Do Prescription Opioids After Traumatic Injury Increase the Risk of SSDI Entry? Instrumental Variables Estimates from the Colorado All-Payer Claims Database*

Michael Dworsky†
RAND

September 2021

Abstract

I use panel data from the Colorado All-Payer Claims Database (CO APCD) to study how receipt of prescription opioids after traumatic injury affects the probability that workers transition from employment to Social Security Disability Insurance (SSDI). Although the CO APCD does not directly measure SSDI participation, I use Medicare enrollment before age 65 as a proxy for the event that an individual enters SSDI and remains enrolled through the 24-month Medicare waiting period. Workers who are opioid-naive at injury are much less likely to enter Medicare (0.58% at 48 months post-injury) than patients with a history of prescription opioid receipt at injury (2.21% at 48 months post-injury). Post-injury opioid receipt is also associated with Medicare entry. Opioid-naive patients who receive prescription opioids within 180 days post-injury are twice as likely (0.88%) to enter Medicare by 48 months post-index as opioid-naive patients with no opioids post-injury (0.44%), while patients with

*I thank conference attendees at the 2021 Conference of the American Society of Health Economists for helpful comments, especially my discussant Nicole Maestas. At RAND, I thank Colleen McCullough for constructing the analytic dataset used in this study; Katie Carman, and Asa Wilks for acquiring and processing the CO APCD files; Christine Buttorff, & Rosalie Pacula, and Asa Wilks for sharing code to measure opioid prescribing in CO APCD. At CIVHC, I thank Eddy Costa and CIVHC/HSRI team for providing data, answering questions, & reviewing output. I also thank Nicole Maestas (Harvard) & Tisamarie Sherry (RAND) for sharing a copy of their manuscript.

The research reported herein was derived in whole or in part from research activities performed pursuant to grant RDR18000003 from the US Social Security Administration (SSA) funded as part of the Retirement and Disability Research Consortium. The opinions and conclusions expressed are solely those of the author(s) and do not represent the opinions or policy of SSA, any agency of the Federal Government, or NBER. Neither the United States Government nor any agency thereof, nor any of their employees, makes any warranty, express or implied, or assumes any legal liability or responsibility for the accuracy, completeness, or usefulness of the contents of this report. Reference herein to any specific commercial product, process or service by trade name, trademark, manufacturer, or otherwise does not necessarily constitute or imply endorsement, recommendation or favoring by the United States Government or any agency thereof.

†mdworsky@rand.org, 310-393-0411, 1776 Main St., Santa Monica, CA 90401

a history of prescription opioid receipt who receive prescription opioids within 180 days post-injury are about 2.5 times as likely (2.85%) to enter Medicare by 48 months post-index as opioid-naive patients with no opioids post-injury (1.11%). It is unclear if this relationship is causal, however: instrumental variables estimates provide some support for the hypothesis that opioid prescribing increases the probability of entering SSDI, but are ultimately inconclusive.

Keywords: opioids, all-payer claims databases, social security disability, Medicare

1 Introduction

Musculoskeletal disorders, pain, and prescription opioid use are highly prevalent among DI beneficiaries and other adults with disabilities who are not in the labor force (Krueger, 2017; Morden et al., 2014; SSA, 2018). Evidence on the causal effect of opioid prescribing on SSDI entry would help SSA predict how policies targeting access to prescription opioids (such as state PDMPs) are likely to affect SSDI enrollment. It has been difficult to answer this question, however: opioid prescribing, local labor market conditions, and SSDI participation are all causally intertwined, and prescribing is endogenous to health status and the severity of pain. While cross-sectional associations between opioid prescribing, poor local labor market outcomes, and SSDI participation have been convincingly documented, more evidence about the causal relationships that drive this association are needed to guide policymaking.

In this study, I use 2012-2018 data from the Colorado All-Payer Claims Database (CO APCD) to estimate how prescription opioids affect the likelihood that unanticipated health shocks resulting in pain – specifically, traumatic injuries resulting in ED visits for patients who are covered by employer-sponsored insurance – result in SSDI entry, as proxied by Medicare entry within 4 years of an ED visit for an injury. Because opioid prescribing reflects injury severity, I use variation across health insurers in post-injury prescribing patterns to instrument for individual patients' probability of receiving prescription opioids after traumatic injury. This analysis thus provides the first individual-level estimates on the impact of opioid use on SSDI participation, providing a helpful complement to the growing literature on county-level or aggregate associations between opioids, labor market outcomes, and SSDI participation.

I identify a cohort of individuals who are a) likely to be eligible for SSDI disabled worker benefits, b) currently employed, and c) experience an unanticipated health event likely to result in pain and receipt of prescription opioids. I focus on adults aged 22-58 who are continuously covered for at least 12 months by Employer-Sponsored Insurance (ESI) in their own name, and who visit an ED for a traumatic injury in 2013 or 2014 after at least 12 months without any ED visits. Because nearly all injury patients who go on to enter Medicare within four years of injury are aged 50-58 (rather than 22-49) at injury, I also estimate regression models and descriptive statistics for the subsample of older adults (aged 50-58 at injury).

Besides being a proxy for labor force attachment, continuous ESI coverage allows me to measure patients' diagnosed comorbidities and prescription opioid receipt over the

year prior to the index ED visit: patients with 1 or more opioid prescriptions in the year before the index visit are coded as having pre-injury opioid prescriptions, and those with no prescriptions are coded as opioid-naive. Injuries occurring in 2013-2014 are identified based on principal diagnosis (ICD-9-CM) codes. Injury ED visits are identified and body part/nature of injury are coded using the Barell matrix (Barell et al., 2002), a widely used algorithm for classifying diagnosis codes related to injuries into a manageable set of injury types.

The paper contains some descriptive findings about the dynamics of opioid prescribing after injuries and associations between prescription opioids and subsequent Medicare enrollment. Opioid receipt rises sharply after an injury ED visit, with the probability of receiving any opioids increasing by 25 percentage points relative to the month before the injury. Patients who are opioid-naive at injury are much less likely to enter Medicare (0.58% at 48 months post-index) than patients with a history of prescription opioid receipt at injury (2.21% at 48 months post-index). Conditional on pre-injury opioid receipt, post-injury opioid receipt is associated with Medicare entry. Opioid-naive patients who receive prescription opioids within 180 days post-injury are twice as likely (0.88%) to enter Medicare by 48 months post-index as opioid-naive patients with no opioids post-injury (0.44%), while patients with a history of prescription opioid receipt who receive prescription opioids within 180 days post-injury are about 2.5 times as likely (2.85%) to enter Medicare by 48 months post-index as opioid-naive patients with no opioids post-injury (1.11%). Consistent with these descriptive findings, OLS regression estimates that adjust for injury type, demographics, and pre-injury comorbidities continue to show a strong association between post-injury opioid receipt and Medicare entry. Including covariates reduces the magnitude of this association by about one third, but it remains highly statistically significant.

These estimates cannot be interpreted causally because opioid prescribing is likely to reflect unobserved variation in health status and disability risk that cannot be controlled for adequately using claims data. I therefore estimated two-stage least square (2SLS) regression models using the leave-one-out mean of opioid prescribing for other injury patients covered by the same insurer. In the main sample (of patients aged 22-58 at injury), 2SLS estimates are positive but are not statistically significant. In the subsample of older adults (aged 50-58 at injury), 2SLS estimates of the effect of average daily morphine milligram equivalents (MME) for patients aged 50+ are statistically significant at 5% (when average daily MME is measured over 90 days) or 1% (when measured over 180 days). The estimated coefficients are large and positive: the coefficient for average daily MME over 180 days post-injury would

imply that a shift from the 75th to 90th percentile of post-injury prescribing for adults over age 50 (+2.74 average daily MME over 180 days) predicts an 8.1 percentage point increase in the probability of Medicare entry by 4 year post-injury, a 260% increase relative to the sample mean probability of Medicare entry for adults aged 50-58 at injury. These 2SLS estimates are marginally significant under the wild bootstrap, but the instruments may be weak, and so I view these results are suggestive at best.

Despite the limitations of the instrumental variables estimates presented here, the ability to examine even a proxy for SSDI entry in a state APCD may offer a useful direction for future work on health status, insurance coverage, and health care utilization prior to SSDI entry. In addition, the descriptive findings presented here on associations between pre- and post-injury opioid receipt and Medicare enrollment may be of interest to policymakers and clinicians for purposes of targeting interventions to help individuals remain employed or in the labor force after traumatic injuries.

The paper is organized as follows: Section 2 briefly discusses policy details relevant to Medicare coverage for SSDI beneficiaries and reviews other studies on relationships between opioids, the labor market, work disability, and SSDI; Section 3 describes the CO APCD data and details the definition of the analysis sample and the construction of key variables; Section 4 describes the OLS and 2SLS regression models estimated in the paper, including definition of the instruments; Section 5 presents the descriptive and regression results on the relationship between opioid prescribing and Medicare enrollment.. Section 6 discusses implications of the study and discusses directions for future work.

2 Background

Medicare Eligibility for SSDI Beneficiaries Social Security Disability Insurance (SSDI) provides cash benefits to workers with a disability, which is defined as "the inability to do any substantial gainful activity by reason of any medically determinable physical or mental impairment which can be expected to result in death or which has lasted or can be expected to last for a continuous period of not less than 12 months" (CFR 404.1505). In addition to cash benefits, SSDI beneficiaries become eligible to enroll in Medicare after they have received SSDI benefits for 24 months (Szymendera, 2009). SSDI benefits begin five months after disability onset, so the total duration from disability onset to Medicare entitlement

for a disabled worker who applies for and is awarded SSDI is 30 months in most cases.[1] Rupp and Riley (2012) show that nearly 100 percent of SSDI beneficiaries enroll in Medicare immediately upon completing the waiting period.

In this paper, I treat Medicare enrollment before age 65 among individuals with a work history as a proxy for having previously entered SSDI. Because the CO APCD contains data on individuals with private insurance as well as records on Medicare enrollment, the CO APCD can be used to study how health events and health care received while individuals are covered by private insurance affect the probability of subsequent SSDI entry. The key assumption for this analysis is that non-elderly Medicare enrollees with a work history are SSDI beneficiaries. Although, as catalogued by Szymendera (2009), there are a number of other reasons for non-elderly adults to become eligible for Medicare, most of these also require the individual to either be disabled or to have End-Stage Renal Disease (ESRD). Enrollment data reported by CMS indicate that, as of 2017, 98% of non-elderly Medicare enrollees in Colorado were disabled without ESRD.[2]

2.1 Related Literature

Most studies on the labor market and disability impacts of prescription opioids have focused on aggregate data. When we look across different geographic areas at a given point in time (i.e., in cross-section), drug overdose mortality is strongly associated with local economic conditions. Monnat (2018) confirms that economic distress–defined as an index reflecting labor market conditions, poverty rates, disability program participation, and other indicators of hardship–predicted higher drug-related mortality during the period from 2006-2015: a one-standard deviation increase in the economic distress index predicted a 6.4 percent increase in the age-adjusted drug overdose mortality rate. Cutler et al. (2017, 2016) similarly provide evidence of an association between state-level opioid prescribing patterns and SSDI application volumes.

However, much remains unknown about the causal relationships driving the aggregate relationship between opioids and the labor market. The cross-sectional relationships between local area opioid supply, labor market conditions, and SSDI could be driven by at least three plausible causal mechanisms with very different policy implications:

[1] The 24-month waiting period from the start of SSDI befits to Medicare eligibility is substantially shortened for SSDI beneficiaries with End-Stage Renal Disease (ESRD) and is eliminated for beneficiaries with amyotrophic lateral sclerosis (ALS) (Szymendera, 2009).

[2] Source: CMS Program Statistics, Table MDCR ENROLL AB 8. Available at `https://www.cms.gov/Research-Statistics-Data-and-Systems/Statistics-Trends-and-Reports/CMSProgramStatistics/2017/2017_Enrollment` as of September 19, 2021.

1. Use of opioids leads to reduced labor force participation, including SSDI entry
2. Chronic pain both reduces labor force participation (including SSDI entry) and leads to use of pain medication, including opioids
3. Labor force non-participation in response to declining labor demand leads to substance abuse, including abuse of opioids

Each of these theories has been substantiated to some degree in credibly identified studies using aggregate data. I briefly discuss these studies before moving on to research specifically about the effect of opioids on disability and participation in SSDI or SSI.

