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

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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 they are often diverted, suggesting a tradeoff between improving medical access and nonmedical abuse. We provide the first causal estimates of the relationship between the medical opioid supply and drug overdoses using Medicare Part D as a differential shock to the geographic distribution of opioids. Our estimates imply that a 10% increase in opioid medical supply leads to a 7.4% increase in opioid-related deaths among the Medicare-ineligible population, suggesting substantial diversion from medical markets. (*JEL* codes: I11, I12, I13)

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1. Introduction

Drug overdose deaths have risen steadily for the past two decades and are the leading cause of death from injuries in the United States, exceeding deaths from motor vehicle accidents.[1] Deaths from prescription opioids have been the dominant driver of this epidemic. In 2015, prescription opioids were involved in 22,598 overdose deaths, more than heroin and cocaine combined and over five times the number of opioid overdoses in 2000 (National Institute on Drug Abuse, 2015). The current level of opioid misuse is a "public health crisis" and the Centers for Disease Control and Prevention (CDC) label it the "fastest growing drug problem in the United States" and the worst overdose epidemic in U.S. history (CDC, 2012; Kolodny et al., 2015).

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.[2] Opioid therapy is an effective instrument for acute pain management, although the efficacy of opioids for chronic non-cancer pain is limited (Dowell, Haegerich, and Chou, 2016). While these drugs have legitimate medical functions, they are also highly-addictive, prone to abuse, and frequently diverted from their intended medical use. Despite clear concurrent national trends in overdoses and medical distribution of opioids since 1999 (Bohnert et al., 2011) as well as geospatial correlations (Paulozzi and Ryan, 2006), there is little empirical evidence of the causal relationship between the increasing supply of medically-intended opioids and spillovers to the nonmedical market. Is the rise in overdoses driven by patients who are overprescribed (Barnett et al., 2017) or is it nonmedical users exploiting a relatively cheap and available source of intoxication? Understanding the nature of this connection is critical for considering appropriate policies to address this epidemic. This paper starts to fill that void. This evidence is especially timely given recent legislation such as the Comprehensive Addiction and Recovery Act (CARA) and 21st Century Cures Act which provide funding to counter the rise in overdoses.

The United States is the largest consumer of opioid pain relievers, consuming twice as much per capita as the second biggest consumer (International Narcotics Control Board, 2011). The CDC estimates that there were 82.5 opioid prescriptions per 100 people in the U.S. in 2012

[1] https://www.cdc.gov/nchs/fastats/injury.htm (last accessed April 15, 2017)
[2] 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.

and 12 states had more opioid prescriptions than people (Paulozzi et al., 2014). While it has been argued that some of this is driven by inappropriate prescribing (Dowell et al., 2016), it is also clear that individuals have engaged in pharmacy and doctor seeking behaviors to try to access cheap prescription opioids for nonmedical use (Jena et al., 2014). Despite the United States' unprecedented opioid supply, little is known about the broader non-medical spillovers caused by increasing access to opioids for medical use or the role of these spillovers in explaining the high rate of drug overdoses. 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), suggesting significant scope for increases in the medical opioid supply to explain proportional rises in overdoses.

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. Furthermore, despite the public health and economic importance of the opioid crisis, there is little research dedicated to understanding its underlying causal mechanisms.

This paper studies the interaction of medical drug markets with illegal ("non-medical") drug use. This interaction is an important feature of the opioid epidemic since, unlike cocaine and heroin markets, reduced supply is not a clear policy goal given that such actions may require diminishing access to patients with legitimate medical need. 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.

While research on the opioid epidemic has established a host of characteristics which predict individual-level opioid abuse, few correlates have the potential to explain the dramatic rise in abuse over time. However, access to opioids has increased at levels proportional to the rise in overdoses and there is evidence of a positive correlation between opioid prescribing and opioid abuse (Dart et al., 2016; Bohnert et al., 2011). We calculate a 376% increase in medically-intended opioid distribution between 2000 and 2011 in the United States. This increase in opioid access coincides with a substantial drop in the cost of opioids. Consumers paid 56% of the total costs for opioid prescriptions in 2000 and only 19% in 2011 (see Appendix

Figure A.1 for the full 1996-2014 time series).[3] Recent work calculates out-of-pocket price trends for opioids and estimates that the price of a morphine equivalent dose[4] to the consumer decreased from $2.64 in 2001 to $0.54 in 2012 (Zhou et al., 2016).

We exploit large and differential geographic changes in opioid supply caused by the implementation of the Medicare Prescription Drug Benefit Program ("Part D") in 2006, a prescription drug insurance expansion targeting older segments of the population with differential concentrations across the country. Part D 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, while 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. The resulting shifts in opioid supply are large and mimic the national growth in opioid access. This has the potential to affect the Medicare-ineligible population if a primary access point is either (1) elderly relatives or friends with multiple concurrent opioid prescriptions, or (2) 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), which makes studying an insurance expansion targeting older age groups ideal. 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).

We leverage the differential effects of the implementation of Part D on states based on 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

[3] Authors' calculations using the Medical Expenditure Panel Survey (MEPS).
[4] A morphine equivalent dose is equal to 60 morphine milligram equivalent (MME) units. Opioids vary in strength so conversion factors are applied to convert a milligram of each type of opioid into morphine equivalent units.

states. Drawing on evidence presented below that states with higher elderly shares have higher Part D enrollment and that enrollment in Part D increased the amount of opioids prescribed to individuals 65 years and older, we test whether the overall supply of opioids increased disproportionately in high elderly share states. Once we establish that the medical distribution of opioids (from producers) is higher to states with a higher elderly share after implementation of Medicare Part D, we then examine whether this differential increase in opioid supply led to disparate growth in opioid abuse rates among the under-65 population as measured by overdose deaths and using a complementary measure of opioid substance abuse treatment admissions. 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 behavioral changes among under-65 individuals covered by Medicare.

This paper provides, to our knowledge, the first causal evidence that increasing prescription opioid access escalates substance abuse and mortality for populations not directly gaining medical access to these drugs. While the rise in medical access to opioids is often blamed for the opioid epidemic, it has been difficult to isolate the effect of increased access from other concurrent health care market factors (such as increased incidence due to a rise in diagnoses of musculoskeletal conditions). Moreover, it is challenging to experimentally replicate the dramatic expansion in access to opioids or disentangle the historical time series increase from other national trends. Our approach, which exploits a large and geographically diverse supply shock, provides a useful and rare opportunity to observe the consequences over time of a large and (conditionally) exogenous increase in opioid access.

We find a strong positive relationship between elderly share and the growth in prescription opioids distributed at the state level. Having determined that elderly share predicts growth in opioid access starting in 2006, we estimate differences-in-differences models to assess the differential impact of Part D on opioid-related 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 opioid-involved treatment admissions among the under-65 population. We provide evidence that these results are not due to individuals gaining prescription drug access through SSDI, state-level health insurance expansions, pill mills, substance abuse reporting issues, or concurrent demand-side shocks for

opioids. We are also careful to account for variation in the underlying age distribution of the state; our results are robust to the inclusion of state-age and age-year fixed effects. We 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. For example, using geocoded prescription drug claims data, we do not find that opioid prescriptions increased more in high elderly states among the under-65 population, ruling out alternative mechanisms such as physician prescribing spillovers or differential SSDI enrollment.

