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David Powell
Rosalie Liccardo Pacula
Erin Taylor

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How Increasing Medical Access to Opioids Contributes to the Opioid Epidemic: Evidence from Medicare Part D

David Powell, Rosalie Liccardo Pacula, and Erin Taylor

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ABSTRACT

Drug overdoses involving opioid analgesics have increased dramatically since 1999, representing one of the United States' top public health crises. Opioids have legitimate medical functions, but improving access may increase abuse rates even among those not prescribed the drugs given that opioids are frequently diverted to nonmedical use. We have little evidence about the causal relationship between increased medical access to opioids and spillovers resulting in abuse. We use the introduction of the Medicare Prescription Drug Benefit Program (Part D) as a large and differential shock to the geographic supply of opioids. We compare growth in opioid supply and abuse rates in states with large 65+ population shares to states with smaller elderly population shares with a focus on abuse among the Medicare-ineligible population. Part D increased opioid utilization for the 65+ population, and we show that this increase in utilization led to significant growth in the overall supply of opioids in high elderly share states relative to low elderly share states. This relative expansion in opioid supply resulted in an escalation in opioid-related substance abuse treatment admissions and opioid-related mortality among the Medicare-ineligible population, implying meaningful spillovers to individuals who did not experience any change in prescription drug benefits. The evidence suggests that increased opioid supply is associated with economically-important levels of diversion for nonmedical purposes. Our estimates imply that a 10% increase in medical opioid distribution leads to a 7.4% increase in opioid-related deaths and a 14.1% increase in substance abuse treatment admission rates for the Medicare-ineligible population.

David Powell
RAND Corporation
1776 Main Street
P.O. Box 2138
Santa Monica, CA 90407
powelld@nber.org

Rosalie Liccardo Pacula
RAND Corporation
1776 Main Street
P.O. Box 2138
Santa Monica, CA 90407-2138
and NBER
pacula@rand.org

Erin Taylor
RAND Corporation
1776 Main St
Santa Monica, CA 90407-2138
Erin_Taylor@rand.org

1. Introduction

Drug overdose deaths have risen steadily for the past two decades, and by 2009 they became the leading cause of preventable death from injuries in the United States, exceeding deaths from motor vehicle accidents (CDC, 2012; CDC, 2015). Deaths from prescription opioids have been the dominant driver of this epidemic. In 2014, prescription opioids were involved in 18,893 overdose deaths, more than heroin and cocaine combined and more than quadrupling the number of opioid overdoses in 2000 (National Institute on Drug Abuse, 2015). The current level of opioid misuse is a "public health crisis" (Kolodny et al., 2015) and the CDC lists it in the top five public health challenges (CDC, 2013) while noting that it is the "fastest growing drug problem in the United States" (CDC, 2012) and "the worst overdose epidemic in [U.S.] history" (Paulozzi, 2010).

While there are clear concurrent national trends in overdoses and increased medical distribution of opioids since 1999 (e.g., Bohnert et al., 2011) as well as geospatial correlations (Paulozzi and Ryan, 2006), there is little empirical evidence of the causal relationship between increasing medical access to opioids and spillovers resulting in nonmedical opioid abuse. In fact, there is little economic evidence concerning the underlying causes of the opioid epidemic more generally. Understanding the externalities associated with access to medical care is fundamental to optimal policy design when considering improving access to specific treatments or drugs.

Unlike many drugs associated with overdose deaths and other harms, opioids remain an important medical tool which, in certain cases, are even believed to be underprescribed.[1] Opioid therapy is an effective tool for acute pain management, although the efficacy of opioids for chronic non-cancer pain is limited (Dowell, Haegerich, and Chou, 2016). These drugs have legitimate medical functions, and yet, they are also highly-addictive, prone to abuse, and frequently diverted from their intended medical use. Consequently, expansions in health insurance which increase medical access to these prescribed pharmaceuticals for a specific population may have detrimental consequences on the broader population through non-medical spillovers.

[1] Greco et al. (2014) provides evidence that undertreatment of pain through opioid therapy is frequent for patients with cancer. Chaparro et al. (2014) finds systematic evidence in the literature of the efficacy of short-term opioid therapy.

Little is known about the broader non-medical spillovers of increasing access to opioids and the role of these spillovers in the rising opioid epidemic. What is known is that two-thirds of people who report nonmedical use of prescription pain relievers get them from a friend or relative (SAMHSA, 2015). Recent guidelines from the CDC regarding prescribing opioids to chronic pain patients (CDC, 2016; Dowell, Haegerich, and Chou, 2016) may be effective at reducing the risk of abuse among these patients, but it will do less to influence abuse rates if spillovers are the primary driver of recent trends.

The economics literature has studied the abuse of illegal drugs (Becker, Grossman and Murphy, 1991; Grossman and Chaloupka, 1998; Jacobson, 2004), shocks to the supply of illegal drugs (Dobkin and Nicosia, 2009; Galenianos, Pacula, and Persico, 2012), and misuse of legal drugs (Carpenter and Dobkin, 2009; Chaloupka, 1991; Manning et al., 1989). There is surprisingly little work on negative spillovers associated with increasing medical access to prescription drugs despite considerable evidence of a positive association between opioid prescribing and opioid abuse (Dart et al., 2016; Bohnert et al., 2011). The economics literature has also provided little evidence about the causal mechanisms underlying the opioid epidemic.

This paper studies the interaction of medical drug markets with illegal drug use. This interaction is an important challenge for the opioid epidemic since, unlike illegal drug markets for drugs such as cocaine and heroin, reduced supply is not a clear policy goal given the legitimate medical benefits of pain relievers. A full welfare analysis of increasing access to addictive opioids must account for the potential benefits and harms to the patient as well as the broader externalities to the general population. The latter is the focus of this paper.

A primary motivation for this paper is identifying a causal link between medical access to prescription opioids and spillovers resulting in opioid abuse. We exploit large and differential geographic changes in opioid supply caused by an insurance expansion targeting a segment of the population with differential concentrations across the country. These changes in opioid supply are large and mimic the national growth in opioid access. Identification originates from a significant insurance expansion in January 2006, with the implementation of the Medicare Prescription Drug Benefit Program ("Part D"), which provides voluntary outpatient prescription drug coverage to millions of Medicare beneficiaries. Safran et al. (2005) estimated that approximately 25% of Medicare beneficiaries did not have any prescription drug coverage prior to 2006. Several studies have shown that passage of Medicare Part D increased access and

utilization of prescription drugs among the elderly (Duggan and Morton, 2010, 2011; Zhang et al., 2009; Ketcham and Simon, 2008).

At a more aggregate level, this expansion differentially affected states based on the proportion of the population eligible for Medicare. States with a relatively large fraction of individuals gaining prescription drug coverage due to Part D experienced a relative increase in opioid supply (i.e., opioids distributed to the state). This has the potential to affect the Medicare-ineligible population if a primary access point is either elderly relatives/friends with multiple concurrent opioid prescriptions or diverted opioids from medical facilities, pain clinics, and pharmacies that care for elderly patients. While the elderly have a relatively modest rate of unintentional opioid overdose deaths (Paulozzi et al., 2011), they are the legitimate medical users of more opioid prescriptions than any other age group (Volkow et al., 2011). Moreover, multiple opioid prescriptions from several providers at the same time – suggesting a high potential for diversion – is fairly common among the Medicare population (Jena et al., 2014).

In this paper, we exploit the differential effects of the implementation of Part D on states based on cross-sectional pre-Part D variation in elderly shares. This approach permits us to account for national effects associated with Part D and other secular trends while also controlling for fixed differences across states. Drawing on evidence presented below that states with higher elderly shares have higher Part D enrollment and enrollment in Part D increased the amount of opioids prescribed to individuals 65 years and older, we study whether the overall supply of opioids increased disproportionately in high elderly share states, which we define using the 2003 percent elderly. Once we establish that the distribution of opioids (from producers) is higher to states with a higher elderly share post-adoption of Medicare Part D, we then examine whether this differential increase in opioid supply led to disparate growth in opioid abuse rates as measured by overdose deaths and using a complementary measure of substance abuse prevalence. While Part D also potentially affected prescription drug access for the Social Security Disability Insurance (SSDI) population, we show that our results are not driven by any systematic behavioral changes among individuals under 65 covered by Medicare.

This paper makes two primary contributions. First, this paper provides, to our knowledge, the first causal evidence that increasing prescription opioid access increases substance abuse and mortality for populations not gaining direct medical access. While increased medical access to opioids is often blamed for the opioid epidemic, it has been difficult

to isolate the effect of increased access (due to affordability) from other concurrent health care market factors (such as increased incidence due to a rise in diagnoses of musculoskeletal conditions). It is challenging to experimentally replicate the dramatic expansion in access to opioids or disentangle the historical time series increase from other national trends. We exploit a large and geographically-varying increase in opioid access to mimic the national expansion. This approach provides a useful and rare opportunity to observe the consequences over time of a large and (conditionally) exogenous increase in opioid access.

Second, a large literature discusses the ramifications of Medicare Part D on the 65+ population but there is limited evidence of its effects on the under-65 population. This paper provides evidence that Part D had important spillovers on the health of the population not covered by the program.

This evidence is especially timely given the ongoing numerous insurance expansions as part of the Patient Protection and Affordable Care Act (ACA) and provides insight regarding the possible contribution of previous insurance expansions, such as the Massachusetts Health Care Reform and state Medicaid expansions. Health insurance expansions may affect opioid abuse through several different and potentially off-setting channels. Health insurance increases medical care utilization (Manning et al., 1988), which could lead to more prescriptions of pain relievers for new conditions diagnosed. Alternatively, health insurance could improve access to substance abuse treatments, particularly if such treatments are included as a benefit, which may independently alter abuse-related outcomes. A key advantage of studying Medicare Part D is that it only altered prescription drug access, not medical care utilization directly (especially for the Medicare-ineligible population), allowing us to isolate the effects of opioid supply from changes in substance abuse treatment access and other factors.

States with relatively large elderly shares experienced faster growth in opioid access due to the implementation of Medicare Part D. Our approach allows us to study general equilibrium spillovers at the state level resulting from this supply shock. Since we are not using individual-level variation in Part D eligibility, we are not studying how individual medical access to opioids puts individuals at risk of long-term opioid addiction. Instead, we use geographic-level variation, comparing people in areas experiencing larger prescription drug expansions to those incurring smaller expansions, isolating the consequences of broader opioid medical access on the general population.

We find a strong positive relationship between elderly share and the growth in prescription opioids distributed at the state level. Having established that 2003 elderly share predicts growth in opioid access, we estimate difference-in-differences and event history models to assess the differential impact of Part D on opioid substance abuse treatment admissions and overdose deaths. We find significant effects on both outcomes and there is no evidence of differential pre-existing trends. Our estimates imply that a 10% increase in medical access to opioids leads to a 7.4% increase in opioid-related mortality and a 14.1% increase in substance abuse treatment admissions. We provide evidence that these results are not due to individuals gaining prescription drug access through SSDI, systematic state-level health insurance expansions, or concurrent demand-side shocks for opioids. We also consider a wide range of alternative causal pathways and provide evidence that Part D increased opioid abuse among the under-65, non-SSDI population through diversion. Our evidence suggests that 73% of the dramatic growth in opioid-related overdose deaths between 2000 and 2011 can be attributed to spillovers resulting from increased medical access.

