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DO OPIOIDS HELP INJURED WORKERS RECOVER AND GET BACK TO WORK? THE IMPACT OF OPIOID PRESCRIPTIONS ON DURATION OF TEMPORARY DISABILITY

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Do Opioids Help Injured Workers Recover and Get Back to Work? The Impact of Opioid Prescriptions on Duration of Temporary Disability
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ABSTRACT

We estimate the effect of opioid prescriptions on the duration of temporary disability benefits among workers with work-related low back injuries. We use local opioid prescribing patterns to construct an instrumental variable that generates variation in opioid prescriptions but is arguably unrelated to injury severity or other factors affecting disability duration. Local prescribing patterns have a strong relationship with whether injured workers receive opioid prescriptions, including longer-term prescriptions. We find that more longer-term opioid prescribing leads to considerably longer duration of temporary disability, but little effect of a small number of opioid prescriptions over a short period of time.

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I. Introduction

When used in accordance with evidence-based guidelines, opioids are an important tool for health care professionals in their quest to provide compassionate and reasoned care. However, what began as a well-intentioned effort to relieve what was thought to be undertreated pain has developed into an opioid crisis of epidemic proportions.[1]

While short-term opioid therapy can provide pain relief to some patients, the benefits of opioid therapies must be weighted against risks of addiction development, abuse, or other potential side effects (see the review in Noble et al., 2010). Evidence suggests that patients who receive multiple opioid prescriptions or patients who are prescribed large daily doses are at greater risk of experiencing overdoses (Paulozzi et al., 2012; Bohnert et al., 2011; Dunn et al., 2010; Franklin et al., 2005; Gomes et al., 2011). Opioid use is associated with nonfatal overdose hospitalizations (Coben et al., 2010), increased likelihood of emergency department visits (Braden et al., 2010), and historically has been a major cause of deaths from unintentional poisoning (Paulozzi and Annest, 2007; Paulozzi, 2012).

Opioid use is common among workers injured at work, with recent studies showing that more than half of injured workers off work for more than seven days with pain medications who did not have surgery received an opioid prescription, and many of them received opioids on a longer-term basis (Thumula et al., 2017). This raises the question about the potential impact of opioid prescriptions on outcomes that workers experience after an injury: Given the risks from opioids, are there nonetheless important benefits that might make the tradeoff worthwhile? We focus on a key potential benefit from the point of view of workers' compensation policy—the duration of disability. Although some of the adverse effects of opioid use would be expected to lengthen duration of disability, there could also be some benefits, via pain reduction, that enable

[1] See, e.g., https://www.hhs.gov/opioids/about-the-epidemic/ (viewed April 4, 2018).

faster return to work. To address this question, we examine the relationship between multiple measures of opioid prescribing and the time that injured workers spend on temporary disability benefits while recovering from an injury.

Work-related injuries represent a substantial share of injuries that occur to working adults. Nearly half of all trauma injuries to working adults were deemed to be work-related and were paid by workers' compensation insurance, and one in five injuries for soft-tissue conditions were deemed work-related (Victor et al., 2015). This suggests that the opioid medications provided in the workers' compensation system are a non-trivial part of the prescribing to which working adults are exposed.

Several studies establish a correlation between opioid prescribing and longer durations of temporary disability benefits. However, this relationship could be non-causal, driven by prescribing of opioids for more severe injuries that, independently, are associated with longer durations of disability. While past studies tried to control for injury severity, unmeasured injury severity may be correlated with both opioid prescribing and the duration of temporary disability. Moreover, opioid prescriptions may be a marker for worker characteristics that result in longer time away from work unrelated to the actual opioids, or, conversely, workers may choose to use opioids to speed up their return to work. Thus, a causal analysis of the effects of opioid prescriptions on the duration of disability is needed.

In this paper, we use empirical methods designed to estimate the causal effect of opioid prescriptions on the duration of disability. We rely on an instrumental variables (IV) approach that isolates the variation in individual opioid prescriptions that is driven by local prescribing patterns, rather than by individual characteristics, preferences, or decisions of workers (or their providers) such as injury severity or desire for faster return to work. This strategy assumes that local prescribing patterns influence whether an injured worker receives prescriptions for opioids, in a manner that is independent of the characteristics, preferences, or decisions of workers or their providers. This is plausible, because it is unlikely that either unmeasured injury severity or propensity to return to work varies systematically across the areas we study, especially given the rich controls included in our models—and indeed we present evidence supportive of this assumption. The local area variation in opioid prescribing patterns that we use in our analysis is relevant from a policy perspective, since policymakers can shape prescribing patterns through a variety of policies.

We examine several measures of opioid prescribing, but our most important empirical results concern longer-term prescribing of opioids. We estimate the impact of opioid prescriptions on the duration of temporary disability benefits, which are received for periods when workers cannot work while recovering from injuries. This duration is strongly correlated with the length of time until return to work, but it is not identical. However, the duration of temporary disability benefits can be measured in a very large sample available for analysis. Our results reflect a combination of the effects opioids may have on return to work because of reductions in pain as well as the potential addictive nature of opioids—effects that presumably act in opposite directions. We do not examine therapeutic, pain management effects of opioids.

Our analysis focuses on claims for which the primary diagnosis is a low back injury, for several reasons. Low back cases are quite common in the workers' compensation system,[2] and prior research has highlighted higher use of opioids for low back pain claims.[3] In addition, evidence-based treatment guidelines recommend against long-term use of opioids for these cases—reserving opioid prescriptions for the most severe cases and only for limited duration,[4]

[2] Cases with spine sprains, strains, and non-specific pain cover between 11 and 19 percent of claims with more than seven days of lost time across states included in the CompScope™ Benchmarks studies (Table TA.8b, Dolinschi and Rothkin, 2017).
[3] For example, estimates in Thumula et al. (2017) show higher use of any opioids and two or more opioids for low back pain claims than in most other injury groups.
[4] See, for example, the discussion in Bigos et al. (1994) or Koes et al. (2001).

suggesting that some of the longer-term opioid prescribing for this group of cases may be excessive. Finally, it is most straightforward to develop controls for injury severity for a narrow group of injuries.

2. Prior Evidence on Opioid Prescribing in Workers' Compensation

Opioids prescribing for injured workers is common and varies widely across states. Thumula et al. (2017) report that the percentage of nonsurgical claims with pain medications that had opioid prescriptions varied from just over 50 percent to over 80 percent across 26 states in that analysis, with 65 to 75 percent of workers who had pain medications receiving at least one opioid prescription across most states, and 25 to 58 percent of workers who had pain medications receiving two or more opioid prescriptions.[5] The amount of opioids per claim (measured as the average morphine equivalent amount (MEA) per claim with opioids) also varied widely across states, although most variation in opioid prescriptions came from differences in duration of opioid prescriptions, rather than variation in MEA daily dose. Longer-term prescribing of opioids is quite common. Wang (2017) found that across 26 study states, between 4 and 18 percent of workers with opioids received opioids on a longer-term basis, defined as having opioid prescriptions within the first three months after an injury and three or more filled opioid prescriptions between the 7th and 12th months after an injury (the same measure of longer-term opioids prescribing that we use). Both Thumula et al. (2017) and Wang (2017) find recent decreases in opioid prescribing among nonsurgical cases, which may be partly attributable to national, state, and local regulatory changes to combat opioid use and abuse, including mandatory use of prescription drug monitoring programs (PDMPs) by prescribers and pharmacies, adoption of guidelines for prescribing opioids, regulation of pain clinics, implementation of drug formularies, and requirements for continuing

[5] Similarly, looking at cases with acute work-related low back pain in 2002–2003, Webster et al. (2009) found that the percentage of cases with opioid prescriptions within the first 15 days after an injury varied from 6 percent in Massachusetts to over 50 percent in South Carolina.

medical education on appropriate opioid prescriptions.

Many studies found longer duration of temporary disability benefits for low back pain cases with opioid prescriptions (Mahmud et al., 2000; Webster et al., 2007; Franklin et al., 2008; Volinn et al., 2009; and Shraim et al., 2015). These studies typically found stronger relationships with duration of temporary disability for larger numbers of prescriptions or amounts of opioids, longer-term filling of opioid prescriptions, or more potent opioids (Schedule II versus Schedule III or IV). Similar evidence was also reported for a broader sample of workers' compensation cases (Gross et al., 2009). However, these studies do not establish a causal link between opioid prescriptions and outcomes. Most of the studies were primarily concerned with controlling for injury severity to try to compare disability durations for similar injuries, which they did in different ways: focusing on a very narrow group of injuries (Mahmud et al., 2000); controlling for condition severity with International Classification of Diseases, 9th Revision (ICD-9) coding (Webster et al., 2007; Shraim et al., 2015); controlling for the nature of the injury, body part, and cause of injury (Swedlow et al., 2008; Gross et al., 2009); or controlling for medical severity ratings derived from detailed medical records (Franklin et al., 2008).

The central concern is that these studies do not account for correlations between unobserved characteristics of workers, opioid prescribing, and return-to-work outcomes, and hence they may generate biased estimates of the actual effects of opioids on disability duration.[6]

3. Data

Our analysis sample was derived from payment information on workers' compensation claims—the WCRI Detailed Benchmark/Evaluation (DBE) database. The DBE covers claims

[6] Some studies directly acknowledge this problem. For example, Franklin et al. (2008) state that "the correlational nature of this study precludes the ability to draw causal inferences concerning the role of early opioid prescription. It is possible that early opioid prescription is a marker for other patient or health care provider characteristics or behaviors that might play roles in development of long-term disability" (p. 203). And Mahmud et al. (2000) state explicitly that their study only "documented associations between certain initial clinical management factors and disability duration. It was not possible to determine whether this association was causal in either direction" (p. 1,186).

from national and regional insurers (including residual market carriers), state funds, and self-insured employers (from their third-party administrators). We extract data on workers, employers, injury characteristics, opioid prescriptions, and duration of temporary disability benefits. Data on opioid prescriptions and duration of benefits is based on payors' records on payments made within 24 months after an injury.[7] The analysis includes workers injured between October 1, 2008, and September 30, 2013,[8] in the 28 states covered in the DBE database.[9] These states represent over 80 percent of benefits paid (Sengupta et al., 2014).

Sample of Workers with Low Back Injuries

Our sample is restricted to claims for which a low back condition was a primary diagnosis code on at least two eligible physician office visits that occurred prior to invasive treatment (surgery or injection), within the first year of an injury (following Yee et al., 2015).[10] The office visit criterion ensures that the sample includes workers who were diagnosed with low back injuries prior to receiving extensive treatment for such injuries, to avoid "diagnosis bias" resulting from providers selecting diagnosis codes that justify their medical treatment. We further limit our sample to cases where more than two-thirds of the office visits included a low back injury diagnosis, to increase the likelihood that our cases are predominantly low back injuries. Finally, our study is restricted to claims with more than seven days of lost time.[11] About 4 percent of all claims with more than seven days of lost work time satisfied our low back pain criteria.

