians are calculated across all observed quarters.

By construction, never- and ever-detailed physicians are broadly comparable on observable characteristics, though some differences remain. Physicians in both groups are predominantly male and concentrated in internal medicine, though never-detailed physicians are somewhat more likely to specialize in internal medicine. Demographic and specialty distributions are similar across the detailing gradient.

Pre-detailing prescribing patterns suggest that Purdue representatives targeted higher-volume prescribers, consistent with strategies referenced in industry documents. Ever-detailed physicians treat more patients per quarter and write more Purdue opioid prescriptions than their never-detailed counterparts. More notably, this pattern extends along the detailing gradient: high-exposure physicians have modestly higher patient volumes and opioid prescribing than low-exposure physicians—particularly with regards to the potency of prescriptions (as captured by MME)—suggesting that detailers not only targeted higher prescribers but returned more frequently to the highest prescribers among those they visited. As noted in Section 3.5, we consider implications of potential bias introduced by this selection into treatment and identify empirical opportunities to explore robustness.

Table A6 describes the patients treated by in-sample physicians. Demographically, patients are broadly similar across exposure groups: patients are predominantly female, White, and aged 64–68. Patients of detailed physicians are less likely to be dually eligible for Medicaid or to be SSDI recipients than patients of never-detailed physicians, though these characteristics do not vary meaningfully along the detailing gradient. Clinically, patients are very similar across groups, with the vast majority having at least one chronic or potentially disabling condition. Fewer than 1% of patients experience an OUD event or a fall or fracture during the study period, consistent with rates reported in prior work (Davenport and Matthews, 2018; Solomon et al., 2010). To the extent that we find significant effects of detailing associated with these outcomes, their infrequency makes those effects all the more notable.

Descriptive Trends. Figure 1 plots unadjusted trends in Purdue opioid prescriptions by exposure group, centered on the first detailing visit (never-detailed physicians are assigned placebo event times). Pre-period trends appear roughly parallel across groups, with some indication of diverging trends among the ever-detailed cohorts relative to the never-detailed cohort. Opioid prescriptions in

the post-period—particularly in later quarters—suggest greater growth among high-exposure physicians relative to the other cohorts. We next estimate Equation 1 to formally test whether these patterns are statistically meaningful and whether pre-trends satisfy the parallel trends assumption.

4.1.2 Main Effects on Prescribing

Figure 2 plots the estimated coefficients from Equation 1 for total Purdue opioid prescriptions per physician-quarter, our primary outcome. Pre-period coefficients are relatively flat and not significantly different from zero, supporting the parallel trends assumption. The grayed-out period (relative quarters 0–3) represents the washout period used to define exposure; the relevant post-period begins in relative quarter 4.

In the post-period, we observe a relative increase in Purdue opioid prescriptions among high-exposure physicians. This effect grows over time; while not statistically significant in the early post-period, we observe approximately one statistically significant additional prescription per quarter by the end of the event study window.

Panel A of Table 2 reports coefficients obtained from estimating Equation 2. Relative to low-exposure physicians, high-exposure physicians prescribe an additional 0.404 and 0.766 Purdue opioid prescriptions per quarter in the washout- and post-periods, respectively (column 1), corresponding to a 6% and 11.2% increase relative to the pre-treatment mean. Total MME increases by approximately 2% and 10% in the washout- and post-periods, respectively, though neither effect is statistically significant (column 2). Finally, the number of patients with at least one Purdue prescription significantly increases by 0.211 patients (9%) in the post-period, and by 0.09 patients (4%) in the washout period, though the washout-period effect is not statistically significant (column 3).

Taken together, these extensive margin results suggest that detailing exposure increases physicians' prescription of the target drug in both volume and the number of patients receiving a prescription. 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. Either mechanism is consistent with the significant extensive-margin effects we observe in columns (1) and (3). To gain further insight into the relative importance of these mechanisms in our setting, we next turn to the intensive margin outcomes.

Panel B of Table 2 examines whether the observed increase in prescription volume reflects changes in

prescribing intensity per patient. We find small and statistically insignificant effects on prescriptions per patient (column 4), MME per patient (column 5), and the share of patients with a Purdue prescription (column 6). The event study coefficients in Figure A9 are consistent with these null findings. This pattern suggests that the extensive-margin increase in prescription volume is not driven by intensified prescribing to existing patients, but rather by other mechanisms—such as changes in patient composition—which we examine in Section 4.1.5.

4.1.3 Dose-Response Heterogeneity

A natural question in this setting is whether additional detailing visits produce additional prescribing—that is, whether the relationship between exposure and response is monotonic. If detailing operates through information provision or relationship-building that accumulates with repeated contact, we might expect larger effects among more intensively detailed physicians. Alternatively, if a single visit is sufficient to shift prescribing, additional visits may yield diminishing returns from the manufacturer's perspective. To examine whether prescribing responses vary with detailing intensity, we re-estimate our model separately for increasingly intensive levels of detailing exposure among high-exposure physicians. Specifically, we partition the high-exposure sample into quartiles based on detailing volume in the washout period, and estimate the model within each quartile, continuing to compare treated physicians to the same low-exposure and never-detailed control groups.

Figure 4 plots the resulting quartile-specific average treatment effects, with percent changes relative to each bin's pre-treatment mean shown above the point estimates. An interesting pattern emerges: while a linear dose-response relationship would predict increasing effects across quartiles, we instead observe a "plateau" of effects beyond the first quartile. Specifically, while the first quartile (2 detailing visits in the washout period) effects are not significant, both relative effects and point estimates in the remaining quartiles are very similar, with relative increases in Purdue opioid prescriptions between 10–15%. Notably, while these effects are statistically significant in quartiles 2 (3–4 visits) and 4 (8–37 visits), they are not significant in quartile 3 (5–7 visits). While it's unclear whether this lack of significance reflects meaningful heterogeneity versus sampling variability, the point estimate (10.1%) is comparable to quartiles 2 and 4, suggesting the pattern is consistent with a plateau rather than a true null effect.

Pre-treatment Purdue opioid prescribing also varies systematically with detailing exposure groups, though in a pattern that is more monotonic. This pattern is consistent with pharmaceutical repre-

sentatives targeting physicians based on baseline opioid prescribing volume, suggesting selection into higher detailing intensity based on pre-existing prescribing behavior.

To explore this further, we compare physician and patient characteristics across exposure quartiles (Table A7). Physicians are uniformly more likely to be male and to have a primary care specialty across all quartiles, with the share of primary care physicians increasing modestly with detailing volume (consistent with stated targeting strategies in industry documents). Patient characteristics are largely balanced, with one notable exception: pre-treatment prevalence of falls/fractures and OUD events *declines* as detailing exposure increases—opposite to what might be expected if representatives targeted physicians with higher-risk patient panels. This pattern counteracts the concern that detailers selectively targeted physicians with higher-risk patient panels; if anything, the opposite appears to be true, which strengthens the interpretation that adverse health effects we observe downstream reflect detailing-induced changes in prescribing rather than pre-existing patient risk.

4.1.4 Heterogeneity by Physician Characteristics

Purdue's internal documents reveal explicit strategies for targeting specific physician populations, such as primary care providers, who manage a large share of chronic pain patients (Van Zee, 2009). Whether these targeting strategies translated into differential behavioral responses is an open question. Physicians who were targeted may have been more receptive to marketing, or they may have had less room to increase prescribing if they were already near capacity. We examine heterogeneity along several clinician dimensions to shed light on this question.

Figure A10 reports subgroup-specific average treatment effects obtained from estimating Equation 2. Male physicians exhibit larger and more precisely estimated responses than female physicians, though sample size differences likely contribute to the imprecision of female physician estimates. Physicians with non-primary-care specialties show responses nearly twice as large as primary care physicians, though these effects are less precisely estimated. Thus, while PCPs may have been more heavily targeted, the larger response among non-PCP physicians either suggests that marketing was more effective to this physician population or that their patient populations—often already receiving opioids for surgical or musculoskeletal conditions—offered greater margin for Purdue-specific prescribing.

Notably, the calendar timing of a physician's first detailing visit does not meaningfully mediate the treatment effect. Despite increasing public awareness of opioid-related harms and an evolving policy

environment, physicians whose first visit occurred in the later part of our study (2012–2016) were at least as responsive—and if anything, more responsive—than physicians detailed earlier (2007–2011). This persistence suggests that detailing remained effective even as the broader environment shifted.

4.1.5 Patient Composition

The extensive-margin increase in prescriptions, combined with null intensive-margin effects, raises the question of whether detailing alters physicians' patient composition. We examine this by estimating Equation 1 using patient characteristics aggregated to the physician-quarter level as outcomes.

Figures A11–A14 report the results. We find little evidence that high-exposure physicians meaningfully alter the observable composition of patients they treat. Total patient volume remains stable, and we observe no significant changes in the demographic or clinical mix of patients—though point estimates suggest modest, statistically insignificant shifts toward younger and SSDI-recipient patients, and away from dually enrolled patients.

Overall, we find little evidence that high-exposure physicians meaningfully change the observable composition of patients they treat. Two caveats apply. First, our tests examine only a limited set of observable characteristics; changes in unobservable dimensions of patient mix cannot be ruled out. Second, the null finding on patient volume is consistent with modest increases in opioid prescribing that are small relative to physicians' overall panels and therefore do not generate detectable compositional shifts. To provide additional insight, we turn to the beneficiary-level analysis, which holds patient characteristics fixed by design.

4.2 Beneficiary-Level Analysis

The physician-level analysis establishes that detailing increases prescription volume, but leaves open the question of *which* patients receive additional opioids. If high-exposure physicians expand their patient panels or selectively increase prescribing to certain patient types, the welfare implications differ from a scenario in which existing patients receive more prescriptions. The beneficiary-level analysis addresses this by examining within-beneficiary changes in opioid use, comparing beneficiaries whose key provider (the physician responsible for the plurality of their E&M visits) has high detailing exposure to those with low-exposure key providers. This design holds patient characteristics fixed, isolating the effect of one's physician being detailed from compositional changes in physician panels.

4.2.1 Sample Characteristics

Table 3 reports beneficiary and key provider characteristics. Panel A presents beneficiary characteristics. Our final sample includes 48,081 never-treated beneficiaries and 16,027 ever-treated beneficiaries, of which 5,612 have low-exposure key providers and 10,415 have high-exposure key providers. Beneficiaries are highly similar across exposure groups. The majority are female with an average age of 68, and approximately 90% are white. SSDI receipt is slightly higher among the ever-detailed cohort (24–25% relative to 22% in the never-detailed cohort), and nearly all patients (99%) have a chronic pain diagnosis at some point during the study, with approximately 90% had such a diagnosis prior to their key provider's first detailing visit.[24] Pre-detailing Purdue opioid use is balanced across groups by construction. Any-opioid use (including non-Purdue products) is modestly lower among beneficiaries with ever-detailed key providers (63% vs. 70%), likely reflecting differences in non-Purdue prescribing patterns among physicians selected for detailing.

Panel B describes key provider characteristics. Key providers are responsible for over 60% of beneficiaries' total E&M visits in the pre-period. The majority (87–90%) of beneficiaries have a key provider with a primary care specialty.

4.2.2 Main Effects on Opioid Use

Figure 5 plots event study coefficients for the probability of filling a Purdue opioid prescription, estimated separately for the full sample (panel a), beneficiaries with pre-existing chronic pain (panel b), and SSDI recipients (panel c). Pre-period coefficients are positive but generally not significantly different from zero.[25] Post-period effects are dynamic: in the full sample, the probability of filling a Purdue prescription increases to approximately 1.6 percentage points by event time 6 (statistically significant), before declining toward the end of the horizon.

Table 4 summarizes these effects using the pooled specification (Equation 2). Relative to beneficiaries with low-exposure key providers, those with high-exposure key providers experience a 0.5 percent-

[24]The near-universal prevalence of chronic pain likely reflects both the broad Chronic Condition Warehouse definition and the "ever" measurement window. Because chronic pain status is used neither to define treatment nor to stratify outcomes, this does not affect our identification strategy, but it does limit the usefulness of this characteristic for assessing balance.

[25]Descriptive trends (Figure A15) reveal that both treated groups exhibit declines in Purdue opioid use around event time -1, with a more pronounced drop among low-exposure beneficiaries; the control group shows no such pattern. Because regression coefficients compare high- versus low-exposure beneficiaries relative to event time -1, this differential dip generates positive pre-period coefficients even absent divergent pre-trends. Re-estimating with event time -2 as the reference period (Figure A25) confirms that post-period effects remain positive, though imprecisely estimated.

age point increase in the probability of filling a Purdue prescription—a 4.3% increase relative to the pre-period mean of 11.5%. Among beneficiaries with pre-existing chronic pain, we estimate a 4.7% relative increase; among SSDI recipients, a 3.1% increase. These effects are consistently positive and economically meaningful, though none are statistically significant at conventional levels in the pooled specification. Effects on prescription strength (MME) are small and similarly imprecise.

One potential explanation for the lack of precision is heterogeneity in treatment effects across beneficiary subgroups. If detailing-induced prescribing changes are concentrated among specific patient populations, the average effect may be attenuated. We explore this possibility in the next section.

4.2.3 Dose-Response Heterogeneity

We estimate Equation 2 separately for beneficiaries grouped by their key provider's detailing exposure during the washout period, using the same four bins as in the physician-level dose-response analysis (2, 3–4, 5–7, and 8–37 visits). Panel (a) of Figure A16 reports the resulting average treatment effects by bin, with percent changes relative to each bin's pre-treatment mean shown above the point estimates. Bin-specific pre-treatment means and beneficiary counts are reported on the y-axis.

Panel (a) reveals a non-monotonic relationship between detailing exposure and the likelihood that existing patients fill a Purdue opioid prescription. Effects are concentrated in the middle of the detailing distribution: beneficiaries whose key providers receive 3–7 visits are approximately 7% more likely to fill a Purdue prescription (marginally significant). By contrast, effects for beneficiaries with the lowest-intensity (2 visits) or highest-intensity (8–37 visits) key providers are small and statistically indistinguishable from zero. Panel (b) reports a similar non-monotonic pattern for dosage intensity, with the largest increases in the middle of the distribution (2–3%), though these effects are not statistically different from 0.

Two features of these results shed light on mechanisms. First, pre-treatment opioid use in both probability and dosage is similar across the first three exposure bins (10–11% probability of use, average MME of 16–18 mg), but notably higher in the top bin (14% probability of use, average MME of 142 mg). This pattern is consistent with baseline differences in patient populations across providers with varying levels of detailing exposure and pre-detailing opioid prescribing, potentially reflecting selection of higher-opioid-use patients into the practices of providers with higher baseline prescribing.

Second, the lack of a significant effect for beneficiaries with the highest-exposure key providers suggests

that the additional prescribing observed among these physicians in Figure 4 does not operate through increased probability that existing patients initiate or continue Purdue opioids. Instead, physician-level increases may reflect prescribing to new patients or to patients for whom the detailed physician is not the key provider.

4.2.4 Heterogeneity by Patient and Provider Characteristics

Figures A17 and A18 examine heterogeneity in treatment effects by beneficiary and key provider characteristics, respectively.

Beneficiary Characteristics. Figure A17 reveals substantial heterogeneity. While effects are similar by sex, they differ meaningfully by race and age. Among White beneficiaries, the estimated effect is small (2.4%) and not statistically significant. Among Black beneficiaries, the effect is substantially larger (18.5%) and statistically significant, though imprecisely estimated due to smaller sample size. We also find pronounced heterogeneity by age: the effect among younger beneficiaries (below-median age) is 5.8%, while the effect among older beneficiaries is close to zero (0.8%). Because the younger Medicare population disproportionately consists of individuals qualifying through disability, this pattern is consistent with opioid prescribing being more responsive to physician discretion in populations where opioid use may be more clinically indicated.

Key Provider Characteristics. Figure A18 shows that beneficiaries whose key provider has a non-primary-care specialty experience substantially larger effects (8.4%) than those with primary care key providers (2.6%), though neither estimate is statistically distinguishable from zero. Effects are modestly larger for beneficiaries with at least three pre-period visits with their key provider (5.0%) compared to those with fewer visits (4.0%), while the share of pre-period visits accounted for by the key provider does not meaningfully mediate the effect.

4.3 Adverse Health Events

The preceding results establish that detailing increases opioid prescribing and use. Whether this additional prescribing improves or harms patient welfare depends on clinical context: for patients with under-treated pain, increased access to opioids may be beneficial; for patients at risk of opioid dependence or adverse events, additional exposure may cause harm. We cannot directly adjudicate clinical

appropriateness, but we can examine whether detailing-induced prescribing is associated with downstream adverse events—specifically, opioid overdose, injection-related infection, and falls or fractures.

Main Effects. Table 5 reports estimated effects on opioid-related adverse events at both the beneficiary level (Panel A) and physician level (Panel B). At the beneficiary level, we find no evidence that high-exposure key providers increase the probability of overdose, infection, falls, or fractures among their existing patients. Point estimates are small and statistically indistinguishable from zero. At the physician level, we similarly find no significant effect on overdose or infection. However, we estimate a statistically significant increase in the number of patients experiencing a fall- or fracture-related hospitalization or ED visit among high-exposure physicians: 0.035 additional patients per quarter (22.3% relative to a pre-treatment mean of 0.157). As we show below, this pooled effect masks substantial heterogeneity across the exposure distribution.

Heterogeneity. The divergence between beneficiary- and physician-level results for falls and fractures is consistent with earlier findings: detailing-induced prescribing may be most salient for patients outside the beneficiary-level sample—*i.e.,* new patients or those for whom the detailed physician is not the attributed key provider. However, it is also possible that these pooled effects mask important heterogeneity, similar to the heterogeneous increases in opioid utilization discussed in Section 4.2.4.

Dose-response analyses by detailing intensity (Figure 6) do not reveal a clear monotonic relationship between exposure and harm at either the physician or beneficiary level, though effects are generally concentrated among providers with intermediate exposure levels. At the physician level, falls and fractures increase significantly in the third exposure quartile (5–7 visits). At the beneficiary level, effects by exposure bin are uniformly imprecise, though the largest relative increase occurs among physicians in the third exposure bin (5–7 visits).

A clearer picture emerges when we examine heterogeneity by patient and provider characteristics (Figures A19 and A20). Recall from Section 4.2.4 that Black and younger beneficiaries experienced the largest increases in the probability of filling a Purdue opioid prescription (18.5% and 5.8%, respectively). We find corresponding increases in falls and fractures among these groups: $> 5\%$[26] Black beneficiaries and 54.9% among younger beneficiaries. The estimate for Black beneficiaries is imprecisely estimated due to small sample size (N=361), but the effect among younger beneficiaries is precisely

[26]The exact number was suppressed due to minimum reportable cell size limitations; we report the minimum relative effect size that can be reported if the pre-detailing mean multiplied by the sample size was at least 11.

estimated. Male beneficiaries, who experienced a modest (5.7%) increase in opioid use, show a $> 55\%$ increase in falls and fractures.

Heterogeneity by key provider characteristics reinforces this pattern. Beneficiaries whose key provider is not a primary care physician—the group with the largest increase in opioid use (8.4%)—experience a $> 70\%$ increase in falls and fractures. By contrast, beneficiaries with PCP key providers show no increase in falls and fractures.

These results suggest that heterogeneity in harm may be better explained by patient and provider characteristics than by detailing intensity per se. To the extent that these characteristics correlate with exposure—for instance, if non-PCP physicians or those treating younger patients receive different levels of detailing—dose-response analyses may obscure the underlying relationships driving adverse outcomes.

4.4 Robustness

Our identification strategy relies on comparing physicians with high detailing exposure to those with low detailing exposure, rather than comparing detailed physicians to never-detailed physicians. We now present evidence supporting this approach and assess the sensitivity of our findings to alternative specifications.

Selection into Detailing. A natural alternative to our main specification would compare detailed physicians directly to never-detailed physicians. However, if Purdue's detailers strategically targeted physicians based on characteristics correlated with opioid prescribing—as internal marketing documents suggest—such comparisons would be confounded by selection. Figures A21 and A22 confirm this concern: when we compare high-exposure or ever-detailed physicians to never-detailed controls, the resulting event-study patterns exhibit clear pre-trends, violating parallel trends assumptions. Figure A23 shows that even low-exposure physicians display similar pre-period divergence relative to never-detailed physicians, reinforcing that selection into detailing poses a threat to identification. These patterns support our decision to identify effects from variation in detailing intensity among detailed physicians, holding selection into any detailing constant.

Staggered Treatment Timing. Recent econometric work has highlighted potential bias in two-way fixed effects estimators when treatment timing is staggered and effects are heterogeneous (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). In our setting, physicians receive their first detailing visit

in different calendar quarters, raising the concern that our pooled estimates may be contaminated by comparisons between newly-treated and already-treated units. To assess this, we estimate treatment effects separately for each first-visit-quarter cohort, using only never-detailed physicians as controls.[27] Figure A28 compares the distribution of these cohort-specific estimates to our pooled ATE. The mean cohort-specific estimate (0.84 additional Purdue prescriptions) lies well within one standard error of the pooled estimate (0.77; SE = 0.294), and the distribution of cohort effects is centered near the pooled value. This suggests that heterogeneity in treatment timing is unlikely to meaningfully bias our main results.

Alternative Attribution. Our main analysis attributes patients to physicians based on evaluation and management visits, which are available throughout our sample period but may imperfectly capture prescribing relationships. As a validation exercise, Figure A27 re-estimates our model using prescriber NPIs recorded directly in Part D claims, available from 2014 onward. This approach bypasses visit-based attribution entirely but substantially reduces our sample: only 144 ever-detailed physicians meet our inclusion criteria.[28] Given limited power, we estimate a simplified specification comparing ever-detailed to never-detailed physicians. Despite the small sample, pre-period coefficients are close to zero and statistically insignificant, while post-period effects are uniformly positive. The pooled estimate (0.48 additional prescriptions; SE = 0.36) is imprecise but qualitatively consistent with our main findings, providing reassurance that our visit-based attribution captures meaningful physician-patient relationships.

Additional Specification Checks. We conduct several additional robustness exercises. First, Figure A24 shows that excluding physicians whose first detailing visit occurred in 2007—who may have left-censored detailing histories—produces qualitatively similar results. Second, Figure A26 estimates effects on medication classes unrelated to opioid marketing, i.e. negative control outcomes: statins, antihypertensives, β-blockers, diabetes agents, glaucoma medications, and proton pump inhibitors. While isolated significant coefficients appear in individual quarters, no outcome exhibits the systematic post-detailing increases observed for opioid prescribing, consistent with our findings reflecting targeted marketing effects rather than unobserved confounders. Third, Figure A29 compares OLS and Poisson

[27] For each cohort containing both high- and low-exposure physicians, we estimate a difference-in-differences model with never-detailed physicians as the comparison group, yielding a cohort-specific ATE.

[28] Physicians must have their first detailing visit no earlier than January 2015 to allow for balanced pre- and post-periods within the 2014–2018 claims window.

pseudo-maximum likelihood estimates to assess sensitivity to functional form. The Poisson-based treatment effect (6.7%) is somewhat attenuated relative to the OLS estimate (11.2%) but remains positive and statistically significant.

5 Conclusion

In this paper, we provide new evidence on the effects of pharmaceutical marketing on opioid prescribing and patient outcomes. Using internal Purdue Pharma detailing records linked to Medicare claims data, we show that increased exposure to detailing leads to an economically meaningful 11.2% increase in physician-level prescribing and a 4.3% increase in patient-level utilization of Purdue-manufactured opioids. As discussed above, our estimates capture the total causal effect of the detailing ecosystem—inclusive of any feedback dynamics between prescribing and subsequent marketing intensity—rather than the marginal effect of an additional visit. These effects persist beyond the immediate post-detailing period and are accompanied by measurable downstream consequences: we find statistically significant increases in falls and fractures, with the largest effects concentrated at intermediate levels of detailing exposure rather than among the most heavily detailed physicians.

The prescribing effects we estimate are consistent with existing work on pharmaceutical marketing and opioid prescribing. Studies documenting associations between industry payments and opioid prescribing report increases of approximately 9–13%, while restrictions on pharmaceutical marketing at academic medical centers have been associated with an 8.8% reduction in opioid prescriptions (Beilfuss and Linde, 2021; Eisenberg et al., 2020; Hadland et al., 2018). Our average 11.2% estimate falls squarely within this range. Another closely related study explores the impact of educational outreach on provider prescribing in the Veterans Affairs healthcare system, finding that such outreach can reduce opioid prescribing (Zhang, Forthcoming). To our knowledge, however, no prior study has used physician-level detailing records to estimate the causal effect of opioid marketing on provider prescribing behavior and downstream patient outcomes.

Far less work has examined the relationship between pharmaceutical marketing and patient welfare. Existing studies link marketing exposure to higher patient mortality, but have largely relied on aggregated geographic variation rather than patient-level data (Alpert et al., 2021; Hadland et al., 2019). While the time horizon of our setting limits our ability to estimate effects on mortality directly, we extend this literature by documenting specific adverse health outcomes—falls and fractures—that have been iden-

tified as an important opioid-related risk among older adults (Krebs et al., 2016; Solomon et al., 2010; Yoshikawa et al., 2020). Whereas much of the existing evidence on opioids, falls, and fractures is associational, our quasi-experimental design provides stronger support for a causal interpretation than purely associational evidence, even as our estimates reflect the combined dynamics of the detailing ecosystem. While we cannot rule out that increased access to opioids can improve patient pain, our findings present compelling evidence that for specific subpopulations—particularly younger beneficiaries and patients of non-primary-care physicians—these benefits, if present, were accompanied by meaningful patient welfare costs.

Our findings can be interpreted through the lens of recent work that considers pharmaceutical marketing in relation to "optimal" prescribing levels. Grennan et al. (2024) develop a model in which marketing can increase patient welfare when drugs are under-prescribed relative to a theoretical optimum, but more generally reduces welfare. The opioid setting is particularly ambiguous along these dimensions. On the one hand, concerns around addiction made physicians historically reluctant to prescribe opioids, raising the possibility of under-treatment of pain. On the other hand, opioids carry substantial risks of dependence and injury, and misleading marketing claims may have shifted prescribing beyond a socially optimal level. The increase in falls and fractures we document following intensive marketing exposure in select subpopulations is consistent with the latter case and provides empirical support for the conclusion in Grennan et al. (2024) that marketing can generate patient welfare losses when marginal prescriptions are harmful.

Schnell (2025) offers additional insight into physician behavior near this prescribing margin. Specifically, when determining whether to prescribe opioids, physicians may also consider the possibility that patients will re-sell these prescriptions on an illegal "secondary" market. Schnell (2025) shows that this consideration induces physicians to be more conservative in their prescriptions, constraining supply and bringing overall opioid prescriptions closer to some optimal level (though harms persist through the reallocation of prescriptions on the secondary market). In our setting, this framework may help explain why average increases in opioid prescribing are modest, even as downstream harms remain detectable: marketing-induced changes may operate at the margin of clinical discretion rather than through large shifts in prescribing volume.

Our beneficiary-level dose-response results lend further support to this interpretation. Because the beneficiary-level design holds patient identity fixed, it isolates within-patient changes in prescribing that

are plausibly attributable to the physician's detailing exposure. Among existing patients of detailed physicians, increases in the probability of filling a Purdue opioid prescription are concentrated at intermediate levels of detailing exposure (6–7% for key providers receiving 3–7 visits), with small and insignificant effects at the lowest and highest exposure levels. This pattern is consistent with physicians at intermediate exposure levels being closest to the prescribing margin for their existing patients: detailing may have tipped clinical decisions for patients about whom the physician was otherwise uncertain, whereas physicians with minimal exposure remained unresponsive and those with the highest exposure may have already been prescribing near capacity.[29]

This interpretation gains additional support from heterogeneity in both prescribing responses and downstream harms across patient and provider characteristics. At the beneficiary level, the largest increases in opioid use and in falls and fractures are concentrated among younger beneficiaries and patients of non-primary-care physicians. Younger Medicare beneficiaries qualify predominantly through disability, and patients whose key provider is a specialist rather than a primary care physician likely have clinical profiles dominated by conditions requiring specialist-level management. For both groups, these characteristics suggest greater medical complexity and clinical settings in which opioid therapy may be more actively considered—potentially placing these patients closer to the margin of opioid indication. That detailing exposure produces both large prescribing increases and large increases in falls and fractures among these patients—but not among older beneficiaries or patients of primary care physicians (for example)—suggests that the welfare consequences of detailing depend critically on which patients receive the marginal prescription. For many patients, additional opioid exposure may have been benign or even beneficial; for younger beneficiaries and patients of specialists, the marginal prescriptions induced by marketing were, on average, welfare-reducing. The channels through which provider specialty shapes responsiveness to detailing are not fully clear from our data, though they may reflect differences in prescribing norms, regulatory scrutiny, or the clinical contexts in which opioid prescribing decisions arise. We observe a similar pattern among Black beneficiaries, who experience large increases in opioid use and suggestive (though imprecisely estimated) increases in falls and fractures following their key provider's exposure to detailing: small cell sizes limit our ability to draw firm conclusions about this subgroup, and the mechanisms underlying this heterogeneity are less clear.