The rest of the paper is organized as follows. In Section 2, we provide background on Medicare Part D and detail the data that we use to estimate our models. Section 3 describes our empirical approach. We present results in Section 4. We close in Section 5 with a summary of our main findings and the policy implications.

2. Background

2.1 Medicare Part D

On December 8, 2003, President George W. Bush signed the Medicare Modernization Act (MMA), which created Medicare Part D. Part D was implemented in 2006 and provided voluntary coverage of prescription drugs for those eligible for Medicare. The introduction of Part D was the largest expansion to Medicare since its creation and in 2015, accounted for $89.8 billion in expenditures.[2] Part D substantially reduced the out-of-pocket price of prescription drugs, and empirical evidence has found that these reduced prices increased use of prescription drugs among the 65+ population.

[2] The 2016 Annual Report of the Boards of Trustees of the Federal Hospital Insurance and Federal Supplementary Medicare Insurance Trust Funds: https://www.cms.gov/Research-Statistics-Data-and-Systems/Statistics-Trends-and-Reports/ReportsTrustFunds/downloads/tr2016.pdf (accessed August 27, 2016)

A large literature has studied the ramifications of Part D on prescription drug utilization (e.g., Ketcham and Simon, 2008; Zhang et al. 2009) and drug prices (e.g., Duggan and Morton, 2010) as well as effects on nondrug medical care utilization (McWilliams et al., 2011). Related work has examined plan choices among enrollees (e.g., Abaluck and Gruber, 2011; Ketcham et al., 2012). Most of this research focuses on the targeted population. There is far less work considering spillovers to the Medicare-ineligible population, which are potentially important given the large size of the program.[3]

Medicare Part D represents a substantial national change in prescription drug access for those with insurance through Medicare. We exploit the magnitude of this change and the differential effects that it had across the country while accounting for the national, uniform effects of Part D.

2.2 Data

Our empirical strategy is designed to estimate the spillovers from Part D so we primarily focus on the under-65 population when studying our opioid abuse measures. We will also present results for the 65+ population. Given the source of variation that we exploit, our estimates for this group still reflect the effect of spillovers from Part D, not the direct effect of increased individual access to prescription drugs. We discuss the sources for our data in detail in this section.

2.2.1 Opioid Distribution

Information regarding the supply of prescribed opioids within the state is captured in the Drug Enforcement Administration's (DEA) Automation of Reports and Consolidated Orders System (ARCOS). The Controlled Substance Act of 1970 requires all manufacturers and distributors to report their transactions and deliveries of all Scheduled II-V substances to the Attorney General. ARCOS is the system that monitors and records the flows of these controlled substances as they move from manufacturers to retail distributors at the local level (down to the street address and zip code, although this level of disaggregation is not made publicly available). Thus, ARCOS can be used to identify the distribution of specific opioid medications that are prescribed for medicinal purposes. We construct a measure of the seven most commonly abused

[3] One exception is Alpert et al. (2015) which shows that Part D increased direct-to-consumer drug advertising (DTCA). The rise in DTCA increased prescription drug utilization in several chronic drug classes among the population ages 40-60. Given that opioids are rarely advertised, DTCA is not a potential driving mechanism to explain our results.

opioid analgesics (Paulozzi et al., 2011; Paulozzi and Ryan, 2006): fentanyl, hydrocodone, hydromorphone, meperidine, methadone, morphine, and oxycodone (including OxyContin).[4] Following prior work, we convert the total grams distributed per capita into morphine equivalent doses drawing on standard multipliers used by the Centers for Medicare & Medicaid Services (CMS).[5] These were aggregated by state and year for the 2000-2011 time period.

2.2.2 Mortality

Information on opioid overdose deaths comes from the National Vital Statistics System (NVSS), a census of deaths in the United States. We code deaths as related to prescription opioid pain relievers using the ICD-10 external cause of injury codes (X40-X44, X60-64, X85, or Y10-Y14) and drug identification codes (T40.2-T40.4), which indicate death by any opioid analgesic. This coding follows the CDC classification system of deaths related to prescription opioids.

We aggregate the data based on state of occurrence and year. Our primary results will focus on ages 0-64, but we will also present estimates for smaller age groups and the 65+ population. We have data for 1999-2013 and use the full data set when presenting figures and estimating event studies while relying on the 2000-2011 sample for our main results.

2.2.3 Substance Abuse Treatment Admissions

For complementary evidence, we use the Treatment Episode Data Set (TEDS) to study substance abuse treatment admissions. The TEDS is collected annually by state substance abuse agencies at the request of the Substance Abuse and Mental Health Service Administration (SAMHSA). The data contain the majority of all publicly funded substance abuse treatment admissions that occur within the United States, as all facilities that receive any government funding (federal block grant funding, state treatment dollars, or even insurance dollars from Medicaid, Medicare, or Tricare) are required to provide basic information. Private facilities that only treat non-publicly insured individuals and that receive no federal or state grant monies are the only facilities that are supposed to be excluded. However, states differ in the scope of facilities covered due to differences in agencies responsible for licensing, certification and accreditation, and disbursement of public funds for treatment. Moreover, the scope of

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[4] Our results are not meaningfully changed if we include other opioids (e.g., codeine) since the seven types listed above dominate (in terms of use and strength) the other possible opioids that could be included in this metric.

[5] https://www.cms.gov/Medicare/Prescription-Drug-Coverage/PrescriptionDrugCovContra/Downloads/Opioid-Morphine-EQ-Conversion-Factors-March-2015.pdf (last accessed April 8, 2016)

admissions captured by those facilities that do report to TEDS also varies across states, as some states only report admissions for clients that were treated with public funds while others report all admissions from within the facility (SAMHSA, 2013).

These exclusions are unlikely to cause problems for our empirical strategy for two reasons. First, all our specifications include state fixed effects which will account for persistent differences in state reporting over time. Second, our source of identification (the interaction of 2003 elderly share and the introduction of Part D) is unlikely to be correlated with changes in the share of unobserved facilities missed by TEDS or changes in which admissions get reported at the state level. Instead, our strategy is problematic only if state *changes* in "unobserved facilities" or "admissions reported" are correlated with 2003 elderly share (and these systematic changes coincided with Part D). In our analyses, we will test this assumption by removing particularly problematic reporting states. We find little difference in the results whether we use the full sample or a smaller sample in which we are more confident of consistent reporting behavior. There is also no evidence that treatment admissions for other substances (e.g., heroin) differentially increased in high elderly share areas due to Part D, suggesting that differential reporting is not an issue.

The unit of observation in the TEDS is an admission, and information is retained on the primary, secondary, and tertiary substances reported at the time of the admission, as well as client demographics, expected source of payment, treatment setting, and treatment characteristics. We use aggregated annual case-level data on admissions for the period 1992-2012. Our main analysis will use the 2000-2011 time period to narrow the time period closer to the implementation of Part D date and to remain consistent across all data sources, though we will also present event studies and figures that use data from all available years.

We include two substance categories in our metric of opioid abuse: "non-prescription methadone" and "other opiates and synthetics." The latter category includes "buprenorphine, codeine, hydrocodone, hydromorphone, meperidine, morphine, opium, oxycodone, pentazocine, propoxyphene, tramadol, and any other drug with morphine-like effects." We include all admissions in which one of these drugs is included as primary, secondary, or tertiary substances.[6]

[6] Our results do not change meaningfully if we only count primary substances or if we exclude non-prescription methadone treatments.

TEDS provides age in broad categories: 12-15, 15-17, 18-20, 21-24, 25-29, 30-34, 35-39, 40-44, 45-49, 50-54, 55+. Consequently, to study the impact of Part D on under-65 age groups, we rely on analyses of the 12-54 age group. We will also show results for smaller age groups as well as the 55+ group. TEDS includes information on source of insurance, so we are able to take out any non-elderly with Medicare insurance (i.e., the SSDI population) and test the sensitivity of our results to including this group. Overall, the TEDS provides a useful, complementary measure to study opioid abuse that may not be captured by the overdose rate.

2.2.4 Other Variables

We study changes in opioid abuse as a function of the percentage of the state population ages 65+ in 2003. We choose 2003 because Medicare Part D was signed into law at the end of that year, and hence 2003 is likely free of any possible anticipation effects (see Alpert, 2016). We will show that our results are not sensitive to the choice of 2003 as a baseline. We use population data from the Census to construct our population variables. We will also show specifications including the state unemployment rate from the Bureau of Labor Statistics and the private health insurance rate from the Current Population Survey.

In analyses using our full set of controls, we also control for the adoption of prescription drug monitoring programs (PDMPs) at the state level. Prescription drug monitoring programs are recommended by the CDC and ONDCP as a useful strategy for combatting prescription drug misuse and harms. The research evaluating these programs, however, is quite inconclusive in terms of their impact on opioid prescribing and related harms (Patrick et al., 2016; Bao et al., 2016; Maughan et al., 2015; Paulozzi and Stier, 2010). Nonetheless, there has been significant growth in the adoption of PDMP programs across states during our sample period. Following Patrick et al. (2016), we include measures of whether a state has an operational PDMP as well as three specific dimensions of PDMPs that have been found previously to possibly deter improper prescription drug misuse: (1) whether the PDMP requires real-time reporting and hence makes information known about prescriptions available to physicians and pharmacists in a timely fashion, (2) whether physicians are mandated to participate in the PDMP (as opposed to the law

only applying to pharmacies), and (3) whether the state PDMP monitors drugs on four or more of the state's controlled substance schedule guidelines.[7]

2.2.5 Descriptive Statistics

We include means for our outcomes and other variables in Table I. The percent elderly in 2003 was 12.6% with a state-level standard deviation of 1.9%. This percent ranges from 6.2% in Alaska and 8.5% in Utah to 15.4% in West Virginia and 17.0% in Florida, representing a significant amount of variation in the Medicare-eligible population across states. The geographic distribution of the percent elderly is shown in Appendix Figure A.1.

There was substantial growth in our opioid measures, as shown in Figure I, throughout our entire analysis period. Distribution of opioid analgesics grew during this period, rising 118% from 2000 to 2005 and then again another 89% between 2005 and 2010. Opioid overdose deaths also show a significant rise during this time period, more than doubling from 2000 to 2005 followed by a 46% rise from 2005 to 2010. Substance abuse treatment admissions also more than doubled between 2000 and 2005 and then doubled again from 2005 to 2010.

There appears to be a greater rise in opioid prescriptions and opioid deaths in the period preceding the implementation of Medicare Part D than in the period following Medicare Part D. Baseline differences account for much of this, but it is also possible that some state- and national-level policies intended to curb opioid abuse have also altered these trends. Consequently, it is important to account for time fixed effects while employing an empirical strategy which exploits differential geographic shocks to opioid access.