Extrapolating our results to the full time series, our evidence suggests that 73% of the dramatic growth in opioid-related overdose deaths 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. In Section 5, we discuss interpreting these results as externalities and the tradeoffs of increased medical access to opioids. We close in Section 6 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.[5] Part D substantially reduced the out-of-pocket price of prescription drugs for the Medicare population, and empirical evidence has found that these reduced prices increased use of prescription drugs.

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

[5] 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)

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.[6] This paper provides evidence that Part D had important spillovers on the health of the population not covered by the program. We will refer to overdoses among the under-65 population resulting from increase opioid supply as "spillovers," though we will discuss interpreting the additional overdoses as negative externalities in Section 5.1.

By exploiting differential eligibility for Part D, our approach allows us to study general equilibrium effects that include spillovers to the non-medical market. 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.

Health insurance expansions, more generally, 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. A key advantage of studying Medicare Part D is that it only altered prescription drug access, not medical care utilization directly, allowing us to isolate the effects of opioid supply from changes in substance abuse treatment access and other factors. By primarily studying outcomes in the Medicare-ineligible population, we further disentangle the consequences of increased opioid supply from other causal impacts of prescription drug coverage.

2.2 Data

In this section, we discuss the sources for our data. To measure supply, we rely on data which records the distribution of opioids to each state. Using prescriptions would potentially

[6] 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.

miss an important source of diversion to the extent that opioids are diverted before they are received by patients, though we will provide evidence about prescriptions in Section 4.6. For abuse, we focus on overdose deaths while also presenting complementary evidence using data on substance abuse treatment admissions.

2.2.1 Opioid Supply

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. Thus, ARCOS can be used to identify the distribution of specific opioid medications that are prescribed for medicinal purposes at the state level. We construct a measure of the seven most commonly abused opioid analgesics (Paulozzi et al., 2011; Paulozzi and Ryan, 2006): fentanyl, hydrocodone, hydromorphone, meperidine, methadone, morphine, and oxycodone (including OxyContin).[7] 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).[8] 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.

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[7] 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.

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

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 overall trends while relying on the 2000-2011 sample for our main results to narrow the time period closer to the implementation of Part D and remain consistent across all data sets.

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.

Some facilities, therefore, are excluded, but these exclusions are unlikely to cause problems for our empirical strategy for two reasons. First, 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 coincide 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. We also show that that treatment admissions for other substances (e.g., alcohol or heroin) did not differentially increase in high elderly share areas at the same time. Instead, the rise in treatment admissions is unique to opioids, suggesting that differential reporting is not an issue.

We aggregate annual case-level data on admissions for the period 1992-2012 but, as before, our main analysis uses 2000-2011. TEDS provides age in broad categories: 12-14, 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 remove any non-elderly with Medicare insurance (i.e., the SSDI population) and test the sensitivity of our results to excluding this group. Overall, the TEDS provides a useful, complementary measure to study opioid abuse that may not be captured by the overdose rate. More details about the TEDS and the construction of our outcome variable are included in Appendix A.

2.2.4 Medical Expenditure Panel Survey (MEPS)

We also make use of the MEPS data to provide supporting evidence of the interpretation of our results as well as to empirically test alternative hypotheses. The MEPS is a set of large-scale surveys of individuals, families, and their medical providers/payers that is maintained by the Agency for Healthcare Research and Quality (AHRQ). The household data are a nationally-representative longitudinal data set which surveys households about demographics, income, health insurance, and medical claims. We use the geocoded version available in the AHRQ Research Data Center (RDC) to study state-level changes over time. The Prescribed Medicines Data Files include prescription drug claims data for each person in the household surveys. These files were linked to the Multum Lexicon database[9] to obtain therapeutic class variables. We follow Stagnitti (2015) in categorizing prescriptions as opioids.

2.2.5 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 insensitive 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.

[9] See Multum.com (last access April 23, 2017) for more information.

In analyses using our full set of controls, we also condition on the adoption of prescription drug monitoring programs (PDMPs) at the state level. Prescription drug monitoring programs are recommended by the CDC and Office of National Drug Control Policy (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.[10]

2.2.6 Descriptive Statistics

We include means for our outcomes and other variables in Table 1. The percent elderly in 2003 was 12.4% 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 across states. The geographic distribution of the percent elderly is mapped in Appendix Figure A.2.

There was substantial growth in opioid supply and abuse, as shown in Figure 1, throughout our analysis period. Distribution of opioid analgesics grew during this period, rising 376% from 2000 to 2011. Per capita opioid overdose deaths also show a significant rise,

[10] 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 Schedule II, III, or IV.

increasing by 345% between 2000 and 2011. During the same time period, substance abuse treatment admissions for opioids increased by 471%.

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 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 variation in the percentage of the population ages 65+ and find that this serves as a useful predictor.[11] 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 migration correlated with opioid abuse. For example, opioid abuse may be related to local economic downturns (Hollingsworth et al., 2017). 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 in principle.[12]

[11] The dually-eligible population was eligible for prescription drug coverage through Medicaid before 2006 and, consequently, 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 in Table 7 will appropriately scale the relationships between opioid access and abuse outcomes.

[12] In practice, the results are similar if we use a time-varying measure of state elderly share.

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

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

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. As we will show, our estimates are similar whether these covariates are included or not. We will also present results controlling flexibly for age composition differences.

We are interested in the estimate of δ, which represents the differential change in the outcome experienced by high elderly share states relative to low elderly share states. We expect this estimate to be positive if Part D increased opioid access and, consequently, opioid-related substance abuse.

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.[13] 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 permits us to exclude SSDI Medicare recipients from the analysis sample. We also analyze prescriptions in the MEPS and do not find an

[13] Individuals who have received Social Security Disability Insurance benefits for 24 consecutive months 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.

increase in prescriptions for the under-65 population in high elderly share states. Even if the SSDI population were directly affected (in terms of additional medical access to opioids) by Part D, this direct effect is not systematically related to our interaction variable of interest.

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. As a result, enrollment in Part D was generally delayed relative to subsequent years and we expect that there is potentially a delayed effect in our analyses as well. In Section 4.5.2, we present estimates excluding 2006 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 *other people* eligible for Part D, we interpret these estimates as spillovers as well. A 65 year old in a high elderly share state experiences the same gain in Part D eligibility in 2006 as a 65 year old in a low elderly share state so the direct effects of access through Part D are similar. For each age group, the estimates reflect the group's propensity to acquire and abuse diverted opioids. Given the relative rarity of nonmedical opioid use among the elderly population (Paulozzi et al., 2011), we do not expect to observe large 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 examining the distribution of opioids, we use the log of morphine equivalent doses per capita since this outcome is skewed, though the results are similar if we use levels (i.e., morphine equivalent doses per capita). In Appendix Section C, we show that are results our robust to functional form (i.e., estimating proportional vs. level effects) for our outcomes. 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, here, we explore intermediate outcomes which are consistent with an increase in

supply. First, we test whether high elderly share states have higher Part D enrollment per capita. We use Part D enrollment data from the CMS aggregated by state and year to study 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 potentially important determinants of the overall increase in opioid supply. Here, we simply verify that high elderly share states have higher Part D enrollment rates after implementation.

