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Abstract

We estimate price elasticities for prescription opioid purchases within the general population, and explore underlying mechanisms. By leveraging random assignment of individuals to health insurance plans from the RAND Health Insurance Experiment, we find an elasticity of -0.19 at the extensive margin, and elasticities ranging from -0.24 to -0.33 at the intensive margin. Responses to price changes result from both additional physician visits and higher opioid prescription rates per visit as plan generosity increases. We find no evidence linking responses to the share of unfilled prescriptions. We illustrate how our elasticity estimates inform the recent national opioid tax policy debate.

JEL No. H22, H51, I13, I18, L11.

Keywords: Opioid prescriptions, Price elasticity, Mechanisms, Health insurance, Pain, Opioid tax.

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1 Introduction

Deaths involving opioids have been the dominant driver of the famous "deaths of despair" conceptualization (Case and Deaton, 2020). Several policies have been considered to address the current opioid epidemic, either focusing on demand- or supply-side factors (Maclean et al., 2020). The predominant emphasis among supply-side proposals has been on regulating the volume of opioids.[1] Economic theory predicts that, despite their addictive nature, the demand for opioids is likely to respond to price changes (Stigler and Becker, 1977; Becker and Murphy, 1988). Following this rationale, price-based solutions have recently been on the private and public agenda. Some examples include the re-design of prescription drug plan formularies and opioid taxes, which have already been implemented in five US states and are currently under debate at the federal level (Harris, Mandell, and Gross, 2022).[2] A key input to the design, targeting, and eventual success of such price-driven policies is the responsiveness of patients to prescription opioid prices. Nonetheless, estimates of this elasticity for a general population are not available.

In this paper, we fill this gap through a retrospective analysis, estimating price elasticities for prescription opioid purchases and exploring underlying mechanisms. Our estimates are then used to quantify the implications of introducing the proposed national opioid tax. We examine measures both at the extensive and intensive margins. The extensive margin captures how the probability of filling an opioid prescription changes as the price varies, though it does not account for the fact that some consumers may still fill the prescription when prices go up, but reduce the quantity of the

[1]Policies to reduce opioid prescribing include the introduction of programs to monitor prescriptions (e.g., Meara et al. (2016); Buchmueller and Carey (2018); Meinhofer (2018); Kim (2021); Horwitz et al. (2021); Nguyen, Meille, and Buchmueller (2023); Alpert, Dykstra, and Jacobson (2024)), the introduction of abuse-deterrent opioids (e.g., Alpert, Powell, and Pacula (2018)), changes to national prescribing guidelines (e.g., Sacks et al. (2021); Stein et al. (2022); Allen, Bradford, and Durrance (2024)), increased enforcement of the Controlled Substances Act of 1970 to curtail inappropriate supply (e.g., Kennedy-Hendricks et al. (2016); Meinhofer (2016); Donahoe (2024); Soliman (2024)), and improving access to substance abuse treatment (e.g., Swensen (2015)).

[2]Appendix OA.1 reviews recent supply-side policies designed to address the opioid epidemic by targeting prices.

fill. To estimate the total quantity margin response, we explore two measures of intensive margin elasticity: days of supply and morphine milligram equivalents (MME) per year. Finally, we delve deeper into the three channels through which an increase in health insurance generosity can affect opioid painkiller purchases: (i) increased physician visits, (ii) increased writing of prescriptions, and (iii) increased filling of prescriptions.

Ideally, we would like to leverage exogenous variation in the out-of-pocket (OOP) price of prescription opioids while holding fixed the OOP price of other healthcare services. However, that experiment is yet to be run. Instead, we use rich data from the RAND Health Insurance Experiment (RAND HIE), a large randomized field trial of alternative insurance plan generosity offered to a representative sample of the non-elderly US population. Two reasons make the RAND HIE particularly well-suited for this study. Firstly, the random assignment of families to plans enables us to overcome the standard adverse selection of sicker patients to more generous insurance tiers (Akerlof, 1970; Rothschild and Stiglitz, 1976), as well as selection based on age (e.g., Medicare), socioeconomic status (e.g., Medicaid), or differences in covered services across plans. Secondly, unlike typical claims data which only document purchased prescriptions, our data allow us to distinguish between *unwritten* and *unfilled* prescriptions.[3]

We start by documenting some novel empirical facts highlighting the widespread usage of prescribed opioid painkillers in the 1970s.[4] Although the RAND HIE was fielded before the first modern wave of the opioid overdose epidemic in the 1990s (CDC, 2021), 57.7% of the prescribed painkillers purchased were opioids, and the share of individuals with at least one opioid prescription filled in a given year ranged from 11.4% in the most generous insurance plan to 6.1% in the least generous plan. To put these numbers into perspective, in 2021, 45% of the prescribed painkillers purchased were opioids, 5% of the non-elderly US population filled at least one opioid prescription, and 93%

[3]Thereby, we add to the scant literature on how physicians' prescribing behavior responds to patients' plan generosity (e.g., Dickstein (2017); Carrera et al. (2018)).

[4]Appendix OA.2 examines the historical context surrounding opioid utilization in the US in the 20th century.

of the MMEs purchased involved drugs also purchased in the RAND HIE data.[5]

A vast literature studies the price elasticity of demand for prescription drugs.[6] Surprisingly, the literature remains mostly silent about the response of prescribed opioid consumption to OOP price changes. There are only three exceptions, all concentrating on an older population group, leveraging the introduction of Medicare Part D in 2006, and finding contradicting results. Powell, Pacula, and Taylor (2020) estimate an elasticity of -0.60 for the annual number of opioid prescriptions filled. Adopting a similar approach, Soni (2019) studies the response of opioid days of supply and estimates an elasticity of -0.89, driven mostly by new users. Einav, Finkelstein, and Polyakova (2018) leverage a discrete price change at the "donut hole" (i.e., a coverage gap) to estimate price elasticities of demand across more than 150 drugs, including opioid painkillers. Focusing on a relatively sicker population, they estimate an elasticity of -0.04 at the extensive margin. Based on these estimates, it is unclear whether price-based mechanisms would play a role in curbing prescription opioid usage.

Focusing on a more general population group, we provide compelling evidence showing that opioid purchases decrease significantly as health insurance generosity declines, both at the extensive and intensive margins. On the extensive margin, we find that individuals with the highest cost-sharing are 5.3%-points (37.3%) less likely to purchase opioid painkillers relative to individuals with full insurance. On the intensive margin, our estimates indicate that individuals in the least generous plan spend \$5.1 (51.0%) less on opioid painkillers, have 1.15 (44.3%) fewer annual days of supply, consume 47.0 (34.9%) fewer MME per year, and fill 0.24 (52.1%) fewer prescriptions per year, compared to individuals with full insurance. Our treatment effects translate into precisely estimated elasticities of -0.19 at the extensive margin and elasticities ranging between -0.24 to -0.33

[5] Authors' calculations based on the 2021 Medical Expenditure Panel Survey (MEPS). See Appendix OA.4.

[6] See e.g., Einav, Finkelstein, and Schrimpf (2015) and Aron-Dine et al. (2015). The RAND HIE has also contributed to this literature. For instance, Leibowitz, Manning, and Newhouse (1985) and Newhouse et al. (1993) estimate an elasticity for prescription drugs of -0.17, similar to the gold standard of -0.20 for all medical services (Manning et al., 1987; Keeler and Rolph, 1988).

at the intensive margin.

These margins of response reflect not only the likelihood that a patient visited a physician but also the likelihood that a physician wrote a prescription, as well as the probability that the patient filled the prescription. Our unique data allow us to disentangle these underlying mechanisms driving opioid elasticities, making a significant and valuable contribution to the existing literature. We find that responses to price changes are partly driven by additional physician visits (i.e., patient behavior). The physician's behavior also plays an important role: we show that physicians are less likely to write an opioid prescription as plan generosity decreases, conditional on visiting a physician. This suggests that physicians internalize that patients enrolled in less generous plans are less likely to buy the prescribed medication and, therefore, less likely to comply with the treatment. In contrast, we find no effect through the share of unfilled prescriptions. Even though one in five opioid prescriptions to adult males are not filled, this share does not seem to vary across plan generosity, after conditioning on patient's pain, health, and prescriber's characteristics.

Finally, we pair our estimated elasticities with current data on prescription opioid quantities and prices to assess the implications of introducing a national opioid tax of one cent per MME, currently on the agenda for the US Congress (Senate Bill 1723, 2021). The aforementioned elasticity estimates would be relevant to evaluate the introduction of a policy that changes the price across the board of healthcare services, such as *Medicare for All* (Senate Bill 4204, 2022; Clemens, Gottlieb, and Hicks, 2021; Einav and Finkelstein, 2023). However, the currently proposed national opioid tax will only affect the price of prescription opioids. We estimate the policy-relevant elasticity using only individuals who visited a physician for pain-related reasons, while also accounting for endogenous selection into physicians. We estimate an 11.8% increase in the price per MME and a 1.9% decrease in the MME volume per year. The collections from the tax represent 16.6% of the per capita societal cost of substance abuse treatment facilities. From these numbers, it is clear that the proposed tax will only have a modest effect on decreasing prescription opioid use but a prominent role in generating revenues to expand access to substance abuse treatment.

A potential limitation of our study is the reliance on data from the RAND HIE, an experiment conducted four decades ago. To address this limitation, we offer new insights into the remarkably high opioid prescribing and consumption rates in the 1970s. These rates closely resemble the 2021 landscape, particularly concerning the ratio of opioids to prescribed painkillers, the prevalence of individuals making at least one opioid purchase, and the opioid drugs utilized. Furthermore, several studies based on more recent data have identified health expenditure elasticities comparable to those observed in the RAND HIE (e.g., Dunn (2016); Einav et al. (2013); Dalton (2014)). Second, one would expect the level of opioid addiction in the population to be higher nowadays than in the mid-70s. Leveraging pre-randomization variables, we show that our price elasticities mostly reflect the responsiveness of opioid-naïve patients. Lastly, since most changes that occurred in the mid-90s were geared toward diminishing the stigma surrounding opioid prescription and consumption for the treatment of chronic, non-cancer pain, it is plausible that the behavior towards opioids has changed. However, recent empirical evidence suggests that the demand for opioids like heroin responds similarly to price changes both in the late 80s and early 2010s (e.g., Chaloupka and Pacula (2000); Olmstead et al. (2015)). All things considered, we believe our results are still informative and relevant.

Beyond the studies referenced above, our paper also relates to two strands of the literature. Firstly, we add to the literature studying the price responsiveness of addictive substances.[7] Focusing on the 1923-1938 period, when opium was illegal in Indonesia, Van Ours (1995) find a short-run price elasticity of demand for opium of -0.70. Centering on heroin, the literature uncovers a relatively high price elasticity. For example, combining experimental and longitudinal survey data, Olmstead et al. (2015) estimate a price elasticity of demand for heroin (conditional on non-zero demand) of approximately -0.80. Focusing on the late 80s, Saffer and Chaloupka (1999) reported a price elasticity estimate for heroin at -0.94. Secondly, our paper relates to recent studies underscoring the continued relevance of the RAND HIE to address current policy questions, even four

[7]See e.g., Nisbet and Vakil (1972), Grossman and Chaloupka (1998), Chaloupka and Pacula (2000), and Dave (2006) for estimates on price responsiveness of non-opioid addictive substances such as marijuana and cocaine.

decades after its conduction.[8]

The rest of the paper is structured as follows. In Section 2, we describe the data used for the analysis. We present the treatment effects in Section 3. In Section 4, we discuss potential threats to the validity of our identification strategy and present robustness checks. We discuss mechanisms in Section 5, derive elasticities in Section 6, and illustrate the introduction of a national opioid tax in Section 7. Finally, in Section 8, we provide concluding remarks.

2 Data

We use rich claim-level data from the RAND HIE, a large-scale randomized controlled trial of alternative health insurance plans conducted between 1974 and 1982 in the United States. A total of 8,254 individuals were randomly assigned to one of six groups of fee-for-service (FFS) plans or to a prepaid group practice, for either three or five years. The FFS plans varied along two principal dimensions: (1) the coinsurance rate, which is the fraction of the bill paid by the patient, and (2) the maximum dollar expenditure (MDE), which caps the family OOP expenditures. Four of the six groups of plans set their coinsurance rates at either 0 (free care), 25, 50, or 95 percent. There was a group of "mixed coinsurance" plans, with a 25 percent coinsurance rate for most services but 50 percent for dental and outpatient mental health services. Finally, the "individual deductible" plan had a coinsurance rate of 95 percent for outpatient services but 0 percent for inpatient services. Except for the free-care plan, each plan had a MDE of either 5, 10, or 15 percent of family income in the previous year. For a detailed description of the RAND HIE, see Newhouse et al. (1993).