Opioid Prescription Measures

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[7] Our results are robust to using data at 12, 36, or 48 months of maturity (results available upon request).
[8] We define injury year 2009 (for example) as claims arising from October 1, 2008, through September 30, 2009. For these 2009 claims.
[9] The states are Alabama, Arkansas, Arizona, California, Connecticut, Florida, Georgia, Illinois, Indiana, Iowa, Kansas, Kentucky, Louisiana, Maryland, Massachusetts, Michigan, Minnesota, Missouri, Nevada, New Jersey, New York, North Carolina, Pennsylvania, South Carolina, Tennessee, Texas, Virginia, and Wisconsin.
[10] We selected cases with ICD-9-CM (International Classification of Diseases, 9th Revision, Clinical Modification) codes that were considered to be diagnoses related to the lumbar region of the spine. The diagnosis codes are available from the authors upon request.
[11] We impose this condition to eliminate differences across states in waiting periods for the receipt of temporary disability benefits. (Some states have waiting periods of fewer than seven days, but none have longer waiting periods.)

Information on opioid prescriptions comes from detailed prescription transaction data collected from workers' compensation payors and their medical bill review and pharmacy benefit management vendors.[12] We capture opioid prescriptions that workers filled, which were paid for by workers' compensation payors. We cannot measure whether workers take opioids, nor can we observe prescriptions filled outside the workers' compensation system. Prescription information is not available for every claim. For low back injuries with more than seven days of lost work time, about 27 percent did not have information on filled prescriptions.[13] Since we cannot be certain about the nature of the drug therapy when prescription information is missing, the analysis sample includes only claims with filled prescriptions.[14]

We focus on longer-term opioid prescriptions, but also explore other measures, listed in the bottom panel of Table 1. Some of these claims only had non-opioid pain medications, muscle relaxants, or other non-pain medications; other claims may have received opioids for a short period of time; while other claims may have received opioids on a longer-term basis. About 71 percent of workers in our sample received at least one opioid prescription within 24 months after an injury during the study period. The other 29 percent received only prescriptions for non-opioid pain medications, prescriptions for muscle relaxants, or other non-pain medications.

Our measure of longer-term opioid prescribing captures workers who had opioid prescriptions within the first three months after an injury and later had three or more filled opioid prescriptions between the 7th and 12th months after an injury (following Wang, 2017). About 12 percent of workers with low back injuries in our sample had longer-term opioid prescriptions.

[12] For more details about the pharmaceutical data, see Thumula et al. (2017) and Wang (2017).

[13] We suspect that some workers in this group may have been prescribed common pain relievers that they already had in their medicine cabinet, so they did not fill a prescription at the pharmacy. Other workers may have used their group health insurance to fill their first few prescriptions. Since we examine the sample of injured workers with a diagnosis related to the lumbar region of the spine, it is hard to imagine that these claims had no medications prescribed.

[14] This has two implications. First, the sample of workers with prescriptions may be skewed toward costlier and more severe cases. Second, some individuals classified as not receiving prescriptions may have received prescriptions via group health, in which case our estimated effects of opioid prescriptions would be understated if the cases without prescription information were included in the analysis sample and coded as not having opioid prescriptions.

Table 1 also reports the shares of workers with multiple opioid prescriptions but not longer-term prescriptions per this definition. Nearly 40 percent of workers had two or more prescriptions but not longer-term prescriptions, and 28 percent had three or more prescriptions but not longer-term opioid prescriptions.

We also capture the MEA per claim with opioids.[15] The average MEA for opioids prescribed for low back cases was more than 4,700 milligrams, and the median was 900 milligrams. We examine sensitivity to cases with larger amounts of opioids by looking at cases with MEAs of more than 2,600 milligrams and MEAs of more than 8,000 milligrams—mean and median MEAs for workers with at least three prescriptions. Looking at estimates for these thresholds addresses concerns that the focus on the number of prescriptions, or longer-term opioid prescriptions, per our definition, may not reveal how much of the drug the person was getting since, in principle, one could have a higher MEA with fewer than three prescriptions and a lower MEA with more than three prescriptions.

Outcome: Duration of Temporary Disability Benefits

Our outcome is the number of weeks of temporary disability benefits that workers receive within 24 months after an injury.[16] Workers receive temporary disability benefits while they are away from work recovering from an injury. The temporary disability benefits often end when workers return to work, when they are released to work by their doctor, when they reach maximum medical improvement, or when they receive permanent partial disability benefits and/or a lump-sum settlement.

[15] This measure is constructed by applying a morphine equivalent equianalgesic conversion to prescriptions of different strengths. For each claim, we calculate a cumulative MEA taking into account the strength in milligrams of each prescribed opioid medication, the analgesic potency ratio between the specific opioid and morphine, and the quantity of the prescription. For example, an MEA of 3,500 milligrams per claim is equivalent to taking a 5-milligram Vicodin® tablet every four hours for nearly four months. For more details, see Thumula et al. (2017, pp. 27–28).

[16] We calculate weeks of temporary disability benefits by dividing temporary disability payments that each worker received by the worker-specific weekly benefit rate. In fewer than 1 percent of cases, there were missing data on the wage or benefit rate, in which case we divided by the state average benefit rate.

Rules about how temporary disability benefits end vary across states. For instance, duration of temporary disability benefits is typically longer in states with wage-loss benefit systems since these states do not shift workers to permanent partial disability payments when workers achieve permanency in their condition, but instead workers often continue receiving temporary disability benefits.[17] We therefore compare duration of temporary disability within each state, where the same rules and procedures for determining temporary disability benefits apply; we do this by estimating models with state fixed effects.

The duration of temporary disability benefits does not exactly reflect the duration of time that workers were away from work. For example, temporary disability benefits may end when workers start receiving permanent partial disability benefits or when workers choose to settle their claim. In some states, temporary disability benefits may be terminated while workers resolve disputes about ability to return to work or disputes about remaining impairment.[18] Finally, the ultimate duration of time off work may not be observed for claims that remain open.

Other Controls

We control for a rich set of covariates that could affect the duration of temporary disability benefits. For example, older workers are less likely to return to work, and workers in some industries (such as construction) may have unique return-to-work problems (Galizzi and Boden, 1996). We include the following worker controls in our regression models: age, gender, marital status, tenure at the time of injury, and preinjury wages. Workplace characteristics include firm's payroll size and dummy variables distinguishing industries and occupations based on injury risk.[19]

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[17] Wage-loss benefits are intended to compensate for what workers earned before their injury.

[18] We exclude from our sample cases with no temporary disability benefits that later received permanent partial disability/lump-sum payments. These cases are likely disputes about compensability that were later paid as a lump-sum settlement. We also exclude from the sample fewer than 1 percent of cases where the number of weeks of temporary disability duration was unreasonably high, i.e., greater than the 104 weeks that we can expect with 24 months of maturity data.

[19] These include high-risk services, low-risk services, clerical/professional occupations (regardless of industry), manufacturing, construction, trade, and other industries (see Dolinschi and Rothkin, 2017).

Characteristics of the local labor market may also affect how long workers stay out of work. We control for the county unemployment rate[20] and residence in a rural zip code.[21] We also control for two other characteristics of the population that may affect ability to work—the percentage of residents with less than a high school degree at the Primary Care Service Area (PCSA) level,[22] and the percentage of residents who were disabled at the county level.[23]

We developed controls for injury severity specific to the low back conditions we study. Our controls categorize injury severity based on the diagnosis and the treatment that workers received. For surgical cases, we developed a severity scoring system based on the intensity and extent of treatment that workers received. This improves on approaches in prior studies that controlled for nature of injury, body part, and cause of injury (Swedlow et al., 2008; Gross et al., 2009), or controlled for broad categories of condition severity reflected in ICD-9 coding (Webster et al., 2007; Shraim et al., 2015).

The idea behind our approach is that the extent of surgical intervention reflects the potential severity of the injury that workers experience—more extensive procedures may reveal more severe injuries. Taking into account medical bill level information about medical procedures and diagnostic codes, our scoring system assigns points based on different types of surgical procedures (discectomy/decompression, fusion, or both); number of levels operated on (one, two, or more than two); and number of procedures (one, two, or more than two). This measure ranges

[20] We use the U.S. Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS); see https://www.bls.gov/lau/.
[21] We use the ZIP code Rural-Urban Commuting Areas (RUCAs) geographic taxonomy; see https://ruralhealth.und.edu/ruca.
[22] Less-educated workers likely face many fewer jobs for which they are qualified.
[23] These last two measures are derived from Area Resource Files. The zip code to PCSA crosswalks used to construct our data were obtained from The Dartmouth Atlas, which is funded by the Robert Wood Johnson Foundation and the Dartmouth Clinical and Translational Science Institute, under award number UL1TR001086 from the National Center for Advancing Translational Sciences (NCATS) of the National Institutes of Health (NIH). See http://www.dartmouthatlas.org/downloads/pcsa/zip5_pcsav31.dbf. The Area Resource Files are based on data from the 2010–2014 American Community Survey (ACS) Summary File.

from zero to nine points, with more complex treatments having a larger number of points.[24] Nonsurgical cases have zero points, but have separate controls for a three-way classification based on intensity of treatments: cases with medications only; cases with medications and physical therapy only; and cases with medications, physical therapy, and injections.[25] The use of separate controls for surgical and nonsurgical severity is equivalent to interacting severity controls with surgery and nonsurgery indicator variables.

4. Methods

We specify the relationship between duration of temporary disability and opioid prescriptions measures as:

$$\mathrm{Log}(Y_{ijst}) = \alpha + \beta\cdot OPSCRIP_{ijst} + X_{ijst}\delta + YR_t\lambda + ST_s\gamma + \varepsilon_{ijst}. \qquad (3.1)$$

Y_{ijst} is duration of temporary disability for worker *i*, in area *j* of state *s*, in year *t*.[26] $OPSCRIP_{ijst}$ is the opioid measure of interest. X_{ijst} is a vector of the control variables. YR_t is a vector of year fixed effects, and ST_s is a vector of state fixed effects.