Figure A.3 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.

Second, our empirical strategy assumes 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.[14] To 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 insured (those 66-71 years of age) to a sample of near elderly (those 59-64 years of age) in the 2002-2009 MEPS. This strategy replicates the empirical strategy found in the literature on the Part D effects on utilization. A complete description of this analysis is included in Appendix Section B. 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), implying an elasticity of

[14]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 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.

-0.6. Despite the relatively small sample in the MEPS,[15] these estimates are statistically significant. This relationship suggests that Part D had the potential to increase the supply of opioids in states with high elderly share. In Section 4.6, we provide complementary evidence by showing that high elderly states experienced a disproportionate increase in opioid prescriptions. The impact of elderly share and the introduction of Part D on the growth in state opioid supply is an empirical question and addressed more directly in the next section.

4.2 State-Level Increases in Opioid Supply

We now turn to our main models to examine whether 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 2. 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, after Part D. 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 are no time-varying confounders 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 the 2003 % Elderly variable to have a separate effect in each year. Figure 2 shows the point estimates along with 95% confidence intervals, normalizing 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 small decline in opioid distribution immediately before 2006. Beginning in 2006, we observe a steady increase in the estimated effect until 2011, consistent with the general rise in Part D enrollment during this time period. 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

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

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 faster growth in under-65 opioid abuse when Part D is implemented if spillovers are an important driving force of opioid abuse.

Figure 3 shows the differential trends in per capita non-elderly mortality and substance abuse treatment admissions. In the left panel, we see that the pre-2006 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 by analyzing 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 3, we present the trends for the above and below median states. We only use the 39 states which report treatment admissions for every year 1992-2012. Before 2006, the above median and below median states had similar trends. 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.

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 among the under-65 population increase in the high elderly states relative to the low elderly states.

4.3.2 Mortality Regression Estimates

We present our regression estimates of the differential impact of Medicare Part D on non-elderly opioid-related mortality in Table 3. 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 state-specific 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 after 2006. Again, the consistency of the estimates across the models is suggestive that the results are not being driven by unobserved time-varying shocks.

Next, we consider the independent effects of variation in age composition by accounting flexibly for differences in age structure. For Column (4), we create opioid-related deaths per 100,000 for each age under 65 (0, 1, 2, …, 64); observations are defined by state-year-age. The specification includes age-year interactions as well as state-age interactions, flexibly accounting for the effects of age composition changes in each state and the time-varying propensities of abuse by age. The estimate, presented in Column (4) of Table 3, is similar when these controls are included as the estimates using the more aggregated approach. In general, we find that the results of this paper are insensitive to flexible controls for state age structure.

Table 4 disaggregates 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 almost 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 3. The effect is 0.5 deaths per 100,000 people for the same age group for women. We also estimate large effects for other age groups highlighted by Case and Deaton (2015). The age profile increases from 20-29 to 30-39, and then steadily declines at older ages. At ages 60+, we observe no statistically significant effects at the 5% level. Appendix Figure A.4 shows the estimates for each age graphically and reveals a similar pattern.

4.3.3 Opioid Abuse Treatment Admissions

Opioid mortality, while extremely important from a public health perspective, is also a relatively 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 estimates to the balanced sample estimates.

In Table 5, 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 leads to an additional 16.99 treatments per 100,000 people after Part D. As we add controls and account for PDMP adoption, the estimate remains relatively consistent in Columns (2) and (3).[16] 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 estimates between Columns (3) and (4) should reduce concerns that our estimates are driven by changes in the states reporting information to TEDS over time.

In Column (5), we further adjust the sample and exclude admissions which report either "Medicare" as the primary expected payment source or list that the person is "Retired/Disabled." These selection criteria appropriately exclude the SSDI population.[17] The estimate is relatively unaffected (eliminating the SSDI population reduces the mean of the outcome variable so the estimates are very similar in proportional terms). In this more narrowly defined population, we estimate 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 after 2006.[18]

In Table 6, we examine this relationship across different age groups and gender, using the available age groupings in the TEDS. We rely on the balanced sample and exclude the SSDI

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[16] Adding flexible age controls has little effect. We have also replicated the Table 3, Column 4 approach for substance abuse treatment admissions using the TEDS age categories (instead of single age categories) and we find similar results.

[17] 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.

[18] 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.

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, and we find significant amounts of heterogeneity across age groups. This heterogeneity is consistent with the age trajectory estimated for mortality. For both men and women, the estimates are largest for the 21-29 age group and are three times the size of the estimated aggregate effect for ages 12-54 (Column 5 in Table 5). The point estimates steadily decrease at older ages.

Given our concern that reporting issues may obfuscate the useful information in 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 (Figure 3). Second, our results are consistent when we select the sample on states that are supplying a less noisy measure of substance abuse treatments. Finally, in Section 4.5.1, we replicate our analysis using measures of non-opioid treatments as the dependent variable. For these outcomes, the estimates are small and never statistically significant from 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 supply and abuse. In the first column of Table 7, we use OLS to estimate the relationship between state morphine equivalent doses (MED) per capita and the state opioid mortality rate for ages 0-64. We find that each additional morphine equivalent dose is associated with an increase in 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 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 4), but the effect is similar in proportional terms.

In the last three columns of Table 7, we present estimates of 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 people ages 12-54. 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.

The Table 7 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%.[19]

4.5 Robustness Tests

We test the sensitivity of our results to several factors. We previously addressed concerns about state age composition (Table 3, Column 4). Here, we consider other possible mechanisms, such as concurrent shocks in the demand for opioids, state insurance expansions during this time period, and confounding reporting trends in potentially problematic states. We provide additional sensitivities analyses examining the robustness of our findings to alternative methodological assumptions in Appendix Section C, which we briefly overview here. Appendix Table A.2 provides nonlinear estimates to test for whether estimating proportional effects (instead of level effects) provides meaningfully different results. The estimates imply similar effects.[20] In Appendix Figure A.5, 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

In this section, we study whether we observe similar results for other substances. If our opioid results are driven by some other concurrent confounding supply or demand shock

[19] 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.

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

affecting substance use more generally, then this shock should influence consumption of other substances. 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 observe relative rises in other drugs as well. Figure 3 suggests 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.

While our main motivation in this section is to test whether we observe similar changes in actual abuse of other drugs, these results also support the prior evidence that the rise in opioid-related treatment admissions is not an artifact of systematic changes in reporting. We find that the large and statistically significant rise in substance abuse treatment admissions is unique to opioids. Table A.3 present estimates for alcohol, marijuana, heroin, and total admissions not involving opioids. None of the estimates are statistically significant and they are small when compared to the equivalent estimate for opioids of 14.7 (Table 5, Column 4), especially relative to baseline. The mean treatments per 100,000 for opioids in the Table 5, Column 4 sample is 87.9 while the means for the Table A.3 outcomes are all above 167, further emphasizing the differences in the magnitude of the estimated effects. When estimating the effect on all non-opioid treatment admissions, we estimate a negative and small effect of -2.2, relative to a mean of 946.4.