3. Empirical Framework

Medicare Part D was implemented as a national program in 2006, but states were affected differentially based on the fraction of their population eligible for Medicare benefits. While there are multiple ways for individuals to become eligible for Medicare, we use cross-state

[7] Like the Federal government, each state has developed their own guidelines for scheduling controlled substances to help facilitate sentencing decisions related to drug offenders, which are mostly tried in state courts. Most states follow the Federal Controlled Substance Act (CSA) (21 U.S.C. 811 et seq.) in their adoption of a five-tier classification system, with those placed on the top most tier (e.g. Schedule I) indicating greatest potential for abuse and little or no medical use, and those on the lowest tier (e.g. Schedule V) representing substances with low potential for abuse and clear therapeutic benefits. However, states have taken different approaches in the placement of particular drugs in specific tiers (see Chriqui et al., 2002 for more about state scheduling). A PDMP that monitors drugs in multiple tiers has the greatest chance of capturing a range of overprescribing of opioids that can fall into low and high classifications.

variation in the percentage of the population ages 65+ and find that this serves as a useful predictor.[8] We fix our population share variable in 2003; identification originates solely from the introduction of Part D interacted with fixed state elderly shares. This strategy allows us to non-parametrically control for the independent effects of Part D (through year fixed effects) and fixed elderly share (through state fixed effects). We do not use a time-varying elderly share measure in the interaction term because there may be systematic migration that is correlated with opioid abuse. For example, opioid abuse may be related to local economic downturns. If declining economic conditions cause younger people to disproportionately migrate out of the state (i.e., increasing the percentage of the population 65+), then this source of variation is problematic.

We use the timing of Part D and cross-sectional differences in elderly share across states for identification. We estimate the specification

$$y_{st} = \alpha_s + \gamma_t + X'_{st}\beta + \delta\left[\%Elderly_{s,2003} \times 1(t \geq 2006)\right] + \varepsilon_{st}\ , \quad (1)$$

where y_{st} is a measure of opioid-related distribution, abuse, or mortality for state *s* in year *t*. *X* is a vector of time-varying covariates, including a time-varying measure of elderly share. We evaluate the robustness of our findings to the inclusion of additional controls, including the unemployment rate, the private insurance rate, the log of population size, and the PDMP policy variables. Our baseline specification does not include these covariates because of concerns that some of these variables may themselves be outcomes related to opioid abuse. Our estimates are generally consistent whether these covariates are included or not.

We are interested in the estimate of δ, which represents the differential change in the outcome experienced by states with a high elderly share relative to other states. We expect this estimate to be positive if Part D increased opioid access and, consequently, opioid-related substance abuse. The specification accounts for time fixed effects and fixed differences across states.

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[8] The dually-eligible population was eligible for prescription drug coverage through Medicaid before 2006, implying that the change in prescription drug coverage was not a one-to-one relationship with elderly share. Given that we are using initial elderly share as a predictor of growth in opioid access, prior Medicaid coverage should not affect our results as long as it does not completely unravel the relationship between elderly share and the change in opioid distribution (i.e., as long as there is still a "first stage"). We empirically verify that 2003 elderly share is correlated with changes in opioid distribution. Our 2SLS estimates will appropriately scale the relationships between opioid access and abuse outcomes.

Elderly share is not a perfect determinant of Medicare eligibility as Part D also increased coverage rates for the non-elderly SSDI population. We are interested in isolating the impact of Medicare Part D on a population not directly gaining access to prescription drug coverage through Part D, and focusing on outcomes for the non-elderly population risks our inclusion of non-elderly SSDI participants. We focus on elderly share because the SSDI population was likely to have prescription drug coverage even before Part D and often experienced a decrease in generosity upon implementation of Part D.[9] One would therefore not anticipate seeing gains in access after Part D implementation due to this population. Consequently, we think that elderly share is the more appropriate measure. We provide evidence that any relationship between elderly share and prescription drug access through SSDI is not driving our results by making use of additional information included in TEDS that allows us to exclude SSDI Medicare recipients from the analysis sample.

Equation (1) assumes that any differential effect begins in 2006, the enactment year of Medicare Part D. However, the enrollment period in 2006 lasted until May 15 and there were no penalties for late enrollment before that date. Consequently, enrollment in Part D was delayed relative to subsequent years and we expect that there is potentially a delayed effect in our analyses as well. We observe some evidence of a delayed effect in our event study analyses. In Section 4.5.2, we present estimates when 2006 is excluded from the analysis.

We focus on substance abuse measures for the under-65 population, those not directly affected by the introduction of Part D, but we will also present results for the 65+ population as well. Since our outcomes are rates and our variation does not originate from individual-level variation in Part D eligibility but, instead, from cross-state variation in the proportion of the state that is eligible for Part D, we interpret these estimates as spillovers as well. The estimates for each age group reflect the group's propensity to acquire and abuse diverted opioids. Given the relative rarity of nonmedical opioid use among the elderly population, we do not expect to observe large spillover effects for this population.

Our outcome measures will typically be specified as deaths per 100,000 people or substance abuse treatments per 100,000. When estimating the relationship between our

[9] Individuals who have received Social Security Disability benefits for 24 consecutive months ("SSDI") receive Medicare benefits, but many also receive benefits from Medicaid; these beneficiaries are called "dual eligible." Prior to Medicare Part D, these dual eligible generally received prescription drug benefits through their state Medicaid program.

interaction variable and the distribution of opioids, we use the log of morphine equivalent doses per capita.[10] We weight all regressions by state population, and standard errors are adjusted for clustering at the state level.

4. Results

4.1 Part D Enrollment & Prescription Opioid Use Among the Elderly

Our empirical strategy relies on the assumption that elderly share predicts changes in state opioid supply due to Part D implementation. We will test this assumption explicitly in the next section but, first, we explore intermediate outcomes which would suggest that we should expect such a relationship. One condition for elderly share to predict growth in opioid access beginning in 2006 due to Part D is that high elderly states have higher Part D enrollment. We use Part D enrollment data from the CMS aggregated by state and year to test for this relationship. Part D may impact access by providing prescription drug coverage to part of the population which would not have had any coverage otherwise or by providing more generous coverage to people who would have had coverage even in the absence of Part D. Both of these mechanisms are important components.

Figure A.2 in the Appendix quantifies the relationship between elderly share and the Part D enrollment rate (Part D enrollment divided by state population). It shows coefficient estimates from cross-sectional year-by-year regressions of the Part D enrollment rate on 2003 elderly share between 2006 and 2011, indicating that each additional percentage point of the state population ages 65+ predicts an additional 0.4 to 0.6 percentage points of the population enrolled in Medicare Part D. This relationship grows over time.

A second necessary condition is that enrollment in Medicare Part D increased the amount of opioids prescribed to individuals 65 years and older. While several papers have identified an impact of Medicare Part D on prescription drug utilization for the 65+ population, we are not aware of any published analyses looking specifically at the effects on opioid utilization.[11] To

[10]The results are qualitatively similar if we use levels (i.e., morphine equivalent doses per capita). We primarily rely on the log of per capita doses because this outcome is skewed.

[11]Kuo et al (2016) show that 90 day opioid use among the elderly insured through Medicare Part A, B and D rose from 4.62% in 2007 to 7.35% in 2012, while Zhou et al. (2016) show using data from the 1999-2012 Medical Expenditure Panel Survey that Medicare became the largest payer of opioid pain relievers with the implementation of Medicare Part D. Neither of the analyses specifically demonstrates that the adoption of Medicare Part D led to an increase in access to opioids among those who became covered.

verify previous findings hold for opioids specifically, we conducted our own examination of the impact of Medicare Part D insurance on the number of opioids prescribed by comparing opioid prescriptions filled by a group of newly Medicare insured (those 66-71 years of age) to a sample of near elderly (those 59-64 years of age) in the 2002-2009 Medical Expenditure Panel Survey (MEPS). A complete description of this analysis is included in Appendix Section A. The main results and numerous sensitivity analyses demonstrate that Medicare Part D decreased the out-of-pocket price of opioids substantially (by 48%) and increased the number of annual prescriptions by 0.174 relative to the 59-64 age group (representing a 28% increase). Despite the relatively small sample in the MEPS,[12] this estimate is statistically significant. This relationship suggests that Part D had the potential to increase the supply of opioids in states with a large elderly share. The impact of elderly share and the introduction of Part D on the growth in state opioid supply is an empirical question and addressed in the next section.

4.2 State-Level Increases in Opioid Distribution

With evidence that Part D decreased the price of opioids for the Medicare-eligible population and that this price decrease led to an increase in the number of opioid prescriptions filled at the individual level, we now turn to our main models to examine whether pre-Part D state elderly share is associated with an increased state supply of opioids. We estimate equation (1) using the log of the morphine equivalent doses per capita from the ARCOS data as our outcome variable and present our estimates in Table II. We estimate that a one percentage point increase in the 2003 elderly share is associated with additional 2.9% growth in per capita opioid distribution, equivalent to about 0.3 morphine equivalent doses per person. This estimate is robust to the inclusion of the unemployment rate, the private insurance rate, and the log of population (Column (2)). In Column (3), we add controls for PDMPs and the estimated effect grows in magnitude further. The consistency of the estimates across models is suggestive that there is not a time-varying confounder that is biasing our estimates.

We present event study results to understand the temporal relationship between fixed elderly share and the log of per capita opioid distribution. We estimate equation (1) but allow

[12] The literature studying the utilization effects of Part D often uses much larger data sets, usually claims data from pharmacies.

the 2003 % Elderly variable to have a separate effect in each year. Figure II shows the point estimates along with 95% confidence intervals. We normalize the estimates to zero in 2003. We observe little evidence of differential trends before 2006 and the pre-2006 estimates are never statistically distinguishable from zero. Higher elderly share is even associated with a *decline* in opioid distribution between 2004 and 2005. Beginning in 2006, we observe a steady increase in the estimated effect until 2011. With the exceptions of 2006 (partially-treated) and 2007 (significant at 10% level), the effect is statistically significant from zero at the 5% level in each year after implementation.

Overall, we find convincing evidence that the introduction of Medicare Part D differentially affected the geographic supply of opioids based on elderly share. In the next sections, we analyze harms associated with this broader opioid availability.

4.3 Opioid Abuse Results

4.3.1 Graphical Evidence

Before we proceed to regression analysis, we show trends in abuse rates graphically. We separate states into two categories based on the fraction of the population in 2003 that is 65 years of age or older: those that are "above median" and those that are "below median." We predict that states with a larger elderly share should experience larger changes in opioid distribution abuse when Part D is implemented in 2006 if spillovers are an important driving force of opioid abuse.

Figure III shows the differential trends in per capita non-elderly mortality and substance abuse treatment admissions. In the left panel, we see that the trends in per capita opioid-related mortality for those aged 0-64 in high versus low elderly share states look similar. The levels are also similar (4.19 deaths per 100,000 in the high elderly share states in 2005; 4.32 deaths in the low elderly share states in 2005). After the enactment of Part D and especially by 2007, the trends diverge and we observe large increases in mortality among the high elderly share states.

We will also provide complementary evidence of abuse outcomes by studying the differential impact of Part D on opioid-related substance abuse treatment admissions among the non-elderly (those aged 12-54). In the right panel of Figure III, we present the trends for the above and below median states. We only use states which report treatment admissions for every year 1992-2012, which includes 39 states. Before 2006, the above median and below median

states look similar. Upon implementation of Medicare Part D, the above median states incur a relative increase in per capita substance abuse treatments, providing further evidence of an increase in opioid abuse in states with high elderly share resulting from Part D. The increases in mortality and substance abuse are occurring in the population not directly gaining medical access to prescription drugs through Part D.