We first estimate equation (3.1) using ordinary least squares (OLS). This approach provides descriptive evidence of the associations between opioid prescriptions and duration of temporary disability. However, these associations may reflect unobserved worker and/or injury characteristics that are correlated with both opioid prescriptions and duration. We think there are most likely two kinds of unmeasured characteristics of workers/injuries that can lead to biased estimates of the causal effects of opioid prescriptions. First, opioid prescriptions can be associated with unmeasured differences in injury severity that directly affect duration of temporary disability benefits, leading the OLS estimate of β to overstate the extent to which opioid prescriptions inhibit

[24] More details on the construction of this measure is available from the authors upon request.
[25] The third group also includes about 3 percent of cases with injections that did not have physical therapy services.
[26] The *j* index is explained below.

return to work (increase the duration of disability).[27]

Second, opioid prescriptions can be associated with unmeasured differences in propensity to return to work quickly. We do not know, a priori, the direction of this association and hence the direction of bias from this unmeasured propensity. If, for example, workers who have a higher propensity to return to work are also more likely to fill opioid prescriptions (conditional on the other controls), because they use opioids to help ease return to work, then OLS estimates can understate the negative effect of opioids on duration of disability. Alternatively, workers less prone to return to work after an injury may seek opioid prescriptions, in which case the bias would overstate the extent to which opioid prescriptions inhibit return to work.

Instrumental Variable Approach

Our IV approach attempts to purge the estimates of the impact of unmeasured injury severity or propensity to return to work and, hence, to obtain unbiased (formally, consistent) estimates of the causal effects of opioid prescriptions.[28] This approach requires an exogenous source of variation that affects opioid prescriptions but does not affect temporary disability duration except via its influence on prescriptions. Our IV is local area prescribing patterns (OPLOCAL). For each individual observation we determine local area prescribing rates based on low back cases in an area (indexed by *j*). The opioid prescription rate for injured worker *i* in area *j* in year *t* is based on all workers in area *j*, excluding individual *i* (we denote the number of observations in area *j* (in state *s* and year *t*) as N_{jst}). Thus, the formula for $\text{OPLOCAL}_{\text{ijst}}$ is

[27] While we believe we improve on the measures of injury severity used in past studies, we cannot be sure that our measures fully capture injury severity.

[28] We use two-stage least squares (2SLS). We do not estimate duration models that are sometimes used to examine spells of time away from work that are not fully observed (censored). Instrumental variables approaches are considerably more complicated with duration data that could be censored (e.g., Bijwaard, 2009). Our results are not sensitive to potential censoring concerns; the results change little when we use duration of temporary disability measured at 36 or 48 months after an injury. Since we use a natural logarithm of temporary disability duration, our approach (in the absence of censoring) is essentially a log-linear model that can be interpreted as a proportional hazard model with a constant hazard for leaving disability.

$$\text{OPLOCAL}_{\text{ijst}} = \frac{\sum_{\text{i}' \neq \text{j}} \left(\text{OPSCRIP}_{\text{i}'\text{jst}}\right)}{N_{jst}-1}. \quad (3.2)$$[29,30]

It is typical to interpret IV estimates as local average treatment effects that put the most weight on effects for those whose opioid prescription variation is driven by variation in the local prescription patterns (the "compliers"). In our view, this is a strength of the IV that we use, because variation in prescribing patterns across areas is the type of variation that policymakers seem likely to be able to influence via policies regulating prescriptions of opioids in workers' compensation cases. We discuss examples of these policies below, when we interpret our estimates.

We define local areas using Hospital Referral Regions (HRRs) developed by the Dartmouth Atlas.[31] We use HRRs as the geographic unit for defining prescribing patterns because they provide a high percentage of within-area prescriptions relative to other possible geographic measures we could use, while allowing for variability in prescriptions across parts of the state. About 74 percent of filled prescriptions for opioids were prescribed by physicians with offices in the same HRR as the worker's residence (and another 14 percent were in a different HRR in the same state). Other definitions of geographic areas, such as CBSAs, produced considerably lower percentages of prescriptions that were prescribed within the same area as workers' residences. We only use observations that are in areas with at least 15 observations within each HRR and year

[29] Excluding individual *i* from this estimation avoids creating a mechanical correlation between the instrument and individual opioid prescriptions—variation that would *not* be purged of unmeasured injury severity or propensity to return to work.

[30] Note that $\text{OPLOCAL}_{\text{ijst}}$ is based on the observed surgical and nonsurgical mix of cases. We want the prescribing pattern measure to reflect only the variation in prescriptions for the same kind of treatment, so that it should also reflect the case mix. We use the same definition of opioid prescriptions (listed in Table 1) for both OPSCRIP and OPLOCAL, depending on the opioid prescribing measure for which we are estimating the specification.

[31] These areas are determined based on use of medical services by Medicare patients. The Dartmouth Atlas divides the United States into 3,436 distinct hospital service areas (HSAs); the areas are defined so that Medicare patients living in an HSA get the majority of their health care from hospitals within the area. These areas are then grouped into 306 HRRs. For each claim, we determined the HRR based on the zip code associated with the claim, based on the worker's zip code of residence. When an injured worker's zip code of residence was missing, we used the zip code of the injured worker's employer. If both of those zip codes were missing, we used the zip code of the first physician office visit for the injured worker. If all three were missing, we used the zip code in which the injury occurred.

combination for creating the IV. Our sample of states includes 219 HRRs, some of which cross state lines.

Instrument Validity

One condition for the validity of the instrument is that it strongly predicts whether workers received opioids. We present evidence of strong predictive power of the IV below. The second condition for a valid instrument is that the IV is not correlated with unobserved claim or claimant characteristics that affect the duration of disability (such as unobserved injury severity or propensity for return to work), conditional on the controls. We cannot directly test this exclusion restriction, but there are good reasons to expect it to hold, and we report evidence below that bolsters this assumption.

Variation in the instrument reflects prescribing patterns for all other workers (excluding the individual) within the same HRR with low back injuries, which should be driven by the treatment patterns followed by doctors within the local area, for other patients. It seems unlikely that prescription patterns for other workers in the same local area would be correlated with an individual worker's duration of disability, conditional on their own opioid prescriptions. Nonetheless, there are possible reasons the exclusion restriction could be violated. In principle, workers could somehow be sorted across high- and low-prescription areas in a way that generates a correlation between prescribing patterns and unobserved injury severity or propensity for return to work. However, this seems unlikely given our extensive controls. For example, although particular areas could have overrepresentation of specific industries and, hence, injuries that are correlated with both prescribing patterns and return to work, we control for industry and injury severity. Some areas may have an older workforce, and age could influence both prescribing patterns and return to work, but we control for workers' ages. Moreover, none of these possibilities explain why local prescribing patterns should matter once we condition on the individual worker's opioid prescriptions.

Yet another dimension of sorting is the variation in surgery rates for similar industries, workers, etc. But we control for treatment variation and the case mix. Local labor market conditions could also matter. For example, weak labor markets could create slower return to work and perhaps also be associated with more opioid use or abuse. To address this, we control for county-level unemployment rates and the percentage of workers with less than a high school education. There could also be regional variation in comorbidities that affect return to work and are correlated with opioid prescriptions. But this should be accounted for by our control for county-level variation in the percentage of disabled workers.

It is possible, in principle, that workers strategically move to HRRs with higher rates of opioid prescriptions when they are more likely to use (or want to use) opioids. But this source of mobility appears unlikely given the size of the local areas that we chose for our analysis.

A more challenging possibility is that workers with a greater need for opioids may gravitate to the high-prescribing doctors within an HRR. This would result in higher average prescribing rates in areas where workers want to use a lot of opioids (even if the distribution of doctors by "innate" prescribing rates—i.e., what we would observe, hypothetically, prior to this sorting—is the same in each HRR). We address this concern by exploring variation in prescribing patterns measured at the physician level rather than the worker level. In particular, we give each provider in an area equal weight when constructing local area prescribing rates to avoid, say, inflating the local prescribing pattern in HRRs where, over the course of treatment, workers gravitate to high-prescribing providers. We show, below, that our results are robust to this alternative specification of the IV (and to using another closely related approach). We believe that this additional evidence strongly suggests that doctor shopping does not drive the results. However, we cannot rule out the possibility that doctor shopping is reflected in the first provider a worker visits—perhaps based on a priori information from other workers about which providers are more likely to write opioid prescriptions. Thus, while our evidence makes it less likely that doctor shopping drives our results and renders our estimates non-causal, we cannot decisively rule this out.

Another potential issue that could undermine the validity of the IV is if physicians who prescribe opioids also use other practices that lead to longer durations of disability (including disability assignment). However, we think this problem is likely minor. If we were just using physician-level prescribing as the IV, the IV could well be correlated with other physician practices. However, we are using HRR-level prescription variation, and there is less reason to believe that individual-level treatment variation (aside from opioid prescribing) varies with HRR-level opioid prescribing. Finally, variation in other practices that is correlated with opioid prescribing is not problematic if the other practices are ultimately driven by opioid-prescribing behavior. In that case, accounting for these other physician practices would be overcontrolling (controlling for variation in opioid prescribing).

5. The Effects of Opioid Prescriptions

Descriptive Statistics

Table 1 provides descriptive statistics on opioid prescriptions and claim outcomes, for various measures of opioid prescriptions: whether workers had any opioid prescriptions in the 24 months after an injury, the number of opioid prescriptions excluding longer-term use, and longer-term prescribing of opioids. The first row shows that the average duration of temporary disability benefits was about 15 weeks longer for claims where workers had any opioid prescriptions. The differences were still large, although less pronounced, for median duration.

Columns (4)-(7) indicate that the differences in measures of temporary disability duration were driven primarily by workers with multiple opioid prescriptions, and more so by workers with longer-term opioid prescriptions. Looking first at prescription counts and excluding longer-term use, we observe a relatively small difference in duration (1 week) between workers with one prescription and workers with no prescriptions. Workers with two prescriptions had 6 weeks longer duration (about a 50 percent difference), and workers with three or more prescriptions had nearly 20 weeks longer duration (almost three times the duration of workers with no prescriptions). We find much longer duration of temporary disability for those with longer-term opioids, for whom average duration was nearly a year (51.6 weeks). These statistics imply that it is important to separately examine the effects of more-intensive prescribing of opioids, and not just any opioid prescriptions versus no opioid prescriptions.[32]

The lower panel of Table 1 reports additional information on opioid prescriptions, including the distribution of observations by number of prescriptions and longer-term prescribing, and information on MEAs associated with different prescribing patterns. We show statistics for the 2,600 milligram and 8,000 milligram MEA thresholds—the median and mean amounts for those with three or more opioid prescriptions. There is a strong link between longer-term opioid prescriptions and high MEAs. For example, 90 percent of those with longer-term prescriptions had an MEA exceeding 2,600 milligrams.

