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MRDRC WP 2021-419

UM20-14

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March 2021

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Acknowledgements

The research reported herein was performed pursuant to a grant from the U.S. Social Security Administration (SSA) funded as part of the Retirement and Disability Research Consortium through the University of Michigan Retirement and Disability Research Center Award RDR18000002-02. The opinions and conclusions expressed are solely those of the author(s) and do not represent the opinions or policy of SSA or any agency of the federal government. Neither the United States government nor any agency thereof, nor any of their employees, makes any warranty, express or implied, or assumes any legal liability or responsibility for the accuracy, completeness, or usefulness of the contents of this report. Reference herein to any specific commercial product, process or service by trade name, trademark, manufacturer, or otherwise does not necessarily constitute or imply endorsement, recommendation or favoring by the United States government or any agency thereof.

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Abstract

This study examines the effects of prescription opioid analgesic use for older Americans, specifically with regard to work disability and disability program participation. We draw on the long-panel structure of the Health and Retirement Study and a newly available 2009 survey module measuring prescription drug use and initiation. We pursue regression-adjustment and nearest neighbor matching approaches, using rich 2008 HRS measures on health, disability, sociodemographic characteristics, and economic status, to account for selection into prescription opioid use, since supply-side instruments used in the opioid literature have little relevance for this population in 2009. Pre-2008 comparisons between individuals with 2009 opioid prescriptions and controls demonstrate face validity of the analytic approach; we then estimate opioid use effects on mortality, self-reported health, labor force participation, work-limiting health conditions, and disability program participation, spanning from 2010 to 2018. We find substantial and significant mortality effects starting in 2010; in estimating effects on other outcomes, we account for differential attrition through mortality via inverse probability reweighting. Our findings are significant, both statistically and economically: up through 2018, individuals with 2009 opioid prescriptions were nearly 40% more likely to develop a health condition that limited their ability to work than those without a prescription. This difference in work disability led to substantial differences in disability program participation: Those using opioids were nearly 300% more likely to apply for or receive Social Security disability benefits by 2018.

Citation

Armour, Philip, Rosanna Smart, and Elliott Brennan. 2021. “The Causes and Consequences of Opioid Use Among Older Americans: A Panel Survey Approach.” Ann Arbor, MI. University of Michigan Retirement and Disability Research Center (MRDRC) Working Paper; MRDRC WP 2021-419. https://mrdrc.isr.umich.edu/publications/papers/pdf/wp419.pdf

Introduction

Over the past 25 years, a dramatic increase in opioid prescriptions in the United States has fueled a deadly trend of misuse and overdose from this drug class. From 1999-2015, the per capita sale of prescription opioids increased by 356%, and the number of deaths from opioid-related overdose increased by 300% (Krueger 2017). According to the U.S. Centers for Disease Control and Prevention (CDC), as of 2019, almost 247,000 Americans have died from prescription opioid overdose since 1999.

As this public health crisis has come to light, researchers have worked to understand the dynamics of the opioid epidemic and identify who has been most affected by the crisis and why. In addition to its disastrous human toll, the opioid epidemic has had repercussions for the social and economic stability of the hardest hit individuals and communities. Although the direct health consequences and policies to address them have been an area of active research, the impact of the opioid epidemic on labor markets has only recently garnered a similar level of attention (Maclean et al. 2020). Recent studies have developed empirical models for the relationship between opioid prescription rates and labor market participation rates, as well as evidence for the causal mechanism for this relationship (Aliprantis et al. 2019; Currie et al. 2019; Harris et al. 2020). Additionally, preliminary research has suggested a link between opioid prescriptions and disability program participation (Cutler et al. 2017), although the causal link between these two is difficult to establish and the mechanisms and trajectories by which opioid use leads to long-term disability have yet to be explored.

This dearth of research is unsurprising since the relationship between opioids and disability is complex: The initial promise of drugs such as OxyContin was the ability to effectively manage chronic pain, thereby potentially allowing for greater work capacity. However, the addictive nature and powerful opioids ‘direct and side effects can interfere with steady employment. This complexity creates a problem for the researcher: Do opioids lead to lower employment, or do those who have trouble working due to chronic health conditions both work less and take more opioids? That is, to what extent is opioid use a cause of work disability or a consequence of it?

To answer this question, this study relies on a data set that tracks older Americans over time, the Health and Retirement Study (HRS). In particular, we employ newly available prescription drug use data from the 2009 Health and Well-Being (HWB) HRS survey module, administered near the height of opioid prescribing (IQVIA Institute 2020). By interviewing and reinterviewing the same respondents, we can observe individuals before they start using opioids and up to a decade after this initiation. We thereby control for a rich array of preinitiation measures. By comparing individuals with and without 2009 opioid prescriptions[1] but who are otherwise observationally similar in 2008, we estimate the differences between the two groups in their mortality, work-limiting health conditions, and disability program participation. We also look at differences in these outcomes by what other prescription drugs they use, namely,

[1] As discussed in the Data section below, we refer to “individuals with 2009 opioid prescriptions” as respondents to the 2009 HWB module who indicate that they both have a prescription for an opioid and have taken that opioid, even if only occasionally. That is, the HWB specifically requests that respondents do not list prescriptions that they do not take.

benzodiazepines and gabapentin, both of which have been associated with significantly higher health risk when used with opioids (Peckham et al. 2018; Sun et al. 2017).

Both our regression and nearest-neighbor matching approaches based on 2008 controls pass face validity checks: There are no statistically significant differences in levels or trends in health or labor market status prior to 2008 across 2009 opioid prescription status. We then turn to estimating differences by 2009 opioid use from 2010 to 2018.

We find that the 50 and older HRS respondents with opioid prescriptions are substantially more likely to have died in the years following the 2009 HWB survey than those without prescriptions. We account for this differential mortality through inverse probability reweighting, and then estimate differences in our disability outcomes.

Our findings are significant, both statistically and in terms of magnitude: By 2014, those with 2009 opioid prescriptions were 15 percentage points more likely to report having a work-limiting health condition, with this difference continuing through 2018, a 40% increase over baseline work-limitation. Although we see that the labor force participation of 2009 opioid prescription holders declines over our eight-year follow-up window, this decline is not statistically significant. Instead, the consequences of the higher levels of work disability manifest directly in terms of disability program participation: In 2016 and 2018, those with 2009 opioid prescriptions were approximately 30 percentage points more likely to have applied for or be receiving Social Security Disability Insurance (SSDI/DI) or Supplemental Security Income (SSI) benefits than their peers without 2009 opioid prescriptions. Given that rates of participation in these programs were only around 11%, those with 2009 opioid prescriptions were nearly four times more likely to participate in SSDI/SSI.

We further examined additional differences by benzodiazepine and gabapentin co-use. Unfortunately, the sample size limited the statistical power to detect differences, and we did not observe any additional differences in mortality effects when examining the subset of individuals with these prescriptions. However, the structure of the sampling process itself may limit our ability to detect higher rates of mortality: Co-users of these drugs must survive long enough to accurately respond to the 2009 HWB survey module. If the mortality effects of co-use manifest quickly, we would not be able to detect them in these data.

This study estimates the long-term disabling consequences of opioid analgesics. Even those taking licit opioid drugs were substantially more likely to develop work-limiting health conditions, apply for disability benefits, and receive these benefits, than otherwise similar individuals who did not have opioid prescriptions. Although the 2009 snapshot of opioid use in the HRS predates the rise of illicit opioids and is over a decade “out-of-date,” the long-panel nature of the HRS presents a unique opportunity to examine nearly a decade of follow-up outcomes. Indeed, our disability program participation estimates do not begin to rapidly rise until seven years after the 2009 module. As the HRS continues, researchers can use the growing follow-up window to examine the diverse consequences of the rapid rise in opioid prescriptions in the U.S.

Background

A full review of the opioid crisis, its health and labor market effects, the design of SSDI programs, and evidence linking them, is beyond the scope of this paper and, fortunately, has been excellently provided by other researchers and in other sources, namely Maclean et al. (2020), Park and Powell (2020), and CBPP (2021). We refer the reader to these pieces, but we cover below what we consider to be the findings from these literatures relevant to this study.

Opioid use and the labor market

In early consideration of the labor market as a component of the opioid crisis, researchers hypothesized that labor market decline could be a contributing factor to the growing use of prescription opioids, i.e., labor market declines could be a “demand-side factors.” This hypothesis already presents a challenge to researchers aiming to establish a causal link between opioid use on labor market outcomes and disability: These two “consequences” of opioid use may also be “causes” of opioid use. But subsequent economic studies indicated that decreasing labor market participation has not played a significant role in the increase in prescription opioid use. Instead, supply-side factors account for the explosion in opioid prescriptions (e.g., Alpert et al. 2019). As such, recent studies have begun to demonstrate the labor market impacts of these opioid prescriptions: Increased prescription opioid use can have a negative, causal effect on labor force participation (Maclean et al. 2020).

For example, a 2020 study based on county-level U.S. Bureau of Labor Statistics (BLS) data and state-level Prescription Drug Monitoring Program (PDMP) data found that a 10% increase in opioid prescription rates led to a 0.53% decline in labor force participation, a significant and substantial adverse effect comparable to the decrease in labor force participation following the liberalization of DI requirements in 1984 (Harris et al. 2020). Recent analyses using individual-level, cross-sectional labor participation data linked to county-level opioid prescription data from the CDC find that higher opioid prescription rates are correlated with higher unemployment rates. There is a substantial gender gradient to this effect, with a stronger link for women than for men (Aliprantis et al. 2019).