But what if the supply or demand shock was specific to high risk opioid use, only observed in overdoses? The last column of Table A.3 examines deaths involving heroin. Again we find that implementation of Part D does not lead to a rise in heroin overdoses. We estimate a small, negative, and statistically insignificant estimate on heroin-related mortality.

The results in Table A.3 reduce concerns of abrupt (illegal) supply-side or demand-side opioid-related shocks coinciding with the implementation of Part D and correlating with elderly share. Note that opioids and heroin may be substitutes such that we might expect a relationship between opioid access and heroin abuse. However, if the rise in opioid deaths is caused by a concurrent and systematic demand shock, then we would expect to observe an increase in heroin abuse, though this increase would be partially muted by the availability of opioids, despite the substitution. Instead, we observe small (statistically insignificant) decreases in heroin-related mortality, which is inconsistent with a concurrent (muted or not) confounding shock. The results suggest that the rise in overdoses was unique to *prescription* opioids.

4.5.2 Excluding 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. Figure 3 confirms this hypothesis and this partial effect should bias the estimates toward zero. In Table 8, Panel A, we replicate our main analyses 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 necessarily problematic to our empirical strategy, but we test the sensitivity of our results to this assumption by replicating our analysis excluding Massachusetts and Oregon (see Table 8, Panel B). The results remain consistent with our previous main analyses for all outcomes.

4.5.4 Excluding Florida

Florida had a unique rise in opioid abuse due to the prevalence of pill mills in the state before the 2011 crackdown.[21] Since Florida is a high elderly share state, we test whether Florida is solely driving the results in Panel C of Table 8. When we exclude Florida from the analyses, our estimates are still large and statistically significant. While the estimates for legal distribution and substance abuse treatments decrease, the mortality estimate actually increases (relative to Table 3, Column 3), which implies 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 Mechanisms

We interpret the relationships estimated in this paper as evidence of economically-meaningful levels of diversion. We study drug abuse among 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

[21] It is not clear that we would want to exclude the Florida pill mills given that it has been suggested that Part D aided the creation of the pill mills in Florida (since it is a high elderly share state) and the state's rise in abuse (Meinhofer, 2016).

physician prescribing patterns, generating similar increases in opioid prescribing to the under-65 population as was observed for the 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. Similarly, we would likely expect most physician prescribing spillovers to occur to older age groups, but our age-specific results suggest stronger abuse responses at younger ages.

We test this possibility more explicitly using the geocoded MEPS. Following Stagnitti (2015) in classifying opioid prescriptions, we constructed the number of opioid prescription per person for ages 0-64 at the state level and estimated our main specification. The results are presented in Panel A of Table A.4. When we include all of our control variables, we estimate that a state with an additional percentage of elderly experienced a decline of 0.249 prescriptions among the 0-64 population after Part D. This estimate is not statistically different from zero. Because opioid prescriptions are relatively rare for younger age groups, we replicate this analysis for the 18-64 population and present the estimates in the last column. Again, we estimate a negative and statistically insignificant effect.

Note further that these tests support our prior evidence that SSDI is not confounding our main estimates. One alternative hypothesis is that elderly share predicts additional opioid prescriptions among the under-65 population after Part D through SSDI, resulting in more drug overdoses from direct medical access. However, we do not observe differential increases in prescriptions to the 0-64 (or 18-64) population in Table A.4. Further, as discussed above, our substance abuse results are similar if we exclude the SSDI population, again suggesting that SSDI is not confounding our results.

In Panel B of Table A.4, we present estimates from a specification which complements our first stage relationship (Section 4.2). We study the number of prescription to ages 65+ scaled by the entire state population.[22] The estimates for this outcome variable represent the increase in prescriptions due to Part D as a fraction of the entire state population.[23] This metric offers an

[22] When the outcome is number of prescriptions to the 65+ population scaled by the size of the 65+ population, we – as expected – do not observe any relationship. State elderly share does not predict changes in the number of prescriptions received by each 65+ person since a 65 year old in a high elderly share state experiences the same change in Part D eligibility as a 65 year old in a low elderly share state.

[23] Alternatively, we could study the total number of opioid prescriptions per person. The results are similar with this outcome.

alternative measure to show that Part D had a differential effect on the geographic supply of opioids.[24] Panel B shows that we observe a positive relationship between elderly share after 2006 and opioid prescriptions to the 65+ population scaled by the state population.[25] While the MEPS is relatively small, we still find statistically significant effects of Part D on consumption for the Medicare-eligible population (Table A.1) and more opioid prescriptions at the state-level in high elderly share states (Table A.4, Panel B). Thus, the MEPS does have the size to detect effects generated by Part D, suggesting that the lack of effects found for the under-65 population are not because of the size of the MEPS.

Overall, our analysis strongly suggests that the rise in abuse operates through nonmedical acquisition, consistent with our knowledge that most opioids used for nonmedical purposes are obtained through someone other than a physician (SAMHSA, 2015). Alternative mechanisms such as systematic price changes,[26] SSDI enrollment, and physician-prescribing spillovers are inconsistent with the available evidence.

5. Discussion

5.1 Spillovers

We interpret our estimates as "spillovers" resulting from the implementation of Part D and, more generally, from increased medical access to opioids. We find that overdoses increase among a population that does not directly gain medical access to these drugs. Nonmedical opioid use is typically the consequence of intentional action so interpreting these effects as negative externalities also requires time-inconsistent behavior, similar to the costs of smoking studied in Gruber and Köszegi (2001), on the part of the nonmedical users such that the additional supply of opioids imposes a cost on these individuals.

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[24] Opioids may be diverted before they are even prescribed so it is not clear that these estimates represent the full differential supply effect of opioids available for nonmedical use. We rely on the ARCOS data to provide a more comprehensive measure of total supply distributed to each state.

[25] The estimates in Panel B of Table A.4 are not directly comparable to the Table A.1 estimates (even beyond the differences in the empirical strategies) since Table A.4 includes a much older population, which has higher opioid consumption, while Table A.1 excludes the 72+ population.

[26] While not shown, we also find no evidence of differential price changes. In principle, the increased demand for opioids could have increased opioid prices more in high elderly share areas. This result would work against the effects that we are finding. However, given that we do not find utilization differences among the non-elderly, it is not surprising that we do not find price differences either.

Gruber and Köszegi (2001) refer to the "internalities" of smoking in a model with time-inconsistent behavior. Our estimates refer to the harms incurred by the population that is not directly prescribed the opioids so the "internalities" of additional medical access are experienced by an "external" population. Assuming that overdoses represent evidence of time-inconsistent preferences, this combination (time-inconsistent preferences plus an external population) leads us to interpret our results as evidence of externalities resulting from increased medical opioid access.

Using a value of statistical life estimate and the life expectancy loss associated with smoking, Gruber and Köszegi (2001) estimate the internality of a pack of cigarettes at $41.27 (in 2016 dollars). Using the same value of a statistical life estimate ($9.2 million in 2016 dollars) and our Table 7, Column (3) estimate, our estimate per morphine equivalent dose is $24.84. This estimate, in the same vein as the calculation in Gruber and Köszegi (2001), does not include the cost of any harms other than deaths, though these costs are potentially important. According to Zhou et al. (2016), a morphine equivalent dose in 2012 cost $2.82 in total and $0.54 out-of-pocket to consumers. Thus, we are finding large costs relative to the price paid by consumers.