The last two columns show that outcomes and opioid prescribing varied substantially between surgical and nonsurgical cases. Surgical cases had longer duration of temporary disability benefits (51 weeks, versus 19 weeks for nonsurgical cases). Surgery is also strongly associated with opioid prescriptions—95 percent of surgical cases had at least one prescription, and one-third of workers with surgeries had longer-term opioid prescriptions. This difference is why we control in our models for whether a case is surgical or nonsurgical, and let the local prescribing patterns reflect the surgical/nonsurgical mix at the HRR level. We also take this one step further and

[32] Workers with only one prescription may include many workers who may have filled prescriptions but did not use them, or who used them short-term but quickly curtailed their use.

estimate separate models for the two types of cases.

Effects of Opioid Prescriptions on Duration of Temporary Disability: OLS Estimates

Table 2 reports OLS estimates that adjust for differences between claims based on characteristics of workers, employers, injuries, and workers' compensation systems. We show estimates using an increasingly detailed set of controls. Specification 4 provides the most compelling evidence among the OLS regressions, as it includes the controls for injury severity, workers, workplaces, and location. Comparing the estimates for the prior specifications is informative about the partial correlations among disability duration, opioid prescriptions, and the control variables. Since we estimate models for log duration, the estimates approximately reflect the percentage change in the duration of temporary disability for a one-unit change in the independent variables, although the approximation can be far from exact for the kinds of large estimates we obtain here. For example, the estimate of 84.8 in the first column (Specification 1) implies that the duration of temporary disability benefits was 133.5 percent higher when workers had at least one opioid prescription than when workers had no opioid prescriptions (base category), for otherwise comparable claims, as shown in the second row of the table.[33] The estimates in Table 2 closely parallel what has been done in past research on opioid prescribing and duration of disability.

While the estimates in Table 2 consistently point to a positive correlation between any opioid prescriptions and duration of temporary disability benefits, conditional on the controls, the estimates are sensitive to which controls we include. Going from Specification 1, with the fewest

[33] Note that we have multiplied the estimated coefficients by 100 to express them as percentages. The 0.848 coefficient estimate, when multiplied by 100, provides an approximate percentage effect (84.8), but this approximation is much more accurate for smaller estimates. A more precise estimate of the percentage change comes from taking the coefficients reported in the table (from equation (3.1), $\beta \cdot OPSCRIP$), and computing $100 \cdot \{\exp(\beta \cdot OPSCRIP/100) - 1\}$; using this calculation, the 84.8 estimate, for example, implies a 133.5 percent change. The standard error of the implied percentage effect (not reported, but used to compute significance levels) is computed from a first-order approximation to this function using the Delta method. The table also reports estimated coefficients for some of the controls; full model estimates are available from the authors upon request.

controls, to Specifications 3 and 4, with the most controls, reduces the strength of the positive relationship between any opioid prescriptions and duration of disability by about two-thirds (in terms of the percentage effect). Yet the association remains positive and strongly statistically significant; in Specification 4, with all the controls, we estimate that workers with any opioid prescriptions had a 42.9 percent longer duration of temporary disability benefits than workers without opioid prescriptions. Given the mean duration of 11.3 weeks for workers with no opioids (Table 1), this is an increase of about 4.8 weeks. The table also shows that workers with more severe injuries had long disability durations, as we would expect, and were also more likely to receive opioids, so controlling for severity reduces the association between any opioid prescriptions and disability duration.[34]

The Role of Local Prescribing Patterns

Before turning to the IV results, we present estimates of our first-stage regressions relating opioid prescriptions at the individual level to local prescribing patterns—which provide our instrumental variables. The predictive power of local prescribing patterns has to be high for the empirical strategy to be valid.

Table 3 provides estimates from the first-stage regressions, including the full set of controls from Specification 4 in Table 2. The evidence indicates—in the first panel of the table—that the IV passes this test for the estimation of the effect of receiving any opioid prescriptions. The estimate implies that a 10 percentage point increase in the local area opioid prescribing rate of any opioids is associated with a 3.4 percentage point higher likelihood that injured workers with

[34] The injury severity measures are strongly correlated with the duration of temporary disability benefits. For instance, for surgical cases, one additional point (one extra modality during a surgery) on the severity score is associated with 12 percent longer duration of temporary disability benefits. For nonsurgical cases, we found that cases that had medications and physical therapy services only, when compared with cases with only medications, had much longer duration of temporary disability (the coefficient estimate is 78). Nonsurgical cases that had medications, physical therapy, and injections had far longer duration of temporary disability benefits than nonsurgical cases with only drugs.

low back injuries will have any opioid prescriptions.[35] The F-statistic on the first-stage regression is very large (147).

The remaining panels of the table report estimates of models for the different measures of opioid prescriptions we use in subsequent analyses, including number of prescriptions, longer-term prescribing, and MEA thresholds. Each case uses the local prescribing pattern that corresponds to the individual-level prescription measure. We find that, regardless of the definition of opioid prescribing, local prescribing patterns are strongly predictive of individual prescription patterns, with injured workers in areas with higher prescribing rates significantly more likely to receive opioid prescriptions.

Note that the results in Table 3, in addition to helping validate our research method, provide interesting information on the extent to which individuals' opioid prescriptions vary across local markets, even for workers with the same measured injury severity, surgical mix, etc. We do not claim to know all of the sources of variation in prescribing patterns across HRRs. But we would suggest that this variation points to differences in prescribing behavior that can be shaped by workers' compensation policy and opioids policy more generally. And the strong relationships between local prescribing patterns and individual opioid prescribing imply that policy-induced reductions in prescribing in high-prescription areas could substantially reduce opioid prescriptions.

Effects of Opioid Prescriptions on Duration of Temporary Disability: Instrumental Variable Estimates

Panel A of Table 4 presents the 2SLS estimates (2SLS) of the effect of any opioid

[35] The local prescribing pattern is measured as a proportion from zero to one, and the dependent variable is either zero or one, so that a 10 percentage point increase in the local prescribing pattern has an effect on the probability of any opioid prescription equal to one-tenth of the estimated coefficient. Prior studies suggest that physicians play an important role in shaping opioid prescribing patterns. For instance, Barnett et al. (2017) documented large differences in opioid prescribing patterns across physicians even within the same hospital. Schnell and Currie (2017) also documented that physicians' education is strongly associated with opioid prescription patterns.

prescriptions and provides a comparison with the OLS estimates from Table 2. Again, we use specifications including the full set of controls from Specification 4 in Table 2.[36,37]

Once we correct for the endogeneity of prescriptions or correlations with unobserved injury severity, we find little evidence of a relationship between any opioid prescriptions and duration of temporary disability. The 2SLS estimate (4.5) is close to zero and statistically insignificant. Although the estimates in Panel A Table 4 suggest that opioid prescriptions do not lead to longer duration of disability, the specification based on any opioid prescriptions combines effects of short-term use (only one prescription) and multiple prescriptions or longer-term prescriptions. Thus, we next explore the effects of opioid prescriptions using different ways to characterize prescriptions. After discussing this more complete set of results, we will return to the issue of interpreting differences between the OLS and 2SLS estimates.

Since the analysis in Tables 2 and 4 is based on one or more opioid prescriptions, we first turn to examining how the results change if we estimate, instead, the effect of having two or more, or three or more, opioid prescriptions. Both OLS and 2SLS estimates for these specifications are reported in Panel B of Table 4, again for the specification including the full set of controls.

The 2SLS estimates in Panel B suggest that the impact of opioids varies with the number of prescriptions that workers filled. Cases with three or more prescriptions, when compared with cases with no prescriptions, had longer duration of temporary disability benefits, by 52 percent. This evidence, coupled with the weaker evidence of an effect for two or more versus no prescriptions (a statistically insignificant 20.2 percent longer duration), suggests that the results for any opioids in Panel A hide substantial differences across different numbers of prescriptions, and

[36] We report only the estimated coefficient of the opioid prescribing variable; full model estimates are available upon request.
[37] We are using individual-level data, and the variation in the treatment variable is at the individual level, so we are not clustering. The IV is aggregated to the HRR level, but that does not imply that the standard errors should be clustered.

that the effect of opioids on disability duration may arise only for workers with multiple prescriptions.

In Panel C, we report estimates for similar specifications, but now excluding from the set of observations those who had longer-term prescriptions, as defined earlier (and in the table notes). In this case, we no longer find evidence of an effect of multiple opioid prescriptions on the duration of disability; the estimated coefficients are closer to zero and not statistically significant.

The differences between the results in Panels B and C Table 4 suggest that it is longer-term opioid prescriptions that are responsible for longer durations of temporary disability. Panel D provides direct evidence of this, showing model estimates where we now characterize workers by whether they had longer-term opioid prescriptions. The 2SLS estimates in Panel D point to a strong effect of longer-term opioid prescribing on the duration of temporary disability benefits; workers with longer-term opioid prescriptions had durations of temporary disability that were 251 percent longer—or more than triple the duration of similar workers, with similar injuries, without opioid prescriptions.

Interpreting the Magnitudes

Our empirical analysis is intended to estimate the causal effect of opioid prescriptions. One can interpret this individual-level estimate as the effect of a policy change that eliminated, for example, longer-term opioid prescriptions. However, from a policy perspective we might ask a different question: What is a reasonable expectation for the reduction in opioids that policy could bring about, and if it did so, how much would we expect the duration of temporary disability benefits to fall?

To get a handle on this question, consider a 5-percentage point decrease in workers with low back injuries who get longer-term opioid prescriptions, from an average of 12 percent to 7 percent of cases (Table 1). This 5 percentage point decrease is a plausible policy effect, based on prior evidence that, after the implementation of Kentucky House Bill 1, the percentage of claims with pain medications that had any opioids decreased 10 percentage points, and the percentage of claims with pain medications that had two or more opioid prescriptions decreased 6 percentage points (Thumula, 2017).[38] Based on the 2SLS estimate in Panel D of Table 4, this change translates to a 12.6 percent decrease in the duration of temporary disability, or 2.8 weeks shorter average duration of temporary disability for workers with low back injuries.

Bias in OLS Estimates

In Panels A-C of Table 4, the 2SLS estimate is smaller, suggesting that OLS provides an upward-biased estimate of the effect of opioid prescriptions on duration of disability. The most plausible reason for upward bias, in our view, is failure of the severity controls to fully capture differences in severity, in which case a positive relationship between opioids and unmeasured severity would overstate the causal effect of opioid prescriptions. In contrast, the IV estimates using only the variation in prescribing patterns across HRRs would break the link between unmeasured severity and opioid prescriptions and, hence, eliminate the positive bias.