We make three restrictions to construct our sample. First, we drop the years in which the individuals do not participate in full and all years thereafter, except for newborns. Second, we drop the year in which individuals move and therefore switch plans, and all years thereafter. Third, we drop

[8] See e.g., Diaz-Campo (2023); Hodor (2021); Lin and Sacks (2019); Aron-Dine, Einav, and Finkelstein (2013); Vera-Hernandez (2003).

individuals enrolled in the prepaid group practice because the method of care delivery is substantially different from the FFS plans. After these restrictions, our sample has 20,004 individual-year observations with 5,922 unique individuals and 3,100 unique families.[9] We combine line-item records from three RAND HIE claims files: (1) services rendered by physicians in outpatient settings, (2) drugs prescribed by physicians in outpatient settings, and (3) drugs purchased from pharmacies. A typical line-item record contains several variables including, but not limited to, patient and provider identifiers, service date, diagnoses and procedure codes, total line-item cost, and the portion paid OOP by the patient. We further use the eligibility and demographic files to build the family composition and define the participation periods for each member.

The line-item records related to drugs prescribed and drugs purchased, provide comprehensive information on medication characteristics, including the drug name, form, strength, quantity, drug therapeutic code, generic drug code, National Drug Code (NDC), and prescription status. To identify opioid painkillers, we use data from the 2020 CDC Oral MME Conversion file, containing all opioid analgesics that are normally prescribed in outpatient settings, dispensed by retail pharmacies, and controlled by the Drug Enforcement Administration (DEA). We make three main restrictions: (1) we classify a drug prescription or purchase as a painkiller only if its associated drug therapeutic code falls under strong analgesics, mild analgesics, or anti-rheumatic agents; (2) we exclude opioid treatment drugs such as methadone and naltrexone; and (3) we exclude all painkiller prescriptions and purchases prescribed by dentists.[10,11]

With these data in hand, we generate three key variables for each opioid painkiller purchase: (a) days of supply, (b) MME per day, and (c) an indicator for high-dose opioid purchase. We follow the CDC guidelines and define these variables as follows:

[9] Appendix OA.3.2 shows the remaining number of observations after each sample restriction.

[10] Appendix OA.3.3 presents the details to identify opioid painkillers in the RAND HIE data.

[11] Appendix OA.5 shows that our analyses are robust to including opioid purchases prescribed by dentists.

$$\text{days of supply} = \frac{\text{number of units}}{\text{quantity per intake} \times \text{intakes per day}} \tag{1a}$$

$$\text{MME per day} = \text{strength per unit} \times \frac{\text{number of units}}{\text{days of supply}} \times \text{MME conversion factor} \tag{1b}$$

$$\text{high-dose} = \mathbb{1}[\text{MME per day} \geq 90] \tag{1c}$$

where the variables *number of units*, *quantity per intake*, *intakes per day*, and *strength per unit* come directly from the claim records, and *MME conversion factor* comes from the CDC Oral MME Conversion file. Lastly, we generate three variables for each physician outpatient visit: (a) an indicator for pain-related visit, (b) an indicator for any opioid prescription written, conditional on a pain-related visit, and (c) an indicator for whether the prescription was filled, conditional on a pain-related visit and an opioid prescription.

Table 1 describes the most relevant statistics of our sample by plan group. Each of the six columns presents raw means and standard deviations at the person-year level by plan. Each row presents a measure related to painkiller utilization, pain-related visits, health and pain levels as measured in the baseline questionnaire, and demographics. Free care is the largest plan, encompassing 33.2% of individuals, followed by the individual deductible and 95 percent coinsurance plans, with 21.3% and 19.1% of individuals, respectively. Comparing the highest cost-sharing plan (the 95 percent coinsurance plan) with the free-care plan, the raw means indicate an 8.1%-point (45.5%) decline in the fraction of individuals with at least one prescribed painkiller purchase and an $11.0 (59.4%) decline in average annual spending in prescribed painkillers (in 2019 dollars). Focusing on the fourth row, the share of individuals with at least one opioid prescription filled in a given year ranges from 11.4% on the free-care plan to 6.1% in the least generous plan. Although the RAND HIE was conducted before the first modern wave of the opioid overdose epidemic (Quinones, 2015;

Cutler and Glaeser, 2021; Alpert et al., 2022; Arteaga and Barone, 2022), we document for the first time that opioid consumption was already prevalent in the 1970s.

Focusing on rows 11 to 13, the use of physician outpatient visits for pain-related reasons correlates unequivocally with changes in the amount paid out of pocket. Individuals in the highest cost-sharing plan are 16.2%-points (33.7%) less likely to have a pain-related visit in a given year, have 1.4 (47.8%) fewer pain visits on average, and, as a consequence, spend \$82 (44.5%) less in pain visits relative to individuals in free care. Thanks to our unique data, rows 14 and 10 present novel evidence on the share of pain-related visits with an opioid prescription *written* and the share of unfilled opioid prescriptions, respectively. There is not much variation across plans for the former: about one in ten pain-related visits end up with an opioid prescription. In contrast, the share of unfilled opioid prescriptions varies non-monotonically with plan generosity between 17.3% and 30.1%. Lastly, the last rows of Table 1 describe the proportion of individuals with different levels of pain and health as measured in the baseline questionnaire. These measures will be used later in the analysis when we explore the mechanisms behind opioid elasticities.

3 Empirical Analysis: Treatment Effects

In this section, we study the response of prescribed painkiller and opioid painkiller purchases to changes in health insurance generosity. To that end, we explicitly leverage the random assignment of individuals to health insurance plans from the RAND HIE. Consider an individual i, in calendar year t, enrolled in health insurance plan $p \in \{1, 6\}$, in location l and starting month m. Mimicking the framework from Aron-Dine, Einav, and Finkelstein (2013), hereinafter referred to as AEF, our baseline regression for outcome $Y_{i,t}$ is,

$$Y_{i,t} = \lambda_p + \tau_t + \alpha_{l,m} + \beta X' + \varepsilon_{i,t} \quad (2)$$

where τ_t are calendar year fixed effects, $\alpha_{l,m}$ are location-by-start-month fixed effects, and the vector

Table 1: Summary Statistics

	(1) Free Care		(2) 25% Coinsurance		(3) Mixed Coinsurance		(4) 50% Coinsurance		(5) Individual Deductible		(6) 95% Coinsurance	
	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd
Painkillers (Rx-only):												
1. Any painkiller purchase	0.178	(0.38)	0.127	(0.33)	0.129	(0.34)	0.107	(0.31)	0.110	(0.31)	0.097	(0.30)
2. Painkiller spending (2019 $)	18.566	(103.67)	8.125	(46.43)	10.923	(73.63)	5.113	(27.47)	9.247	(63.19)	7.538	(46.52)
Opioid Painkillers:												
3. Any opioid Rx	0.127	(0.33)	0.093	(0.29)	0.095	(0.29)	0.076	(0.26)	0.085	(0.28)	0.073	(0.26)
4. Any opioid purchase	0.114	(0.32)	0.072	(0.26)	0.080	(0.27)	0.067	(0.25)	0.069	(0.25)	0.061	(0.24)
5. Any high-dose opioid purchase	0.027	(0.16)	0.009	(0.09)	0.022	(0.15)	0.006	(0.08)	0.017	(0.13)	0.012	(0.11)
6. Opioid spending (2019 $)	7.940	(64.77)	2.332	(13.54)	4.650	(56.80)	1.945	(12.00)	3.701	(39.13)	2.723	(21.77)
7. Annual days of supply	2.046	(16.50)	0.625	(3.92)	1.334	(15.33)	0.783	(7.02)	1.070	(11.83)	0.916	(9.76)
8. Annual MME	98.708	(825.63)	29.977	(264.96)	84.268	(991.74)	28.001	(315.82)	56.807	(586.01)	54.931	(629.77)
9. MME/day | opioid purchase	54.268	(39.49)	49.570	(33.68)	57.174	(45.42)	48.547	(37.61)	58.222	(40.76)	57.963	(39.08)
10. Share unfilled Rx | Rx	0.183	(0.32)	0.301	(0.41)	0.231	(0.37)	0.173	(0.32)	0.260	(0.38)	0.222	(0.36)
Physician Outpatient Visits:												
11. Any pain visit	0.481	(0.50)	0.406	(0.49)	0.420	(0.49)	0.367	(0.48)	0.360	(0.48)	0.319	(0.47)
12. Number of pain visits	2.843	(6.67)	1.815	(4.83)	2.289	(5.93)	1.500	(3.44)	1.862	(5.02)	1.483	(4.67)
13. Pain visits spending (2019 $)	184.426	(532.62)	128.138	(463.11)	160.912	(500.13)	105.912	(440.73)	133.733	(441.62)	102.302	(407.90)
14. Share opioid Rx | pain visit	0.122	(0.27)	0.110	(0.27)	0.096	(0.24)	0.107	(0.27)	0.111	(0.26)	0.111	(0.27)
15. Any opioid Rx | any pain visit	0.259	(0.44)	0.225	(0.42)	0.225	(0.42)	0.198	(0.40)	0.232	(0.42)	0.219	(0.41)
Pain Level at Baseline:												
16. A great deal	0.035	(0.18)	0.027	(0.16)	0.040	(0.20)	0.025	(0.16)	0.044	(0.21)	0.033	(0.18)
17. Some pain	0.113	(0.32)	0.080	(0.27)	0.151	(0.36)	0.090	(0.29)	0.115	(0.32)	0.102	(0.30)
18. A little pain	0.325	(0.47)	0.389	(0.49)	0.302	(0.46)	0.334	(0.47)	0.324	(0.47)	0.369	(0.48)
19. No pain at all	0.425	(0.49)	0.441	(0.50)	0.432	(0.50)	0.494	(0.50)	0.425	(0.49)	0.405	(0.49)
20. Missing	0.103	(0.30)	0.063	(0.24)	0.075	(0.26)	0.056	(0.23)	0.092	(0.29)	0.091	(0.29)
Health Status at Baseline:												
21. Poor	0.020	(0.14)	0.006	(0.08)	0.009	(0.09)	0.014	(0.12)	0.017	(0.13)	0.012	(0.11)
22. Fair	0.078	(0.27)	0.084	(0.28)	0.075	(0.26)	0.070	(0.26)	0.075	(0.26)	0.072	(0.26)
23. Good	0.342	(0.47)	0.358	(0.48)	0.388	(0.49)	0.335	(0.47)	0.366	(0.48)	0.362	(0.48)
24. Excellent	0.457	(0.50)	0.492	(0.50)	0.455	(0.50)	0.525	(0.50)	0.447	(0.50)	0.463	(0.50)
25. Missing	0.103	(0.30)	0.059	(0.24)	0.073	(0.26)	0.056	(0.23)	0.095	(0.29)	0.091	(0.29)
Demographics:												
26. Share age < 18	0.420	(0.49)	0.424	(0.49)	0.437	(0.50)	0.411	(0.49)	0.390	(0.49)	0.422	(0.49)
27. Share female	0.511	(0.50)	0.511	(0.50)	0.533	(0.50)	0.510	(0.50)	0.526	(0.50)	0.523	(0.50)
# Families	1040		327		285		208		668		572	
# Individuals	1964		663		507		393		1261		1134	
# Individual-years	6724		2333		1704		1417		4087		3739	

Notes: This table reports summary statistics from our RAND HIE analysis sample, by health insurance plan. Each of the six columns presents raw means and standard deviations at the individual-year level by plan. Standard deviations are reported in parentheses beside the mean.

X contains dummies for gender and age. Specifically, we include a dummy variable for women and a dummy variable for individuals under the age of 18. The main parameters of interest are the health insurance plan fixed effects, λ_p, measuring the average effect of each health insurance plan on outcome variable $Y_{i,t}$ for adult males. All standard errors are clustered at the family level.

We begin by evaluating the response of all prescribed painkiller purchases (i.e., opioid and non-opioid). We consider two outcomes: (1) a dummy variable for any painkiller purchase in the year, and (2) annual spending on painkillers. The former measures whether the individual purchased at least one prescribed painkiller in a given year. The latter is the sum of both the portion paid OOP by the individual and the portion paid by the insurer, aggregated at the annual level (in 2019 dollars). The first two columns of Table 2 report the estimated λ_p coefficients from Equation (2) for these two outcomes. The estimates indicate that painkiller purchases, both at the extensive and intensive margins, decrease significantly as health insurance generosity declines. For instance, individuals with the highest cost-sharing are 8.4%-points (37.3%) less likely to purchase painkillers relative to individuals with full insurance. They also spend $11.1 (47.7%) less on painkillers in a given year.