While strong empirical evidence indicates that the major causes of the opioid crisis lie on the supply side — in particular, the pharmaceutical companies' marketing efforts and physicians' prescribing practice — analyzing the causes and consequences of prescription opioid demand is valuable for addressing the crisis. In particular, longitudinal data that allows for tracking the same individuals over time can address issues related to the direction of causality with regard to labor market outcomes and opioid prescriptions.

According to a 2017 report from the Brookings Institute, approximately half of the men ages 25 to 54 in the U.S. who are not employed take pain medication on a daily basis, and nearly two-thirds of those medications are prescription (Krueger 2017), yet the evidence on pain medications' impact on medium- to long-term labor market outcomes is still preliminary.

Opioid use and SSDI programs

The 2020 BLS-PDMP study referred to above highlighted that future research should consider the joint relationship of prescription opioid use, labor force participation, and disability status (Harris et al. 2020). There is the potential for a strong relationship between prescription opioid use and disability status as determined by Social Security through SSDI or SSI since the conditions that tend to require prescription opioids (chronic pain, musculoskeletal injuries, or mental health conditions) also tend to be qualifying conditions for Social Security-administered disability programs. Both SSDI and SSI require that alleged disabling health conditions last for at least 12 months or be expected to result in death, and prevent the applicant from earning at the substantial gainful activity level ($1,310 per month for nonblind applicants in 2021). Again, disentangling the causal relationship depends on the ability to observe the timing of health and opioid use. If the onset of a musculoskeletal condition leads to both SSDI entry and opioid use, then opioid use is a consequence of disability. If opioid use leads to the deterioration of earnings capacity or aggravates or creates other health conditions that lead to SSDI entry, then opioid use is a cause of observed disability. These mechanisms are not mutually exclusive at either the population or individual level, and, indeed, initial injury or illness could lead to opioid use, which in turn could lead to addiction or misuse, resulting in further diminishing of work capacity.

Recent research has demonstrated opioids' potential for causing a decline in work capacity by identifying a causal link between prescription opioid use and duration of temporary disability. Namely, opioid prescriptions for low-back injuries varies substantially across geographies by workers' compensation-physician prescribing behavior: Injured workers who were, thus, prescribed more opioids had temporary disability durations three times that of similar workers who did not receive opioid prescriptions (Savych et al. 2018).

At the state level, Park and Powell (2020) show that the 2010 reformulation of OxyContin to be less susceptible to abuse, and the substitution of illicit opioids that resulted, led to immediate increases in Social Security disability program application, among additional negative labor market outcomes.

For this study, multiple measures of disability status both prior to and after opioid use provide a more targeted lens on the question of opioid use's impact on labor market participation. In contrast to prior research, these measures provide longitudinal evidence on both the causes and consequences of opioid use for disability program participation.

Putting numbers to the opioid crisis

As researchers have dedicated themselves to unraveling the dynamics of the opioid crisis, an ongoing challenge has been finding and leveraging appropriate data sources (Smart et al. 2020). As discussed in the next section, this study takes advantage of newly available data on prescription opioid use with links to a long-panel survey that contains a broad range of social, economic, and demographic indicators.

A study in 2017 reviewed the data sources and commonly used strategies for opioid research, focusing on the key topic areas of prescribing practices, misuse prevention, overdose treatment, and public health surveillance. In terms of public health surveillance of opioid use, the most common sources of data tend to be state PDMP databases, electronic health record (EHR) databases, and proprietary databases that provide information on opioid diversion and self-reported nonmedical prescription opioid use (Smart et al. 2020). However, these data sources often capture only one aspect of prescription drug use, health outcomes, disability, or health care use. Other novel sources of data on opioid use — such as the HWB module used by this study linked to the HRS — are needed to provide individual-level, longitudinal data across a full range of outcomes of interest.

State policies regulating opioid supply and demand

To account for a potential exogenous source of variation in prescription opioid use, this study also examines the differences across states in terms of opioid regulation policies. These differences are one of several sources of quasi-random variation of prescription opioid use identified in the literature, along with the geographic concentration of "pill mills" and the rate of patients switching to new physicians (Aliprantis et al., 2019). The study makes use of RAND data sets of state policies dealing with prescription opioids' supply and demand in order to consider other explanations for changes in prescription opioid use across the sample population from the HWB module. These variations include prescription drug monitoring programs (PDMPs), so-called "triplicate states" which pharmaceutical companies did not market to as extensively, and overall prevailing opioid prescription rates at the state and county level.

A 2016 article in the New England Journal of Medicine reviewed the array of state laws enacted between 2006 and 2012 for the purpose of regulating the prescribing and dispensing of opioids. The review identified 81 such laws, which primarily addressed potentially hazardous prescription patterns (Meara 2016). A more recent study in 2018 looked at the outcomes of specific drug control policies targeting new users via limits on initial prescription length and mandatory PDMP, finding that the latter policy was successful in reducing prescription opioid use while the former was not.

Other common policy responses typically aim to reduce access or reduce harm associated with prescription opioids, including abuse-deterrent reformulation and rescheduling of opioids to make them harder to obtain (Sacks et al. 2021).

As discussed below in the analysis section, we attempt to leverage this array of state policies as a source of exogenous variation in opioid use. However, the time period, population under study, and the rich array of pre-use health measures led to these policy variables having limited predictive power in our context.

Data

This study draws on the HRS, a nationally representative panel survey of U.S. individuals older than 50 and their spouses/partners. It is structured as a biennial survey, first fielded in 1992, with a new cohort of 51- to 56-year-olds added every six years to maintain representation of the 50-plus population. The HRS collects a rich array of economic, sociodemographic, and health information from respondents, and most notably for this study, contains measures of respondents' labor-force status, work-limiting health conditions, functional limitations, mobility impairments, disability program participation, and mortality.

Although the core HRS contains questions as to overall prescription drug use and a small set of reasons for use (notably, pain and anxiety or depression), the HRS elicited more detailed use at three points: the 2005 Prescription Drug Study (PDS), a 2007 follow-up PDS, and a 2009 Health and Well-Being (HWB) module, all conducted as mail surveys for a subset of HRS respondents between interview years. These modules asked respondents to provide detailed information about use of, access to, and insurance coverage of prescription and nonprescription drugs. Specifically, the HWB begins their section eliciting prescription drug use (HRS 2012) with:

> *"To really understand the impact of prescription medications on the health and economic security of Americans like you, it is important to know something about the specific medications that people actually take. This section asks you to provide some information about each of the different medications you take. Please list all the medications prescribed, including those you only take occasionally…"*

The 2005 and 2007 PDS modules were targeted to current or soon-to-be Medicare beneficiaries, designed to capture changes in prescription drug use after the implementation of the Medicare Modernization Act of 2003 and its introduction of Medicare Part D's prescription drug coverage in 2006. We do not use these two PDS modules to study our disability outcomes among this Medicare-eligible population, since our outcomes of labor-force status, work-limitation status, and transitions into disability programs are less applicable to the elderly or already disabled Medicare population.

The 2009 HWB, on the other hand, was representative of the broader HRS respondent sample, and was administered just before what is considered the peak of opioid prescribing: 2010 to 2012 (Pacula and Powell 2018; Schuchat et al. 2017). As the HRS-provided documentation (HRS 2012) explains:

> *"The sample for HWB 2009 included everyone from the PDS 2005/2007 sample plus a 22% random sample of respondents who: a) gave an interview in 2008, b) were not included in the sample for the Consumption and Activities Mail Survey or already included in the PDS sample, and c) were born before 1943, and a 64% random sample of respondents who a) gave an interview in 2008, b) were not included in the sample for the Consumption and Activities Mail Survey or already included in the PDS sample, and c) were born in 1943 or*

later. This sample was designed so that 20% of the HRS sample would not be asked to complete a mail survey in the fall of 2009…of the 7,080 remaining eligible cases, 5,333 returned questionnaires or completed a telephone interview, for a response rate of 75%."

The HWB, like the PDS files, contains sensitive health data and are available by request to the HRS. Furthermore, the HWB's prescription drug use measures have not been publicly released, as they are still in the process of being cleaned and formatted, but preliminary data were provided to the research team as restricted-use data.

Tables 1a and 1b below provide descriptive statistics of demographics, prescription drug use, and outcomes of interest for the sample of HWB respondents, as well as notable opioid- or benzodiazepine-use related measures, such as reporting taking any prescription medication for pain, reporting taking any prescription medication for anxiety/depression, and when current opioid use began. These calculations are weighted to be nationally representative of the 55 and older population, and given that those younger than 55 receive zero weight, these statistics are thus based on 55 and older respondents.[2] **Table 1a** shows statistics weighted for all respondents with positive weight, whereas **Table 1b** shows these statistics for those younger than 70.

Table 1a shows that those with 2009 opioid prescriptions, on average, skew older, more suburban/rural, have less educational attainment, are more likely to be women, are less likely to be married, and are more likely to also be taking medication for anxiety or depression. Although the U.S.' peak of opioid prescribing would be in the years directly after the HWB was fielded, nearly half of those with opioid prescriptions in

[2] Unweighted statistics for the full age range of respondents are available upon request.

the HWB reported started taking opioids at least three years ago (i.e., 2006 or earlier), and a fifth reported taking opioids for more than five years.