5.2 Tradeoffs

This paper examines the negative spillovers resulting from increased medical access to opioids. Understanding these harms is critical for designing policy to curb overdoses. It is also important to consider the benefits of expanded access to pain relievers, such as reductions in severe pain among the Medicare Part D population. Given the necessary reliance on coarse self-reported measures of pain, this exercise is difficult in our context and generally beyond the scope of the paper.[27]

As policymakers and medical professionals consider guidelines and regulations governing appropriate opioid prescribing, it is important to consider the benefits of opioids as an effective pain management tool. However, it is also critical for policy to internalize the spillovers to the rest of the population. We find evidence of sizable increases in abuse due to diverted opioids intended for medical purposes.

[27] Using self-reported pain measures in the Health and Retirement Study (HRS) and the same empirical strategy used in Section A.1, we find no evidence of reductions in pain resulting from Medicare Part D (in fact, we estimate rather precise zero effects). Results are available upon request.

6. Conclusion

According to the CDC, 91 people die each day from an opioid overdose in the United States and at least half of those involve a prescription opioid.[28] 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 federal government has proposed new funding to help those with opioid abuse disorders obtain treatment through the 21st Century Cures Act and the Comprehensive Addiction and Recovery Act (CARA).

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 dramatic rise in drug overdoses. Using our estimates in Table 2, 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 dramatic national trends in medical opioid supply and observe the spillover effects while conditioning on time fixed effects. Studying drug abuse is always difficult given the necessary reliance on noisy measures and extreme events, but we are able to leverage a large shock to prescription drug access which has the power to identify effects, even when studying rare events

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.

[28] https://www.cdc.gov/drugoverdose/epidemic/ (last accessed on April 11, 2017)

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 345% over the same time period. Extrapolating our results to the national context, our Table 7 (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). [29] Attributing this magnitude to unintentional spillovers does not rule out the importance of more direct, complementary mechanisms. Opioid overprescribing 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.

Our results are robust to functional form assumptions, 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. Graphical evidence suggests that the effects began in 2006 and grew as Part D enrollment increased. These effects are not mirrored by other non-prescription opioid measures of drug abuse. The robustness of our findings to these sensitivity checks provides greater confidence that our results reflect true behavioral changes in abuse.

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 overprescribing, enforcing PDMPs, educating physicians on inappropriate prescribing, and managing utilization), the opioid epidemic may not be over. It is possible that as we continue to expand insurance health insurance coverage that we also further exacerbate growing trends in opioid mortality and morbidity. Optimal policy must account for the spillovers of improving medical care access to drugs that are easy to abuse and divert.

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[29] A one percent increase in opioid supply increases deaths by $\frac{0.271/5.26}{1/13.0} = 0.67\%$. Thus, a 376 percent increase leads to a 252% increase in per capita deaths.

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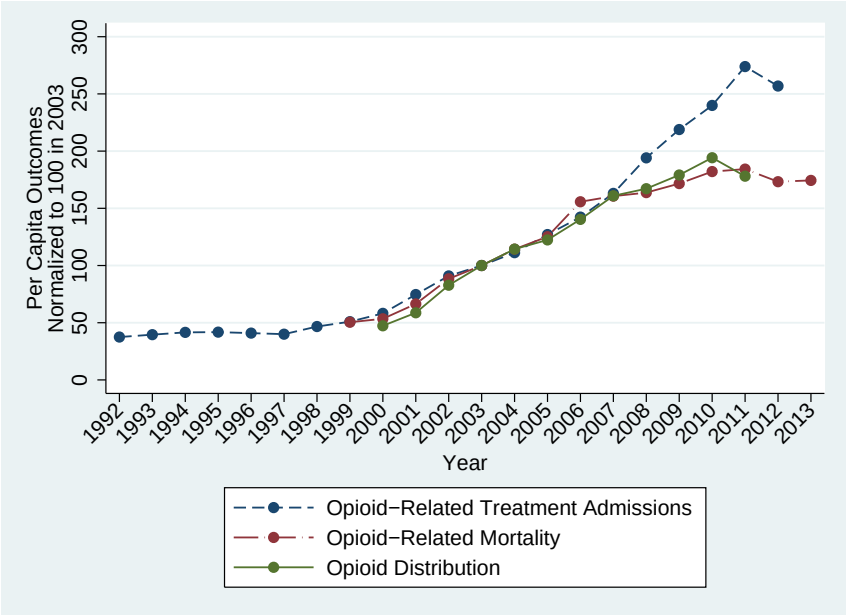
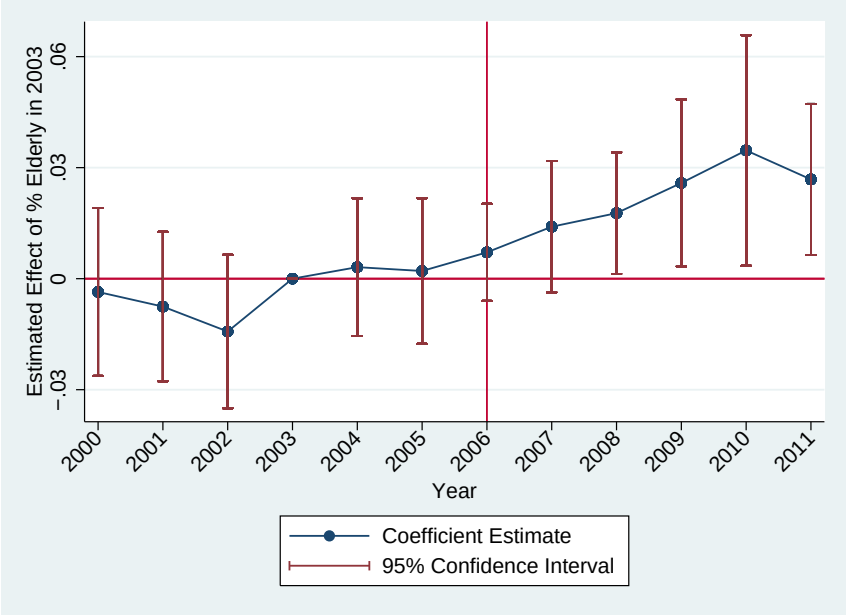
Figures

Figure 1: 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.