[29]Physician-level results show a somewhat different pattern, with prescribing increases of 10–15% that plateau across the upper three exposure quartiles. Because physician-level effects could reflect changes in patient composition rather than changes in prescribing to existing patients, we focus here on the beneficiary-level results when interpreting prescribing-margin dynamics.

The nature of opioid prescribing as a clinical decision may help explain why marketing is effective in this context. Pain is inherently subjective and difficult to verify, creating uncertainty around appropriate treatment thresholds and allowing for substantial heterogeneity in prescribing behavior among otherwise similar physicians and for otherwise similar patients. In such settings, marketing may be particularly influential, shaping beliefs or lowering perceived thresholds for treatment where objective clinical signals provide limited guidance. Consistent with this, detailing effects persist even among physicians first visited in the later years of our study—despite increasing public awareness of opioid-related harms—suggesting that marketing retained its influence even as the broader clinical environment evolved.

Our analysis focuses on Massachusetts, which offers a distinctive setting for studying detailing effects. Despite being in the lowest quartile nationally for opioid prescribing rates, Massachusetts experienced disproportionately severe opioid-related mortality, with overdose death rates roughly double the national average by 2014 (Barocas et al., 2018; Rudd et al., 2016; Schuchat et al., 2017). The fact that detailing produces measurable shifts in prescribing even in a relatively conservative prescribing environment suggests that our estimates may represent a lower bound of marketing's influence in higher-prescribing states. At the same time, the single-state setting limits generalizability: Massachusetts is more urban, has broader insurance coverage, and adopted several opioid-related policy interventions during our study period. Our data are available because the Massachusetts Attorney General filed a lawsuit against Purdue Pharma and the Sackler family (Office of the Attorney General, 2021), producing internal detailing records through litigation that would otherwise remain confidential—underscoring the broader role of transparency in enabling research on pharmaceutical marketing.

Several additional limitations merit discussion. Our attribution approach links patients to physicians via evaluation and management visits, but only 19% of prescriptions for patients attributed to never-detailed physicians—and 38% for ever-detailed physicians—are written by the attributed physician, based on matching to prescriber NPIs in Part D event records from 2014–2018.[30] Despite these low match rates, results are qualitatively consistent when using prescriber NPIs directly, though with reduced statistical power. The consistency of our findings across both physician- and patient-level analyses suggests that even when an attributed physician does not write the prescription directly, their detailing exposure may influence prescribing within their patient network, potentially through practice norms or care

[30]Expanding the matching window to ±2 quarters increases these rates only modestly, to 22% and 44%, suggesting that timing lag does not drive the discrepancy.

coordination with other members of the care team. This interpretation resonates with recent work by Agha and Zeltzer (2022) documenting the role of physician networks in shaping prescribing behavior, and suggests that the effects of detailing may extend beyond the directly targeted physician. Second, our adverse health outcome measures capture only events that result in hospitalizations or ED visits, potentially understating the true burden of opioid-related harm. Third, we cannot observe patient pain levels, which limits our ability to assess whether detailing-induced prescribing was clinically appropriate for individual patients.

Our findings inform ongoing debates about the regulation of direct-to-physician marketing. Many U.S. academic medical centers have restricted or banned in-person pharmaceutical detailing, and prior work has shown that such bans can reduce opioid prescribing (Eisenberg et al., 2020; Zhang, Forthcoming). Our results complement this evidence by demonstrating that detailing affects prescribing in welfare-relevant ways, suggesting that closer scrutiny of marketing practices may be warranted.

At the same time, opportunities to regulate detailing are constrained by constitutional protections for commercial speech. Policy efforts have largely emphasized transparency, most notably through the Physician Payments Sunshine Act, which requires public disclosure of financial relationships between manufacturers and providers. However, not all detailing interactions involve a reportable financial transfer, limiting the extent to which existing disclosure requirements capture the full scope of marketing activity. Our findings highlight the importance of transparency not only around financial relationships, but also around routine marketing interactions that may not generate reportable payments yet nevertheless influence clinical decision-making. More comprehensive reporting of detailing activity—such as the number of marketing visits a physician receives from a manufacturer over a specified period—could meaningfully expand the information available to regulators, researchers, and patients.

In the absence of such reporting requirements, comprehensive detailing information has instead become available through litigation. In 2021, Purdue Pharma was dissolved in a bankruptcy settlement that included two landmark conditions: the payment of over $4.3 billion—subsequently increased to $7.4 billion (Stempel, 2025)—to communities nationwide for opioid misuse prevention, treatment, and recovery; and "unprecedented and complete disclosure about the role Purdue and the Sacklers played in the opioid crisis," making more than 30 million documents publicly available (Office of the Attorney General, 2021). While these penalties represent only a fraction of OxyContin's cumulative revenues, they illustrate both the scale of harm attributed to aggressive pharmaceutical marketing and the critical

role that document disclosure can play in enabling accountability and informing evidence-based policy.

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Tables

Table 1: Summary Statistics, Physician-Level Sample

			Detailing Volume in Washout Period	
	Never Detailed	Ever Detailed	Low Exposure	High Exposure
Panel A. Physician demographic and specialty characteristics				
Men (%)	64	69	67	71
Specialty (%)				
Internal Medicine	73	58	56	59
Family Medicine	9	18	18	18
Orthopaedic Surgery	4	8	8	8
Anesthesiology	0	3	2	3
Physical Medicine & Rehabilitation	1	2	2	3
Panel B. Treatment activity				
Total patients per quarter, mean (SD)	16 (15)	25 (19)	23 (18)	27 (20)
Total Purdue prescriptions per quarter, mean (SD)	4.46 (5.00)	6.61 (7.28)	6.29 (7.03)	6.83 (7.44)
Total Purdue MME per quarter, mean (SD)	5136 (8009)	9514 (21587)	8735 (17168)	10046 (24135)
Total opioid prescriptions per quarter, mean (SD)	6.13 (6.61)	10.44 (10.48)	9.71 (10.02)	10.93 (10.76)
Total opioid MME per quarter, mean (SD)	7517 (11080)	14784 (27178)	13421 (22718)	15715 (29813)
Detailing volume in washout period, mean (SD)		3.8 (4.3)	1.0 (0.0)	5.7 (4.7)
Distinct physicians (N)	5362	2076	842	1234

This table reports descriptive statistics for in-sample physicians in the physician-level analysis. For time-variant measures, means and standard deviations (SD) were calculated from observations in the pre-period for ever-detailed physician, and were calculated based on all quarters a physician was observed for never-detailed physician. Detailing volume in the washout period refers to the number of detailing visits a physician receives in relative quarters $r \in [0, 3]$, where $r = 0$ is the quarter in which the physician first has a detailing visit. Low-exposure physicians receive 1 visit during the washout period; high-exposure physicians receive 2–37 visits during the washout period.

Table 2: Pooled Event Study, Physician-Level Analysis

Panel A. Extensive Margin	Total Opioid Prescriptions	Log(MME)	Number of Patients with Opioid Prescription
	(1)	(2)	(3)
High Exposure × Post	0.766	0.098	0.211
	(0.294)	(0.114)	(0.085)
High Exposure × Washout	0.404	0.015	0.090
	(0.230)	(0.101)	(0.069)
Observations	246,887	246,887	246,887
Low Exposure Mean	6.29	8,735.0	2.26
High Exposure Mean	6.83	10,046.0	2.43
Panel B. Intensive Margin	Opioid Prescriptions per Patient	Log(MME per Patient)	Share of Patients with Opioid Prescription
	(4)	(5)	(6)
High Exposure × Post	−0.002	0.094	0.002
	(0.014)	(0.081)	(0.004)
High Exposure × Washout	−0.004	0.038	−0.001
	(0.013)	(0.070)	(0.003)
Observations	246,887	246,887	246,887
Low Exposure Mean	0.304	439.4	0.106
High Exposure Mean	0.332	493.7	0.109

This table reports coefficients from estimating Equation 2. "High Exposure" indicates that the physician had above-median detailing visits during the washout period (relative quarters $r \in [0, 3]$); "Low Exposure" indicates below-median visits. "Post" is an indicator for relative quarters $r \in [4, 9]$; "Washout" is an indicator for $r \in [0, 3]$. The pre-period ($r \in [-6, -1]$) is the omitted reference category. The sample includes high-exposure, low-exposure, and never-detailed physicians. Fixed effects are included for physician and calendar quarter. Standard errors, clustered at the physician level, are in parentheses. Means are pre-treatment averages for ever-detailed physicians, by exposure group. Means for log-transformed outcomes are reported in levels.

Table 3: Summary Statistics, Beneficiary-Level Sample

			Detailing Volume in Washout Period	
	Never Detailed	Ever Detailed	Low Exposure	High Exposure
Panel A. Beneficiaries				
Male (%)	37	37	36	37
Age, mean (SD)	68 (14)	68 (15)	68 (14)	68 (15)
Race (%)				
White	89	89	89	90
Black	4	4	5	3
Hispanic	2	2	2	2
Asian	2	2	1	2
SSDI Recipient (%)	22	24	24	25
Chronic pain ever (%)	99	99	99	99
Chronic pain before first detailing visit (%)		90	91	89
Any opioid prescription ever (%)	70	63	63	63
Any Purdue opioid prescription ever (%)	44	44	45	43
Quarters observed, mean (SD)	35 (13)	16 (0)	16 (0)	16 (0)
Panel B. Key Providers				
Share of visits in pre-period, mean (SD)		0.63 (0.26)	0.61 (0.26)	0.64 (0.25)
Primary care (%)		89	87	90
Detailing volume in washout period, mean (SD)		4 (4)	1 (0)	6 (5)
Total Distinct Beneficiaries	48081	16027	5612	10415
Total Distinct Key Providers		1875	743	1132

This table reports descriptive statistics for in-sample beneficiaries and key providers in the beneficiary-level analysis. Characteristics qualified by the term "ever" indicate that the beneficiary had such a characteristic at some point during the sample period. "Chronic pain before first detailing visit" indicates that the beneficiary had a diagnosis of chronic pain at some point before their key provider's first detailing visit. "Share of visits in pre-period" refers to the share of E&M visits attributed to a beneficiary's key provider in the four quarters prior to their first detailing visit. Detailing volume in the washout period refers to the number of detailing visits a physician (key provider) receives within the first four quarters of their first detailing visit. Low-exposure physicians receive 1 visit during the washout period; high-exposure physicians receive 2–37 visits during the washout period.

Table 4: Differences-in-Differences, Purdue Opioid Prescriptions, Beneficiary Level

	Probability of Opioid Prescription (1)	Log(MME) (2)
Panel A. Full Sample		
High Exposure × Post	0.005	0.014
	(0.003)	(0.014)
High Exposure × Washout	0.0009	0.003
	(0.003)	(0.013)
Observations	1,952,570	1,952,570
Low Exposure Mean	0.119	21.6
High Exposure Mean	0.115	50.0
Panel B. Chronic Pain Population		
High Exposure × Post	0.006	0.016
	(0.004)	(0.016)
High Exposure × Washout	0.0008	0.002
	(0.003)	(0.014)
Observations	1,918,173	1,918,173
Low Exposure Mean	0.130	23.7
High Exposure Mean	0.127	55.6
Panel C. SSDI Population		
High Exposure × Post	0.007	0.009
	(0.008)	(0.039)
High Exposure × Washout	−0.002	−0.017
	(0.008)	(0.035)
Observations	478,497	478,497
Low Exposure Mean	0.217	59.7
High Exposure Mean	0.229	176.1

This table reports estimated coefficients from the pooled version of Equation 1 for our main beneficiary-level analysis. "High Exposure" is an indicator that the ever-detailed physician had an above-median (2–37) volume of visits in the washout period (relative quarters $r \in [0, 3]$, where $r = 0$ is the first quarter a physician experiences a detailing visit). "Post" is an indicator for relative quarters $r \in [4, 9]$ after the first detailing visit; the washout period is omitted from the post-period. Fixed effects are included for beneficiaries and calendar quarter. All models include beneficiaries with high-exposure key providers, beneficiaries with low-exposure key providers (ever-detailed physicians with 1 visit in the washout period), and beneficiaries who are never treated by a detailed physician. Omitted category is an indicator for relative quarters $r \in [-6, -1]$. Standard errors are clustered at the beneficiary level and are reported in parentheses. All outcomes are defined at the beneficiary-quarter level. Means are based on pre-treatment values for beneficiaries whose key providers are ever detailed, by exposure class. Means for log-transformed variables are reported in the untransformed form.

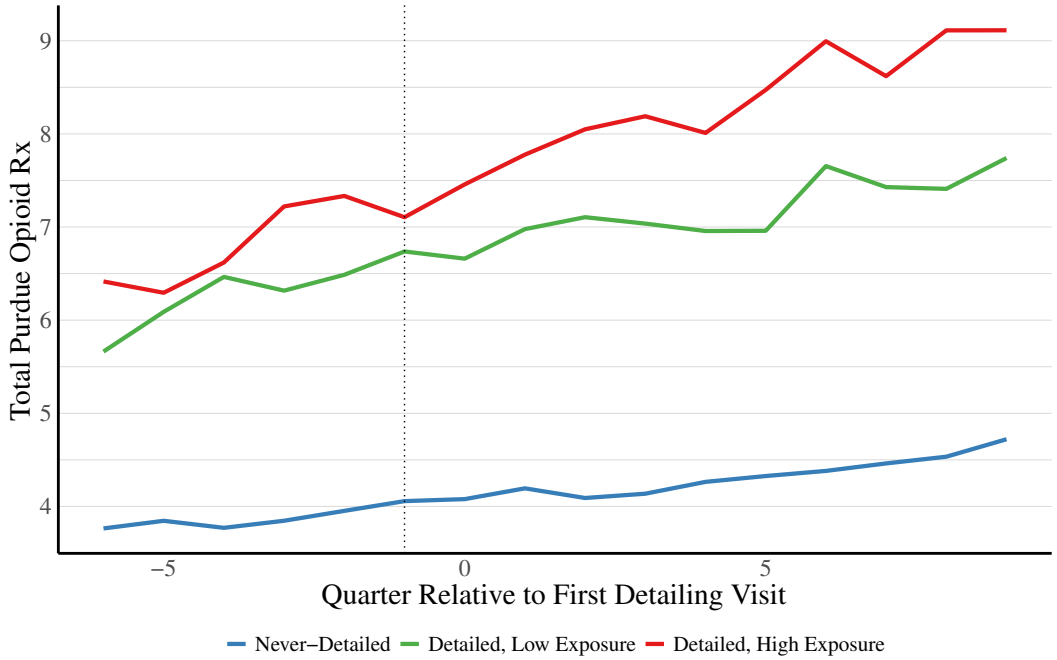
Table 5: Differences-in-Differences, Opioid-Related Adverse Health Events

Panel A. Beneficiary Level	Probability of: Overdose or Infection	Fall or Fracture
	(1)	(2)
High Exposure × Post	−0.0002	0.0007
	(0.0009)	(0.0008)
High Exposure × Washout	−0.0001	0.0005
	(0.001)	(0.0008)
Observations	1,952,570	1,952,570
Low Exposure Mean	0.007	0.005
High Exposure Mean	0.008	0.005
Panel B. Physician Level	Number of Patients With: Overdose or Infection	Fall or Fracture
	(1)	(2)
High Exposure × Post	0.005	0.035
	(0.015)	(0.014)
High Exposure × Washout	0.024	0.010
	(0.015)	(0.013)
Observations	246,887	246,887
Low Exposure Mean	0.213	0.154
High Exposure Mean	0.217	0.157

This table reports coefficients from estimating Equation 2 at the beneficiary level (Panel A) and physician level (Panel B). "High Exposure" indicates above-median detailing visits during the washout period (relative quarters $r \in [0, 3]$); "Low Exposure" indicates below-median visits. "Post" is an indicator for relative quarters $r \in [4, 9]$; "Washout" is an indicator for $r \in [0, 3]$. The pre-period ($r \in [-6, -1]$) is the omitted reference category. Both samples include high-exposure, low-exposure, and never-treated units. Panel A includes fixed effects for beneficiary and calendar quarter, with standard errors clustered at the beneficiary level. Panel B includes fixed effects for physician and calendar quarter, with standard errors clustered at the physician level. Standard errors are reported in parentheses. Means are pre-treatment averages for ever-treated units, by exposure group.

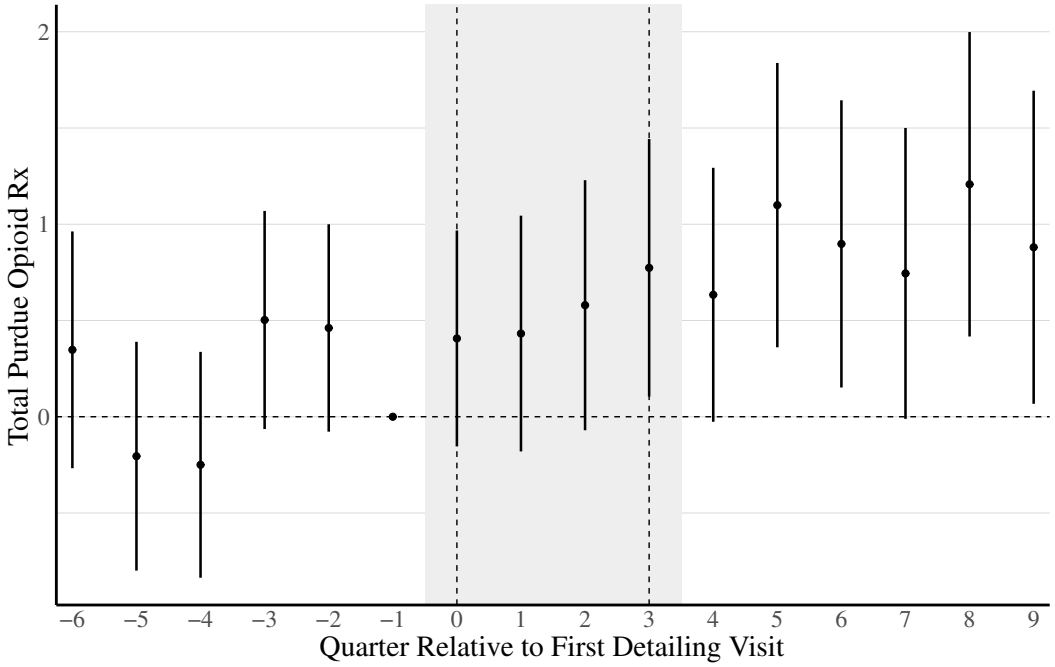
Figures

Figure 1: Unadjusted Trends in Purdue Opioid Prescriptions, Physician Level



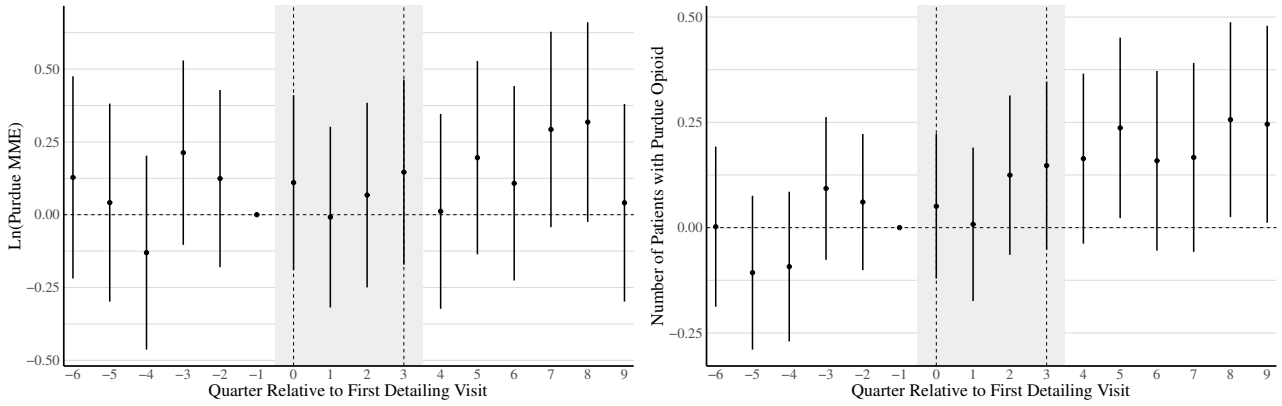
This figure plots unadjusted trends in the average number of Purdue opioid prescriptions per physician in the quarter relative to their first detailing visit. Never-detailed physicians are randomly assigned a placebo treatment date for purposes of comparison. Trend lines represent unweighted averages within detailing category. "Never-detailed" physicians are those who never receive a detailing visit. "Detailed, Low Exposure" are physicians who receive 1 visit in the washout period. "Detailed, High Exposure" are physicians who receive 2–37 visits in the washout period.

Figure 2: Purdue Opioid Prescriptions Relative to Detailing Exposure, Physician Level



This figure plots estimated coefficients from Equation 1 for our main physician-level analysis. The grayed out period around relative quarters $r \in [0, 3]$ represents the "washout period;" $r = 0$ is the first quarter a physician experiences a detailing visit. Fixed effects are included for physicians and calendar quarter. All models include high-exposure physicians (ever-detailed physicians with 2–37 visits in the washout period), low-exposure physicians (ever-detailed physicians with 1 visit in the washout period), and never-detailed physicians. Omitted category is an indicator for relative quarter $r = -1$. All outcomes are defined at the physician-quarter level. Standard errors are clustered at the physician level; 95% confidence intervals are given by solid black lines.

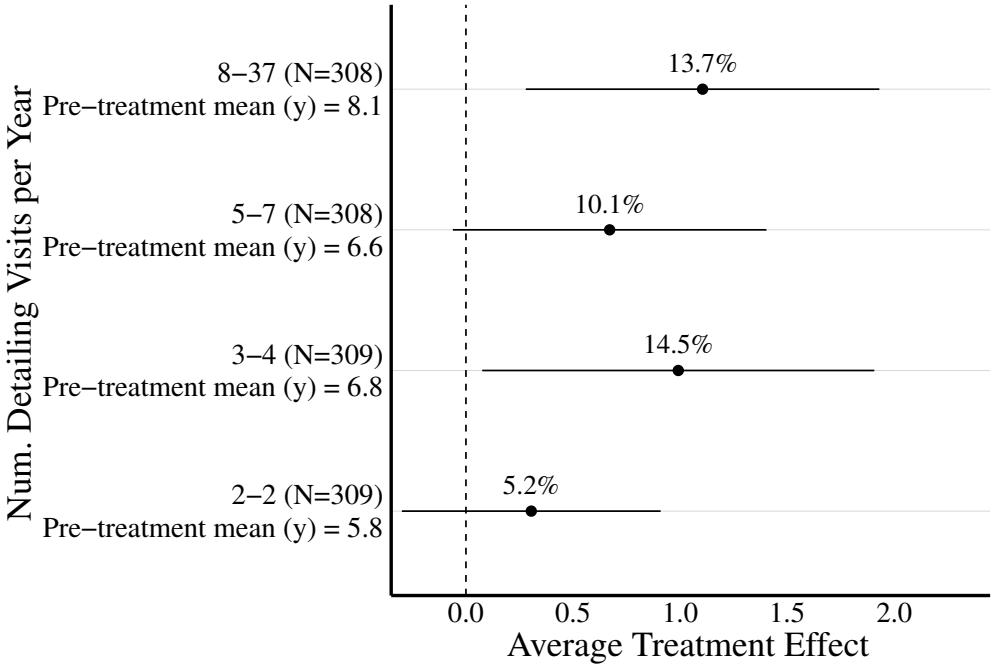
Figure 3: Additional Opioid Utilization Measures Relative to Detailing Exposure, Physician Level



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

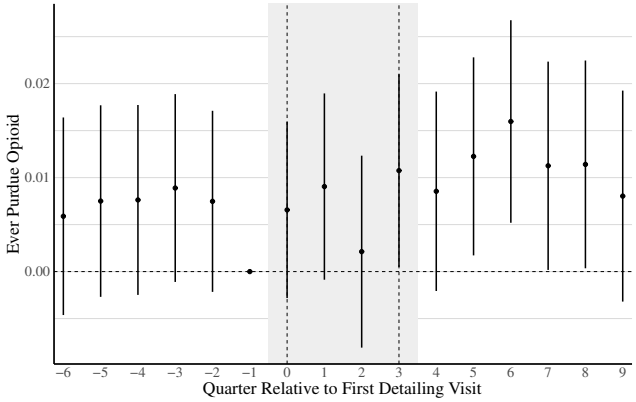
This figure plots estimated coefficients from Equation 1 for our main physician-level analysis. The grayed out period around relative quarters $r \in [0, 3]$ represents the "washout period;" $r = 0$ is the first quarter a physician experiences a detailing visit. Fixed effects are included for physicians and calendar quarter. All models include high-exposure physicians (ever-detailed physicians with 2–37 visits in the washout period), low-exposure physicians (ever-detailed physicians with 1 visit in the washout period), and never-detailed physicians. Omitted category is an indicator for relative quarter $r = -1$. All outcomes are defined at the physician-quarter level. Standard errors are clustered at the physician level; 95% confidence intervals are given by solid black lines.

Figure 4: Dose-Response Effects on Purdue Opioid Prescriptions, Physician Level

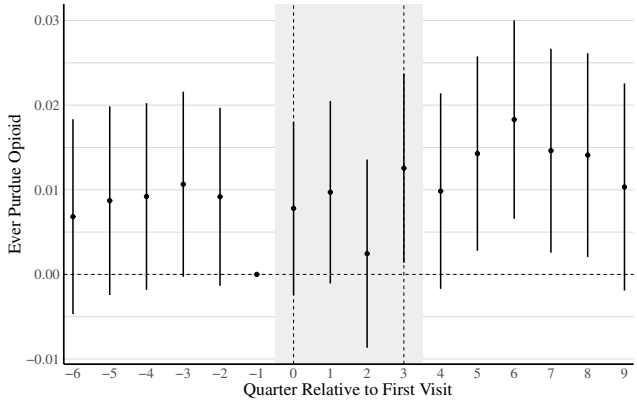


This figure reports coefficients from estimating Equation 2 separately on physicians in each quartile of detailing visit volume in the washout period. The pre-period ($r \in [-6, -1]$) is the omitted reference category. The sample includes high-exposure, low-exposure, and never-detailed physicians. Fixed effects are included for physician and calendar quarter. All outcomes are defined at the physician-quarter level. Sample sizes of the high-exposure (treatment) group are given by "N." Means are based on pre-treatment values for high-exposure detailed physicians and are given as "pre-treatment mean (y)." Standard errors are clustered at the physician level; 90% confidence intervals are given by solid black lines.