Effects of Opioid Supply on Labor Market Outcomes Several papers with strong research designs have shown that greater opioid prescribing in a geographic area reduces employment or labor force participation, including Aliprantis and Schweitzer (2018), Harris et al. (2020), and Beheshti (2019). A paper by Currie et al. (2019), however, raised some questions about the interpretation of these findings: they found important differences across demographic groups in the direction and significance of labor supply impacts of prescription opioids. More recently, Park and Powell (2021) found that the 2010 reformulation of OxyContin (which reduced the supply of prescription opioids but induced substitution toward heroin and other illicit opioids) reduced labor supply in geographic areas that had higher rates of OxyContin use prior to the reformulation; related work by Alpert et al. (2018) studying heroin overdose mortality strongly suggests that the mechanism that explains the reduced-form relationship found by Park and Powell (2021) is substitution from prescription opioids to illicit opioids.

Effects of Labor Market Outcomes on Opioid Use and Mortality Several recent studies have also shown a causal connection in the opposite direction, from reduced labor demand or employment to increased opioid prescribing or opioid-related overdose mortality. The most convincing papers have focused specifically on manufacturing employment. Pierce and Schott (2020) study changes in unemployment between 1990 and 2013 that resulted from the accession of China to the WTO in 2000, measuring import exposure at the local labor market level by comparing the mix of products in an area's manufacturing output to changes in tariffs. They find that exposure to Chinese import competition is strongly related to higher rates of accidental poisoning deaths (which have been driven principally by opioid overdoses), as well as higher suicide rates. A study by Venkataramani et al. (2020) compares

counties with auto plant closures between 1999 and 2016 to other counties with auto plants that did not close. They find very large mortality effects associated with large declines in labor demand, with opioid-related mortality increasing by 85 percent as of five years after the plant closure.

That said, other well-designed studies that look at different industries beyond the manufacturing sector paint a more nuanced picture of the relationship between labor market conditions and the opioid crisis. Metcalf and Wang (2019) examine impacts of coal mine employment on opioid mortality and more or less reach the opposite conclusion from Pierce and Schott (2020) and Venkataramani et al. (2020) about the relationship between labor demand and opioid overdose mortality. Specifically, they find that coal mine employment increases opioid overdose mortality. Other working papers that look at employment across all industries (not just manufacturing) also find that the effect of employment rates on opioid prescribing differs meaningfully by educational attainment and gender (Currie et al., 2019) and by the industry-level workplace injury rate (Musse, 2019).

To sum up, previous research on aggregate outcomes at the county or state unit of observation support the existence of economically significant causal connections in both directions, from opioid supply to labor market outcomes, and from labor market conditions to opioid use and overdose mortality.

Effects of Prescription Opioids on Disability Several papers have also examined the effect of prescription opioids on work disability and outcomes related to federal disability programs, such as applications, new awards, and the participation rates.

Savych et al. (2019) study opioid prescribing and temporary disability duration for workers who are injured on the job and file workers' compensation claims. They use an instrumental variables strategy that leverages variation across health care markets in the rate at which health care providers prescribe opioids. Their main findings focus on long-term opioid prescribing, which they define as receipt of one or more opioid prescriptions in the first thee months after injury and three or more opioid prescriptions in the sixth through twelfth months after injury. There are two major findings in this paper. First, Savych et al. (2019) find that long-term opioid prescribing results in more than a tripling (251% increase) of temporary disability duration relative to injured patients who received no prescription opioids. These results represent the first estimates of the causal effect of opioid prescribing on disability duration among injured workers. A second and more subtle finding is that their

instrumental variables estimates for most other opioid prescribing patters are dramatically different from regression estimates that do not attempt to isolate exogenous variation in opioid prescribing. Regression-adjusted OLS estimates show that any opioid prescribing, including short-term prescribing without longer-term prescribing, is associated with large increases (60 to 80 percent increase) in disability duration. Instrumental variables estimates for these shorter-term forms of opioid prescribing are much smaller (20 to 50 percent increase) and are not statistically significant at the five percent level.

The study by Park and Powell (2021) discussed above also estimated reduced-form impacts of the OxyContin reformulation on applications to and participation in federal disability insurance programs (including SSI as well as SSDI). They studied the SSDI application rate, the SSDI allowance rate, the probability of a favorable initial determination, and the stock of disability program beneficiaries, all measured relative to the population aged 18-64 in a state. Consistent with their findings that OxyContin reformulation reduced labor supply (likely via substitution from prescription opioids to heroin), they find that areas with higher OxyContin misuse before the reformulation experienced sizable increases in disability program applications and the proportion of adults receiving disability benefits over the next five years.

A new paper by Maestas and Sherry (2020) also leverages geographic variation in provider behavior to construct instruments for prescription opioid receipt. They use individual-level microdata from a large, nationwide claims database to measure, at the county or PUMA (couma) level, the probability that providers prescribe opioids to opioid-naive patients. The microdata allow them to take a very flexible approach to case mix adjustment so that prescribing propensities are measured after conditioning on pain-related diagnoses as well as other individual-level patient characteristics. These county-level prescribing propensities are then used to instrument for county-level opioid prescribing volumes from the CDC (derived from IQVIA) in a regression model for county-level SSDI applications and initial allowances per 100 adults aged 25-64. This is a highly credible IV strategy.

They find that increases in prescription opioid supply result in a large increase in SSDI applications per 100 adults: an increase of 10 opioid prescriptions per 100 adults (one-third of a standard deviation) results in an 8% increase in SSDI applications. In a novel use of SSA administrative data, they are able to confirm that this increase in applications is driven in large part by increased applications from workers whose SSDI applications report use of prescription opioids. They also find, consistent with other studies, that increases

in opioid supply cause worse labor market outcomes. Notably, the IV estimates are larger than the OLS estimates, which is the opposite relationship from what would be expected if endogeneity of opioids to SSDI applications were driven primarily by unmeasured health status. Maestas and Sherry suggest, plausibly, that the predominant source of endogeneity could instead be diversion of prescription opioids or illicit opioid supply.

Contribution of This Paper Like Maestas and Sherry (2020), my paper estimates the effect of prescription opioids on SSDI entry. However, my paper differs from Maestas and Sherry (2020) on a number of key dimensions. First, the outcome is different. I lack data that directly measures individual-level SSDI application or program entry, and instead focus on Medicare enrollment before age 65 as a proxy for the event that an individual enters SSDI and survives to the end of the Medicare waiting period (i.e., to the 30th month after disability onset). Second, while my identification strategy resembles the geographic variation strategies used by Maestas and Sherry (2020) and Savych et al. (2019) in that cross-sectional variation in prescribing behavior is used to construct instruments for prescription volumes, I construct instruments using variation in prescribing behavior across health insurers within geographic areas, rather than variation across geographic areas. My empirical analysis more closely resembles Savych et al. (2019), who also had microdata on prescription opioid receipt and disability outcomes for the same individuals. As in that paper, I construct a leave-one-out instrument, using the average of opioid prescribing measures for similar individuals to instrument for each individual's actual opioid receipt.

Third, this paper is distinguished from the previous work on the causal effect of prescription opioids and disability outcomes because I study the effect of prescription opioids on SSDI entry at the individual (rather than the county or state level), and therefore am attempting to estimate a different quantity than the aggregate-level relationship identified by Maestas and Sherry (2020). Both quantities should be of interest: the aggregate-level relationship is more directly relevant for policy interventions focused on changing prescribing behavior or opioid supply, while the individual-level relationship can be interpreted more directly in terms of the behavioral or medical impacts of opioid prescribing on the risk of transition to SSDI, and thus may shed more light on the value of other interventions that focus on higher-risk individuals. The context of my study is also much narrower than in studies that used multi-state data, as my estimates are focused on adults in Colorado with employer-sponsored insurance who experience a traumatic injury. Despite the narrower scope of this study, it may also be of interest for demonstrating the feasibility of using panel data

derived from a state APCD to study transitions from work to SSDI.

3 Data

Data for this study come from the Colorado All-Payer Claims Database (CO APCD). The Center for Improving Value in Health Care (CIVHC), which administers the CO APCD, provided RAND with an extract of the CO APCD for all non-elderly adults (ages 19-64) observed in the CO APCD between January 2012 and December 2019; the most recent extract was created and transferred to RAND in the summer of 2020. As I discuss below, the CO APCD also received data on Medicare Fee-for-Service (Parts A and B) from the federal Centers for Medicare & Medicaid Services (CMS), but with a much greater data lag. Data on enrollment and claims in Medicare FFS were available only through the end of 2018 at the time when data were last refreshed for this study in summer of 2020. I therefore limit the analysis to claims and enrollment in the years 2012-2018. This study builds on a larger, ongoing project at RAND that is using the CO APCD to study transitions in health insurance coverage more broadly.

The basic structure of the CO APCD is similar to APCDs in Massachusetts and many other states. Claims and enrollment data are regularly submitted to the CO APCD by over 50 payers covering Medicaid, Medicare, and the largest commercial payers participating in the group and nongroup markets. Separate files are submitted for medical claims, prescription drug claims, and (in CO and many, but not all other states) dental claims. This study used the prescription drug claims to measure prescription opioid receipt (from all APCD-covered payers). I also used the medical claims data to identify injury emergency department (ED) visits, and to construct indicators for comorbidities (diagnoses) appearing on medical claims in the year prior to the index ED visit.

Payers also must submit *eligibility* files containing identifiers and plan information about all individuals who are covered by insurance for one or more days in each month. The unit of observation in the CO APCD eligibility file is thus a person-month-plan. As I discuss below, eligibility and claims files from a given plan can be linked together using a *member ID* number, typically the person's health insurance card number.

The research question of this paper focuses on transitions from ESI to Medicare. It is possible to study these transitions in the CO APCD because the CIVHC creates a longitudinal person identifier (the *member composite ID*) that makes it possible to track individuals across multiple payers, both at a single point in time and over time. The composite ID is

created by CIVHC using name, date of birth, and social security number, as well as other information such as residential address. A substantial data cleaning effort was necessary due to a high volume of anomalies that were identified after initial processing of the data. These efforts included quality checks for the eligibility data, member data, and the crosswalk between plan-specific member IDs and longitudinal member composite IDs.

Several variables from the eligibility file were crucial to the analysis conducted in this study. The primary outcome variable is an indicator for Medicare coverage. Medicare coverage was coded primarily on the basis of the insurance product type code. More details on ascertainment of Medicare coverage are provided below. In the broader project that this study builds on, insurance product type code and other variables describing plan characteristics were used to assign individuals to five broad coverage types in each month: Medicare, Medicaid, ESI, ACA Marketplace, or Other (non-Marketplace) Nongroup coverage. I use this measure of ESI coverage to identify individuals with 12 months or more of continuous coverage. In addition to determining the type of coverage, the eligibility file was used to identify the policyholder (or subscriber) on the ESI plan. I used this information to limit the analysis sample to ESI policyholders, who I assume were employed at the time of the index ED visit.

Payers Covered by the CO APCD Data submission is mandatory for covered payers, with the notable exception that self-funded employer plans regulated under the federal Employee Retirement Income Security Act (ERISA) cannot be compelled to submit data to state APCDs under the Supreme Court's 2016 ruling in *Gobeille v. Liberty Mutual Insurance Company*. With the exception of health insurance offered by state and local government agencies or by churches, self-funded ESI coverage is generally regulated under ERISA and thus is observed in the APCD only for employers and plan administrators that choose to voluntarily submit data.

Data from self-funded plans (including state and local government ESI coverage not regulated under ERISA) were accordingly excluded from my analytic dataset, so that the data on ESI coverage and claims used in this study reflect only fully-insured ESI plans. Because most (67% in 2020) people covered by ESI in the US are covered by self-funded plans, the exclusion of self-funded plans from this study poses a limitation of this study's external validity for the entire ESI sector. Private insurers that cover fewer than 1,000 Colorado residents are also exempt from mandatory submission to the CO APCD.

Notwithstanding these limitations, however, the CO APCD provides data on the entire universe of adults enrolled in fully-insured ESI plans in Colorado. A comparison of the commercial insurance carriers that submitted data to the CO APCD to market share data reported in the NAIC Supplemental Health Care Exhibit indicates that participating insurers accounted for 97 to 99 percent of covered lives in the small group and large group markets in each year from 2011 to 2017.