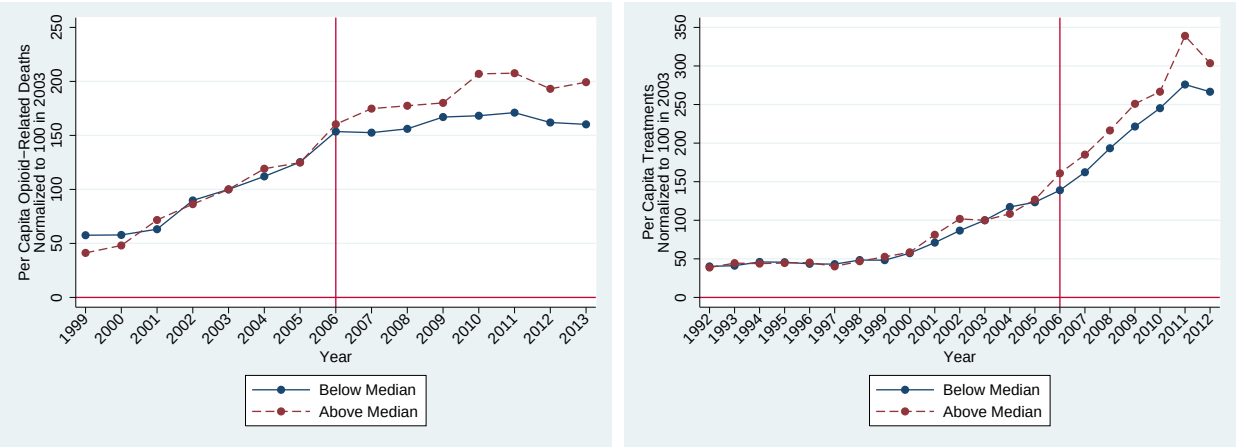
Figure 2: Opioid Distribution: Event Study



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 3: 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.

Tables

Table 1: 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
% 65+ (in 2003)	12.36	1.89
% 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 2: Medical Supply of Opioids

Outcome:	**log(Morphine Equivalent Doses Per Capita)**		
	(1)	(2)	(3)
% $\text{Elderly}_{2003}\times$ Post	0.029**	0.032***	0.034***
	(0.012)	(0.008)	(0.007)
State time-varying controls	No	Yes	Yes
PDMP Laws	No	No	Yes
Mean MEDs Per Capita	9.95	9.95	9.95
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+. State time-varying controls include the unemployment rate, private insurance rate, and the log of the population. PDMP Laws include 4 indicators described in text.

Table 3: Opioid-Related Mortality, Ages 0-64

Outcome:	**Opioid-Related Mortality per 100,000**			**Age-Specific Mortality**
	(1)	(2)	(3)	(4)
% $\text{Elderly}_{2003}\times$ Post	0.372***	0.404***	0.360***	0.320***
	(0.106)	(0.111)	(0.099)	(0.097)
State time-varying controls	No	Yes	Yes	Yes
PDMP Laws	No	No	Yes	Yes
State-Age Interactions	No	No	No	Yes
Age-Year Interactions	No	No	No	Yes
Mean Outcome	4.53	4.53	4.53	4.53
N	612	612	612	39,780

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Standard errors in parentheses adjusted for clustering at state level. All regressions weighted by population. In Column (4), observations are defined by state-year-age and the outcome is the number of opioid-related deaths per 100,000 in that cell. Controls also included but not shown: state fixed effects, year fixed effects, and Percentage 65+. State time-varying controls include the unemployment rate, private insurance rate, and the log of the population. PDMP Laws include 4 indicators described in text. In Columns (1)-(3), population refers to size of 0-64 population. In Column (4), population refers to the size of the population for that age.

Table 4: Opioid-Related Mortality by Age Group

Outcome:	**Opioid-Related Mortality per 100,000**						
				Panel A: Men			
Age Group:	10-19	20-29	30-39	40-49	50-59	60-64	65+
% $\text{Elderly}_{2003}\times$ Post	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)
Mean Outcome:	1.15	7.20	8.13	10.31	7.65	3.01	0.93
				Panel B: Women			
Age Group:	10-19	20-29	30-39	40-49	50-59	60-64	65+
% $\text{Elderly}_{2003}\times$ Post	0.004	0.352***	0.460***	0.458**	0.211	0.064	0.004
	(0.014)	(0.118)	(0.168)	(0.182)	(0.134)	(0.083)	(0.056)
Mean Outcome:	0.36	2.60	4.48	7.19	5.80	2.69	0.98

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. N=612 for all cells. 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 size refers to the size of the population for that gender and age group.

Table 5: Opioid-Related Substance Abuse Treatments, Ages 12-54

Outcome:	**Opioid-Related Treatment Admissions Per 100,000**				
	(1)	(2)	(3)	(4)	(5)
% $\text{Elderly}_{2003}\times$ Post	16.988***	15.178***	14.749***	14.662***	13.423***
	(3.283)	(3.335)	(3.249)	(3.391)	(3.116)
State time-varying controls	No	Yes	Yes	Yes	Yes
PDMP Laws	No	No	Yes	Yes	Yes
Sample	Full	Full	Full	Balanced	Balanced
Population	All	All	All	All	No SSDI
Mean Outcome:	86.69	86.69	86.69	87.91	82.03
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. State time-varying controls include the unemployment rate, private insurance rate, and the log of the population. PDMP Laws include 4 indicators described in text. "Balanced" uses the sample of states reporting to TEDS in all years 2000-2011. The "No SSDI" population excludes individuals reporting labor force participation of "Retired/Disabled" or with Medicare as the expected payment source.

Table 6: Opioid-Related Substance Abuse Treatments by Age Group

Outcome:	**Opioid-Related Treatment Admissions Per 100,000**					
			Panel A: Men			
Age Group	12-20	21-29	30-39	40-49	50-54	55+
% $\text{Elderly}_{2003}\times$ Post	9.322***	45.240***	16.092***	4.501***	2.616***	0.130
	(3.079)	(10.685)	(4.707)	(1.451)	(0.941)	(0.218)
Mean Outcome:	58.56	179.73	102.00	65.63	40.04	7.18
			Panel B: Women			
Age Group	12-20	21-29	30-39	40-49	50-54	55+
% $\text{Elderly}_{2003}\times$ Post	8.062***	42.864***	13.551***	3.416***	1.141*	-0.102
	(1.494)	(6.994)	(3.346)	(1.108)	(0.602)	(0.129)
Mean Outcome:	36.26	139.53	88.72	51.31	23.78	3.29

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. N=516 for all cells. 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. Population size refers to the size of the population for that gender and age group.

Table 7: Relationship Between Opioid Supply and Harms

Outcome:	**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 Outcome (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 8: Robustness Tests

	Panel A: Excluding 2006		
	(1)	(2)	(3)
Outcome:	**log(MED Per Capita)**	**Deaths Per 100,000**	**Admissions Per 100,000**
% Elderly$_{2003}$× Post	0.037***	0.406***	16.931***
	(0.008)	(0.105)	(3.538)
N	561	561	473
	Panel B: Excluding Massachusetts and Oregon		
	(4)	(5)	(6)
Outcome:	**log(MED Per Capita)**	**Deaths Per 100,000**	**Admissions Per 100,000**
% Elderly$_{2003}$× Post	0.034***	0.366***	15.180***
	(0.007)	(0.101)	(3.425)
N	588	588	492
	Panel C: Excluding Florida		
	(7)	(8)	(9)
Outcome:	**log(MED Per Capita)**	**Deaths Per 100,000**	**Admissions Per 100,000**
% Elderly$_{2003}$× 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: For Online Publication

Appendix A: Additional Details for TEDS Data

The TEDS 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 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). In the main text, we provide several reasons why these differences across states should not affect our results.