Interestingly, in Table D of Table 4 the 2SLS estimate of the effect of opioids—in this case, longer-term opioids—is larger, although not statistically significantly so, than the OLS estimate. This suggests that there is another factor at work that biases the OLS estimate downward. Our conjecture is that the other factor in this case is that some workers who want to return to work use opioids longer-term to enable them to do so, and ignoring this "endogenous" choice to use opioids longer-term obscures part of the causal effect of longer-term opioid prescriptions in increasing the duration of disability. This interpretation does not mean that, overall, longer-term opioids enable return to work. Rather, it just means that for some workers this

[38] Kentucky House Bill 1 required prescribers to check the prescription drug monitoring database prior to prescribing opioids, limited opioid prescriptions, and implemented mandatory educational and patient treatment practices. The Thumula study did not present evidence on changes in longer-term opioid prescribing.

mechanism is active. The 2SLS estimates imply that, on the whole, longer-term opioids lead to substantially longer duration of temporary disability.

Morphine Equivalent Amounts

Panels E-G of Table 4 focus on the effects on disability duration of whether the amount of opioids (MEA) that workers were prescribed was over 2,600 milligrams or over 8,000 milligrams. We estimate the effects of MEAs exceeding these thresholds three ways. First, we simply substitute dummy variables for whether MEAs exceeded these thresholds for the other measures of opioid prescriptions. These estimates, reported in Panel A, indicate that workers with MEAs over 2,600 milligrams had more than twice the duration of temporary disability benefits than workers with no opioids. Similarly, workers with MEAs over 8,000 milligrams had more than twice the duration of temporary disability benefits compared with workers without opioids. The 2SLS estimates are a bit larger than the OLS estimates, consistent with a positive causal effect of opioid prescription amounts above these thresholds.

Following on the findings from Panel D that longer-term opioid prescriptions drive longer durations of temporary disability, in Panels F and G of Table 4 we estimate models using the same MEA thresholds, first excluding claims with longer-term prescriptions, and then, conversely, including only the claims with longer-term prescriptions. In the former case—excluding longer-term prescriptions—the 2SLS estimates no longer point to statistically significant effects on durations, and the estimated effects are smaller than in Panel A (and imprecise). In contrast, for the longer-term cases reported in Panel G, higher MEAs lead to longer disability durations. Moreover, the estimates are similar for the two thresholds, suggesting—and consistent with the other estimates—that the key factor is whether or not opioids are prescribed on a longer-term basis. Note that the estimates are quite similar in Panels E and G, consistent with the evidence from Table 1 that longer-term prescriptions and high MEAs are strongly related. Thus, these results reinforce the conclusion that it is longer-term opioid prescriptions that lead to longer durations of temporary disability. Receipt of a small number of prescriptions (or even a large MEA) over a shorter period does not appear to lead to statistically significantly longer disability durations.

Heterogenous Effects

The relationship between opioid prescriptions and duration of temporary disability may differ between surgical and nonsurgical cases, perhaps because the same intensity of opioids may be more medically-indicated for the same diagnosis in surgical cases than nonsurgical cases. The results for nonsurgical cases are very similar to the full-sample results, as shown in Table 5.[39] We could not learn much about the much smaller number of surgical cases (a bit over 10 percent of cases) using our strategy, as the prescribing patterns IV did not strongly predict opioid prescriptions for this subset of cases. Moreover, it is not meaningful to examine the relationship between *any* opioids and duration of temporary disability benefits for surgical cases since nearly all surgical cases receive some opioids.

Robustness of Estimates and Validity of the Instrument

We conducted several checks examining whether the results are sensitive to different definitions of the dependent variable, to changes in the sample definitions, or alternative ways of constructing the IV that address potential challenges to the validity of the instrument.[40]

Our results are robust to using cost measures derived at different maturities. We continued to find a large, positive, and statistically significant effect of longer-term opioid prescriptions across claims with different maturities (12, 36, and 48 months), with all the estimates showing an approximate doubling to tripling of duration. This evidence implies that our findings are not

[39] We do not show all of the estimates from the previous tables. We show results for any prescriptions, two/three or more prescriptions excluding longer-term prescriptions, longer-term prescriptions, and high MEAs. As indicated above, the latter two sets of estimates both largely capture the effects of longer-term opioid prescriptions.
[40] We summarize some of the robustness checks here; full estimates are available from the authors upon request.

influenced by open claims.

We find similar percentage effects from using linear rather than log specifications for the duration of temporary disability benefits. Our main results are also robust to changing the threshold size of the local areas that we use for estimating local practice patterns.[41] Our results are also robust to alternative definitions of the comparison group that are not restricted only to those with no opioid prescriptions.

In our main analyses, we limit the sample to primarily cases with a low back pain diagnosis. An alternative sample specification is to relax some of the constraints imposed on the data. In particular, we explored the sensitivity of our estimates to the sample that no longer excludes cases where less than two-thirds of the office visits were for a low back pain diagnosis. We find that our pattern of results described in the main specifications holds with this change in the sample. While the cases included in this analysis are more heterogeneous, the precision of the estimates increases owing to the larger samples.[42]

We also estimated alternative specifications for the effects of longer-term opioid prescriptions, expanding or varying the comparison group to (1) include also those with one or two prescriptions; and (2) only include those with one or two prescriptions, but not zero. We do the latter because claims with one or two prescriptions may in some ways be more comparable to those with longer-term prescriptions, although we already include detailed severity measures and focus on a narrow set of injuries. On the other hand, the distinction relative to no opioid prescriptions is the least ambiguous one to measure. Regardless, the qualitative conclusions are the same in each case.

One robustness check merits more discussion, and we report the estimates. In our main

[41] This refers to either increasing or decreasing the minimum number of claims in an HRR to estimate the prescribing pattern—a minimum we imposed to increase the precision of the estimate.
[42] Results for this and other sensitivity analyses for which tables are not provided are available from the authors upon request.

specifications we used state fixed effects to control for state-specific factors that are constant over the period of the study, such as time-invariant state-specific workers' compensation system features. An alternative approach is to control for state fixed effects interacted with year fixed effects, which will also capture state-specific system features (or other factors) that change over time. In this specification, the impact of opioids is estimated from the variation in opioid prescriptions within each year of data for each state. Because adding state-by-year interactions potentially eliminates a good deal of identifying information—in particular, changes in opioid prescribing over time that are common to HRRs in a state— we might obtain much less precise 2SLS estimates.

Table 6 provides coefficient estimates from our regressions for the original specifications, and the alternative specifications that control for state-year specific fixed effects. The results are considerably less precise, as expected—and we find somewhat smaller effects of longer-term opioid prescriptions and MEA exceeding 8,000 milligrams (estimates that are no longer statistically significant in these cases). However, the sign pattern of the estimates is the same, and, for longer-term opioids, the point estimates of 89.9 for longer-term prescriptions and 82.9 for 8,000 milligrams still imply very large effects (approximately 129–146 percent longer duration), and would still be statistically significant based on the standard errors of the estimates without the state-by-year interactions (35.0 and 32.1, respectively).

Adding state-by-year fixed effects is most important if there are important determinants of the duration of temporary disability that vary by state and year, that are correlated with opioids prescribing, and that are not captured in our controls. We already control for time-varying local labor market conditions. And there is not much variation in workers' compensation policies in our sample period. Moreover, there may be some policy variation that is useful as it could potentially drive variation in opioid prescribing—such as policies on adoption and enforcement of prescription drug monitoring programs, rules about dispensing of Schedule II or III prescriptions, and other limits on opioid prescriptions. We would not want to control for policy variation that generates exogenous variation in prescribing patterns across states and years. That is why our preferred specifications are those with separate state and year fixed effects.

We also conducted additional analysis to address the validity of the instrument. There is no way to directly test whether the IV is correlated with unobservables in equation (3.1) that affect the duration of disability, as it is an identifying assumption. (That is, the condition must hold for the residual in equation (3.1) once we obtain a consistent estimate of the effect of opioid prescribing, which we can only do by using the IV.) However, one indirect test is to ask whether the 2SLS estimates are sensitive to excluding controls. If the estimates are sensitive, it is because the IV is correlated with the controls, in which case it might be plausible that it is also non-negligibly correlated with the residual. For example, we likely do not measure injury severity perfectly, so sensitivity of the 2SLS estimates to excluding our severity controls might suggest the IV is correlated with unmeasured severity. Table 7 presents OLS and 2SLS estimates of our original specification as well as specifications without controls for injury severity. We also report estimates that instead exclude the controls for location characteristics for the same reason. In both cases the results are very similar, which bolsters the identification strategy.

Another concern is that "doctor shopping" generates a correlation between local area prescribing rates and variation across HRRs in worker preferences for opioids. If these preferences are also correlated with duration of temporary disability, this could invalidate our IV. To assess this, we define the IV differently, measuring prescribing rates at the level of prescribers rather workers. Whereas a worker-level measure could reflect the sorting of workers with strong preferences for opioids to high-prescribing providers, a provider-level measure could avoid the influence of this sorting. For example, suppose that HRR A and HRR B each have two providers, one of whom prescribes longer-term opioids in 100 percent of cases and the other in 0 percent of cases. Suppose that in HRR A, workers do not have particularly strong preferences for opioids, and cases are split between the two providers, generating a worker-level longer-term opioid percentage of 50. But suppose in HRR B workers all sort to the high provider. In that case, the worker-level measure would be 50 percent in HRR A and 100 percent in HRR B, but the difference reflects only worker sorting. The provider-level measure would be 50 percent in *both* HRRs, which accurately reflects that provider prescribing is the same in both HRRs.

We create measures of prescriptions for workers treated by providers corresponding to the same measures used earlier, such as the percentage of physicians who prescribed any opioids for low back conditions, the percentage of physicians who prescribed at least two opioid prescriptions, etc. To create these measures, we first determine whether a physician/practice prescribed opioids (in the corresponding manner) for each of the workers for whom they wrote prescriptions, and then average these measures across physicians in an area.[43] As reported in Table 8, using this alternative construction of the IV, the estimated effects of opioid prescribing on the duration of temporary disability are similar to the prior results. The 2SLS estimates still point strongly to much longer durations of temporary disability from longer-term opioid prescriptions.

An alternative IV that addresses the same issue is derived only from the first prescribers that patients visited. A typical case in a sample of low back injuries had five different prescribing physicians and/or practices. Limiting the measurement of local prescribing patterns to only the first prescriber is another way of addressing the concern that aggregate prescribing patterns reflect the impact of patients who want more prescriptions finding providers who are more likely to prescribe opioids due to the nature of the medical services that they provide. As reported in Table

[43] For this extension we use practice identifiers to determine prescriber-level rates. Since practice identifiers are not always available, we limit the sample to claims that have at least 90 percent of prescriptions with practice identifiers. This reduces the sample that we can use to about 27,000 claims. While for many prescriptions this identifies an individual prescriber, for many other prescriptions this identifies a practice or even a large hospital.