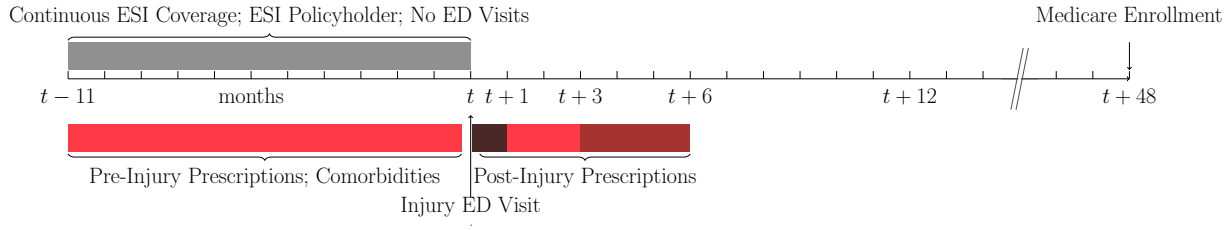
Cohort Definition The population of interest in this study consists of working adults who receive Emergency Department (ED) care for an injury. In order to isolate adults who are working at the time of their injury, I select adults who had at least 12 months of continuous ESI coverage as a policyholder priort to the month of the index ED. (The 12-month continuous coverage period includes the month of the index ED visit, so the requirement is 11 full months of continuous coverage leading up to coverage in the month of the index ED visit.)

Because the APCD does not capture information on employment status or income, I limit attention to ESI policyholders with a year of continuous coverage. I make the assumption that ESI policyholders are working and view this cohort as individuals who are employed. I cannot rule out the possibility that some of these individuals may have COBRA coverage, potentially including extended COBRA coverage available to SSDI beneficiaries during the Medicare waiting period, however. Finally, a small number of patients had Medicare enrollment records during the pre-injury period: these individuals are excluded from the sample.

To allow a four-year follow-up period in which to observe Medicare enrollment, I must focus on patients with an index ED visit no later than December 2014. Also, to allow for a full year of pre-index data in which to verify continuous coverage and measure baseline opioid receipt and comorbidities, I must focus on patients with an index ED visit no earlier than January 2013. The sample is therefore limited to individuals meeting the above criteria whose index ED visit is in 2013 or 2014.

Figure 1 illustrates this cohort definition and the periods of time, relative to the index ED visit, when various the key explanatory variables and outcomes are measured. As described more fully in Section 4, this paper uses the panel dimension of the data solely to construct variables on individuals'

Figure 1: Cohort Definition and Timing of Opioid and Medicare Enrollment Measures Relative to Index ED Visit



Identifying Emergency Department (ED) Visits for Injury ED visits were identified using bill type codes appearing on claims submitted to the APCD. ED visits for injuries occurring were identified based on principal diagnosis (ICD-9-CM) codes appearing on the APCD claim header file. Injury ED visits are identified using the Barell matrix (Barell et al., 2002; see here for details), a widely used algorithm for classifying diagnosis codes related to injuries into a manageable set of injury types. An ED visit was coded as an injury ED visit if the principal diagnosis appears in the Barell matrix.

To summarize, the criteria for sample inclusion are as follows:

- Patient had ED visit for an injury (defined as principal diagnosis code on ED visit bill header matching diagnosis codes appearing in Barell injury matrix) in 2013-2014
- Patient had 12+ months of continuous ESI coverage at time of injury ED visit
- Patient had no prior ED visits during 12+ month continuous coverage spell preceding index ED visit
- Patient was policyholder on ESI coverage
- Patient was aged 22-58 in month of index ED visit

There are 9,150 such individuals in the CO APCD. Table 1 shows demographic summary statistics for this sample. The mean age at injury is 41.5. Age range at injury is 22-58. Quartiles of age are 33, 42, and 50. 42.3% of patients are female.

Coding Injury Types The Barell matrix assigns ICD-9 codes to a site and type of injury, e.g., Forearm and Elbow (site) Fractures (type). To describe the mix of injuries, and to define control variables for the regression analysis, I constructed indicators for all injury type/site combinations with 100 or more injuries in the full sample (of all adults in the cohort aged 22-58 at injury). Type/site combinations with fewer than 100 injuries in the

full sample were assigned to a residual category for each injury site (labeled "Other Injury Type") to avod disclosing results for small cells. This results in 35 injury types, which I control for in regression models using fixed effects. Table A1 lists the distribution of injury type (as coded in our regression models) the full sample. Open wounds to the hand and fingers are the most common injury type.

Subgroup Analysis of Older Adults Medicare enrollment within four years post-index is overwhelmingly concentrated among older adults (those aged 50 and above at the injury date). Of 73 injury patients who enter Medicare within 48 months post-injury, 55 are age 50 or higher at the index ED visit. There are several mechanisms that might explain this pattern:

1. Older adults may experience more severe injuries. This is consistent with findings from the occupational safety literature that older adults are at lower risk of workplace injury than younger adults, but have more severe injuries conditional on occurrence.
2. Conditional on severity, older adults may recover more slowly and less completely.
3. Conditional on injury severity and post-injury health status, adults over age 50 are likely to enter SSDI due to use of the "vocational grid" criteria in the SSDI evaluation process (see Chen & van der Klaauw, 2008).

I therefore repeat all descriptive and regression analyses in the paper for the subsample of patients who are age 50+ at the index ED visit. There are 2,442 such individuals in the sample. Appendix Table A2 lists the distribution of injury type (as coded in our regression models) for adults aged 50-58 at injury.

Table 1: Demographics of Analysis Sample

Sample Descriptives	Mean	SD	Min	p25	p50	p75	Max	N
Age	41.5	9.6	22	33	42	50	58	9150
Female	42.30%							

Measuring Opioid Prescribing in the CO APCD Opioid prescribing was measured using prescription claims submitted to the APCD. Colleagues at RAND who are using the CO APCD to study geographic variation in opioid prescribing shared a set of algorithms they had developed to clean the prescription data and convert information on prescription claims (such as national drug code and days' supply) into morphine milligram equivalent

(MME) amounts, allowing comparison of the volume of opioids prescribed across different medications. For each prescription, the algorithms calculated the daily MME dose that would result from taking the medication as instructed. These daily MME amounts were then averaged over time to construct measures used in my analysis.

The population of interest for this study consists of adults who experience traumatic injury and seek care in an ED. My main explanatory variables therefore measure prescription opioids dispensed to these patients during various windows of time relative to the index ED visit. Specifically, the average daily MME was then calculated for windows of time (30, 90, 180, and 365 days) before and after the index ED visit, with prescriptions filled on day of the ED visit assigned to the post-index period. Pre-index opioid prescribing measures are used to identify opioid-naive patients (based on the 365 days leading up to the index ED visit). Post-index opioid prescribing measures are the explanatory variables of primary interest in this analysis.

Table 2: Pre-, Post-Injury Opioid Prescribing (Average Daily MME)

Avg Daily MME over ...	Mean	SD	Min	p25	p50	p75	p90	p95	Max	N
30 Days Pre-Index	0.80	10.80	0.00	0.00	0.00	0.00	0.00	0.00	630.00	9,150
90 Days Pre-Index	0.77	10.45	0.00	0.00	0.00	0.00	0.00	1.00	630.00	9,150
365 Days Pre-Index	0.70	10.14	0.00	0.00	0.00	0.00	0.41	0.97	616.44	9,150
Avg Daily MME over ...										
30 Days Post-Index	4.23	14.85	0.00	0.00	0.00	3.23	12.40	21.77	670.65	9,150
90 Days Post-Index	2.33	11.68	0.00	0.00	0.00	1.37	5.14	10.16	617.14	9,150
180 Days Post-Index	1.67	10.29	0.00	0.00	0.00	0.83	3.31	6.63	608.62	9,150

Table 2 shows summary statistics for pre- and post-injury opioid prescribing measures for the full sample (ages 22-58 at the index ED visit). In interpreting these summary statistics, it is important to note that person-day observations with no opioid prescriptions are coded as zeroes. The daily MME amounts reported in this table are low relative to typically prescribed doses because they are unconditional, and not conditional on prescribing. Put differently, the opioid measures reported in Table 2 capture both variation in prescribing behavior on the extensive margin (whether any prescriptions are dispensed) and the intensive margin (the duration of dispensed prescriptions and the daily MME prescribed)

Table 2 indicates that average opioid prescribing increases sharply after the index

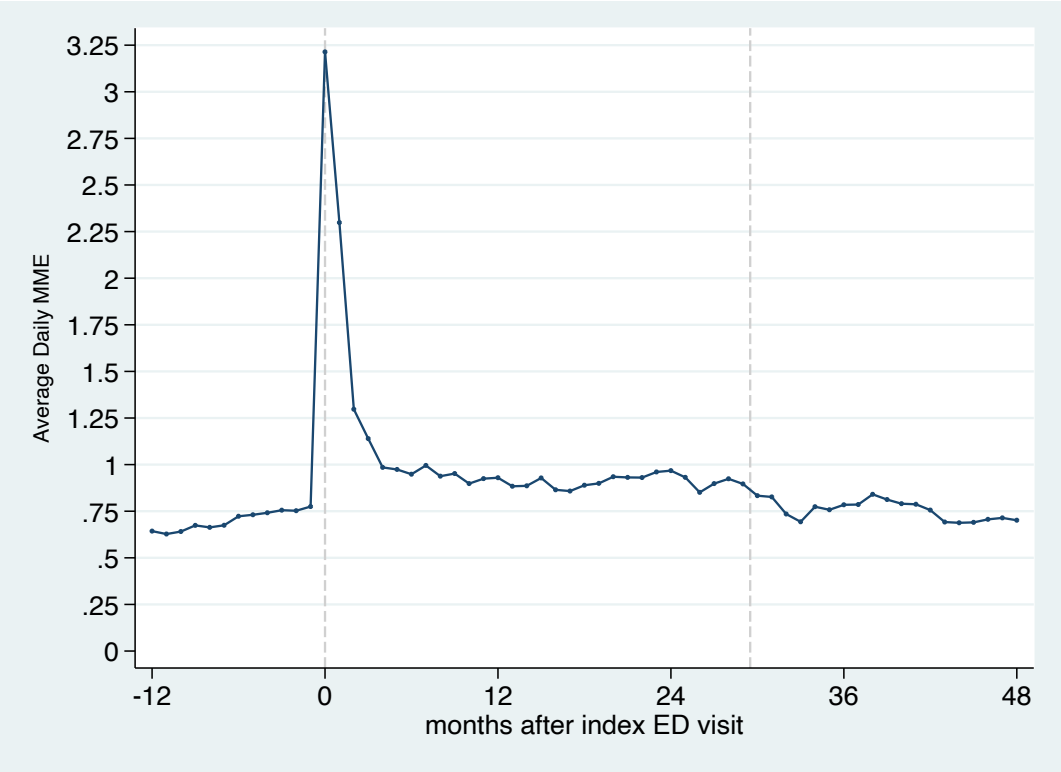
ED visit, from an average of 0.80 daily MME over the 30 days before the index ED visit to an average of 4.23 MME over the 30 days after the index ED visit.

To provide more insight into the dynamics of prescribing before and after injury, Figure 2 shows average daily MME at the monthly frequency from 12 months pre-index to 48 months post-index. Average daily MME increases by about 300% from the month before the index ED visit to the month of the index ED visit. Average daily MME declines quickly from this one-month spike, but remains elevated for several months post-injury and does not fully revert to pre-injury levels for several years. Appendix Figure A2 shows very similar dynamics on the extensive margin: the probability that a patient receives prescriptions covering 1 or more days in each month also spikes in the month of the index ED visit, by about 25 percentage points, and does not revert to the pre-injury baseline.

Some caution is necessary in interpreting these summary statistics due to the fact that opioid prescribing is observed in the APCD only in months when patients are covered by insurance provided by a payer that reports to the CO APCD. Person-months where the patient is not observed in the CO APCD are excluded from the calculations in Figure 2. Appendix Figure A1 shows that about a third of the sample is unobserved in the CO APCD by 48 months post-injury, and so it is important to bear this in mind when interpreting the average daily MME reported for later months in Figure 2. Attrition from the CO APCD should have a more limited impact on the analysis in this paper, however: prescription opioid receipt is measured over 1, 3, and 6 months post-index, when the fraction of patients observed in the CO APCD remains relatively high (98% at 1 month, 95% at 3 months post-index, and 89% at 6 months post-index). I also note that the measures shown in this figure aggregate time by calendar month (rather than time relative to the day of the index ED visit, as in the measures from Table 2).