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 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. Our results do not change meaningfully if we only count primary substance or if we exclude non-prescription methadone.

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

Several papers compare changes in prescription drug utilization for the 65+ population after the implementation of Medicare Part D to utilization changes for individuals under 65. 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. We exclude age 65 in this analysis since those individuals are partially-treated. We follow Stagnitti (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 the 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. This estimate is statistically significant at the 5% level. While the literature often uses large data sets of pharmacy claims, we are able to statistically reject that there was no effect even with our 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 these ages should reduce concerns that the control group is also "treated" by Part D because they defer some treatments until they are eligible for Medicare. 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.

Using prescriptions as the outcome, the above result is also reinforced by the MEPS analysis discussed in Section 4.6. We find that high elderly share states experienced a rise in prescriptions for the 65+ age group scaled by the entire state population. Overall, using multiple data sets and empirical strategies, the evidence strongly suggests that the supply of opioids increased faster in high elderly share states after Medicare Part D.

Appendix C: 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. We use the log of 2003 elderly share interacted with a post dummy as our interaction term. Poisson regression has several advantages over OLS estimation of the equivalent log-linear specification (see Santos Silva and Tenreyro (2006) for a discussion) as well as related estimators such a negative binomial estimation (Wooldridge (2002), Chapter 19 details that Poisson regression is "more robust" than negative binomial regression and similar techniques). Related specifications, however, produce similar conclusions.

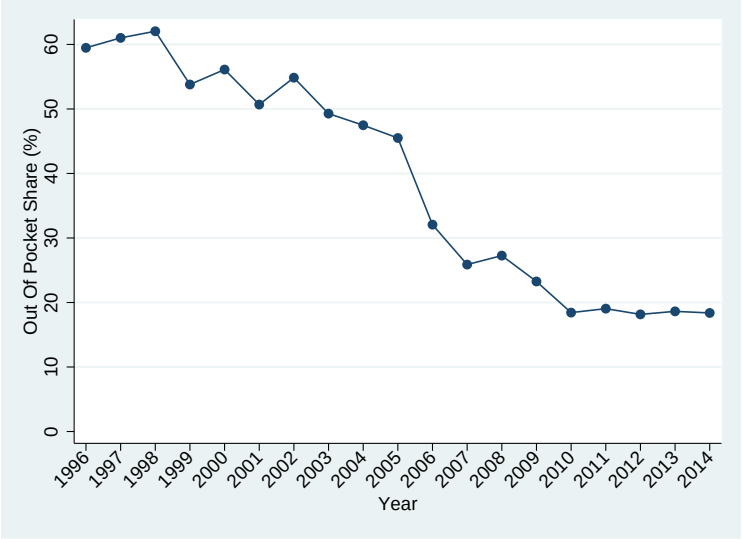
The estimates are presented in Table A.2. We estimate statistically significant effects for our outcomes and the magnitudes imply similar effects as the linear estimates presented in the main text. Functional form assumptions do not appear to be driving our results which is consistent with the small pre-Part D differences in the outcomes based on elderly share. Since the pre-intervention level differences are small, it is not surprising that level effects (OLS) and proportional effects (Poisson regression) produce similar results.

Next, we test whether our choice of fixing elderly share in 2003 is an important factor in our results. Figure A.5 replicates Column (3) of Table 3. 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. Though not shown, our 2SLS estimates (Table 7) are relatively constant regardless of which elderly share year is used in the instrument.

Appendix Figures and Tables

Figures

Figure A.1: Out-of-Pocket Share for Opioids, 1996-2014



We use the MEPS, Prescribed Medicine Files to calculate out-of-pocket share for opioids (following the categorization by Stagnitti, 2015).

Figure A.2: Elderly Share in 2003

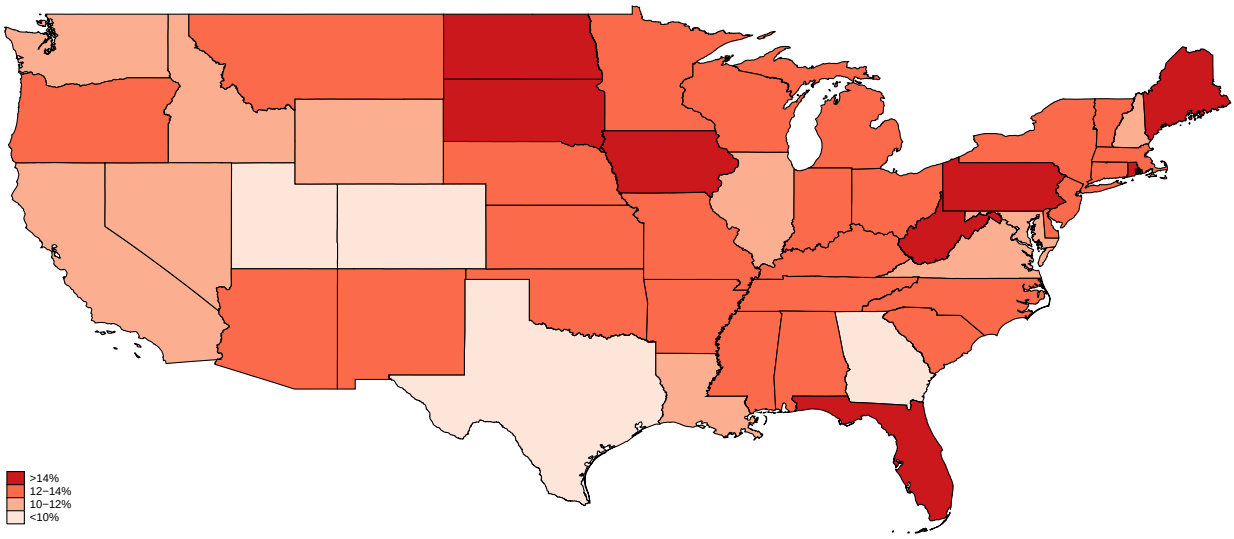
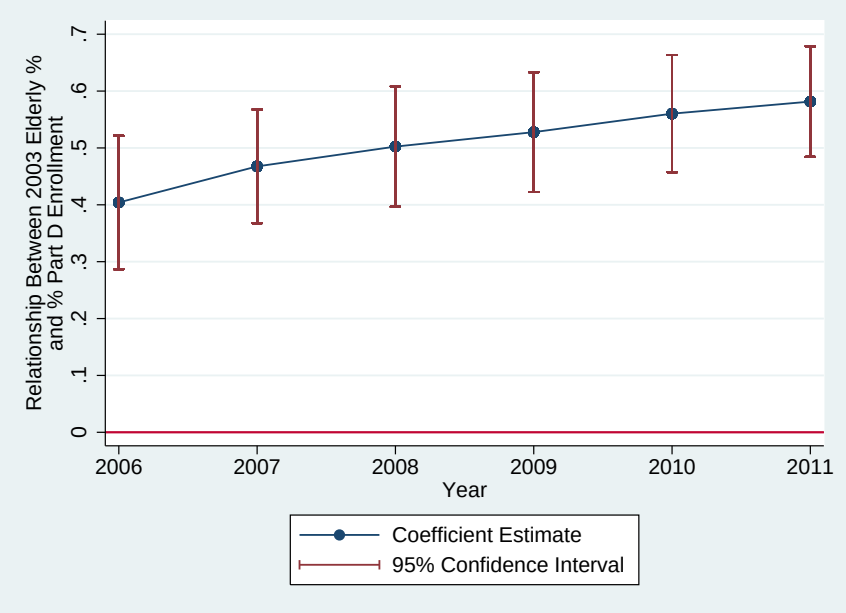
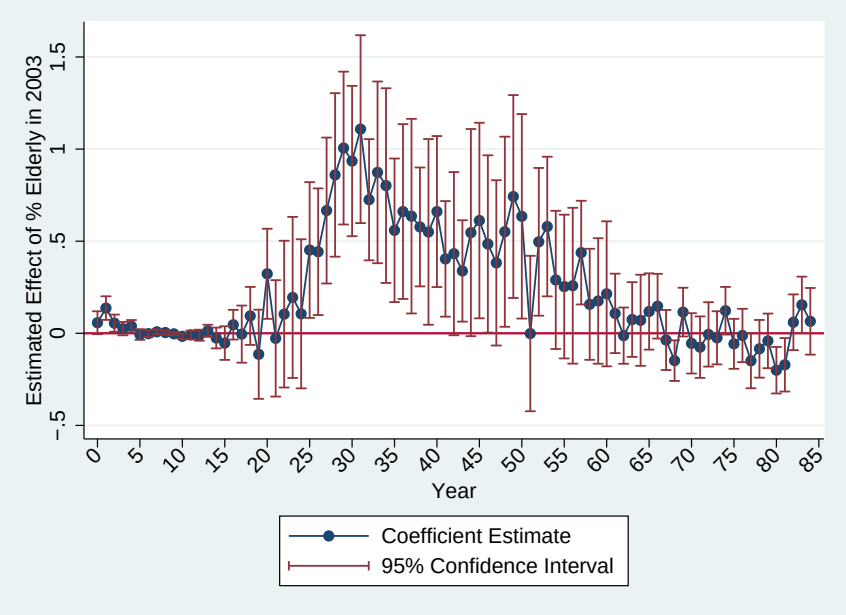