Table 1a: Characteristics of 2009 Health and Well-Being Module respondents, by whether they report an opioid prescription

		Opioid Rx in 2009	
		No	**Yes**
	Average Age	68.5	70.1
Age Group	**55-59**	0.21	0.15
	60-64	0.22	0.17
	65-69	0.16	0.21
	70-74	0.13	0.14
	75-79	0.11	0.16
	80-84	0.09	0.09
	85 or Over	0.08	0.09
Education	**Less than High School**	0.15	0.24
	High School	0.36	0.40
	Some College	0.25	0.20
	College and Above	0.24	0.16
	Female	0.56	0.61
Race/Ethnicity	**White, Non-Hispanic**	0.81	0.77
	Black, Non-Hispanic	0.09	0.12
	Hispanic	0.03	0.03
	Non-White, Non-Black, Non-Hispanic	0.07	0.08
Marital Status	**Married**	0.67	0.55
	Divorced	0.11	0.18
	Widowed	0.18	0.24
Urbanicity/Rurality	**Urban**	0.47	0.41
	Suburban	0.20	0.23
	Rural	0.33	0.37
	Any Pain Rx (2008)	0.21	0.55
	Any Anxiety/Depression Rx (2008)	0.16	0.25
	Benzodiazepine Rx (2009)	0.06	0.16
	Gabapentin Rx (2009)	0.02	0.11
Opioid Initiation	**Just Started**		0.09
	1-5 Months Ago		0.09
	6-12 Months Ago		0.18
	1-2 Years Ago		0.17
	3-5 Years Ago		0.27
	More than 5 Years Ago		0.20
	N	3,842	477

Note: Authors' calculation; weighted averages among 2009 Health and Well-Being Module HRS Respondents.

Table 1b: Characteristics of 2009 Health and Well-Being Module respondents younger than 70, by whether they report an opioid prescription

		Opioid Rx in 2009	
		No	**Yes**
	Average Age	61.6	62.5
Age Group	**55-59**	0.35	0.28
	60-64	0.38	0.32
	65-69	0.28	0.40
Education	**Less than High School**	0.10	0.21
	High School	0.35	0.40
	Some College	0.28	0.22
	College and Above	0.27	0.17
	Female	0.54	0.61
Race/Ethnicity	**White, Non-Hispanic**	0.79	0.71
	Black, Non-Hispanic	0.10	0.14
	Hispanic	0.03	0.06
	Non-White, Non-Black, Non-Hispanic	0.08	0.09
Marital Status	**Married**	0.67	0.55
	Divorced	0.11	0.18
	Widowed	0.18	0.24
Urbanicity/Rurality	**Urban**	0.47	0.43
	Suburban	0.18	0.24
	Rural	0.35	0.34
	Any Pain Rx (2008)	0.22	0.55
	Any Anxiety/Depression Rx (2008)	0.18	0.30
	Benzodiazepine Rx (2009)	0.06	0.16
	Gabapentin Rx (2009)	0.02	0.11
Opioid Initiation	**Just Started**		0.10
	1-5 Months Ago		0.08
	6-12 Months Ago		0.21
	1-2 Years Ago		0.19
	3-5 Years Ago		0.22
	More than 5 Years Ago		0.20
	N	1,660	202

Note: Authors' calculation; weighted averages among 2009 Health and Well-Being Module HRS respondents younger than 70 in 2009

These patterns hold for the younger than 70 population of 2009 HWB respondents, as shown in **Table 1b**. We note here that the variable of taking any prescription for pain comes from the core HRS in 2008 and, thus, corresponds to the previous year. Prescription opioid use in 2009, by definition, implies prescription pain medication, but approximately 40% of those with 2009 opioid prescriptions reported starting in the last year, accounting for why just over half of these individuals reported taking prescription pain medication when interviewed a year previously.

Tables 2a and 2b below provide comparisons between 2009 HWB respondents younger than 70 with and without opioid prescriptions based on health status and labor market measures in the 2002 and 2008 HRS respectively. These measures correspond to the outcomes of interest to the study's analysis, including employment status, health status, impairments that limit work or daily living, and SSDI status. These pre-2008 differences in outcomes of interest provide a control for the study's analysis of outcomes by 2009 opioid prescription status from 2010 to 2018.

Based on 2002 and 2008 health and employment status, individuals with opioid prescriptions in 2009 are significantly different across the board from individuals without 2009 opioid prescriptions. As shown in **Table 2a**, those with 2009 opioid prescriptions score significantly more negatively on these measures in 2002 compared to those without these prescriptions in almost every category (except for applications for SSDI/SSI). Similar differences hold for these measures in 2008 as shown in **Table 2b**, except for in receiving SSA retirement benefits (i.e., Old Age Insurance or OAI). The main take-away from these two comparisons is that there is high selection for many different labor market and health dimensions into who has an opioid prescription in 2009. This selection exists in t2008 as well as *seven years before* this 2009 snapshot of opioid use. In the following analysis, we condition outcome measures on 2008 variables; however, we test the controls' efficacy by conducting comparisons in the years prior to 2008 to determine whether we have eliminated observed selection.

Table 2a: Comparison of 2002 outcomes, by 2009 opioid prescription

	Opioid Rx in 2009	
2002 Measures	No	Yes
In the Labor Force***	0.75	0.59
Any Back Problems***	0.42	1.00
Fair/Poor Health***	0.14	0.40
Work-Limiting Health Condition***	0.16	0.42
Any ADL***	0.07	0.33
# ADLs[1]	1.63	1.57
Any iADL*	0.03	0.07
# iADLs[1]	1.19	1.17
Any Mobility Impairments***	0.36	0.61
# Mobility Impairments[1]	1.81	2.49
CESD Score (0-8)***	1.25	2.25
Applying for SSDI/SSI	0.01	0.02
Receiving SSD/SSI***	0.03	0.13
Receiving SSA OAI***	0.03	0.10
N	1,133	118

Table 2b: Comparison of 2008 outcomes by 2009 opioid prescription

	Opioid Rx in 2009	
2008 Measures	No	Yes
In the Labor Force***	0.60	0.32
Any Back Problems***	0.34	0.68
Fair/Poor Health***	0.23	0.50
Work-Limiting Health Condition***	0.25	0.67
Any ADL***	0.10	0.31
# ADLs[1]	1.69	1.98
Any iADL***	0.05	0.11
# iADLs[1]	1.36	10.10
Any Mobility Impairments***	0.40	0.77
# Mobility Impairments[1]	1.97	2.78
CESD Score (0-8)***	1.33	2.37
Applying for SSDI/SSI***	0.01	0.09
Receiving SSD/SSI***	0.10	0.32
Receiving SSA OAI	0.62	0.62
N	1,660	202

Note for Tables 2a & b: Authors' calculation; weighted averages among 2009 Health and Well-Being Module HRS respondents younger than 70 in 2009 and, for those with a 2009 opioid prescription, who report initiating opioid use within five years of 2009. * 5% significance, ** 1% significance, *** 0.1% significance [1] Number of conditions contingent on having any conditions.

Finally, there has been substantial geographic variation in the opioid epidemic along a range of margins, including initial OxyContin marketing, physician prescribing behavior, per-capita prescriptions, prescription drug monitoring programs, composition of opioid type, access to illicit prescription-opioid alternatives, and access to opioid use disorder treatment options (Maclean et al. 2020). The public-use HRS has geographic detail only down to the census division. We link our HRS and HWB files to restricted-use geocodes, identifying the state and county of residence of HRS respondents in each wave. We rely on these links to test the relevance of opioid supply policies, as well as the local prevalence of opioid prescriptions, to individual opioid use, providing a potential identification source for the consequences of opioid use.

Analytic approach

Central to any analysis of the effect of a medication on an outcome of interest is this issue of selection: Why is it that some individuals take a medication while others do not? If the reason for initiating use of a medication relates to an outcome of interest — e.g., work capacity, pain, and pain medication — then merely examining the relationship between the medication and the outcome will conflate the medication's effect with the fact that the individual was in debilitating pain and, thus, sought out the medication. Indeed, the results previously presented highlight that individuals receiving opioid prescriptions in 2009 had far different labor market and health histories than those not receiving opioid prescriptions.

An analyst's ideal setting would be for the reason for taking a medication to be entirely unrelated to the outcome of interest, as exemplified by a randomized control trial. Large differences in policy or natural experiments may provide similar exogenous variation in medication take-up. An alternative approach is to control for pre-existing differences across those taking a medication or not, hopefully accounting for selection effects either through regression adjustment or propensity-score matching techniques (aka “selection on observables”). Ultimately, the analyses presented in this paper are based on the latter approach, which, by definition, cannot account for selection on unobservable characteristics. We rely on this approach for two reasons: First, as described below, the instruments typically used as exogenous sources of variation for opioid use behaviors do not have sufficient predictive power in this context; and second, the HRS has many longitudinal health status measures, allowing for both a sophisticated array of controls and the validation of these controls through examination of pretrends.

Attempted instruments

The primary observed “treatment” in this analysis is prescription opioid use in the 2009 HWB. As discussed above, this point-in-time measurement directly precedes the U.S.’ opioid prescription peak, and before implementation of specific measures to curb opioid misuse (e.g., Oxycontin reformulation, must-access PDMPs, up-scheduling of hydrocodone combination medications). The sampling frame of the HRS is limited to those 50 and older. The restriction to this age group during this time period, combined with sample size limitations (fewer than 2,000 respondents younger than 70), leads to limited predictive power of policy variables over opioid use. That is, “triplicate states” (Alpert et al. 2019), county-level prescription rates, or the presence of PDMPs are all weak instruments in this context, made all the weaker once pre-2009 health measures, such as the presence of a back problem, fair/poor self-reported health, or work-limitations, are included in the first stage. The authors' supposition is that during this particular time period for the 50 and older population, opioid access was not primarily contingent on prevailing supply restrictions, but instead on health-status-induced demand. This can be observed with substantial accuracy in the HRS through the measures discussed above. As such, we turn to analyses leveraging these proxy measures in the interviews prior to the 2009 HWB opioid use measurement.