Prescribing Patterns for Opioid Naive vs. Non-Naive Patients As noted above, patients with no opioid prescriptions for a full year (365 days) prior to the index ED visit are coded as *opioid-naive*. I note that this definition cannot rule out illicit opioid use or prescription opioid receipt prior to 1 year pre-index. I use opioid-naive status primarily as a control variable in the regression models for Medicare entry. Even after controlling for the presence of broadly defined groups of comorbidities (as discussed below), pre-injury opioid receipt may reflect whether patients suffer from chronic pain, which may independently affect the risk of future SSDI entry. Although I do not attempt to estimate the causal effect of pre-injury opioid receipt on post-injury SSDI entry, any association between pre-injury opioid

Figure 2: Average Daily MME by Month Relative to Index ED Visit, Ages 22-58 at Injury



receipt and SSDI entry may also be of interest to clinicians and policymakers seeking to reduce the severity of disability after injury.

Table 3 shows the proportion of opioid-naive patients and reports the probability of opioid receipt after injury conditional on opioid-naive status. Most patients (87%) are opioid-naive at the time of injury. Patients with pre-injury opioid prescriptions are about twice as likely to receive prescriptions during each post-injury time period reported in the table. Appendix Table A4 shows the probability of post-injury opioid prescriptions, stratified by opioid-naive status, for patients aged 50-58 at injury. Older patients are slightly more likely than the full sample (16.3% of patients aged 50-58 vs. 13.3% of patients aged 22-58) to have pre-injury opioid prescriptions, and post-injury prescribing probabilities for opioid-naive adults are slightly (2 to 4 percentage points) higher than for the full sample. Post-injury prescribing probabilities among non-naive patients in the older adults subsample are very similar to those for the full sample.

Table 3: Probability of Post-Injury Opioid Receipt, by Pre-Injury Opioid Receipt, Ages 22-58 at Injury

Time Period Relative to Index ED Visit	All	Opioid-Naive	With Pre-Injury Opioid Prescriptions
1 year pre-index	13.36%	0%	100%
30 days post-index	29.69%	25.90%	54.34%
90 days post-index	32.24%	28.09%	59.17%
180 days post-index	35.68%	31.46%	63.09%
N	9,150	7,928	1,222

Measuring Comorbidities and Other Covariates Panel data from the CO APCD also makes it possible to observe patients' diagnosed comorbidities prior to the index ED visit. I used all diagnoses appearing on medical claims from the 12 months leading up to the index ED visit to construct a set of indicators for the appearance of health conditions on 1 or more claims. I grouped health conditions by chapters of the ICD-9 (roughly corresponding to distinct body systems), so the indicators used in this study reflect whether a patient had one or more health conditions in each body system prior to the index ED visit.

Regression models and other analyses in this paper also control for age and gender, as measured at the time of the index ED visit. Age and gender are taken from data on

member characteristics reported by insurers to the CO APCD. Regression models include indicators for age (binned into 5-year increments from 20 to 65) interacted with an indicator for female gender.

Medicare Data in the CO APCD Medicare is a complex program that comprises a number of different insurance plans provided both by the federal government (CMS) and by private (commercial or non-profit) insurers. The four major parts of Medicare are as follows:

- Part A (Hospital Insurance): provided by CMS
- Part B (Medical Insurance): provided by CMS
- Part C (Medicare Advantage): may be chosen by beneficiaries to replace Part A/B coverage, provided by private insurers to all beneficaries who elect Part C coverage
- Part D (Drug coverage): optional coverage, may be chosen by beneficiaries in Parts A/B or Part C, provided by private insurers to all beneficiaries who elect Part D coverage

Medicare beneficiaries may also have other forms of public or private insurance to reduce patient cost-sharing associated with Medicare:

- Medicare Supplement Insurance (Medigap): private insurance that covers patient cost-sharing in Medicare Part A/B
- Various forms of Medicaid coverage are available to Medicare beneficiaries with low incomes who meet other eligibility criteria

The CO APCD receives data on Medicare coverage and claims from several sources. As with other commercial insurance products, insurers are required to submit data to the APCD on enrollment and claims from their Medicare products, including Medicare Advantage, Part D prescription drug plans, and Medicare Supplement Insurance (Medigap). These data are routinely submitted to the APCD by insurers on the same timelines that apply to their ESI plans and other insurance products. Data on private insurance (Parts C and D) and Medicaid were available through the end of 2019 for this study.

Meanwhile, the CO APCD periodically receives data from CMS on enrollment and claims in Medicare Part A and Part B, as well as data on Part D claims (for which payers sometimes receive claim-level reinsurance payments from CMS). CMS data submissions to

the CO APCD are less frequent than submissions from insurers, and CMS data on enrollment and claims were available only through the end of 2018 at the time when data were last refreshed for this study in summer of 2020. We therefore limit our analysis to claims and enrollment in the years 2012-2018.

Measuring Medicare Enrollment in the CO APCD Information about individuals' coverage status and the source of coverage (e.g., Medicare vs. ESI) must be derived from the CO APCD's eligibility files. Each observation in the eligibility file corresponds to a unique person-month-plan combination: most individuals are covered by only one plan at a point in time, but individuals enrolled in multiple plans at a point in time will have multiple eligibility records. Although the eligibility records contain multiple variables describing plan characteristics, additional processing is needed to assign enrollment records to the coverage source categories of primary interest in this study: ESI and Medicare. ESI plans were identified base based on the line of business code, market category code, and insurance product type code. The insurance product type code and coverage type code were used to exclude self-funded ESI plans from the analytic file.

The outcome of primary interest in this study is Medicare enrollment. Information about different types of Medicare coverage and dual-eligible status is provided by different data submitters: Part A and Part B enrollment records are reported by CMS, Part C and Part D enrollment records are reported by the commercial insurers that sell those plans, and QMB records are reported to the APCD by Colorado's Medicaid Agency. Table 4 lists the insurance product type codes I used to identify these different forms of Medicare coverage.

Table 4: Approach to Coding Medicare Eligibility Records in the CO APCD

Insurance Product Type Codes Used to Ascertain Medicare Enrollment	
Medicare Coverage Type	Insurance Product Type Codes
Part A	MA (Medicare Part A)
Part B	MB (Medicare Part B)
Part C	HN (HMO Medicare Risk/ Medicare Part C) or 16 (Health Maintenance Organization (HMO) Medicare Advantage)
Part D	MD (Medicare Part D)
QMB (Dual-Eligible)	QM (Qualified Medicare Beneficiary)

In this study, I coded individuals as enrolled in Medicare if they had eligibility

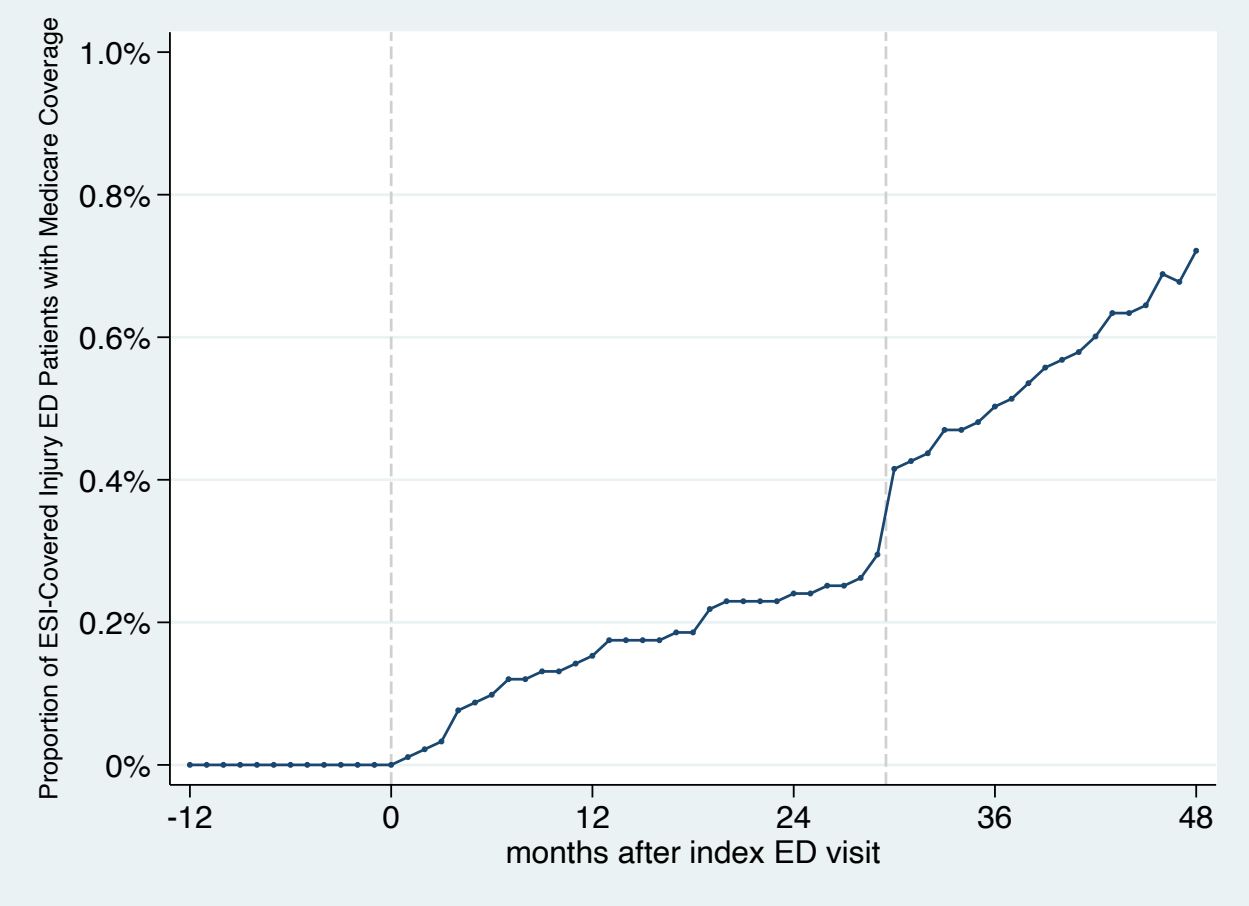
records indicating coverage from any of the four parts of Medicare (A, B, C, or D), or if they had a record from the state Medicaid agency indicating that they had Qualified Medicare Beneficiary (QMB) coverage, a specific type of dual eligible coverage. Results excluding the QMB records were nearly identical to those shown here, but have not been submitted to CIVHC for output review. A challenge in ascertaining Medicare coverage in the CO APCD is that commercial insurers sometimes appear to use line of business codes associated with Medicare to report coverage in certain retiree health plans. I was concerned that similar problems affected Medigap coverage, and so I do do not include Medigap or retiree coverage eligibility records in my definition of Medicare coverage.

It is rare for SSDI beneficiaries to exit Medicare for reasons other than death, and so Medicare enrollment should be very close to an absorbing state. There are sometimes gaps in APCD eligibility records, however (i.e., individuals may have missing months between spells of reported Medicare coverage), and so we code individuals as Medicare-enrolled at a certain point in time if they have *ever* been observed with 1 month or more of Medicare coverage. This means that our outcome variable is the cumulative incidence of Medicare entry, rather than a direct measure of current Medicare enrollment. To the extent that Medicare is an absorbing state and death rates are low, these two concepts (cumulative incidence and current Medicare enrollment) should approximately align with one another.

The CO APCD, unlike APCDs in some other states, is not routinely linked to state vital records. It is therefore impossible to tell if attrition from the APCD reflects migration out of state, death, loss of insurance coverage (i.e., transition to uninsurance), or a transition to insurance coverage from a payer that is not captured in the APCD. Transitions from Medicare to non-covered payers or uninsurance seem very unlikely, but we cannot distinguish between out-of-state migration and death. This limitation should not directly affect our analysis of Medicare entry, but it is important to understand for interpreting the limitations of APCD data more broadly.

Figure 3 shows the proportion of patients enrolled in Medicare by month relative to the injury date. Between 29 and 30 months post-injury, there is a sharp increase in Medicare enrollment. This would correspond to the end of the Medicare waiting period if disability onset coincided with the injury ED visit. However, some patients enter Medicare less than 30 months after the index ED visits. This could indicate that the month of disability onset is earlier than the index ED visit. After month 30 post-injury, Medicare enrollment continues to increase, but at a slightly faster rate between 30 and 48 months post-injury. In the

Figure 3: Probability of Medicare Enrollment by Month Relative to Index ED Visit, Ages 22-58 at Injury



empirical analysis below, I focus on Medicare enrollment by 48 months post-index as the primary outcome measure of interest.