Overall, the graphical evidence is consistent across data sets and outcomes. We see no evidence of different pre-existing trends based on fixed elderly share. After the implementation of Medicare Part D, opioid abuse rates increase in the high elderly states relative to the low elderly states.

4.3.2 Mortality Regression Estimates

We present our estimates of the differential impact of Medicare Part D on non-elderly opioid-related mortality in Table III. The outcome variable is opioid-related deaths per 100,000. We estimate that each additional percentage point of the percentage elderly is associated with 0.37 additional deaths per 100,000 people after the enactment of Part D (Column (1)). This estimate is statistically significant from zero at the 1% level. In Column (2), we add time-varying controls and find that the estimate is robust to accounting for these factors. We control for PDMP policy variables in Column (3) and estimate that each additional percentage point of the percentage elderly is associated with 0.36 additional deaths per 100,000 people. Again, the consistency of the estimates across the models is suggestive that the results are not being driven by unobserved time-varying shocks.

Table IV breaks down the relationship between Part D expansion and opioid-related mortality by sex and age group. The results show that the effect is larger for men across all age groups and, for both men and women, largest for the 30-39 age group. Men and women have similar age gradients. For men, each percentage point of elderly share leads to 1.1 additional opioid-related deaths per 100,000 people for the 30-39 age group, more than twice as large as the aggregate effect shown in Table III. The effect is 0.5 deaths per 100,000 people for the same age group for women. We also estimate large effects for the other age groups highlighted by Case and Deaton (2015). The age profile appears to increase from 20-29 to 30-39, and then steadily decline at older ages, until ages 60+ where we observe no statistically significant effects at the 5% level.

Trend lines presented in Figure III suggest that pre-existing trends are not driving our mortality estimates. We perform an event study using our continuous elderly share measure and present the results in Figure IV. We use the full available data set (1999-2013) in the event study. Before Part D, the effect of 2003 elderly share is never statistically significant from zero and the effect even declines immediately prior to Part D implementation. After 2006, the estimate increases and is statistically significant from zero in 2007 and several years after. While it is often difficult to observe mortality policy responses given that deaths are rare events, we observe consistent evidence even when disaggregating the effects by year.

4.3.3 Opioid Abuse Treatment Admissions

Opioid mortality, while extremely important from a public health perspective, is also a somewhat rare outcome. A more common outcome indicative of problematic use or abuse of opioids is treatment admissions. Because some states have historically been poor at reporting these admissions consistently over time (e.g., Washington D.C., Georgia), we restrict most of our analyses to states that report over the entire period, though we initially show results comparing the full sample and the balanced sample.

In Table V, we present estimates for opioid-related substance abuse treatment admissions for ages 12-54. The outcome variable is the number of treatment admissions per 100,000. In Column (1), we use the full sample and estimate that a one percentage point increase in the percentage of the state population ages 65+ in 2003 increases the rate of substance abuse treatments by 16.99 treatments per 100,000 people. As we add controls and account for PDMP adoption, our estimate remains relatively consistent in Columns (2) and (3). In Column (4), we select on states reporting in all years (i.e., the "balanced sample") and estimate a similar effect. The consistency of the estimate between Columns (3) and (4) should reduce concerns that our estimates are driven by changes in the states reporting information to TEDS over time. As we expected, elderly share is not correlated with changes in reporting behavior.

In Column (5), we further adjust the sample and exclude admissions which report either "Medicare" as the primary expected payment source or state that person is "Retired/Disabled."

These selection criteria appropriately exclude the SSDI population.[13] The estimate is relatively unaffected and actually increases in proportional terms since eliminating the SSDI population reduces the mean of the outcome variable. In this more narrowly defined population we see that a one percentage point increase in the elderly population (65+) is significantly associated with 13.42 additional substance abuse treatments per 100,000 people in the 12-54 age group.[14]

In Table VI, we examine this relationship across different age groups and sex, using the available age groupings in the TEDS. We rely on the balanced sample and exclude the SSDI population. We observe statistically significant effects throughout the age distribution, except for the 55+ age group. As with the mortality effects, the estimates are consistently larger for men. For both men and women, the estimates are largest for the 21-29 age group, where the effects are three times the size of the estimated aggregate effect for ages 12-54 (Column 5 in Table V). The point estimates steadily decrease at older ages. We find significant amounts of heterogeneity across age groups. This heterogeneity is consistent with the age trajectory estimated for mortality.

As shown previously in Figure III, we find little evidence of differential pre-existing trends between below median and above median elderly share states in terms of opioid-related mortality and substance abuse treatment admissions. We can further study whether there are pre-existing trends correlated with our continuous measure of 2003 elderly share by conducting an event study analysis for all the years of available data (1992-2012) in the TEDS. We present the results of such an analysis in Figure V using the balanced sample and excluding the SSDI population (results are similar using the full sample; see Appendix Figure A.3 for the equivalent event study). We find no evidence of pre-existing trends as the estimated effect is insignificant throughout the entire pre-period and the estimated effect actually declines between 2002 and 2004. The magnitude increases in 2006 and is statistically significant from zero in four out of the seven years after the enactment of Part D. Although noisier than the year-by-year estimates for opioid access presented in Figure II, the effects on substance abuse closely track the corresponding findings for distribution. Overall, these results provide strong evidence that

[13] In fact, there is some concern that we are excluding *more* people than we should since some of these people may not be on SSDI. This may add noise to our estimates but should not bias the results. The consistency of the estimates across all analysis samples suggests that our sample criteria are not problematic.

[14] It is also possible in the TEDS to identify individuals referred by the criminal justice system. Our results are similar in proportional terms if we exclude this population.

opioid access through insurance benefits is associated with spillover effects resulting in more substance abuse treatment admissions among populations that do not directly benefit from the insurance expansion.

Given our concern that reporting issues may obfuscate the useful information in the analyses using the TEDS, we briefly summarize why we believe that the estimates in this section reflect true changes in substance abuse. First, there is little reason to believe that elderly share predicts changes in reporting behavior starting precisely in 2006 when we begin to observe evidence of effects. Second, our results are consistent when we select the sample on states that we are confident are supplying a less noisy measure of substance abuse treatments. Finally, we have replicated Table V using per capita heroin treatments and per capita treatments (across all substances) as the dependent variables.[15] For these outcomes, the estimates are never statistically significant and are close to zero. If reporting issues were the driving mechanism, then we would expect to observe effects on all types of treatments, not just opioid-related treatments.

4.4 Parameterizing the Relationship between Opioid Supply and Abuse

In this section, we parameterize the relationship between opioid access and spillovers to the under-65 population and the full population using 2SLS. In the first column of Table VII, we estimate the relationship between state morphine equivalent doses (MED) per capita and the state opioid mortality rate using OLS for ages 0-64. We find that each additional morphine equivalent dose increases the number of deaths by 0.308 per 100,000 people ages 0-64. When we instrument with our interaction term ($\%Elderly_{s,2003} \times 1(t \geq 2006)$), this estimate increases slightly to 0.333. In Column (3), we present the 2SLS estimate for the full population (including the elderly) and estimate a coefficient of 0.271. This estimate is smaller than the Column (2) estimate given the low abuse response of the 65+ population to additional opioid access (as shown in Table IV), but the effect is similar in proportional terms.

In the last three columns of Table VII, we present the same specifications for substance abuse treatment admissions as we did for opioid mortality. With OLS, we estimate that each morphine equivalent dose is associated with 6.9 additional treatment admissions per 100,000

[15] Results are available from the authors upon request.

people. When we estimate using 2SLS, the effect again gets larger, this time doubling in magnitude. In the final column, when we estimate the relationship for the population ages 12+, we find that each additional per capita morphine equivalent dose increases the substance abuse treatments by 8.5 treatments per 100,000 people. This effect is larger in proportional terms than the estimated effect for the 12-54 population.

In proportional terms, the estimates imply that a 10% increase in opioid supply increases opioid-related mortality rates (for ages 0-64) by 7.4% and substance abuse treatment admission rates (for ages 12-54) by 14.1%.[16]

4.5 Robustness Tests

We test the sensitivity of our results to several factors, particularly those factors that could provide alternative explanations for these results. We consider other possible mechanisms, such as a concurrent shock in the supply or demand for opioids, other insurance expansions that are also occurring during this time period, and confounding trends in potentially problematic states. We provide additional sensitivities analyses examining the robustness of our findings to alternative methodological assumptions in Appendix Section B. Appendix Table A.4 provides Poisson estimates to test for whether estimating proportional effects (instead of level effects) provides meaningfully different results. The estimates imply similar effects. In Appendix Figure A.4, we vary the base year used to calculate our initial elderly share measure and graph the estimated mortality effects for each base year. The estimates are similar regardless of which baseline we use.

4.5.1 Concurrent Supply-Side and Demand-Side Shocks

Although heroin is a type of opioid, it is supplied in illegal markets by drug cartels, not pharmaceutical companies. Hence, the supply of heroin should not be impacted by the implementation of Part D. However, if our opioid mortality and treatment results are being driven by some other concurrent confounding supply or demand shock, then it is possible that such a shock would impact not just opioids but also the heroin market. For example, if high elderly share states were disproportionately affected by the Great Recession and economic downturns are associated with increases in drug abuse, then we should also observe relative rises

[16] To calculate these estimates, we use the mean value in 2006-2011 for each outcome as the baseline. The mean for per capita morphine equivalent doses during this time period is 13.0.

in heroin and opioid abuse. The opioid-related mortality results in Figure IV suggest that the timing of any concurrent shock must coincide with the implementation of Part D, ruling out confounding factors that would imply gradual increases in opioid abuse rates. To assess the possibility that our results are being driven by some other unobserved confounder affecting substance abuse more broadly beginning in 2006, we conduct an additional event study analysis focusing on heroin mortality per capita. The results, presented in Figure VI, show there is little relationship between elderly share and heroin-related mortality growth. This reduces concerns of more abrupt (illegal) supply-side or demand-side opioid-related shocks. We also find little evidence of any relationship with heroin-related substance abuse treatment admissions in the TEDS.

4.5.2 Eliminating 2006

Medicare Part D was implemented at the start of 2006, but individuals were not penalized for delaying enrollment until May 15 and we expect that there was only a partial effect in the first year. Our event study analyses confirm this hypothesis and this partial effect should bias the estimates toward zero. In Table VIII, Panel A, we replicate our main estimates in each data set while excluding 2006. As expected, the magnitudes increase for all outcomes.

4.5.3 Other Insurance Expansions

We study a large prescription drug expansion and its differential effects at the state-level. During our time period, there were also large state-level health insurance expansions. In 2006, Massachusetts enacted a health care reform law which expanded health insurance to nearly the entire population. In 2008, Oregon expanded its Medicaid program. These expansions are not problematic to our empirical strategy if they are not systematically related to elderly share. We test the sensitivity of our results to this assumption by replicating our analysis excluding Massachusetts and Oregon (see the second panel of Table VIII). The results remain consistent with our previous main analyses for all of the outcomes.