Regression-based analytic approach

The underlying structure of the "selection-on-observable" approach we use relies on the HRS' long-panel structure. We control for 2008 health and economic status measurements, validate the success of these controls by examining pre-2008 differences in outcomes of interest, and then estimate differences in outcomes of interest from 2010 to 2018. The clearest formulation of this approach is in the regression format:

$$Outcome_{i,t} = \alpha + \beta_O Opioid_{i,2009} + \Gamma Y_i + \theta X_{i,2008} + \rho_t + \gamma_s + \varepsilon_{it}$$

That is, outcomes are at the individual-by-year level, indexed by both *i* and *t*, while all controls are either time-invariant (e.g., educational attainment), captured by the vector *Y,* or are fixed to 2008 measures (e.g., self-reported health status), captured by the vector *X*. Finally, year and geocode (namely, census division, given the limited sample size) fixed effects are included.

We present results from weighted regressions, using the 2009 HWB medication-respondent weights. Furthermore, we adjust these weights using inverse-probability weights of surviving to the interview in question. That is, we over-weight individuals in each interview year for whom logit models predict a lower survival probability based on

2008 metrics. Unweighted regression results are shown in the appendix, as are analyses that include deaths within each poor health/disability outcome (e.g., recoding poor/fair health as either the respondent reporting poor/fair health or having died). Finally, we present results based on linear probability models to simplify interpretation; probit and logit analyses produce statistically similar results and are available upon request.

The major outcomes of interest in our analysis are all indicator variables:

i. Mortality: Is the respondent deceased as of the year in question?
ii. Poor/fair health: Does the respondent indicate they are in the bottom two categories of the five-category self-reported health measure?
iii. Work-limited: Does the respondent report having a health condition that limits the kind or amount of paid work they can do?
iv. In the labor force: Does the respondent report being in the labor force?
v. SSDI/SSI participation: Does the respondent report having applied for either program or receiving benefits from either program? This analysis is limited to individuals younger than 65.

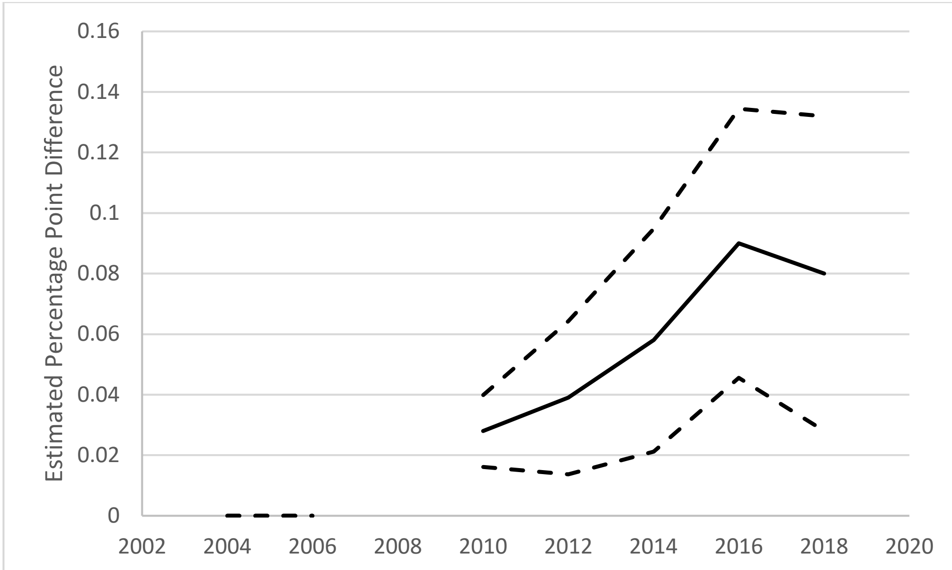
We conduct additional subanalyses of the fifth outcome to measure the application timing to SSA disability programs versus benefit receipt from one or both of these programs.

Results

We present our findings as event-studies figures, showing the mean difference in the outcome of interest for those with 2009 opioid prescriptions, by year (full regression results are available upon request, with example estimated models included in the appendix). All regressions control for 2008 measures of age, gender, educational attainment, race/ethnicity, urbanicity/rurality, marital status, household income, labor force status, earnings if employed, benzodiazepine use, poor/fair health, work-limitation, any mobility/ADL/IADL impairments, and any SSDI/SSI program participation. For the analyses examining labor force status, we restrict the sample to those in the labor force in 2008. For the analyses examining SSDI/SSI program participation, we restrict the analysis to those *not* applying for or receiving SSDI/SSI benefits in 2008. We run separate linear probability model (LPM) specifications for each outcome year from 2004 to 2018. Pre-2008 estimates provide evidence of the controls' efficacy in eliminating pretrends, while post-2008 estimates describe the outcome of interest's evolution after the difference in 2009 opioid use. Because all time-varying controls are fixed in 2008, including ones that are outcomes of interest, we omit 2008 a as year for which we provide estimates. All figures provide the point estimate in question and a 95% confidence interval.

The first outcome straightforwardly illustrates our estimated effect of opioid prescription: the difference in the likelihood of having died, by year.

Figure 1: Regression-estimated effect of 2009 opioid use on cumulative mortality

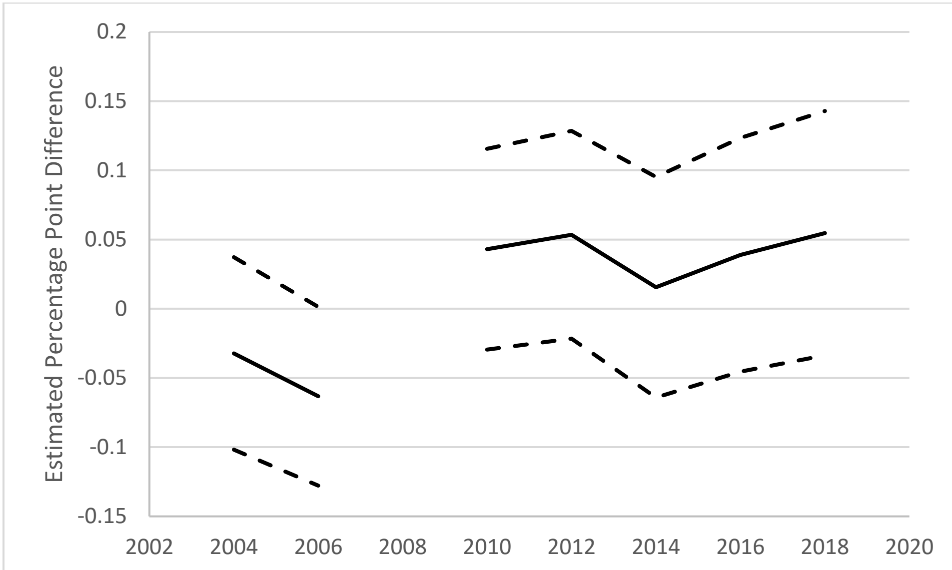


Note: The solid line corresponds to linear probability model coefficient estimates of Health and Retirement Study respondents' 2009 prescription drug use for the outcome variable in that year, and the dashed lines represent the 95% confidence interval of that estimate; each outcome year is a separate regression. All regressions control for 2008 measures of age, gender, educational attainment, race/ethnicity, urbanicity/rurality, marital status, household income, labor force status, earnings if employed, benzodiazepine use, poor/fair health, work-limitation, any mobility/ADL/IADL impairments, and any SSDI/SSI program participation.

From the construction of our analysis, all individuals live to the year 2009, and hence there is no difference in mortality prior to that year. But immediately, a statistically significantly difference arises in mortality, starting with those with 2009 opioid prescriptions having a 3-percentage-point higher chance of dying by the year 2010. This difference rises, plateauing at an approximately 8 percentage point excess cumulative mortality by 2016 and 2018. These differences are statistically significant at every year after 2009. Although we conduct subanalyses for benzodiazepine and gabapentin co-use, we detect no statistically significant differences in mortality across opioid use than those estimated in Figure 1.

This significant differential mortality creates the potential for severe attrition bias in estimating health- or functional-limitation estimates, since opioid use's effects on subsequent health outcomes may be biased by survivors' better health or functioning. For the analyses to follow, we, therefore, reweight by developing inverse probability weights, estimating survival probability by observable characteristics, then adjusting the 2009 HWB weights by the inverse of one minus these estimated probabilities, thereby weighting more heavily those individuals who are observationally more likely to die earlier. The appendix contains unweighted regressions as well as nearest-neighbor matching estimates, also unweighted, all of which are consistent, if less statistically precise, with the estimates shown. We, thus, turn to our next outcome measure: reporting being in poor or fair health as opposed to good, very good, or excellent health.