4 Empirical Strategy

The equation of interest in this paper is a linear probability model for Medicare enrollment at or before 48 months after the index ED visit:

$$MCR_{i,t+48} = \alpha + X_i\beta^X + OP_i\tau + \varepsilon_{it} \tag{1}$$

where

- $MCR_{i,t+48}$
- α is a constant
- X_i is a vector of individual characteristics measured at or before the index ED visit
- OP_i is a measure of post-injury opioid receipt, either an indicator for receipt of any opioid prescriptions or a measure of the average daily MME. OP_i is measured over 30, 90, or 180 days post-injury.
- ε_{it} is an error term
- β^X and τ are regression coefficients

Included covariates (X_i) are:

- Patient demographics: age (5-year bins), gender, and age interacted with gender
- Pre-injury diagnosed comorbidities: 12 indicators for the presence of one or more diagnoses in each ICD-9 chapter (roughly corresponding to distinct body systems) on any medical claims submitted to insurance over the year preceding the index ED visit.
- Barell injury type: indicators for unique combinations of injury site (36 injury sites, e.g., cervical vertebral column injury, lumbar vertebral column injury, shoulder & upper arm) and injury type (12 injury types, e.g., fractures, sprains & strains, burns). See Table A1 for the distribution of injury types in the full sample, and see Table A2 for the distribution of injury types in the age 50+ sample. Injury types with 100 or fewer cases in the full sample were aggregated to a residual "other injury type" category for each of 9 body regions defined by the Barell matrix.

- Geography: 3-digit ZIP code of patient residence
- Opioid-naive status: an indicator equal to one if the patient was opioid-naive over the year before the injury, and zero if not

I estimate this model using OLS with standard errors clustered on payer. Wild bootstrap p-values for significance of $\hat{\tau}$ are also calculated using the `boottest` package in Stata (Roodman et al., 2019).

Instrumental Variables Opioid receipt OP_i is endogenous to future medicare enrollment because we cannot perfectly control for patient injury severity or other characteristics not fully captured in claims data that may affect both disability risk (and Medicare entry) and patient-level opioid prescribing. I.e., ε_{it} may not be mean-independent of OP_i, and OLS estimates of this model may be biased for the causal effect of opioid prescribing on Medicare enrollment.

I therefore use the following instrumental variables model to obtain causal estimates of the effect of post-injury opioid prescribing:

$$\begin{aligned} MCR_{i,t+48} =& \alpha + X_i\beta^X + OP_i\tau + \varepsilon_{it} \\ OP_i =& \kappa + X_i\gamma^X + OP_{i-,p(i)}\pi^O + \eta_{it} \end{aligned} \tag{2}$$

where

- $OP_{i-,p(i)}$ is the leave-one-out mean of opioid prescribing assigned to patient i, which is calculated based on opioid prescribing to other injury patients enrolled in the same payer ($p(i)$) as patient i.
- κ is a constant
- γ^X and π^O are regression coefficients

4.1 Instruments

I use a leave-one-out instrument, similar to the strategy used by Savych et al. (2018). After grouping together patients, the instrument is constructed by taking the average of the potentially endogenous opioid prescribing measure over all other patients in the group. I use the unadjusted leave-one-out mean as the instrument and instead include comorbidities and patient characteristics in the regression model. I note that this differs from the approach taken by Maestas and Sherry (2020), who adjust for comorbidities and patient characteristics

while constructing their instrument to obtain an adjusted prescribing propensity at the couma level. However, inclusion of patient characteristics in the regression model (which is possible here because I am using individual-level microdata) should function similarly.

There are many potential ways to group together patients in the APCD. I explored the following groupings:

- 3-digit ZIP code of patient residence
- Insurer (i.e., Payer Code) providing ESI at time of index ED visit, from bill header for index ED visit
- Provider ID on bill header for index ED visit

3-digit ZIP and provider ID were weak instruments with F-statistics below 10 after including covariates. Payer code, in contrast, was a strong instrument, with (Kleibergen-Paap, two-way clustered) F-statistics in the hundreds for indicators of any opioid receipt and between 16 and 49 for average daily MME. First-stage F-statistics for each model are reported in the regression tables below.

Exclusion Restriction The exclusion restriction is that the leave-one-out mean (i.e., probability that other patients covered by the same insurer receive opioids post-injury) does not have a causal effect on a patient's probability of transitioning to Medicare, except through its impact on that patient's prescription opioid receipt. Included covariates are:

- Patient demographics
- Pre-injury diagnosed comorbidities
- Barell injury type
- Geography
- Opioid-naive status

In a narrow sense, leave-one-out instruments like that used in this paper should satisfy the exclusion restriction, since any given patient's outcomes should not be affected by treatment provided to other patients. However, it is possible that aspects of treatment other than opioid prescribing vary systematically across insurers in ways that are correlated with opioid prescribing patterns. It is useful to distinguish between two such types of potential variation across insurers. One possibility is that opioid prescribing may substitute for

other types of medical care. Patients prescribed opioids may be less likely to receive other pain medications, or to receive pain management using non-pharmaceutical treatments, for example.

If this variation also reflects differences across insurers in in-network providers or other factors that are common to enrollees in each insurer, then the leave-one-out instrument would predict this variation in addition to variation in opioid prescribing. This type of variation in treatment patterns may not be a problem for causal inference if it reflects changes in treatment that would result from an exogenous change in opioid prescribing. It also could be investigated using the APCD, but I have not conducted any analysis that would indicate how overall treatment patterns are changing when opioid prescribing changes.

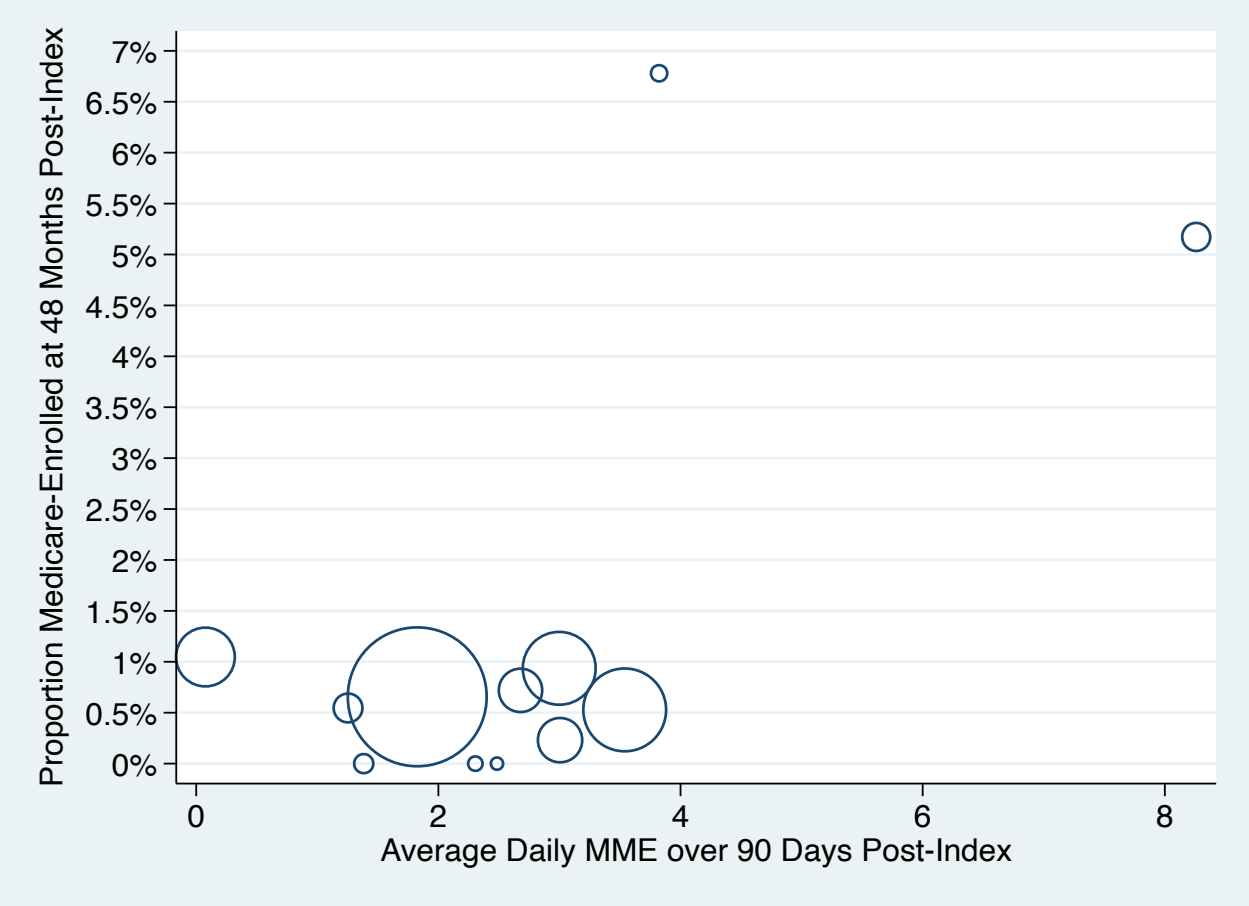
A different possibility is that insurers with lower opioid prescribing rates may also have other practices, such as patient engagement or case management activities, that are provided independently of providers' treatment decisions. If these types of payer activities also affected the probability of SSDi entry, then the exclusion restriction would be violated. While I am not aware of any examples of interventions by commercial insurers that would fit this description, I was not able to rule out the possibility that such interventions exist.

Inclusion Restriction The leave-one-out instrument must also satisfy the inclusion restriction to deliver informative estimates.This would be the case to the extent that enrollment in different insurers is associated with different groups of providers (e.g., due to participation of providers in different networks). Another potential source of variation across insurers would be differences in insurers' administrative processes that may affect post-injury prescribing (such as differences in health IT or review of physician prescribing behavior). This paper does not investigate the sources of variation in prescription rates across payers in detail, however.

Figure 4 below shows how the proportion of each payer's enrollees who have entered Medicare by 48 months post-injury (y-axis) varies with the average monthly MME over 90 days post-injury (y-axis) in that payer. Points are weighted by payer enrollment. Key points:

- There is variation in payer-average monthly MME, although I note that these averages are not adjusted for demographics or case-mix.
- A couple of small payers have particularly high monthly average MME and high prob-

Figure 4: Payer-Level Averages of Medicare Entry Probability and Post-Injury Average Monthly MME



abilities of Medicare entry, but there is also variation in average monthly MME and Medicare entry among the larger payers concentrated in the lower left of the figure.

5 Results

5.1 Descriptive Findings

Table 5 reports the probability of Medicare enrollment at six points in time relative to the index ED visit, stratified by pre-injury opioid receipt and post-injury opioid receipt (measured over 180 days post-index). While the regression models focus on the cumulative incidence of enrollment at 48 months post-index as the main outcome, it is interesting to look at the timing of Medicare enrollment during the four years following injury. Several noteworthy patterns emerge. First, Patients who are opioid-naive at injury are much less likely to enter Medicare (0.58% at 48 months post-index) than patients with a history of

prescription opioid receipt at injury (2.21% at 48 months post-index). Second, conditional on opioid-naive status, post-injury opioid receipt is also associated with Medicare entry. Opioid-naive patients who receive prescription opioids within 180 days post-injury are twice as likely (0.88%) to enter Medicare by 48 months post-index as opioid-naive patients with no opioids post-injury (0.44%). Similarly, Patients with a history of prescription opioid receipt who receive prescription opioids within 180 days post-injury are about 2.5 times as likely (2.85%) to enter Medicare by 48 months post-index as opioid-naive patients with no opioids post-injury (1.11%).

Third, there is a sharp increase between months 29 and 30 post-injury (which would correspond to the end of the Medicare waiting period if disability onset coincided with the injury ED visit) in Medicare enrollment. For both opioid-naive patients and patients with a history of prescription opioid receipt , this increase is driven mostly by patients with post-injury opioid prescriptions. Among opioid-naive patients , 0.16% are Medicare-enrolled in month 29, rising to 0.40% Medicare-enrolled in month 30. Among patients with a history of prescription opioid receipt , 1.43% are Medicare-enrolled in month 29, rising to 1.82% Medicare-enrolled in month 30.