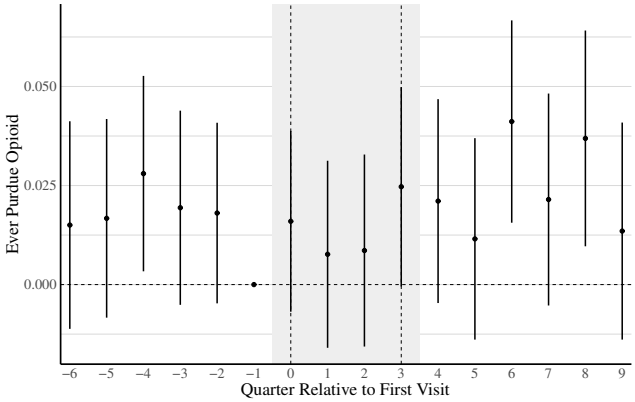
Figure 5: Purdue Opioid Prescription Fills Relative to Detailing Exposure, Beneficiary Level



(a) Full Sample



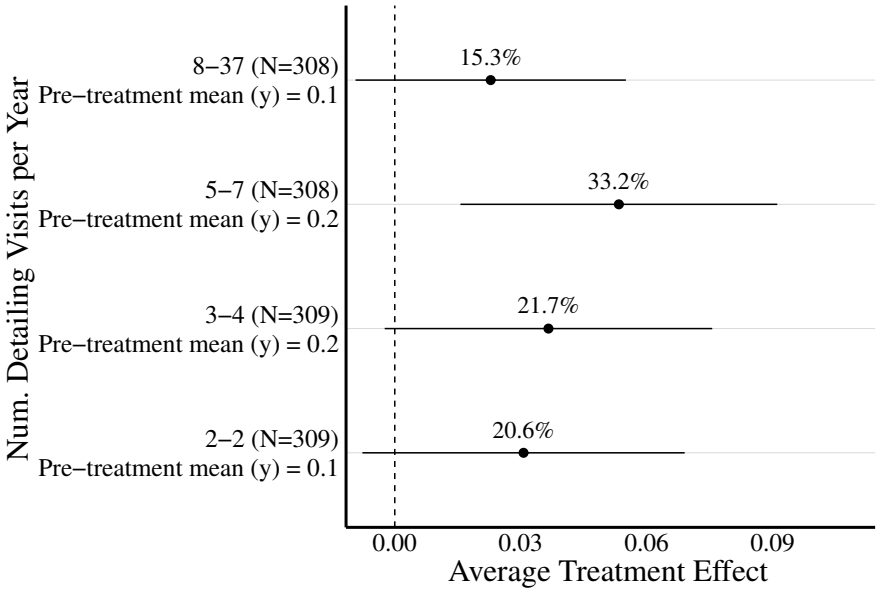
(b) Chronic Pain Sample



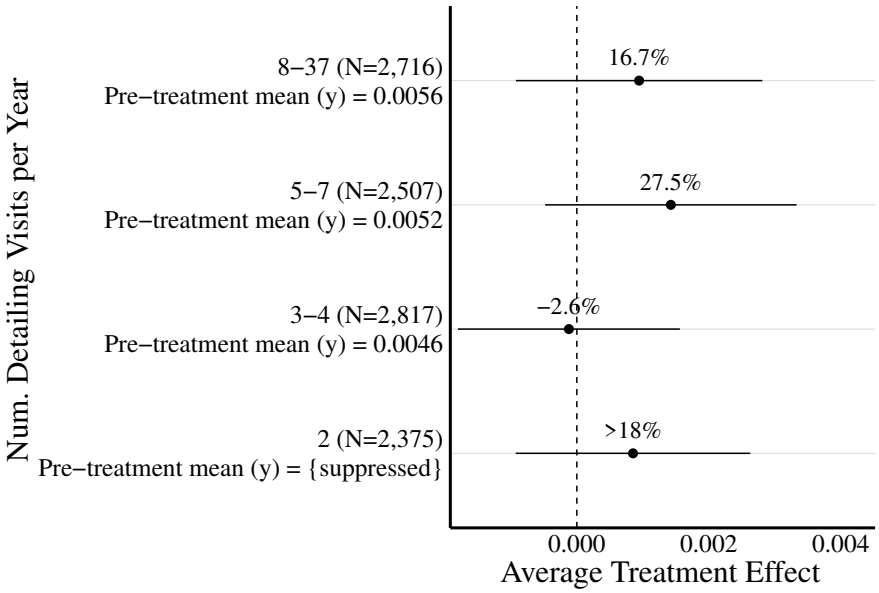
(c) SSDI Sample

This figure plots estimated coefficients from the pooled version of Equation 1 for our main beneficiary-level analysis. The grayed out period around relative quarters $r \in [0, 3]$ represents the "washout period;" $r = 0$ is the first quarter a key provider experiences a detailing visit. Fixed effects are included for beneficiaries and calendar quarter. All models include beneficiaries with high-exposure key providers (ever-detailed physicians with 2–37 visits in the washout period), beneficiaries with low-exposure key providers (ever-detailed physicians with 1 visit in the washout period), and beneficiaries who are never treated by a detailed physician. Omitted category is an indicator for relative quarter $r = -1$. Standard errors are clustered at the beneficiary level and are reported in parentheses. All outcomes are defined at the beneficiary-quarter level. 95% confidence intervals are given by solid black lines.

Figure 6: Dose-Response Effects on Falls or Fractures



(a) Physician Level, Number of Patients with a Fall- or Fracture-Related Visit



(b) Beneficiary Level, Probability of Having a Visit Related to a Fall or Fracture

This figure reports coefficients from estimating Equation 2 separately on physicians and beneficiaries in each quantile of detailing visit volume in the washout period. Panel (a) reports physician-level results, binning physicians into four quartiles based on detailing exposure in the washout period. Panel (b) reports beneficiary-level results, binning beneficiaries into four groups based on key providers' exposure during the washout period, where bins manually match physician-level quartiles. The pre-period ($r \in [-6, -1]$) is the omitted reference category. The sample includes high-exposure, low-exposure, and never-detailed physicians. Fixed effects are included for physician (panel a) or beneficiary (panel b), and calendar quarter. Sample sizes of the high-exposure (treatment) group are given by "N." Outcomes in panel (a) are defined at the physician-quarter level; outcomes in panel (b) are defined at the beneficiary-quarter level. Means in panel (a) are based on pre-treatment values for high-exposure detailed physicians; means in panel (b) are based on pre-treatment values for beneficiaries of key providers with high exposure to detailing. Standard errors are clustered at the physician (panel a) and beneficiary (panel b) levels; 90% confidence intervals are given by solid black lines.

Appendix

A Additional Tables and Figures

Appendix Table A1: Match Rates Between Attributed NPI and NPI in PDE Records by Exposure Category, 2014–2018

Year	Total Attrib	% Same Qtr	% ±1 Qtr	% ±2 Qtr
Panel A. Never detailed				
2014	49077	19.9%	22.4%	23.5%
2015	48824	19.3%	22%	23.3%
2016	48893	18.5%	21%	22.1%
2017	44552	18.3%	20.6%	21.7%
2018	40325	18.1%	19.9%	20.6%
Overall	231671	18.9%	21.2%	22.3%
Panel B. Low exposure				
2014	3551	35.9%	39.9%	41.7%
2015	2818	33.7%	37.3%	39.1%
2016	1706	30.2%	33.4%	34.8%
2017	883	29.1%	32.4%	34.1%
2018	278	38.8%	39.2%	40.3%
Overall	9236	33.6%	37.2%	38.9%
Panel C. High exposure				
2014	3331	44.4%	48.8%	50.5%
2015	2442	41.6%	46.2%	49.1%
2016	1471	42.8%	47.7%	49.7%
2017	409	34%	38.6%	40.3%
2018	141	33.3%	38.3%	41.1%
Overall	7794	42.5%	47%	49.2%

Data is at the BENE_ID × attributed NPI × quarter level. "Total Match" represents the total number of attributed NPIs that match any NPI in a PDE record in the corresponding quarter or surrouding quarters for the beneficiary. "Overall" is the match rate across all years (2014 - 2018).

Appendix Table A2: Detailed vs Never-Detailed Physicians in Medicare, 2006–2018

Characteristic	Never-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 beneficiaries per month (SD)	8.8 (11.9)	14.8 (11.2)
Total	29980	3322

This table compares characteristics of detailed and never-detailed physicians treating beneficiaries in Massachusetts between 2016 and 2018.

Appendix Table A3: Compare Characteristics of Matched and Unmatched Never-Detailed Physicians

Characteristic	Unmatched Sample	Matched Sample
Male (%)	59.1	63.6
Specialty (%)		
Internal Medicine (%)	39	73.2
Family Medicine (%)	6.4	8.9
Orthopaedic Surgery (%)	3.7	4.3
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)	15.8 (15)
Total Purdue Rx, Mean (SD)	3 (4.4)	4.5 (5)
Total Purdue MME, Mean (SD)	3328.4 (7143.5)	5136.2 (8009.2)
Total Opioid Rx, Mean (SD)	4.1 (5.9)	6.1 (6.6)
Total MME, Mean (SD)	4863.6 (10018.4)	7517.2 (11080.2)
Distinct Physicians (N)	19761	5362

This table compares characteristics of never-detailed physicians in the matched and unmatched sample.

Appendix Table A4: Physician Counts by Restriction

Restriction Step	Control (N)	Treated (N)
Initial counts	19761	3217
After dropping physicians in first half of 2007	19761	2759
NPIs with [-6,9] quarters	19761	2076
Final count	19761	2076

Appendix Table A5: Provider Counts by Treatment Status

Treatment Status	N	%	Range of visits
Ever detailed	2076	27.90	
Above median	1234	16.60	2-37
Below median	842	11.30	1-1
Never detailed	5362	72.10	
Total NPI	7438	100.00	

Appendix Table A6: Summary Statistics of Patients in the Physician-Level Analytic Sample

			Detailing Volume in Washout Period	
Patient Characteristics	Never Detailed	Ever Detailed	Low Exposure	High Exposure
Male (%)	42	38	38	38
Race (%)				
White	86	92	91	92
Black	7	4	5	4
Hispanic	2	1	1	1
Asian	1	1	1	1
Native	0	0	0	0
Age, mean (SD)	64 (16)	68 (13)	68 (15)	69 (13)
SSDI recipient (%)	22	16	16	16
Medicare-Medicaid dually eligible (%)	60	29	30	27
Has chronic condition(s) (%)	90	91	91	91
Has other chronic or potentially disabling condition(s) (%)	89	91	91	91
Has cancer (%)	13	12	12	12
Has leukemia/lymphoma (%)	5	5	5	5
Has opioid use disorder (%)	5	4	4	4
Has Parts A/B/D, not C (%)	67	55	56	55
Ever OUD event (%)	1.53	0.92	0.96	0.9
Ever fall or fracture (%)	0.62	0.66	0.68	0.65

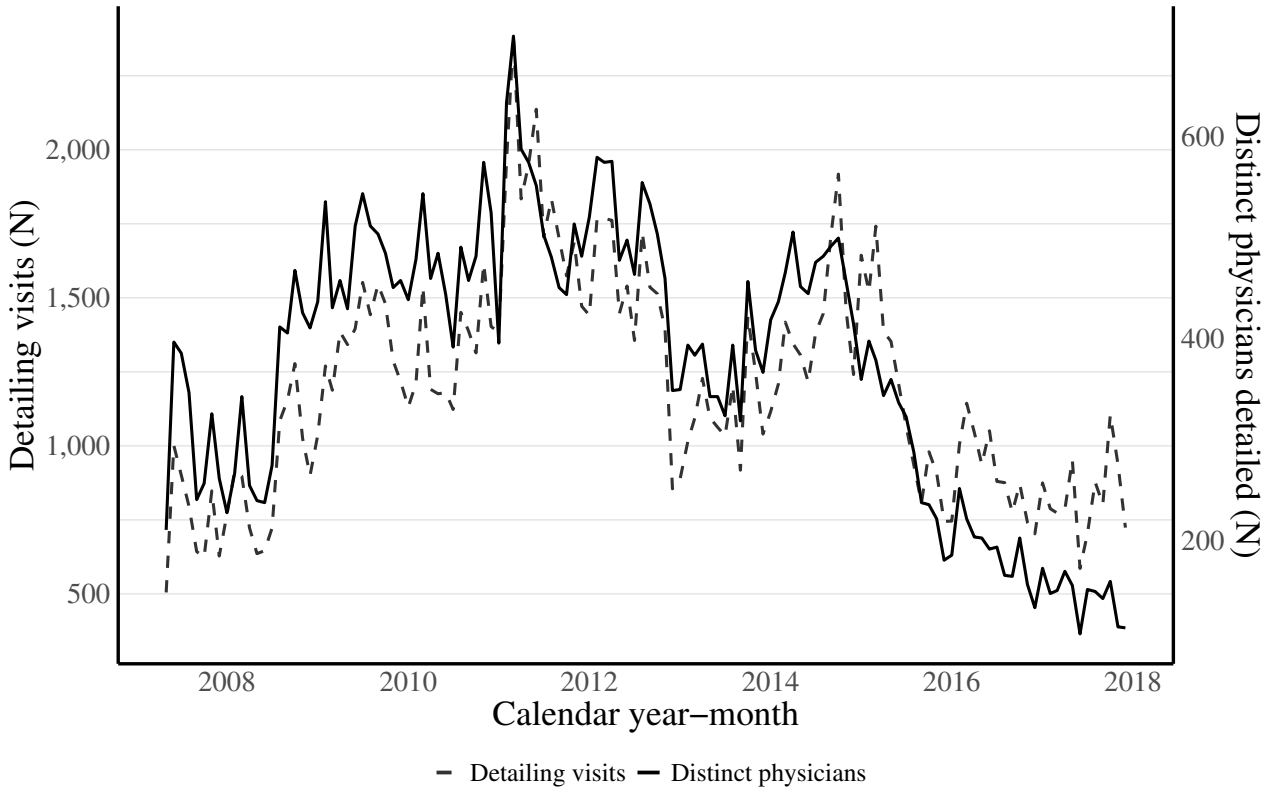
This table reports descriptive statistics of characteristics of the patients treated by physicians in the physician-level analytic sample. Patient characteristics are stored as averages or shares at the physician-quarter level in the underlying data. For time-variant measures, means and standard deviations (SD) were calculated from observations in the pre-period for ever-detailed physician, and were calculated based on all quarters a physician was observed for never-detailed physician. Detailing volume in the washout period refers to the number of detailing visits a physician receives within the first four quarters of their first detailing visit. Low-exposure physicians receive 1 visit during the washout period; high-exposure physicians receive 2–37 visits during the washout period. "Has chronic condition(s)" refers to the beneficiary having a record of one of 27 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)" includes an additional 8 chronic conditions, including mental health; tobacco use, alcohol and drug use; developmental disorders; disability-related conditions; and behavioral health. "Ever OUD event" is an indicator that the patient ever had one of the OUD-related adverse health events we study (captured by our composite measure), and "Ever fall or fracture" is an indicator that the patient ever had an inpatient or emergency department record associated with a fall or fracture.

Appendix Table A7: Pre-treatment Physician and Patient Characteristics by Detailing Intensity

	Detailing Volume in Washout Period			
	2	3–4	5–7	8–37
Panel A. Physician characteristics				
Male (%)	71	69	73	70
Primary care specialty (%)	74	80	81	77
Total patients per quarter, mean (SD)	24 (19)	27 (19)	30 (22)	26 (20)
Total Purdue Rx per quarter, mean (SD)	5.81 (6.69)	6.84 (6.60)	6.62 (6.84)	8.06 (9.20)
Total Purdue MME per quarter, mean (SD)	7538 (12993)	9381 (15148)	9399 (14787)	13876 (41222)
Total opioid Rx per quarter, mean (SD)	9.07 (9.22)	10.96 (9.78)	10.78 (9.78)	12.93 (13.44)
Total opioid MME per quarter, mean (SD)	12643 (20029)	15074 (23841)	15101 (20799)	20052 (46210)
Panel B. Patient characteristics				
Male (%)	39	37	39	39
Race (%)				
White	92	93	92	91
Black	4	3	3	4
Age, mean (SD)	68 (14)	70 (11)	69 (12)	68 (13)
SSDI recipient (%)	16	15	15	19
Has chronic condition(s) (%)	91	91	91	91
Ever OUD event (%)	0.97	0.90	0.88	0.84
Ever fall or fracture (%)	0.76	0.65	0.61	0.56
Total physicians	309	309	308	308

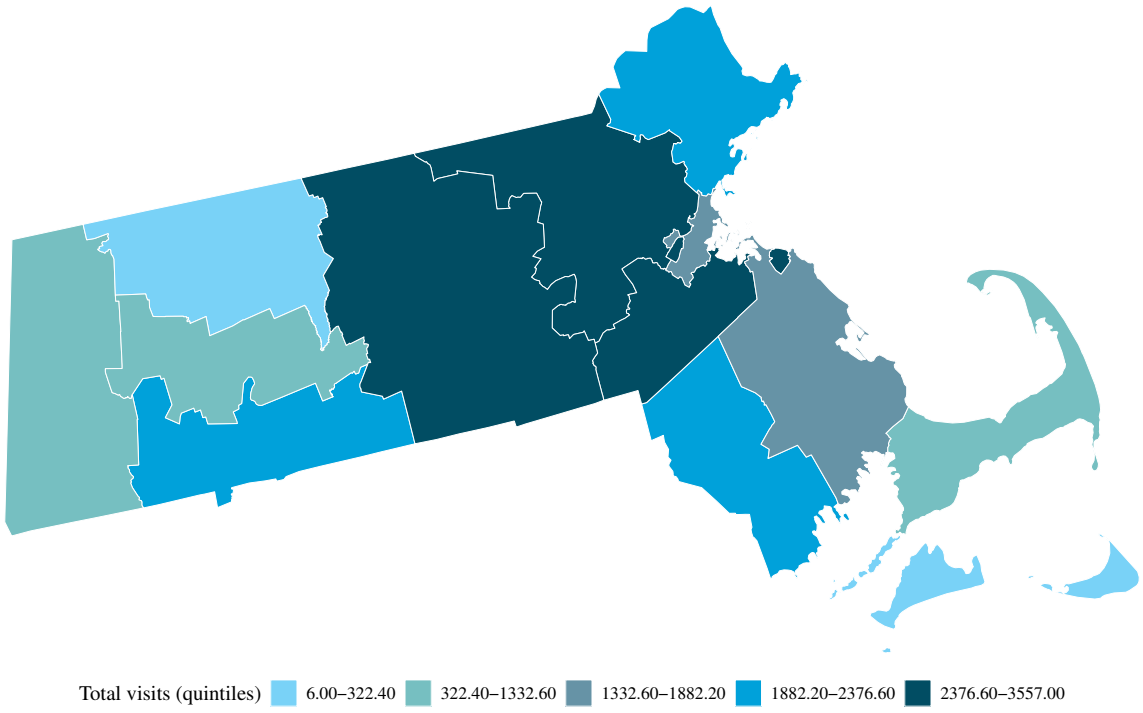
This table compares characteristics of physicians (Panel A) and the patients they treat (Panel B) by volume of detailing visits in the washout period. Detailing volume in the washout period refers to the number of detailing visits a physician receives in relative quarters $r \in [0, 3]$, where $r = 0$ is the quarter in which the physician first has a detailing visit.

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

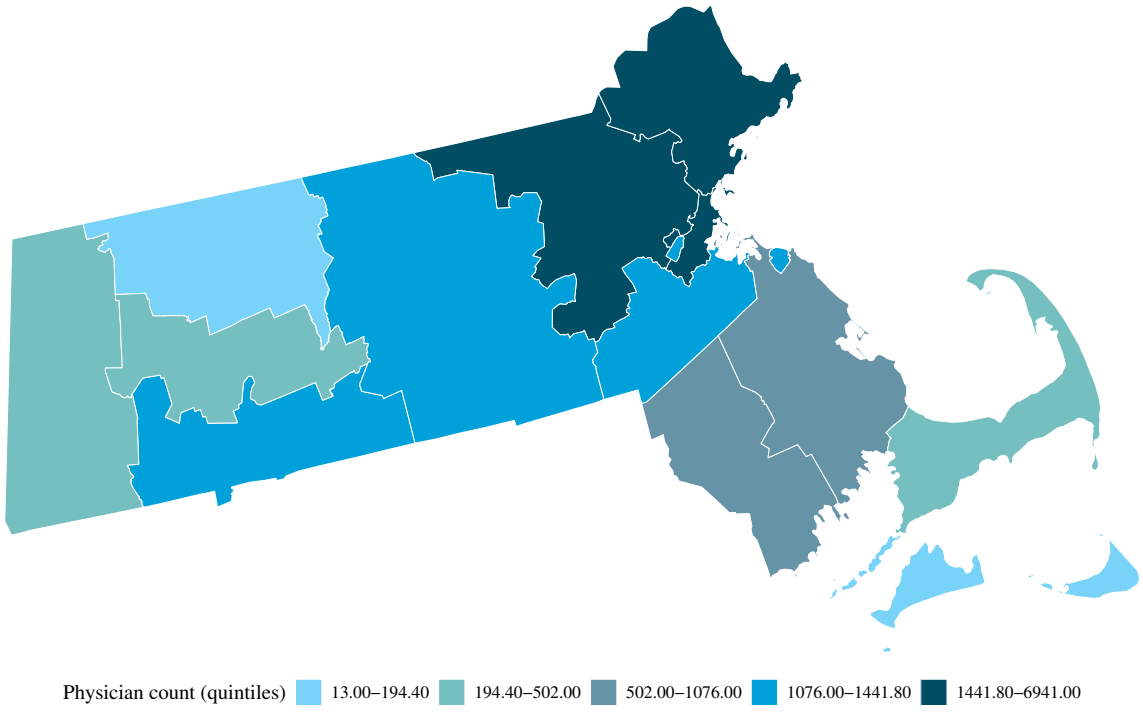


This figure plots the number of detailing visits (dashed line; left axis) and detailed physicians (solid black line; right axis) in Massachusetts from 2007–2017, as recorded in the Purdue Pharma detailing records.

Appendix Figure A2: Detailing Activity, County Level, 2011



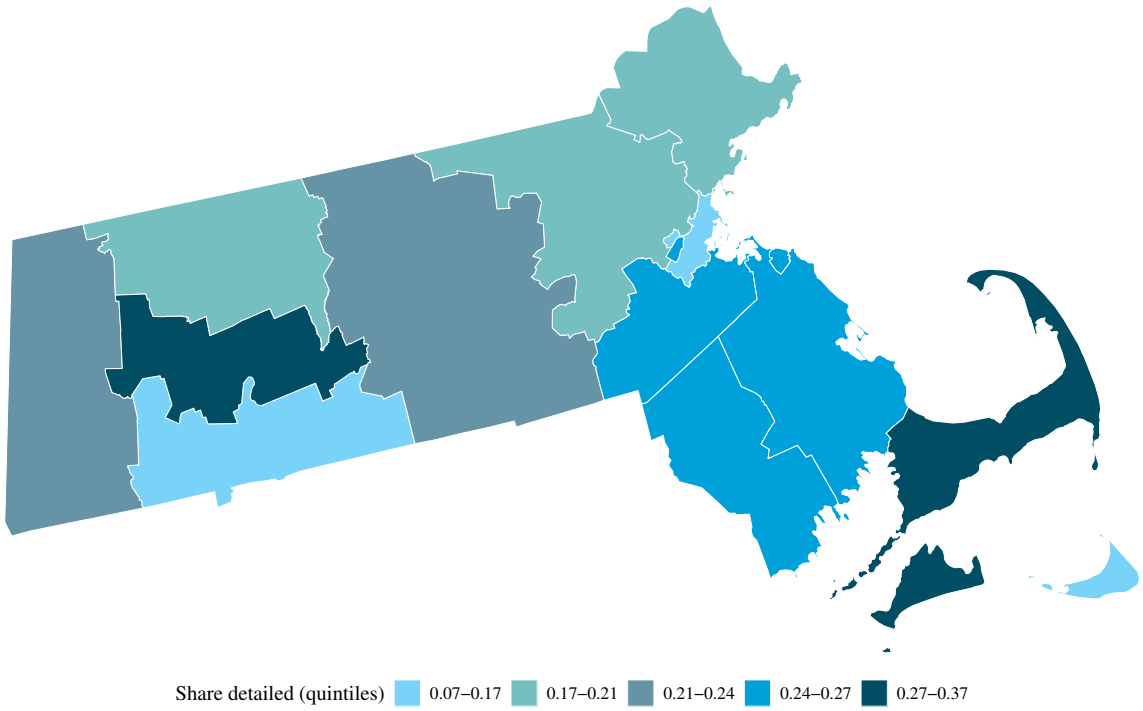
(a) Detailing Visits



(b) Medicare-Participating Physicians

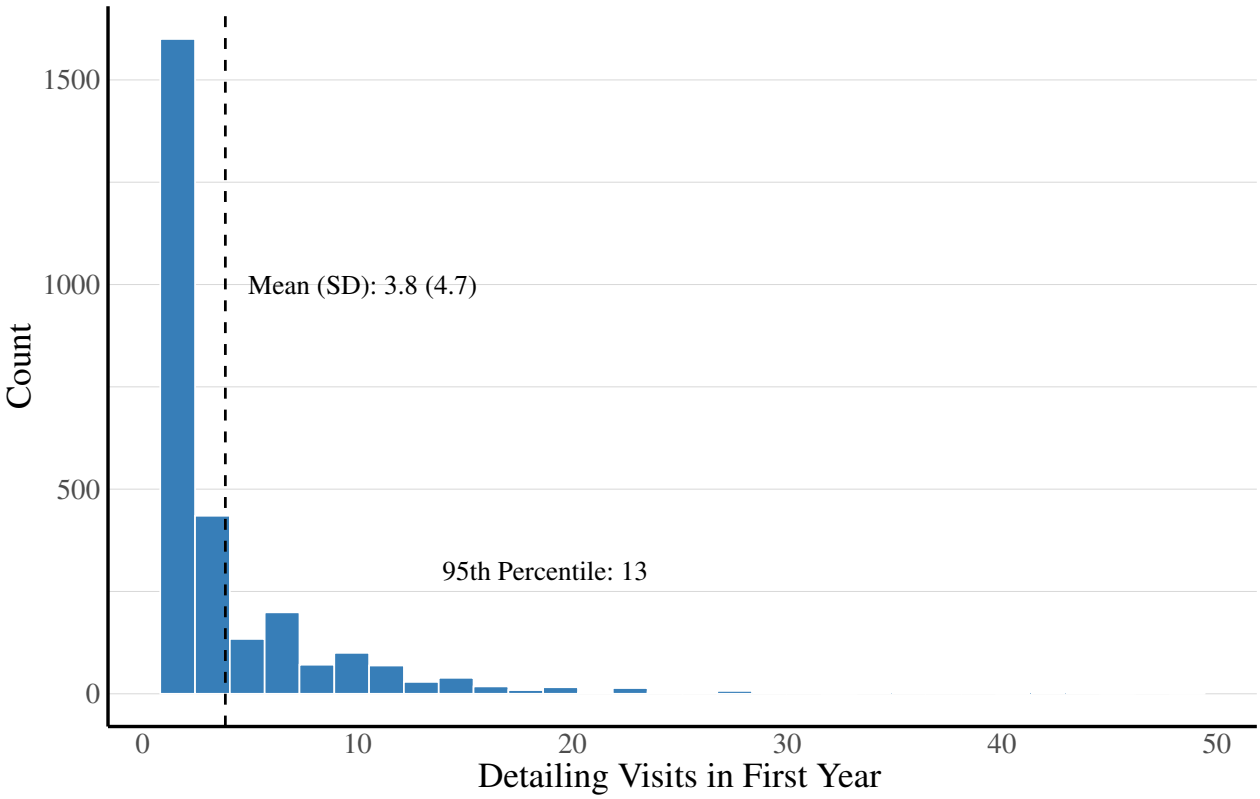
This figure maps county-level variation in detailing and Medicare participation in Massachusetts in 2011. Panel (a) maps the volume of detailing visits between Purdue Pharma representatives and all providers in Massachusetts in quintiles. Panel (b) maps the volume of physicians observed treating patients in Medicare in quintiles.