9, the estimates defining the IV this way indicate a similar relationship between opioid prescribing measures and duration of temporary disability to what we found and reported in the earlier tables. These findings, like others reported in subsection, help bolster the validity of our IV estimation strategy.

The Impact of Opioid Prescriptions Beyond Low Back Conditions

We have focused on low back conditions because we can include detailed controls for injury severity and, as noted earlier, the issue of opioid prescribing in such cases is critically important and has been the focus of past research. But what happens if we instead look at all claims (with more than seven days of lost time)? Extending the analysis to all cases requires a different approach to controlling for injury severity, whereas the controls in our analysis of low back injuries were specific to these kinds of injuries. In analyzing all cases with more than seven days of lost time, we must control more coarsely, simply adding controls for injury type. We also define the prescribing-patterns IV to reflect opioid prescriptions within each HRR, year, and injury group.

Table 10 reports the results from this broader analysis. We find patterns of results that are similar to what we found for low back injuries. The estimated effect of any opioids prescription is small, and masks differential effects across cases with different numbers of prescriptions.[44] We estimate that the effect of workers having three or more prescriptions (excluding longer-term prescriptions) is to lengthen the duration of temporary disability benefits by 32 percent; the corresponding estimate for low back injuries is not statistically significant. Most important, however, is that the estimated effect for longer-term (and high MEA) prescriptions is similar to the earlier estimates for low back cases. The 2SLS estimate indicates that longer-term opioid

[44] The negative and significant 2SLS estimate differs; for low back injuries this estimate was not significantly different from zero. But the effect is small, and there is no reason shorter-term opioid use could not help with recovery and return to work.

prescriptions lengthen duration by over 150 percent.

6. Conclusions

We provide evidence on the effect of opioid prescriptions on the duration of temporary disability benefits among workers with primarily low back injuries who had more than seven days of lost time after their injuries. We use a research design intended to estimate the causal effect of opioid prescriptions, whereas past studies of opioids and return to work have estimated associations that can reflect a combination of causal effects and unobserved injury severity or other sources of variation in return to work that influence both opioid prescriptions and the duration of disability. Our research strategy uses local opioid prescribing patterns to isolate variation in opioids that is unrelated to characteristics of individual workers, their injuries, and their providers that can affect both opioid prescriptions and return to work. These local prescribing patterns exert a strong influence on whether injured workers receive opioid prescriptions, an interesting finding in itself.

We find that prolonged prescribing of opioids leads to longer duration of temporary disability benefits among workers with work-related low back injuries. Our estimates indicate that longer-term opioid prescriptions roughly triple the duration of temporary disability benefits, compared to similar workers with similar injuries who do not get opioid prescriptions. Thus, we do not find evidence, on average, of beneficial effects of opioids prescribed in workers' compensation cases—benefits that would need to be weighed against the costs of opioid use.

These results warrant a more detailed policy focus on longer-term opioid prescriptions. While longer-term prescribing of opioids is not typically recommended for low back pain cases (ACOEM, 2008; Bigos et al., 1994; Chou et al., 2007), it is striking to see that about 12 percent of our sample had longer-term opioids and about 39 percent of workers had at least three opioid prescriptions. Since longer-term opioids lead to longer duration of temporary disability benefits, it is important to understand the reasons why workers are receiving opioids on a longer-term basis, so that policy interventions can be targeted toward reducing inappropriate longer-term use.

Note that nothing in our research directly addresses the medical appropriateness of longer-term opioid prescriptions, and indeed in some cases (as some of our evidence suggests), longer-term opioids may enable return to work. But the evidence that workers in areas with higher rates of prescribing longer-term opioids, for similar injuries, have longer disability duration suggests that there is at least some overuse of longer-term opioids that is leading to longer disability duration. We need to understand what generates variation in opioid prescription patterns across geographic areas and what policies could reduce prescription rates in high-prescription areas.

Research has started to provide information on changes in opioid prescribing after policy changes intending to regulate use. These policy changes (and the associated studies) include: a Florida regulation that banned physician dispensing of opioids (Thumula, 2013); Texas regulations implementing a pharmacy closed formulary (TDI, 2013); Kentucky House Bill 1 regulating pain clinics and establishing standards for dispensing and prescribing opioids, including requiring that providers check the state's prescription drug monitoring program before prescribing opioids (Thumula, 2017); and Washington State implementing opioid dosing guidelines for chronic noncancer pain (Franklin et al., 2012; Garg et al., 2013). However, more research is needed in this area, including verifying whether these policy changes reduce longer-term opioid prescribing and, in turn, speed up return to work.

While our analysis captures major dimensions of variation in opioid prescribing, we leave it for future research to examine the effects of specific types of opioids or opioid combinations, as well as interactions between opioid prescriptions and use and other care provided. We see no reason such analyses would undermine our broad conclusions, but they could provide more specific guidance regarding how opioids might be used to improve return to work. However, it might be harder to use our identification strategy for estimating the causal effects of opioids on return to work, if more detailed local prescribing and practice patterns are less predictive of individual treatment.

Our results also offer important takeaways for future research linking opioids and outcomes. We show that simple regressions, even those that account for injury severity, do not reveal the causal effect of opioid prescriptions on outcomes. Even after controlling for observed injury severity, opioid prescribing measures may still be a marker for unobserved dimensions of injury severity or for unobserved worker characteristics related to return-to-work outcomes, in which case empirical associations between opioids and return to work—or other outcomes—may not reflect the actual effects of opioid prescribing. We have proposed an empirical strategy to estimate these causal effects, which we think is compelling. Additional evidence on whether related strategies corroborate our findings would be invaluable.

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Table 1. Differences in Duration of Temporary Disability Benefits, and Other Descriptive Statistics, for the Overall Sample and by Opioid Prescribing Measures

Variables		Any Opioid Prescriptions		Number of Opioid Prescriptions (among those without longer-term opioid prescriptions)				Low Back Injuries	
	Sample of Low Back Injuries	No	Yes	1	2	3 or More	Longer-Term Opioid Prescriptions	With Surgeries	Without Surgeries
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Duration of temporary disability benefits									
Weeks of temporary disability benefit payments (average)	22.3	11.3	26.7	12.4	17.3	30.7	51.6	51.1	18.6
Weeks of temporary disability benefit payments (median)	9.6	4.4	13.7	4.9	8.1	19.4	49.7	47.7	7.8
Opioid variables									
Percentage with any opioid prescriptions within 24 months after an injury	71%	0%	100%	100%	100%	100%	100%	95%	68%
Percentage with different numbers of opioid prescriptions (excluding longer-term opioid prescriptions)									
None	29%	100%	0%	0%	0%	0%	0%	5%	32%
One	21%	0%	29%	100%	0%	0%	0%	8%	23%
Two	11%	0%	15%	0%	100%	0%	0%	7%	11%
Three or more	28%	0%	39%	0%	0%	100%	0%	47%	25%
Percentage with longer-term opioid prescriptions	12%	0%	16%	0%	0%	0%	100%	33%	9%
Average amount of opioids within 24 months (MEA) among those with opioids (mg)	4,760	0	4,760	266	602	4,208	17,395	9,989	3,818
Median amount of opioids within 24 months (MEA) among those with opioids (mg)	900	0	900	150	400	1,800	9,718	3,813	645
Opioid amounts consistent with 3+ prescriptions									
Percentage with amount of opioids (MEA) greater than 2,600 mg (median)	21%	0%	29%	0%	1%	36%	90%	57%	16%
Percentage with amount of opioids (MEA) greater than 8,000 mg (mean)	10%	0%	14%	0%	0%	11%	58%	30%	7%
Observations	32,405	9,323	23,082	6,784	3,569	8,933	3,796	3,631	28,774

Notes: The sample includes low back injuries with more than seven days of lost time with prescriptions. The data cover workers with primarily low back injuries between October 1, 2008, and September 30, 2013, across 28 states evaluated at 24 months of maturity. Longer-term opioid prescriptions are defined as having prescriptions within the first three months after an injury and three or more filled opioid prescriptions between the 7th and 12th months after an injury. The one, two, or three or more prescriptions measures, and the MEA measures, are also based on the period within 24 months after an injury.
Key: MEA: morphine equivalent amount. mg: milligrams.

Table 2. Coefficient Estimates from OLS Regression for Duration of Temporary Disability (logged) on "Any Opioid Prescriptions" Variable

	Specification 1		Specification 2		Specification 3		Specification 4	
Control Variables	**Coef.**	**S.E.**	**Coef.**	**S.E.**	**Coef.**	**S.E.**	**Coef.**	**S.E.**
Any opioid prescriptions within 24 months after an injury	84.8***	(1.45)	62.7***	(1.44)	36.0***	(1.30)	35.7***	(1.30)
Implied percentage effect	133.5***		87.2***		43.3***		42.9***	
Injury severity/treatment characteristics								
Low back surgery			123.4***	(1.70)	166.7***	(7.77)	167.5***	(7.76)
Early MRI			39.5***	(1.42)	18.3***	(1.29)	18.3***	(1.29)
Severity measures								
Surgery severity score					12.1***	(1.29)	12.0***	(1.29)
Non-operative severity								
Medications only (base)								
Medications and PT only					78.3***	(1.55)	78.5***	(1.55)
Medications, PT, and injections					183.1***	(1.83)	183.4***	(1.83)
Observations	32,405		32,405		32,405		32,405	
R-squared	0.14		0.25		0.43		0.43	
Other controls								
Worker characteristics	X		X		X		X	
Workplace characteristics	X		X		X		X	
Location characteristics							X	
State and year fixed effects	X		X		X		X	

Notes: Estimates are from a sample of workers with low back injuries between October 1, 2008, and September 30, 2013, across 28 states. Claims reflect duration of temporary disability payments within 24 months after an injury. All specifications include controls for state and year dummies, as well as controls for worker and workplace characteristics (age, gender, marital status, preinjury wage and tenure, and firms' payroll and industry). Specification 2 adds controls for low back surgery and early MRI; specification 3 adds severity controls; and specification 4 adds controls for location characteristics (county unemployment rate, whether a zip code reflects a rural area, percentage of population with less than a high school education at the PCSA level, and percentage of county population who were disabled). The full set of estimates are available upon request. Estimated coefficients and standard errors are multiplied by 100, and hence they should be interpreted as approximate percentage changes.

*** Statistically significant at 1% level, ** statistically significant at 5% level, * statistically significant at 10% level.

Key: Coef.: coefficient. MRI: magnetic resonance imaging. OLS: ordinary least squares. PCSA: primary care service area. PT: physical therapy. S.E.: standard error.