Our focus now turns to prescribed opioid painkillers. To evaluate effects at the extensive margin, we consider two measures: (1) a dummy variable for any opioid painkiller purchase in the year, and (2) a dummy variable for any high-dose opioid painkiller purchase in the year. At the intensive margin, we consider four outcomes: (1) annual spending on opioid painkillers, (2) annual days of supply, (3) annual MME, and (4) the number of opioid prescriptions filled in the year. The measures of days of supply and MME are the sum across all prescribed opioid painkiller purchases at the individual-year level. Finally, outcome (4) counts the number of opioid prescriptions filled at the individual-year level. By construction, these four measures are zero for individuals without opioid painkiller purchases in a given year. Among the four measures at the intensive margin, (2) and (3) are the most convenient as they allow meaningful comparisons across individuals and opioid drugs.

The results are reported in columns 3 to 8 of Table 2. The estimates provide clear evidence that all six outcomes decrease significantly as health insurance generosity declines. On the extensive margin, individuals with the highest cost-sharing are 5.3%-points (37.3%) and 1.5%-points (46.9%) less likely to purchase opioid and high-dose opioid painkillers, respectively, relative to individuals with full insurance. On the intensive margin, the estimates indicate that individuals in the least generous plan spend $5.1 (51.0%) less on opioid painkillers, have 1.15 (44.3%) fewer days of supply, consume 47.0 (34.9%) fewer MME, and fill 0.24 (52.1%) fewer prescriptions, compared to individuals with full insurance. All point estimates show a consistent pattern of fewer opioid purchases, at all margins, in higher cost-sharing plans.

Table 2: Plans' Effects on Prescribed Painkiller Purchases

	Painkiller Purchase		Opioid Painkiller Purchase					
	Share with Any (1)	Spending in $ (2)	Share with Any (3)	Share with Any High-Dose (4)	Spending in $ (5)	Annual Days of Supply (6)	Annual MME (7)	Number of Rx Purchased (8)
Const. (Free Care)	0.225	23.191	0.142	0.032	10.013	2.599	134.635	0.453
	(0.008)	(2.143)	(0.006)	(0.003)	(1.257)	(0.345)	(17.736)	(0.053)
25% Coinsurance	-0.055	-9.963	-0.041	-0.017	-5.468	-1.503	-73.221	-0.234
	(0.011)	(2.297)	(0.008)	(0.003)	(1.094)	(0.310)	(16.693)	(0.046)
Mixed Coinsurance	-0.045	-7.030	-0.034	-0.006	-2.913	-0.611	-11.266	-0.188
	(0.013)	(2.863)	(0.010)	(0.004)	(1.842)	(0.513)	(32.957)	(0.053)
50% Coinsurance	-0.077	-13.211	-0.049	-0.022	-5.570	-1.316	-79.601	-0.239
	(0.012)	(2.125)	(0.010)	(0.003)	(1.128)	(0.397)	(20.378)	(0.051)
Ind. Deductible	-0.070	-9.279	-0.047	-0.011	-4.144	-0.957	-41.329	-0.230
	(0.009)	(2.361)	(0.007)	(0.003)	(1.440)	(0.411)	(21.472)	(0.048)
95% Coinsurance	-0.084	-11.054	-0.053	-0.015	-5.109	-1.151	-46.963	-0.236
	(0.010)	(2.271)	(0.008)	(0.003)	(1.222)	(0.399)	(23.526)	(0.049)
Adjusted R^2	0.07	0.03	0.04	0.02	0.01	0.01	0.01	0.02
# Families	3100	3100	3100	3100	3100	3100	3100	3100
# Individuals	5922	5922	5922	5922	5922	5922	5922	5922
# Individual-Years	20004	20004	20004	20004	20004	20004	20004	20004

Notes: The reported coefficients are from ordinary least squares regressions and indicate the effect of the various plans on the outcome given in the column relative to the free-care plan (whose mean is given by the constant term). The outcomes are: (1) a dummy variable for annual purchase of painkillers, (2) annual spending on painkillers, (3) a dummy variable for annual purchase of opioid painkillers, (4) a dummy variable for annual purchase of high-dose opioid painkillers, (5) annual spending on opioid painkillers, (6) annual days of supply of opioid painkillers, (7) annual MME for opioid painkillers, and (8) annual number of opioid prescriptions purchased. Because assignment to plans was random only conditional on site and start month (Newhouse et al., 1993), all regressions include site-by-start-month dummy variables, as well as calendar year fixed-effects. Regressions also include a dummy variable for women and a dummy variable for individuals under the age of 18. All spending variables are inflation-adjusted to 2019 dollars (adjusted using the CPI-U). Site by start month and year dummy variables are de-meaned so that the coefficients reflect estimates for adult males at the "average" site-month-year mix. Standard errors, clustered on family, are reported in parentheses below the coefficients.

4 Threats to Validity and Robustness Checks

In this section, we present results from alternative specifications designed to address potential threats to our identification strategy and to illustrate the robustness of our main findings. First, we build on AEF and provide additional evidence that our identification strategy is valid. Second, we show that our results are robust to controlling for additional covariates and adjusting for under-reporting.

Our estimates from Table 2 rely on random assignment of individuals to health insurance plans from the RAND HIE. One potential concern is that random assignment failed to produce comparable experimental conditions on characteristics measured before the treatment was administered. To mitigate this concern, AEF estimate Equation (2) using pre-randomization covariates as the main outcome. The authors consider several characteristics, both utilized and excluded from the finite selection model used for randomization (Morris et al., 1979), including self-reported measures of health and pain, age, education, family income, employment, health insurance coverage, medical and dental visits, and hospitalizations. In most cases, the authors fail to reject the null hypothesis that the characteristics are balanced across plans, with a few exceptions for variables not used in the finite selection model.

To further validate the credibility of the initial randomization, we conduct the same analysis on pre-randomization characteristics that were not considered by AEF, and that are pertinent to our analysis of opioid painkiller purchases. We examine variables related to smoking and drinking behavior, which are often correlated with other risky behaviors such as opioid use (Cawley and Ruhm, 2011). Specifically, we use dummy variables for whether the individual is a current smoker, a former smoker, or whether smoking information is missing. In addition, we use dummy variables for whether the individual has a drinking problem, missing information on drinking issues, a continuous variable measuring the average monthly volume of ethanol consumption, and a dummy variable for missing information on alcohol volume. All these variables come from the baseline questionnaire. The first panel of Table 3 reports the estimated λ_p coefficients from Equation (2) for each outcome.

In all cases, we fail to reject the null hypothesis that characteristics related to smoking and drinking are balanced across plans.

Despite the encouraging outcomes from the balance tests, we present additional results from an alternative specification of Equation (2) that adds all pre-randomization covariates as controls (i.e., those considered by AEF plus the smoking and drinking variables). The purpose of this exercise is to illustrate the robustness of our core findings regarding sensitivity of opioid painkiller purchases to health insurance generosity. These results for the six measures of opioid painkiller purchases are displayed in the odd columns of the second panel of Table 3. In all cases, our results are very robust to adding pre-randomization covariates as controls and, consequently, we further validate the credibility of our core findings.

As noted early on by Newhouse et al. (1993), refusal and attrition were higher on the cost-sharing plans, though they seem to be random with respect to the characteristics of the participants. To mitigate this concern, we present results from an alternative specification in which we attempt to adjust our outcome measures for underreporting. Mimicking AEF, we scale up the share of individuals with any purchase of opioid and high-dose opioid painkillers, spending, days of supply, MME per year, and number of prescriptions filled using the plan-specific underreporting percentages identified in Rogers and Newhouse (1985).[12] The results are displayed in the even columns of the second panel of Table 3. Once again, our estimates remain largely unchanged and, therefore, we confirm the robustness of our core findings to underreporting.

[12]Following Rogers and Newhouse (1985), we use a 4% underreporting rate for individuals in free care; 6% for the 25 percent, 50 percent and mixed coinsurance plans; 14% for the individual deductible plan; and 11% for the 95 percent coinsurance plan.

Table 3: Threats to Validity and Robustness Checks

(a) Panel A: Pre-randomization covariates and plan assignment

	Former Smoker (1)	Current Smoker (2)	Smoking Missing (3)	Ethanol Vol (4)	Ethanol Missing (5)	Drink Problem (6)	Drink Missing (7)
Constant (Free Care Plan)	0.204	0.441	0.032	16.861	0.042	0.141	0.016
	(0.012)	(0.017)	(0.007)	(1.024)	(0.007)	(0.009)	(0.006)
25% Coinsurance	0.011	0.001	0.015	0.558	0.015	0.011	0.015
	(0.022)	(0.030)	(0.010)	(1.350)	(0.011)	(0.014)	(0.009)
Mixed Coinsurance	0.007	0.019	-0.018	-0.288	-0.020	-0.009	-0.019
	(0.021)	(0.030)	(0.012)	(1.127)	(0.013)	(0.013)	(0.012)
50% Coinsurance	0.017	-0.021	0.007	-0.461	0.025	-0.010	0.013
	(0.025)	(0.037)	(0.013)	(1.381)	(0.014)	(0.015)	(0.012)
Individual Deductible	-0.022	0.014	0.008	1.489	0.003	-0.001	0.004
	(0.015)	(0.024)	(0.009)	(1.171)	(0.009)	(0.010)	(0.008)
95% Coinsurance	0.007	-0.003	0.011	2.495	0.002	0.013	0.006
	(0.017)	(0.024)	(0.010)	(1.488)	(0.010)	(0.012)	(0.009)
Adjusted R^2	0.04	0.04	0.72	0.07	0.69	0.06	0.75
P-Value F	0.483	0.934	0.173	0.329	0.124	0.630	0.144
# Families	2993	2993	3100	2997	3100	3038	3100
# Individuals	5922	5922	5922	5922	5922	5922	5922
# Individual-Years	12466	12466	20004	12319	20004	12653	20004

(b) Panel B: Controlling for pre-randomization covariates and adjustment for underreporting

	Share with Any		Share with Any High-Dose		Spending in $		Annual Days of Supply		Annual MME		Number of Rx Purchased	
	Covs. (1)	Adj. (2)	Adj. (3)	Adj. (4)	Covs. (5)	Adj. (6)	Covs. (7)	Adj. (8)	Covs. (9)	Adj. (10)	Covs. (11)	Adj. (12)
Constant (Free Care Plan)	0.115	0.149	0.025	0.034	7.716	10.441	1.898	2.709	96.277	140.833	0.353	0.473
	(0.008)	(0.007)	(0.004)	(0.003)	(1.041)	(1.310)	(0.282)	(0.360)	(15.227)	(18.574)	(0.042)	(0.055)
25% Coinsurance	-0.039	-0.041	-0.015	-0.017	-4.795	-5.651	-1.334	-1.559	-65.590	-76.016	-0.210	-0.241
	(0.008)	(0.009)	(0.003)	(0.003)	(0.969)	(1.145)	(0.279)	(0.327)	(16.223)	(17.658)	(0.040)	(0.048)
Mixed Coinsurance	-0.036	-0.034	-0.005	-0.006	-2.946	-2.914	-0.606	-0.600	-7.299	-9.499	-0.197	-0.192
	(0.010)	(0.010)	(0.004)	(0.005)	(1.870)	(1.942)	(0.522)	(0.541)	(32.401)	(34.837)	(0.057)	(0.056)
50% Coinsurance	-0.046	-0.050	-0.020	-0.023	-4.465	-5.761	-0.988	-1.361	-65.440	-82.962	-0.198	-0.246
	(0.010)	(0.010)	(0.003)	(0.004)	(1.020)	(1.182)	(0.361)	(0.418)	(20.118)	(21.588)	(0.045)	(0.053)
Individual Deductible	-0.047	-0.042	-0.010	-0.009	-4.045	-3.938	-0.946	-0.887	-37.528	-37.114	-0.230	-0.225
	(0.007)	(0.008)	(0.003)	(0.004)	(1.399)	(1.568)	(0.412)	(0.450)	(20.736)	(23.466)	(0.049)	(0.050)
95% Coinsurance	-0.051	-0.051	-0.013	-0.014	-4.347	-5.127	-0.940	-1.135	-35.499	-45.122	-0.213	-0.237
	(0.007)	(0.008)	(0.003)	(0.003)	(1.123)	(1.295)	(0.393)	(0.430)	(23.555)	(25.512)	(0.046)	(0.051)
Adjusted R^2	0.07	0.04	0.03	0.01	0.06	0.01	0.06	0.01	0.06	0.01	0.07	0.02
# Families	3100	3100	3100	3100	3100	3100	3100	3100	3100	3100	3100	3100
# Individuals	5922	5922	5922	5922	5922	5922	5922	5922	5922	5922	5922	5922
# Individual-Years	20004	20004	20004	20004	20004	20004	20004	20004	20004	20004	20004	20004

Notes: The reported coefficients are from an ordinary least squares regression and indicate the effect of the various plans on the outcome given in the column relative to the free-care plan. In panel A, the outcomes, measured at the baseline survey, are: (1) a dummy variable for whether the individual is a former smoker, (2) a dummy for whether he is a current smoker, (3) a dummy for missing smoking status, (4) average monthly volume of ethanol consumption, (5) a dummy variable for missing ethanol consumption, (6) a dummy variable for whether the individual reports having a drinking problem, and (7) a dummy variable for missing information on drinking issues. In Panel B, the outcomes are: (1) a dummy variable for annual purchase of opioid painkillers, (2) a dummy variable for annual purchase of high-dose opioid painkillers, (3) annual spending on opioid painkillers, (4) annual days of supply of opioid painkillers, (5) annual MME for opioid painkillers, and (6) annual number of opioid prescriptions purchased. For each outcome of panel B, the specification in the odd column includes all pre-randomization covariates as controls; the specification in the even column adjusts outcomes for underreporting. All regressions include a dummy variable for women, a dummy variable for individuals under the age of 18, de-meaned site by start month dummy variables, and de-meaned year fixed effects. All spending variables are inflation-adjusted to 2019 dollars (adjusted using the CPI-U). Standard errors, clustered on family, are reported in parentheses below the coefficients.