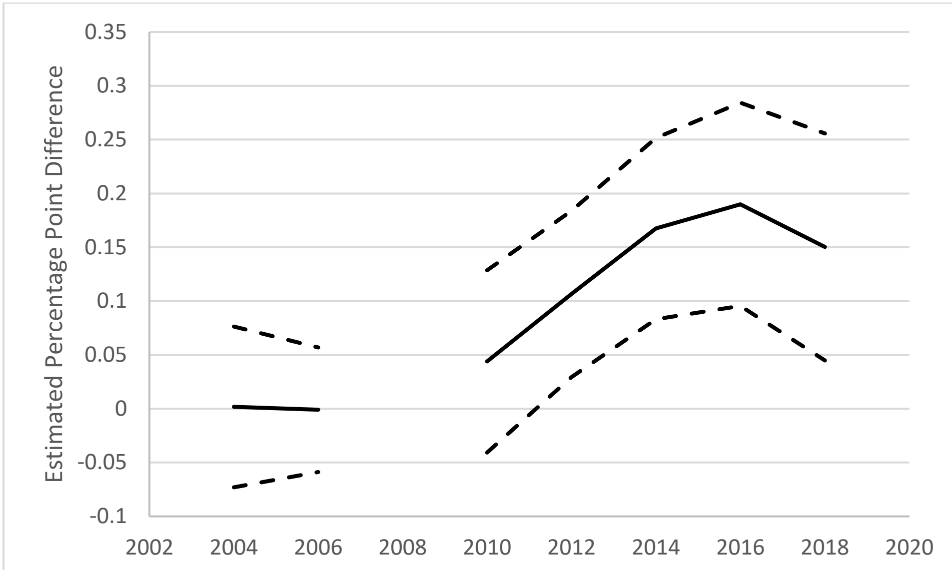
Figure 2: Regression-estimated effect of 2009 opioid use on reporting poor/fair health



Note: The solid line corresponds to inverse-mortality-probability-weighted linear probability model coefficient estimates of Health and Retirement Study respondents' 2009 prescription drug use for the outcome variable in that year, and the dashed lines represent the 95% confidence interval of that estimate; each outcome year is a separate regression. All regressions control for 2008 measures of age, gender, educational attainment, race/ethnicity, urbanicity/rurality, marital status, household income, labor force status, earnings if employed, benzodiazepine use, poor/fair health, work-limitation, any mobility/ADL/IADL impairments, and any SSDI/SSI program participation.

Here, we see a lack of a statistically significant difference in reporting poor or fair health before the 2009 HWB module. If anything, those with 2009 opioid prescriptions are slightly trending toward better health. Starting in 2010, however, opioid use is associated with being more likely to report being in these two bottom rungs of the self-reported health distribution, albeit not statistically significantly so. We, thus, turn to more relevant measures for the question at hand: Are those with 2009 opioid prescriptions more likely to report work-limiting health conditions?

Figure 3: Regression-estimated effect of 2009 opioid use on likelihood of reporting a work-limiting health condition

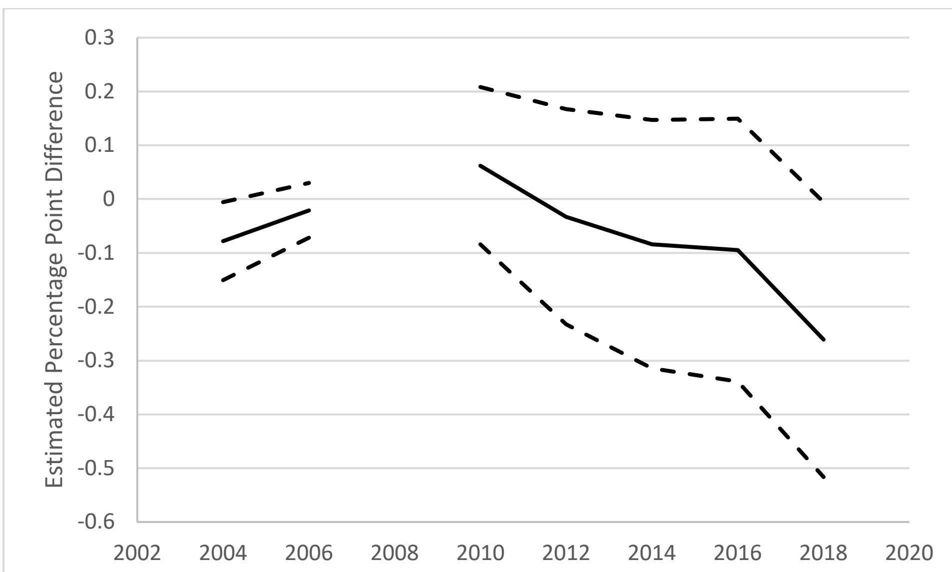


Note: The solid line corresponds to inverse-mortality-probability-weighted linear probability model coefficient estimates of Health and Retirement Study respondents' 2009 prescription drug use for the outcome variable in that year, and the dashed lines represent the 95% confidence interval of that estimate; each outcome year is a separate regression. All regressions control for 2008 measures of age, gender, educational attainment, race/ethnicity, urbanicity/rurality, marital status, household income, labor force status, earnings if employed, benzodiazepine use, poor/fair health, work-limitation, any mobility/ADL/IADL impairments, and any SSDI/SSI program participation.

Again, our regression adjustments appear to have completely accounted for pre-2009 differences in work disability. Furthermore, in 2010, there was not a statistically significant difference in work disability by 2009 opioid prescription status. However, starting in 2012 and continuing through 2018, those with 2009 opioid prescriptions were 10 to 19 percentage points more likely to report having a work-limiting health condition, an approximately 40% increase over the baseline. Remember that we are accounting for observable differences alone. Although the nonexistence of pre-2009 differences and the limited 2010 differences perhaps speak to addressing work-disability-based selection into 2009 opioid use, we do not have an experimental design that fully purges selection as an explanation for these estimates. Nevertheless, given the magnitude of these estimates, we next examine two margins: remaining in the labor force or applying for SSDI/SSI.

To examine labor force participation, we further limit our sample to HRS respondents who report being in the labor force in 2008.

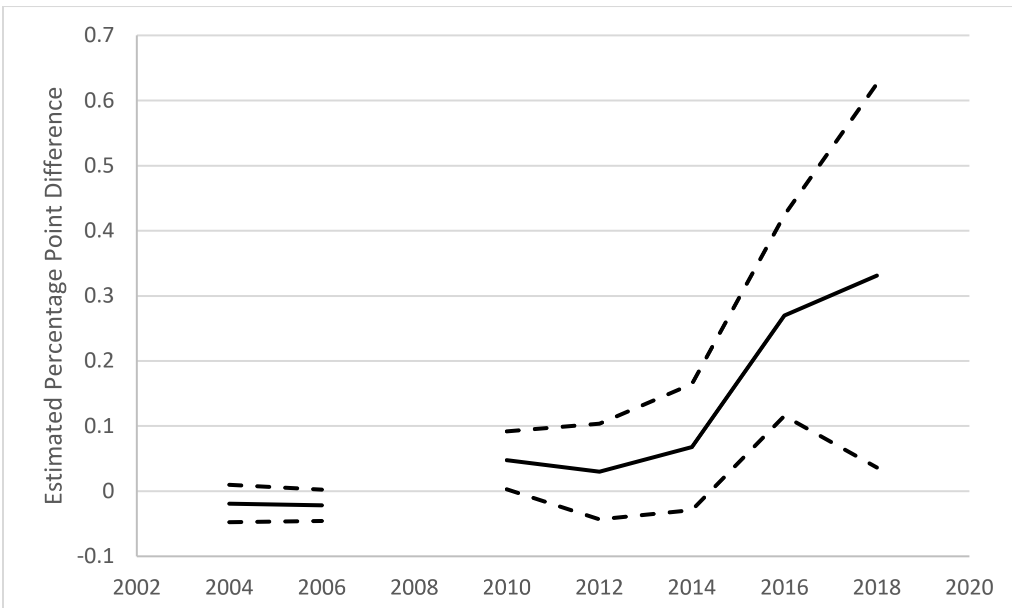
Figure 4: Regression-estimated effect of 2009 opioid use on labor force participation



Note: The solid line corresponds to inverse-mortality-probability-weighted linear probability model coefficient estimates of Health and Retirement Study respondents' 2009 prescription drug use for the outcome variable in that year, and the dashed lines represent the 95% confidence interval of that estimate; each outcome year is a separate regression. All regressions control for 2008 measures of age, gender, educational attainment, race/ethnicity, urbanicity/rurality, marital status, household income, earnings if employed, benzodiazepine use, poor/fair health, work-limitation, any mobility/ADL/IADL impairments, and any SSDI/SSI program participation. Sample limited to respondents in the labor force in 2008.

Again, we see minimal pre-2009 differences across 2009 opioid prescription status. Despite what appears to be a general trend downward in labor force participation, none of these estimates individually are statistically significant. We now turn to a major outcome of interest: disability program participation.