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



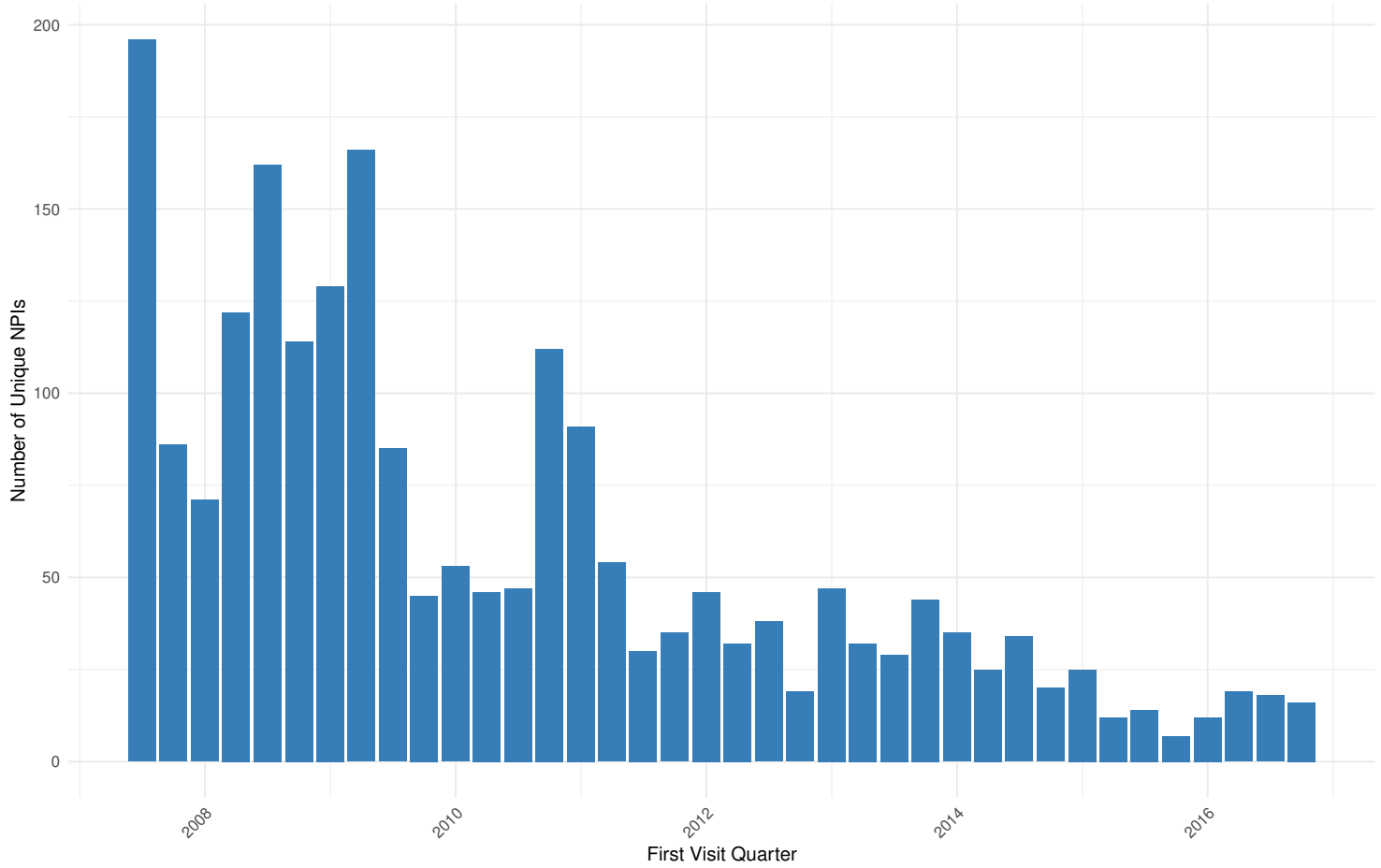
This figure maps county-level variation in the share of Medicare-participating physicians who were detailed by Purdue Pharma representatives in 2011, in quintiles.

Appendix Figure A4: Distribution of Volume of Detailing Visits in the Washout Period



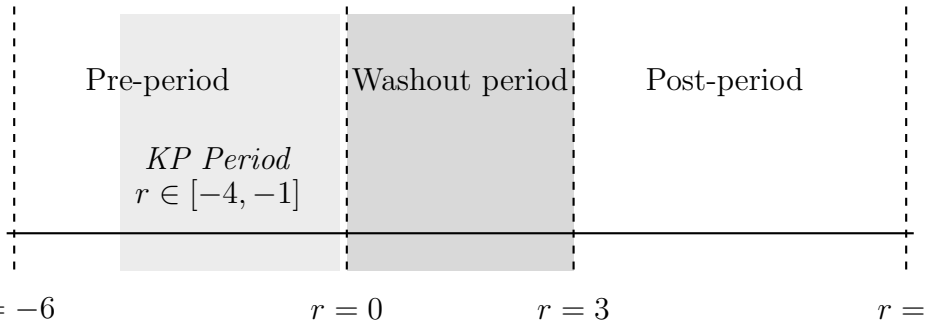
This figure plots the distribution of the volume of detailing visits among in-sample physicians during the washout period. The washout period refers to relative quarters $r \in [0, 3]$, where $r = 0$ is the quarter in which the physician first has a detailing visit. Note that while 95% of providers have 13 or fewer visits in the first year, a subset of providers have over 50. Our analytic sample construction ultimately drops providers with over 37 detailing visits per year.

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



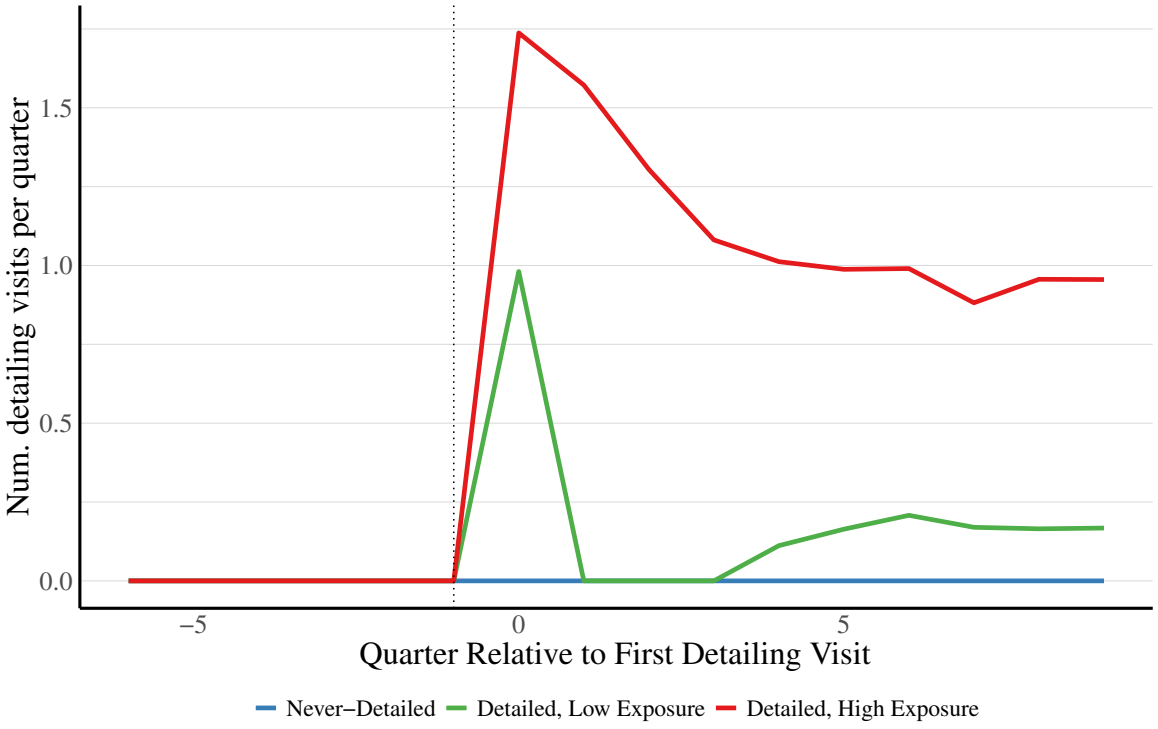
This figure plots the distribution of the timing (in calendar quarter) of in-sample physicians' first detailing visits.

Appendix Figure A6: Empirical Timeline

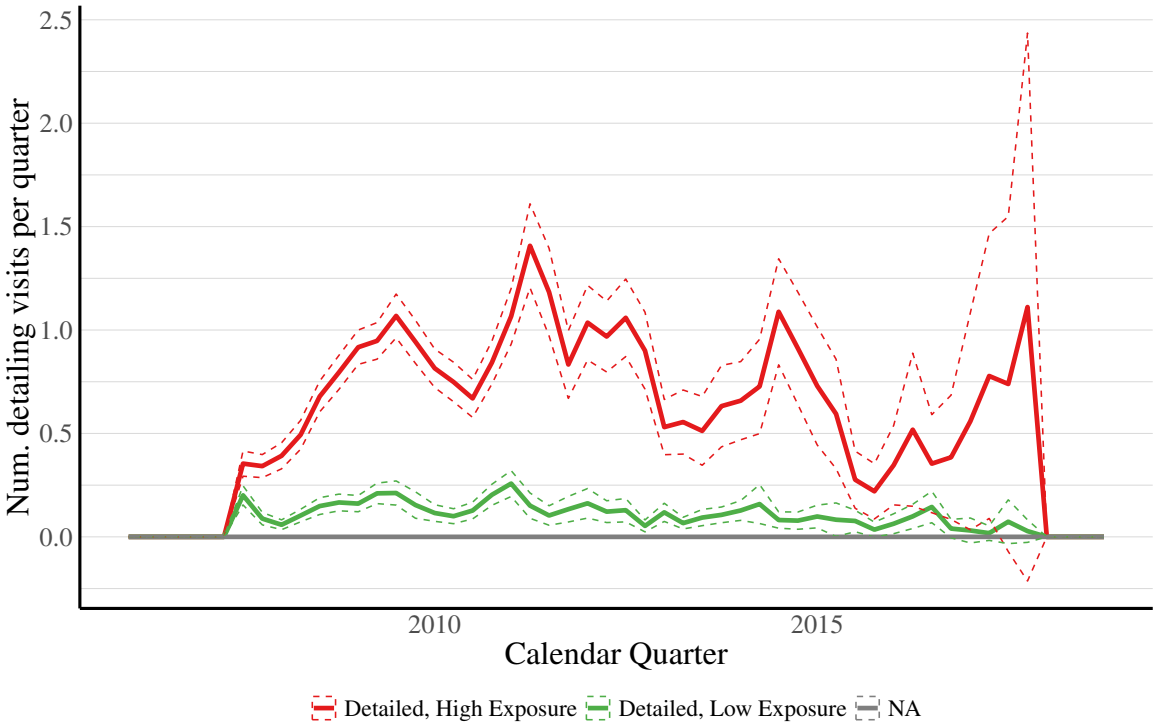


This figure illustrates the empirical timeline, where r represents the quarter relative to the physician's first detailing visit, which occurs in $r = 0$. "KP" stands for Key Provider; the *Key Provider* period is relevant for the beneficiary-level analyses only. The pre-period is defined as $r \in [-6, 1]$; the washout period is defined as $r \in [0, 3]$; the post-period is defined as $r \in [4, 9]$.

Appendix Figure A7: Trends in Detailing Visits



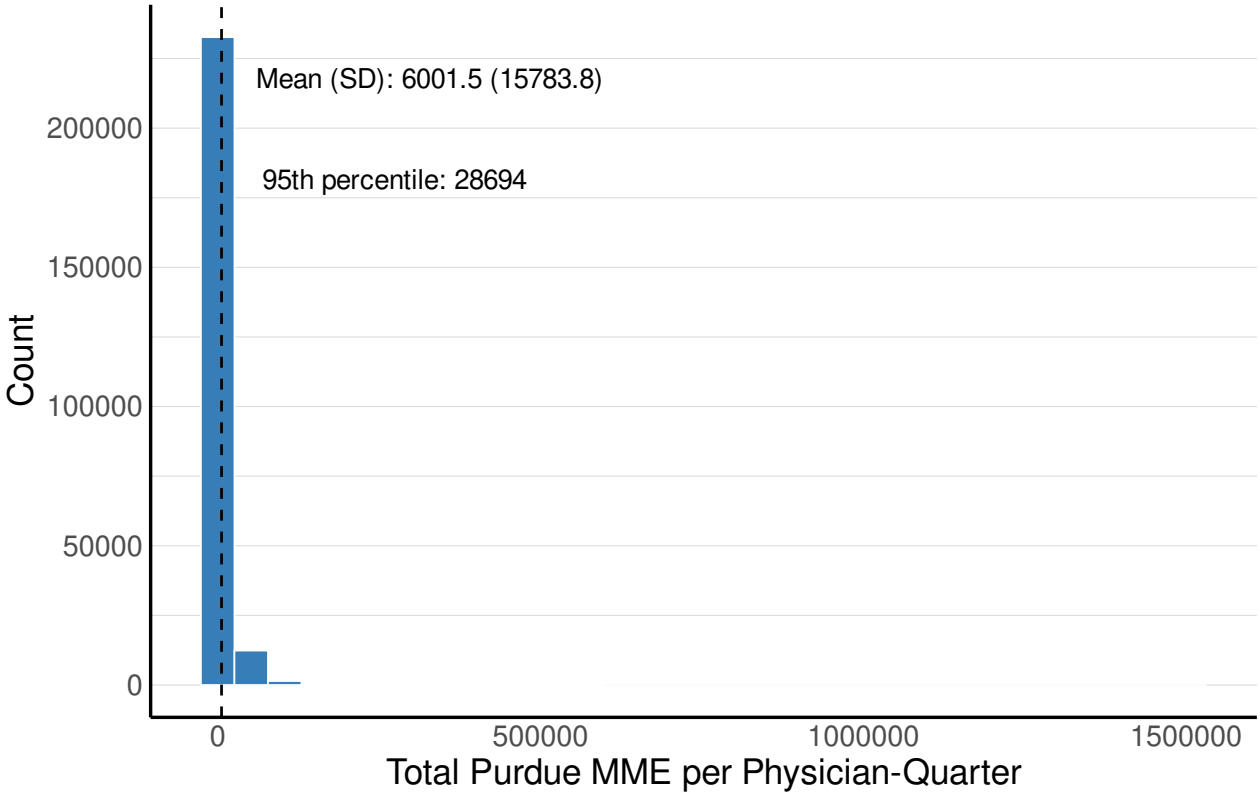
(a) Relative to First Visit



(b) Trends over Time

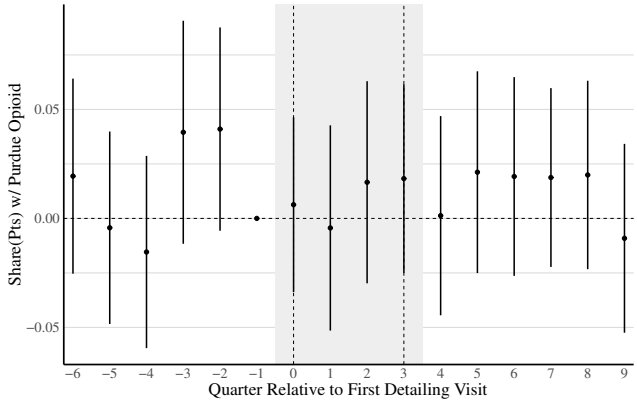
This figure plots unadjusted trends in the average number of detailing visits per physician over time. Panel (a) plots detailing visits per quarter relative to first detailing visit. Never-detailed physicians are randomly assigned a placebo treatment date for purposes of comparison. Panel (b) plots detailing visits per calendar quarter. Trend lines represent unweighted averages within detailing category. "Never-detailed" physicians are those who never receive a detailing visit. "Detailed, Low Exposure" are physicians who receive 1 visit in the washout period. "Detailed, High Exposure" are physicians who receive 2–37 visits in the washout period. "NA" in panel (b) represent physicians who are never detailed.

Appendix Figure A8: Distribution of Total MME per Physician-Quarter Associated with Purdue-Manufactured Opioids

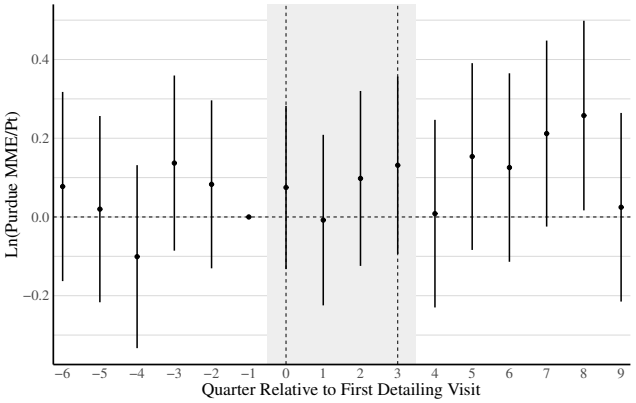


This figure plots the distribution of total MME attributed to Purdue-manufactured opioids aggregated to the physician-quarter level, for in-sample physicians.

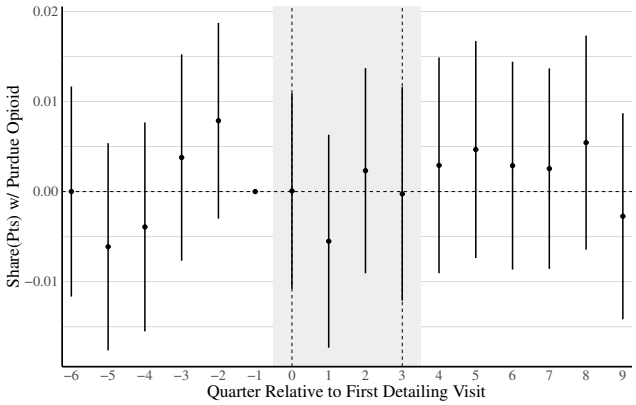
Appendix Figure A9: Additional Intensive Margin Opioid Utilization Measures Relative to Detailing Exposure, Physician Level



(a) Num. Opioid Rx per Pt



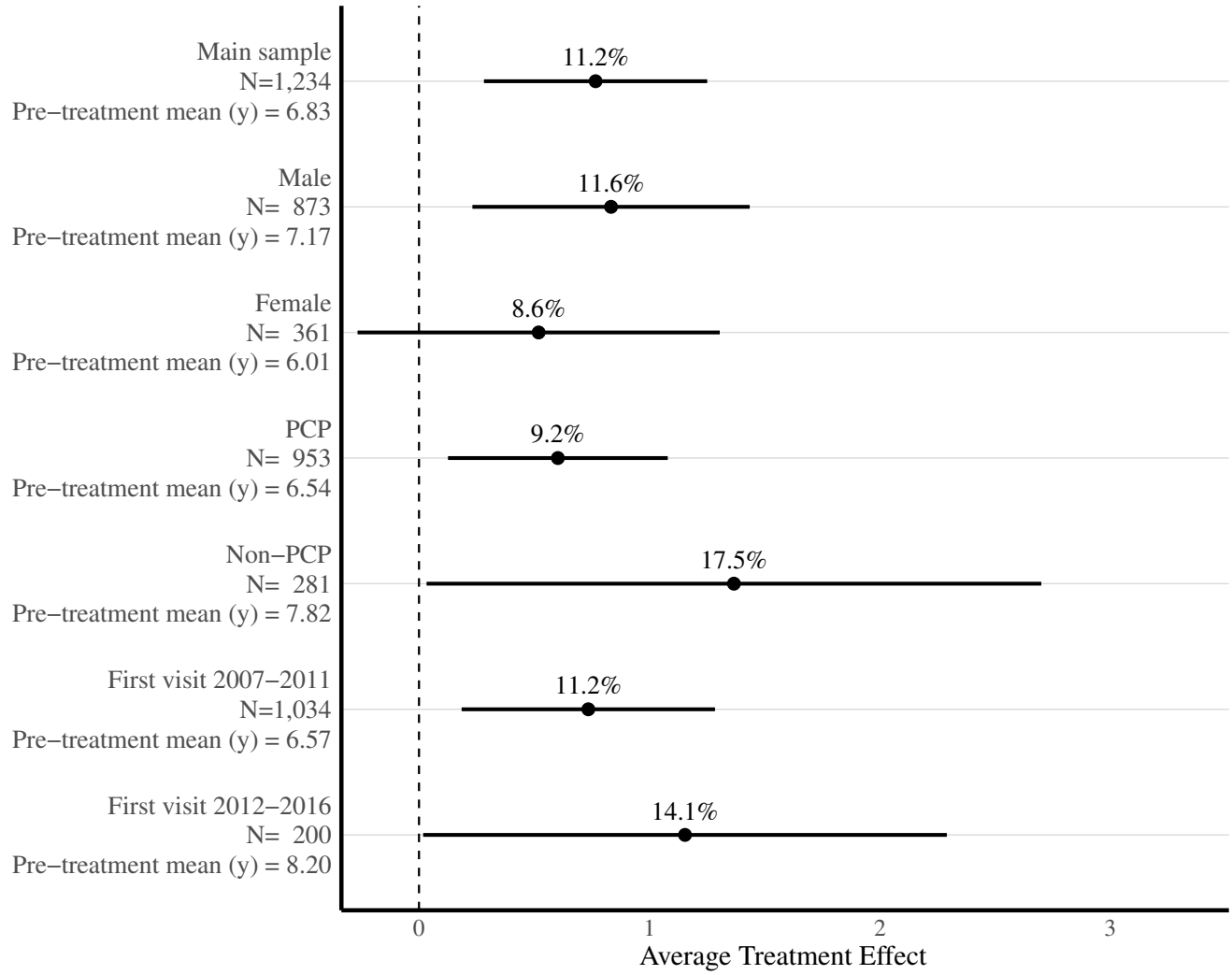
(b) Ln(MME/Pt)



(c) Share Pts w/ Opioid Rx

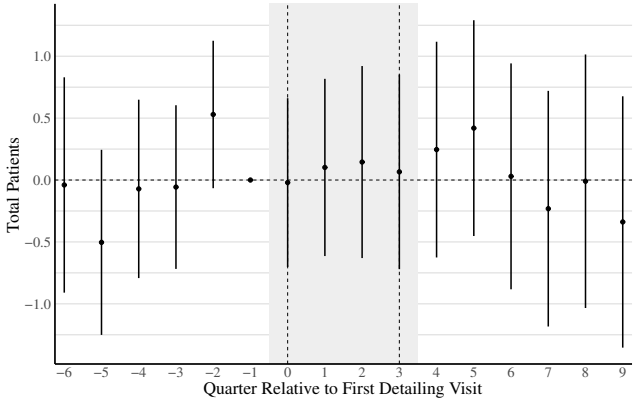
This figure plots estimated coefficients from Equation 1 for our main physician-level analysis. The grayed out period around relative quarters $r \in [0, 3]$ represents the "washout period;" $r = 0$ is the first quarter a physician experiences a detailing visit. Fixed effects are included for physicians and calendar quarter. All models include high-exposure physicians (ever-detailed physicians with 2–37 visits in the washout period), low-exposure physicians (ever-detailed physicians with 1 visit in the washout period), and never-detailed physicians. Omitted category is an indicator for relative quarter $r = -1$. Standard errors are clustered at the physician level and are reported in parentheses. All outcomes are defined at the physician-quarter level. 95% confidence intervals are given by solid black lines.

Appendix Figure A10: Heterogeneity in Average Treatment Effect of Detailing Exposure on Total Purdue Opioid Prescriptions per Quarter, by Physician Characteristics

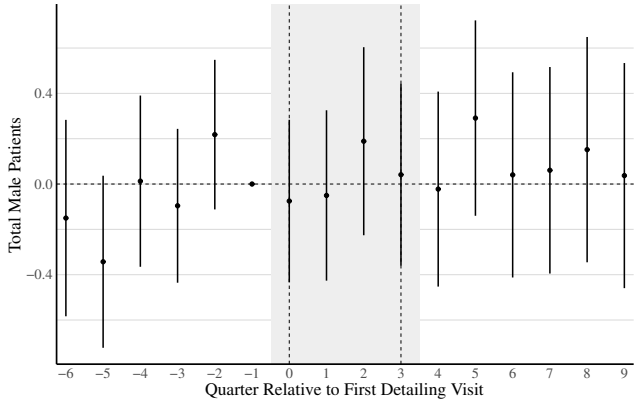


This figure reports coefficients from estimating Equation 2 on sub-populations of physicians: (i) male physicians ("Male"); (ii) female physicians ("Female"); (iii) physicians with a primary care specialty of internal medicine or family medicine ("PCP"); (iv) physicians with a non-primary care specialty, of which the five most common are orthopaedic surgery, psychiatry & neurology, anesthesiology, physical medicine & rehabilitation, and surgery ("Non-PCP"); (v) physicians whose first detailing visit occurs between 2007–2011 ("First visit 2007–2011"); (vi) physician whose first detailing visit occurs between 2012–2017 ("First visit 2012-2017"); (vii) sample that excludes physicians whose first visit was in 2007 ("Drop first visit in 2007"). The coefficient for the main sample is reported for comparison ("Main sample"). The pre-period ($r \in [-6, -1]$) is the omitted reference category. The sample includes high-exposure, low-exposure, and never-detailed physicians. Fixed effects are included for physician and calendar quarter. All outcomes are defined at the physician-quarter level. Sample sizes of the high-exposure (treatment) group are given by "N." Means are based on pre-treatment values for high-exposure detailed physicians and are given as "pre-treatment mean (y)." Standard errors are clustered at the physician level; 90% confidence intervals are given by solid black lines.

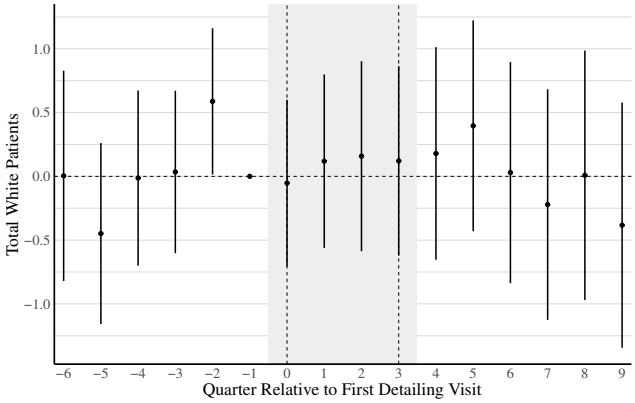
Appendix Figure A11: Balance in Patient Demographics Across Detailing Exposure



(a) Total Patients



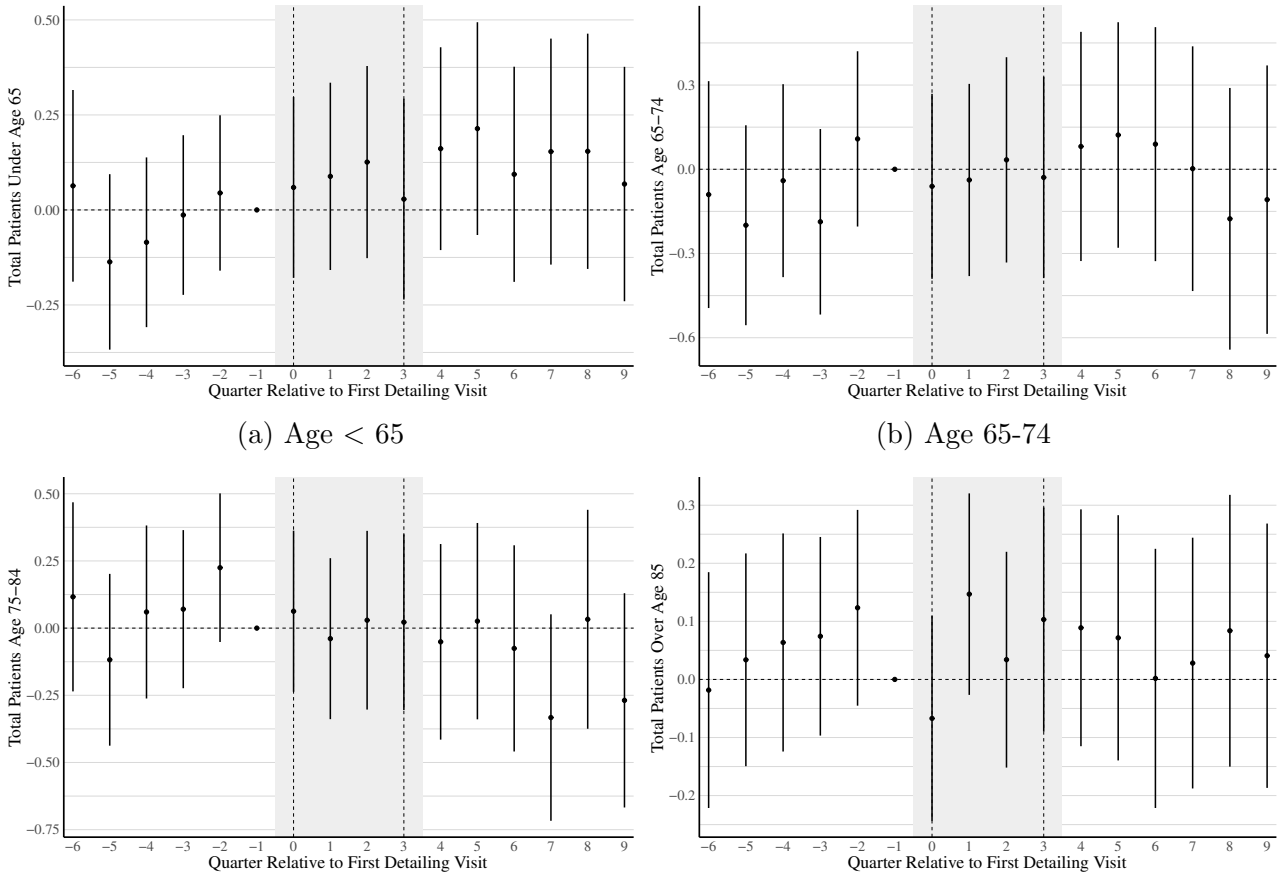
(b) Total Male Patients



(c) Total White Patients

This figure plots estimated coefficients from Equation 1 for our main physician-level analysis. The grayed out period around relative quarters $r \in [0, 3]$ represents the "washout period;" $r = 0$ is the first quarter a physician experiences a detailing visit. Fixed effects are included for physicians and calendar quarter. All models include high-exposure physicians (ever-detailed physicians with 2–37 visits in the washout period), low-exposure physicians (ever-detailed physicians with 1 visit in the washout period), and never-detailed physicians. Omitted category is an indicator for relative quarter $r = -1$. Standard errors are clustered at the physician level and are reported in parentheses. All outcomes are defined at the physician-quarter level. 95% confidence intervals are given by solid black lines.