Table 3. First-Stage OLS Estimates

	Observations	Coefficient	S.E.	F-Statistics
Any opioid prescriptions versus no prescriptions				
Percentage with any opioid prescriptions at HRR level	32,405	34.2***	(2.82)	146.8
R-squared		0.13		
Two or more opioid prescriptions versus no prescriptions				
Percentage with 2 or more opioid prescriptions at HRR level	24,284	35.6***	(2.80)	161.2
R-squared		0.23		
Three or more opioid prescriptions versus no prescriptions				
Percentage with 3 or more opioid prescriptions at HRR level	20,186	30.9***	(2.84)	118.7
R-squared		0.31		
Two or more opioid prescriptions (excluding longer-term opioid prescriptions) versus no prescriptions				
Percentage with 2 or more opioid prescriptions at HRR level	19,814	32.3***	(3.21)	101.0
R-squared		0.20		
Three or more opioid prescriptions (excluding longer-term opioid prescriptions) versus no prescriptions				
Percentage with 3 or more opioid prescriptions at HRR level	15,812	25.6***	(3.44)	55.3
R-squared		0.26		
Longer-term opioid prescriptions relative to cases with no opioid prescriptions				
Percentage with longer-term opioid prescriptions at HRR level	9,988	26.3***	(3.50)	56.4
R-squared		0.45		
Opioid amount (MEA) over 2,600 mg				
Percentage with opioid amount (MEA) over 2,600 mg at HRR level	13,040	25.5***	(3.04)	70.6
R-squared		0.45		
Opioid amount (MEA) over 8,000 mg				
Percentage with opioid amount (MEA) over 8,000 mg at HRR level	9,203	30.1***	(3.59)	70.4
R-squared		0.45		
Opioid amount (MEA) over 2,600 mg (excluding cases with longer-term prescriptions) relative to cases with no opioid prescriptions				
Percentage with opioid amount (MEA) over 2,600 mg at HRR level	9,448	19.1***	(4.10)	21.6
R-squared		0.36		
Opioid amount (MEA) over 8,000 mg (excluding cases with longer-term prescriptions) relative to cases with no opioid prescriptions				
Percentage with opioid amount (MEA) over 8,000 mg at HRR level	7,145	22.9***	(5.78)	15.7
R-squared		0.29		
Opioid amount (MEA) over 2,600 mg and longer-term prescriptions relative to cases with no opioid prescriptions				
Percentage with opioid amount (MEA) over 2,600 mg at HRR level	9,492	26.6***	(3.67)	52.7
R-squared		0.45		
Opioid amount (MEA) over 8,000 mg and longer-term prescriptions relative to cases with no opioid prescriptions				
Percentage with opioid amount (MEA) over 8,000 mg at HRR level	8,270	31.3***	(4.11)	58.0
R-squared		0.42		

Notes: Estimates are from OLS regressions predicting opioid prescribing measures. Controls are described in notes to Table 2, corresponding to Specification 4. The full set of estimates are available upon request. The local prescribing pattern is measured as a proportion from zero to one, and the dependent variable is either zero or one. Regressions are based on the sample with at least 15 observations within each HRR and year combination for constructing an instrument. See Table 1 and notes for definitions of opioid prescription measures, and Table 2 notes for sample and variable definitions.

*** Statistically significant at 1% level, ** statistically significant at 5% level, * statistically significant at 10% level.

Key: HRR: hospital referral region. MEA: morphine equivalent amount. mg: milligrams. OLS: ordinary least squares. S.E.: standard error.

Table 4. Estimates from OLS and 2SLS Regressions of Duration of Temporary Disability (logged) on Alternative Measures of Opioid Prescriptions

	Sample Size	OLS Coef.	OLS S.E.	OLS Implied % Effect	2SLS Coef.	2SLS S.E.	2SLS Implied % Effect
A. Estimates for any prescriptions							
Any opioid prescriptions within 24 months after an injury	32,405	35.7***	(1.3)	42.9***	4.5	(19.3)	4.6
B. Estimates for multiple prescriptions							
Two or more opioid prescriptions relative to no prescriptions	24,284	58.3***	(1.6)	79.2***	18.4	(18.4)	20.2
Three or more opioid prescriptions relative to no prescriptions	20,186	74.1***	(1.8)	109.8***	41.9*	(21.7)	52.1
C. Estimates for multiple prescriptions (excluding those with longer-term prescriptions)							
Two or more opioid prescriptions (excluding longer-term prescriptions) relative to no prescriptions	19,814	48.2***	(1.6)	61.9***	-6.5	(22.3)	-6.3
Three or more opioid prescriptions (excluding longer-term prescriptions) relative to no prescriptions	15,812	61.5***	(1.9)	84.9***	15.8	(30.7)	17.1
D. Estimates for longer-term prescriptions							
Longer-term opioid prescriptions relative to cases with no opioid prescriptions	9,988	106.9***	(3.2)	191.3***	125.6***	(35.0)	251.0**
E. Estimates for opioid amount thresholds							
Opioid amount (MEA) over 2,600 mg relative to cases with no opioid prescriptions	13,040	106.1***	(2.6)	189.0***	135.6***	(30.3)	288.1**
Opioid amount (MEA) over 8,000 mg relative to cases with no opioid prescriptions	9,203	111.4***	(3.5)	204.6***	117.3***	(32.1)	223.0**
F. Estimates for opioid amount thresholds (excluding cases with longer-term opioid prescriptions)							
Opioid amount (MEA) over 2,600 mg (excluding cases with longer-term prescriptions) relative to cases with no opioid prescriptions	9,448	92.0***	(3.3)	151.0***	76.7	(55.5)	115.4
Opioid amount (MEA) over 8,000 mg (excluding cases with longer-term prescriptions) relative to cases with no opioid prescriptions	7,145	98.3***	(5.3)	167.2***	25.2	(78.7)	28.7
G. Estimates for opioid amount thresholds and longer-term opioid prescriptions							
Opioid amount (MEA) over 2,600 mg and longer-term prescriptions relative to cases with no opioid prescriptions	9,492	107.2***	(3.4)	192.1***	133.8***	(37.4)	281.0**
Opioid amount (MEA) over 8,000 mg and longer-term prescriptions relative to cases with no opioid prescriptions	8,270	108.4***	(4.0)	195.7***	122.4***	(37.1)	240.1*

Notes: Estimates are from OLS and 2SLS regressions for the indicated measure of opioid prescriptions relative to the base category of "no opioid prescriptions" within 24 months after an injury. Controls are described in notes to Table 2. Regressions are based on the sample with at least 15 observations within each HRR and year combination for constructing an instrument. Estimated coefficients and standard errors are multiplied by 100, and hence they should be interpreted as approximate percentage changes. See Table 1 and notes to Tables 1-3 for additional details on variables and sample.

*** Statistically significant at 1% level, ** statistically significant at 5% level, * statistically significant at 10% level.

Key: 2SLS: two-stage least squares. Coef.: coefficient. HRR: hospital referral region. OLS: ordinary least squares. S.E.: standard error.

Table 5. Estimates from OLS and 2SLS Regressions of Duration of Temporary Disability (logged) on Opioid Prescribing Measures for All and for Nonsurgical Cases

		OLS			2SLS			First Stage	
Specification	**Sample Size**	**Coef.**	**S.E.**	**Implied % Effect**	**Coef.**	**S.E.**	**Implied % Effect**	**Coef.**	**S.E.**
Any opioid prescriptions within 24 months after an injury									
All cases	32,405	35.7***	(1.3)	42.9***	4.5	(19.3)	4.6	34.2***	(2.8)
Nonsurgical cases	28,108	35.8***	(1.3)	43.0***	12.9	(21.1)	13.8	32.9***	(3.0)
Two or more opioid prescriptions (excluding longer-term prescriptions)									
All cases	19,814	48.2***	(1.6)	61.9***	-6.5	(22.3)	-6.3	32.3***	(3.2)
Nonsurgical cases	17,263	49.0***	(1.7)	63.2***	-3.5	(23.6)	-3.4	32.7***	(3.5)
Three or more opioid prescriptions (excluding longer-term prescriptions)									
All cases	15,812	61.5***	(1.9)	84.9***	15.8	(30.7)	17.1	25.6***	(3.4)
Nonsurgical cases	13,515	63.8***	(2.1)	89.3***	-11.8	(38.0)	-11.1	24.2***	(3.9)
Longer-term opioid prescriptions									
All cases	9,988	106.9***	(3.2)	191.3***	125.6***	(35.0)	251.0**	26.3***	(3.5)
Nonsurgical cases	8,568	113.5***	(3.5)	211.0***	126.6***	(41.9)	254.8*	26.5***	(4.3)
Opioid amount (MEA) over 2,600 mg									
All cases	13,040	106.1***	(2.6)	189.0***	135.6***	(30.3)	288.1**	25.5***	(3.0)
Nonsurgical cases	10,592	111.9***	(2.8)	206.1***	139.6***	(37.3)	304.1**	25.1***	(3.8)
Opioid amount (MEA) over 8,000 mg									
All cases	9,203	111.4***	(3.5)	204.6***	117.3***	(32.1)	223.0**	30.1***	(3.6)
Nonsurgical cases	7,945	117.7***	(3.9)	224.6***	94.7***	(35.2)	157.8*	34.3***	(4.5)

Notes: Estimates are from OLS and 2SLS regressions for opioid measures reflecting experience within 24 months after an injury. Longer-term opioid prescriptions are defined as having prescriptions within the first three months after an injury and three or more filled opioid prescriptions between the 7th and 12th months after an injury. Specifications include controls for worker, workplace, injury, and location characteristics, as described in the notes to Table 2, including state and year dummies. Estimates are from a sample of workers with low back injuries between October 1, 2008, and September 30, 2013, across 28 states. Claims reflect duration of temporary disability payments within 24 months after an injury. Regressions are based on the sample with at least 15 observations within each HRR and year for constructing an instrument. Estimated coefficients and standard errors are multiplied by 100, and hence they should be interpreted as approximate percentage changes.

*** Statistically significant at 1% level, ** statistically significant at 5% level, * statistically significant at 10% level.

Key: 2SLS: two-stage least squares. Coef.: coefficient. HRR: hospital referral region. MEA: morphine equivalent amount. mg: milligrams. OLS: ordinary least squares. S.E.: standard error.