Figure 5a: Regression-estimated effect of 2009 opioid use on applying for or receiving SSDI or SSI benefits

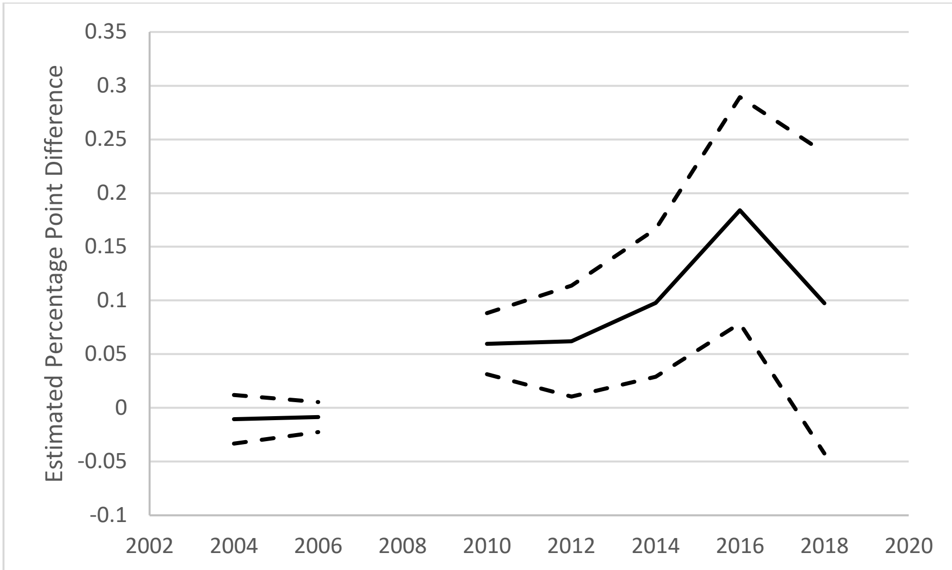


Note: The solid line corresponds to inverse-mortality-probability-weighted linear probability model coefficient estimates of Health and Retirement Study respondents' 2009 prescription drug use for the outcome variable in that year, and the dashed lines represent the 95% confidence interval of that estimate; each outcome year is a separate regression. All regressions control for 2008 measures of age, gender, educational attainment, race/ethnicity, urbanicity/rurality, marital status, household income, labor force status, earnings if employed, benzodiazepine use, poor/fair health, work-limitation, and any mobility/ADL/IADL impairments. Sample limited to 2008 SSDI/SSI nonparticipants.

Again, there are minimal pre-2009 differences, although since we construct our sample contingent on no 2008 SSDI/SSI program participation, this lack of pre-trends is not surprising. We note that we also limit our sample to HRS respondents younger than 65 in the given outcome year.

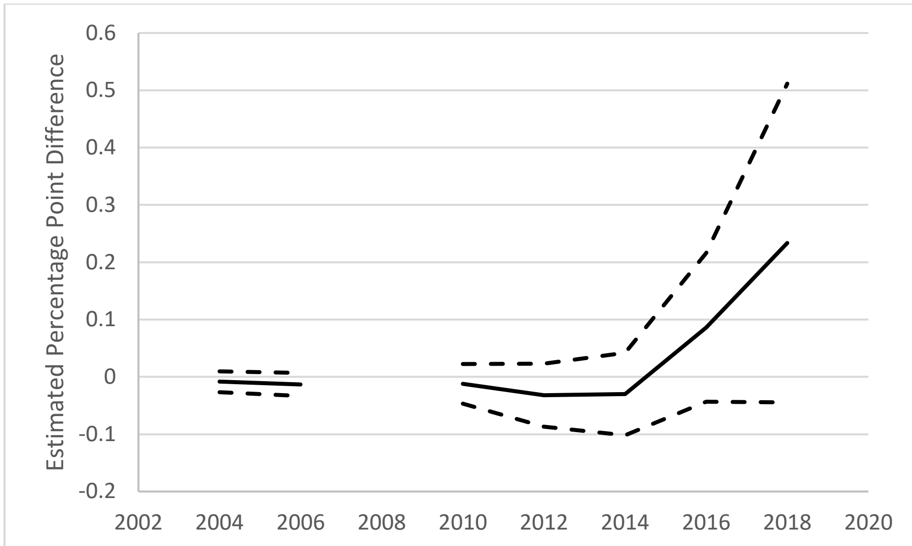
We see a slightly higher rate of SSDI/SSI program participation for those with 2009 opioid prescriptions in 2010 through 2014. By 2016, this difference has increased and is statistically significant, with those with 2009 opioid prescriptions being nearly *30 percentage points* more likely to have applied for or be receiving SSDI/SSI benefits. Since the baseline rate for SSDI/SSI participation in 2008 is 11%, this difference amounts to nearly a 300% increase in disability program participation. We note in Figures 5b and 5c that the difference from 2010 to 2016 appears to be driven by applications, with differences in benefit receipt arising in 2016 and 2018.

Figure 5b: Regression-estimated effect of 2009 opioid use on applying for SSDI or SSI benefits



Note: The solid line corresponds to inverse-mortality-probability-weighted linear probability model coefficient estimates of Health and Retirement Study respondents' 2009 prescription drug use for the outcome variable in that year, and the dashed lines represent the 95% confidence interval of that estimate; each outcome year is a separate regression. All regressions control for 2008 measures of age, gender, educational attainment, race/ethnicity, urbanicity/rurality, marital status, household income, labor force status, earnings if employed, benzodiazepine use, poor/fair health, work-limitation, and any mobility/ADL/IADL impairments. Sample limited to 2008 SSDI/SSI nonparticipants.

Figure 5c: Regression-estimated effect of 2009 opioid use on receiving SSDI or SSI benefits



Finally, we refer the reader to the appendix for unweighted regressions that place higher weight on groups with higher predicted mortality. By doing so, we attempt to isolate of opioid use's impact on disability outcomes. If one is interested in the impact of opioid use on disability *program* outcomes, then the unweighted regressions should be consulted.

Conclusion

This study finds long-term disabling consequences of opioid use: Those taking prescription opioids in 2009 were substantially more likely to develop work-limiting health conditions, apply for disability benefits, and receive these benefits, than otherwise similar individuals who did not have opioid prescriptions. Although the HRS' 2009 snapshot of opioid use predates the rise of illicit opioids and is more than a decade "out-of-date," the HRS' long-panel nature presents a unique opportunity to examine nearly a decade of follow-up outcomes. Indeed, our disability program participation estimates do not begin to rise until seven years after the 2009 module, indicating the importance of panel observation. Similarly, substantial, highly statistically significant differences in labor supply, work disability, and health status existed at least seven years prior to our 2009 opioid use measure. The presence of pre-use measures of these variables allowed us to control for these differences and check the validity of these controls. As the HRS continues, researchers can use the growing follow-up window to examine the diverse consequences of opioid prescriptions' rapid rise in the U.S.

Our work contributes to an emerging literature that has sought to assess how population-level rates of prescription opioid availability and use impact labor market and social insurance program participation. However, our use of a rich, longitudinal, individual-level data set allows us to go beyond the ecological analyses of this prior work, which is subject to noted limitations for inferring causality (Robinson 1950; Finney et al. 2015). By directly observing individuals with an opioid prescription in 2009 as well as a comparable group of nonopioid-prescription recipients, our findings complement existing work by further clarifying prescription opioid use's long-term consequences on health and economic functioning among an at-risk population (Chang 2018; Schuler et al. 2020; Schepis and McCabe 2019).

This analysis' results indicate that, in addition to the clear mortality risk, prescription opioid use does have a measurable, yet gradual, impact on work disability and disability program participation, leading to higher rates of disability among older workers. There are corresponding implications for prescribers and policymakers:

Although the pain relief is an important health goal, the consequences to workers and social programs of powerful prescription painkillers are substantial and long-lasting, potentially arising only gradually.

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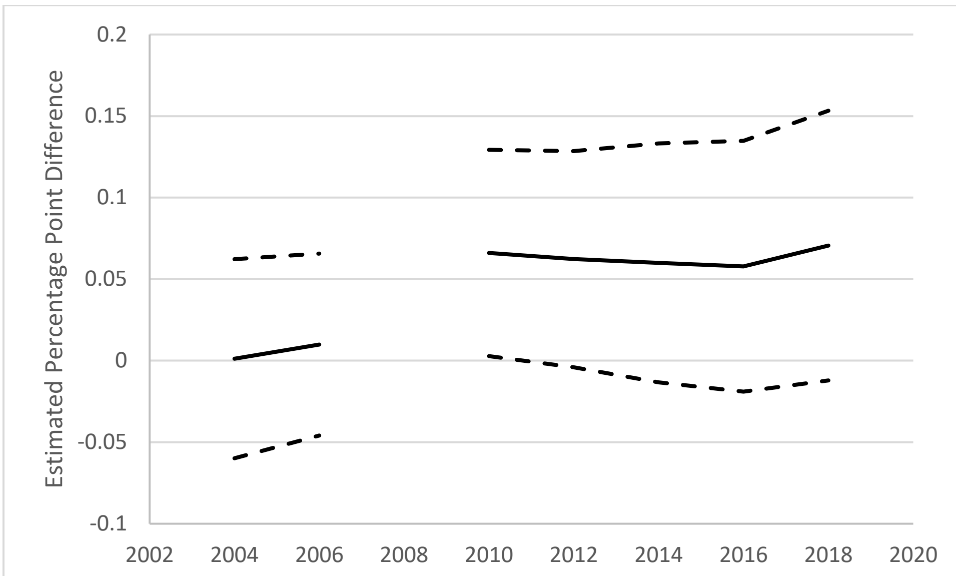
Appendix: Figures and tables

Section 1: Unweighted regressions

Figure A1: Unweighted regression-estimated effect of 2009 opioid use on reporting poor/fair health



Note: The solid line corresponds to linear probability model coefficient estimates of Health and Retirement Study respondents' 2009 prescription drug use for the outcome variable in that year, and the dashed lines represent the 95% confidence interval of that estimate; each outcome year is a separate regression. All regressions control for 2008 measures of age, gender, educational attainment, race/ethnicity, urbanicity/rurality, marital status, household income, labor force status, earnings if employed, benzodiazepine use, poor/fair health, work-limitation, any mobility/ADL/IADL impairments, and any SSDI/SSI program participation.