Appendix Figure A12: Balance in Patient Age Across Detailing Exposure

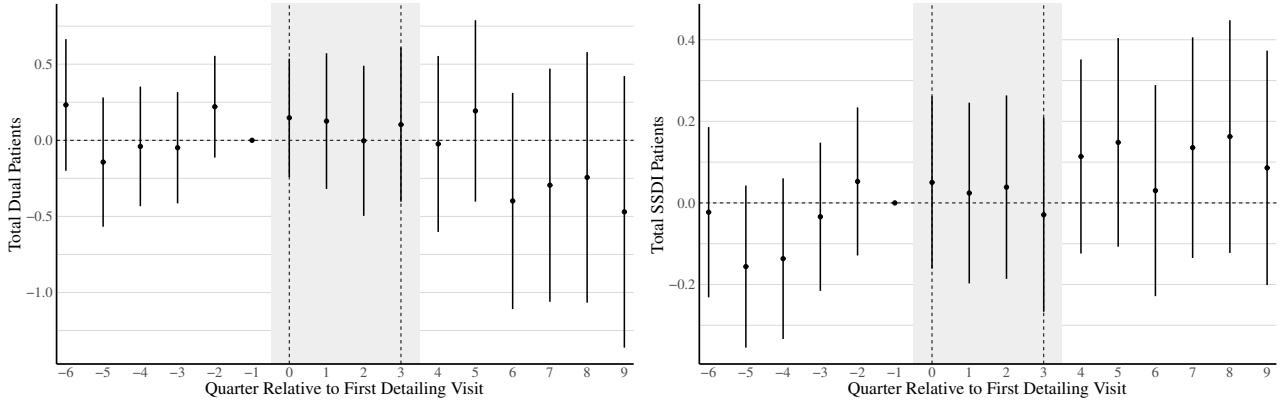


(a) Age < 65 (b) Age 65-74

(c) Age 75-84 (d) Age > 85

This figure plots estimated coefficients from Equation 1 for our main physician-level analysis. The grayed out period around relative quarters $r \in [0, 3]$ represents the "washout period;" $r = 0$ is the first quarter a physician experiences a detailing visit. Fixed effects are included for physicians and calendar quarter. All models include high-exposure physicians (ever-detailed physicians with 2–37 visits in the washout period), low-exposure physicians (ever-detailed physicians with 1 visit in the washout period), and never-detailed physicians. Omitted category is an indicator for relative quarter $r = -1$. Standard errors are clustered at the physician level and are reported in parentheses. All outcomes are defined at the physician-quarter level. 95% confidence intervals are given by solid black lines.

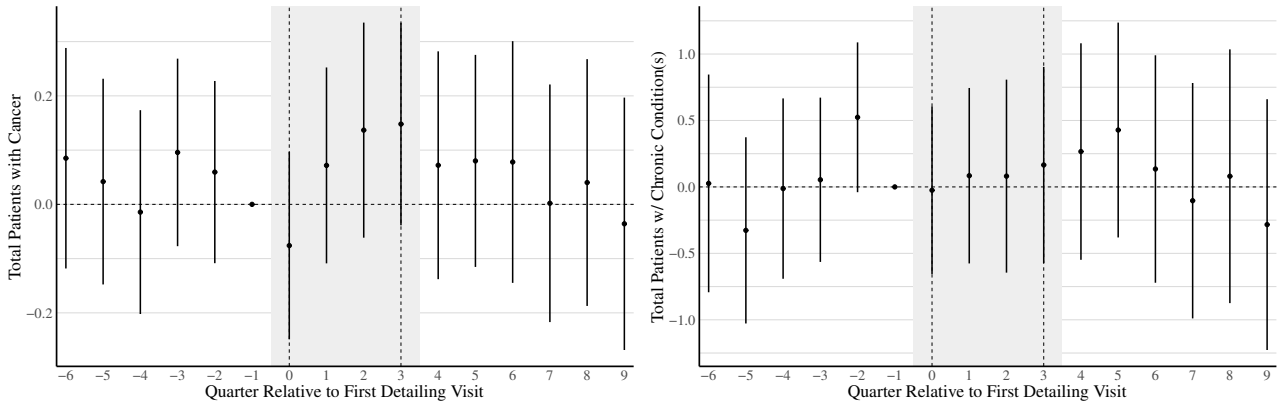
Appendix Figure A13: Balance in Enrollment Characteristics Across Detailing Exposure



(a) Total Duals (b) Total SSDI Recipients

This figure plots estimated coefficients from Equation 1 for our main physician-level analysis. The grayed out period around relative quarters $r \in [0, 3]$ represents the "washout period;" $r = 0$ is the first quarter a physician experiences a detailing visit. Fixed effects are included for physicians and calendar quarter. All models include high-exposure physicians (ever-detailed physicians with 2–37 visits in the washout period), low-exposure physicians (ever-detailed physicians with 1 visit in the washout period), and never-detailed physicians. Omitted category is an indicator for relative quarter $r = -1$. Standard errors are clustered at the physician level and are reported in parentheses. All outcomes are defined at the physician-quarter level. 95% confidence intervals are given by solid black lines.

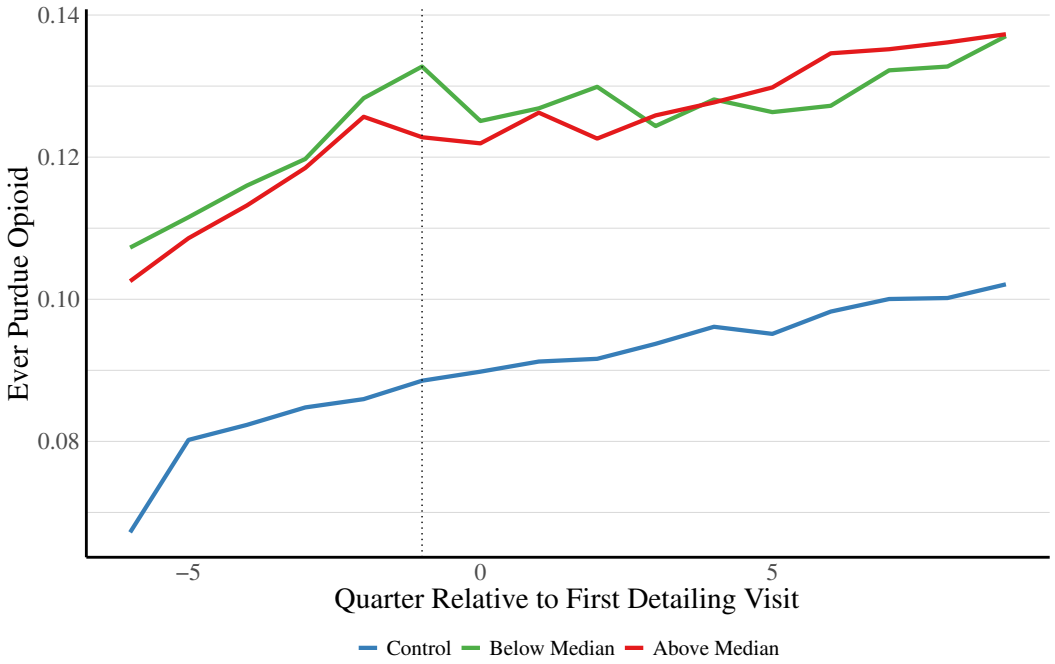
Appendix Figure A14: Balance in Clinical Characteristics Across Detailing Exposure



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

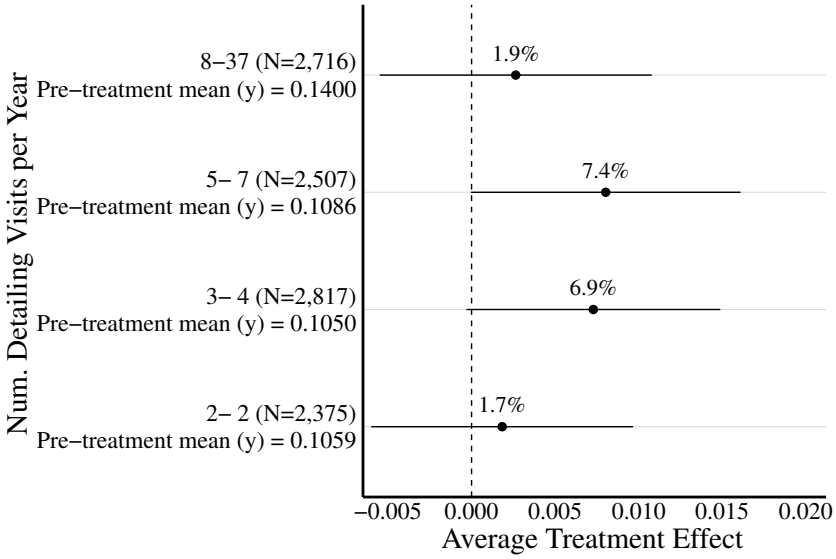
This figure plots estimated coefficients from Equation 1 for our main physician-level analysis. The grayed out period around relative quarters $r \in [0, 3]$ represents the "washout period;" $r = 0$ is the first quarter a physician experiences a detailing visit. Fixed effects are included for physicians and calendar quarter. All models include high-exposure physicians (ever-detailed physicians with 2–37 visits in the washout period), low-exposure physicians (ever-detailed physicians with 1 visit in the washout period), and never-detailed physicians. Omitted category is an indicator for relative quarter $r = -1$. Standard errors are clustered at the physician level and are reported in parentheses. All outcomes are defined at the physician-quarter level. 95% confidence intervals are given by solid black lines.

Appendix Figure A15: Unadjusted Trends in Probability of Filling a Purdue Opioid Prescription Relative to First Detailing Visit, Beneficiary Level

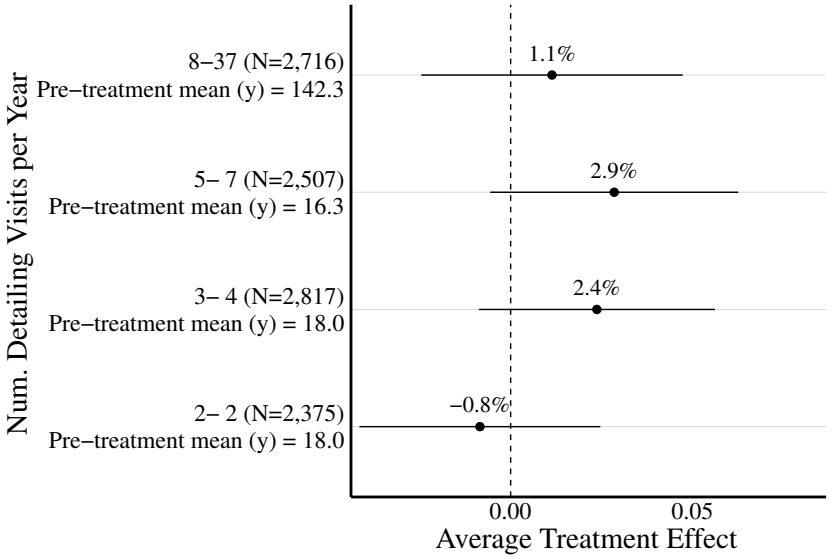


This figure plots unadjusted trends in the average probability of a Purdue opioid prescription fill per beneficiary in the quarter relative to their key provider's first detailing visit. Beneficiaries in the never-detailed cohort are randomly assigned a placebo treatment date for purposes of comparison. Trend lines represent unweighted averages within detailing category. "Never-detailed" beneficiaries are those who are not yet treated by a detailed physician as of that quarter. "Detailed, Low Exposure" are beneficiaries whose key provider receives 1 visit in the washout period. "Detailed, High Exposure" are beneficiaries whose key provider receives 2–37 visits in the washout period.

Appendix Figure A16: Dose-Response to Detailing Exposure on Opioid Utilization, Beneficiary Level



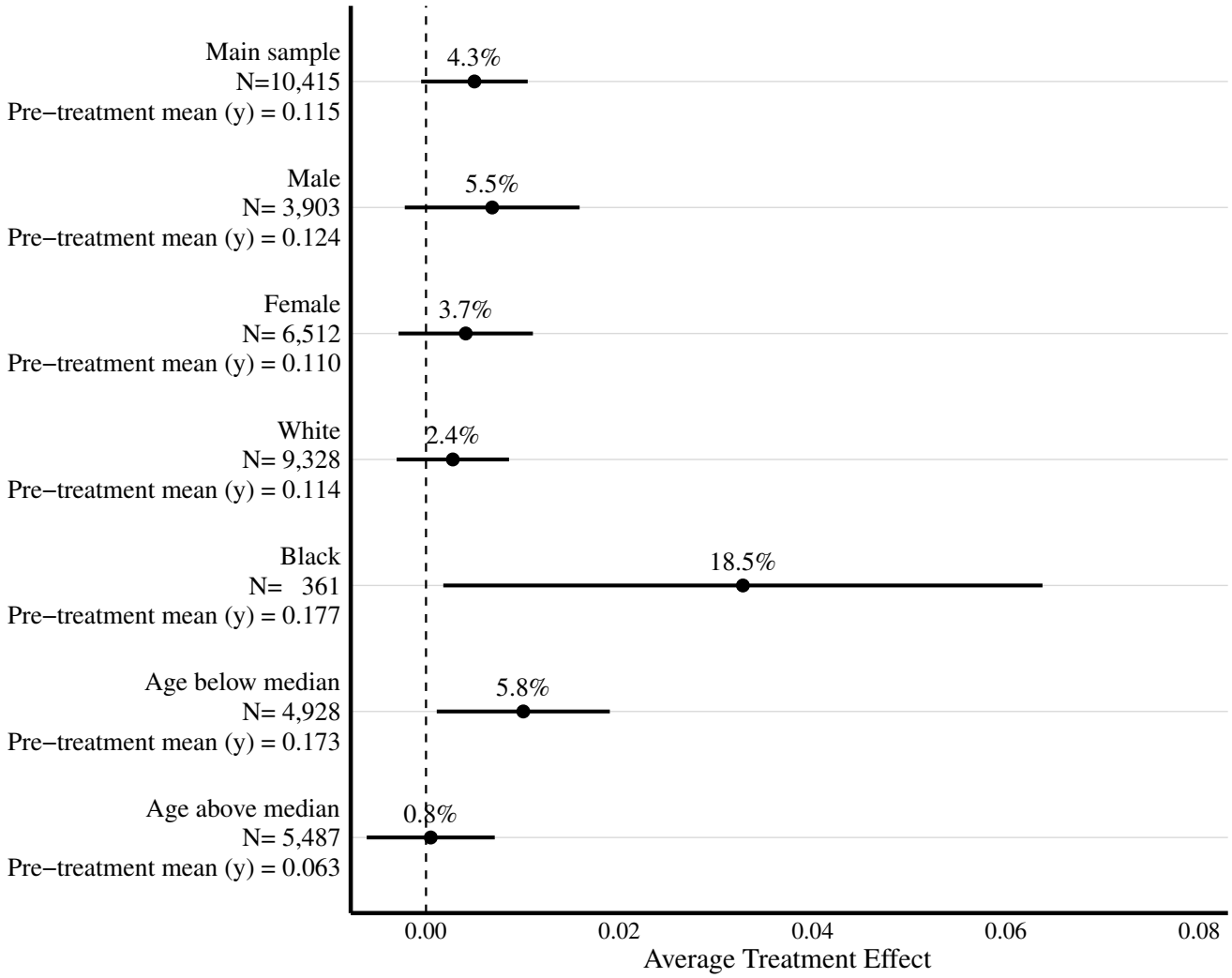
(a) Pr Purdue Opioid Rx



(b) Ln(MME)

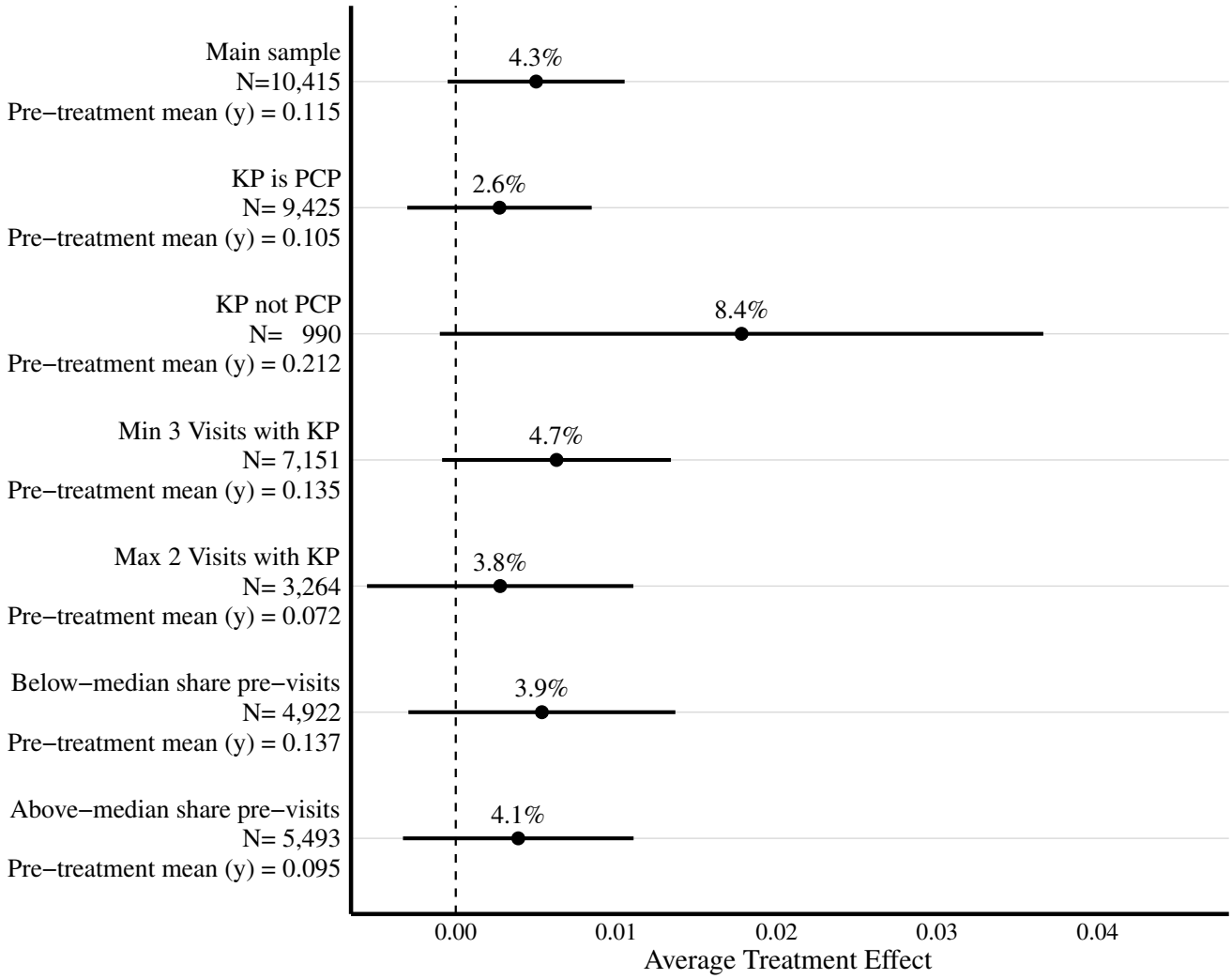
This figure reports coefficients from estimating Equation 2 separately on beneficiaries in four bins of detailing visit volume (corresponding to physician-level detailing quartiles) in the washout period. Panel (a) reports results for estimating the model on the probability that a beneficiary fills a prescription for a Purdue-manufactured opioid in a given quarter. Panel (b) reports results for estimating the model on log MME associated with the beneficiary's Purdue-manufactured opioid prescriptions in a given quarter. The pre-period ($r \in [-6, -1]$) is the omitted reference category. The sample includes beneficiaries with high-exposure, low-exposure, and never-detailed key providers. Fixed effects are included for beneficiary and calendar quarter. All outcomes are defined at the beneficiary-quarter level. Sample sizes of the high-exposure (treatment) group are given by "N." Means are based on pre-treatment values for beneficiaries with high-exposure key providers and are given as "pre-treatment mean (y)." Means for log-transformed variables are reported in the untransformed form. Standard errors are clustered at the physician level; 90% confidence intervals are given by solid black lines.

Appendix Figure A17: Heterogeneity in Average Treatment Effect of Detailing Exposure on Probability of Purdue Opioid Prescription, by Beneficiary Characteristics



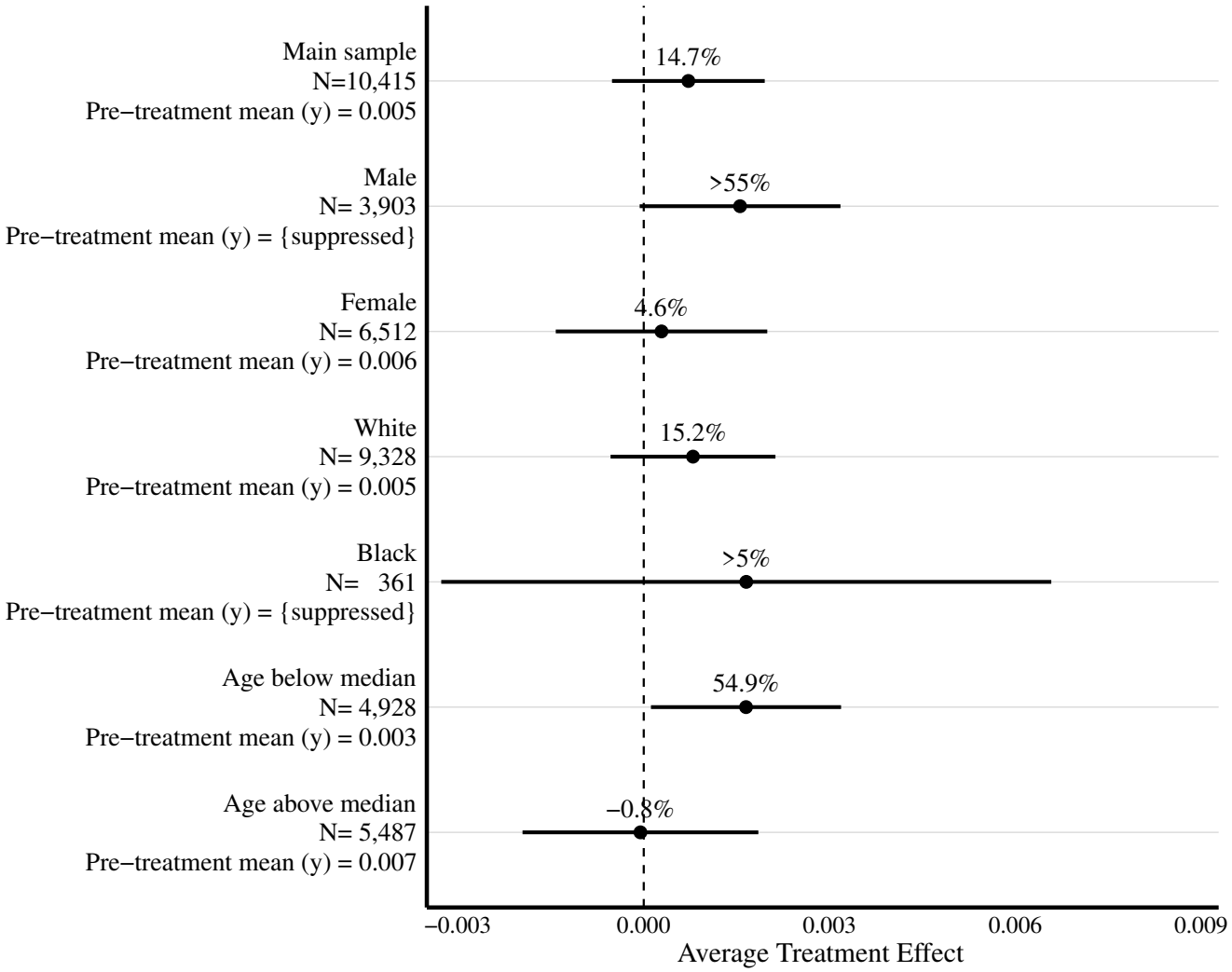
This figure reports coefficients from estimating Equation 2 on sub-populations of beneficiaries: (i) male beneficiaries ("Male"); (ii) female beneficiaries ("Female"); (iii) White beneficiaries ("White"); (iv) black beneficiaries ("Black"); (v) beneficiaries of below-median age ("Age 13-69"); and (vi) beneficiaries of above-median age ("Age $>$ 69"). The coefficient for the main sample is reported for comparison ("Main sample"). The pre-period ($r \in [-6, -1]$) is the omitted reference category. The sample includes beneficiaries with high-exposure, low-exposure, and never-detailed key providers. Fixed effects are included for beneficiary and calendar quarter. All outcomes are defined at the beneficiary-quarter level. Sample sizes of the high-exposure (treatment) group are given by "N." Means are based on pre-treatment values for beneficiaries with high-exposure key providers and are given as "pre-treatment mean (y)." Standard errors are clustered at the physician level; 90% confidence intervals are given by solid black lines.