Figure A.3: Relationship between % Elderly in 2003 and % Enrolled in Part D



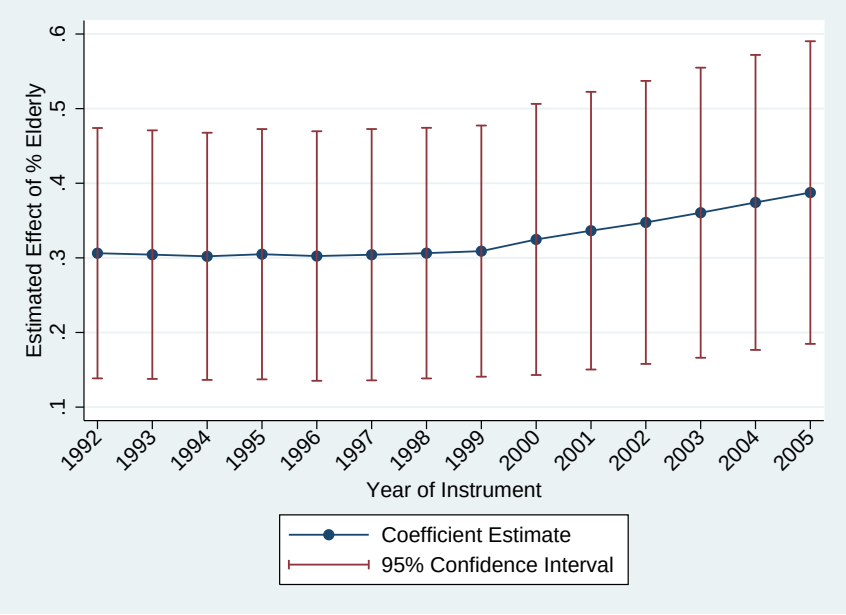
Notes: We regress the percentage of the population enrolled in Part D on the percentage of the 2003 population ages 65+. We perform this cross-sectional regression by year.

Figure A.4: Relationship between % Elderly in 2003 and Mortality Rate by Age



Notes: We estimate equation (1) for each age between ages 1 and 85. The models include all covariates, including the PDMP policy variables. Confidence intervals are adjusted for within-state clustering.

Figure A.5: Mortality Effects: Varying Year to Calculate Initial % Elderly



We replicate the model presented in Column (3) of Table 3 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. The 2003 estimate is the result reported in the paper. Confidence intervals are adjusted for within-state clustering.

Tables

Table A.1: Did Part D Increase Opioid Prescriptions Among the 65+ Population?

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)**
$\ln(\%\ \text{Elderly}_{2003}) \times \text{Post}$	0.518***	0.443**	1.072***
	(0.153)	(0.223)	(0.326)
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 (defined by the relevant ages) is also included so that the estimates can be interpreted as effects on per capita outcomes.

Table A.3: Abuse of Other Substances

Outcome:	**Treatment Admissions per 100,000** Ages 12-54				**Deaths per 100,000** Ages 0-64
	Alcohol	Marijuana	Heroin	Non-Opioid	Heroin
$\%\ \text{Elderly}_{2003} \times \text{Post}$	-2.192	5.385	4.261	-2.191	-0.038
	(5.930)	(3.938)	(3.091)	(8.262)	(0.037)
Mean Outcome Variable	638.65	368.14	167.99	946.38	0.995
N	516	516	516	516	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. Treatment admissions sample limited to balanced sample. Marijuana and heroin outcomes exclude treatment admissions that also involve opioids. Non-opioid treatment admissions include all treatment admissions that do not involve opioids.

Table A.4: Geocoded MEPS Analysis

Panel A: Prescriptions for Under-65 Population				
Outcome:	**Prescriptions Per Person**			
% Elderly$_{2003}$× Post	0.410	0.096	-0.249	-1.417
	(1.397)	(1.602)	(1.651)	(2.129)
State time-varying controls	No	Yes	Yes	Yes
PDMP Laws	No	No	Yes	Yes
Ages	0-64	0-64	0-64	18-64
N	609	609	609	609
Panel B: Prescriptions for 65+ Population as Share of State Population				
Outcome:	**65+ Prescriptions / State Population**			
% Elderly$_{2003}$× Post	0.684*	0.722*	0.825**	
	(0.358)	(0.406)	(0.407)	
State time-varying controls	No	Yes	Yes	
PDMP Laws	No	No	Yes	
N	609	609	609	

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Standard errors in parentheses adjusted for clustering at state level. All regressions weighted by population. Not all states have data in each year so we have 609 observations, instead of 612. Controls also included but not shown: state fixed effects, year fixed effects, and Percentage 65+. State time-varying controls include unemployment rate, private insurance rate, and log of population size. PDMP Laws refer to four PDMP variables discussed in text. In Panel A, the outcome is the number of prescriptions per person in the sample. In Panel B, the outcome is the number of opioid prescriptions prescribed to people ages 65+ divided by the size of the full state sample.