Figure A2: Unweighted regression-estimated effect of 2009 opioid use on likelihood of reporting a work-limiting health condition

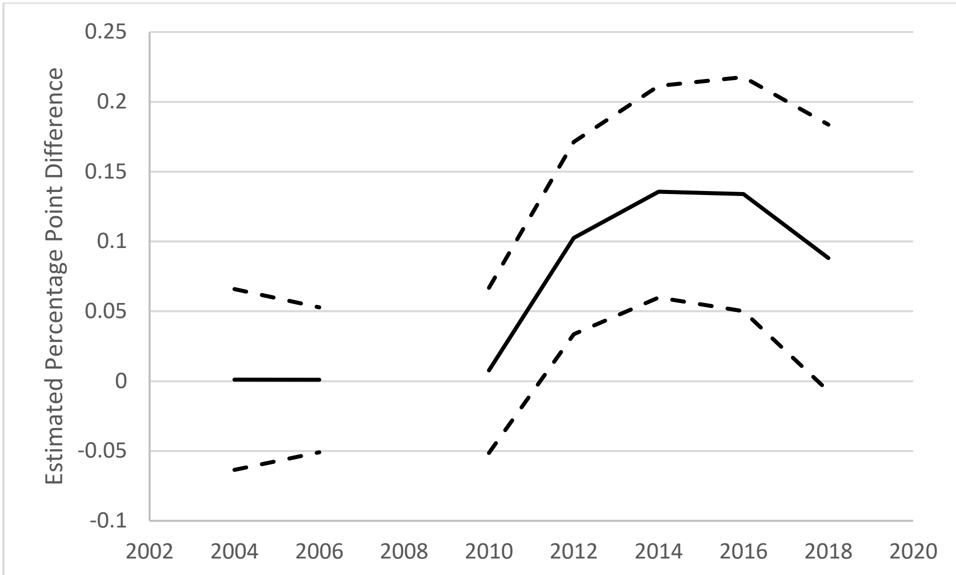


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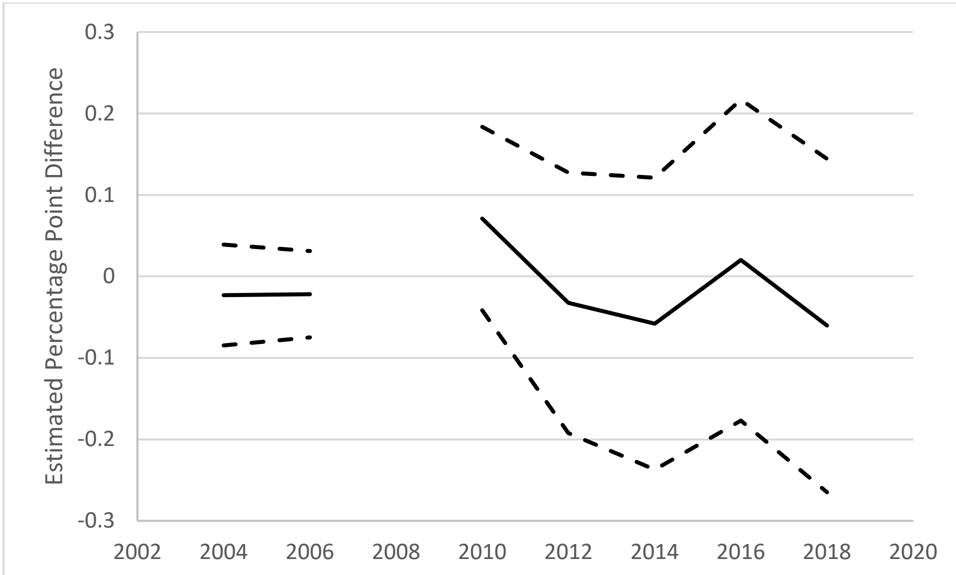
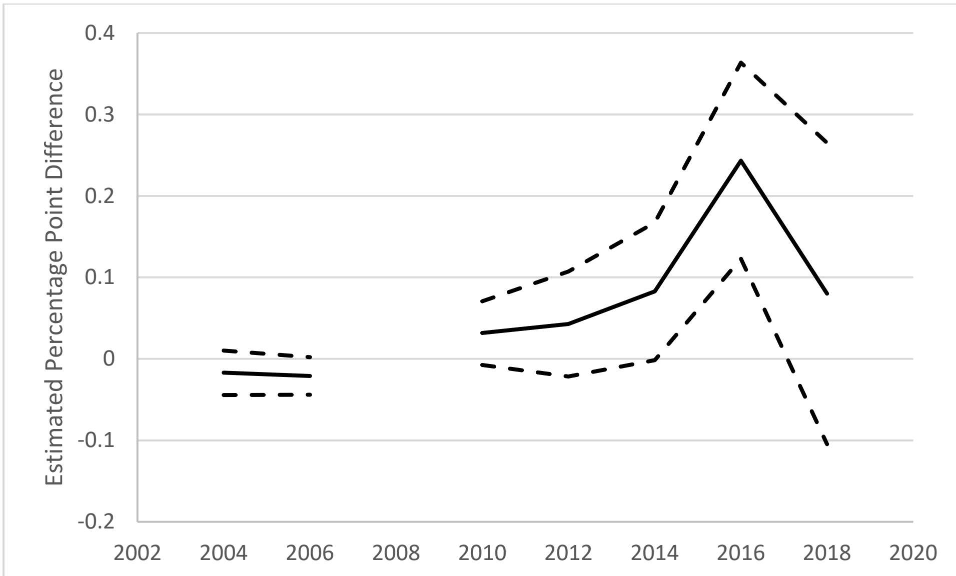
Figure A3: Unweighted regression-estimated effect of 2009 opioid use on labor force participation



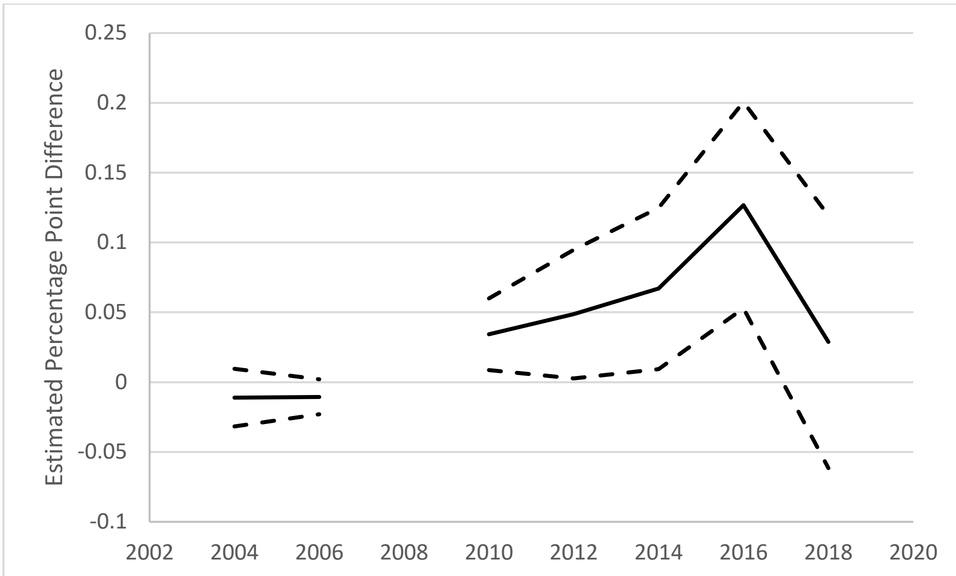
Note: The solid line corresponds to linear probability model coefficient estimates of Health and Retirement Study respondents' 2009 prescription drug use for the outcome variable in that year, and the dashed lines represent the 95% confidence interval of that estimate; each outcome year is a separate regression. All regressions control for 2008 measures of age, gender, educational attainment, race/ethnicity, urbanicity/rurality, marital status, household income, earnings if employed, benzodiazepine use, poor/fair health, work-limitation, any mobility/ADL/IADL impairments, and any SSDI/SSI program participation. Sample limited to those in the labor force in 2008.

Figure A4: Unweighted regression-estimated effect of 2009 opioid use on applying for or receiving SSDI or SSI benefits



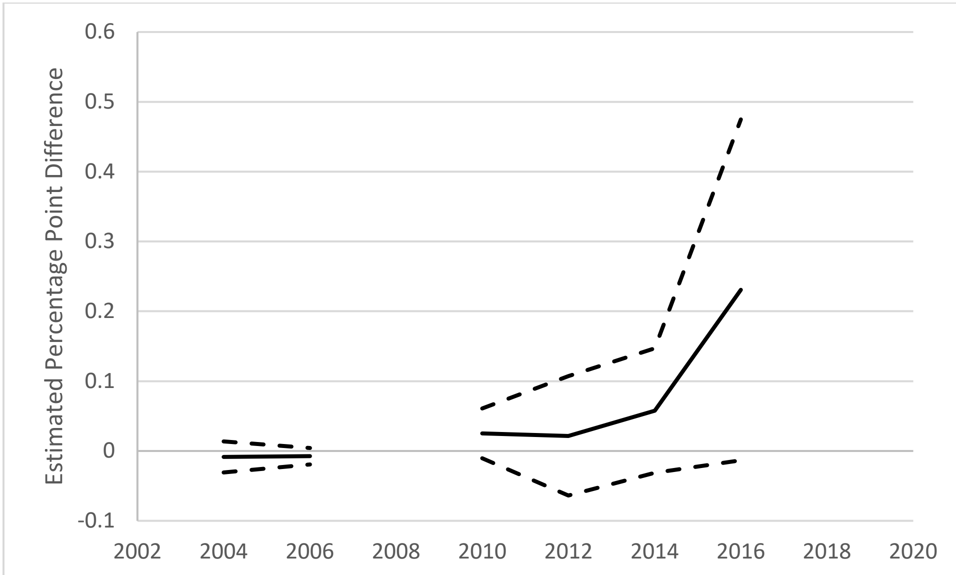
Note: The solid line corresponds to linear probability model coefficient estimates of Health and Retirement Study respondents' 2009 prescription drug use for the outcome variable in that year, and the dashed lines represent the 95% confidence interval of that estimate; each outcome year is a separate regression. All regressions control for 2008 measures of age, gender, educational attainment, race/ethnicity, urbanicity/rurality, marital status, household income, labor force status, earnings if employed, benzodiazepine use, poor/fair health, work-limitation, and any mobility/ADL/IADL impairments. Sample limited to those with 2008 SSDI/SSI program participation.