Appendix Figure A18: Heterogeneity in Average Treatment Effect of Detailing Exposure on Probability of Purdue Opioid Prescription, by Key Provider Characteristics



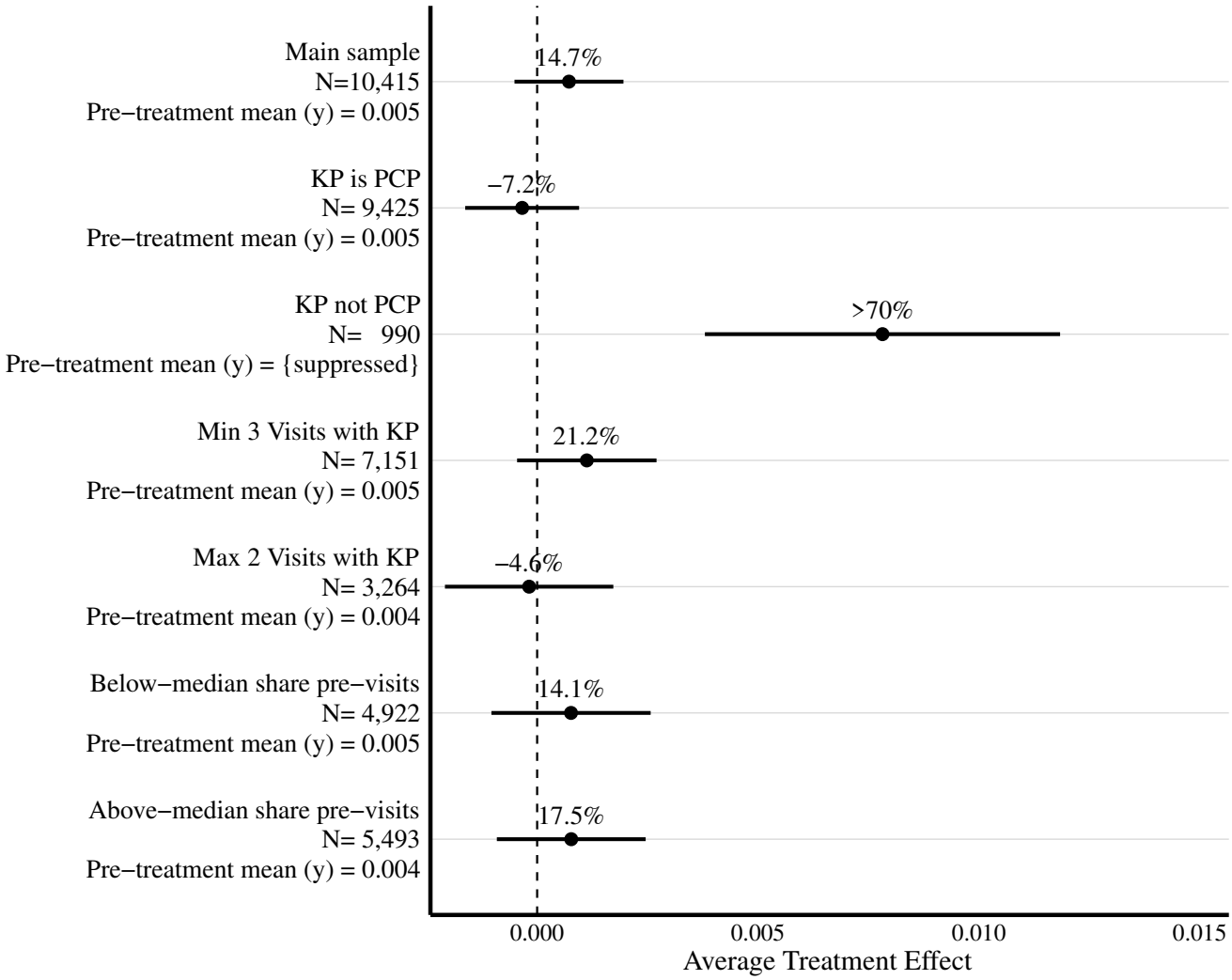
This figure reports coefficients from estimating Equation 2 on sub-populations of key providers: (i) beneficiaries whose key provider had a primary care specialty (family medicine or internal medicine; "KP is PCP"); (ii) beneficiaries whose key provider had a non-primary care specialty ("KP not PCP"); (iii) beneficiaries with at least 3 visits with their key provider in the pre-period ("Min 3 Visits w/ KP"); (iv) beneficiaries with at most 2 visits with their key provider in the pre-period ("Max 2 Visits w/ KP"); (v) beneficiaries who had a below-median share of visits with their key provider in the pre-period ("Below-median share pre-visits"); and (vi) beneficiaries who had an above-median share of visits with their key provider in the pre-period ("Above-median share pre-visits"). The coefficient for the main sample is reported for comparison ("Main sample"). The pre-period ($r \in [-6, -1]$) is the omitted reference category. The sample includes beneficiaries with high-exposure, low-exposure, and never-detailed key providers. Fixed effects are included for beneficiary and calendar quarter. All outcomes are defined at the beneficiary-quarter level. Sample sizes of the high-exposure (treatment) group are given by "N." Means are based on pre-treatment values for beneficiaries with high-exposure key providers and are given as "pre-treatment mean (y)." Standard errors are clustered at the physician level; 90% confidence intervals are given by solid black lines.

Appendix Figure A19: Heterogeneity in Average Treatment Effect of Detailing Exposure on Probability of Fall or Fracture, by Beneficiary Characteristics



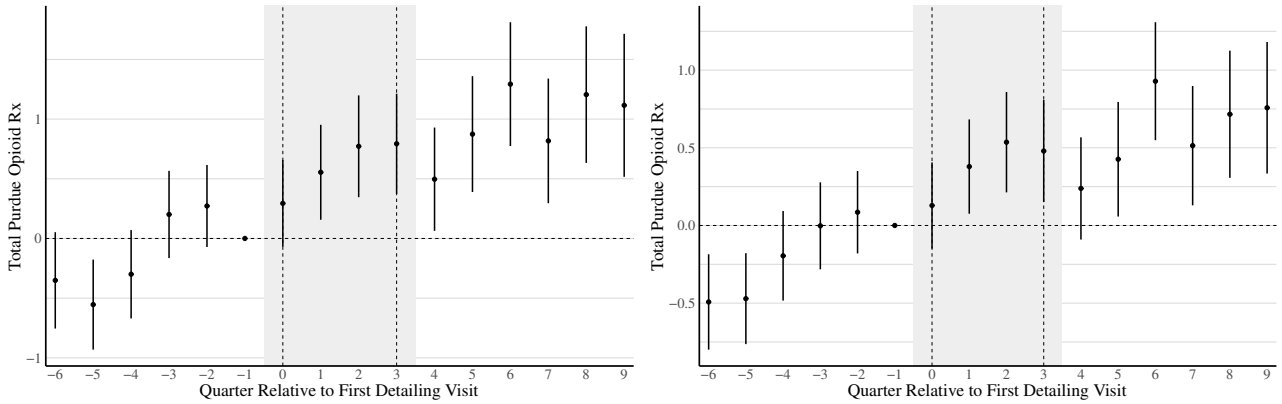
This figure reports coefficients from estimating Equation 2 on sub-populations of beneficiaries: (i) male beneficiaries ("Male"); (ii) female beneficiaries ("Female"); (iii) White beneficiaries ("White"); (iv) black beneficiaries ("Black"); (v) beneficiaries of below-median age ("Age 13-69"); and (vi) beneficiaries of above-median age ("Age > 69"). The coefficient for the main sample is reported for comparison ("Main sample"). The pre-period ($r \in [-6, -1]$) is the omitted reference category. The sample includes beneficiaries with high-exposure, low-exposure, and never-detailed key providers. Fixed effects are included for beneficiary and calendar quarter. All outcomes are defined at the beneficiary-quarter level. Sample sizes of the high-exposure (treatment) group are given by "N." Means are based on pre-treatment values for beneficiaries with high-exposure key providers and are given as "pre-treatment mean (y)." Means and relative percent changes that would violate cell suppression thresholds are redacted (indicated as "suppressed" for means, and > [minimum threshold] for percent changes). Standard errors are clustered at the physician level; 90% confidence intervals are given by solid black lines.

Appendix Figure A20: Heterogeneity in Average Treatment Effect of Detailing Exposure on Probability of Fall or Fracture, by Key Provider Characteristics



This figure reports coefficients from estimating Equation 2 on sub-populations of key providers: (i) beneficiaries whose key provider had a primary care specialty (family medicine or internal medicine; "KP is PCP"); (ii) beneficiaries whose key provider had a non-primary care specialty ("KP not PCP"); (iii) beneficiaries with at least 3 visits with their key provider in the pre-period ("Min 3 Visits w/ KP"); (iv) beneficiaries with at most 2 visits with their key provider in the pre-period ("Max 2 Visits w/ KP"); (v) beneficiaries who had a below-median share of visits with their key provider in the pre-period ("Below-median share pre-visits"); and (vi) beneficiaries who had an above-median share of visits with their key provider in the pre-period ("Above-median share pre-visits"). The coefficient for the main sample is reported for comparison ("Main sample"). The pre-period ($r \in [-6, -1]$) is the omitted reference category. The sample includes beneficiaries with high-exposure, low-exposure, and never-detailed key providers. Fixed effects are included for beneficiary and calendar quarter. All outcomes are defined at the beneficiary-quarter level. Sample sizes of the high-exposure (treatment) group are given by "N." Means are based on pre-treatment values for beneficiaries with high-exposure key providers and are given as "pre-treatment mean (y)." Standard errors are clustered at the physician level; 90% confidence intervals are given by solid black lines.

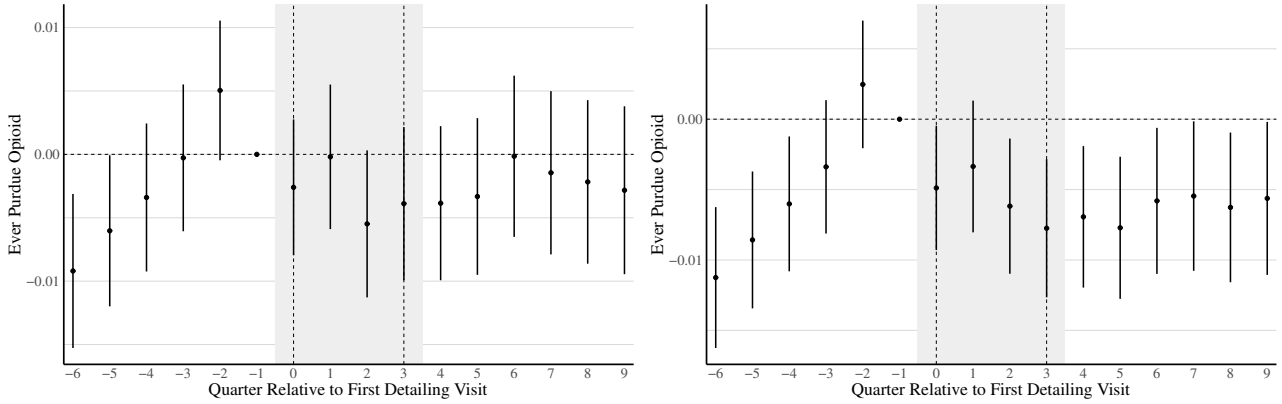
Appendix Figure A21: Alternative Comparisons, Physician Level



(a) High-Exposure Detailed v Never-Detailed (b) Ever- v Never-Detailed

This figure plots estimated coefficients from versions of Equation 1 for our main physician-level analysis. The grayed out period around relative quarters $r \in [0, 3]$ represents the "washout period;" $r = 0$ is the first quarter a physician experiences a detailing visit. Fixed effects are included for physicians and calendar quarter. Panel (a) compares high-exposure detailed physicians to never-detailed physicians (omitting low-exposure physicians). Panel (b) compares ever-detailed to never-detailed physicians (aggregating high- and low-exposure physicians into one ever-detailed category). Omitted category is an indicator for relative quarter $r = -1$. All outcomes are defined at the physician-quarter level. Standard errors are clustered at the physician level; 95% confidence intervals are given by solid black lines.

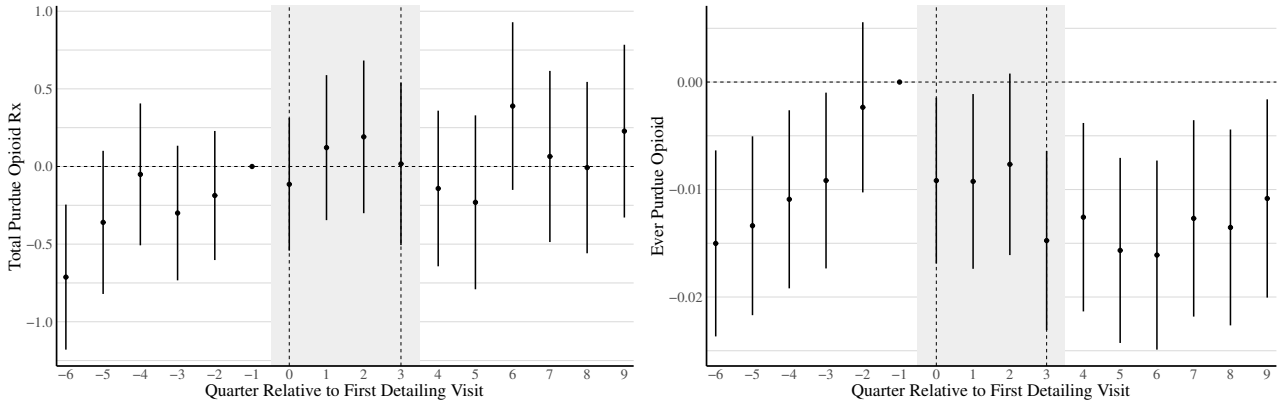
Appendix Figure A22: Alternative Comparisons, Beneficiary Level



(a) High-Exposure Detailed v Never Detailed (b) Ever- v Never-Detailed

This figure plots estimated coefficients from versions of Equation 1 for our main beneficiary-level analysis. The grayed out period around relative quarters $r \in [0, 3]$ represents the "washout period;" $r = 0$ is the first quarter a physician experiences a detailing visit. Fixed effects are included for physicians and calendar quarter. Panel (a) compares beneficiaries with high-exposure detailed key providers to beneficiaries never treated by detailed physicians (omitting low-exposure beneficiaries). Panel (b) aggregates beneficiaries with high- and low-exposure key providers into one cohort of beneficiaries with ever-detailed key providers and compare them to beneficiaries who are never treated by by detailed physicians. Omitted category is an indicator for relative quarter $r = -1$. All outcomes are defined at the physician-quarter level. Standard errors are clustered at the beneficiary level; 95% confidence intervals are given by solid black lines.

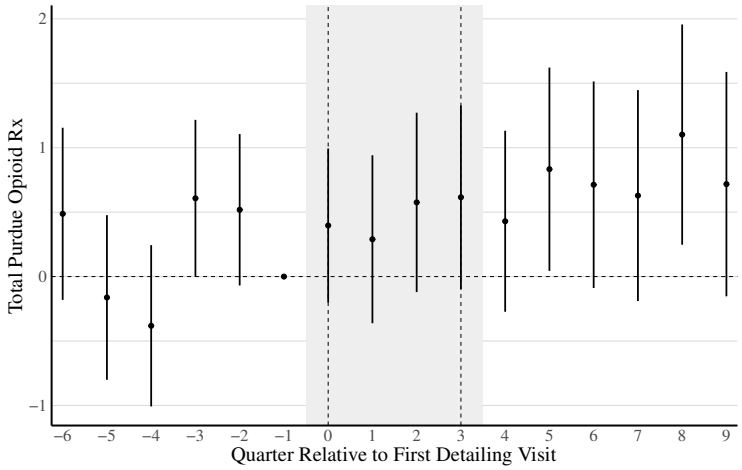
Appendix Figure A23: Low-Exposure versus Never-Detailed Physicians



(a) Physician Level (b) Beneficiary Level

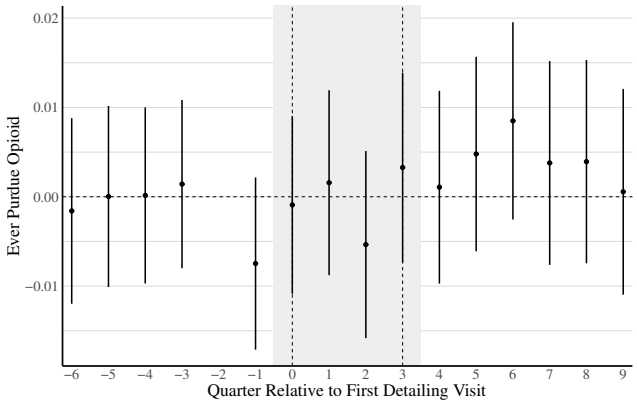
This figure plots estimated coefficients from versions of Equation 1. Panel (a) compares low-exposure detailed physicians to never-detailed physicians (omitting high-exposure physicians); panel (b) compares beneficiaries with low-exposure detailed key providers to beneficiaries never treated by detailed physicians (omitting high-exposure beneficiaries). The grayed out period around relative quarters $r \in [0, 3]$ represents the "washout period;" $r = 0$ is the first quarter a physician experiences a detailing visit. Fixed effects are included for physicians/beneficiaries (panel a/panel b) and calendar quarter. Omitted category is an indicator for relative quarter $r = -1$. All outcomes are defined at the physician- or beneficiary-quarter level (panels a and b, respectively). Standard errors are clustered at the physician/beneficiary level;95% confidence intervals are given by solid black lines.

Appendix Figure A24: Number of Purdue-Manufactured Opioid Prescriptions Relative to Detailing Exposure, Physician Level, Drop if First Detailing Visit in 2007



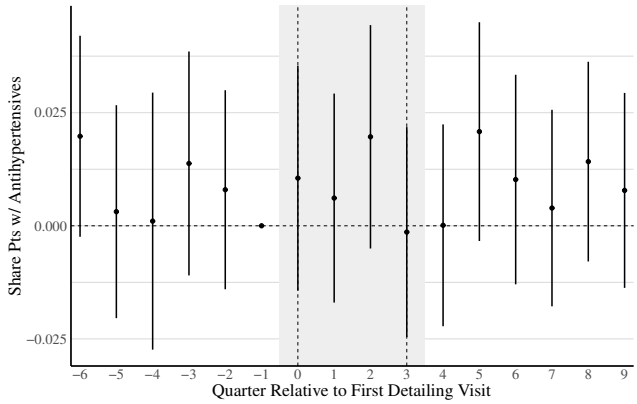
This table reports estimated coefficients from Equation 1 after dropping 278 ever-detailed providers whose first detailing visit was in 2007. The grayed out period around relative quarters $r \in [0, 3]$ represents the "washout period;" $r = 0$ is the first quarter a physician experiences a detailing visit. Fixed effects are included for physicians and calendar quarter. All models include high-exposure physicians (ever-detailed physicians with 2–37 visits in the washout period), low-exposure physicians (ever-detailed physicians with 1 visit in the washout period), and never-detailed physicians. Omitted category is an indicator for relative quarter $r = -1$. All outcomes are defined at the physician-quarter level. Standard errors are clustered at the physician level; 95% confidence intervals are given by solid black lines.

Appendix Figure A25: Beneficiary-Level Event Study, Full Sample, Omit Relative Quarter -2

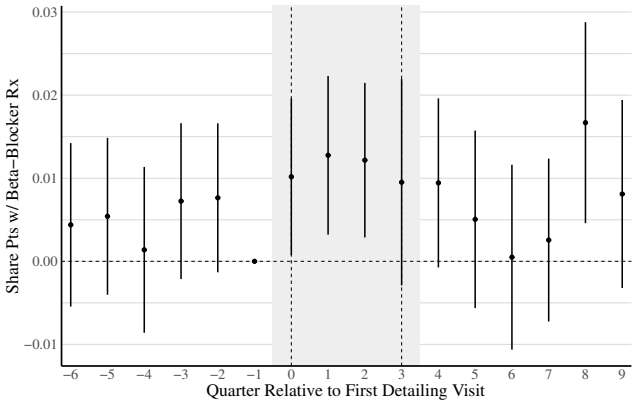


This table reports estimated coefficients from Equation 1 on our main physician-level sample. Omitted category is an indicator for relative quarter $r = -2$. The grayed out period around relative quarters $r \in [0, 3]$ represents the "washout period;" $r = 0$ is the first quarter a physician experiences a detailing visit. Fixed effects are included for physicians and calendar quarter. All models include high-exposure physicians (ever-detailed physicians with 2–37 visits in the washout period), low-exposure physicians (ever-detailed physicians with 1 visit in the washout period), and never-detailed physicians. All outcomes are defined at the physician-quarter level. Standard errors are clustered at the physician level; 95% confidence intervals are given by solid black lines.

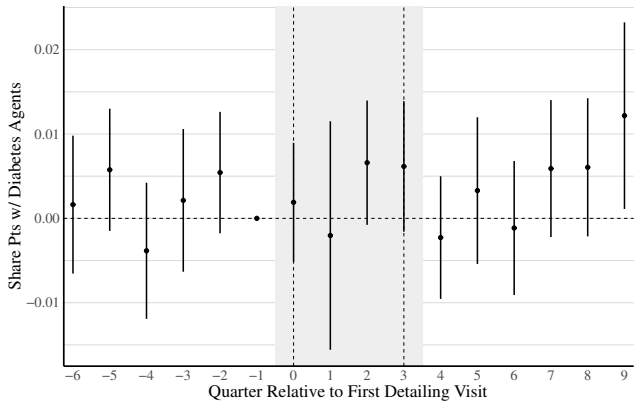
Appendix Figure A26: Negative Control Outcomes Relative to Detailing Exposure



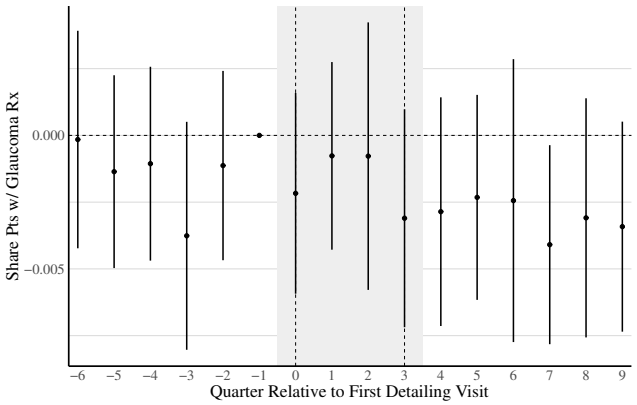
(a) Antihypertensives



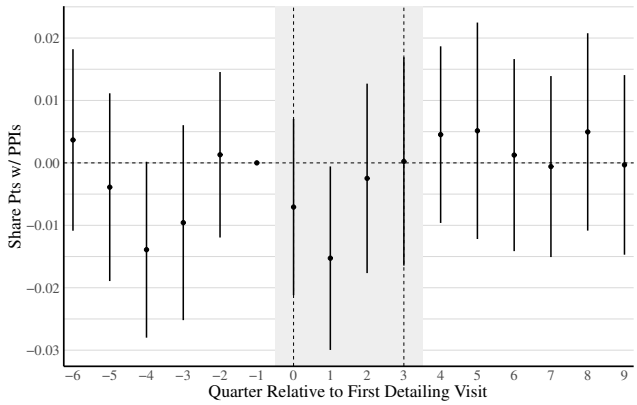
(b) β-blocker



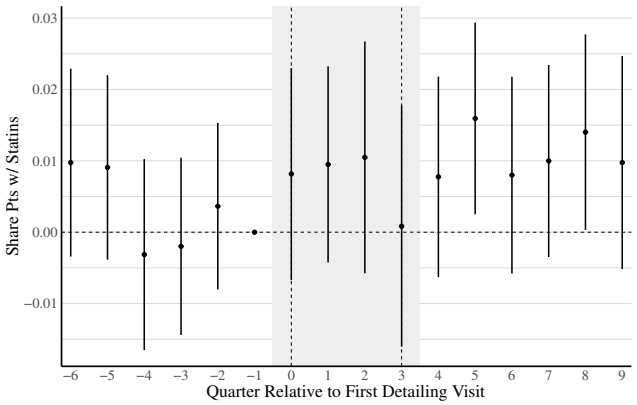
(c) Diabetes



(d) Glaucoma



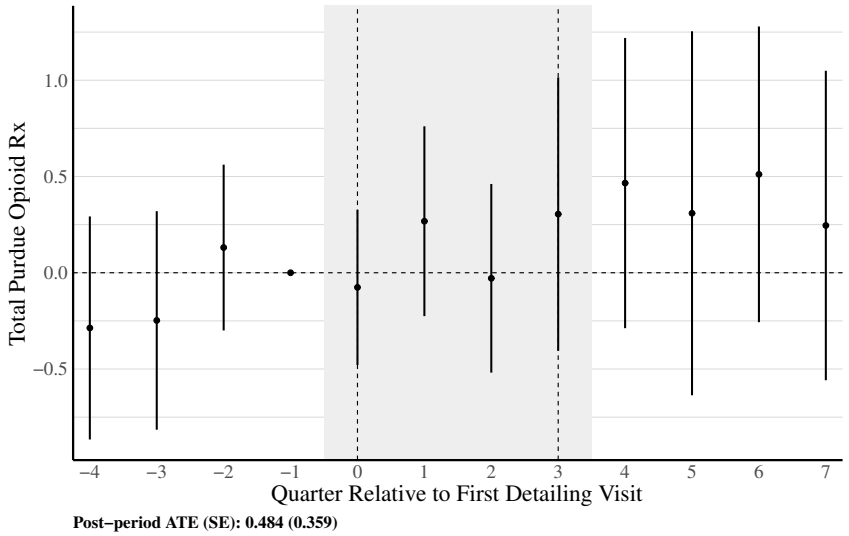
(e) PPI



(f) Statins

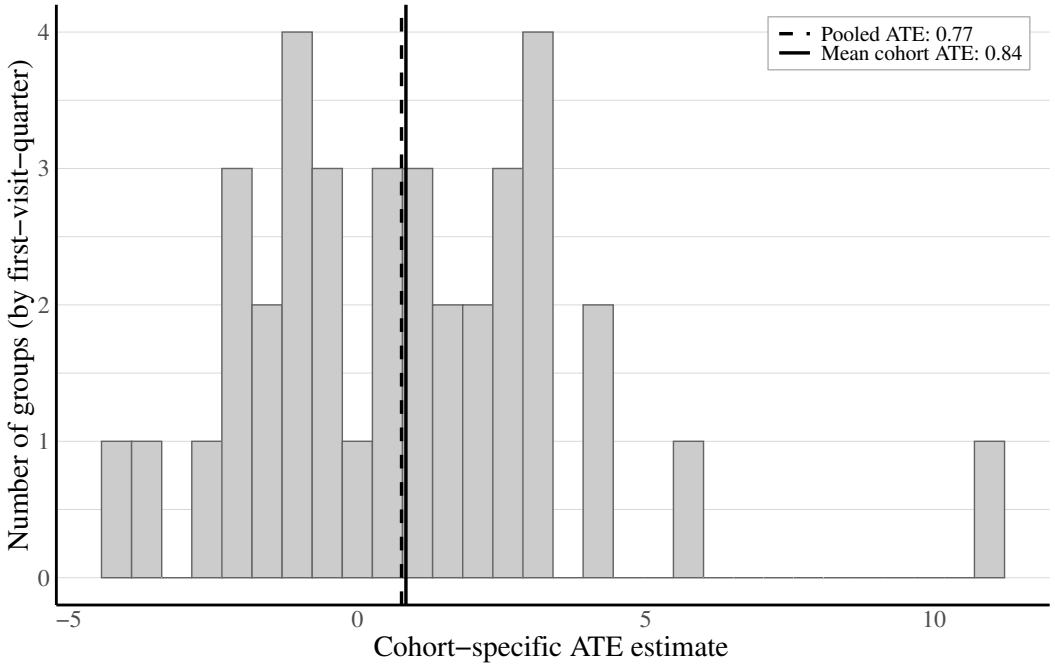
This table reports estimated coefficients from Equation 1 on our main physician-level sample. The grayed out period around relative quarters $r \in [0, 3]$ represents the "washout period;" $r = 0$ is the first quarter a physician experiences a detailing visit. Fixed effects are included for physicians and calendar quarter. All models include high-exposure physicians (ever-detailed physicians with 2–37 visits in the washout period), low-exposure physicians (ever-detailed physicians with 1 visit in the washout period), and never-detailed physicians. Omitted category is an indicator for relative quarter $r = -1$. All outcomes are defined at the physician-quarter level. Standard errors are clustered at the physician level; 95% confidence intervals are given by solid black lines.