Table 5: Post-Injury Medicare Enrollment by Pre-, Post-Injury Opioid Prescription Receipt, Ages 22-58 at Injury

	Opioid-Naive			With Pre-Injury Opioid Rx			Total
Any opioid prescriptions over 180 days post-index?	N	Y	Total	N	Y	Total	Total
Medicare Enrollment At							
12 months post-index	0.06%	0.08%	0.06%	0.22%	1.04%	0.74%	0.15%
24 months post-index	0.15%	0.12%	0.14%	0.44%	1.17%	0.90%	0.24%
29 months post-index	0.18%	0.16%	0.18%	0.44%	1.43%	1.06%	0.30%
30 months post-index	0.22%	0.40%	0.28%	0.44%	1.82%	1.31%	0.42%
36 months post-index	0.26%	0.56%	0.35%	0.44%	2.33%	1.64%	0.52%
48 months post-index	0.44%	0.88%	0.58%	1.11%	2.85%	2.21%	0.80%
N	5,434	2,494	7,928	451	771	1,222	9,150

Descriptive Results for Older Adult Subsample In Appendix Table A5, the same statistics are reported for the older adult subsample (those aged 50-58 at the index ED visit). The qualitative patterns seen in Table 5 are also apparent among the older adult subsample, with higher Medicare enrollment probabilities among patients with a history of

prescription opioid receipt than opioid-naive patients , and with higher Medicare enrollment probabilities among those with post-injury opioid prescriptions conditional on opioid-naive status. Because Medicare enrollment is concentrated among those aged 50 and older at injury, the Medicare entry probabilities are much higher in the older adult subsample, and the differences between groups are larger in magnitude. Patients with a history of prescription opioid receipt aged 50-58 at index have a 5.26% probability of Medicare entry at 48 months post-index, compared to 1.66% for opioid-naive patients . Among opioid-naive patients , those with post-injury opioid prescriptions (measured over 180 days post-index) have a 2.24% probability of Medicare entry at 48 months post-index, vs. 1.36% for opioid-naive patients without post-injury opioid prescriptions. Among patients with a history of prescription opioid receipt , those with post-injury opioid prescriptions (measured over 180 days post-index) have a 6.30% probability of Medicare entry at 48 months post-index, vs. 3.45% for opioid-naive patients without post-injury opioid prescriptions.

5.2 Regression Results

Table 6 shows regression results for the full sample of adults aged 22-58 at injury. Each cell of the table reports the regression coefficient on an opioid measure from a separate regression model; clustered standard errors (in parentheses); wild-bootstrap p-values for the opioid measure; and, in the IV models, the Kleibergen-Paap Wald statistic as a test for weak instruments. Models with covariates included also control for age, gender, age-gender interactions, indicators for within-state geography (3-digit ZIP code of patient residence), indicators for pre-injury comorbidities, and indicators for injury type. Coefficients on opioid-naive status and demographic covariates for selected models are shown separately in Table 7 below. Coefficients on other covariates were not submitted to output review with the CO APCD and so cannot be included in this version of the paper.

Interpretation of Coefficients The leftmost four columns of the table show results for a binary measure of opioid receipt (any prescription opioids dispensed) over 30, 90, or 180 days post-index. This measure captures only the extensive margin of opioid prescribing post-injury. The rightmost four columns of the table show results for a continuous measure of opioid receipt (average daily MME) over 30, 90, or 180 days post-index. In all models, the outcome is an indicator for Medicare enrollment at 4 years post-index, so coefficients can be interpreted as the change in the probability of Medicare enrollment predicted by a unit increase in the explanatory variable. A unit increase in the indicator for any opioid prescribing means a change from no more opioid prescriptions to 1 or more opioid prescriptions.

For the average daily MME, however, interpretation of a unit change is somewhat more complicated because the distribution of average daily MME changes with the post-injury time period, as shown in Table 2 above. Furthermore, the average daily MME measure reflects the combined effect of dosage, duration of prescription (or quantity dispensed), and number of prescriptions dispensed. For instance, 3.3 MME per day over 180 days (the 90th percentile of post-injury prescribing for the full sample) could reflect one prescription containing 40 10-mg oxycodone tablets, or 8 prescriptions containing 30 2.5-milligram hydrocodone tablets, or myriad other combinations of medications or prescribing patterns over the post-injury window. In some places below, I use the difference between the 75th and 90th percentile of post-injury average daily MME to interpret coefficient magnitudes, but it is important to note that the analysis reported here does not indicate exactly what change in prescribing patterns corresponds to such a shift in prescription volumes.

Results for Full Sample (Ages 22-58 at Injury) For the full sample results reported in Table 6, the OLS estimates show a strong association between receiving post-injury opioid prescriptions and Medicare entry. Including covariates reduces the magnitude of this association by about one third, but it remains highly statistically significant. With covariates included, receipt of any prescription opioids over 30 (90, 180) days post- injury predicts an increase of 0.55 percentage points (0.43 percentage points, 0.45 percentage points) in the probability of Medicare entry within 48 months. This is a sizable (over 50%) increase relative to the sample mean Medicare entry probability of 0.80 percent.

Average daily MME post-injury is also strongly associated with higher probabilities of Medicare entry in the OLS estimates; including covariates has a very small impact on the magnitude of this association.

These estimates cannot be interpreted causally because opioid prescribing likely reflects unobserved variation in health status and disability risk that cannot be controlled for adequately using claims data. I therefore estimated 2SLS regression models using the leave-one-out mean of opioid prescribing for other injury patients covered by the same insurer. The Kleibergen-Paap Wald statistics reported in Table 6 generally indicate that weak instrument concerns should not be a problem for these instruments. With covariates included, the Wald statistics are between 150 and 202 for any opioid prescribing, and between 16 and 49 for average daily MME.

For the indicator of any opioid prescribing, 2SLS estimates are close to zero and

Table 6: OLS and 2SLS Estimates of Prescription Opioid Effects on Risk of Medicare Entry, Ages 22-58 at Injury

Outcome	Medicare enrollment at 4 years post-index							
Opioid Measure	Any Opioid RX?				Average Daily MME?			
Covariates included?	N	Y	N	Y	N	Y	N	Y
Estimator	OLS	OLS	2SLS	2SLS	OLS	OLS	2SLS	2SLS
Opioid RX measured over ...								
30 days post-index	0.0085***	0.0055***	0.0056	0.0004	.0006***	0.0005**	0.0018	0.0027
	(0.0019)	(0.0012)	(0.0126)	(0.0124)	(0.0001)	(0.0002)	(0.0016)	(0.0021)
wild bootstrap p-value	0.021	0.004	0.725	0.974	0.026	0.006	0.469	0.428
F (K-P)			322.5	180.3			102.5	49.49
90 days post-index	.0077***	0.0043***	0.0040	-0.0013	0.0009**	0.0008**	0.0034	0.0057*
	(0.0017)	(0.0009)	(0.0112)	(0.0109)	(0.0003)	(0.0003)	(0.0026)	(0.0031)
wild bootstrap p-value	0.026	0.005	0.763	0.913	0.018	0.013	0.447	0.283
F (K-P)			371.7	202.1			86.51	25.34
180 days post-index	0.0085***	.0045***	0.0042	-0.0004	0.0010**	0.0008**	0.0046	0.0084**
	(0.0014)	(0.0012)	(0.0120)	(0.0125)	(00004)	(0.0004)	(0.0036)	(0.0041)
wild bootstrap p-value	0.028	0.011	0.780	0.980	0.023	0.013	0.533	0.251
F (K-P)			385.1	154.4			91.36	16.48
N	9,150	9,150	9,150	9,150	9,150	9,150	9,150	9,150
Mean of DV	0.0080	0.0080	0.0080	0.0080	0.0080	0.0080	0.0080	0.0080

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. F (K-P) = Kleibergen-Paap Wald statistic. Clustered standard errors in parentheses. OLS clustered on payer (G = 12). 2SLS clustered on payer (G = 12) and date of ED visit (G = 24).

statistically insignificant—a stark contrast from the positive and highly significant OLS estimates. 2SLS estimates for average daily MME are positive, but statistically insignificant when measured over 30 days post-injury. When post-injury prescriptions are measured over longer post-injury time periods, the coefficients on average daily MME are larger and more statistically significant, at 10% for average daily MME over 90 days, and at 5% for average daily MME over 180 days. However, inference based on the wild cluster bootstrap (rather than analytical two-way standard errors clustered on payer and month of injury) indicates that these results are not statistically significant, so I do not view these 2SLS results as strong evidence that the relationship suggested by the OLS estimates is causal.

Coefficients on Opioid-Naive Status and Demographics Table 7 presents the coefficients on the indicator for opioid-naive status and the demographic variables. Consistent with the descriptive statistics in Table 5 above, the coefficients on the opioid-naive indicator are negative in the OLS models and are sizable in comparison to the mean of the outcome variable. When average daily MME is used to measure post-injury prescribing, the opioid-naive coefficient from the OLS model is not statistically significant at the 10% level, however, and the 2SLS estimates are very noisy.

The demographic coefficients indicate that the probability of Medicare entry increases sharply above age 50. Gender differences are generally small and statistically insignificant with the exception of women aged 55-58 at injury. Point estimates suggest that women in this age range are about 1.3 to 1.7 percentage points (depending on the model specification and opioid measure used) more likely to enter Medicare than men at the same ages to enter Medicare within 4 years. While these estimates are significant only at the 10% level, the magnitude is substantial relative to the sample average Medicare entry rate. Point estimates on the female gender interaction at ages 50-54 are also positive and large, but are not statistically significant.

Results for Older Adults Subsample Table 7 suggests that the concentration of SSDI entry and Medicare enrollment among older adults aged 50-58 at injury is not driven by the covariates included in these regression models, most notably pre-injury comorbidities, injury type, or opioid-naive status. As discussed above, I speculate that this may reflect the application of the vocational grid, although other explanations (such as differences in injury severity or ability to recover conditional on injury type) may also contribute. I accordingly estimated regression models on the subsample of older adults.

Table 7: Coefficients for Demographic Control Variables

Outcome	Medicare enrollment at 4 years post-index			
Opioid Measure	Any Opioid RX?		Average Daily MME	
Opioid Measure	(90 days post-index)?		(90 days post-index)	
Covariates included?	Y	Y	Y	Y
Estimator	OLS	2SLS	OLS	2SLS
Opioid-Naive?	-0.0086**	-0.0102***	-0.0048	0.0256
	(0.0032)	(0.0022)	(0.0034)	(0.0190)
Age 25-29	0.0030	0.0032	0.0025	-0.0016
	(0.0039)	(0.0038)	(0.0037)	(0.0031)
Age 30-34	0.0046	0.0050	0.0043	0.0011
	(0.0054)	(0.0052)	(0.0051)	(0.0023)
Age 35-39	0.0038	0.0041	0.0034	-0.0005
	(0.0029)	(0.0028)	(0.0028)	(0.0049)
Age 40-44	0.0044	0.0047	0.0041	0.0008
	(0.0046)	(0.0043)	(0.0044)	(0.0033)
Age 45-49	0.0040	0.0043	0.0039	0.0018
	(0.0037)	(0.0036)	(0.0035)	(0.0036)
Age 50-54	0.0174**	0.0178**	0.0172**	0.0142***
	(0.0070)	(0.0073)	(0.0070)	(0.0052)
Age 55-58	0.0135***	0.0140***	0.0134***	0.0106***
	(0.0039)	(0.0022)	(0.0039)	(0.0039)
Female?	-0.0003	-0.0006	-0.0007	-0.0016
	(0.0044)	(0.0037)	(0.0044)	(0.0041)
Age 25-29 × Female?	-0.0009	-0.0008	0.0000	0.0052
	(0.0051)	(0.0042)	(0.0049)	(0.0056)
Age 30-34 × Female?	-0.0040	-0.0039	-0.0030	0.0024
	(0.0060)	(0.0053)	(0.0055)	(0.0050)
Age 35-39 × Female?	-0.0041	-0.0038	-0.0030	0.0019
	(0.0039)	(0.0035)	(0.0039)	(0.0082)
Age 40-44 × Female?	-0.0051	-0.0049	-0.0041	0.0008
	(0.0062)	(0.0058)	(0.0060)	(0.0057)
Age 45-49 × Female?	0.0029	0.0032	0.0031	0.0035
	(0.0081)	(0.0071)	(0.0084)	(0.0097)
Age 50-54 × Female?	0.0078	0.0079	0.0082	0.0102
	(0.0063)	(0.0070)	(0.0065)	(0.0081)
Age 55-58 × Female?	0.0130	0.0133*	0.0140*	0.0189*
	(0.0081)	(0.0073)	(0.0077)	(0.0101)
N	9,150	9,150	9,150	9,150
Mean of DV	0.00798	0.00798	0.00798	0.00798

Excluded age category is ages 22-24 at injury.