4.5.4 Excluding Florida

Florida had a unique rise in opioid abuse during this time period due to the prevalence of pill mills in the state before the 2011 crackdown. Since Florida is also a high elderly share state, it is important to test whether Florida is solely driving the results, which we do in Panel C of Table VIII. When we exclude Florida from the analyses, our estimates are still large and statistically significant and, overall, our conclusions would be unaffected by the exclusion of

Florida from the analysis. While the estimates for legal distribution and substance abuse treatments decrease, the mortality estimate actually increases (relative to Table III, Column 3), which would imply even larger 2SLS estimates. We cannot statistically reject the equality of any of the estimates excluding Florida with the corresponding estimates including Florida.

4.6 Discussion

Studying drug abuse is always difficult given the necessary reliance on noisy measures and extreme events such as deaths. While these problems raise concerns in the analysis of this paper as well, our approach circumvents them in a few ways. First, we study a large shock to prescription drug access which has the power to identify effects, even when studying rare events. Second, our shock is likely orthogonal to concerns about changes in reporting of events (instead of actual changes in abuse).

As noted previously, the TEDS data from which we estimate impacts of Medicare Part D expansion on treatment admissions are not collected in the same systematic fashion in each state. States differ in the types of facilities that are included in their reporting for a variety of reasons, including differences in agencies responsible for licensing, certification and disbursement of public funds that may or may not be included in reporting requirements (SAMHSA, 2013). While we see little reason to believe that 2003 elderly share predicts systematic changes in reporting behavior, we also test this assumption by selecting on states with more consistent reporting patterns and find similar effects. The parallels with the estimated mortality effects (in both the overall effects and the age-specific patterns) also support the view that reporting behavior is not biasing our estimates.

We also find little evidence that our findings of spillover effects are driven by SSDI recipients, who are also eligible for coverage through Part D. First, the SSDI population is relatively small and typically had access to (more) generous prescription drug coverage before 2006. Second, the largest effects are actually observed at relatively young ages while the under-65 SSDI population is typically older. Third, we are able to explicitly exclude the SSDI population in the TEDS analysis and estimate similar effects.

Our results are also robust to functional form assumptions,[17] inclusion or exclusion of states with their own health insurance expansions, using different baseline years to construct the fixed elderly share measure, and several other assumptions made in the primary models. Event study analysis suggests that the effects began in 2006 and grew as Part D enrollment increased. These effects are not mimicked by other non-opioid measures of drug abuse. The robustness of our findings to these and several other sensitivity checks provide greater confidence that our results reflect true behavioral changes in abuse.

Finally, we interpret our effects as evidence of economically-meaningful levels of diversion when a state's opioid supply increases. We estimate effects on a population not directly impacted by the implementation of Part D using variation unrelated to personal changes in prescription drug coverage. An alternative mechanism would be that Part D led to differential changes in physician prescribing patterns resulting in spillovers to the under-65 population. We find little support for this interpretation given that opioids were already heavily-prescribed before Part D. In 2005, an average of 9.4 morphine equivalent doses were distributed per person in the United States. Furthermore, we observe the largest effects for younger age groups, not age groups close to 65, ruling out the possibility that physicians were more likely to mistakenly believe that their patients were covered by Part D in high elderly share states after 2006. Similarly, we would likely expect most physician prescribing spillovers to occur to older age groups, but the evidence suggests that the opposite is true.

Moreover, in the MEPS (used in Appendix Section A), while we observe an increase in the number of opioid prescriptions for the 66-71 population, we actually observe a *decrease* in opioid utilization for the 59-64 age group in 2006. This decrease is inconsistent with changes in physician prescribing behavior and suggests that the additional abuse operated through nonmedical acquisition of the drugs.

In Appendix Section C, we also include a more explicit test of this hypothesis. If prescribing patterns changed, then we would expect to observe that high elderly share would have larger effects in states with higher private insurance rates (among the 0-64 population). We

[17] Though not shown, we have also estimated other models in addition to those presented in Appendix Section B which test for the importance of functional form assumptions. We have also estimated models which use the log of 2003 elderly share, logged outcome variables, etc. The results are consistent across all models.

do not observe this relationship, suggesting that the rise in abuse operates through nonmedical acquisition.

Finally, it is also unlikely that our effects could be explained by differential geographic changes in price. If Part D increased demand for opioids, this may have driven prices up. However, our time fixed effects capture national price level changes. If opioid prices increased more in high elderly share states because of the increased demand, this rise would actually imply that our estimates are biased away from zero since the higher prices would deter acquisition. Consequently, we interpret our estimates as the relationship between increased supply and the consequences of opioid diversion.

5. Conclusion

According to the CDC, 78 people die each day from an opioid overdose in the United States and at least half of those involve a prescription opioid (CDC, 2016). More than 1.4 million emergency department visits occur each year (SAMHSA, 2013), and the most recent household estimates suggest that 1.9 million individuals meet the criteria for abuse or dependence on pain medication (SAMHSA, 2015). In response, the Obama Administration has proposed $1.1 billion in new funding to help those with opioid abuse disorders obtain treatment and, in March 2016, it proposed additional funding to support several new avenues for expanding access to treatment (White House, 2016).

While many federal, state and community strategies have been offered to try to counteract the tide, explanations and empirical evidence for what caused the rise of the opioid epidemic in the first place have been rare. This paper is the first to evaluate the extent to which expansions in medical access, specifically insurance that reduced the cost of prescription drugs to patients, may have contributed to the opioid epidemic. By exploiting geographic variation in the location of the elderly, who were the primary beneficiaries of Medicare Part D implementation, we are able to evaluate how expansion of prescription drug benefits (independent of expansions in access to medical care) might have influenced the opioid epidemic. Using our estimates in Table II, the differential growth in opioid supply caused by Part D between the highest and lowest elderly share states is equivalent to the overall national growth in opioid distribution between 2004 and 2008. Part D provides a rare opportunity to mimic the dramatic national trends in medical opioid supply and observe the spillover effects, while conditioning on time fixed effects.

It is difficult to conceive of alternative mechanisms for these spillover results than those already explored here. Evidence from SAMHSA (2015) indicates that friends and relatives are the primary source of prescription opioid medication, and elderly with multiple concurrent prescriptions are an easy target for some individuals interested in diverting opioids into the black market. Our results are consistent with these stylized facts and provide evidence about its causal relationship with opioid-related overdoses.

We interpret our results as clear evidence of diversion from the medical market to the illegal nonmedical use market. Opioid distribution in the United States increased between 2000 and 2011 by 376% while opioid-related overdose mortality rates increased by 346% over the same time period. Our Table VII (Column 3) estimates imply that the increased access to opioids explains 73% of this rise (i.e., the estimates predict a 252% causal mortality increase). Attributing this magnitude to unintentional spillovers does not rule out the importance of more direct effects. Opioid prescribing behavior may lead to high addiction rates which are then exacerbated by nonmedical opioid access through diversion. Our results imply that the diversion component is a critical driver of the opioid epidemic.

The implications of these findings is that, unless supply side mechanisms become much more effective at reducing the opportunities for diversion of these prescription opioids from patients (by reducing over prescribing, enforcing PDMPs, educating physicians on inappropriate prescribing, and managing utilization), the opioid epidemic may not be over. There are a number of provisions of the Affordable Care Act that expand insurance to the previously uninsured (dependent care coverage, Medicaid expansion, and insurance exchanges), and it is possible that as we continue to expand insurance coverage we also further exacerbate growing trends in opioid mortality and morbidity. Optimal policy must account for the externalities of improving medical care access to drugs that are easy to abuse and divert.

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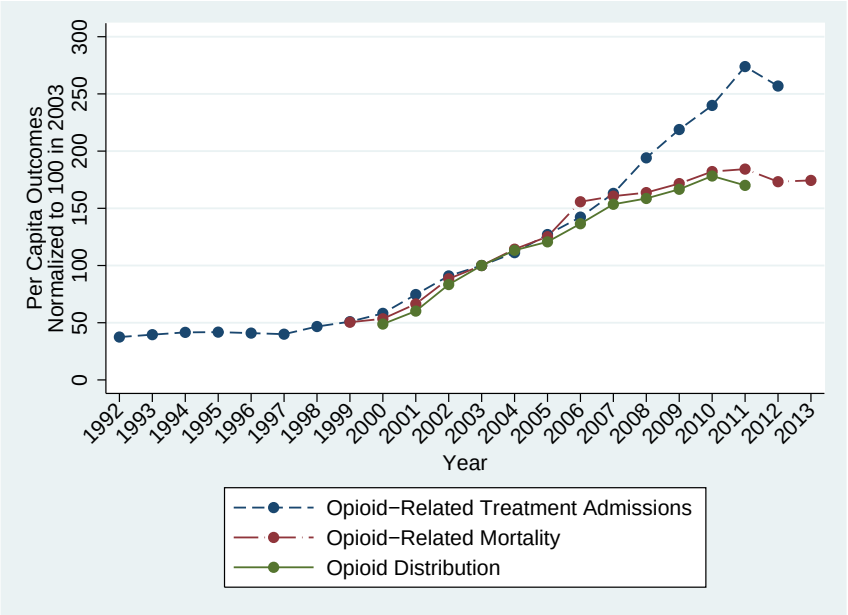
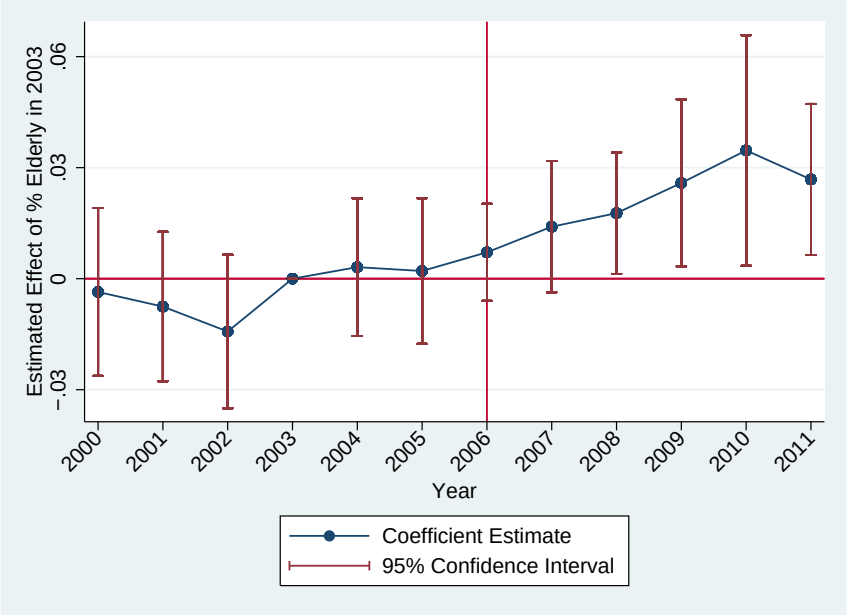
Figures

Figure I: Opioid Use and Abuse



Notes: We use ARCOS data to generate per capita opioid distribution, NVSS to create per capita opioid-related mortality, and TEDS to calculate per capita substance abuse treatments for opiates. We normalize each time series to 100 in 2003. The ARCOS time series spans 2000-2011; NVSS 1999-2013; TEDS 1992-2012.