Appendix Figure A27: Number of Purdue-Manufactured Opioid Prescriptions Relative to Detailing Exposure, Physician-Level, Using Prescribing NPI from the PDE Files (2014–2018)



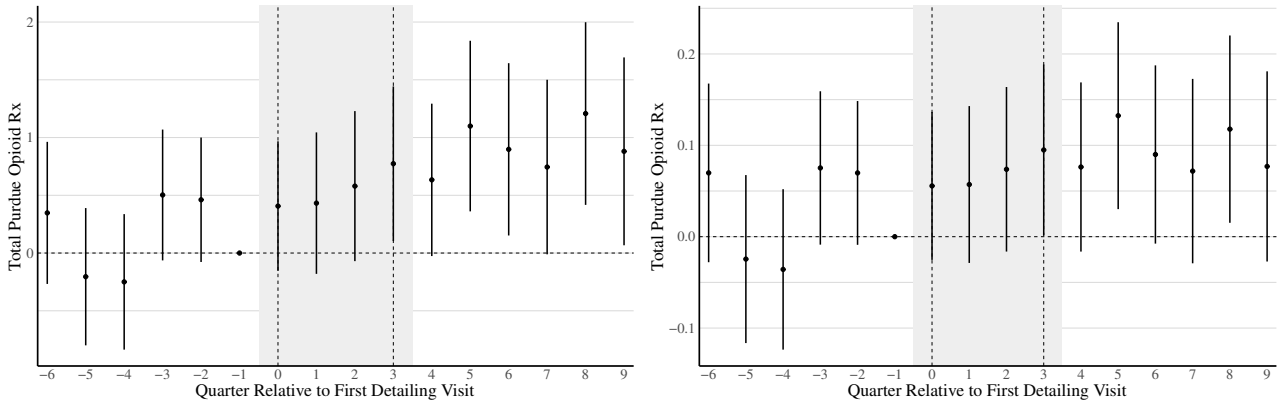
This figure reports estimated coefficients from a version of Equation 1 comparing ever- to never-detailed physicians who are listed as the prescribing provider in the Part D Event files from 2014–2018. The grayed out period around relative quarters $r \in [0, 3]$ represents the "washout period;" $r = 0$ is the first quarter a physician experiences a detailing visit. Fixed effects are included for physicians and calendar quarter. Omitted category is an indicator for relative quarter $r = -1$. All outcomes are defined at the physician-quarter level. Standard errors are clustered at the physician level; 95% confidence intervals are given by solid black lines.

Appendix Figure A28: Robustness Test for Staggered Treatment Timing



This figure assesses the sensitivity of our main difference-in-differences estimates to staggered treatment timing. We group ever-detailed physicians into cohorts based on the calendar quarter of their first detailing visit and estimate separate difference-in-differences models (using the pooled version of Equation 1) for each cohort. Each cohort-specific model includes high-exposure physicians (above-median detailing volume in the washout period), low-exposure physicians (ever-detailed physicians with one visit in the washout period), and all never-detailed physicians as controls. The histogram plots the distribution of cohort-specific average treatment effect (ATE) estimates for high-exposure physicians relative to low-exposure physicians. The dashed vertical line denotes the pooled ATE from our main specification, and the solid vertical line denotes the mean ATE across cohorts. All models use the same specification as the main analysis, including physician and calendar-quarter fixed effects, and exclude the washout period (relative quarters $r \in [0, 3]$). Standard errors are clustered at the physician level. Outcomes are defined at the physician-quarter level.

Appendix Figure A29: Number of Purdue-Manufactured Opioid Prescriptions, OLS v Poisson



(a) OLS (b) Poisson

This figure reports estimated coefficients from the OLS version (panel a) and Poisson version (panel b) of Equation 1. The grayed out period around relative quarters $r \in [0, 3]$ represents the "washout period;" $r = 0$ is the first quarter a physician experiences a detailing visit. Fixed effects are included for physicians and calendar quarter. Omitted category is an indicator for relative quarter $r = -1$. All outcomes are defined at the physician-quarter level. Standard errors are clustered at the physician level; 95% confidence intervals are given by solid black lines.

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.[31] 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 the National Bureau of Economic

[31] Available here: https://download.cms.gov/nppes/NPI_Files.html

Research. [32]. 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.[33]

C Overlap in Open Payments and Detailing Records

Background

The Physician Payments Sunshine Act (Grassley, 2009), enacted as part of the Affordable Care Act in 2010, requires that pharmaceutical and medical device manufacturers report to the Centers for Medicare & Medicaid Services (CMS) any payments and transfers over a certain monetary threshold[34] made to physicians and teaching hospitals. Responding to growing concerns around conflicts of interest in patient treatment and medical research, the law aimed to increase transparency around financial relationships between health care providers and industry.

[32] Available here: https://data.nber.org/npi/backfiles/

[33] See: https://www.nucc.org/index.php/code-sets-mainmenu-41/provider-taxonomy-mainmenu-40/csv-mainmenu-57.

[34] Manufacturers must report payments exceeding $10 individually or $100 in aggregate per calendar year, including meals, consulting fees, speaking fees, travel, and research payments.

Notably, the Sunshine Act only requires disclosure of financial transactions—payments and transfers of value—and not in-person interactions (such as detailing visits) that do not generate such transfers. This distinction raises an important empirical question: to what extent do financial transactions in Open Payments capture the full scope of pharmaceutical marketing interactions with physicians? While numerous studies have used Open Payments data as a proxy for pharmaceutical marketing exposure, we are not aware of prior work that has directly validated Open Payments records against internal pharmaceutical company detailing records.

Data and Methods

We compare internal detailing records from Purdue Pharma to Open Payments transaction data for Massachusetts physicians from 2015–2017. These years represent the overlap between the Purdue detailing data and the period when Open Payments included National Provider Identifiers (NPIs), enabling physician-level linkage.[35]

We restrict the Open Payments records to transactions from Purdue Pharma L.P. and to physicians practicing in Massachusetts. The Purdue detailing data contain visit-level records including the date, physician NPI, and sales representative identifier. We aggregate both datasets to the physician-year level and examine: (1) the overlap in physicians appearing in each dataset; (2) the relationship between detailing visit counts and total payment amounts; and (3) the relationship between detailing visit counts and the number of reported transactions.

Findings

Table A8 reports the overlap between Purdue-detailed physicians and those appearing in Open Payments with Purdue-attributed transactions. The overlap is notably asymmetric. Among physicians appearing in Open Payments with Purdue payments, 76–89% also appear in the Purdue detailing records—indicating that most physicians receiving payments from Purdue were also being actively detailed. However, the reverse is not true: only 32–38% of physicians in the Purdue detailing data have a corresponding Purdue transaction in Open Payments.

This asymmetry suggests that while financial payments typically accompany detailing relationships, the majority of detailing interactions do not result in a reportable financial transaction. In absolute terms,

[35]Open Payments data from 2013–2014 used Physician Profile IDs rather than NPIs, precluding linkage to external datasets.

196–376 physicians per year were detailed by Purdue but had no Purdue-attributed payment in Open Payments, while only 14–49 physicians per year received Purdue payments without appearing in the detailing data. These coverage rates may also reflect payments falling below the $10 reporting threshold, or detailing visits involving brief informational exchanges without meals or other transfers of value.

Table A9 compares the characteristics of Open Payments transactions for physicians appearing in both datasets versus those appearing only in Open Payments. Physicians in the "OP Only" group—those receiving Purdue payments but without documented detailing visits—have fewer transactions per physician (median of 1 vs. 3) and lower total payment amounts (median of $26 vs. $43). Notably, 79% of OP-only physicians appear in only a single year of data, compared to 57% of physicians in both datasets. These patterns are consistent with incidental contacts—such as attendance at a group meal or conference event—rather than sustained detailing relationships.

Appendix Table A8: Bidirectional Overlap: Purdue Detailing vs OpenPayments (Purdue Transactions), MA Physicians

Measure	2015	2016	2017
Purdue Detailing NPIs	606	407	308
OpenPayments NPIs	279	173	126
NPIs in Both	230	132	112
Purdue Only	376	275	196
OpenPayments Only	49	41	14
% Purdue in OP	38.0	32.4	36.4
% OP in Purdue	82.4	76.3	88.9

Appendix Table A9: Comparison of OpenPayments Transactions: Physicians in Both Datasets vs OpenPayments Only

Measure	In Both	OP Only
N Physicians	301	65
Mean Transactions/Physician	5.8	4.5
Median Transactions/Physician	3.0	1.0
Mean Total Amount/Physician ($)	299.55	326.10
Median Total Amount/Physician ($)	42.89	25.82
% Appearing in Single Year	56.8	78.5

Among physicians appearing in both datasets, Table A10 reports summary statistics and correlations. The correlation between detailing visits and Open Payments transaction counts ranges from $r = 0.6$ to $r = 0.8$, indicating a moderate-to-strong positive relationship—in other words, physicians with more detailing visits tend to have more reported transactions, though with substantial unexplained variation.

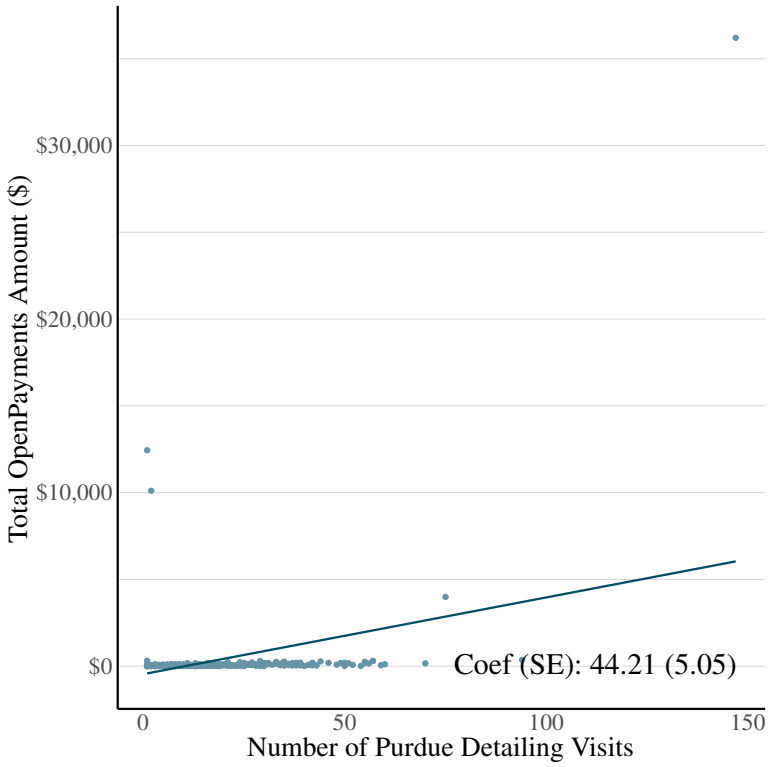
However, the correlation between detailing visits and total payment amounts is weaker and inconsistent, ranging from $r = 0.1$ to $r = 0.5$. The near-zero correlation in 2016 ($r = 0.1$) is particularly striking, suggesting that payment amounts capture a fundamentally different dimension of the physician-industry relationship than detailing intensity. In other words, while payment amount may be important in terms of better understanding potential conflicts of interest, it should not be conflated with sustained marketing interactions.

Appendix Table A10: Summary Statistics: Purdue Visits vs OpenPayments (Physicians in Both Datasets)

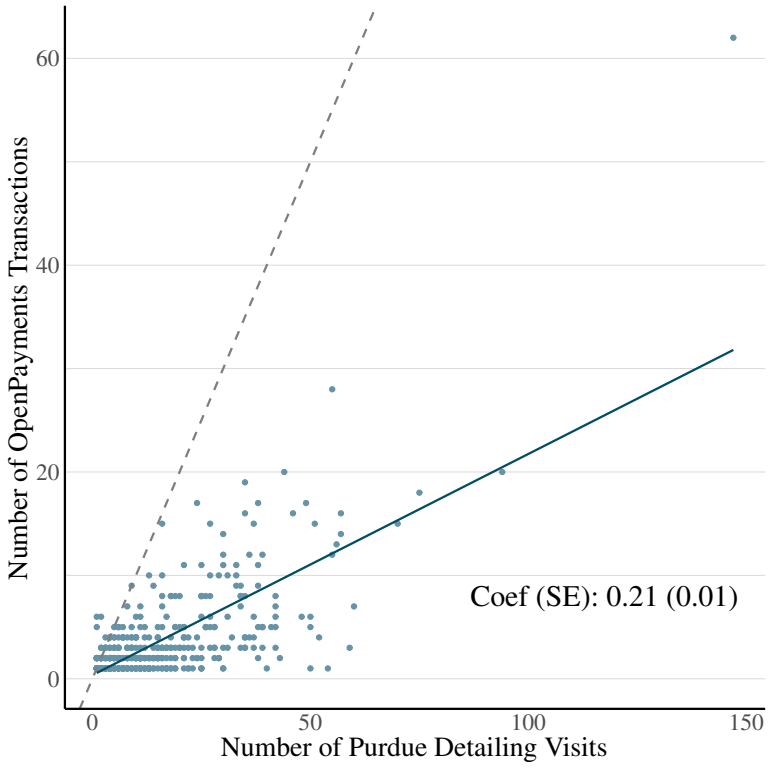
Measure	2015	2016	2017
N Physicians	230	132	112
Mean Visits	14.6	14.7	14.8
Mean Transactions	3.5	3.8	3.1
Mean Amount ($)	257.36	182.00	50.77
Corr (Visits-Trans)	0.8	0.7	0.6
Corr (Visits-Amt)	0.5	0.1	0.5

Figure A30 illustrates these relationships graphically. The visit–transaction relationship in panel (a) shows a positive slope, though with substantial dispersion around the regression line. The visit–amount relationship in panel (b) is essentially flat, confirming that payment amounts do not meaningfully proxy for detailing intensity. Regression coefficients are reported in each figure.

Appendix Figure A30: Relationship in Intensive Margins, Open Payment v Purdue Detailing Records, 2015–2017



(a) Financial Amount (OP) v Visit Volume (Purdue)



(b) Transactions (OP) v Visit Volume (Purdue)

Each point represents a physician-year observation for physicians appearing in both datasets, 2015–2017. Dashed line in panel (b) indicates 1:1 relationship; solid line in both panels is OLS fit of regressing y-axis variables on number of Purdue detailing visits.

D Opioid and OxyContin Use in Medicare, 2006 – 2018

This section provides additional background on prescribing/fill patterns and prescription drug coverage of opioids in Medicare during our study period, with particular attention to OxyContin.

Medicare Part D, the outpatient prescription drug benefit, was implemented on January 1, 2006 as part of the Medicare Prescription Drug, Improvement, and Modernization Act of 2003. Take-up of this benefit increased steadily in the years following implementation. Panel (a) of Appendix Figure A31 illustrates this growth, with the number of distinct beneficiaries enrolled in Part D increasing from approximately 3 million in the first quarter of 2006 to over 8 million by the fourth quarter of 2018.

Over the same period, the number of Part D beneficiaries receiving at least one opioid prescription also increased (panel b). Coverage of opioids by Part D formularies expanded between 2006 and 2015, although plan sponsors increasingly relied on utilization management tools—such as quantity limits and prior authorization—to restrict daily allowable prescribed dosing (Samuels et al., 2017).

By the mid-2010s, opioid use among Medicare beneficiaries had become a major focus of federal oversight. In 2016, when roughly one-third of Part D beneficiaries received an opioid prescription, the Office of Inspector General identified protecting Medicare beneficiaries from prescription drug abuse as a top enforcement priority. 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).

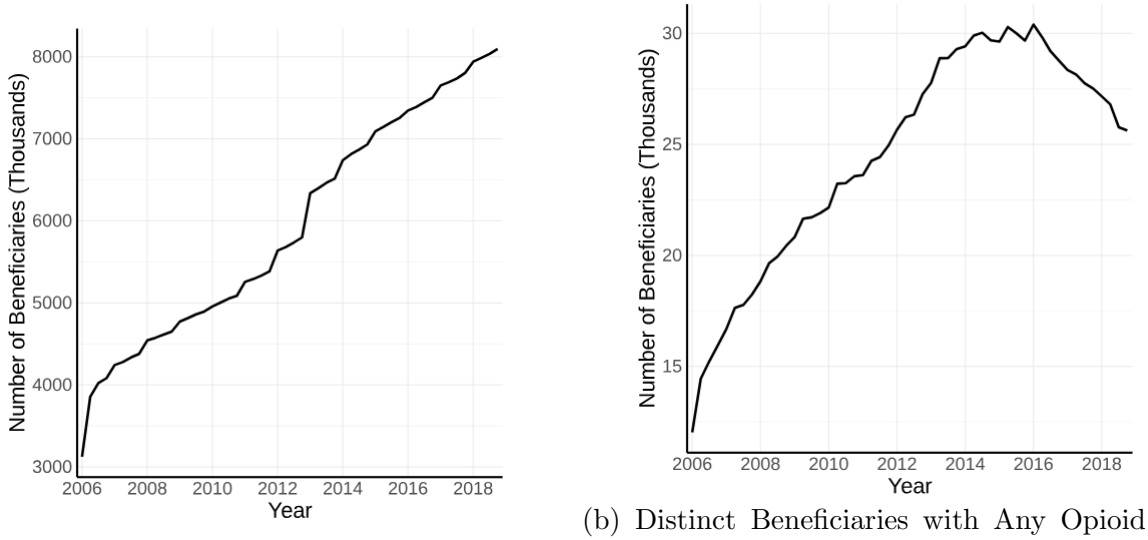
Appendix Figure A32 plots OxyContin use over time among Medicare beneficiaries. We identify OxyContin prescriptions in Part D records based on the recorded generic name of the dispensed drug.[36] Panel (a) shows that the number of OxyContin prescription fills increased sharply from 2006 through 2010, plateaued for several years, and began to decline after 2015. Panel (b) shows a similar pattern for total morphine milligram equivalents (MME), which rise rapidly through 2010 before declining through the end of the study period.

Appendix Figure A33 reports the share of opioid use attributable to OxyContin in our study sample. While OxyContin accounted for a relatively small share of opioid prescription fills—peaking at approx-

[36] This approach may undercount OxyContin use if generic names are inconsistently recorded.

imately 5% in 2008 and falling below 4% by 2018—it represented a substantially larger share of total opioid dosage. At its peak in 2010, aproximately 16% of all MME in our sample was attributable to OxyContin prescriptions. This pattern reflects the high potency of extended-release oxycodone formulations and is consistent with prior evidence showing that Purdue-manufactured opioids represented a modest share of pill volume but a much larger share of total opioid potency (Armstrong and Ernsthausen, 2019).

Appendix Figure A31: Beneficiaries in Part D by Quarter

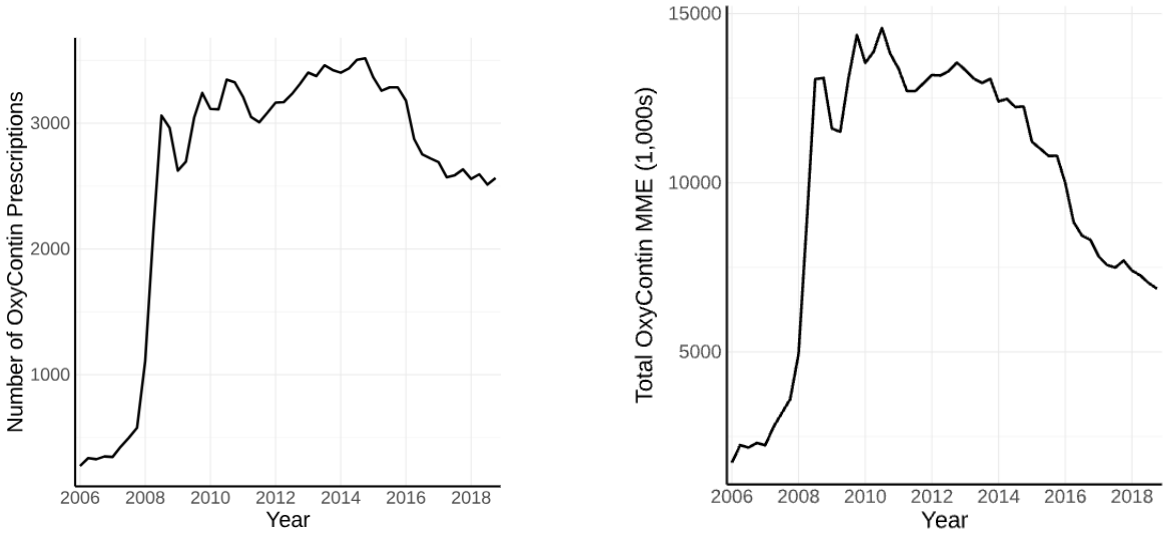


(a) Distinct Beneficiaries (20% National Sample)

(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.

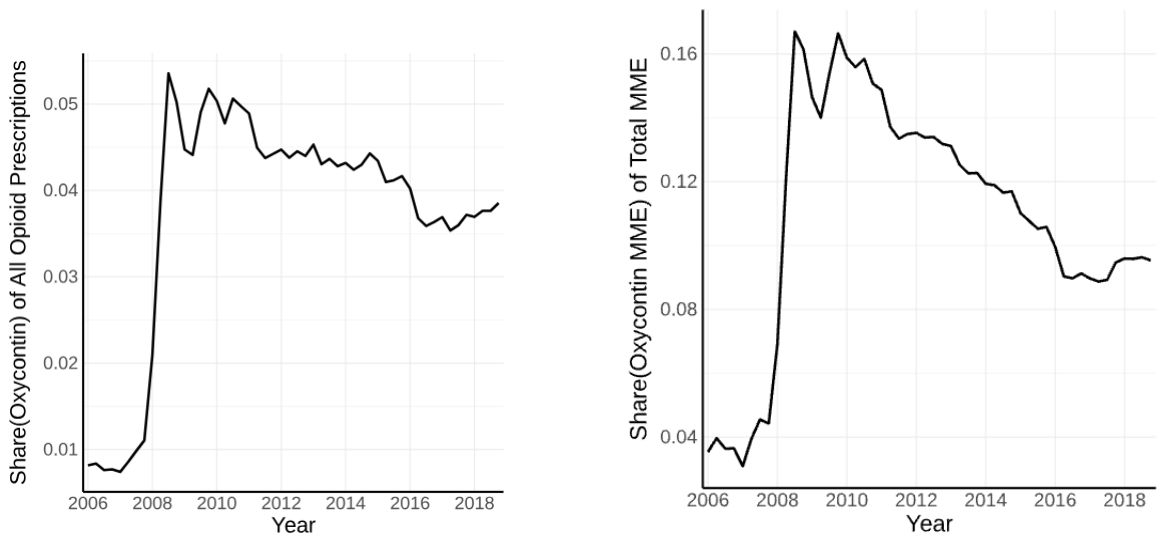
Appendix Figure A32: OxyContin Use Over Time (Study Sample)



(a) Total Prescription Fills (b) Total MME

This figure plots OxyContin prescription fills and total MME, aggregated to the year-quarter level, among beneficiaries in our study.

Appendix Figure A33: Share of Oxycontin Out of All Opioid Use Over Time (Study Sample)



(a) Total Rx (b) Total MME

This figure plots the share of all Part D opioid prescriptions associated with OxyContin, aggregated to the year-quarter level, among beneficiaries in our study.

E Sample Construction

E.1 Physician-Level Analytic Sample

Carrier claims and clinician identifiers. We use 20% Medicare Fee-for-service Carrier claims—administrativ claims submitted by healthcare professionals for services rendered to Medicare beneficiaries—to identify our physician sample, restricting to services delivered in Massachusetts from 2006–2018.[37] Prior to 2007, Carrier claims used a legacy Unique Physician Identification Number (UPIN) to identify the clinician who provided the service; midway through 2007, clinicians began to be identified using the National Provider Identifier that is used in many public and private databases today. We construct a UPIN–NPI crosswalk based on claims in which both identifiers are observed (`PRF_PHYSN_UPIN` and `PRF_PHYSN_NPI`) and use this crosswalk to "decode" UPINs when NPIs are missing or non-informative.

We merge clinician characteristics from NPPES and retain clinicians with MD/DO degrees ("physicians") and, where relevant for descriptive statistics, NP/PA degrees ("advanced practice providers"). For our main analyses that focus on physicians, we restrict to clinicians with an MD or DO degree.

Linkage to detailing records and treatment classification. We download Purdue detailing records, drop observations for which we cannot link to an NPI (as described in Appendix B), and collapse the detailing file to the NPI-quarter level, retaining each clinician's first detailing date and counts of detailing visits by quarter. Clinicians are classified as "ever detailed" if they appear in the Purdue records at least once during 2006–2018; never-detailed clinicians serve as potential controls.

To characterize clinicians' practice setting, we extract tax identification numbers (TINs) from Carrier claims and assign each clinician to a single TIN per quarter using the plurality of distinct service dates within that quarter (ties broken at random). We additionally define "detailed TINs" as TINs that ever include at least one ever-detailed clinician during the study period, and "never-detailed TINs" as TINs that never include any detailed clinician.[38]

Patient attribution and construction of the provider-quarter panel. Because the Part D Event (PDE) files we have access to do not contain the NPI of the provider who wrote the prescription for a given fill record prior to 2014, we uniformly attribute beneficiaries to clinicians using outpatient E&M

[37] Throughout, calendar quarters are defined from claim service dates.

[38] This TIN classification is descriptive and is used primarily to characterize practice environments and to support construction of beneficiary control cohorts in the beneficiary-level analysis.

visits observed in Carrier claims. Specifically, we identify E&M visits using CPT codes `99201--99205` (new patient office/outpatient visits) and `99211--99215` (established patient office/outpatient visits), and create a distinct list of `BENE_ID`–`NPI` links by calendar quarter based on qualifying E&M encounters. This attribution is non-exclusive: a beneficiary may be linked to multiple clinicians within a quarter if they have qualifying visits with multiple clinicians during that period.

We then merge beneficiary-quarter opioid prescription outcomes (constructed from Part D Event files; Appendix F) back onto the attributed `BENE_ID`–`NPI`–quarter links and aggregate to a clinician-quarter panel. The resulting provider-level analytic dataset is at the `NPI` $\times$ quarter level. The physician-quarter panel data includes all quarters from a physician's first quarter appearing in the Carrier files to their last; as such, the physician panels will include quarters in which physicians do not treat any Medicare beneficiaries.

Final physician-level panel preparation. We drop treated physicians whose first detailing quarter occurs in 2007Q1–2007Q2. We retain treated physicians who are observed for the full analysis window $t' \in [-6, 9]$ and generate relative-quarter dummy variables for treated physicians across this window. We impose no minimum quarter presence on control physicians.

Matching never-detailed physicians to ever-detailed physicians. To construct a comparison group with similar baseline characteristics to ever-detailed physicians, we match each ever-detailed physician to up to ten never-detailed physicians using nearest-neighbor propensity score matching without replacement. We match exactly on specialty, sex, degree type (MD vs. DO), and 5-digit zip code, and estimate propensity scores from average number of attributed patients per quarter, average MME, and average number of prescriptions among attributed patients in the pre-period. This matching procedure reduces the never-detailed sample from 19,761 to 5,362 physicians. Table A3 compares characteristics of matched and unmatched never-detailed physicians.

E.2 Beneficiary-Level Analytic Sample

The beneficiary-level analysis evaluates outcomes for beneficiaries exposed to varying levels of detailing through their relationship with a key provider. We construct treated and control beneficiary cohorts from Carrier claims and then build a beneficiary-quarter panel that combines utilization outcomes and prescription outcomes.

Beneficiary eligibility and baseline characteristics from MBSF. We construct beneficiary-level enrollment and demographic measures using the Medicare Denominator files (2006–2013) and the Master Beneficiary Summary File (MBSF) base segment (2014–2018), supplemented by the MBSF Part D segment for early years. We harmonize beneficiary sex and race across records using a "majority rules" approach (modal non-missing value) with ties resolved by the most recent non-missing value.