5 Mechanisms

In this section, we explore the mechanisms driving the documented responses in opioid painkiller purchases. An increase in health insurance generosity can affect opioid purchases via two main mechanisms. First, individuals may respond by seeking additional pain-related visits to the physician. Second, conditional on a visit, individuals may be more likely to purchase opioid painkillers.

We begin by exploring the role of additional physician visits. To do so, we estimate Equation (2) using three outcomes connected to pain-related visits in any given year: (1) a dummy variable for any visit, (2) annual spending on visits, and (3) number of visits. The results are displayed in the first three columns of Table 4. Consistent with the findings for all outpatient medical visits in Newhouse et al. (1993), the estimates provide clear evidence that pain-related visits decrease significantly as health insurance generosity declines. For instance, individuals with the highest cost-sharing are 17.2%-points (34.1%) less likely to visit a physician for pain-related issues, spend \$84.5 (36.9%) less on physician visits, and have 1.4 (40.7%) fewer visits, relative to individuals with full insurance.

Our focus now turns to the second mechanism. As health insurance generosity increases, there is a corresponding decrease in OOP expenses for both physician visits and prescription medications. Conditional on a pain-related visit, the remaining variation in prices across patients comes from prescription drugs only. For individual-year pairs with at least one pain-related visit, we estimate Equation (2) using our usual six measures of opioid purchases. In columns (4) to (15) of Table 4, we evaluate the response of opioid painkiller purchases conditional on having a pain-related visit. The estimates in even columns provide clear evidence that conditional on a pain-related visit, all outcomes decrease significantly as health insurance generosity declines.

One potential concern with this specification is that, unlike the first mechanism, it relies on a non-random sample of physician visits. A simple theoretical model *à la* Grossman (1972) featuring health capital and medical visits would suggest that, given exogenous variation in the price of visits,

Table 4: Plans' Effects on Physician Visits and Painkiller Purchases (Conditional on Physician Visits)

	Pain-Related Physician Visits			Share with Any Opioid Purch.		Share with Any High-Dose Opioid Purch.		Spending in $ Opioid Purch.		Annual Days of Supply Opioid Purch.		Annual MME Opioid Purch.		Number of Opioid Rx Purchased	
	Share with Any (1)	Spending in $ (2)	Number of Visits (3)	Base (4)	Pain and Health Dummies (5)	Base (6)	Pain and Health Dummies (7)	Base (8)	Pain and Health Dummies (9)	Base (10)	Pain and Health Dummies (11)	Base (12)	Pain and Health Dummies (13)	Base (14)	Pain and Health Dummies (15)
Constant (FC)	0.505	229.074	3.382	0.285	0.279	0.064	0.060	19.883	18.533	5.161	4.780	270.568	247.264	0.902	0.858
	(0.011)	(11.472)	(0.158)	(0.012)	(0.012)	(0.006)	(0.006)	(2.443)	(2.272)	(0.682)	(0.640)	(36.482)	(32.552)	(0.101)	(0.096)
25% Co.	-0.093	-63.042	-1.096	-0.047	-0.047	-0.030	-0.028	-10.075	-9.620	-2.865	-2.725	-141.452	-129.021	-0.414	-0.401
	(0.018)	(14.987)	(0.198)	(0.017)	(0.016)	(0.007)	(0.007)	(2.293)	(2.260)	(0.675)	(0.653)	(36.931)	(34.894)	(0.094)	(0.093)
Mixed Co.	-0.055	-24.248	-0.541	-0.049	-0.051	-0.008	-0.008	-4.623	-4.143	-0.909	-0.790	-5.874	1.124	-0.348	-0.343
	(0.020)	(17.882)	(0.244)	(0.019)	(0.019)	(0.010)	(0.010)	(4.260)	(4.253)	(1.174)	(1.169)	(73.237)	(71.895)	(0.114)	(0.117)
50% Co.	-0.129	-73.856	-1.305	-0.049	-0.043	-0.039	-0.036	-8.702	-7.628	-1.830	-1.499	-129.743	-112.195	-0.354	-0.311
	(0.022)	(15.218)	(0.185)	(0.020)	(0.020)	(0.008)	(0.008)	(2.149)	(2.078)	(0.865)	(0.822)	(45.990)	(43.602)	(0.097)	(0.092)
Individual Ded.	-0.118	-53.914	-1.026	-0.053	-0.057	-0.011	-0.013	-5.844	-7.043	-1.229	-1.584	-47.794	-68.553	-0.381	-0.415
	(0.016)	(13.587)	(0.183)	(0.016)	(0.015)	(0.008)	(0.008)	(3.379)	(3.163)	(0.996)	(0.920)	(52.000)	(47.544)	(0.100)	(0.099)
95% Co.	-0.172	-84.487	-1.376	-0.041	-0.041	-0.017	-0.017	-6.989	-6.912	-1.308	-1.272	-36.165	-32.990	-0.327	-0.324
	(0.016)	(12.413)	(0.175)	(0.017)	(0.017)	(0.008)	(0.008)	(2.755)	(2.567)	(1.031)	(0.970)	(63.581)	(59.465)	(0.105)	(0.100)
Some Pain					-0.103		-0.069		-54.256		-15.950		-983.048		-1.363
					(0.035)		(0.022)		(14.773)		(4.324)		(239.715)		(0.420)
A Little Pain					-0.163		-0.101		-57.861		-17.571		-1065.716		-1.643
					(0.034)		(0.021)		(14.112)		(4.102)		(231.527)		(0.393)
No Pain at All					-0.177		-0.102		-59.006		-17.672		-1062.878		-1.691
					(0.034)		(0.021)		(13.708)		(4.010)		(227.704)		(0.386)
Fair Health					0.005		-0.047		-0.255		0.819		-139.160		-0.142
					(0.050)		(0.038)		(20.032)		(5.637)		(353.677)		(0.551)
Good Health					-0.026		-0.062		-17.108		-3.694		-350.403		-0.469
					(0.049)		(0.036)		(16.658)		(4.720)		(313.726)		(0.483)
Excellent Health					-0.058		-0.071		-17.808		-3.921		-353.937		-0.591
					(0.050)		(0.036)		(16.584)		(4.677)		(310.791)		(0.477)
Adjusted R^2	0.04	0.02	0.04	0.06	0.07	0.03	0.05	0.03	0.07	0.02	0.07	0.02	0.09	0.04	0.08
# Families	3100	3100	3100	2370	2370	2370	2370	2370	2370	2370	2370	2370	2370	2370	2370
# Individuals	5922	5922	5922	4009	4009	4009	4009	4009	4009	4009	4009	4009	4009	4009	4009
# Individual-Years	20004	20004	20004	8081	8081	8081	8081	8081	8081	8081	8081	8081	8081	8081	8081

Notes: The reported coefficients are from ordinary least squares regressions and indicate the effect of the various plans on the outcome given in the column relative to the free-care plan (whose mean is given by the constant term). The outcomes are: a dummy variable for any visit (column 1), annual spending on visits (column 2), number of visits (column 3), a dummy variable for annual purchase of opioid painkillers (columns 4 and 5), a dummy variable for annual purchase of high-dose opioid painkillers (columns 6 and 7), annual spending on opioid painkillers (columns 8 and 9), annual days of supply of opioid painkillers (columns 10 and 11), annual MME for opioid painkillers (columns 12 and 13), and annual number of opioid prescriptions purchased (columns 14 and 15). Regressions in columns 1 to 3 are conducted using the full sample, while regressions in columns 4 to 15 are conducted on a subsample of individuals who visited a physician on a given year. Because assignment to plans was random only conditional on site and start month (Newhouse et al., 1993), all regressions include site by start month dummy variables, as well as year fixed effects. Regressions also include a dummy variable for women and a dummy variable for individuals under the age of 18. Regressions in odd columns 5 to 15 add de-meaned dummies for self-reported pain and health at the baseline survey. All spending variables are inflation-adjusted to 2019 dollars (adjusted using the CPI-U). Site by start month and year dummy variables are de-meaned so that the coefficients reflect estimates for adult males at the "average" site-month-year mix. Standard errors, clustered on family, are reported in parentheses below the coefficients.

patients' decision to visit the physician would be a function of their health and pain. One would expect that, as patient cost-sharing increases, the pain threshold above which the patient decides to consult a physician also increases. In such sense, among individuals who choose to visit the physician, those in the least generous plan should be sicker and suffer more pain relative to those in free care, on average, and therefore more likely to purchase opioid painkillers.

To control for selection bias related to health and pain, we include demeaned dummy variables for self-reported health and pain at baseline as covariates in the odd columns (5) through (15) of Table 4. As expected, individuals in higher pain have higher measures of opioid utilization at all margins, conditional on visiting a physician for a pain-related reason. The final estimates show a consistent pattern of fewer opioid purchases, at all margins, in higher cost-sharing plans, even after accounting for health and pain. Unsurprisingly, while all six outcomes still decrease significantly as health insurance generosity declines, the decrease is smaller after conditioning on visiting a physician. On the extensive margin, individuals with the highest cost-sharing are 4.1%-points (14.7%) and 1.7%-points (28.3%) less likely to purchase opioid and high-dose opioid painkillers, respectively, relative to individuals with full insurance. On the intensive margin, the estimates indicate that individuals in the least generous plan spend \$6.9 (37.3%) less on opioid painkillers, fill 0.3 (37.8%) fewer prescriptions, have 1.3 (26.6%) fewer annual days of supply, and consume 33.0 (13.3%) fewer MME per year compared to individuals with full insurance, although the last two estimates are imprecise.

The second mechanism can be further decomposed to separately account for the roles of physicians and patients as plan generosity increases. Conditional on a pain-related visit, physicians may respond via additional opioid prescriptions. Subsequently, conditional on having a prescription, individuals may respond by increasing the probability of filling the prescription. The latter channel is inherent to the patient, while the former is within the discretion of the physician. Typical claims data do not include information on unfilled prescriptions. By exploiting the uniqueness of our data, we are able to tease out these two channels.