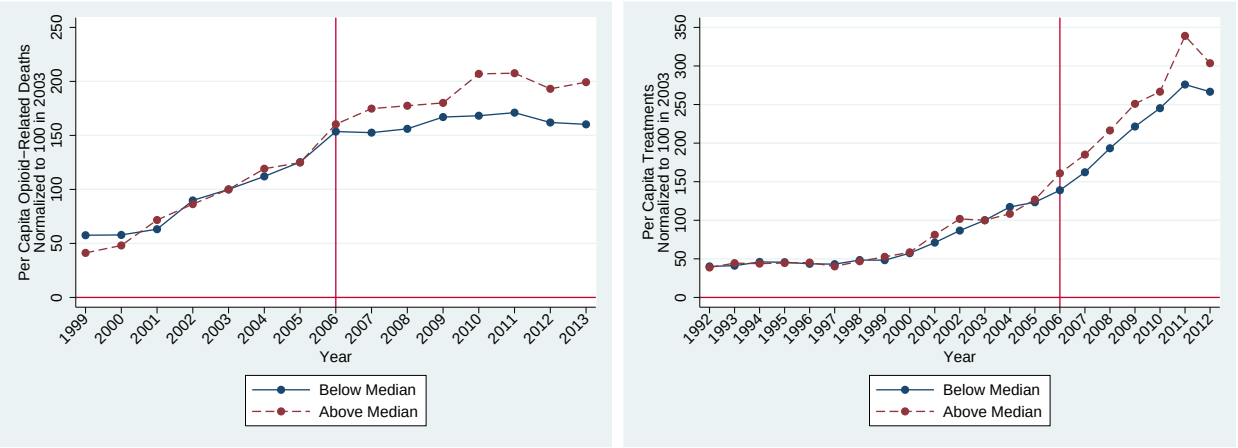
Figure II: Opioid Distribution: Event Study



Notes: We estimate equation (1) but allow the effect of Elderly Share in 2003 to vary by year, normalizing the coefficient for 2003 to zero. The outcome is the log of morphine equivalent doses per capita.

Figure III: Per Capita Opioid-Related Mortality and Substance Abuse Treatments

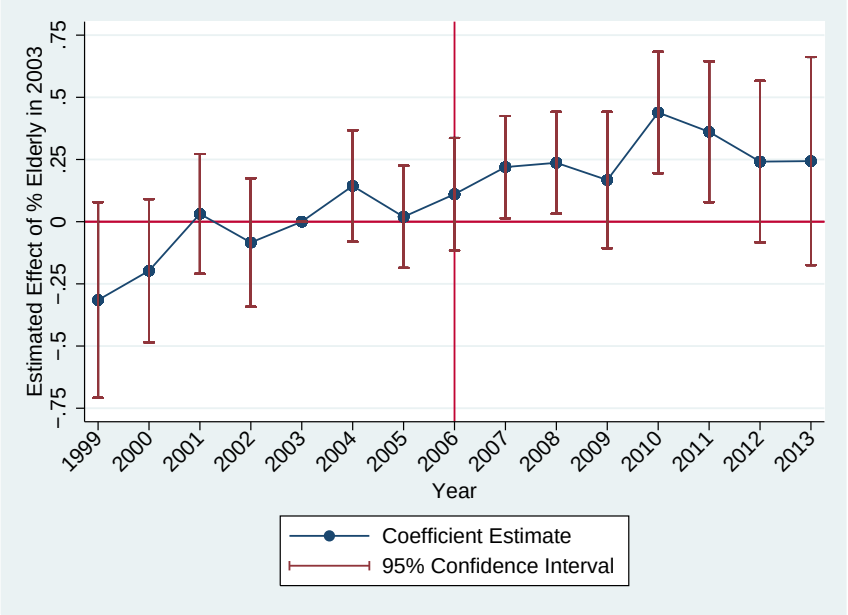
States with High Elderly Shares Versus States with Low Elderly Shares



Mortality, Ages 0-64 Substance Abuse Treatments, Ages 12-54

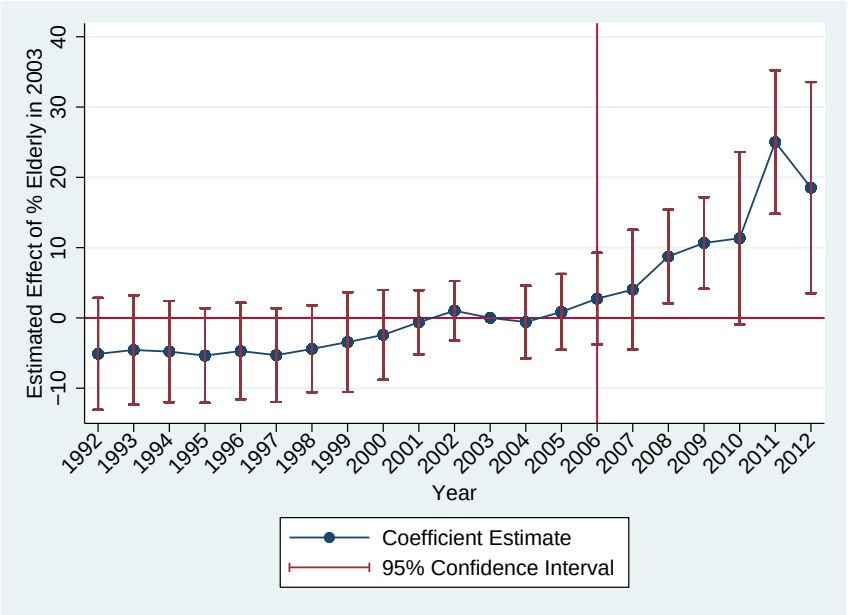
Sources: National Vital Statistics System and Treatment Episode Data Set
Notes: "Above Median" and "Below Median" refer to the elderly share of the population in 2003.

Figure IV: Opioid Abuse: Mortality per 100,000 (Ages 0-64)



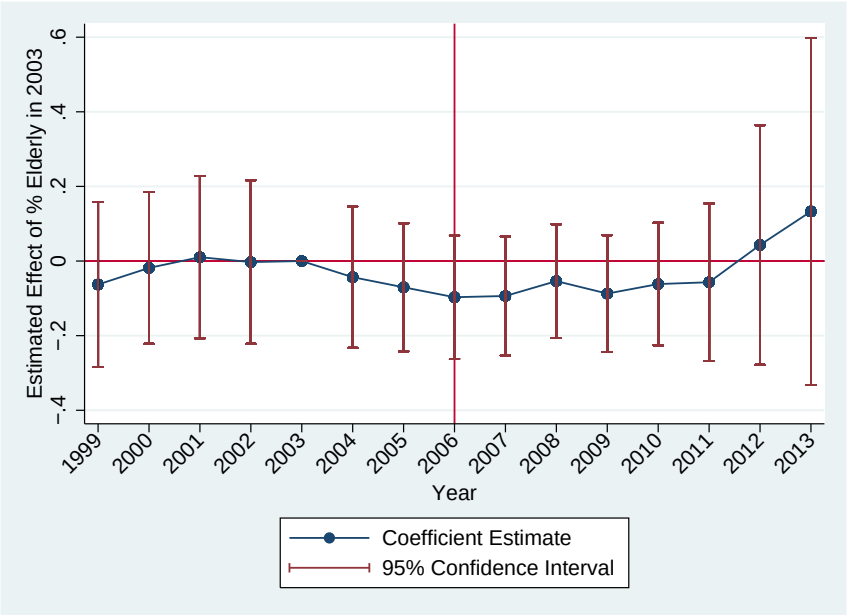
Notes: We estimate equation (1) but allow the effect of Elderly Share in 2003 to vary by year, normalizing the coefficient for 2003 to zero.

Figure V: Opioid Abuse: Substance Abuse Treatment Admissions (Ages 12-54)



Notes: We estimate equation (1) but allow the effect of Elderly Share in 2003 to vary by year, normalizing the coefficient for 2003 to zero. We use a balanced sample of states and exclude the SSDI population. Estimates using the full samples are included in the Appendix. See Appendix Figure A.3.

Figure VI: Heroin Mortality per 100,000 (Ages 0-64)



Notes: We estimate equation (1) but allow the effect of Elderly Share in 2003 to vary by year, normalizing the coefficient for 2003 to zero.

Tables

Table I: Summary Statistics

Variable	Mean	Standard Deviation
Substance Abuse Treatment per 100,000	54.23	50.93
Deaths per 100,000	4.08	2.69
Morphine Equivalent Doses per capita	9.95	4.93
Unemployment Rate	6.22	2.23
% Private Insurance	69.75	6.62
% 65+	12.62	1.86
% Part D (2006-2011)	7.55	1.36
PDMP Law	0.52	0.50
PDMP Mandatory	0.29	0.45
PDMP Real Time	0.17	0.38
PDMP 4+ Schedules	0.22	0.41

Notes: All statistics for years 2000-2011 unless otherwise noted.

Table II: Legal Distribution of Opioids

Outcome:	log(Morphine Equivalent Doses Per Capita)		
	(1)	(2)	(3)
% Elderly$_{2003}$× Post	0.029**	0.032***	0.034***
	(0.012)	(0.008)	(0.007)
% Unemployed		0.011	0.007
		(0.008)	(0.008)
% Private Insurance		0.003	0.001
		(0.004)	(0.003)
ln(Population)		0.675**	0.758**
		(0.312)	(0.301)
PDMP Law			-0.008
			(0.047)
PDMP Mandatory			0.086*
			(0.046)
PDMP Real Time			-0.053
			(0.035)
PDMP 4+ Schedules			-0.012
			(0.032)
N	612	612	612

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Standard errors in parentheses adjusted for clustering at state level. All regressions weighted by population. Controls included in all models but not shown: state fixed effects, year fixed effects, and Percentage 65+.

Table III: Opioid-Related Mortality, Ages 0-64

Outcome:	Opioid-Related Mortality per 100,000		
	(1)	(2)	(3)
% Elderly$_{2003}$× Post	0.372***	0.404***	0.360***
	(0.106)	(0.111)	(0.099)
% Unemployed		0.040	0.061
		(0.115)	(0.108)
% Private Insurance		0.010	0.017
		(0.040)	(0.036)
ln(Population)		5.166	4.201
		(3.843)	(3.795)
PDMP Law			0.314
			(0.484)
PDMP Mandatory			-0.682*
			(0.348)
PDMP Real Time			0.284
			(0.505)
PDMP 4+ Schedules			-0.411
			(0.417)
N	612	612	612

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Standard errors in parentheses adjusted for clustering at state level. All regressions weighted by population. Controls also included but not shown: state fixed effects, year fixed effects, and Percentage 65+. Population refers to size of 0-64 population.

Table IV: Opioid-Related Mortality by Age Group

Outcome:	Opioid-Related Mortality per 100,000						
				Men			
Age Group	10-19	20-29	30-39	40-49	50-59	60-64	65+
% Elderly in 2003	0.008	0.440**	1.062***	0.559***	0.456***	0.170*	-0.009
	(0.038)	(0.212)	(0.253)	(0.207)	(0.163)	(0.094)	(0.032)
				Women			
Age Group	10-19	20-29	30-39	40-49	50-59	60-64	65+
% Elderly in 2003	0.004	0.352***	0.460***	0.458**	0.211	0.064	0.021
	(0.014)	(0.118)	(0.168)	(0.182)	(0.134)	(0.083)	(0.076)
N	612	612	612	612	612	612	612

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Standard errors in parentheses adjusted for clustering at state level. All regressions weighted by population. Controls also included but not shown: state fixed effects, year fixed effects, Percentage 65+, unemployment rate, private insurance rate, log of population size, and four PDMP indicators.