Table 8: OLS and 2SLS Estimates of Prescription Opioid Effects on Risk of Medicare Entry, Ages 50-58 at Injury

Outcome	Medicare enrollment at 4 years post-index							
Opioid Measure	Any Opioid RX?				Average Daily MME?			
Covariates included?	N	Y	N	Y	N	Y	N	Y
Estimator	OLS	OLS	2SLS	2SLS	OLS	OLS	2SLS	2SLS
Opioid RX measured over ...								
30 days post-index	0.0190**	0.0105*	0.0277	0.0143	0.0006***	0.0004***	0.0052	0.0100
	(0.0065)	(0.0052)	(0.0328)	(0.0364)	(0.0001)	(0.0001)	(0.0037)	(0.0071)
wild bootstrap p-value	0.052	0.065	0.530	0.776	0.017	0.112	0.332	0.328
F (K-P)			222.7	60.55			82.68	18.43
90 days post-index	0.0171**	0.0073	0.0239	0.0100	0.0012*	0.0010	0.0088	0.0221**
	(0.0059)	(0.0042)	(0.0301)	(0.0336)	(0.0006)	(0.0006)	(0.0056)	(0.0090)
wild bootstrap p-value	0.051	0.099	0.546	0.843	0.035	0.105	0.352	0.075
F (K-P)			176.3	47.38			67.03	5.675
180 days post-index	0.0174***	0.0059	0.0239	0.0129	0.0012	0.0010	0.0115	0.0295***
	(0.0049)	(0.0043)	(0.0313)	(0.0374)	(0.0007)	(0.0007)	(0.0076)	(0.0105)
wild bootstrap p-value	0.049	0.185	0.552	0.808	0.030	0.073	0.393	0.068
F (K-P)			264.2	61.60			67.09	4.367
N	2,442	2,442	2,442	2,442	2,442	2,442	2,442	2,442
Mean of DV	0.0225	0.0225	0.0225	0.0225	0.0225	0.0225	0.0225	0.0225

* p < 0.10, ** p < 0.05, *** p ¡ 0.01. F (K-P) = Kleibergen-Paap Wald statistic. Clustered standard errors in parentheses. OLS clustered on payer (G = 12). 2SLS clustered on payer (G = 12) and date of ED visit (G = 24).

Table 8 reports coefficients on the opioid measures from models estimated in the older adults subsample. Note that the leave-one-out instruments were recalculated in the older adults subsample (i.e., the leave-one-out mean is calculated using only other adults aged 50+, rather than prescribing rates in the full sample). While there was no pre-analysis plan filed for this study, I also caution that this subgroup analysis was not pre-specified, and was added to the paper only when it became clear in the descriptive analysis that Medicare entry for adults aged 49 or under at injury was extremely rare.

OLS estimates of the effect of opioid prescribing (any prescribing or average daily MME) are not significant for the age 50+ sample when covariates are included, in contrast to the full sample. As in the full sample, IV estimates for the effect of any opioid prescriptions are imprecisely estimated and statistically insignificant.

However, IV estimates of the effect of average daily MME for patients aged 50-58 are statistically significant at 5% (measured over 90 days) or 1% (measured over 180 days). The estimated coefficients are large and positive: the coefficient for average daily MME over 180 days post-injury would imply that a shift from the 75th to 90th percentile of post-injury prescribing for adults over age 50 (+2.74 average daily MME over 180 days) predicts an 8.1 percentage point increase in the probability of Medicare entry by 4 year post-injury, a 260% increase relative to the sample mean probability of Medicare entry for adults aged 50-58 at injury.

Unlike the results for all ages, the IV estimates for adults aged 50+ are marginally significant under the wild bootstrap (wild bootstrap p = 0.075 for average MME over 90 days post-index, p = 0.068 for average MME over 180 days post-index). However, the Kleibergen-Paap Wald statistics suggest that instruments may be weak in these models: the Wald statistic (with two-way clustering) is just 5.7 for average MME over 90 days post-index and 4.4 for average MME over 180 days post-index.

6 Conclusion

This paper provided new evidence on the relationship between prescription opioid receipt and transitions from employment to SSDI by using data from a state APCD. Focusing on a cohort of ESI policyholders who experience a traumatic injury, I documented some descriptive findings about the association between opioid receipt and Medicare enrollment before age 65, which I argue can be interpreted as a proxy for SSDI entry. Patients who are opioid-naive at injury are much less likely to enter Medicare (0.58% at 48 months post-index) than pa-

tients with a history of prescription opioid receipt at injury (2.21% at 48 months post-index). Conditional on pre-injury opioid receipt, post-injury opioid receipt is also strongly associated with Medicare entry. These associations remain statistically significant and large after controlling for patient demographics, pre-injury comorbidities, and the type of injury.

I also estimated 2SLS models that used a leave-one-out instrument (constructed from injury patients covered by the same health insurer) to identify the causal effect of post-injury prescription opioids on SSDI entry. These results were less clear than the descriptive findings; results for the full sample were not statistically significant under a wild bootstrap, while results for older adults aged 50-58 at injury (who accounted for nearly all Medicare enrollment in the sample) were marginally significant but undermined by potentially weak instruments. Thus, although these 2SLS estimates suggest that post-injury opioid prescriptions increase the risk of SSDI entry, the evidence is not strong and these results must ultimately be viewed as suggestive, at best.

Several improvements to the analysis in this paper can be carried out using the APCD data collected for this study. Different statistical models are likely more appropriate to the questions addressed here, including statistical models better able to accommodate rare events (such as logit with two-stage residual inclusion for the IV estimates) and discrete-time hazard models that use information about the timing of Medicare enrollment. Measures of opioid prescribing patterns that isolate specific prescribing patterns (such as high daily doses or other risky prescribing patterns) could help substantiate the findings in this paper by testing the importance of different mechanisms. Initial attempts to measure very high-risk prescribing (such as daily doses above 100 or 120 MME) identified very few patients with such prescribing patterns and could not be used in the regression analysis, but other measures could also be examined in future work.

More could be done, also, to explore and justify the exclusion restriction. Because I do not include controls for other, non-opioid, features of post-injury treatment patterns, I cannot currently rule out the potential for other violations of the exclusion restriction that might arise from insurer-level variation in case management or patient outreach activities that could directly affect disability outcomes. In principle, the APCD makes it possible to construct measures for use of other types of health care that might be substituted for prescription opioids, such as other pain medication or, in some cases, physical therapy or pain management. It may be possible to include such controls in future versions of this paper. A more difficult question is whether insurers or health systems that are more likely

to participate with specific insurers use other disability prevention interventions that are not documented in claims data. I am not aware of such efforts in the context of ESI, but the relatively limited number of insurers in the Colorado ESI market would make it possible to investigate this more carefully.

Despite this study's many limitations, the use of APCD data to identify transitions from employment to SSDI (proxied by transitions from ESI to Medicare) represents a novel use of state APCD data. State APCD data can potentially be used to study a much wider range of questions about the health status and health care utilization of non-elderly prior to Medicare enrollment, and about the potential implications of care provided through private health insurance for federal disability programs and Medicare.

References

Aliprantis, D. and M. E. Schweitzer (2018). Opioids and the Labor Market.

Barell, V., L. Aharonson-Daniel, L. A. Fingerhut, E. J. Mackenzie, A. Ziv, V. Boyko, A. Abargel, M. Avitzour, and R. Heruti (2002). An introduction to the Barell body region by nature of injury diagnosis matrix. *Injury Prevention 8*(2), 91–96.

Beheshti, D. (2019). The Impact of Opioids on the Labor Market: Evidence from Drug Rescheduling. pp. 0–41.

Currie, J., J. Y. Jin, and M. Schnell (2019). U.S. Employment and Opioids: Is There a Connection?

Cutler, D., E. Meara, and S. Stewart (2016). The Rise of Prescription Opioids and Enrollment in Disability Insurance.

Cutler, D., E. Meara, and S. Stewart (2017). Has Wider Availability of Prescription Drugs for Pain Relief Affected SSDI and SSI Enrollment?

Harris, M. C., L. M. Kessler, M. N. Murray, and E. Glenn (2020). Prescription Opioids and Labor Market Pains. *Journal of Human Resources 55*(Fall), 1319–1364.

Krueger, A. B. (2017). Where Have All the Workers Gone? An Inquiry into the Decline of the U.S. Labor Force Participation Rate. *Brookings Papers on Economic Activity* (Fall), 1–59.

Maestas, N. and T. B. Sherry (2020). Opioid Treatment for Pain and Work and Disability Outcomes: Evidence from Health Care Providers' Prescribing Patterns.

Metcalf, G. E. and Q. Wang (2019). Abandoned by Coal, Swallowed by Opioids?

Monnat, S. M. (2018). Factors Associated With County-Level Differences in U.S. Drug-Related Mortality Rates. *American Journal of Preventive Medicine 54*(5), 611–619.

Morden, N. E., J. C. Munson, C. H. Colla, J. S. Skinner, J. P. Bynum, W. Zhou, and E. Meara (2014). Prescription Opioid Use Among Disabled Medicare Beneficiaries. *Medical Care 52*(9), 852–859.

Musse, I. (2019). Employment Shocks and Demand for Pain Medication.

Park, S. and D. Powell (2021). Is the rise in illicit opioids affecting labor supply and disability claiming rates? *Journal of Health Economics 76*, 102430.

Pierce, J. R. and P. K. Schott (2020). Trade Liberalization and Mortality: Evidence from US Counties. *American Economic Journal: Insights 2*(1), 47–64.

Roodman, D., J. G. MacKinnon, M. Ø. Nielsen, and M. D. Webb (2019). Fast and wild: Bootstrap inference in Stata using boottest. *Stata Journal 19*(1), 4–60.

Rupp, K. and G. F. Riley (2012). Longitudinal Patterns of Medicaid and Medicare Coverage Among Disability Cash Benefit Awardees. *Social Security Bulletin 72*(3), 19–35.

Savych, B., D. Neumark, and R. Lea (2019). Do Opioids Help Injured Workers Recover and Get Back to Work? The Impact of Opioid Prescriptions on Duration of Temporary Disability. *Industrial Relations 58*(4), 549–590.

SSA (2018, oct). Annual Statistical Report on the Social Security Disability Insurance Program, 2017. Technical report, SSA, Washington, DC.

Szymendera, S. (2009). Social Security Disability Insurance (SSDI) and Medicare: The 24-Month Waiting Period for SSDI Beneficiaries Under Age 65. Technical report, CRS.

Venkataramani, A. S., E. F. Bair, R. L. O'Brien, and A. C. Tsai (2020). Association between Automotive Assembly Plant Closures and Opioid Overdose Mortality in the United States: A Difference-in-Differences Analysis. *JAMA Internal Medicine 180*(2), 254–262.

A Appendix

Table A1 lists the distribution of injury type (as coded in our regression models) for adults aged 22-58 at injury.