Table 6. Testing Sensitivity of 2SLS Estimates to Controlling for State-Year Fixed Effects

Specification	Sample Size	2SLS Coef.	2SLS S.E.	2SLS Implied % Effect	First Stage Coef.	First Stage S.E.
Any opioid prescriptions						
Original specification	32,405	4.5	(19.3)	4.6	34.2***	(2.8)
With state-year specific fixed effects	32,405	-4.7	(25.1)	-4.6	28.2***	(2.8)
Two or more opioid prescriptions (excluding longer-term prescriptions) relative to no prescriptions						
Original specification	19,814	-6.5	(22.3)	-6.3	32.3***	(3.2)
With state-year specific fixed effects	19,814	-31.6	(31.1)	-27.1	25.5***	(3.3)
Three or more opioid prescriptions (excluding longer-term prescriptions) relative to no prescriptions						
Original specification	15,812	15.8	(30.7)	17.1	25.6***	(3.4)
With state-year specific fixed effects	15,812	-16.8	(48.0)	-15.5	18.9***	(3.5)
Longer-term opioid prescriptions relative to cases with no opioid prescriptions						
Original specification	9,988	125.6***	(35.0)	251.0**	26.3***	(3.5)
With state-year specific fixed effects	9,988	89.9	(55.6)	145.7	18.8***	(3.7)
Opioid amount (MEA) over 2,600 mg relative to cases with no opioid prescriptions						
Original specification	13,040	135.6***	(30.3)	288.1**	25.5***	(3.0)
With state-year specific fixed effects	13,040	128.5***	(45.9)	261.5	18.8***	(3.2)
Opioid amount (MEA) over 8,000 mg relative to cases with no opioid prescriptions						
Original specification	9,203	117.3***	(32.1)	223.0**	30.1***	(3.6)
With state-year specific fixed effects	9,203	82.9	(57.2)	129.1	20.0***	(3.9)

Notes: See notes to Table 5.
*** Statistically significant at 1% level, ** statistically significant at 5% level, * statistically significant at 10% level.
Key: 2SLS: two-stage least squares. Coef.: coefficient. S.E.: standard error. HRR: hospital referral region. MEA: morphine equivalent amount. mg: milligrams.

Table 7: Testing Sensitivity of 2SLS Estimates to Different Controls

		2SLS		
Specification	**Observations**	**Coef.**	**S.E.**	**Implied % Effect**
Any opioid prescriptions				
Original specification	32,405	4.5	(19.3)	4.6
Specification without controls for injury severity	32,405	6.3	(22.2)	6.5
Specification without controls for location characteristics	32,405	23.4	(14.5)	26.3
Two or more opioid prescriptions (excluding longer-term prescriptions) relative to no prescriptions				
Original specification	19,814	-6.5	(22.3)	-6.3
Specification without controls for injury severity	19,814	-7.2	(25.4)	-6.9
Specification without controls for location characteristics	19,814	10.1	(16.4)	10.6
Three or more opioid prescriptions (excluding longer-term prescriptions) relative to no prescriptions				
Original specification	15,812	15.8	(30.7)	17.1
Specification without controls for injury severity	15,812	19.1	(33.5)	21.0
Specification without controls for location characteristics	15,812	28.8	(21.7)	33.4
Longer-term opioid prescriptions relative to cases with no opioid prescriptions				
Original specification	9,988	125.6***	(35.0)	251.0**
Specification without controls for injury severity	9,988	141.1***	(35.9)	309.9**
Specification without controls for location characteristics	9,988	121.2***	(26.5)	236.0***
Opioid amount (MEA) over 2,600 mg relative to cases with no opioid prescriptions				
Original specification	13,040	135.6***	(30.3)	288.1**
Specification without controls for injury severity	13,040	139.9***	(31.0)	305.3**
Specification without controls for location characteristics	13,040	130.4***	(20.4)	268.6***
Opioid amount (MEA) over 8,000 mg relative to cases with no opioid prescriptions				
Original specification	9,203	117.3***	(32.1)	223.0**
Specification without controls for injury severity	9,203	102.3***	(38.0)	178.1*
Specification without controls for location characteristics	9,203	113.7***	(23.0)	211.8***

Notes: See notes to Table 5.

*** Statistically significant at 1% level, ** statistically significant at 5% level, * statistically significant at 10% level.

Key: 2SLS: two-stage least squares. Coef.: coefficient. HRR: hospital referral region. MEA: morphine equivalent amount. mg: milligrams. S.E.: standard error.

Table 8. Estimates from Specifications for Alternative Instrument Reflecting Billing Practice Aggregation

		OLS			2SLS			First Stage	
Specification	**Sample Size**	**Coef.**	**S.E.**	**Implied % Effect**	**Coef.**	**S.E.**	**Implied % Effect**	**Coef.**	**S.E.**
Any opioid prescriptions within 24 months after an injury									
Original instrument, new subsample	27,420	34.4***	(1.37)	41.0***	12.0	(19.55)	12.7	36.8***	(3.0)
Practice level aggregation for instrument	27,419	34.4***	(1.37)	41.0***	3.3	(28.35)	3.4	24.8***	(tab)
Two or more opioid prescriptions (excluding longer-term prescriptions) relative to no prescriptions									
Original instrument, new subsample	16,724	48.1***	(1.76)	61.8***	1.0	(22.91)	1.0	34.2***	(3.4)
Practice level aggregation for instrument	16,724	48.1***	(1.76)	61.8***	16.4	(33.87)	17.8	29.6***	(4.6)
Three or more opioid prescriptions (excluding longer-term prescriptions) relative to no prescriptions									
Original instrument, new subsample	13,338	62.4***	(2.12)	86.7***	13.4	(32.37)	14.3	27.2***	(3.6)
Practice level aggregation for instrument	13,338	62.4***	(2.12)	86.7***	-16.9	(44.97)	-15.5	31.0***	(5.9)
Longer-term opioid prescriptions relative to cases with no opioid prescriptions with instrument based on 3 or more prescriptions									
Original instrument, new subsample	11,412	109.2***	(3.13)	198.1***	129.8***	(36.15)	266.0**	29.0***	(3.7)
Practice level aggregation for instrument	11,411	109.2***	(3.13)	198.1***	149.5***	(49.54)	345.9	24.8***	(4.4)
Opioid amount (MEA) over 2,600 mg									
Original instrument, new subsample	10,979	111.4***	(2.93)	204.6***	127.3***	(32.98)	257.3**	25.7***	(3.1)
Practice level aggregation for instrument	10,979	111.4***	(2.93)	204.6***	176.1***	(50.90)	482.1	36.4***	(7.1)
Opioid amount (MEA) over 8,000 mg									
Original instrument, new subsample	8,027	116.9***	(3.94)	221.9***	111.1***	(36.89)	203.7*	29.6***	(3.6)
Practice level aggregation for instrument	8,027	116.9***	(3.94)	221.9***	129.4*	(68.79)	264.7	49.1***	(11.7)

Notes: See notes to Table 5. Sample includes claims with at least 90 percent of medical bills with billing practice identifiers.

*** Statistically significant at 1% level, ** statistically significant at 5% level, * statistically significant at 10% level.

Key: 2SLS: two-stage least squares. Coef.: coefficient. HRR: hospital referral region. MEA: morphine equivalent amount. mg: milligrams. OLS: ordinary least squares. S.E.: standard error.

Table 9. Estimates from Specifications for Instrument Defined Based on First Prescriber

		OLS			2SLS			First Stage	
Specification	**Sample Size**	**Coef.**	**S.E.**	**Implied % Effect**	**Coef.**	**S.E.**	**Implied % Effect**	**Coef.**	**S.E.**
Any opioid prescriptions within 24 months after an injury	27,275	34.2***	(1.37)	40.8***	9.6	(27.0)	10.1	20.8***	(2.5)
Two or more opioid prescriptions (excluding longer-term prescriptions) relative to no prescriptions	16,653	48.1***	(1.76)	61.8***	22.8	(38.6)	25.6	21.8***	(3.9)
Longer-term opioid prescriptions relative to cases with no opioid prescriptions with instrument based on 3 or more prescriptions	11,344	109.2***	(3.13)	198.1***	171.4***	(65.2)	455.1	15.9***	(3.7)
Opioid amount (MEA) over 2,600 mg with instrument based on 3 or more prescriptions	13,330	109.2***	(2.62)	197.9***	163.9***	(60.5)	415.1	16.0***	(3.7)
Opioid amount (MEA) over 8,000 mg with instrument based on 3 or more prescriptions	10,759	116.3***	(3.38)	219.9***	263.9***	(69.2)	1299.4	16.7***	(3.6)

Notes: See notes to Table 5. Regressions are based on the sample with at least 30 observations within each HRR for constructing an instrument.
*** Statistically significant at 1% level, ** statistically significant at 5% level, * statistically significant at 10% level.
Key: 2SLS: two-stage least squares. Coef.: coefficient. HRR: hospital referral region. MEA: morphine equivalent amount. mg: milligrams.
OLS: ordinary least squares. S.E.: standard error.

Table 10. Estimates from OLS and 2SLS Regressions for Opioid Prescribing Measures for Sample of All Cases with More Than 7 Days of Lost Time

		OLS			2SLS			First Stage	
Opioid Prescribing Measures	**Sample Size**	**Coef.**	**S.E.**	**Implied % Effect**	**Coef.**	**S.E.**	**Implied % Effect**	**Coef.**	**S.E.**
Any opioid prescriptions	836,875	40.8***	(0.3)	50.3***	-13.4***	(3.3)	-12.5***	53.7***	(0.7)
Two or more opioid prescriptions (excluding longer-term prescriptions) relative to no opioid prescriptions	554,171	56.6***	(0.3)	76.2***	3.5	(3.5)	3.5	49.1***	(0.7)
Three or more opioid prescriptions (excluding longer-term prescriptions) relative to no opioid prescriptions	438,223	73.3***	(0.4)	108.1***	27.7***	(4.0)	31.9***	44.8***	(0.7)
Longer-term opioid prescriptions relative to no opioid prescriptions	262,945	124.3***	(0.6)	246.5***	94.9***	(5.8)	158.4***	42.9***	(0.9)
Opioid amount (MEA) over 2,600 mg relative to no opioid prescriptions	319,313	120.6***	(0.5)	234.1***	99.9***	(4.7)	171.5***	42.3***	(0.8)
Opioid amount (MEA) over 8,000 mg relative to no opioid prescriptions	237,814	136.2***	(0.7)	290.4***	121.5***	(5.6)	237.0***	49.0***	(1.0)

Notes: See notes to Table 5. Estimates are from a sample of all claims for injuries between October 1, 2008, and September 30, 2013, across 28 states. Claims reflect duration of temporary disability payments within 24 months after an injury.
*** Statistically significant at 1% level, ** statistically significant at 5% level, * statistically significant at 10% level.
Key: 2SLS: two-stage least squares. Coef.: coefficient. HRR: hospital referral region. MEA: morphine equivalent amount. mg: milligrams.
OLS: ordinary least squares. S.E.: standard error.