Table V: Opioid-Related Substance Abuse Treatments, Ages 12-54

Outcome:	Opioid-Related Substance Abuse Treatment Admissions Per 100,000				
	(1)	(2)	(3)	(4)	(5)
% Elderly in 2003	16.988***	15.178***	14.749***	14.662***	13.423***
	(3.283)	(3.335)	(3.249)	(3.391)	(3.116)
% Unemployed		-5.476	-5.367	-5.274	-4.486
		(4.492)	(4.467)	(4.754)	(4.459)
% Private Insurance		-43.436	-73.937	-54.561	-60.026
		(118.359)	(98.529)	(106.871)	(99.677)
ln(Population)		-401.450***	-430.465***	-438.899***	-393.900***
		(130.294)	(134.163)	(138.109)	(130.292)
PDMP Law			-15.75	-9.785	-8.502
			(15.066)	(15.093)	(14.284)
PDMP Mandatory			-11.701	-13.562	-10.741
			(12.914)	(13.194)	(12.367)
PDMP Real Time			30.054*	25.301	22.592
			(15.796)	(15.909)	(14.975)
PDMP 4+ Schedules			-9.345	-7.953	-7.192
			(8.949)	(9.261)	(8.708)
Sample	Full	Full	Full	Balanced	Balanced
Population	All	All	All	All	Exclude SSDI
N	587	587	587	516	516

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Standard errors in parentheses adjusted for clustering at state level. All regressions weighted by population. Controls included in all models but not shown: state fixed effects, year fixed effects, and Percentage 65+. Population refers to size of 12-54 population. "Balanced" uses the sample of states reporting to TEDS in all years 2000-2011. The "Exclude SSDI" population excludes individuals reporting labor force participation of "Retired/Disabled" or with Medicare as the expected payment source.

Table VI: Opioid-Related Substance Abuse Treatments by Age Group

Outcome:	Opioid-Related Substance Abuse Treatment Admissions 100,000					
			Men			
Age Group	12-20	21-29	30-39	40-49	50-54	55+
% Elderly in 2003	9.322***	45.240***	16.092***	4.501***	2.616***	0.130
	(3.079)	(10.685)	(4.707)	(1.451)	(0.941)	(0.218)
			Women			
Age Group	12-20	21-29	30-39	40-49	50-54	55+
% Elderly in 2003	8.062***	42.864***	13.551***	3.416***	1.141*	-0.102
	(1.494)	(6.994)	(3.346)	(1.108)	(0.602)	(0.129)
N	516	516	516	516	516	516

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Standard errors in parentheses adjusted for clustering at state level. All regressions weighted by population. Controls also included but not shown: state fixed effects, year fixed effects, Percentage 65+, unemployment rate, private insurance rate, log of population size, and four PDMP indicators. Sample limited to states reporting to TEDS in all years 2000-2011. SSDI population excluded.

Table VII: Relationship Between Opioid Distribution and Harms

Outcomes:	Deaths Per 100,000			Admissions Per 100,000		
	(1)	(2)	(3)	(4)	(5)	(6)
MED Per Capita	0.308***	0.333***	0.271***	6.859***	13.311***	8.541***
	(0.056)	(0.095)	(0.084)	(1.825)	(2.612)	(1.717)
Ages	0-64	0-64	All	12-54	12-54	12+
Estimator	OLS	IV	IV	OLS	IV	IV
Mean Dep. Var. (2006-2011)	5.87	5.87	5.26	122.68	122.68	75.11
N	612	612	612	587	587	587

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Standard errors in parentheses adjusted for clustering at state level. All regressions weighted by population. Controls also included but not shown: state fixed effects, year fixed effects, Percentage 65+, unemployment rate, private insurance rate, log of population size, and four PDMP indicators. Population refers to size of population for the relevant age group. The excluded instrument is % $\text{Elderly}_{2003}\times$ Post. MED = morphine equivalent doses. The mean MED per capita in 2006-2011 was 13.0.

Table VIII: Robustness Tests

	Excluding 2006		
	(1)	(2)	(3)
	log(MED Per Capita)	Deaths per 100,000	Admissions per 100,000
% $\text{Elderly}_{2003}\times$ Post	0.037***	0.406***	16.931***
	(0.008)	(0.105)	(3.538)
N	561	561	473
	Excluding MA and OR		
	(4)	(5)	(6)
	log(MED Per Capita)	Deaths per 100,000	Admissions per 100,000
% $\text{Elderly}_{2003}\times$ Post	0.034***	0.366***	15.180***
	(0.007)	(0.101)	(3.425)
N	588	588	492
	Excluding FL		
	(7)	(8)	(9)
	log(MED Per Capita)	Deaths per 100,000	Admissions per 100,000
% $\text{Elderly}_{2003}\times$ Post	0.020**	0.406**	11.975**
	(0.008)	(0.162)	(4.996)
N	600	600	504

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Standard errors in parentheses adjusted for clustering at state level. All regressions weighted by population. Controls also included but not shown: state fixed effects, year fixed effects, Percentage 65+, unemployment rate, private insurance rate, log of population size, and four PDMP indicators. Columns 1-3 exclude 2006. Columns 4-6 exclude Massachusetts and Oregon. Columns 7-9 exclude Florida. MED = morphine equivalent doses. TEDS estimates use balanced sample and exclude SSDI population.

Appendix

Figures

Figure A.1: Elderly Share in 2003

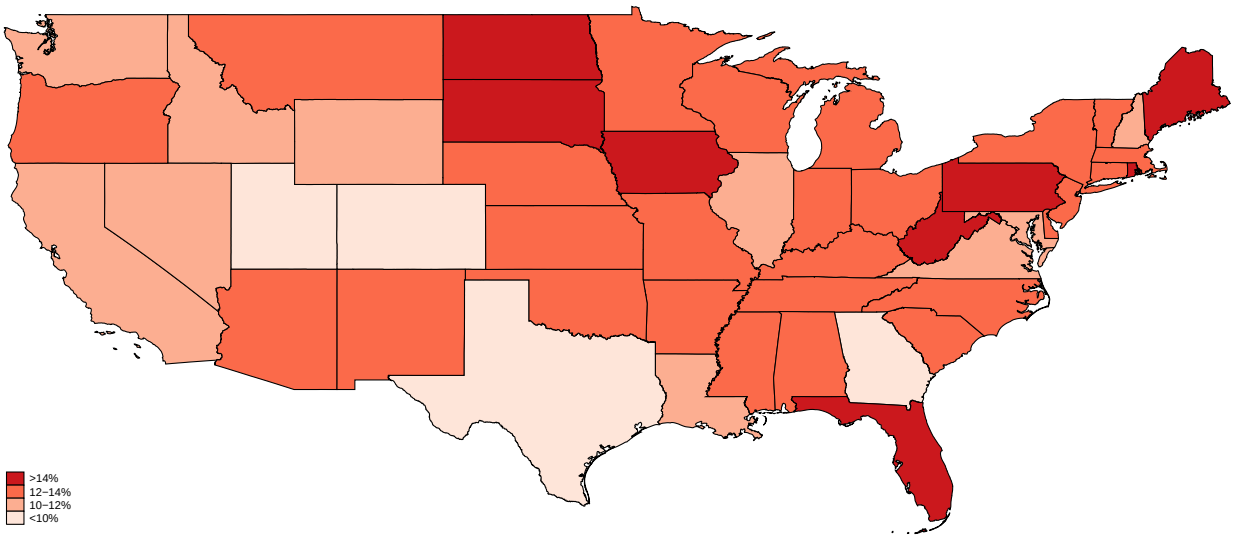
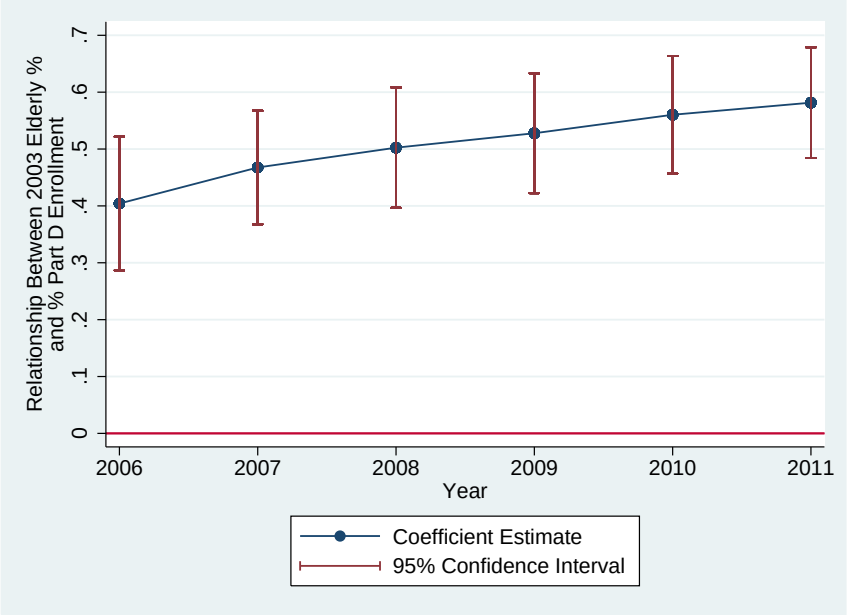
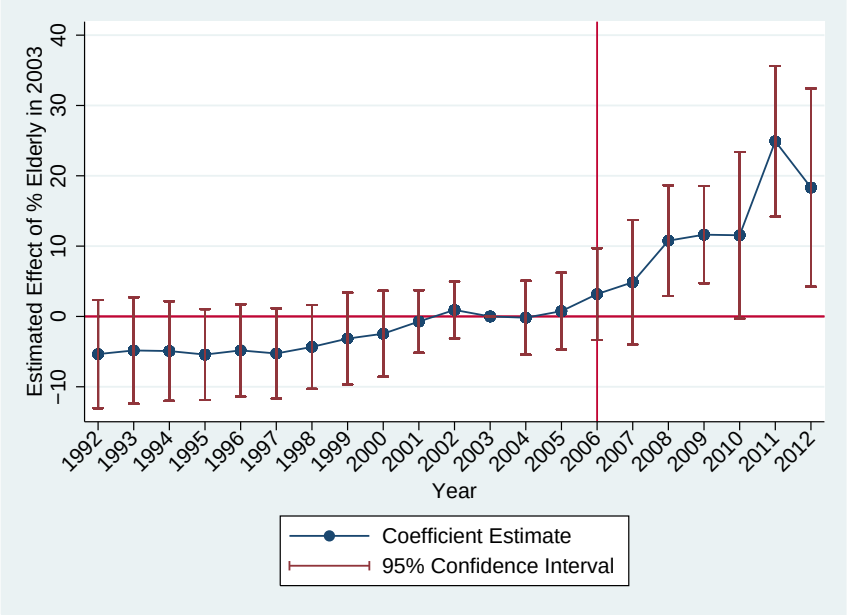


Figure A.2: Relationship between % Elderly in 2003 and % Enrolled in Part D



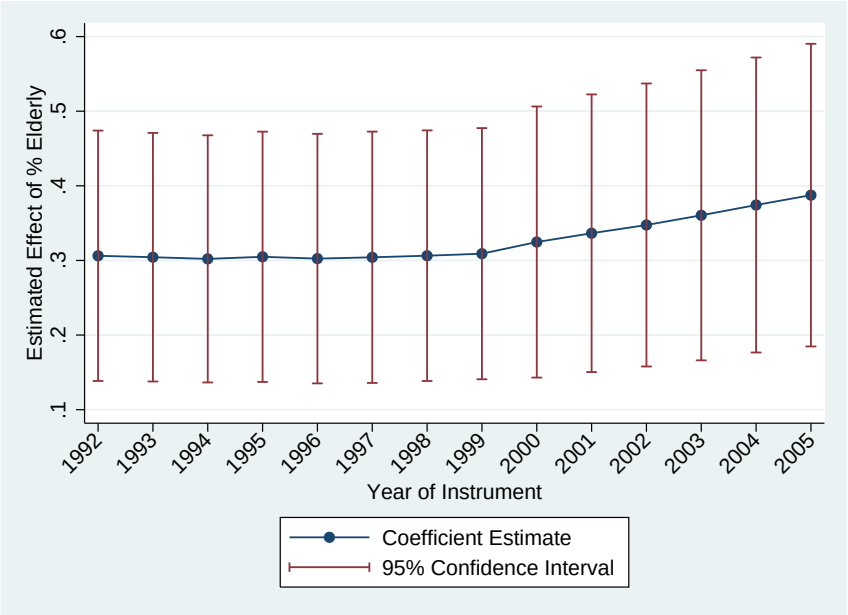
Notes: We regress the percentage of the population enrolled in Part D on the percentage of the 2003 population ages 65+. We perform this cross-sectional regression by year.