Table A1: Injury Type Distribution, Ages 22-58 at Injury

All ages Injury Type Distribution Injury Site and Type	N	% of sample
Hand & Wrist & Fingers Open Wounds	1,215	13.3%
Lower Extremity: Other Injury Type	589	6.4%
System Wide & Late Effects	508	5.6%
Face Open Wounds	500	5.5%
Upper Extremity: Other Injury Type	437	4.8%
Lower Leg & Ankle Sprains & Strains	385	4.2%
Lower Leg & Ankle Fractures	377	4.1%
Torso: Other Injury Type	374	4.1%
Other Head Unspecified	327	3.6%
Cervical VCI Sprains & Strains	307	3.4%
Forearm & Elbow Fractures	277	3.0%
Hand & Wrist & Fingers Fractures	273	3.0%
Type 2 TBI Internal Organ	264	2.9%
Shoulder & Upper Arm Fractures	240	2.6%
Other & Unspec Lower Extrem Sprains & Strains	237	2.6%
Foot & Toes Fractures	231	2.5%
Other & Unspec Lower Extrem Open Wounds	229	2.5%
Oth Head, Face, Neck: Other Injury Type	177	1.9%
Shoulder & Upper Arm Dislocation	176	1.9%
Lumbar VCI Sprains & Strains	169	1.8%
Other Head Open Wounds	167	1.8%
Head, Face, Neck Unspec Superfic/Cont	165	1.8%
Eye Superfic/Cont	161	1.8%
Forearm & Elbow Open Wounds	151	1.7%
Hand & Wrist & Fingers Superfic/Cont	149	1.6%
VCI: Other Injury Type	136	1.5%
Other & Unspecified: Other Injury Type	134	1.5%
Chest Superfic/Cont	128	1.4%
Chest Fractures	125	1.4%
Shoulder & Upper Arm Sprains & Strains	121	1.3%
Foot & Toes Open Wounds	121	1.3%
Hand & Wrist & Fingers Sprains & Strains	114	1.2%
Other & Unspec Lower Extrem Unspecified	102	1.1%
TBI: Other Injury Type	< 100	$< 1.1\%$
SCI: Other Injury Type	< 100	$< 1.1\%$
Total	9, 150	100%

Figure A1: Coverage in CO APCD (Any Source of Coverage) by Month Relative to Index ED Visit

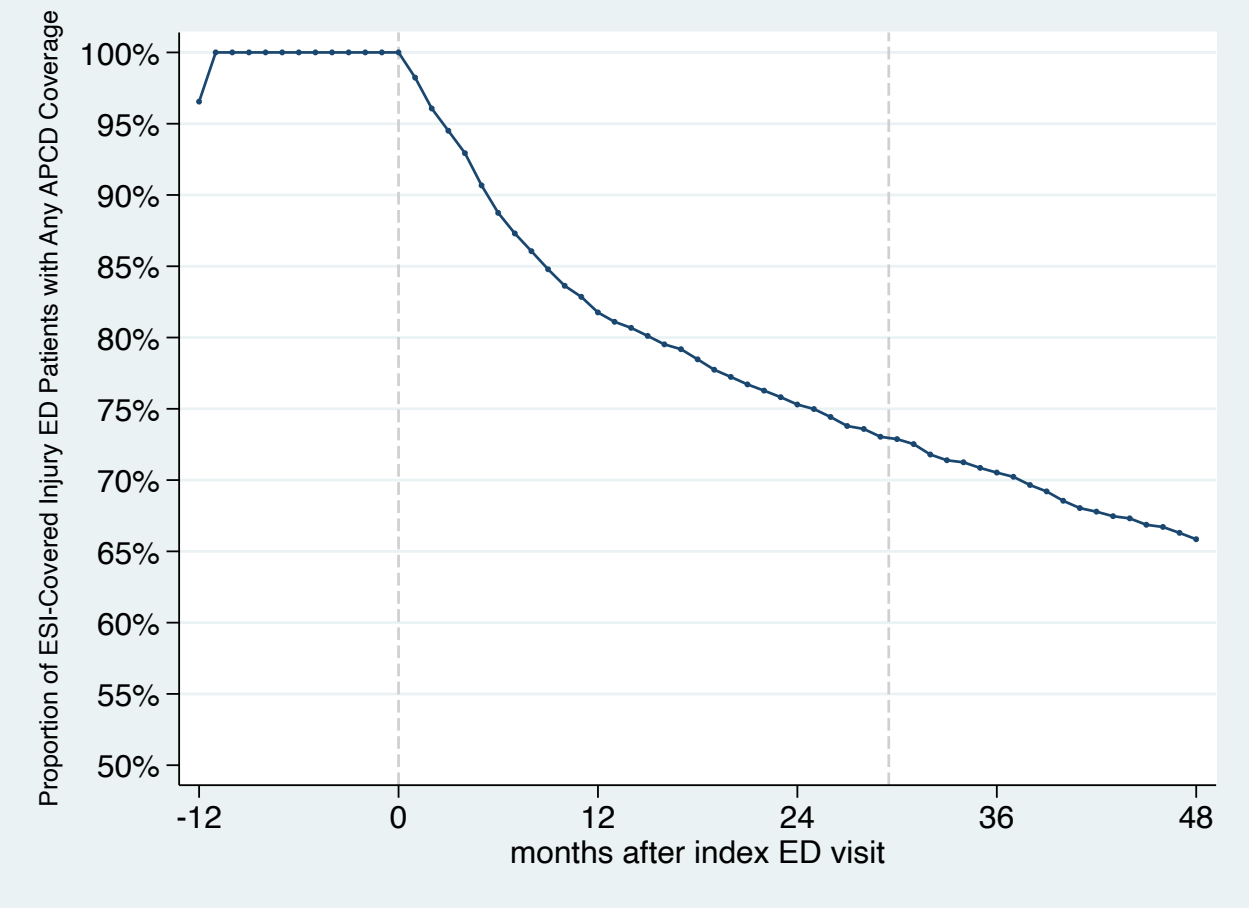


Table A2 lists the distribution of injury type (as coded in our regression models) for the older adults subsample (patients aged 50-58 at the index ED visit).

Table A3 shows summary statistics for pre- and post-injury opioid prescribing measures for the older adults subsample (patients aged 50-58 at the index ED visit).

Figure A1 shows the proportion of the sample with coverage from any source in the APCD by month relative to the injury (including Medicare, Medicaid, ACA Marketplace or Other Nongroup coverage in addition to fully-insured ESI). The proportion of patients who remain covered from these sources of coverage declines from 100% in the month of the injury to 66% at 48 months post-injury. This is important to keep in mind when interpreting our findings because prescription opioid receipt is observable only in months when patients are observed with coverage in the APCD.

Table A2: Injury Type Distribution, Ages 50-58 at Injury

Injury type (Barell site and type)	N	%
Hand & Wrist & Fingers Open Wounds	315	12.90%
Lower Extremity: Other Injury Type	164	6.72%
System Wide & Late Effects System Wide	154	6.31%
Lower Leg & Ankle Fractures	121	4.95%
Upper Extremity: Other Injury Type	116	4.75%
Forearm & Elbow Fractures	107	4.38%
Torso: Other Injury Type	105	4.30%
Face Open Wounds	103	4.22%
Other Head Unspecified	85	3.48%
Foot & Toes Fractures	78	3.19%
Lower Leg & Ankle Sprains & Strains	75	3.07%
Shoulder & Upper Arm Fractures	66	2.70%
Other & Unspec Lower Extrem Sprains & Strai	66	2.70%
Cervical VCI Sprains & Strains	63	2.58%
Hand & Wrist & Fingers Fractures	63	2.58%
Type 2 TBI Internal Organ	61	2.50%
Other & Unspec Lower Extrem Open Wounds	61	2.50%
Head, Face, Neck Unspec Superfic/Cont	54	2.21%
Chest Fractures	54	2.21%
Other Head Open Wounds	45	1.84%
Eye Superfic/Cont	45	1.84%
Chest Superfic/Cont	43	1.76%
Oth Head, Face, Neck: Other Injury Type	42	1.72%
VCI: Other Injury Type	40	1.64%
Hand & Wrist & Fingers Superfic/Cont	39	1.60%
Forearm & Elbow Open Wounds	38	1.56%
Other & Unspecified: Other Injury Type	37	1.52%
Shoulder & Upper Arm Sprains & Strains	34	1.39%
Lumbar VCI Sprains & Strains	32	1.31%
Shoulder & Upper Arm Dislocation	31	1.27%
Hand & Wrist & Fingers Sprains & Strains	29	1.19%
Foot & Toes Open Wounds	29	1.19%
Other & Unspec Lower Extrem Unspecified	< 25	$< 1\%$
TBI: Other Injury Type	< 25	$< 1\%$
SCI: Other Injury Type	< 25	$< 1\%$
Total	2, 442	100%

Table A3: Pre-, Post-Injury Opioid Prescribing (Average Daily MME), Ages 50-58 at Injury

Avg Daily	Mean	SD	Min	p25	p50	p75	p90	p95	Max	N
MME over ...										
30 days pre-index	1.17	16.48	0.00	0.00	0.00	0.00	0.62	1.64	616.44	2,442
90 days pre-index	1.18	15.85	0.00	0.00	0.00	0.00	0.00	2.50	630.00	2,442
365 days pre-index	1.20	16.03	0.00	0.00	0.00	0.00	0.00	1.00	630.00	2,442
Avg Daily MME over ...										
30 days post-index	4.91	18.99	0.00	0.00	0.00	4.35	13.79	24.02	670.65	2,442
90 days post-index	2.92	16.44	0.00	0.00	0.00	1.65	6.59	10.71	617.14	2,442
180 days post-index	2.09	14.83	0.00	0.00	0.00	1.24	3.98	7.18	608.62	2,442

Figure A2 shows the probability of opioid receipt by month relative to the index ED visit.

Table A4 shows the probability of post-injury opioid prescriptions, stratified by opioid-naive status, for patients aged 50-58 at injury.

Table A4: Probability of Post-Injury Opioid Receipt, by Pre-Injury Opioid Receipt, Ages 50-58 at Injury

Time Period Relative to Index ED Visit	All	Opioid-Naive	With Pre-Injury Opioid Prescriptions
1 year pre-index	16.34%	0.00%	100.00%
30 days post-index	32.39%	28.19%	53.88%
90 days post-index	35.38%	30.79%	58.90%
180 days post-index	39.68%	35.00%	63.66%
N	2,442	2,043	399

Table A5 shows reports the probability of Medicare enrollment among the older adult subsample at six points in time relative to the index ED visit, stratified by pre-injury opioid receipt and post-injury opioid receipt. (Medicare enrollment at 29 and 30 months post-index was not submitted to CO APCD output review, and so cannot be reported in this draft of the paper).

Figure A2: Probability of Opioid Receipt by Month Relative to Index ED Visit, Ages 22-58 at Injury

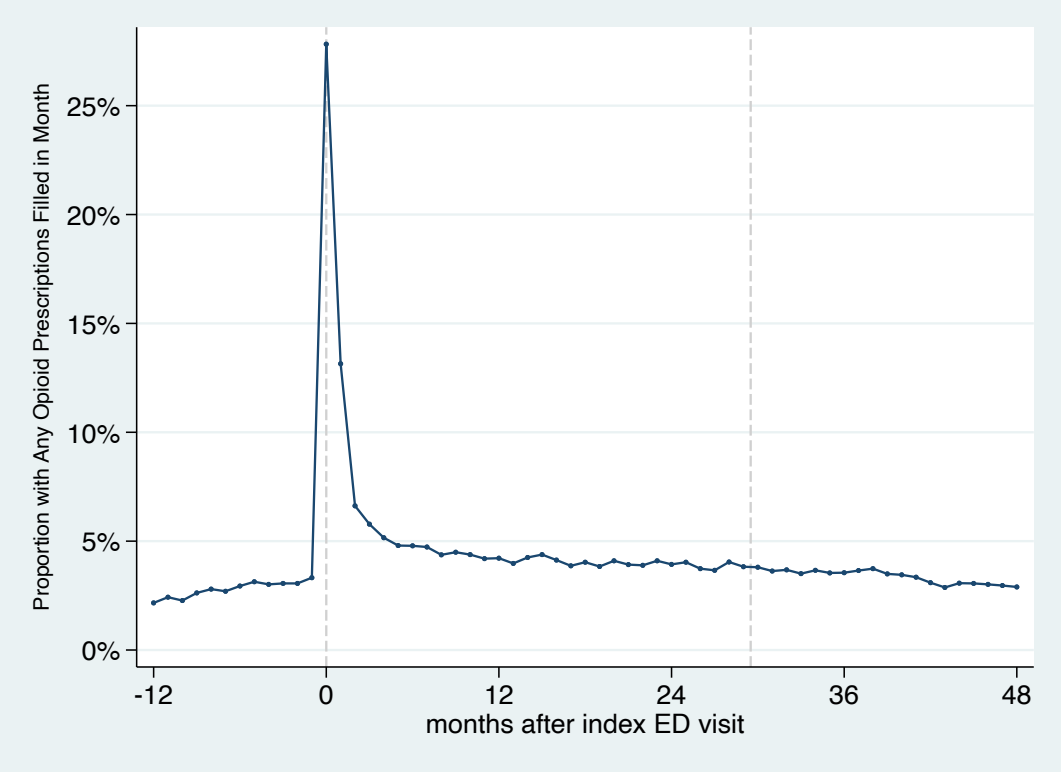


Table A5: Post-Injury Medicare Enrollment by Pre-, Post-Injury Opioid Prescription Receipt, Ages 50-58 at Injury

	Opioid-Naive			With Pre-Injury Opioid Rx			Total
Any opioid prescriptions over 180 days post-index?	N	Y	Total	N	Y	Total	Total
Medicare Enrollment At							
12 months post-index	0.23%	0.28%	0.24%	0.69%	1.97%	1.50%	0.45%
24 months post-index	0.45%	0.42%	0.44%	1.38%	1.97%	1.75%	0.66%
36 months post-index	0.68%	1.40%	0.93%	1.38%	5.12%	3.76%	1.39%
48 months post-index	1.36%	2.24%	1.66%	3.45%	6.30%	5.26%	2.25%
N	1,328	715	2,043	145	254	399	2,442
N Medicare-Enrolled at 48 months post-index	18	16	34	5	16	21	55