Table 5: Physician Visits, Prescribing Behavior, and Unfilled Prescriptions

	Share with Any Opioid Painkiller Prescription		Number of Opioid Painkiller Prescriptions Per Visit		Share of Unfilled Opioid Prescriptions		
	Baseline (1)	Pain and Health Dummies (2)	Baseline (3)	Pain and Health Dummies (4)	Baseline (5)	Pain and Health Dummies (6)	Pain and Health Dummies, + Prescriber FE (7)
Constant (Free Care Plan)	0.150	0.148	0.180	0.178	0.199	0.198	0.200
	(0.008)	(0.008)	(0.011)	(0.011)	(0.016)	(0.016)	(0.027)
25% Coinsurance	-0.005	-0.005	-0.017	-0.018	0.090	0.088	0.065
	(0.012)	(0.012)	(0.014)	(0.014)	(0.033)	(0.034)	(0.064)
Mixed Coinsurance	-0.027	-0.027	-0.042	-0.042	0.033	0.036	0.018
	(0.012)	(0.012)	(0.015)	(0.016)	(0.031)	(0.031)	(0.051)
50% Coinsurance	-0.011	-0.009	-0.028	-0.026	-0.033	-0.035	0.075
	(0.013)	(0.013)	(0.015)	(0.014)	(0.037)	(0.037)	(0.068)
Individual Deductible	-0.015	-0.016	-0.025	-0.027	0.072	0.077	0.069
	(0.010)	(0.010)	(0.013)	(0.013)	(0.027)	(0.027)	(0.043)
95% Coinsurance	-0.010	-0.010	-0.019	-0.019	0.020	0.022	0.032
	(0.011)	(0.011)	(0.014)	(0.014)	(0.026)	(0.026)	(0.043)
Some Pain		-0.044		-0.090		0.049	0.009
		(0.022)		(0.035)		(0.028)	(0.043)
A Little Pain		-0.059		-0.108		0.080	0.041
		(0.021)		(0.034)		(0.027)	(0.045)
No Pain at All		-0.059		-0.106		0.100	0.068
		(0.021)		(0.034)		(0.030)	(0.052)
Fair Health		0.029		0.054		-0.023	0.008
		(0.030)		(0.048)		(0.033)	(0.060)
Good Health		0.009		0.020		-0.029	-0.024
		(0.028)		(0.044)		(0.032)	(0.061)
Excellent Health		-0.001		0.007		-0.034	-0.016
		(0.028)		(0.045)		(0.036)	(0.072)
Adjusted R^2	0.04	0.04	0.04	0.04	0.03	0.04	0.30
Prescriber FE	No	No	No	No	No	No	Yes
# Families	2370	2370	2370	2370	1155	1155	1155
# Individuals	4009	4009	4009	4009	1365	1365	1365
# Individual-Years	8081	8081	8081	8081	1956	1956	1956

Notes: The reported coefficients are from ordinary least squares regressions and indicate the effect of the various plans on the outcome given in the column relative to the free-care plan (whose mean is given by the constant term). The outcomes are: the share with any opioid painkiller prescription (columns 1 and 2), number of opioid painkiller prescriptions per visit (columns 3 and 4), and the share of unfilled opioid prescriptions (columns 5 to 7). Because assignment to plans was random only conditional on site and start month (Newhouse et al., 1993), all regressions include site by start month dummy variables, as well as year fixed effects. Regressions also include a dummy variable for women and a dummy variable for individuals under the age of 18. Regressions in columns 2, 4, and 6 add de-meaned dummies for self-reported pain and health at the baseline survey; column 7 further adds prescriber fixed effects. All spending variables are inflation-adjusted to 2019 dollars (adjusted using the CPI-U). Site by start month and year dummy variables are de-meaned so that the coefficients reflect estimates for adult males at the "average" site-month-year mix. Standard errors, clustered on family, are reported in parentheses below the coefficients.

Conditional on a pain-related visit, the physician may be more likely to write an opioid prescription when patients face a more generous insurance plan. This would suggest higher prescription rates per visit for patients in free care. However, any tendency to do so may be offset by the additional visits on the free-care plan that are for less serious reasons, diminishing the likelihood of receiving a prescription. The overall impact of plan generosity on prescription rates is a priori unknown due to the conflicting effects of prescription likelihood and the selection on health and pain across plans.

In columns (1) to (4) of Table 5, we provide evidence regarding the response of physicians. For individual-year pairs with at least one pain-related visit, we estimate Equation (2) using two outcomes at the visit level: (1) a dummy variable for any opioid prescription, and (2) number of opioid prescriptions. The estimates in columns (1) and (3) indicate that the aforementioned combined effect is negative. We find a negative correlation between patient cost-sharing and opioid prescriptions, primarily at the intensive margin. In this sense, the first hypothesis (i.e., physicians being less likely to prescribe as generosity declines) seems to dominate, yielding, for example, 2.7%-points (18.0%) lower opioid prescription rates and 4.2%-points (23.3%) fewer opioid prescriptions per visit for patients in the mixed coinsurance plan, relative to full insurance. To control for the selection on health and pain previously mentioned, in columns (2) and (4), we add as covariates demeaned dummy variables for self-reported health and pain measured at baseline. The final estimates suggest that physicians are less likely to prescribe as generosity declines, even after accounting for health and pain.

Conditional on having an opioid prescription, the patient chooses whether to fill it or not. Individuals may be more likely to fill the prescription as coverage increases.[13] Once again, this higher propensity may also be offset by the differential patient selection on health and pain across plans. In addition, some physicians may be more prone to write a prescription, other things equal (see e.g., Barnett, Olenski, and Jena (2017); Eichmeyer and Zhang (2022, 2023)). To provide evidence

[13]This mechanism may be somewhat attenuated for opioids given their potential for addiction and dependence.

regarding the choice of patients to fill a prescription, we estimate Equation (2) using as outcome a dummy variable for unfilled opioid prescription, conditional on having one. The estimates in column (5) of Table 5 suggest that the first channel dominates, in that, for example, patients in the 25 percent coinsurance group are 9.0%-points (45.2%) more likely to have an unfilled opioid prescription relative to full insurance. To account for the potential patient's selection on health and pain and the physician's prescribing propensity, we add as covariates demeaned dummy variables for self-reported health and pain measured at baseline (column (6)) and physician fixed effects (column (7)), respectively.[14] We find that, even though one in five opioid prescriptions to adult males are not filled, this probability does not vary significantly by plan generosity.

In all, we find that the price response of opioid purchases is mostly driven by two main channels: the decision of patients to visit the physician and the decision of physicians to prescribe opioids. We find no evidence linking responses to the share of unfilled prescriptions, once we control for selection based on health, pain, and physicians' characteristics. This last result is consistent with the finding related to all (painkiller and non-painkiller) prescription drugs in Newhouse et al. (1993).

6 Elasticity Estimates

In this section, we transform our treatment effects into estimates of the price elasticity of demand for prescribed opioid painkillers. We report two types of elasticities: *compound* and *simple*. The *compound* elasticity measures the responsiveness of opioid purchases to changes in the prices of both physician visits and drugs. It is computed using our treatment effects from Table 2. The *compound* elasticity is the policy-relevant input to study the effects of making health care cheaper across the board, such as *Medicare for All*. The *simple* elasticity reflects the responsiveness of opioid purchases to changes in the price of drugs only. This elasticity is derived from our treatment effects in Table 4. The *simple* elasticity informs policies focusing solely on drug pricing mechanisms, such as the introduction of a national opioid tax.

[14] Note that more than one physician fixed effect can be turned on simultaneously for a given individual-year pair.

Usually, the price elasticity of demand is calculated as the percent change in quantity divided by the percent change in price. In our context, percent changes in prices are not well defined when the reference price is zero (i.e., the free-care plan has a zero coinsurance rate). Since free care is the largest plan in the RAND HIE, we instead use pairwise arc elasticities with respect to the coinsurance rate, which is standard when using data from the RAND HIE (see e.g., Keeler and Rolph (1988), AEF). Pairwise arc elasticities are defined as the change in quantity as a percentage of the average quantity, divided by the change in coinsurance rate as a percentage of the average coinsurance rate. Using the results from Table 2, we calculate pairwise *compound* arc elasticities for each health insurance plan with respect to free care. Our primary measure of *compound* elasticity is then computed as a sample-size weighted average of all pairwise arc elasticities. Our primary measure of *simple* elasticity is computed analogously, employing estimates from Table 4.

The *compound* elasticities of opioid painkiller purchases are displayed in Table 6. We uncover statistically significant negative pairwise elasticities with respect to free care. At the extensive margin, we estimate average elasticities of -0.190 and -0.285 for any opioid and any high-dose opioid painkiller purchase in a given year, respectively. At the intensive margin, we estimate average elasticities of -0.307 for annual spending, -0.277 for annual days of supply, -0.242 for annual MME, and -0.333 for annual number of prescriptions filled, which are all precisely estimated.

The *simple* elasticities of opioid painkiller purchases are displayed in Table 7. Following the analysis in Section 5, we report estimates with and without controls for self-reported health and pain. We concentrate on estimates with controls, which address endogenous selection into physician visits. We find negative pairwise elasticities with respect to free care that are precisely estimated. Consistent with our point estimates from Tables 2 and 4, *simple* elasticities are smaller than *compound* elasticities, in absolute terms. At the extensive margin, we estimate average elasticities of -0.094 and -0.209 for any opioid and any high-dose opioid painkiller purchase in a given year, respectively. At the intensive margin, we estimate average elasticities of -0.240 for annual spending, -0.205 for annual days of supply, -0.172 for annual MME, and -0.268 for annual number of prescriptions filled, which are all precisely estimated.

Table 6: Arc Elasticities: Opioid Painkillers

	Share with any (1)	Share with any high-dose (2)	Spending in $ (3)	Annual Days of Supply (4)	Annual MME (5)	Number of Rx purchased (6)
25 vs FC	-0.167	-0.349	-0.376	-0.407	-0.373	-0.348
	(0.036)	(0.074)	(0.067)	(0.085)	(0.091)	(0.059)
Mixed vs FC	-0.136	-0.101	-0.170	-0.133	-0.044	-0.262
	(0.043)	(0.081)	(0.117)	(0.123)	(0.138)	(0.071)
50 vs FC	-0.209	-0.514	-0.385	-0.339	-0.420	-0.357
	(0.046)	(0.103)	(0.071)	(0.111)	(0.126)	(0.072)
ID vs FC	-0.198	-0.196	-0.261	-0.226	-0.181	-0.339
	(0.032)	(0.066)	(0.086)	(0.094)	(0.096)	(0.053)
95 vs FC	-0.231	-0.292	-0.343	-0.285	-0.211	-0.352
	(0.034)	(0.067)	(0.078)	(0.109)	(0.118)	(0.061)
Weighted Average	-0.190	-0.285	-0.307	-0.277	-0.242	-0.333
	(0.023)	(0.049)	(0.054)	(0.058)	(0.065)	(0.041)
Observations	20004	20004	20004	20004	20004	20004

Notes: The reported coefficients are pairwise arc elasticities for each health insurance plan with respect to free care, which are defined as the change in a given outcome as a percentage of the average outcome, divided by the change in coinsurance rate as a percentage of the average coinsurance rate. Arc elasticities are calculated using the estimates from Table 2. The last row reports the sample-size weighted average of all five arc elasticities. The outcomes are: (1) a dummy variable for annual purchase of opioid painkillers, (2) a dummy variable for annual purchase of high-dose opioid painkillers, (3) annual spending on opioid painkillers, (4) annual days of supply of opioid painkillers, (5) annual MME for opioid painkillers, and (6) annual number of opioid prescriptions purchased. Standard errors, clustered on family, are reported in parentheses below the coefficients.

7 Introduction of A National Opioid Tax

In this section, we use our *simple* elasticity estimates to evaluate the effects of introducing a national opioid tax. The US Congress is currently debating a national opioid tax (Senate Bill 1723, 2021), which amounts to one cent per MME of the sold opioid (Harris, Mandell, and Gross, 2022). One key parameter for the proposed exercise is the change in the equilibrium price of prescribed opioids following the introduction of the tax. The pass-through rate of taxes to consumer prices is a function of the relative size of the supply and demand elasticities (Harberger, 1962) and is given by $\frac{\eta^s}{\eta^s-\eta^d}$, where η^s and η^d denote the price elasticity of supply and demand of opioid painkillers, respectively. Given the absence of estimates regarding the price elasticity of supply for opioid painkillers, we