We define monthly eligibility for inclusion as enrollment in Parts A, B, and D and not enrolled in Medicare Advantage (Part C). Specifically, we flag Parts A/B enrollment using entitlement/buy-in indicators prior to 2014 and `MDCR_STATUS_CODE_XX` beginning in 2014; we flag Part D enrollment based on contract identifiers `PTD_CNTRCT_ID_XX`; and we flag Medicare Advantage enrollment using `BENE_HMO_IND_XX` (pre-2014) or `HMO_IND_XX` (2014+). We additionally flag SSDI eligibility (using entitlement codes consistent with Finkelstein et al., 2022) and dual eligibility (using monthly dual/buy-in measures appropriate to each period). We use these month-level indicators to define episodes of continuous A/B/D enrollment without Part C and subsequently map episodes to calendar quarters for panel construction.

We flag whether a beneficiary had a chronic pain diagnosis by following the Chroinc Condition Warehouse guidelines.

Treated beneficiaries: exposure through ever-detailed providers. We define treated beneficiaries as those who are observed receiving care from an ever-detailed clinician prior to that clinician receiving their first detailing visit. Using Carrier claims linked to the clinician-level detailing file, we identify beneficiaries who have at least one Carrier claim with an ever-detailed clinician in the four quarters preceding that clinician's first detailing quarter (i.e., $t' \in [-4, -1]$ relative to the clinician's first detailing quarter). We allow beneficiaries to be associated with multiple ever-detailed clinicians over time; we therefore define "episodes" of exposure, indexed by the first detailing quarter of the associated clinician. A beneficiary can contribute multiple episodes if they are connected to multiple clinicians whose first detailing quarters differ. In practice, we keep each beneficiary's first treatment episode to avoid contamination from the inclusion of already-treated units.

Control beneficiaries: never- and not-yet-treated cohorts. We construct a comparison cohort intended to be plausibly unexposed to detailing at the time their outcomes are measured. We flag TINs that ever contain a detailed clinician ("detailed TINs") and then identify beneficiaries whose observed clinicians are affiliated only with never-detailed TINs. We further define a "not-yet-treated" set of

beneficiaries whose clinicians practice in TINs that will be detailed in the future but are at least one year away from the TIN's first exposure to detailing; for these beneficiaries we define a beneficiary-specific cutoff quarter equal to one year prior to the TIN's first detailing quarter, after which the beneficiary is no longer considered untreated. The final control file contains `BENE_ID` and a beneficiary-specific cutoff quarter (`bene_cutoff_quarter`) that governs the eligible observation window in beneficiary-quarter panels.

Enrollment duration and sampling. To be included in the analytic sample, beneficiaries must have a continuous enrollment episode (Parts A, B, and D without Part C) spanning at least 15 quarters, corresponding to the full event study window. Given the large number of eligible never-treated beneficiaries relative to ever-treated beneficiaries, we draw a random sample of never-treated beneficiaries targeting a 4:1 ratio of never-treated to ever-treated observations.

Matching never-treated to ever-treated beneficiaries. We construct a matched comparison sample using nearest-neighbor propensity score matching with a 3:1 ratio (three never-treated to each ever-treated beneficiary), matching without replacement. We match exactly on sex and race, and estimate propensity scores from age, SSDI receipt, presence of a chronic pain diagnosis, and any Purdue opioid prescription in the pre-period. The final matched sample includes 48,081 never-treated beneficiaries.

E.3 Key Provider Assignment

For beneficiary-level analyses, we assign each treated beneficiary-episode to a single "key provider" intended to capture the clinician most likely to influence prescribing behavior and downstream outcomes for that beneficiary in the relevant pre-treatment period.

Definition. For each treated beneficiary-episode, the key provider is defined as the clinician who accounts for the plurality of the beneficiary's outpatient E&M visits in the four quarters preceding the episode's first detailing quarter (i.e., $t' \in [-4, -1]$ relative to the episode's first detailing quarter). E&M visits are identified using CPT codes `99201--99205` and `99211--99215`. We count unique E&M visit dates to avoid overweighting same-day multiple line items.

Implementation. Within each beneficiary-episode, we compute the share of qualifying E&M visits attributable to each clinician during the pre-period and assign the clinician with the highest share as

the key provider (ties broken deterministically by lowest NPI value). We retain summary measures of key-provider strength—including the number of E&M visits with the key provider and the key provider's share of all E&M visits in the pre-period—which are used for descriptive validation and, in robustness checks, to restrict to beneficiaries with a strong pre-period relationship to the assigned provider.

We further flag whether the key provider is ever detailed and retain the key provider's first detailing quarter, which serves as the index date for the beneficiary-episode event-time construction in beneficiary-level regressions.

F Outcome Construction

This section summarizes construction of prescription and utilization outcomes for physician- and beneficiary-level analyses. Unless otherwise noted, outcomes are constructed at the calendar-quarter level.

F.1 Prescription Outcomes from Part D Event (PDE) Files

Identifying Purdue-associated opioid prescriptions. We pull PDE records for beneficiaries appearing in the patient–provider attribution step and retain distinct fill events at the `PDE_ID` level. We identify opioid prescriptions associated with molecules marketed by Purdue Pharma during our study period. Table A11 lists Purdue's opioid product portfolio; Table A12 reports the generic name stems we use to flag prescriptions for these molecules in the PDE data.

Because brand identifiers in PDE data do not consistently capture whether a dispensed opioid is a branded product—brand/generic indicators are not available until 2012, and brand names are not necessarily recorded in a way that reliably identifies brand dispensing—we construct our primary measure using generic drug names (GNNs). Specifically, our main outcome, `total_purdue_gnn_quarter`, counts the number of fills in a beneficiary–quarter whose recorded GNN matches a Purdue-associated molecule (Table A12). We aggregate these beneficiary–quarter counts to the physician–quarter level by summing across beneficiaries attributed to each physician in the corresponding quarter.

This generic-name approach captures prescriptions for Purdue-associated molecules regardless of whether the dispensed product was a Purdue-branded formulation or a generic equivalent. This is appropriate in Massachusetts, a mandatory generic substitution state in which generic drugs must be substituted for branded products when available unless "dispense as written" is specified. As a result, a prescription written for a branded product (e.g., OxyContin) may be dispensed as a generic formulation of the same

molecule (e.g., generic oxycodone ER). We therefore interpret our measure as capturing prescriptions for molecules that Purdue marketed and promoted, rather than Purdue-manufactured products specifically.

Table A13 reports the 15 most common prescriptions flagged using this approach. Several recorded brand names correspond to products manufactured by companies other than Purdue: Roxicet (Roxane Laboratories), Endocet (Mallinckrodt), Kadian (Alpharma), and Percocet (Endo Pharmaceuticals). However, these products account for only approximately 2.0% of flagged prescriptions. Furthermore, the "brand name" variable in PDE files is not original to the submitted claim but is populated by linking to an external data source (First DataBank MedKnowledge) via the National Drug Code (NDC); if the NDC is incorrect, missing, or ambiguous, the brand name may not reflect the actual product dispensed (Chronic Condition Warehouse, 2024).

We exclude buprenorphine from our analysis given its use in opioid use disorder treatment. Although Purdue marketed Butrans (a buprenorphine patch) during our study period, buprenorphine prescriptions are not relevant to the research question of interest.

MME calculation. We merge PDE records to CDC opioid conversion factors and calculate morphine milligram equivalents (MME) for each prescription based on strength and days supplied.

Aggregation to beneficiary-quarter and physician-quarter outcomes. We collapse prescription outcomes to the beneficiary-quarter level, producing measures including total Purdue-associated opioid prescriptions, total MME, indicators for any opioid use, counts of high-dose prescriptions, and the overlapping-prescription indicator. For the physician-level analysis, we merge these beneficiary-quarter measures onto the `BENE_ID`–`NPI` attribution links and aggregate to clinician-quarter totals (e.g., total opioid prescriptions among attributed patients).

Our primary physician-level outcomes are defined in levels (e.g., total opioid prescriptions) rather than per-patient rates. This choice avoids ambiguity introduced by quarters in which a clinician has no attributed patients: per-patient measures would conflate a true zero (no opioid prescribing among attributed patients) with the absence of attributed patients in that quarter.

Appendix Table A11: Purdue Pharma Opioid Products

Brand Name	Generic Name	FDA Approval	Source
OxyContin	oxycodone HCl ER	December 1995	FDA NDA 20-553
MS Contin	morphine sulfate ER	1984	GAO-04-110
Dilaudid	hydromorphone HCl	1926[a]	FDA NDA 19-892
Hysingla ER	hydrocodone bitartrate ER	November 2014	FDA NDA 206627
Ryzolt	tramadol HCl ER	2009[b]	FDA Orange Book
Butrans[c]	buprenorphine (patch)	2010	U.S. Department of Justice (2020)

Notes: This table lists opioid products manufactured or marketed by Purdue Pharma L.P. during the study period (2007–2017).

[a] Dilaudid was originally developed by Knoll Pharmaceuticals; rights were transferred to Purdue Pharma in 2007.

[b] Ryzolt was discontinued in 2012.

[c] Buprenorphine products are excluded from our analysis given their use in opioid use disorder treatment.

Appendix Table A12: Generic Names Used to Identify Purdue-Manufactured Opioids

Purdue Brand	Generic Name Stems Used for Matching
OxyContin	oxycodone hcl, oxycodone hydrochloride
MS Contin	morphine sulfate
Dilaudid	hydromorphone hcl, hydromorphone hydrochloride
Hysingla ER	hydrocodone bitartrate
Ryzolt	tramadol hcl, tramadol hydrochloride

Notes: We flag prescriptions in Part D Event files as Purdue-associated if the recorded generic name (GNN) matches one of the stems listed above. This approach captures prescriptions for Purdue-manufactured molecules regardless of whether the dispensed product was branded or generic. See text for discussion of Massachusetts mandatory generic substitution laws.

Appendix Table A13: Top 15 Most Common Prescriptions Flagged as Purdue-Manufactured

Rank	Brand Name	Generic Name	%
1	OXYCODONE HCL	OXYCODONE HCL	17.7%
2	OXYCODONE-ACETAMINOPHEN	OXYCODONE HCL/ACETAMINOPHEN	17.3%
3	TRAMADOL HCL	TRAMADOL HCL	15.8%
4	OXYCONTIN	OXYCODONE HCL	3.9%
5	MORPHINE SULFATE ER	MORPHINE SULFATE	3.2%
6	MORPHINE SULFATE	MORPHINE SULFATE	2.9%
7	HYDROMORPHONE HCL	HYDROMORPHONE HCL	2.5%
8	OXYCODONE HCL-ACETAMINOPHEN	OXYCODONE HCL/ACETAMINOPHEN	1.8%
9	ROXICET	OXYCODONE HCL/ACETAMINOPHEN	0.9%
10	ENDOCET	OXYCODONE HCL/ACETAMINOPHEN	0.9%
11	TRAMADOL HCL-ACETAMINOPHEN	TRAMADOL HCL/ACETAMINOPHEN	0.5%
12	KADIAN	MORPHINE SULFATE	0.1%
13	PERCOCET	OXYCODONE HCL/ACETAMINOPHEN	0.1%
14	OXYCODONE HCL ER	OXYCODONE HCL	0.1%
15	TRAMADOL HCL ER	TRAMADOL HCL	0.1%

F.2 Utilization Outcomes from Outpatient and Inpatient Claims

Opioid-related adverse events and related clinical outcomes. We flag opioid overdose, injection drug use (IDU)-related infections, and falls and/or fractures informed by diagnosis-code definitions from Barnett et al. (2023). For all outcomes, we require a diagnosis in an inpatient setting or an opioid-poisoning diagnosis in an outpatient setting accompanied by an ED revenue code. Tables A14–A16 report the specific ICD-9-CM and ICD-10-CM diagnoses used to construct these outcomes.

Appendix Table A14: Non-Fatal Opioid Overdose

ICD-9	Poisoning by opiates and related narcotics: 965.00, 965.01, 965.02, 965.09; 909.0 External cause codes (unintentional, self-harm, assault, undetermined): E850.0, E850.1, E850.2, E929.2, E950.0, E959, E962.0, E969, E980.0, E989 Aftercare/encounter-related code: V58.89
ICD-10	Opioid poisoning codes with intent and encounter type: T40.0X1A, T40.0X1D, T40.0X1S, T40.0X2A, T40.0X2D, T40.0X2S, T40.0X3A, T40.0X3D, T40.0X3S, T40.0X4A, T40.0X4D, T40.0X4S; T40.1X1A–T40.1X4S; T40.2X1A–T40.2X4S; T40.3X1A–T40.3X4S; T40.4X1A–T40.4X4S
PLUS Emergency Department visit revenue code for outpatient setting	
Revenue Center Code	0450, 0451, 0452, 0456, 0459, 0981

Note: A beneficiary is classified as experiencing a non-fatal opioid overdose if they have an opioid poisoning diagnosis in an inpatient claim or an opioid poisoning diagnosis in an outpatient claim accompanied by an emergency department revenue center code.

Appendix Table A15: Injection Drug Use–Related Infections

Phlebitis / Thrombophlebitis	
ICD-09	451*
ICD-10	I80.1*–I80.9*
Abscess or Cellulitis	
ICD-09	680*, 681*, 682*
ICD-10	G06.1, G06.2, L02*, L03*, L98.3
Hepatitis C	
ICD-10	B17.10, B17.11, B18.2, Z22.52
Infectious Arthritis	
ICD-10	M00*
Infectious Endocarditis	
ICD-10	I33.0, I33.9, I38, I39
PLUS Emergency Department visit revenue code for outpatient setting	
Revenue Center Code	0450, 0451, 0452, 0456, 0459, 0981

Note: A beneficiary is classified as experiencing an injection drug use–related infection if they have one of the above diagnoses recorded in an inpatient claim or in an outpatient claim accompanied by an emergency department revenue center code. In the original definition, the patient additionally needed to have a diagnosis for Opioid Use Disorder (ICD10: F11.1*) on service date or 30 days prior to be included in cohort for inpatient detox or rehabilitation; as we are not examining this set of outcomes, we do not impose this restriction. The original table listed B17.1; this was updated to B17.10 and B17.11 for completeness. Two misprints were identified in the original table: L03.390 and L03.391 should be L03.90 and LO3.91 (corrected above).

Appendix Table A16: Falls and Fractures

Falls	
ICD-09	E880*, E882*, E884*, E885*
ICD-10	W00*–W19*
Fractures	
ICD-09	808*, 812*, 813*, 820*, 733.11, 733.12, 733.14
ICD-10	S02*, S12*, S22*, S32*, S42*, S52*, S62*, S72*, S82*, S92*, M80*

Note: Falls and fractures are identified using diagnosis codes recorded on inpatient claims or on outpatient claims that include an emergency department (ED) revenue center code.

Negative control prescriptions. As a falsification test, we construct negative control prescription outcomes from PDE files following the logic of negative control outcomes in observational designs (**?**). We identify prescriptions in drug classes unlikely to be affected by opioid detailing (e.g., antihypertensives, statins, and selected chronic disease medications) using generic name lists (Appendix Table A17) and aggregate these measures to the beneficiary-quarter and clinician-quarter levels analogously to opioid prescriptions.

Appendix Table A17: Negative Control Outcomes (Placebo Rx)

Drug class	Generic Name
Antihypertensives	AMLODIPINE, BENAZEPRIL, CANDESARTAN, CAPTOPRIL, CHLORTHALIDONE, CLONIDINE, DILTIAZEM, ENALAPRIL, EPLERENONE, FELODIPINE, FOSINOPRIL, HYDRALAZINE, HYDROCHLOROTHIAZIDE, IRBESARTAN, LISINOPRIL, LOSARTAN, METOLAZONE, NIFEDIPINE, OLMESARTAN, PROPRANOLOL, QUINAPRIL, RAMIPRIL, SPIRONOLACTONE, TELMISARTAN, VALSARTAN
β-blockers	ACEBUTOLOL, ATENOLOL, BISOPROLOL, CARVEDILOL, LABETALOL, METOPROLOL, NADOLOL, NEBIVOLOL, PROPRANOLOL, SOTALOL
Diabetes agents (non-insulin)	ACARBOSE, ALOGLIPTIN, CANAGLIFLOZIN, DAPAGLIFLOZIN, DULAGLUTIDE, EMPAGLIFLOZIN, EXENATIDE, GLIMEPIRIDE, GLIPIZIDE, GLYBURIDE, LINAGLIPTIN, LIRAGLUTIDE, METFORMIN, PIOGLITAZONE, REPAGLINIDE, ROSIGLITAZONE, SAXAGLIPTIN, SITAGLIPTIN
Glaucoma medications	BIMATOPROST, BRIMONIDINE, BRINZOLAMIDE, DORZOLAMIDE, LATANOPROST, TIMOLOL, TRAVOPROST
Proton pump inhibitors (PPIs)	DEXLANSOPRAZOLE, ESOMEPRAZOLE, LANSOPRAZOLE, OMEPRAZOLE, PANTOPRAZOLE, RABEPRAZOLE
Statins	ATORVASTATIN, FLUVASTATIN, LOVASTATIN, PITAVASTATIN, PRAVASTATIN, ROSUVASTATIN, SIMVASTATIN

F.3 Final Beneficiary-Level Panel

For beneficiary-level analyses, we merge (i) utilization outcomes from OP and MedPAR, (ii) opioid and placebo prescription outcomes from PDE, and (iii) time-varying enrollment eligibility from MBSF-derived episodes into a beneficiary-quarter panel. For treated beneficiaries, we assign an episode-specific index quarter equal to the key provider's first detailing quarter and retain all observed beneficiary–quarter observations before and after this index quarter. Control beneficiaries are observed only through their beneficiary-specific cutoff quarter to maintain a comparison group that is plausibly unexposed to detailing at the time outcomes are measured. We include quarters within enrollment episodes even when no utilization or prescription event is observed, setting outcomes to zero in those

quarters.

We drop treated beneficiaries whose associated first detailing quarter occurs in 2007Q1–2007Q2. We then merge in key-provider characteristics (degree indicator, specialty, visit volume measures) from the physician-level prepared file and restrict treated beneficiary episodes to those whose assigned key provider is in the final in-sample provider file (dropping treated observations with missing merged key-provider characteristics). Finally, we impose event-time balance requirements: treated beneficiaries are retained only if they are observed for the full event-time window $t' \in [-6, 9]$, and controls are retained only if they are observed for at least 16 quarters. We generate event-time dummy variables for each relative quarter for treated beneficiaries and trim treated observations to this window in the final analysis file. Table A18 reports distinct beneficiary counts by cleaning step.

Appendix Table A18: Distinct beneficiaries by restriction step

Restriction step	Control (N)	Treated (N)
Initial counts	156884	39221
After 2007H1 drop	156884	30280
After in-sample KP restriction	156884	24741
Benes with [-6,9] quarters	156884	16027
After control panel length restriction	100300	16027
Final count	100300	16027

G Reverse Causality and Bounding Exercise

In our empirical setting, it is possible that a positive feedback loop exists between physicians and detailers such that the more the physician prescribes, the more frequently a detailer returns to visit them. The existence of such a feedback loop complicates the interpretation of our estimated effects. Namely, our baseline interpretation implicitly assumes a unidirectional pathway (detailing $\rightarrow$ prescribing). However, our estimated effects may also reflect the reverse causal pathway (prescribing $\rightarrow$ detailing), which then feeds back into prescribing.

Notably, because the feedback loop does not exist prior to treatment, we do not view it as mechanically biasing our estimates away from a policy-relevant object. Instead, one can interpret our estimates as capturing the *total effect of detailing exposure* in a real-world ecosystem in which prescribing and detailing mutually reinforce one another.

Nevertheless, the prescribing $\rightarrow$ detailing pathway potentially violates key assumptions underlying the

difference-in-differences research design. To make this explicit, we decompose the observed event-study coefficient as $\hat{\beta}$:

$$\hat{\beta} = \underbrace{\tau}_{\substack{\text{causal effect of high} \\ \text{exposure to detailing}}} + \underbrace{\eta}_{\substack{\text{endogenous feedback between} \\ \text{detailing volume and prescribing}}} + \underbrace{\delta}_{\substack{\text{other sources of} \\ \text{bias unrelated to feedback}}}$$

Here, τ is the structural causal effect of a high volume of detailing visits on opioid prescribing; η captures endogenous feedback effects that arise because detailing intensity responds to physicians' prescribing behavior (and are therefore absent in the pre-period); and δ represents other sources of bias unrelated to feedback, such as differential pre-trends or spillovers across physicians. Consistent with the flat pre-period coefficients in our main event-study estimates, we view δ as likely to be limited in magnitude. Accordingly, we focus on the combined object $\tau+\eta$, recognizing that remaining non-feedback bias cannot be fully ruled out.

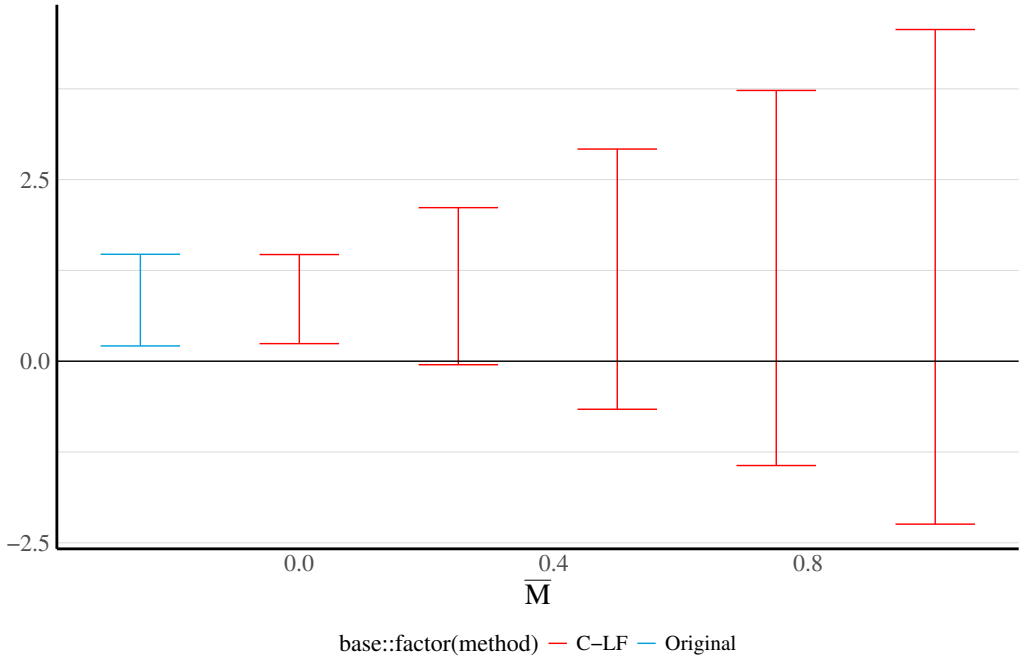
However, even if we view η as part of the policy-relevant "treatment ecosystem," the presence of this feedback implies that detailing intensity evolves endogenously as a function of realized outcomes. This creates an endogeneity concern: if treatment intensity responds to outcomes, we cannot cleanly separate the causal effect of detailing from feedback-driven selection. For this reason, we take two complementary steps. First, we introduce a "washout period" that is used solely to characterize physicians' exposure to detailing, and we separate this from the post-period in which we estimate effect sizes. Physicians may continue to receive detailing visits in the post-period, but this information is not used to redefine exposure. While detailing intensity is persistent—physicians with high exposure in the washout period tend to remain highly exposed thereafter—this design choice seeks to isolate the most salient reverse-causality concerns from the period in which we interpret coefficients as our main effects.

Second, we apply the sensitivity analysis framework of Rambachan and Roth (2023) to assess whether post-period estimates can be distinguished from a continuation of washout-period forces. To be precise about terminology: by "bias," we refer to the component of the estimated effect attributable to forces other than the structural causal effect τ—that is, endogenous feedback η and other confounders δ. Importantly, unless causal effects in the washout period are negligible, any deviations observed during this period cannot be interpreted as pure bias. Accordingly, our bounding exercise assesses whether post-period estimates can be explained by persistence or evolution in the same forces—whether causal, feedback-driven, or both—that generate washout-period deviations.

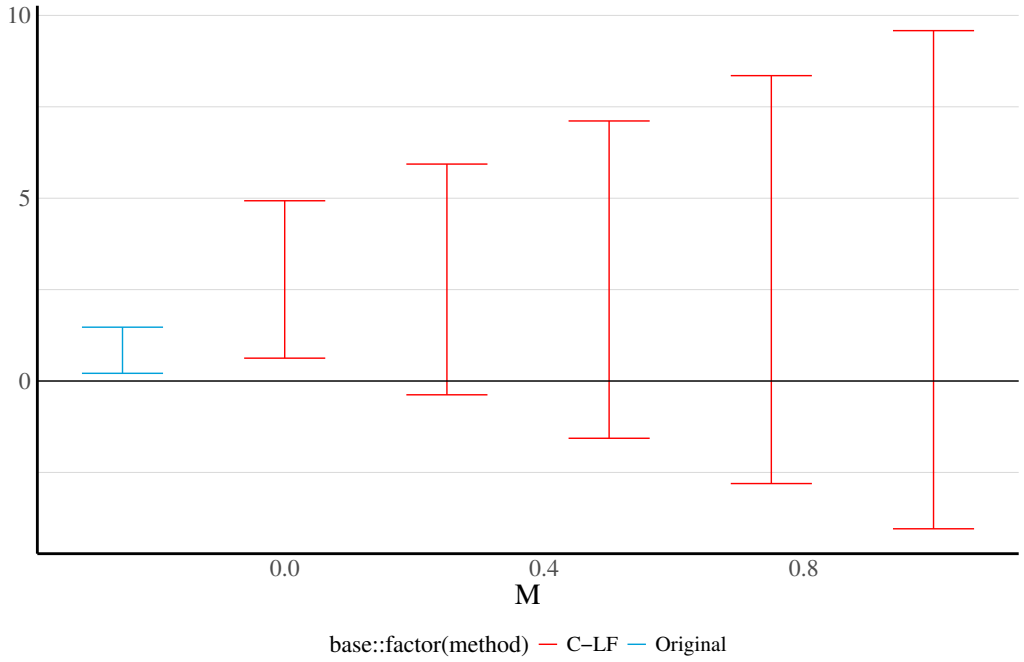
Figure A34 presents the results of this exercise. In both panels, blue intervals correspond to conventional event-study confidence intervals. In panel (a), red intervals are constructed under a relative-magnitude restriction, which limits the size of the post-period non-structural component of the event-study coefficients to be at most $\bar{M}$ times as large as the largest such component observed during the washout period. In panel (b), red intervals are constructed under a smooth-deviation restriction, in which M bounds how quickly the non-structural component can evolve over time.

Under the assumption of no post-treatment deviation from parallel trends ($\bar{M} = 0$), the average post-period effect remains statistically significant (95% CI: [0.31, 1.52]). However, confidence intervals widen rapidly even under tight restrictions—allowing post-treatment deviations of just 25% of the largest washout-period deviation ($\bar{M} = 0.25$) yields confidence intervals that include zero. This indicates that the data cannot separately identify a persistent post-period treatment effect from a continuation or evolution of washout-period dynamics. While this does not imply that the causal effect of detailing exposure is zero, it does imply that our post-period estimates cannot be cleanly separated from washout-period dynamics under relatively weak and empirically plausible assumptions. We therefore interpret our post-period estimates with appropriate caution and view this limitation as inherent to settings in which treatment intensity responds endogenously to outcomes.

Appendix Figure A34: Bounding Exercise Based on Rambachan and Roth (2023)



(a) Δ^{RM}



(b) Δ^{SD}

Blue intervals are original 95% confident intervals; red intervals incorporate the sensitivity restriction. Δ^{RM} restricts the magnitude of post-period deviations relative to the largest washout-period deviation. Δ^{SD} restricts how quickly deviations can change across periods. $\bar{M}$ (panel a) and M (panel b) parameterize the degree of relaxation from exact parallel trends. For Δ^{RM}, $\bar{M} = 1$ allows post-period deviations up to the magnitude of the largest washout-period deviation. For Δ^{SD}, $M = 0$ requires trends to continue linearly; $M > 0$ allows the slope to change across periods.