Figure A.3: Opioid Abuse: Substance Abuse Treatment Admissions (Ages 12-54)



Notes: We estimate equation (1) but allow the effect of Elderly Share in 2003 to vary by year, normalizing the coefficient for 2003 to 0. We use the full sample – all states and including the SSDI population.

Figure A.4: Mortality Effects: Varying Year to Calculate Initial % Elderly



We replicate the model presented in Column (3) of Table III but vary the year used to construct initial elderly share. The x-axis marks the year used to construct this measure. The y-axis denotes the coefficient estimate when that year is used.

Tables

Table A.1: MEPS Analysis

Panel A: Opioid Prescriptions				
	(1)	(2)	(3)	(4)
(Age $\geq$ 65) x (Year $\geq$ 2006)	0.174**	0.191**	0.181*	0.177*
	(0.089)	(0.096)	(0.094)	(0.099)
Age Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Years (2002-2009)	All	All	No 2004-2005	No 2004-2005
Ages (59-71)	All	No 63-64	All	No 63-64
N	23,190	19,205	17,754	14,694
Panel B: ln(Price)				
	(1)	(2)	(3)	(4)
(Age $\geq$ 65) x (Year $\geq$ 2006)	-0.476***	-0.459***	-0.491***	-0.488***
	(0.121)	(0.114)	(0.142)	(0.142)
NDC x Year Fixed Effects	Yes	Yes	Yes	Yes
NDC x Age Fixed Effects	Yes	Yes	Yes	Yes
Years (2002-2009)	All	All	No 2004-2005	No 2004-2005
Ages (59-71)	All	No 63-64	All	No 63-64
N	11,995	9,978	9,230	7,697

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. In Panel A, each observation is an individual-year and standard errors in parentheses are adjusted for clustering at individual level. In Panel B, each observation is a prescription and standard errors are adjusted for two-way clustering at individual- and NDC-level. Age 65 excluded in all regressions.

Table A.2: Poisson Estimates

Outcome:	MED	Deaths (0-64)	Admissions (12-54)
ln % Elderly$_{2003}$× Post	0.475***	0.630*	1.070***
	(0.164)	(0.326)	(0.318)
N	612	612	587

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Standard errors in parentheses adjusted for clustering at state level. All regressions weighted by population. Controls also included but not shown: state fixed effects, year fixed effects, and Percentage 65+. MED = morphine equivalent doses. The log of population is also included and the coefficient estimate is constrained to equal 1.

Table A.3: Interactions with Insurance Rate

Outcome:	log(MED Per Capita)	Deaths per 100,000	Admissions per 100,000
% Elderly$_{2003}$× Post	0.022***	0.270*	13.000***
	(0.005)	(0.140)	(4.579)
% Insured$_{2003}$× Post	-0.004	-0.019	-0.944
	(0.003)	(0.034)	(1.271)
% Elderly$_{2003}$× % Insured$_{2003}$× Post	-0.005***	-0.042**	-0.358
	(0.001)	(0.017)	(0.747)
N	612	612	516

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Standard errors in parentheses adjusted for clustering at state level. All regressions weighted by population. Controls also included but not shown: state fixed effects, year fixed effects, Percentage 65+, unemployment rate, private insurance rate, log of population size, and four PDMP indicators. % Elderly$_{2003}$ and % Insured$_{2003}$ are de-meaned. MED = morphine equivalent doses.

Appendix A: Did Part D increase opioid prescriptions among the 65+ population?

Several papers compare changes in prescription drug utilization for the 65+ population around the implementation of Medicare Part D to utilization changes for other age groups. This approach isolates the effect of Part D from other secular trends in drug utilization. The literature consistently finds that Part D increased overall prescription drug utilization, but there is no research focusing specifically on opioid prescriptions. A necessary condition for our empirical strategy is that Medicare Part D increased opioid prescriptions for the 65+ population.

We use the Medical Expenditure Panel Survey (MEPS) to study changes in the number of opioid prescription for ages 66-71 relative to ages 59-64.[1] The MEPS is a nationally-representative longitudinal data set which surveys households about demographics, income, health insurance, and medical claims. The Prescribed Medicines Data Files include prescription drug claims data for each person in the data. These files were linked to the Multum Lexicon database to obtain therapeutic class variables starting in 2002. We follow Stagnatti (2015) by defining opioid prescriptions as those with therapeutic subclasses "narcotic analgesics" and "narcotic analgesic combinations." We use the 2002-2009 data files and consider each claim as a prescription, which is standard in this literature (see Alpert, 2016). The MEPS surveys households for two consecutive years so we account for the panel structure by adjusting standard errors for clustering. We estimate the following specification:

$$y_{iat} = \theta_a + \gamma_t + \rho[1(a \geq 65) \times 1(t \geq 2006)] + \varepsilon_{st}, \qquad (2)$$

where y_{iat} represents the number of opioid prescriptions filled by individual i at age a in year t. The specification includes age and year fixed effects. The parameter of interest is coefficient on the interaction of the implementation of Part D and an indicator for ages 65+.

We present the main estimates in Column 1 of Table A.1. The estimate implies that individuals ages 65+ increased the number of annual prescriptions by 0.174 more prescriptions than individuals ages 59-64. While the literature often relies on large pharmacy claims data, this estimate is statistically significant at the 5% level despite the relatively small sample.

We replicate this analysis in Column 2 but exclude ages 63 and 64. Alpert (2016) provides evidence of important anticipation effects with respect to Medicare Part D. Excluding

[1] We exclude age 65 individuals since they would only have a partial year with Part D eligibility.

these ages should reduce concerns that the control group is also "treated" by Part D. We find similar estimates when we exclude 63-64 year olds. Alpert (2016) shows that the anticipation effects occurred in 2004-2005 since Part D was announced at the end of 2003, providing individuals the opportunity to alter prescription drug utilization given the intertemporal price changes. In Column 3, we exclude 2004 and 2005 from the analysis and estimate a similar effect. In Column 4, we exclude 2004-2004 *and* ages 63-64. Again, we observe similar effects.

We have also estimated the above models using Poisson regression to estimate proportional effects. The evidence (not shown) is consistent with the estimates presented in Table A.1, which is not surprising given that the pre-Part D utilization rates between these two groups are relatively similar.

In Panel B of Table A.1, we present corresponding estimates of the effect of Part D on the price of opioids. Part D decreased out-of-pocket prices for the 65+ population, driving the increased utilization. We estimate

$$\ln(p_{idat}) = \theta_{da} + \gamma_{dt} + \varphi[1(a \geq 65) \times 1(t \geq 2006)] + \upsilon_{st}, \qquad (3)$$

where p_{idat} is the out-of-pocket price of National Drug Code (NDC) *d* purchased by individual *i* of age *a* in year *t*. We control for interactions based on NDC-age and NDC-year. Each observation is an opioid prescription purchased in the sample for ages 59-71 (excluding 65). We adjust our standard errors using two-way clustering (Cameron et al., 2012) by individual and by NDC.

The estimates are consistent whether we account for anticipation effects. Our main estimate (Column 1 in Panel B) implies that individuals ages 65+ experienced a 48% reduction in out-of-pocket payments relative to the 59-64 population after the implementation of Part D.

Thus, we find evidence that Part D decreased the price of opioids for the Medicare-eligible population and that this price decrease led to an increase in the number of prescriptions. In Section 4.2, we study whether this individual-level increase in opioid access can be observed at a more aggregate level by studying whether elderly share predicts increases in state opioid supply. We find that higher elderly share states experienced relative increases in opioid supply after Part D implementation.

Appendix B: Additional Robustness Tests

In this section, we test for the importance of functional form assumptions by replicating our main results using Poisson regression. The dependent variable is the level of the outcome of interest and we control for the log of population with the effect of this variable constrained to equal 1. We use the log of 2003 elderly share interacted with a post dummy as our interaction term. Poisson regression has several advantages over log-linear specifications (see Santos Silva and Tenreyro (2006) for details). Related specifications produce similar conclusions.

The estimates are presented in Table A.2. We estimate significant effects for our outcomes and the magnitudes imply similar effects as the linear estimates presented in the main text. Functional form assumptions do not appear to be driving our results which is consistent with the small pre-Part D differences in the outcomes based on elderly share.

Figure A.4 replicates Column (3) of Table III. However, we vary which year is used to construct the "pre-Part D elderly share" measure. We find consistent estimates regardless of which year is used, implying that there is nothing special about 2003 elderly share which is driving our conclusions.

Appendix C: Physician Prescribing Behavior

In the main text, we consider several reasons why it is unlikely that our results can be explained by changes in physician prescribing patterns. We provide a more explicit test here. Physician prescribing spillovers would imply that elderly share should predict even larger increases in states with high health insurance rates for the under-65 population. Individuals with health insurance are more likely to seek care from a physician so the spillovers resulting from high elderly share would primarily affect high-insurance states. We fix the private health insurance rate in 2003 (using data from the Current Population Survey) and interact this variable with 2003 elderly share variable, examining whether there were differential effects starting in 2006. We control separately for the post-Part D effects of the 2003 private insurance rate. The estimates are presented in Table A.3 for our three outcomes. For distribution and deaths, we actually observe *negative* and statistically significant effects of our triple-difference term. This negative relationship is evidence that nonmedical opioid acquisition (and use) and medical acquisition are potentially substitutes. For example, people with chronic pain but without health insurance may obtain diverted opioids without instructions on use (or may acquire more

dangerous and addictive opioids), leading to greater harms than if they had been prescribed the opioids legally. We do not observe any evidence of positive relationship between elderly share and private insurance rates. The estimates in Table A.3 are consistent with diversion and inconsistent with medical-based spillovers.