Table 7: Arc Elasticities Conditional on Physician Visit: Opioid Painkillers

	Share with Any		Share with Any High-Dose		Spending in $		Annual Days of Supply		Annual MME		Number of Rx Purchased	
	Baseline (1)	Pain and Health Dummies (2)	Baseline (3)	Pain and Health Dummies (4)	Baseline (5)	Pain and Health Dummies (6)	Baseline (7)	Pain and Health Dummies (8)	Baseline (9)	Pain and Health Dummies (10)	Baseline (11)	Pain and Health Dummies (12)
25 vs FC	-0.089	-0.092	-0.312	-0.308	-0.339	-0.351	-0.384	-0.399	-0.354	-0.353	-0.298	-0.305
	(0.033)	(0.033)	(0.086)	(0.086)	(0.081)	(0.098)	(0.109)	(0.132)	(0.114)	(0.131)	(0.066)	(0.071)
Mixed vs FC	-0.094	-0.100	-0.069	-0.066	-0.132	-0.126	-0.097	-0.090	-0.011	0.002	-0.239	-0.250
	(0.038)	(0.039)	(0.086)	(0.090)	(0.136)	(0.143)	(0.137)	(0.146)	(0.141)	(0.150)	(0.082)	(0.089)
50 vs FC	-0.095	-0.083	-0.449	-0.427	-0.280	-0.259	-0.216	-0.186	-0.315	-0.293	-0.244	-0.221
	(0.042)	(0.042)	(0.120)	(0.128)	(0.074)	(0.079)	(0.119)	(0.119)	(0.140)	(0.143)	(0.069)	(0.069)
ID vs FC	-0.102	-0.113	-0.098	-0.121	-0.172	-0.235	-0.135	-0.199	-0.097	-0.161	-0.267	-0.319
	(0.031)	(0.031)	(0.073)	(0.076)	(0.104)	(0.113)	(0.113)	(0.123)	(0.107)	(0.118)	(0.064)	(0.070)
95 vs FC	-0.078	-0.080	-0.154	-0.159	-0.213	-0.229	-0.145	-0.153	-0.072	-0.071	-0.222	-0.233
	(0.035)	(0.034)	(0.076)	(0.076)	(0.092)	(0.093)	(0.129)	(0.132)	(0.137)	(0.140)	(0.074)	(0.074)
Weighted Average	-0.091	-0.094	-0.208	-0.209	-0.225	-0.240	-0.192	-0.205	-0.163	-0.172	-0.254	-0.268
	(0.021)	(0.021)	(0.050)	(0.050)	(0.063)	(0.067)	(0.066)	(0.070)	(0.070)	(0.073)	(0.045)	(0.048)
Observations	8081	8081	8081	8081	8081	8081	8081	8081	8081	8081	8081	8081

Notes: The reported coefficients are pairwise arc elasticities for each health insurance plan with respect to free care, which are defined as the change in a given outcome as a percentage of the average outcome, divided by the change in coinsurance rate as a percentage of the average coinsurance rate. Arc elasticities are calculated using the estimates in columns 4 to 15 from Table 4, which are based on a subsample of individuals who visited a physician on a given year. The last row reports the sample-size weighted average of all five arc elasticities. The outcomes are: (1) a dummy variable for annual purchase of opioid painkillers, (2) a dummy variable for annual purchase of high-dose opioid painkillers, (3) annual spending on opioid painkillers, (4) annual days of supply of opioid painkillers, (5) annual MME for opioid painkillers, and (6) annual number of opioid prescriptions purchased. Elasticities in even columns are based on regressions that add de-meaned dummies for self-reported pain and health at the baseline survey. Standard errors, clustered on family, are reported in parentheses below the coefficients.

evaluate three potential values for η^s: 1 (scenario 1), 0.1 (scenario 2), and 100 (scenario 3). All calculations are reported in Appendix OA.7.

Since most changes that occurred in the mid-90s were geared toward diminishing the stigma surrounding opioid prescription and consumption for the treatment of chronic, non-cancer pain, one would expect the level of opioid addiction in the population to be certainly higher now than during our study period. In Appendix OA.6, we leverage pre-randomization variables to show that our price elasticities mostly reflect the responsiveness of opioid-naïve patients. Therefore, we compute η^d by taking a weighted average of price elasticities for opioid-naïve patients and opioid-non-naïve patients. For opioid-naïve patients, we use the *simple* elasticity estimate of -0.172 for MME per year (Table 7). Given opioids' addictive nature, we assume a zero price elasticity of demand for non-naïve patients. We utilize the MEPS 2021 dataset to compute the share of opioid-naïve individuals between the ages of 18 and 64, providing a result of 93.7%.[15] The implied value of η^d is -0.161. Under scenario 1, the fraction of the tax that is passed on to consumers is 86.1%.[16]

The average opioid painkiller price per MME is calculated using the 2021 MEPS data and is equal to \$0.073. Given the price and pass-through rate under scenario 1, the introduction of the national opioid tax of one cent per MME implies an 11.8% increase in the price per MME, and a 2.0% decrease in the annual MMEs consumed by opioid-naïve patients.[17] Following our assumption of zero price elasticity of demand, non-naïve patients do not respond to the introduction of the tax.

To put these numbers in perspective, we proceed to calculate expected revenue per capita from the tax and compare it against the per capita societal cost of substance abuse treatment facilities Florence et al. (2016). To calculate expected revenue per capita, we compute the average annual MME quantity purchased by naïve and non-naïve individuals in the 2021 MEPS data, which

[15]See Section OA.4.2 for details about how we identify opioid-naïve individuals in MEPS 2021.

[16]The pass-through rate of taxes to consumer prices is 38.3% and 99.8% under scenarios 2 and 3, respectively.

[17]The decrease in the annual MMEs consumed by opioid-naïve patients is 0.9% and 2.4% under scenarios 2 and 3, respectively.

amounts to 23.13 and 2242.9, respectively. The average expected annual revenue per capita from the tax adds up to 1.63 dollars, representing 16.6% of the per capita societal cost of substance abuse treatment facilities.[18]

The implementation of a national opioid tax can therefore serve as a viable funding mechanism for bolstering resources allocated to substance abuse treatment facilities. Notably, our estimates represent a lower bound, since we assume non-naïve users have a zero price elasticity of demand for prescribed opioid painkillers. Moreover, we estimate that only 54.43% of the tax collections would be paid by parties other than the federal, state, or local government.

8 Discussion and Concluding Remarks

Overdose deaths involving opioids continue to be the dominant driver of the current drug overdose epidemic, which has devastated communities across the country. Several policies have been implemented to address this opioid epidemic. Recently, policy makers and private insurers have been considering price-driven interventions targeting the out-of-pocket price patients pay for prescribed opioid painkillers, such as the introduction of a national opioid tax. A key input to the design, targeting, and eventual success of these price-driven policies is the responsiveness of patients to prescription opioid prices. Nonetheless, estimates of this elasticity for a general non-elderly population are not currently available mainly due to limited exogenous price variation required for their estimation.

In this paper, we attempt to fill this gap through a retrospective analysis using data from the RAND Health Insurance Experiment. At the extensive margin, we estimate a price elasticity of -0.19 for any opioid painkiller purchase in a given year. At the intensive margin, we estimate elasticities of -0.31 for annual spending, -0.28 for annual days of supply, and -0.24 for MME per year. These elasticities are the relevant inputs to evaluate policies that simultaneously change the price across

[18]The share remains practically unchanged under scenarios 2 and 3 for the elasticity of supply.

all healthcare services, such as *Medicare for All*. However, the proposed national opioid tax would only affect the price of prescribed opioids. To evaluate this latter policy, we also estimate *simple* price elasticities isolating the change in the price of prescription medications from other healthcare prices. Since we are the first to provide opioid elasticities for a general population, our estimates are not directly comparable to the very few studies on this topic. For instance, focusing on an older and relatively sicker population (i.e., Medicare beneficiaries just below or above the Medicare Part D "donut hole"), Einav, Finkelstein, and Polyakova (2018) estimate an opioid elasticity of -0.04 at the extensive margin. Our comparable elasticity estimate of -0.09 is more than two times larger, which undoubtedly reflects our younger (i.e., less than 65 years old) and healthier (i.e., not negatively selected in terms of health) population.

The studied margins of response, like expenditure on prescribed opioids and number of opioid prescriptions filled, reflect not only the likelihood that a patient visited a physician but also the likelihood that a physician wrote a prescription, as well as the probability that the patient filled the prescription. We infrequently get to observe the patients' and physicians' decisions in each of these instances using the information contained in typical health insurance claims. By exploiting unique data from the RAND HIE, we are able to decompose the price response into three mechanisms: the portion driven by additional physician visits, the portion driven by additional prescriptions upon visiting the physician, and the portion driven by the share of prescriptions filled. We find that the increase in opioid painkiller purchases in more generous insurance plans is explained by a combination of patient behavior, via additional physician visits (first mechanism), and physician behavior, primarily through an increase in opioid prescription rates per visit (second mechanism). Our findings do not indicate any significant impact on response stemming from the proportion of unfilled prescriptions (third mechanism).

We further utilize our *simple* elasticity estimates to assess the potential effects of a national opioid tax of one cent per MME, currently under debate in the US Congress. While we estimate the proposed tax would only have a modest impact on reducing prescription opioid use, it will play a significant role in generating revenue to expand access to substance abuse treatment. Our counterfactual analysis indicates that revenue from the tax could represent 16.6% of the per capita societal cost of substance abuse treatment facilities.

Our study is subject to some limitations. It draws upon data from the 1970s, a time when opioid addiction rates were likely lower, and the stigma surrounding opioid prescription was higher. To address these concerns, we offer novel evidence showcasing the extensive utilization of opioids during our sample period, emphasizing that our estimated price elasticities primarily pertain to the opioid-naïve subpopulation. At the same time, these limitations can also be viewed as strengths of our work. Drawing upon data from the 1970s provides new perspectives into the origins of the opioid epidemic, commonly dated to the mid-1990s (Cutler and Glaeser, 2021). This approach can help us understand how we got here and offers insights into strategies for prevention, mitigation, and enhanced responsiveness to future crises involving addictive substances.

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Online Appendix

OA.1 A Review of Price-Based Solutions Targeting the Opioid Epidemic

In this section, we present a brief review of recent supply-side policies designed to address the opioid epidemic by targeting prices. Our focus is on policies that have undergone implementation or are currently under discussion, since year 2010. Although our primary emphasis is on the United States, where the opioid epidemic is a critical concern, we also extend our examination to include relevant policies in Canada.

Restrictions and re-design of prescription drug plan formularies

- Starting from 2019, all Medicare Part D plans limited opioid coverage for new users with acute pain to seven days (Soni, 2019).
- Private insurance companies are also adjusting drug plan formularies. For example, as of January 1, 2018, the major health insurer Cigna Corp and Florida Blue (Florida's largest health insurance company) stopped covering OxyContin. This move was also followed by BlueCross BlueShield of Tennessee and Alabama as of January 1, 2019 (Kaiser Family Foundation, 2018; Beasley, 2017; Blue Cross Blue Shield, 2018).
- In 2012, seven Canadian provinces delisted OxyContin from their drug formularies. Afterwards, in most provinces, a reformulated tamper-resistant form of Oxycodone was only available to cancer and palliative care patients (Karamouzian et al., 2022).

Taxes on Opioids and Registration Fees

- Since 2015, lawmakers in various US states have endeavored to implement taxes on opioids, ranging from taxing a percentage of manufacturers' annual gross receipts, to taxes based on dosage and potency (Kwon, 2020).
- Five US states successfully implemented opioid taxes or introduced registration and licensing fees.

- In Delaware, the law imposed 1.0 and 0.25 cents per MME for brand and generic opioids, respectively (Del Code Tit. 16 §4804B(b)(1)-(2)).
- In Maine, the law imposes a registration fee of 55 thousand dollars and an annual product registration fee of 250 thousand dollar for opioid manufacturers (Me Rev Stat. Tit. 32, §13724,13800-C).
- In New York, the law establishes a tax of 0.25 or 1.5 cents per MME, depending on the wholesale acquisition cost (NY Pub Health Law §3323).
- In Minnesota, the law imposes several fees on opioid manufacturers and wholesalers, including a 250 thousand dollar registration fee and a 250 thousand dollar per year licensing fee for manufacturers with sales above 2 million units (Minn Stat. §256.043).
- In Rhode Island, the law establishes a registration fee based on the manufacturers' market share (RI Gen Laws. §21-28.10-1 to 13).

- The US Congress is currently debating a national opioid tax (Senate Bill 1723) (Harris, Mandell, and Gross, 2022). The Senate Bill enforces an excise tax on the sale of every active opioid, levied on the manufacturer, producer, or importer of the opioid. This tax amounts to one cent per MME of the sold opioid.

OA.2 Opioid Use in 20th Century US - Historical Context

In this section, we present a brief examination of the historical context surrounding opioid utilization in the United States in the twentieth century. Two disruptive inventions set the tone of the first half of the nineteenth century: morphine was distilled from opium for the first time in 1804, and the hypodermic syringe was invented in 1853. The context in the late nineteenth century was characterized by the wide availability of morphine and opium, marketed aggressively not only for adults but also to pacify children. In 1898, the German drug company Bayer invented diacetylmorphine, naming it heroin, and commercializing it as a cough, cold, and pain remedy. Perhaps unsurprisingly, at the beginning of the twentieth century, there was a narcotic problem of considerable dimensions (Quinones, 2015), defined as the iatrogenic wave of opium and morphine addiction by Macy (2018) and as the first great American opioid epidemic by Case and Deaton (2020). This scenario prompted increased intervention by the federal government, culminating in the introduction of the Harrison Narcotics Tax Act in 1914. This landmark legislation aimed to restrict the distribution and sales of narcotics, signifying the first comprehensive legal framework to regulate whole classes of drugs.