Figure A5: Unweighted regression-estimated effect of 2009 opioid use on applying for SSDI or SSI benefits



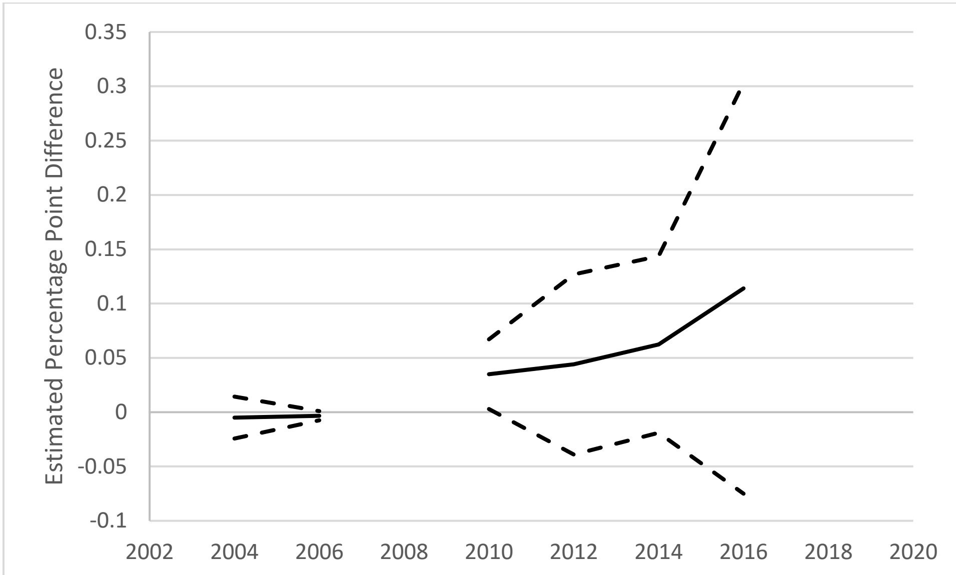
Note: The solid line corresponds to linear probability model coefficient estimates of Health and Retirement Study respondents' 2009 prescription drug use for the outcome variable in that year, and the dashed lines represent the 95% confidence interval of that estimate; each outcome year is a separate regression. All regressions control for 2008 measures of age, gender, educational attainment, race/ethnicity, urbanicity/rurality, marital status, household income, labor force status, earnings if employed, benzodiazepine use, poor/fair health, work-limitation, and any mobility/ADL/IADL impairments. Sample limited to those with 2008 SSDI/SSI program participation.

Figure A10: Nearest-neighbor estimated effect of 2009 opioid use on applying for or receiving SSDI or SSI benefits



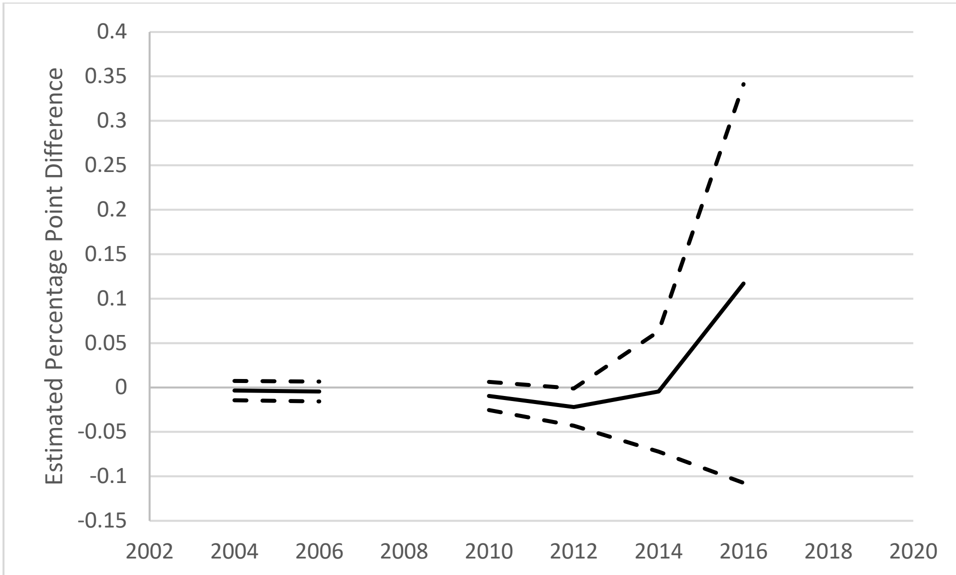
Note: The solid line corresponds to estimated average treatment effects from 1:1 nearest neighbor matches among Health and Retirement Study respondents' 2009 prescription drug use for the outcome variable in that year, and the dashed lines represent the 95% confidence interval of that estimate; each outcome year is a separate estimated model. Matches based on 2008 measures of age, gender, educational attainment, race/ethnicity, urbanicity/rurality, marital status, household income, earnings if employed, benzodiazepine use, poor/fair health, work-limitation, any mobility/ADL/IADL impairments, and labor force status. Sample limited to those without 2008 SSDI/SSI program participation.

Figure A11: Nearest-neighbor estimated effect of 2009 opioid use on applying for SSDI or SSI benefits



Note: The solid line corresponds to estimated average treatment effects from 1:1 nearest neighbor matches among Health and Retirement Study respondents' 2009 prescription drug use for the outcome variable in that year, and the dashed lines represent the 95% confidence interval of that estimate; each outcome year is a separate estimated model. Matches based on 2008 measures of age, gender, educational attainment, race/ethnicity, urbanicity/rurality, marital status, household income, earnings if employed, benzodiazepine use, poor/fair health, work-limitation, any mobility/ADL/IADL impairments, and labor force status. Sample limited to those without 2008 SSDI/SSI program participation.

Figure A12: Nearest-neighbor estimated effect of 2009 opioid use on receiving SSDI or SSI benefits



Note: The solid line corresponds to estimated average treatment effects from 1:1 nearest neighbor matches among Health and Retirement Study respondents' 2009 prescription drug use for the outcome variable in that year, and the dashed lines represent the 95% confidence interval of that estimate; each outcome year is a separate estimated model. Matches based on 2008 measures of age, gender, educational attainment, race/ethnicity, urbanicity/rurality, marital status, household income, earnings if employed, benzodiazepine use, poor/fair health, work-limitation, any mobility/ADL/IADL impairments, and labor force status. Sample limited to those without 2008 SSDI/SSI program participation.

Appendix table A1: Linear probability model estimates, dependent variable is "Has Died by 2016"

2008 Measures (Unless Indicated Otherwise)		Coefficent	SE
2009 Opioid Prescription		0.086	0.023
Female		-0.027	0.014
Urbanicity/Rurality	**Urban**	-	-
	Suburban	0.035	0.018
	Rural	0.033	0.015
Race/Ethnicity	**White, Non-Hispanic**	-	-
	Black, Non-Hispanic	0.011	0.020
	Hispanic	-0.032	0.037
	Non-White, Non-Black, Non-Hispanic	-0.017	0.024
	Household Income/100,000	0.043	0.069
	Earnings/100,000	-0.071	0.181
	Age	0.006	0.001
Education	**Less than High School**	-	-
	High School	0.017	0.022
	Some College	0.025	0.023
	College and Above	0.025	0.025
	Married	0.014	0.016
	Fair/Poor Health	0.073	0.018
	Any SSDI/SSI	0.062	0.024
	Any ADL	0.058	0.024
	Any iADL	0.091	0.031
	Any Mobility Impairments	0.030	0.016
	Benzodiazepine Rx (2009)	-0.034	0.025
	In Labor Force	0.009	0.017
	Work-Limiting Health Condition	0.008	0.020
	Adjusted R^2	0.103	
	N		1,486

Appendix table A2: Linear probability model estimates, dependent variable is

"Any SSDI/SSI in 2016"

2008 Measures (Unless Indicated Otherwise)		Coefficent	SE
	2009 Opioid Prescription	0.270	0.079
	Female	0.001	0.041
Urbanicity/Rurality	Urban	-	-
	Suburban	-0.111	0.058
	Rural	0.012	0.045
Race/Ethnicity	White, Non-Hispanic	-	-
	Black, Non-Hispanic	0.170	0.064
	Hispanic	-0.064	0.102
	Non-White, Non-Black, Non-Hispanic	-0.120	0.070
	Household Income/100,000	0.108	0.212
	Earnings/100,000	-0.163	0.403
	Age	0.008	0.020
Education	Less than High School	-	-
	High School	-0.234	0.094
	Some College	-0.291	0.095
	College and Above	-0.306	0.097
	Married	-0.010	0.052
	Fair/Poor Health	0.171	0.055
	Any ADL	0.238	0.086
	Any iADL	-0.725	0.234
	Any Mobility Impairments	0.060	0.053
	Benzodiazepine Rx (2009)	0.012	0.091
	In Labor Force	0.014	0.060
	Work-Limiting Health Condition	0.019	0.067
	Adjusted R2	0.255	
	N		23