The law managed to curb the illicit use of opioids for some time. However, in the early 1970s, the administration of President Nixon acknowledged the resurgence of drug abuse, with a particular emphasis on heroin addiction, as a significant public health concern. This realization was largely prompted by the observed prevalence of substance abuse among military personnel deployed in Vietnam (Case and Deaton, 2020). Concurrently, fentanyl had already been introduced in 1968 for general anesthesia, establishing itself as a staple in anesthesia practice for the ensuing five decades. The Controlled Substances Act was established in 1970 to regulate the manufacture, importation, possession, use, and distribution of certain substances. The legislation created five schedules, with the Drug Enforcement Administration (DEA) and the Food Drug Administration (FDA) in charge of determining which substances are included in each schedule.

The surge in the population of disabled veterans during the period spanning the 1940s to the

1960s prompted a heightened emphasis on pain and its treatment. The inception of pain as a distinct field within the medical domain took shape in the 1970s, reflected in the establishment of the Pain Journal and the International Association for the Study of Pain. Perceptions of under treatment of pain surged, partly influenced by the introduction of the gate control theory of pain (Melzack and Wall, 1965) and the McGill Pain Questionaire (Melzack, 1975), widely employed for the multidimensional assessment of pain.

Early in 1980, the New England Journal of Medicine published a letter authored by Janer Porter and Hershel Jick, reporting the findings of a study that scrutinized medical records of 11,882 opioid-naïve patients who had undergone hospitalization and received minimal opioid doses. The study revealed that merely four of those patients had developed addiction (Porter and Jick, 1980). Subsequently, this letter gained widespread citation, often invoked to assert the non-addictive nature of opioids. Later In 1984, Purdue released MS Contin, a timed-release morphine painkiller marketed to cancer patients. In 1986, the Pain Journal published a study reviewing the cases of thirty-eight cancer patients with chronic pain treated with opioids for at least four years. The study found that only two patients became addicted, both with a history of prior drug abuse, suggesting that opioids were not inherently addictive (Portenoy and Foley, 1986). The concept of pain continued to evolve throughout the 1980s, reaching a pivotal moment with the official recognition of pain as the fifth vital sign by the American Pain Society in 1995.

OA.3 Details about the RAND HIE data

In this section, we provide further details about the RAND HIE.[19] The experiment excluded individuals age 62 and over at enrollment, as well as those eligible for Medicaid; those with family incomes greater than \$25,000 (in 1973 dollars); those who were institutionalized; those in the military and their dependents; and veterans with service-connected disabilities. To further limit participants' financial exposure, the MDE was capped at \$1,000 in 1973 dollars, corresponding to \$6,000 in 2020 dollars, based on the US Consumer Price Index (CPI-U).

OA.3.1 Self-reported measures of pain

The RAND HIE collected information about the participants' pain levels at three different instances: baseline, enrollment, and exit. Some of the questions differ between the Dayton and non-Dayton questionnaires, partly because they were administered to the former two years earlier than to the latter. The measure of self-reported pain level at baseline comes from the Baseline Questionnaire or the EVF new person supplement (for persons not present at baseline).

OA.3.2 Sample construction

Table OA.3.1: Analysis Sample Derivation

Dropping Condition	Remaining Observations (Individual-Year)	Remaining Individuals
Initial sample		26,148
Drop never HIE-insured or control group	27,458	7,438
Drop incomplete years of participation	26,636	7,216
Drop years after switching plan	25,939	7,029
Drop if missing age	25,936	7,028
Drop HMO experimental group	20,004	5,922
Analysis Sample 1	20,004	5,922

[19]The RAND HIE data can be downloaded from https://www.icpsr.umich.edu/web/ICPSR/studies/6439.

OA.3.3 Steps to identify painkillers in the RAND HIE

This section describes how we identify opioid and non-opioid painkillers in the RAND HIE data.

Step 1: We start by identifying painkillers. To that end, we use the drug therapeutic codes reported in the claims files. The therapeutic codes in the HIE were taken from the AMA Drug Evaluations, 1973. We define painkillers as those drug purchases or prescriptions falling under three therapeutic codes: (a) strong analgesics, (b) mild analgesics, and (c) anti-rheumatic agents. It is important to stress that drug therapeutic codes vary within generic drug codes. This is particularly important since some drugs that are usually used to treat pain may be serving a different purpose (e.g., antitussive).[20] The full list of drug therapeutic codes used in the HIE claims files can be found in Codebook 211 of the publicly available data.[21] Out of 53,320 drug prescriptions/suggestions and 108,458 drug purchases, 6,509 (12.21%) and 13,435 (12.39%) records correspond to painkillers, respectively.

Step 2: The next step is to identify opioid painkillers. To this end, we use data from the CDC Oral MME Conversion file, containing all opioid analgesics that are normally prescribed in outpatient settings, dispensed by retail pharmacies, and controlled by the Drug Enforcement Administration (DEA).[22,23,24] We match the generic drugs listed in the CDC file to the generic components listed in the HIE claims files. For each drug purchase or prescription, we have detailed

[20]For instance, codeine phosphate, typically used to treat pain, is prescribed as an antitussive agent in 28% of RAND HIE pharmacy claims involving it.

[21]The share of missing drug therapeutic codes is less than 0.1% for the file containing drug purchases and 0.3% for the file containing drug prescriptions/suggestions.

[22]The CDC stopped updating this file and it is no longer available online. For details, visit https://www.cdc.gov/opioids/data-resources/index.html.

[23]The CDC Oral MME Conversion file excludes most opioid medications that are normally used in inpatient settings (e.g., medications administered by an injection route), among others. We address this issue in Step 3.

[24]An alternative is to use the NDC Directory to identify opioids. For each product listed in the NDC Directory, it identifies the underlying generic drugs and their pharma-class category (e.g., full opioid agonist), which can be later

information on up to ten generic drug components.[25]

Step 3: Since the CDC Opioid NDC and Oral MME Conversion File excludes most opioid medications that are normally used in inpatient settings and some injectable opioids, we browse through all the observations identified as non-opioids and identify one additional generic component that was miss-classified as non-opioid: pentazocine lactate.

matched to the generic codes in the HIE claims files.

[25] The share of missing all generic components is 0.02% in prescriptions/suggestions of painkillers and 0.04% in purchases of painkillers.

OA.4 Comparison with Opioid Drugs in the 2021 MEPS

This section describes the main steps for cleaning the MEPS data files and compares the opioid painkiller drugs purchased in the 2021 MEPS data versus the 1974-1982 RAND HIE data.

OA.4.1 Steps to identify opioid painkillers in the 2021 MEPS data

1. We use the 2021 MEPS Full-Year Consolidated Data File (HC-233) and the 2021 Prescribed Medicines File (HC-229A). The MEPS Full-Year Consolidated Data File for 2021 includes 28,336 individuals. The MEPS Prescribed Medicines File for 2021 contains 303,394 observations from 16,534 unique individuals.

2. To make the MEPS and the RAND HIE sample comparable in age, we drop 6,541 unique individuals aged 65 or more and keep 21,795 unique individuals younger than 65.

3. We classify as painkillers those observations with Therapeutic Class equal to "analgesics", "miscellaneous analgesics", or "analgesic combinations." We identify 11,292 painkiller purchases, which represent 6.55% of all prescribed medicines purchased weighted for national representation.

4. Following (Moriya and Fang, 2023), we classify as opioid painkillers those observations with Therapeutic Sub-Class equal to "narcotic analgesics" or "narcotic analgesic combinations" in the Multum Lexicon database from Cerner Multum, Inc. We exclude respiratory agents, antitussives, and drugs commonly used in medication-assisted treatment, as these opioids are not primarily indicated for pain management. We identify 5,249 opioid painkiller purchases, which represent 45.22% of all painkillers weighted for national representation.

5. In 2021, a total of 13.7 million individuals under age 65, or 5% of this population, filled at least one opioid prescription. During the same period, 3.8 million or 1.4% of individuals under age 65 obtained four or more opioid prescription fills or refills.

6. Of the 5,249 opioid painkiller observations, 890 are missing both NDCs and drug names. We rename the opioid drug names to "Unknown Opioids" for these observations.

7. We incorporate additional information on MME units by merging this file with the CDC Oral MME file from 2020 using the NDC identifiers. The CDC file successfully matches 96.26% of the opioid painkiller observations with non-missing NDCs.

Table OA.4.1 presents a comparison between the opioid drugs purchased in the 2021 MEPS versus the 1974-1982 RAND HIE data. The columns show the results using alternative measures to weight the observations. We find that between 84% and 93% of the opioid painkiller drugs in the MEPS data were also in the RAND HIE data.

Table OA.4.1: Comparison of Opioid Painkiller Drugs in MEPS versus RAND HIE Data

Opioids in MEPS 2021	Raw Frequency (1)	Percent MEPS adjusted (2)	Percent MEPS adjusted and MME weighted (3)	Percent MEPS adjusted and days-supplied weighted (4)
Only in MEPS	737	16.45	7.12	12.80
Both in MEPS and RAND HIE	3622	83.55	92.88	87.20
Total	4359	100.00	100.00	100.00

Notes: The reported statistics are derived from the MEPS Prescribed Medicines File for 2021. We restrict the sample to opioid painkillers with non-missing drug names purchased by individuals aged 64 or less. We classify these opioid painkiller purchases into two mutually exclusive groups: (a) opioid drugs present only in MEPS and (b) opioid drugs present both in MEPS and RAND HIE. Drugs in group (a) include only Tramadol, approved by the US Federal Drug Administration in 1995. Drugs in group (b) include codeine, hydrocodone, hydromorphone, morphine, and oxycodone. Column (1) shows the raw frequencies of each group. Column (2) shows the group percentages adjusted by MEPS survey weights for national representation. Column (3) displays percentages weighted by Morphine Milligram Equivalents (MMEs) and adjusted by MEPS survey weights for national representation. Column (4) displays percentages weighted by days supplied and adjusted by MEPS survey weights for national representation.

OA.4.2 Steps to identify opioid-naïve users in the 2021 MEPS data

1. We use the 2020 MEPS Full-Year Consolidated Data File (HC-224) and the 2020 Prescribed Medicines File (HC-220A). The MEPS Full-Year Consolidated Data File for 2020 includes 27,805 individuals. The MEPS Prescribed Medicines File for 2020 contains 279,755 observations from 15,743 unique individuals.

2. Following the steps from section OA.4.1, we identify 25,874 survey participants who did not fill any opioid prescription in 2020, which represent 93.6 percent of the US population in 2020.

We classify these individuals as opioid-naïve users for the purpose of analysing their opioid consumption in 2021.

3. We merge the 2021 MEPS data files with their 2020 counterpart to assess the opioid utilization patterns of opioid-naïve users. Of the 16,011 survey participants between the ages of 18 and 64 in 2021, 11,088 were also present in 2020.

4. Of the survey participants present in both 2020 and 2021, 10,310 are classified as opioid-naïve users. Therefore, we calculate that the 2021 US national share of opioid-naïve users among the population between 18 and 64 years old is 93.7 percent.

OA.5 Analyses Including Opioid Painkillers Prescribed by Dentists

In this section, we replicate our main analysis using different inclusion criteria for observations of the analysis sample. In particular, we include opioid painkiller purchases prescribed by both dentists and non-dentists.

Table OA.5.1 describes the summary statistics by plan group for this sample inclusion criteria. Each of the six columns presents raw means and standard deviations at the person-year level by plan. Each row presents a measure related to opioid painkiller utilization. On the extensive margin, we find that individuals with the highest cost-sharing are 7.2%-points (47.7%) less likely to purchase opioid painkillers relative to individuals with full insurance. On the intensive margin, the estimates indicate that individuals in the least generous plan spend $6.0 (65.7%) less on opioid painkillers, have 1.26 (56.0%) fewer annual days of supply, and consume 48.5 (45.6%) fewer MME per year, compared to individuals with full insurance.

Table OA.5.2 reports the treatment effects when all opioid painkiller purchases are considered. Overall, our main conclusions remain robust after including opioid purchases prescribed by dentists.

Table OA.5.1: Summary Statistics - Opioid Painkillers Prescribed by Non-Dentists and Dentists

	(1) Free Care		(2) 25% Coinsurance		(3) Mixed Coinsurance		(4) 50% Coinsurance		(5) Individual Deductible		(6) 95% Coinsurance	
	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd
Opioid Painkillers:												
1. Any opioid Rx	0.166	(0.37)	0.124	(0.33)	0.129	(0.33)	0.102	(0.30)	0.114	(0.32)	0.093	(0.29)
2. Any opioid purchase	0.151	(0.36)	0.100	(0.30)	0.104	(0.31)	0.088	(0.28)	0.093	(0.29)	0.079	(0.27)
3. Any high-dose opioid purchase	0.031	(0.17)	0.011	(0.10)	0.022	(0.15)	0.009	(0.10)	0.018	(0.13)	0.014	(0.12)
4. Opioid spending (2019 $)	9.096	(65.57)	2.991	(15.38)	5.142	(56.90)	2.415	(12.52)	4.367	(39.48)	3.122	(22.16)
5. Annual days of supply	2.251	(16.62)	0.745	(4.34)	1.424	(15.35)	0.866	(7.07)	1.182	(11.87)	0.990	(9.80)
6. Annual MME	106.460	(828.80)	34.045	(272.32)	87.294	(991.95)	30.776	(316.39)	60.996	(587.10)	57.925	(631.50)
7. MME/day $\mid$ opioid purchase	50.838	(36.30)	46.625	(31.00)	51.268	(40.14)	47.200	(37.72)	52.669	(36.82)	54.861	(37.96)
8. Share unfilled Rx $\mid$ Rx	0.173	(0.31)	0.257	(0.40)	0.252	(0.38)	0.186	(0.34)	0.258	(0.38)	0.220	(0.36)
# Families	1040		327		285		208		668		572	
# Individuals	1964		663		507		393		1261		1134	
# Individual-years	6724		2333		1704		1417		4087		3739	

Notes: This table reports summary statistics from our RAND HIE analysis sample, by health insurance plan. Each of the six columns presents raw means and standard deviations at the individual-year level by plan. Standard deviations are reported in parentheses beside the mean.

Table OA.5.2: Plans' Effects on Opioid Painkiller Purchases Prescribed by Non-Dentists and Dentists

	Opioid purchase					
	Share with Any (1)	Share with Any High-Dose (2)	Spending in $ (3)	Annual Days of Supply (4)	Annual MME (5)	Number of Rx Purchased (6)
Constant (Free Care)	0.189	0.036	11.514	2.858	144.533	0.549
	(0.007)	(0.003)	(1.281)	(0.348)	(17.807)	(0.054)
25% Coinsurance	-0.050	-0.019	-5.985	-1.594	-77.316	-0.263
	(0.009)	(0.003)	(1.129)	(0.316)	(16.837)	(0.050)
Mixed Coinsurance	-0.047	-0.009	-3.574	-0.730	-15.881	-0.226
	(0.012)	(0.005)	(1.855)	(0.515)	(33.005)	(0.055)
50% Coinsurance	-0.064	-0.023	-6.173	-1.432	-84.356	-0.275
	(0.011)	(0.004)	(1.150)	(0.399)	(20.424)	(0.053)
Individual Deductible	-0.061	-0.014	-4.673	-1.058	-45.190	-0.258
	(0.008)	(0.004)	(1.457)	(0.413)	(21.518)	(0.049)
95% Coinsurance	-0.073	-0.017	-5.868	-1.288	-51.920	-0.283
	(0.009)	(0.003)	(1.243)	(0.401)	(23.616)	(0.050)
Adjusted R^2	0.05	0.02	0.02	0.01	0.01	0.03
# Families	3100	3100	3100	3100	3100	3100
# Individuals	5922	5922	5922	5922	5922	5922
# Individual-Years	20004	20004	20004	20004	20004	20004

Notes: The reported coefficients are from ordinary least squares regressions and indicate the effect of the various plans on the outcome given in the column relative to the free-care plan (whose mean is given by the constant term). The outcomes are: (1) a dummy variable for annual purchase of opioid painkillers, (2) a dummy variable for annual purchase of high-dose opioid painkillers, (3) annual spending on opioid painkillers, (4) annual days of supply of opioid painkillers, (5) annual MME for opioid painkillers, and (6) annual number of opioid prescriptions purchased. All outcomes account for opioid purchases prescribed by dentists and non-dentists. Because assignment to plans was random only conditional on site and start month (Newhouse et al., 1993), all regressions include site-by-start-month dummy variables, as well as year fixed effects. Regressions also include a dummy variable for women and a dummy variable for individuals under the age of 18. All spending variables are inflation-adjusted to 2019 dollars (adjusted using the CPI-U). Site by start month and year dummy variables are de-meaned so that the coefficients reflect estimates for adult males at the "average" site-month-year mix. Standard errors, clustered on family, are reported in parentheses below the coefficients.

OA.6 Analysis for Opioid-Naïve Users

In this section, we leverage pre-randomization variables to examine the price responsiveness of opioid-naïve users. We employ combinations of two variables to identify opioid-naïve users in the sample: a self-reported measure indicating whether the individual has a drinking problem and a self-reported measure of pain, both assessed at baseline. Initially, we define opioid-naïve individuals as those without a drinking problem at the beginning of the experiment. Subsequently, we classify opioid-naïve users as individuals reporting no pain or little pain at the beginning of the experiment. Next, we combine both measures, defining opioid-naïve users as individuals without a drinking problem and reporting no or little pain at the beginning of the experiment.

Tables OA.6.1 and OA.6.2 provide estimates of our preferred measures of *compound* and *simple* elasticities, respectively, for opioid-naïve users, employing the aforementioned definitions. Elasticities for opioid-naïve users are, for the most part, very similar to the baseline estimates. Overall, the results suggest that our baseline price elasticities mostly reflect the responsiveness of opioid-naïve patients.

Table OA.6.1: Arc Elasticities: Opioid Painkillers - Naive Users

	Obs	Share with any (1)	Share with any high-dose (2)	Spending in $ (3)	Annual Days of Supply (4)	Annual MME (5)	Number of Rx purchased (6)
Baseline	20004	-0.190	-0.285	-0.307	-0.277	-0.242	-0.333
		(0.023)	(0.049)	(0.054)	(0.058)	(0.066)	(0.041)
No Drinking Problem	19089	-0.196	-0.298	-0.307	-0.292	-0.290	-0.335
		(0.026)	(0.051)	(0.062)	(0.068)	(0.074)	(0.046)
Little or No Pain	15355	-0.192	-0.262	-0.285	-0.279	-0.302	-0.321
		(0.027)	(0.065)	(0.070)	(0.073)	(0.086)	(0.049)
No Drink & Little Pain	14692	-0.190	-0.254	-0.264	-0.260	-0.291	-0.308
		(0.030)	(0.072)	(0.081)	(0.082)	(0.101)	(0.055)

Notes: The reported coefficients in the first row corresponds to the sample-size weighted average of arc elasticities for each health insurance plan with respect to free care, using the full sample. These estimates are our baseline estimates from Table 6. The following rows report the same sample-size weighted average of arc elasticities for subsamples of opioid-naïve users, using alternative definitions based on pre-experiment variables. In the second row, opioid-naïve users comprise individuals without a drinking problem at the time of the experiment. In the third row, opioid-naïve users comprise individuals reporting no pain or little pain at the time of the experiment. In the fourth row, opioid-naïve users are individuals without a drinking problem, no pain, or little pain at the time of the experiment. The outcomes (in columns) are: (1) a dummy variable for annual purchase of opioid painkillers, (2) a dummy variable for annual purchase of high-dose opioid painkillers, (3) annual spending on opioid painkillers, (4) annual days of supply of opioid painkillers, (5) annual MME for opioid painkillers, and (6) annual number of opioid prescriptions purchased. The number of observations for each regression are reported in the first column. Standard errors, clustered on family, are reported in parentheses below the coefficients.

Table OA.6.2: Arc Elasticities for Opioid Painkillers Conditional on Physician Visit: Naive Users

	Obs	Share with Any		Share with Any High-Dose		Spending in $		Annual Days of Supply		Annual MME		Number of Rx Purchased	
		Baseline (1)	Pain and Health Dummies (2)	Baseline (3)	Pain and Health Dummies (4)	Baseline (5)	Pain and Health Dummies (6)	Baseline (7)	Pain and Health Dummies (8)	Baseline (9)	Pain and Health Dummies (10)	Baseline (11)	Pain and Health Dummies (12)
Baseline	8081	-0.091	-0.094	-0.208	-0.209	-0.225	-0.240	-0.192	-0.205	-0.163	-0.172	-0.254	-0.268
		(0.021)	(0.021)	(0.050)	(0.050)	(0.063)	(0.067)	(0.066)	(0.070)	(0.070)	(0.073)	(0.045)	(0.048)
No Drinking Problem	7678	-0.101	-0.102	-0.224	-0.220	-0.227	-0.236	-0.211	-0.217	-0.219	-0.221	-0.259	-0.267
		(0.023)	(0.023)	(0.058)	(0.059)	(0.072)	(0.076)	(0.079)	(0.081)	(0.088)	(0.091)	(0.050)	(0.053)
Little or No Pain	6019	-0.095	-0.094	-0.183	-0.184	-0.193	-0.192	-0.179	-0.179	-0.213	-0.213	-0.232	-0.232
		(0.024)	(0.024)	(0.065)	(0.067)	(0.081)	(0.079)	(0.084)	(0.083)	(0.102)	(0.103)	(0.053)	(0.054)
No Drink & Little Pain	5749	-0.094	-0.092	-0.172	-0.173	-0.169	-0.165	-0.159	-0.155	-0.201	-0.199	-0.219	-0.215
		(0.025)	(0.024)	(0.070)	(0.073)	(0.081)	(0.077)	(0.084)	(0.082)	(0.101)	(0.102)	(0.051)	(0.050)

Notes: The reported coefficients in the first row corresponds to the sample-size weighted average of arc elasticities for each health insurance plan with respect to free care, using the subsample of individuals with at least one physician visit in a given year. These estimates are our estimates from Table 7. The following rows report the same sample-size weighted average of arc elasticities for subsamples of opioid-naïve users, using alternative definitions based on pre-experiment variables. In the second row, opioid-naïve users comprise individuals without a drinking problem at the time of the experiment. In the third row, opioid-naïve users comprise individuals reporting no pain or little pain at the time of the experiment. In the fourth row, opioid-naïve users are individuals without a drinking problem, no pain, or little pain at the time of the experiment. The outcomes are: a dummy variable for annual purchase of opioid painkillers (columns 1 and 2), a dummy variable for annual purchase of high-dose opioid painkillers (columns 3 and 4), annual spending on opioid painkillers (columns 5 and 6), annual days of supply of opioid painkillers (columns 7 and 8), annual MME for opioid painkillers (columns 9 and 10), and annual number of opioid prescriptions purchased (columns 11 and 12). Elasticities in even columns are based on regressions that add de-meaned dummies for self-reported health and pain (when appropriate) at the baseline survey. The number of observations for each regression are reported in the first column. Standard errors, clustered on family, are reported in parentheses below the coefficients.

OA.7 National Opioid Tax: Details

Table OA.7.1: Parameters Values for Counterfactual Exercise - National Opioid Tax

	Value (1)	Source (2)
Panel (a): Price Elasticity of MME/year		
1. Opioid-naïve users	-0.172	Authors' estimation based on RAND HIE
2. Opioid-addict users	-0.000	Assumption
Panel (b): Opioids Volume and Price		
3. Avg. MME/year opioid-naïve users	23.13	
4. Avg. MME/year opioid-addict users	$2,242.9$	
5. Percent of opioid-naïve users	93.70	Authors' calculations based on MEPS 2021
6. Avg. price per MME (2021 \$)	0.073	
7. Percent by non-public payer	54.43	
Panel (c): Societal Cost of Opioid Abuse		
8. Cost treat. facilities (2013 million \$)	$2,820$	Florence et al. (2016)
9. US population 2013 (millions)	333.3	US Census Bureau
10. CPI 2021 (2013 = 100)	115.8	US Bureau of Labor Statistics
11. Societal cost per capita (2021 \$)	9.80	Authors' calculation
Panel (d): National Opioid Tax		
12. Price elasticity of supply (η^s)	1.00	Assumption: scenario 1
13. Tax per MME (\$)	0.01	Senate Bill 1723 (2021)
14. Price elasticity of demand (η^d)	-0.161	
15. Pass-through to consumers (%)	86.12	
16. Percent change in price	11.80	
17. New price per MME	0.082	
18. %Δ MME/year opioid-naïve users	-2.03	Authors' calculations
19. New avg. MME/year opioid-naïve	22.66	
20. Annual tax revenue per capita	1.625	
21. Percent of societal cost	16.58	

Notes: The percent by non-public payer in row 7 of Panel (b) refers to the proportion of the total opioid expenditures paid by parties other than the federal, state, or local government.